

How do assessed values affect transaction prices of homes?*

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Abstract

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Property taxes are commonly calculated as a fraction of a home's assessed value (AV). AVs should impact trading prices of the underlying homes through two counteracting channels. First, an unexpected increase in the AV implies increased tax payments, which should negatively affect a home's trading price (tax channel). Second, AVs are potential reference prices, which should lead to a positive effect (anchoring channel). In a quasi-experimental setting that exploits geographic variation in AV publication dates and reassessment frequencies, I show that a higher AV leads to a lower trading price, i.e., that the tax channel dominates.

JEL Classification Codes: R38, H29

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1 Introduction

For many governments worldwide, property taxes are a major source of income. In 2019, for instance, US homeowners paid more than 616 billion USD in property taxes, accounting for almost 40% of state and local tax revenue.¹ To take account of wealth disparities among homeowners, authorities tax each home by a fraction of its official assessed value (AV). AVs constitute an important factor for homeowners, as they are not only central for the calculation of individual tax bills, but also constitute updated market value estimates for the underlying homes. Despite their significant role for homeowners and the sizable redistribution of wealth, it remains largely unexplored how the information contained in AVs is incorporated in homes' transaction prices.

In this paper, I investigate the causal effect of AVs on transaction prices of homes. This effect should be driven by two counteracting channels. On the one hand, once a home's AV is publicly available, it serves as a potential reference price, e.g., during price negotiations. An increasing AV should therefore increase the trading price of the corresponding home (anchoring channel). On the other hand, an unexpected increase in the AV implies a higher future property tax burden, which should in turn negatively affect a home's trading price (tax channel). I identify the dominating channel by employing a novel Difference-in-Differences (DiD) regression setup that exploits AV publication dates and geographic variation in the frequencies in which homes are reassessed. My results document that the tax channel prevails: An unexpectedly higher AV causes a decline in the corresponding transaction price.

My empirical analysis is based on two representative datasets that contain historical tax records and individual transactions of homes, respectively. I focus on single-family homes in New York State for several reasons. First, within each municipality, AVs are published annually at a particular date, allowing me to compare pre- and post-publication periods. Second, reassessment frequencies vary on the municipal level, such that homes sold in tax years without reassessment serve as control group. Third, AVs are not forecasts, but have to reflect home values in the past. Fourth, unlike, e.g., in California, there is no post-transaction adjustment of AVs. Instead, AVs are collectively generated by a local assessor. Together, these characteristics provide the opportunity to employ a DiD setting that allows for causal inference.

The term "assessed value" requires clarification. While local assessors estimate the fair market value of each home, "assessed values" in the most technical sense can also describe the *share* of the estimated market value that is taxable. In this paper, I avoid this technical

¹Source: US Census Bureau, Quarterly Summary of State and Local Government Tax Revenue. Last retrieved on August 17, 2020 from <https://www.census.gov/data/tables/2019/econ/qtax/historical.Q4.html>.

definition and use the term “AV” to refer to the full market value estimate provided by the local assessor, which is favorable for several reasons. First, using the full market value preserves cross-municipal comparability, as the taxable share, in contrast, varies over jurisdictions and time. Second, the external validity of this study is enhanced, as market values are not only the nationwide basis for property taxation, but are also similarly employed in many other countries around the world such as Canada or Japan. Third, while the taxable share of market values might be capped in particular areas such as New York City, the fair market value is not, such that its growth rate can inform about the future development of tax payments.²

To identify the effect of AVs on transaction prices, a measure other than raw sales prices is necessary. This is the case as homes can be heterogeneously affected through the same channel, even within the same treated municipality. For instance, regardless of which channel prevails, homes with unexpectedly low AVs face opposite price changes compared to homes with unexpectedly high AVs. Consequently, it is possible that AV-induced positive and negative price changes cancel each other, making the investigation of aggregate price changes uninformative for this study. I solve this problem by investigating the relative distance of sales price and AV. In a theory section, I formally show that this distance measure should change in opposing directions under each channel, respectively. In particular, the sales-price-AV distance should decrease under the anchoring channel, as sales prices move towards homes’ AVs. In contrast, under the tax channel, the sales-price-AV distance should generally increase. Intuitively, an unexpectedly higher AV decreases the transaction price, which should increase the distance between both quantities.

My results document that AVs negatively affect transaction prices, in line with the tax channel. In particular, the absolute sales-price-AV ratio increases by one percentage point after updated AVs are published. A back-of-the-envelope calculation shows that this result translates to a change of the initial sales price of an average home of about 0.9%, with the effect’s sign depending on whether the AV of a given home is unexpectedly high or low, respectively. The results are robust to the inclusion of different local and temporal fixed effects, respectively. Consistent with the tax channel, units associated with higher effective tax rates are stronger affected than units with lower rates. Right after AV publication, no immediate effect is observed, suggesting that when AVs are most salient and up-to-date, anchoring is outweighing the tax channel.

The identification strategy faces multiple challenges that have to be addressed. First, the common trend assumption, crucial for DiD regressions, must be fulfilled. As sale dates are known by day, it is possible to analyze pre-publication trends of treatment and control

²My findings are robust to the exclusion of areas with assessment caps as shown in Section 6.

group, respectively. I find that the conditional means of each group follow similar trends, indicating that the common trend assumption plausibly holds. This conclusion is further supported by similar trends found in several subsamples. I additionally run placebo tests on pre-publication observations, which document no significant difference in pre-treatment trends and coefficient estimates close to zero.

Second, it is possible that buyers and sellers postpone transactions until updated AVs are published to reduce the uncertainty associated with the transaction. This should be of minor concern, as waiting in the housing market is costly, e.g., due to maintenance and opportunity costs, reducing the incentives to hold a property longer than necessary. Additionally, an investigation of transactions in a window around the publication date does not show a jump or a clear trend, and the number of transactions evolve similarly in treatment and control group.

Third, homeowners can challenge the assessment of their home, i.e., the values published at the considered dates are only tentative. To investigate how often AVs are changed after their initial publication, I make use of a dataset from the New York City government on notes sent to homeowners if their AV was changed post-publication. These homes account for only 0.9% of the single-family homes found in my New York City dataset for the following tax year, suggesting that the possibility to contest AVs is of only minor importance for this study.

This work contributes to a growing strand of literature on the effect of property taxation on trading prices. Bai et al. (2014) and Du and Zhang (2015) show that an introduction of a property tax can have a negative or no effect on price growth, exploiting a trial tax in China. Similarly, Elinder and Persson (2017) find only extremely high-valued homes to respond with price declines to an unexpected tax cut in Sweden. Further work, such as Wassmer (1993), Palmon and Smith (1998), Hilber (2017), and Livy (2018), documents that tax rate changes are negatively capitalized in sales prices. While the literature focuses on the tax rate and the introduction of property tax systems, the tax base (here, the AV) received much less attention. This is surprising, given that tax bases can change in different directions within the same district and thus potentially affect prices in opposite directions, even for neighboring homes. This is in sharp contrast to changes in the tax rate, after which prices of all treated homes should adjust in the same direction collectively. I thus contribute to the taxation literature by first, showing that the tax base itself affects trading prices and second, by uncovering an additional taxation effect that is heterogeneous even within the same treated location.

I further contribute to the literature on inequity in property taxation by uncovering price distortion as an additional source of inequitable outcomes. So far, other work such as Goolsby

(1997), Allen and Dare (2002), Sirmans et al. (2008), and Hodge et al. (2017) focusing on inequity in tax payments. I extend this strand of literature by showing that misspecified assessments are even capable of distorting sales prices themselves. As my results indicate that AVs negatively impact sales prices, I uncover a double-punishment for homeowners with unjustified high AVs. Not only do they have to pay an excessive amount of taxes, they also suffer from a price discount when selling their home.

By investigating AVs as a potential anchor for buyers and sellers of homes, I contribute to the literature that underpins the importance of reference quantities in the housing market. So far, several anchoring phenomena have been investigated. Northcraft and Neale (1987) document that even professional real estate agents adjust their appraisals towards a randomized listing price. Genesove and Mayer (2001) show that homeowners consider the initial purchase price as reference point when they are selling their home. Andersen et al. (2021) estimate a structural model of listing decisions and identify the nominal purchase price as a reference point for homeowners. Fischer et al. (2021) document that realized returns of homes traded in close neighborhoods have an increased predictive power for future prices once they are publicly recorded. Similarly, Bailey et al. (2018) show that individuals rely on the house price growth experienced by distant friends when making their buy or rent decision.

In the context of anchoring on AVs, Jones (2020) shows that homeowners confronted with an increase in their AV have a higher propensity to contest their home's assessment, which can be linked to loss aversion. Considering the AVs as an anchor for valuations instead, the evidence provided by the current literature is mixed. Cypher and Hansz (2003) do not find anchoring on assessed values in an experimental setting. In contrast, Levy et al. (2016) find homeowners in New Zealand to be influenced by values that are used for property taxation, but are not necessarily market value estimates. While these studies investigate AVs primarily as anchor, this study expands this view by studying the interplay between tax and anchoring channel in a quasi-experimental framework.

The remainder of this paper is structured as follows. In the next Section 2, I motivate the channels through which AVs should influence trading prices within a theoretical framework. Section 3 describes the empirical approach followed in this paper. The data used and the validity of the methodology applied is discussed in Section 4. Results and robustness checks are presented in Sections 5 and 6, respectively. Section 7 concludes.

2 Theoretical considerations

In this section, I first briefly present crucial features of the New York State property tax practices to set the practical foundations for the model presented afterwards. This model first, theoretically motivates both anchoring and tax channel, respectively, and second, illustrates both channels' implications for the causal impact of AVs on transaction prices.

2.1 Some preliminaries

In each municipality in New York State, AVs are published annually by the local assessor at a particular date T . The frequencies in which homes are reassessed are determined by the municipal governments from annual reassessment to once in several decades. If homes are reassessed in a given period, the updated AVs become available to all market participants at T , e.g., on a public webpage. In contrast, if the municipality is not reassessing homes in the given period, the known AVs from the prior year are published.³ The existence of pre- and post-publication periods as well as years without reassessment in some municipalities yield the key ingredients for the DiD analysis that is described in Section 3.

Apart from AVs, taxes paid by homeowners depend on multiple factors, such as the local tax rate, the local assessment ratio, individual exemptions, and the total municipal tax levy, such that the effective tax rate, τ , can vary substantially across individual homeowners, even within the same jurisdiction. These include the local tax rate, the local assessment ratio, local budgeting, as well as individual exemptions, such that effective tax rates can vary substantially even within the same jurisdiction.⁴ An extended description of the New York State property tax system, based on the state's property tax calendar,⁵ is provided in Appendix A. The following section provides a theoretical model that illustrates the effect of AVs on transaction prices of homes.

³In some municipality-tax-year combinations, AVs are adjusted collectively by the same factor, e.g., to adjust for inflation or general market movements. I exclude such observations in the analysis as they can be neither assigned to treatment nor control group.

⁴Additionally, there are two policies that are limiting the increase of tax payments. First, in New York City and Nassau county, the taxable share of the fair market values can maximally increase by 6% per year or 20% over five years, which might limit the effect of the tax channel. In the robustness Section 6, I show that my results are robust to the exclusion of these areas. Second, in most areas, such as counties and cities outside NYC, the annual increase of the districts' total tax levy is capped by the minimum of either 2% or the CPI inflation rate. Tax levy limits do not rule out significant individual changes in tax payments, however. For instance, a substantial AV increase relative to other properties in the municipality would still lead to a stark increase in tax payments, even if the total tax levy remained constant.

⁵The official tax calendar can be found at the New York State Department of Taxation and Finance website: <https://www.tax.ny.gov/pit/property/learn/proptaxcal.htm> last retrieved on August 25, 2020

2.2 A simple model

I consider a model similar to the closed-form framework in Landvoigt et al. (2015), extended by a second period and property taxation, but with divisible housing. A representative household maximizes lifetime utility, V , over two periods by choosing between (numéraire) consumption c_t ($t = 1, 2$) and units of (divisible) housing stock $n > 0$.

In the first period, the household buys a home at a price that is determined by $p(n) = \dot{p}n$, in which $\dot{p} > 0$ is the price per unit of a home. In the second period, the household pays property taxes as a fraction of the home's known AV, $\tau AV(n)$, in which $AV(n) = \hat{p}n$ with $\hat{p} > 0$ is again linearly increasing in n to reflect that larger homes tend to be assessed at a higher value. The per-unit transaction price, \dot{p} , is likely to diverge from the per-unit AV, \hat{p} , as the local assessor is uncertain about the true model and can thus only provide a best estimate for \dot{p} . The parameter τ is the household's effective tax rate.

For simplicity, further assume that the household is a home buyer and holds initial wealth, $W > 0$, that is used to pay for the home, property taxes, as well as consumption in both periods. Assuming a log-additive utility function and that wealth can be frictionlessly transferred to the second period, the household solves the optimization problem

$$\max_{c_1, c_2, n} V(c_1, c_2, n) = \ln(c_1) + \beta \ln(c_2) + (1 + \beta)\theta \ln(n) \quad (1)$$

$$s.t. \quad W = c_1 + c_2 + n(\dot{p} + \tau\hat{p}), \quad (2)$$

in which $0 < \beta < 1$ is a time preference parameter and $\theta > 0$ determines the importance of housing relative to regular consumption. Optimizing for c_1 , c_2 , and n , the first order conditions imply

$$\frac{1}{c_1} = \beta \frac{1}{c_2} \quad \text{and} \quad (3)$$

$$\frac{(1 + \beta)\theta c_1}{n} = \dot{p} + \tau\hat{p}. \quad (4)$$

Equation (4) shows that, in addition to the closed-form model of Landvoigt et al. (2015), the marginal rate of substitution between lifetime housing utility and consumption depends not only on the marginal house price at size n , \dot{p} , but also on the marginal tax payment at n , $\tau\hat{p}$. Increasing marginal tax payments through either higher τ or \hat{p} therefore imply a higher willingness of the household to substitute housing with consumption. Solving the first-order conditions and assuming market clearing at fixed housing supply \bar{n} , I find the equilibrium

per-unit transaction price given by

$$\dot{p}^o = \frac{W\theta}{(1+\theta)\bar{n}} - \tau\hat{p}. \quad (5)$$

It follows from Equation (5) that the market clearing price of one unit of a home is decreasing in the per-unit AV, \hat{p} , illustrating the tax channel: A *ceteris paribus* higher AV decreases the sales price of a home through an increased tax burden.

The rational choice model described so far does not include anchoring. This is the case as anchoring itself is not rational. For instance, Northcraft and Neale (1987) document that even professional real estate agents adjust their appraisals towards randomly assigned ask prices. Similarly, Black and Diaz III (1996) document random adjustments of ask prices to influence offering prices as well as final transaction prices in an experimental setting.⁶ A rational agent would simply adjust the optimal choice with respect to the purchase price to account for the heuristic bias. The anchoring-adjusted price function of a rational agent would then simply coincide with the optimal decision, \dot{p}^o . In consequence, anchoring must result in a deviation from the optimal choice, unless the AV is a perfect prediction of the sales price before anchoring.

With anchoring, the final price per unit (and thus the overall price paid for a home) can be written as a linear combination of the per-unit price from Equation (5) and the per-unit AV (see, e.g., Gibbs and Kulish, 2017), given as

$$\dot{p}^{anch} = (1 - \alpha) \left(\frac{W\theta}{(1+\theta)\bar{n}} - \tau\hat{p} \right) + \alpha\hat{p}, \quad (6)$$

in which $0 \leq \alpha \leq 1$ is the equilibrium degree of anchoring. If $\alpha = 0$, \dot{p}^{anch} corresponds to the rational choice, \dot{p}^o , and if $\alpha = 1$, the transaction price corresponds to the AV.

The effect of each channel on the transaction price can be now illustrated with the first derivative with respect to the per-unit AV, \hat{p} ,

$$\frac{\partial \dot{p}^{anch}}{\partial \hat{p}} = -(1 - \alpha)\tau + \alpha, \quad (7)$$

which indicates that the tax channel dominates, i.e., an increasing AV decreases the transaction price, if $\alpha - (1 - \alpha)\tau < 0$, and the anchoring channel dominates, i.e., an increasing AV leads to an increasing sales price, if $\alpha - (1 - \alpha)\tau > 0$.

The order of magnitude of which a channel is dominating should be further influenced by (i) the drivers of the degree of anchoring, i.e., the determinants of α and (ii) the indi-

⁶In the pioneering work of Tversky and Kahneman (1974), subjects are influenced in their judgment by a (seemingly) random wheel of fortune, illustrating the irrationality of the anchoring heuristic.

vidual effective tax rate τ . Accordingly, it should hold that first, the tax channel is most pronounced for units associated with high effective tax rates. Second, anchoring should be most dominant when AVs are most salient and up-to-date, i.e., right after publication. Both of these hypotheses are tested in Section 5.2.

2.3 Measuring the impact of AVs on transaction prices

So far, the model illustrated that whether a sales price is positively or negatively influenced by the AV depends on two factors, first, whether tax or anchoring channel dominates and, second, whether the AV is relatively high or low. Empirically, the latter factor results in an issue that needs to be addressed, since individual homes within a treated municipality are affected heterogeneously. Intuitively, while over- and undervalued homes should be affected in opposite directions regardless which channel dominates, the aggregate effect on prices at the treatment level might well be zero. In consequence, DiD regressions investigating changes in nominal prices are uninformative for the causal effect of AVs on transaction prices.⁷

I solve this issue by investigating the absolute distance between AV and sales price P , given as

$$DA(n) = |P(n) - AV(n)| = n \left| \hat{p}^{anch} - \hat{p} \right|. \quad (8)$$

Importantly, the true AV (or the true per-unit price, \hat{p}) that is subtracted is known after publication, but not before. Thus, when later constructing the dataset, the AV that is matched to the sales price is always the one published at the tentative roll date closest to the transaction date.

In the empirical analysis, I make use of a standardized version of DA , as further outlined in Section 3. For simplicity, I illustrate the effects from anchoring and tax channel on the absolute measure DA first. It is then straightforward to show that the same advantages of DA also hold for the relative measure.

In the remainder of this section, I show that DA should change in opposing directions for anchoring and tax channel, respectively. That is, relative to the control group, DA should increase for all units in treated municipalities if the tax channel dominates, and decrease for all units in treated municipalities if the anchoring channel dominates. For the sake of clarity, the following two sections separately investigate each channel's effect on DA .

⁷Another approach would be to investigate subsamples of over- and undervalued homes separately. Following this approach, however, requires knowledge of additional factors that are difficult to identify, such as which homes are actually under- or overvalued and the expectations of market participants. Additionally, due to the associated selection process, the construction of a suitable control group would be rather difficult.

2.3.1 Anchoring channel

In this section, I show that the anchoring channel reduces the absolute distance between sales price and AV, DA . Intuitively, this is the case as anchoring moves the transaction price towards the AV, reducing the distance between both quantities, DA . This intuition is formally outlined below.

For the sake of simplification, define $\dot{p}^* := \frac{W\theta}{(1+\theta)\bar{n}}$. Then, the change in the absolute sales-price-AV difference from pre- to post AV publication, given that only the anchoring channel is present, can be described with

$$DA^{pre}(n) - DA^{post}(n) = n|\dot{p}^* - \hat{p}| - n|(1 - \alpha)\dot{p}^* + \alpha\hat{p} - \hat{p}| = \alpha n|\dot{p} - \hat{p}|. \quad (9)$$

Note that anchoring is not possible pre-publication since \hat{p} is not yet known. From Equation (9), it holds that $DA^{pre} \geq DA^{post}$, as $n > 0$ as well as $\alpha \geq 0$. Notably, anchoring strictly reduces DA for all homes, whether over- or undervalued, given that \hat{p} does not perfectly predict \dot{p}^* .

2.3.2 Tax channel

The tax channel, in contrast, generally implies an opposite, increasing effect on DA . Intuitively, an unexpectedly higher AV leads to a lower sales price, such that the distance between both quantities, DA increases. In the opposite case, an unexpectedly lower AV increases the transaction price, again increasing DA . The formal conditions under which this intuition holds are derived in the following.

As future tax payments should play a role for homeowners in any case, I assume that they form expectations about their future tax burden, $n\tau E[\hat{p}]$. Hence, before updated AVs are published, households purchase their home under the expected future AV, and replace expectations with the true value post-publication. Thus, when considering the tax channel isolated from anchoring, it holds that

$$DA^{pre}(n) - DA^{post}(n) = n|\dot{p}^* - \tau E[\hat{p}] - \hat{p}| - n|\dot{p}^* - \tau\hat{p} - \hat{p}|. \quad (10)$$

In Appendix B.1, I show that $DA^{pre}(n) \leq DA^{post}(n)$, i.e., that the tax channel has an increasing effect on DA relative to the pre-publication value, if one of the sufficient conditions

$$\hat{p} \leq E[\hat{p}] \leq \dot{p}^{pre} + \frac{|\epsilon|}{2} \quad (11)$$

$$\hat{p} \geq E[\hat{p}] \geq \dot{p}^{pre} - \frac{|\epsilon|}{2} \quad (12)$$

holds, in which ϵ is the valuation error, i.e., the difference between $\dot{p}^{pre} = \dot{p}^* - \tau E[\hat{p}]$ and $\dot{p}^{post} = \dot{p}^* - \tau \hat{p}$. This shows that when the household’s expectations about the AV, $E[\hat{p}]$, lie between the actual AV and the pre-publication price, the tax channel has an increasing effect on DA , regardless whether a home is over- or undervalued. Furthermore, as DA as well as the change in DA for treated and control group, respectively, are observable, it is not necessary to know the expectations of households.

3 Methodology

The goal of this paper is to analyze the effects of value-based property taxation on transaction prices. To be able to causally interpret the results, I run DiD regressions that compare pre- and post-publication transactions of homes in municipalities publishing updated and previously unknown AVs, with homes in municipalities that did not reassess homes, thus publishing the already known AVs from the previous year.

As my dataset contains multiple years of data for a large amount of jurisdictions, I define municipality-tax-year clusters c by employing symmetric time-windows of ± 180 days around each publication date T_c .⁸ I do not consider longer time-spans to avoid overlaps between treated and control clusters.

In the previous section, I proposed a simple, absolute measure, DA , that is changing homogeneously for under- and overvalued homes and that is moving in opposing directions for tax and anchoring channel, respectively. In the empirical application, I use a standardized version of DA , to prevent that higher-priced homes drive the regression results. Leaving the theoretical framework from Section 2, I calculate for each home i , transacted within municipality-tax-year cluster c , the absolute ratio between P_{ic} and AV_{ic} as

$$DAR_{ic} = \left| \frac{DA_{ic}}{AV_{ic}} \right| = \frac{|P_{ic} - AV_{ic}|}{AV_{ic}}. \quad (13)$$

The conditions derived for DA_{ic} in theory Section 2 hold for DAR_{ic} as well, as AV_{ic} is strictly larger than zero and is fixed for each home i within a given tax year. That is, post-publication, the tax channel should lead to an increase in DAR_{ic} for treated municipalities, while the anchoring channel should lead to a decrease in DAR_{ic} for treated units. Intuitively, after AVs are published, the anchoring channel moves prices towards these values, reducing the sales-price-AV distance, whereas the tax channel drives prices away from AVs, increasing the distance between both quantities. Note that the AV that is matched to the sales price

⁸The results are robust for alternative time-windows of 90, 120, and 150 days, respectively, as shown in Table 5.

is always the one published at T_c . Thus, the empirical analysis compares the relationship of sales prices to assessed values, which stem from the same assessment year within a cluster c . Under the condition that the control group follows the common trend assumption, the treatment effect on transaction prices thus stems from the knowledge about the true AV.

It is necessary to standardize by AV_{ic} rather than P_{ic} in Equation (13), as AV_{ic} remains constant before and after T by definition, whereas the transaction price should be, as illustrated above, affected by anchoring and tax considerations. Consequently, dividing DA_{ic} by the transaction price, P_{ic} , instead of AV_{ic} would yield an unstable and endogenous measure. It is important to note that investigating the absolute value of a relative measure makes the implicit assumption that an overvaluation of 50% can be treated equally to an undervaluation of the same amount. While this assumption should be reasonable for most ratios, it becomes less plausible for larger deviations, e.g., an overvaluation of 99% should be generally less extreme than an undervaluation of 99% due to the natural lower bound of -100%. Therefore, in the robustness section, I first show that my results quantitatively hold when investigating the non-standardized measure DA_{ic} , and second, that they are robust to setting a conservative upper bound for DAR_{ic} .

I define a dummy variable $Treat_{ic}$ that equals one, if home i is sold within a municipality-tax-year cluster c in which homes have been reassessed collectively, i.e., a new AV is available for all respective homes, and zero otherwise. I continue on following the standard DiD framework by defining a dummy variable $Post_{itc}$ that equals one if home i was sold after T_c , and zero otherwise. Consequently, I run regressions of the form

$$DAR_{itc} = \beta_0 + \beta_1 Treat_{ic} + \beta_2 Post_{itc} + \gamma Treat_{ic} \times Post_{itc} + \delta_c + \nu_t + \epsilon_{itc}, \quad (14)$$

in which DAR_{itc} , as defined in Equation (13), is the absolute ratio between P_i and AV_i , β_0 is an intercept, δ_c and ν_t denote location-tax-year (e.g, zip-code-tax-year) and temporal (e.g., year-quarter) fixed effects, respectively, $Treat_{ic} \times Post_{itc}$ is the interaction between the dummies identifying treatment and post groups, respectively, and ϵ_{itc} is a nuisance term. The coefficient of interest γ measures the treatment effect on DAR_{itc} from the updated AVs relative to the control group. As illustrated in Section 2, if γ is positive, the tax channel is dominating. In contrast, the anchoring channel is dominating if γ is negative. Before presenting the empirical results, I introduce the data used and discuss the identification in the following section.

4 Data and identification

The first part of this section briefly describes the data cleaning process, how the datasets are merged, and how treated and non-treated units are identified. After illustrating the cleaned dataset, I discuss the validity of my identification strategy.

4.1 Data cleaning

I merge property transactions of single-family homes with historical tax records from 2007 to 2017 that contain assessments for up to eleven years per property. Both datasets are obtained from the data vendor *CoreLogic*, who provides a US property record coverage of more than 99%.⁹ I focus on New York State properties for several reasons. First, the municipality-dependent publication dates T_c are easily accessible through the municipal profile webpage provided by the state government.¹⁰ Second, the revaluation frequencies across municipalities differ, allowing for the construction of a control group. Third, AVs are published annually, providing a distinct point at which the new information is available. Fourth, the state includes the largest US city as well as more rural areas, strengthening the external validity of the results.

I start out by cleaning the transaction dataset by following DeFusco et al. (2020) in dismissing all observations that are not classified as “arms length”, have a missing sales price, are associated with a foreclosure, or ones identified as duplicate.¹¹ Additionally, I remove all transactions without an assessor’s parcel number (APN), which is used to merge the transactions with the tax records. Afterwards, I follow Bollerslev et al. (2016) and set sharp nominal bounds for the transaction prices. Based on municipality and sale date, I identify for each of the remaining homes the corresponding municipality-tax-year cluster c . Based on this classification, I then match the corresponding AV_{ic} to the respective home i .

In addition to the contemporaneous AV that is used to construct DAR_{ic} , I match the one-year AV lag for two reasons. First, to filter out observations with unusually large valuation changes, e.g., induced by substantial renovations. Second, to employ a data-driven way to identify treatment and control clusters c . I do this by investigating whether at least 75% of the remaining observations have the same one-year AV return. To rule out that small deviations confound the classification, I round the returns to the third digit before doing so. If at least 75% of observations in a municipality-tax-year cluster c have a one-year AV return of zero, I assign all observations in c control unit status and dismiss all observations

⁹As reported by the data provider at <https://www.corelogic.com/solutions/university-data-portal.aspx>, last retrieved on August 25, 2020.

¹⁰To be found at <http://orps1.orpts.ny.gov/cfapps/MuniPro/>, last retrieved on August 25, 2020.

¹¹A more detailed description of the data cleaning procedure is provided in Appendix C.

in this c with a return different from zero. If at least 75% of returns within a particular c have the same return, but the return is different from zero, e.g., because the local assessor market-adjusted AVs in the given year, I dismiss all observations in this cluster c , as they can neither be considered treated nor control units. The remaining observations are then assigned to the treatment group.

After filtering out extreme values of DAR_{ic} and DA_{ic} based on sample quantiles, respectively, I conduct a final step by dismissing clusters c with only few observations (less than 100), as treated units appear to be more often located in larger municipalities, such that comparability of treatment and control group is increased. The final dataset consists of 193,494 observations.

Table 1 provides summary statistics of key variables used in the later analyses. Panel A reports statistics for the treated units, Panel B for units sold within control clusters. From the total 193,494 observations, about 59% are treated units. For them, the average sales price is about 383,000 USD, while the mean AV is slightly lower with about 381,000 USD. In contrast, the control group contains units that tend to be comparably lower priced with an average sales price of 255,000 USD. For both groups, a larger share of transactions are realized after the publication date, with 55% for the treated and 59% for the control units. A potential reason is that most municipalities publish AVs at the beginning of the year and in spring, and turnover is typically highest in summer (e.g., Ngai and Tenreyro, 2014). For the treatment group, the average increase of the AV is 1%, reflecting the general increase of house prices over the sample period, but also that the sample includes the post-bust period from 2007 to 2010. With a standard deviation of about 9%, substantial changes in the AV from one year to another appear to be rather common. The effective tax rate is on average higher for the control group. One reason for this observation could be the necessity to increase tax rates in lower-priced areas, to generate sufficient governmental income.

Figure 1 illustrates the geographic distribution of transactions within the state of New York, separated by treatment status. Panel A shows transactions for treated units, and Panel B for control ones. Unsurprisingly, properties in New York City, found in the southeast of the state, are all treated units, as here, homes are reassessed annually. Other cities, such as Buffalo, located in the western end of the state, reassess less frequently, which is why homes from Buffalo can be found in both groups. Note that as at least hundred observations are required per municipality-tax-year cluster to increase comparability between treated and control sample, there are fewer observations found in rural areas. The comparison of Panels A and B illustrates why the control group should match the treated units well: there are many overlaps among cities and towns, such that the AVs of both groups should be similarly precise and distributed, as the local assessors should remain more or less constant over time.

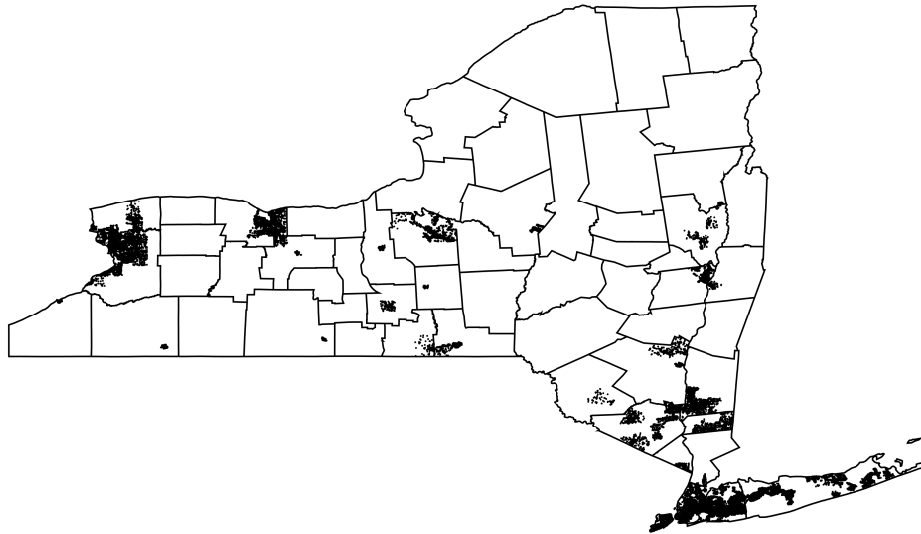
Table 1
Summary statistics

| <i>Panel A: Treated units</i> | | | | | | |
|-------------------------------|---------|---------|---------|---------|---------|---------|
| | Obs. | Mean | Std. | 1st Q. | Median | 3rd Q. |
| Sales price | 113,701 | 382,521 | 307,676 | 184,000 | 342,000 | 485,000 |
| Assessed value (AV) | 113,701 | 380,698 | 295,568 | 191,863 | 346,000 | 485,000 |
| Abs. price-AV ratio (DAR) | 113,701 | 0.174 | 0.158 | 0.059 | 0.129 | 0.240 |
| Abs. price-AV diff. (DA) | 113,701 | 61,688 | 71,564 | 15,000 | 37,500 | 81,209 |
| One-year return AV | 113,701 | 0.010 | 0.087 | -0.038 | 0 | 0.046 |
| Post | 113,701 | 0.548 | 0.498 | 0 | 1 | 1 |
| Crisis (2007-2010) | 113,701 | 0.226 | 0.418 | 0 | 0 | 0 |
| Effective tax rate seller | 93,903 | 0.017 | 0.011 | 0.008 | 0.014 | 0.025 |
| Effective tax rate buyer | 88,671 | 0.019 | 0.011 | 0.010 | 0.016 | 0.027 |
| <i>Panel B: Control units</i> | | | | | | |
| | Obs. | Mean | Std. | 1st Q. | Median | 3rd Q. |
| Sales price | 79,793 | 255,205 | 206,511 | 121,000 | 203,000 | 333,900 |
| Assessed value (AV) | 79,793 | 247,902 | 193,296 | 118,600 | 207,000 | 325,000 |
| Abs. price-AV ratio (DAR) | 79,793 | 0.174 | 0.162 | 0.059 | 0.125 | 0.238 |
| Abs. price-AV diff. (DA) | 79,793 | 40,613 | 48,977 | 10,000 | 23,000 | 51,545 |
| One-year return AV | 79,793 | 0 | 0 | 0 | 0 | 0 |
| Post | 79,793 | 0.594 | 0.491 | 0 | 1 | 1 |
| Crisis (2007-2010) | 79,793 | 0.231 | 0.421 | 0 | 0 | 0 |
| Effective tax rate seller | 72,236 | 0.026 | 0.010 | 0.021 | 0.027 | 0.032 |
| Effective tax rate buyer | 58,253 | 0.028 | 0.010 | 0.023 | 0.029 | 0.034 |

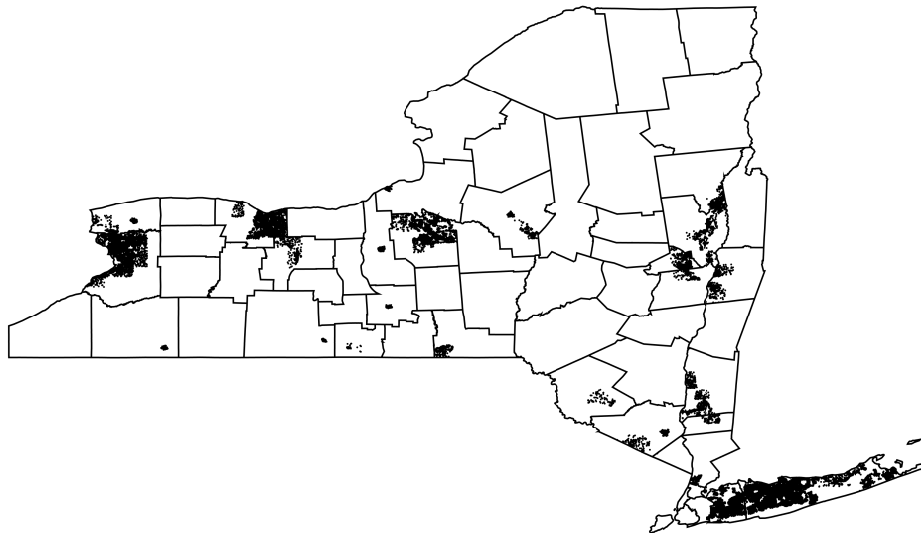
This table provides summary statistics for the variables used in the empirical application. Panel A shows summary statistics for the homes located in treated municipality-tax-year-clusters c (i.e., municipalities in which homes have been reassessed in a given year surrounding the respective AV publication date). Panel B shows the same statistics for the control units, i.e., sales of homes within municipality-tax-year clusters that did not reassess homes. “Sales price” denotes the nominal transaction price, and the “Assessed value (AV)” is the publicly available estimated market value provided by the local assessor, which is known if the corresponding unit was sold before the publication date T_c , and known afterwards. “Abs. price-AV ratio” is the absolute value of the ratio between nominal sales price and AV (DAR). “Abs. price-AV diff.” is the absolute value of the difference between nominal sales price and AV (DA). “One-year return AV” is the relative change in AV with respect to the prior year. “Post” is a dummy indicating whether a property was sold before or after the publication of AVs. “Crisis” is a dummy that equals one if the property value was published between 2007 and 2010, and zero otherwise. The “Effective tax rates” are defined as tax amount paid within a particular year, divided by a given AV. For the effective tax rate of the seller (buyer), the tax amount in the year prior to (after) the respective tax year is used. The AV used to derive the seller’s (buyer’s) tax rate is the one from the prior (contemporaneous) tax year.

Figure 1 Geographic dispersion of transactions

Panel A: Treated units



Panel B: Control units



Panels A and B show the geographic dispersion of observations in the cleaned dataset within New York State. Note that observations from smaller areas are excluded to increase comparability between treatment and control group, as smaller municipalities tend to reassess less often than larger municipalities. Panel A shows the distribution of observations assigned to the treatment group, i.e., sales that took place in a time window of ± 180 days around the publication of updated AVs. Panel B shows the geographic distribution of homes in the control group, i.e., ones that were transacted ± 180 days around AV publication in tax-years in which the AVs were not updated (i.e., coincide with last year's AV). The solid lines indicate county and state borders. County and state border data is provided by the US Census Bureau.

4.2 Identification

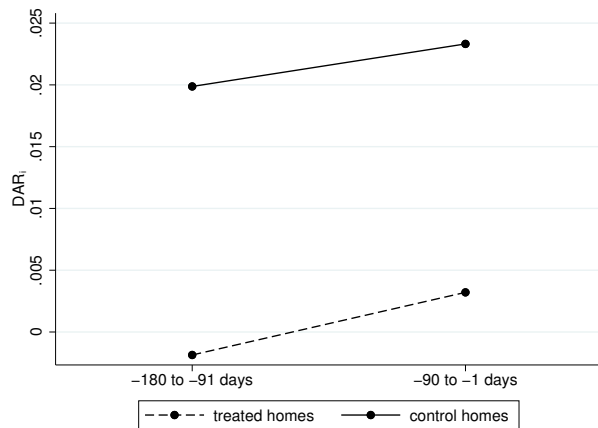
The key underlying assumption of the DiD analysis is that the trend of the dependent variable DAR_{ic} would have been the same for treated and control group in the absence of treatment, i.e., the one of a common trend. If this assumption holds, the control units can be used to infer about the counterfactual outcome of the treated units. In other words, the homes in the control group inform about how the dependent variable DAR_{ic} of the treated homes would have developed if AVs had not been updated.

To check the validity of this assumption, I plot pre-publication trends of the dependent variable DAR_{ic} for both groups in Panels A and B of Figure 2. Each panel shows conditional means of DAR_{ic} for two periods prior to publication, one for 180 to 91 days, and one for 90 to 1 day before publication of AVs. Panel A shows trends conditional on the base case controls zip-code-tax-year and year-quarter fixed effect dummies for both groups, respectively. DAR_{ic} is upward sloping for both groups at a highly similar magnitude. Likewise, Panel B shows the trends of DAR_{ic} for both groups when conditioning on separate sets of municipality and tax-year level fixed effect dummies, respectively. Even under the much simpler controls, both means are upward sloping at a similar magnitude, indicating that the common trend assumption holds reasonably well, and homes in control municipalities can thus be used to infer about the counterfactual outcome for the treated homes. Yet, the comparison of Panels A and B indicates that more fine-grained controls are helpful in correcting for unobserved heterogeneity over locations and time. Note that the absolute level of each point is uninformative as the conditional means displayed are estimated relative to a base category. This does not constitute a problem, as only the trend needs to be common in DiD regressions. Additional pre-publication trends for subsamples are presented in Section 5.2, e.g., for samples based on different effective tax rates. Several (placebo) tests on differences in pre-treatment trends are presented in Section 6.

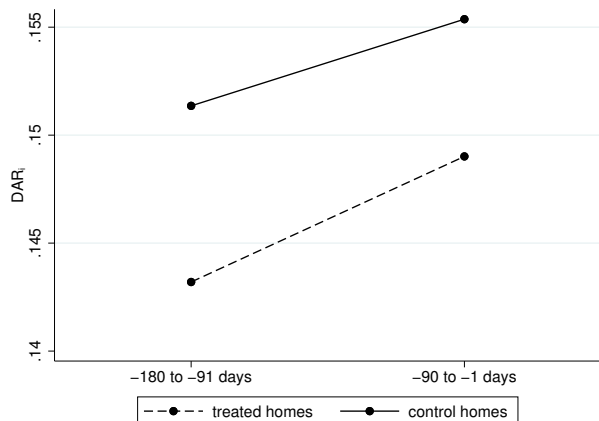
A further potential issue is that homeowners wait for publication until they sell their homes. Figure 3 shows the frequency of transactions relative to AV publication for both treated (Panel A) and non-treated units (Panel B). For the treatment group, no change in the turnover trend is visible around day zero, indicating no immediate effect of AV publication on liquidity. With increasing amount of days after the publication date, however, there is an increase in observation frequency. Similarly, an increase in observations is observed for the control group, as shown in Panel B. For the control units, the increase is even higher, suggesting that AVs do not play a role when considering the timing of a sale. A potential reason for the observed increase over time for both groups is likely to be that AVs are mostly published in spring, and turnover is typically highest in summer. Additionally, waiting for AV updates should not be a concern in the housing market, as it is costly due to factors such

Figure 2
Trends of DAR_{ic} before treatment

Panel A: Base case controls



Panel B: Simple controls



This figure shows the pre-publication development of the average absolute sales-price-AV ratio, DAR , defined in Equation (13), for both treated (dashed line) and control units (solid line). The points displayed indicate means for two subperiods of 90 days each. Panel A depicts trends conditional on zip-code-tax-year and year-quarter fixed effect dummies, respectively. The means in Panel B are conditional on separate sets of municipality as well as tax-year fixed effect dummies, respectively.

as interest payments and opportunity costs.

Another concern that needs to be discussed is that homeowners are able to challenge their assessment once they received notice of their updated AV. To investigate how frequently AVs are changed, I utilize data from the New York City government, which provides such information for the year of 2016.¹² Investigating the subsample for single-family homes, I find that only about 6,000 AVs have been updated after the tentative roll date. This accounts for only about 0.9% of the about 697,000 homes that are listed in the following year’s tax records in the *CoreLogic* dataset, suggesting that the possibility to contest public estimates is affecting only a small share of observations and should therefore be of minor relevance.

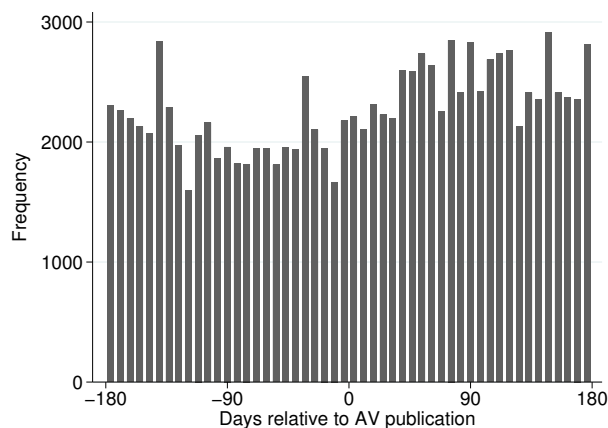
Given that successful contests of tentative AVs are rather rare, the estimates published at T_c can be viewed as quasi-fixed, thus mitigating further endogeneity concerns. Taking further into account that the estimates, derived by using sales prices of comparable properties, are not meant to be forecasts but have to reflect home values at a particular date in the past (the “valuation date”) should further support the causal interpretation of the results.

Finally, spillovers across municipalities should not be a concern, as homes are individually affected, depending on whether the updated AV is relatively high or low. That is, within a municipality, some homes can be positively, and some negatively affected. As this implies

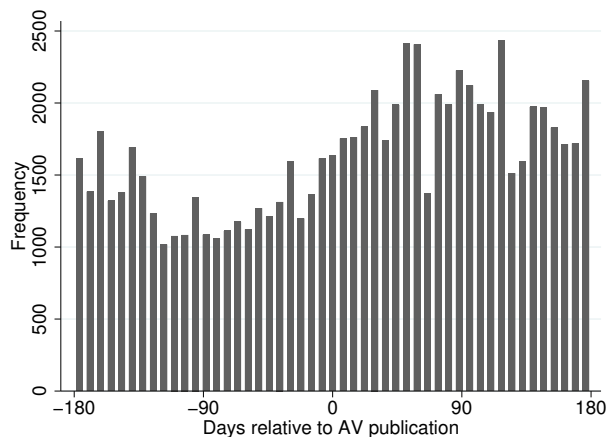
¹²Available at <https://data.cityofnewyork.us/City-Government/Revised-Notice-of-Property-Value-RNOPV-/8vgb-zm6e>, last retrieved on August 17, 2020.

Figure 3
Amount of sales relative to the event date

Panel A: Treated units



Panel B: Control units



The histograms displayed in Panels A and B show the number of observations in the dataset relative to AV publication in a time window of ± 180 days. Panel A shows the frequency for treated units, i.e., units for which the AV is revalued and is known by market participants only after day zero (T_c). Panel B shows the same variable for control units, i.e., ones for which the (old and new) AV is not revalued and thus known before and after day zero.

no shift in the aggregate price level into a particular direction within a municipality-tax-year cluster, no spillover effects across units due to AV publication should be expected. Having discussed the validity of the identification strategy, the following section presents the empirical results.

5 Results

The aim of this work is to show the impact of AVs on sales prices. To identify this effect, I investigate DAR_{ic} , the absolute ratio between the sales price of a home i and the corresponding AV, and exploit the timing of publication at time T_c in a time window of ± 180 days. The sign of this effect is ex-ante not clear. As illustrated in Section 2, the tax channel should lead to an increase in DAR_{ic} through an induced change in tax payments. In contrast, anchoring should reduce DAR_{ic} . Using municipality-tax-year clusters in which homes are not reassessed as a control group, I run DiD regressions following Equation (14).

5.1 Base case

Table 2 shows results for OLS regressions with DAR_{ic} , the absolute sales-price-AV ratio, as dependent variable. The causal effect of AVs on transaction prices that is measured by DAR_{ic}

is given by the interaction between dummy variables “Treatment” and “Post”. Standard errors are clustered over counties and are shown in parentheses below.¹³ Column (1) shows estimates when only municipality fixed effects are included. The effect of the interaction between treatment and post-publication dummy is estimated at 1.1%. The coefficient is statistically significant at the 0.1% level and indicates that knowledge about new AVs is influencing DAR_{ic} positively, in line with the tax channel. That is, an increased assessed value decreases the transaction price of the respective home.

Column (2) includes municipality-tax-year fixed effects, thus additionally accounting for temporal variation within local districts. The result is significant at the 0.1% level and suggests a positive causal effect of AVs on DAR_{ic} of 1.1 percentage points. Using the more fine-grained zip-code-tax-year fixed effects and additionally year-quarter fixed effects as shown in columns (3) and (4), respectively, slightly reduces the base case estimate to one percentage point. The common pre-publication trend when using the base case specification in column (4) is shown in Panel A of Figure 2. Together, the positive estimates indicate that the tax channel is dominating and increasing the absolute sales-price-AV ratio by about one percentage point.

To provide an economic interpretation of these results, I do a back-of-the-envelope calculation making use of the relationship

$$\Delta P_{ic} = \frac{\gamma AV_{ic}}{P_{ic}^c}, \quad (15)$$

in which P_{ic}^c is the price under control conditions, and ΔP_{ic} the relative change in the sales price between treatment and control state. The coefficient γ is the treatment effect, which is estimated to be 1% in the base case. The simplifying assumption that allows this closed-form solution is that both transaction prices, under treated and control conditions, are larger than the AV, as shown in Appendix B.2.¹⁴

Applying Equation (15) to the sample means of the treatment group, it is possible to derive an effect on transaction prices. Plugging-in the average AV of the treatment group (about 383,000 USD), and using the average difference between sales price and the AV of 62,000 USD (which must be added to the sales price such that the underlying assumption is fulfilled), the change in P due to treatment is 0.9%. This indicates that value-based

¹³An inspection showed that the base case results are robust to alternative clustering of standard errors, such as on municipality or zip-code level, as well as Cameron et al. (2011) two-way clustering with county and tax year.

¹⁴This assumption is in line with the sufficient condition (12) from Section 2, which implies an increase in the sales price post-publication as well. Doing the same calculations, but assuming instead that the transaction prices are smaller than the AV, a decline in the transaction price is implied. Again, this decline is in line with the corresponding condition (11).

Table 2
Base case results

| | (1) | (2) | (3) | (4) |
|-------------------------------------|---------------------|---------------------|---------------------|----------------------|
| Treatment | -0.009** (0.003) | | | -0.024*** (0.006) |
| Post | 0.005** (0.001) | 0.005*** (0.001) | -0.003 (0.002) | -0.005 (0.003) |
| Treatment \times Post | 0.011*** (0.002) | 0.011*** (0.002) | 0.010*** (0.002) | 0.010*** (0.002) |
| Municipality fixed effects | X | - | - | - |
| Municipality tax-year fixed effects | - | X | - | - |
| Zip-code-tax-year fixed effects | - | - | X | X |
| Year-quarter fixed effects | - | - | - | X |
| Adj. R-sq. | 0.054 | 0.070 | 0.070 | 0.097 |
| Observations | 193,494 | 193,494 | 193,494 | 193,494 |

This table provides coefficient estimates on four separate Difference-in-Differences regressions based on Equation (14), with DAR_{ic} , the absolute sales-price-AV ratio, as dependent variable. “Treatment” is a dummy that equals one if the unit was sold within a municipality-tax-year cluster in which AVs were updated, and zero otherwise. “Post” is a dummy that equals one if the unit was sold after the AV was known, and zero otherwise. “ \times ” denotes an interaction between two variables. “X” indicates that the set of control variables is used, whereas “-” indicates that the set remained unused for the particular model. Standard errors are clustered over counties. *, **, and *** indicate significance on the 5%, 1%, and 0.1% level, respectively.

property taxation leads to economically significant price distortions of about one percent of the non-treatment sales price.

5.2 Evidence for the underlying channels

The results presented in the prior section indicate that the tax channel is dominating. The purpose of this section is to first, present supporting evidence that this is indeed the case, and second, to investigate the interplay between tax and anchoring channel in more detail.

If the tax channel is indeed in play, transactions that involve units associated with higher effective tax rates should be affected stronger, as illustrated in Equation (7). To test this prediction, I analyze subsamples selected on the basis of the effective tax rates of buyers and sellers, which can differ for several reasons. First, property tax rates change over time. Second, different households can have different exemptions on their payments (e.g., veterans or senior citizens). Third, the annual increase in payments might be capped.

I define the effective tax rate as tax payments made during a particular year, divided by a given AV. The ratio therefore indicates how many cents the homeowner has to pay in

taxes per dollar AV. As it remains unclear in the data whether buyer or seller paid the tax amount in the year of the underlying transaction, I make use of the tax payments made for a given home in the years before or after the respective AV was published, respectively. Accordingly, the seller's effective tax rate is defined as the prior year's tax payments divided by the one-year AV lag. For the buyer, I use the forward lag of tax payments, but divide by the current AV to avoid the additional loss of a larger amount of data. As this is done for all observations in the sample collectively, and as most municipalities do not reassess on an annual basis, this definition should not significantly affect the results, while simultaneously preserving the size of the dataset.

Based on the sample medians of buyer and seller effective tax rates, respectively, I define a high and a low tax subsample. As a high tax paying individual might sell to a low tax buyer (i.e., with some tax exemptions), I make use of a combined measure that indicates whether a property is associated with a higher or lower tax rate. Accordingly, the high tax sample includes observations for which both buyers' and sellers' effective tax rates lie above their respective sample median. I proceed similarly for the low tax subsample, but check whether tax rates are below the same medians, respectively.

Table 3 presents results on DiD regressions for the two subsamples. Importantly, the treatment dummy cannot be displayed as it is absorbed in the fixed effects. The corresponding common trends are shown in Panels A and B of Figure 4, indicating that the common trend assumption holds well even for different subsamples. The first two columns of Table 3 show results when separating sellers by high and low tax rates. The table shows that the estimated treatment effect is at about 1.4% for the high tax sample and 40% lower for the low tax subsample. The coefficient for the low tax is only borderline significant, further indicating that the low tax group is less affected through the tax channel.

Having underpinned the tax channel as driving force of the impact of AVs on transaction prices, I further analyze how this impact varies across states of the housing market cycle. The dataset used for AVs starts in 2007, right when the housing bubble began to burst. This raises the question whether the subsequent years of turmoil that followed affected the impact of AVs on trading prices as well. For instance, when prices are more volatile, an anchor such as the AV could be more relevant for market participants, as it remains unclear where prices are heading. In contrast, the volatility of AVs itself should increase and therefore be perceived as less trustworthy for buyers and sellers, thus lowering their relevance for future tax payments. In contrast, when the economy is struggling and marginal consumption of households is high, saving taxes could become a more important issue. Taking these considerations together, it remains unclear how the situation of the economy and the financial system in the sample years 2007 to 2010 influence the effect of AVs on trading prices.

Table 3
Treatment effect by effective property tax rates

| | High tax | Low tax |
|---------------------------------|---------------------|--------------------|
| Post | 0.000 (0.002) | -0.008* (0.003) |
| Treatment \times Post | 0.014*** (0.003) | 0.010* (0.004) |
| Zip-code-tax-year fixed effects | X | X |
| Year-quarter fixed effects | X | X |
| Adj. R-sq. | 0.101 | 0.119 |
| Observations | 48,328 | 59,939 |

This table provides coefficient estimates on four separate Difference-in-Differences regressions based on Equation (14), with DAR_{ic} , the absolute sales-price-AV ratio, as dependent variable. The subsamples are selected according to the effective tax rates of the buyer and seller, respectively. The buyer (seller) effective tax rate is derived as tax amount paid in the year after (before) the underlying transaction, divided by corresponding AV of the previous (contemporaneous) tax year, respectively. The “High tax” (“Low tax”) subsample is based on observations with effective tax rates of both buyer and seller above (below) the respective sample medians. “Treatment” is a dummy that equals one if the unit was sold within a municipality-tax-year cluster in which AVs were updated, and zero otherwise. Note that the non-interacting “Treatment” dummy is absorbed by the fixed effects. “Post” is a dummy that equals one if the unit was sold after the AV was known, and zero otherwise. “ \times ” denotes an interaction between two variables. “X” indicates that the set of control variables is used. Standard errors are clustered over counties. *, **, and *** indicate significance on the 5%, 1%, and 0.1% level, respectively.

Table 4 shows results for two separate DiD regressions based on subsamples related to the year in which the respective AVs were published. The corresponding common trends are shown in Panels C and D of Figure 4. The graphs show that for both subperiods, the pre-publication trends of treatment and control group are remarkably similar, indicating that the common trend assumption holds well even for different states of the housing market cycle.

The first column of Table 4, depicting results for the 2007-2010 crisis subsample, shows that the coefficient estimate is very close to zero and insignificant. This could imply that with volatile prices, homeowners value the AV as an anchor, outweighing tax considerations. In contrast, the coefficient for the 2010-2017 sample is positive and at about 1.3%, slightly larger than for the base case scenario. Hence, the results indicate that in upward trending periods, market participants emphasize on tax considerations.

To investigate post-publication dynamics of anchoring and tax channel, respectively, I study subsamples according different time windows around the publication date. Intuitively, if anchoring is present, it should be strongest right after homeowners and other market participants become aware of the updated AVs. Over time, AVs become more and more

Table 4
Treatment effect by subperiods

| | Crisis (2007-2010) | 2011-2017 |
|---------------------------------|--------------------|---------------------|
| Post | -0.005 (0.005) | -0.004 (0.002) |
| Treatment \times Post | -0.003 (0.003) | 0.013*** (0.002) |
| Zip-code-tax-year fixed effects | X | X |
| Year-quarter fixed effects | X | X |
| Adj. R-sq. | 0.113 | 0.092 |
| Observations | 44,140 | 149,354 |

This table provides coefficient estimates on three separate Difference-in-Differences regressions based on Equation (14), with DAR_{ic} , the absolute sales-price-AV ratio, as dependent variable. “Treatment” is a dummy that equals one if the unit was sold within a municipality-tax-year cluster in which AVs were updated, and zero otherwise. Note that the non-interacting “Treatment” dummy is absorbed by the fixed effects. “Post” is a dummy that equals one if the unit was sold after the AV was known, and zero otherwise. The column names indicate period according to the publication year on which the regressions are based. “ \times ” denotes an interaction between two variables. “X” indicates that the set of control variables is used. Standard errors are clustered over counties. *, **, and *** indicate significance on the 5%, 1%, and 0.1% level, respectively.

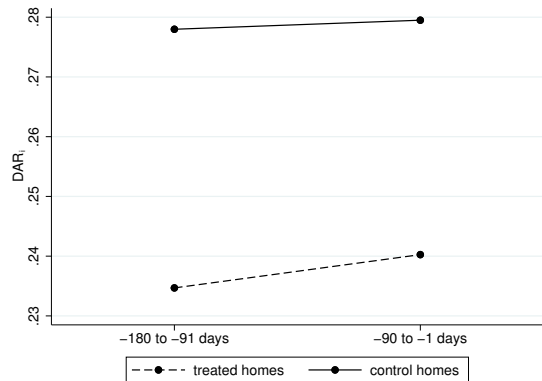
outdated, such that the tax channel should become increasingly dominant.

Table 5 shows results on four DiD regressions based on subsamples of time windows with 60, 90, 120, and 150 days before and after the publication date, respectively. The first column, which analyzes a time window of ± 60 days around T_c shows that there is no significant effect observed immediately after publication, and the coefficient estimate is close to zero. As outlined above, this result could suggest that the anchoring channel offsets the tax channel right after the publication date, at which the AV should be most salient to homeowners.

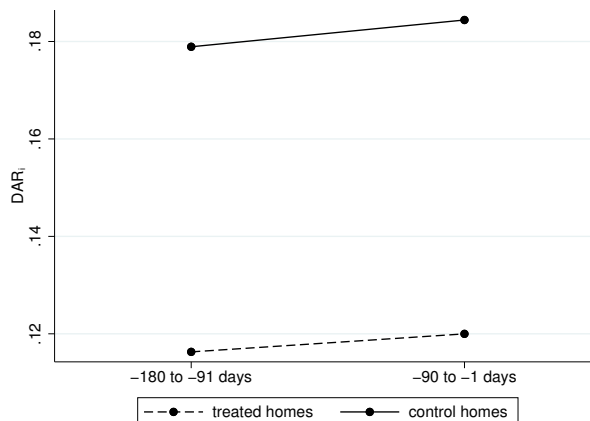
When investigating 90 days around the publication date, the estimate becomes significant at the 1% level and closer to the base case estimate, suggesting that anchoring is reduced when AVs become less salient and more outdated over time. Expanding the time window to 150 days pre- and post-publication, the coefficient estimate is again significant at the 1% as documented in the base case analysis using a time window of 180 days. Table 5 thus indicates that first, the base case results are robust to reducing the time-window by half, and second, it takes time until the information contained in the new AVs is capitalized into transaction prices, potentially due to the offsetting anchoring channel. The base case effect of 1% is almost reached after 150 days. This suggests that after about 150 days, the tax channel reaches its maximum impact, while anchoring is only present when AVs are most

Figure 4
Trends of DAR_{ic} before treatment by subsamples

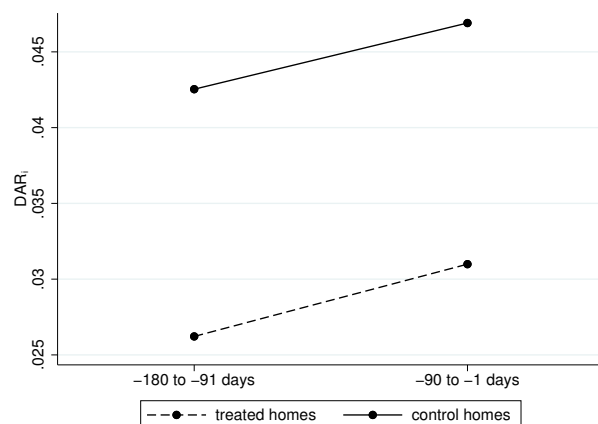
Panel A: High tax sellers



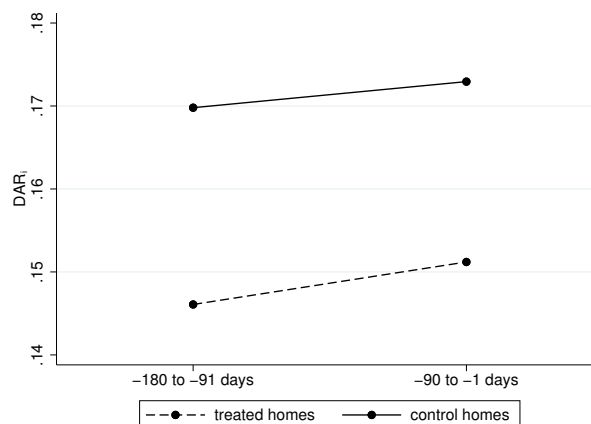
Panel B: Low tax sellers



Panel C: 2007-2010



Panel D: 2011-2017



This figure shows the pre-publication development of the absolute sales-price-AV ratio, DAR_{ic} , defined in Equation (13), for both treated (dashed line) and control units (solid line). Panel A (B) shows pre-publication trends for observations for which the effective tax rates of seller and buyer is both above (below) the respective sample medians. The buyer (seller) effective tax rate is derived as tax amount paid in the year after (before) the underlying transaction, divided by corresponding AV of the previous (contemporaneous) tax year, respectively. Panel C shows pre-publication trends for homes with AVs published during the crisis period of 2007 to 2010. Panel D shows pre-publication trends for the period of recovery from 2011 to 2017.

salient and timely.

6 Robustness

The purpose of this section is to analyze whether the results presented in Section 5 are robust with respect to several choices made throughout the paper. Table 6 summarizes all

Table 5
Results for different time windows

| | 60 days | 90 days | 120 days | 150 days |
|---------------------------------|-------------------|-------------------|--------------------|---------------------|
| Post | -0.000 (0.002) | -0.004 (0.003) | -0.005 (0.003) | -0.005 (0.003) |
| Treatment \times Post | 0.001 (0.002) | 0.007* (0.002) | 0.008** (0.003) | 0.009*** (0.002) |
| Zip-code-tax-year fixed effects | X | X | X | X |
| Year-quarter fixed effects | X | X | X | X |
| Adj. R-sq. | 0.105 | 0.102 | 0.098 | 0.096 |
| Observations | 64,805 | 95,427 | 127,529 | 160,488 |

This table provides coefficient estimates on three separate Difference-in-Differences regressions based on Equation (14), with DAR_{ic} , the absolute sales-price-AV ratio, as dependent variable. “Treatment” is a dummy that equals one if the unit was sold within a municipality-tax-year cluster in which AVs were updated, and zero otherwise. Note that the non-interacting “Treatment” dummy is absorbed by the fixed effects. “Post” is a dummy that equals one if the unit was sold after the AV was known, and zero otherwise. The column names indicate the time window relative to the publication date used for the regression: “60”, “90”, “120”, and “150 days” before and after T_c . “ \times ” denotes an interaction between variables. “X” indicates that the set of control variables is used. Standard errors are clustered over counties. *, **, and *** indicate significance on the 5%, 1%, and 0.1% level, respectively.

robustness checks. In Panel A, results with respect to alternative models and subsamples are presented. For the sake of brevity, only the interaction coefficient is presented. All models use zip-code-tax-year and year-quarter fixed effects, respectively, as in the base case. Panel B provides several placebo tests to check for differences in common trends pre-publication.

About 19% of properties analyzed in the paper are located in New York City (NYC). This might be an issue as first, NYC reassesses homes annually, which is not the case for most of the other municipalities in the sample. Second, NYC is the largest city in the US and thus might behave differently from other, less urbanized regions in the sample. I therefore check whether the base case results hold when excluding NYC properties. As shown in Panel A of Table 6, the coefficient estimate of the interaction term is at 0.8%, similar to the base case. This underpins the external validity of the results: The tax channel remains at a similar order of magnitude, whether properties are located in highly urbanized areas or in comparably rural ones. As NYC and Nassau county impose limits on how much the taxable share of the AV can increase, I check whether my results hold when excluding observations in these locations. The point estimate of 0.7% that is shown in the second row indicates that this appears to be the case.

When investigating only larger municipalities, defined by having at least 1,000 observations per municipality-tax-year cluster, the treatment effect is still highly significant and

positive. The same is true for subsamples divided by the median nominal sales price (about 280,000 USD) in the sample. The treatment effect is slightly higher for above-median sales prices with a coefficient estimate difference of 0.3%. Together, these results document that the tax channel is consistently dominating in different market segments.

As mentioned earlier in the paper, using the absolute value of a relative measure, such as the base case variable DAR , might be associated with some drawbacks, as undervaluations of, e.g., 90% are valued the same as overvaluations of the same magnitude, even though such undervaluations can be considered to be much more extreme. I therefore limit DAR to be less than 50% to see whether my base case results still hold. With a significant coefficient of 0.9%, this appears to be the case. I further investigate the unstandardized measure DA . The coefficient is positive and highly significant as well, suggesting that my results are robust with respect to standardization.

Panel B of Table 6 depicts regression results for three placebo tests using a subsample of pre-publication observations. The placebo date is set to 90 days prior to the actual publication, thus symmetrically dividing the subsample period. The idea is to check for the existence of differences in pre-publication trends between treatment and control units. Models (1)-(3) in Panel B indicate no significant difference between treatment and control group in the dependent variable after the pseudo-publication date. For the base case scenario, shown in column (3), the coefficient estimate is very close to zero, further supporting the assumption of common trends.

7 Conclusion

Property taxes are commonly based on assessed values (AVs) of homes which can only be estimated and thus are prone to valuation errors. This paper analyzes whether AVs themselves affect trading prices of homes. Theoretically, the impact of AVs on transaction prices should be driven by two opposing channels. As AVs constitute salient reference prices for market participants, an anchoring channel indicates a positive causal effect of AVs on transaction prices. At the same time, an unexpected increase in the AV implies a higher future property tax burden associated with the underlying home. Thus, a tax channel should lead to a negative effect of AVs on sales prices.

I analyze this ambiguous relationship with a Difference-in-Differences framework that exploits AV publication dates and variation in reassessment frequencies. The results document a robust negative causal impact of AVs on transaction prices, in line with the tax channel. With an AV-induced change of about 0.9% in the transaction price of the average home, the results of this paper are not only statistically robust, but also of economic significance.

Table 6
Robustness checks

| <i>Panel A: Alternative models and subsamples</i> | | | |
|---|-------------------------|-------------------|------------------|
| | Treatment \times Post | Observations | Adj. R-sq. |
| Without NYC | 0.008*** (0.002) | 156,195 | 0.096 |
| Without NYC & Nassau | 0.007** (0.002) | 123,646 | 0.097 |
| Larger municipalities only | 0.011** (0.003) | 67,212 | 0.091 |
| Below median sales price | 0.008** (0.003) | 95,927 | 0.167 |
| Above median sales price | 0.011*** (0.002) | 97,567 | 0.117 |
| $DAR < 0.5$ | 0.007*** (0.002) | 183,876 | 0.079 |
| DA as dependent variable | 0.085*** (0.013) | 193,494 | 0.302 |
| <i>Panel B: Placebo tests</i> | | | |
| | (1) | (2) | (3) |
| Post (placebo) | 0.005* (0.002) | 0.005* (0.002) | 0.004 (0.003) |
| Treatment \times Post (placebo) | 0.005 (0.003) | 0.004 (0.003) | 0.002 (0.003) |
| Location-tax-year fixed effects | municipality | zip-code | zip-code |
| Year-quarter fixed effects | - | - | X |
| Adj. R-sq. | 0.073 | 0.107 | 0.108 |
| Observations | 83,755 | 83,755 | 83,755 |

This table documents the results of multiple robustness checks. Panel A shows estimates for the interaction term of treatment group and post-AV publication (“Treatment \times Post”) that informs about the causal effect of AV publication on transaction prices, measured by the change in the dependent variable DAR , the absolute value of the sales-price-AV ratio. Each row of Panel A shows results for a separate regression. The specification is indicated in the first column. “Without NYC” indicates that all observations placed in New York City have been left out, and “Without NYC & Nassau” indicates that all observations placed in New York City or Nassau county are excluded, respectively. “ $DAR < 0.5$ ” indicates an upper bound for the dependent variable DAR of fifty percent. For model “Larger municipalities only”, only municipality-tax-year clusters with more than 1,000 observations have been included. “Below (Above) median sales price” investigates subsamples of transactions with sales prices of less than (equal or more than) the median nominal sample sales price (about 280,000 USD). “ DA as dependent variable” indicates that the (unstandardized) absolute distance between sales price and AV was used as dependent variable. For all regressions in Panel A, the base case scenario with zip-code-tax-year and year-quarter fixed effect dummies, respectively, is employed. Each column in Panel B shows a placebo regression with varying control variables. All regressions investigate observations before AV publication. The “Post (placebo)” dummy was defined to split the pre-publication date into two sales with equal time window (90 days each). “X” indicates that the set of control variables is used, whereas “-” indicates that the controls are not used for the specification. Standard errors are clustered over counties. *, **, and *** indicate significance on the 5%, 1%, and 0.1% level, respectively.

Supporting the tax channel as driver of my findings, homes associated with high tax rates are affected stronger than homes related to lower tax rates. The results documented in this work are robust with respect to different subsamples and fixed effects. The findings of this work have implications for the redistribution of wealth through the property tax system: Imperfect valuation does not only lead to inequitable tax payments, but also adversely distorts transaction prices of homes.

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A The property tax system in New York State

This section gives an extended overview of the New York State property tax system. Figure A1 illustrates the sequence of important dates according to the official tax calendar that is followed by all municipalities in the state.¹⁵

In each jurisdiction, AVs are published annually at the “tentative roll date”, T , as shown in the center of Figure A1. Beginning at T , the new information contained in the updated AVs cannot only be used by homeowners, but also by other market participants, as property assessments are publicly available. The most common tentative roll date is May 1, but other dates are used as well, such as January 15 in New York City. Although AVs are published every year, homes are not necessarily reassessed at the same frequency. The frequencies are decided by municipal governments and vary substantially. For instance, homes in New York City are reassessed annually, while some smaller municipalities, such as Westerlo, did not reassess since 1974.¹⁶ In some years, AVs either remain unchanged or are collectively adjusted by the same percentage, e.g., to correct for inflation. As discussed in Section 4.1, I exclude municipality tax-year clusters in which AVs have been collectively adjusted by the same percentage, as the corresponding observations can be neither assigned to treatment nor control group.

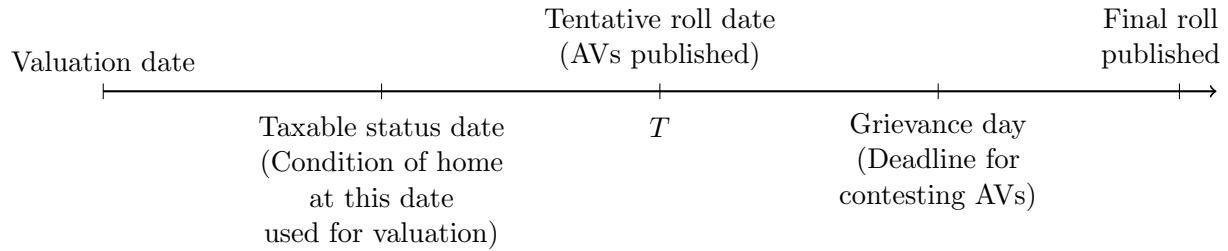
Prior to AV publication at T , there are two dates that are relevant for this work. The first is the “valuation date”. If a municipality reassesses homes for the upcoming tax year, the AVs have to reflect home values at this particular day. AVs are thus not meant as a forecast, but reflect pre-publication prices. Valuation of residential real estate is typically done with a sales comparison approach. AVs are thus based on past sales prices of units that are similar to the home that is assessed. The second relevant date before T is the “taxable status date”. The AV has to be based on the condition of the considered home at this particular date. Hence, homes whose condition changes after this date (e.g., through a fire), should be severely mispriced by authorities. In the data cleaning process, I therefore remove observations with extreme one-year AV returns.

At T , AVs are not final. Until the “grievance day”, typically four weeks after T , homeowners have the chance to contest their home’s assessment. This is a potential concern for the empirical analysis, since the values are not necessarily fixed at T , raising endogeneity concerns. In Section 4.2, I show that successful contests are relatively rare in a dataset from New York City, affecting less than 1% of all single-family homes, and thus are likely to be of minor concern. AV contests that have not been accepted are not necessarily final, but

¹⁵The link to the New York State Department of Taxation and Finance website is given in Section 2.1.

¹⁶As reported by the New York State Department of Taxation and Finance on <https://www.tax.ny.gov/pdf/ORPTS/recent-reassessments.pdf>, last retrieved on March 15, 2021.

Figure A1
The New York State property tax calendar



This figure visualizes the timeline of the assessment process in New York State for a representative municipality in a given year. Market value estimates have to reflect the homes’ values at the “valuation date”. Homes have to be valued according to the condition of the home at the “taxable status date”. AVs are published at the “tentative roll date” T . AVs can be challenged until the “grievance date”. The “final roll” is published at the beginning of the new tax year.

homeowners have to apply for judicial review, making further changes rather difficult.

The “final roll” is published at the beginning of the new tax year and contains the finalized AVs. Until the next reassessment, which fraction of AVs is taxed depends on local and individual factors. These include the local tax rate, the local assessment ratio, local budgeting, as well as individual exemptions, such that effective tax rates can vary substantially even within the same jurisdiction. Additionally, there are two policies that are limiting the increase of tax payments. First, in New York City (NYC) and Nassau county, assessed values can maximally increase by 6% per year or 20% over five years. Second, changes in the total tax levy (i.e., how much taxes a given entity can collect in total) are capped in most jurisdictions (e.g., counties outside NYC and independent school districts) by the minimum of either two percent or the CPI inflation rate. Both caps should not be an issue for this paper. First, In the robustness section, I show that my results are robust to the exclusion of observations in NYC and Nassau. Second, tax levy limits do not rule out substantial individual changes in tax payments. For instance, an AV increase relative to other properties in the municipality would lead to an increase in tax payments even if the total tax levy remained constant.

B Derivations

B.1 Derivations for the tax channel

The tax channel increases the absolute sales-price-AV distance, DA , post AV publication if

$$DA^{pre}(\bar{n}) \leq DA^{post}(\bar{n}) \quad (\text{A1})$$

$$\Leftrightarrow \bar{n}|\dot{p}^* - \tau E[\hat{p}] - \hat{p}| \leq \bar{n}|\dot{p}^* - \tau \hat{p} - \hat{p}| \quad (\text{A2})$$

$$\Leftrightarrow |\dot{p}^* - \tau E[\hat{p}] - \hat{p}| \leq |\dot{p}^* - \tau \hat{p} - \hat{p}|, \quad (\text{A3})$$

holds. The last equivalence follows as the total housing supply is larger than zero, i.e., $\bar{n} > 0$. The expected tax payments, $\tau E[\hat{p}]$, are replaced with the true value, $\tau \hat{p}$, once the AV is published (post). Using $(|a| \leq b) \Leftrightarrow (-b \leq a \leq b)$, inequality (A1) holds if

$$-|\dot{p}^* - \tau \hat{p} - \hat{p}| \leq \dot{p}^* - \tau E[\hat{p}] - \hat{p} \quad \text{and} \quad (\text{A4})$$

$$|\dot{p}^* - \tau \hat{p} - \hat{p}| \geq \dot{p}^* - \tau E[\hat{p}] - \hat{p}. \quad (\text{A5})$$

It is now to show under which conditions inequalities (A4) and (A5) hold. Starting with (A4), as $(|a| \geq b) \Leftrightarrow (a \leq -b) \text{ or } (a \geq b)$, it must hold that either

$$\dot{p}^* - \tau \hat{p} - \hat{p} \geq \hat{p} - \dot{p}^* + \tau E[\hat{p}] \quad \text{or} \quad (\text{A6})$$

$$\dot{p}^* - \tau \hat{p} - \hat{p} \leq \dot{p}^* - \tau E[\hat{p}] - \hat{p}. \quad (\text{A7})$$

Defining $\dot{p}^{pre} := \dot{p}^* - \tau E[\hat{p}]$ and $\dot{p}^{post} := \dot{p}^* - \tau \hat{p}$, it is easy to show that it follows from inequality (A6) that

$$\frac{\dot{p}^{pre} + \dot{p}^{post}}{2} \geq \hat{p}, \quad (\text{A8})$$

and from (A7) that

$$E[\hat{p}] \leq \hat{p}. \quad (\text{A9})$$

Proceeding with similar calculations and arguments for , it can be shown that (A5) is true if either

$$\frac{\dot{p}^{pre} + \dot{p}^{post}}{2} \leq \hat{p} \quad \text{or} \quad (\text{A10})$$

$$E[\hat{p}] \geq \hat{p} \quad (\text{A11})$$

holds. This means that (A1) is fulfilled when one out of the two inequalities (A8) and (A9) holds together with either (A10) or (A11). This leaves four combinations to check. Inequalities (A8) and (A10) hold together if $\hat{p} = E[\hat{p}]$. The same is true for inequalities (A9) and (A11). From these two pairs, it follows that DA remains unchanged when expectations match the outcome. Now, consider (A8) and (A11). From (A11), it follows that $\dot{p}^{post} \geq \dot{p}^{pre} \Leftrightarrow \epsilon > 0$ and thus rewriting (A8) yields

$$\frac{2\dot{p}^{pre} + \epsilon}{2} \geq \hat{p}, \quad (\text{A12})$$

in which $\epsilon := \dot{p}^{post} - \dot{p}^{pre}$ is the difference between expected AV and actual AV times the effective tax rate, it follows that (A1) holds if

$$\hat{p} \leq E[\hat{p}] \leq \dot{p}^{pre} + \frac{|\epsilon|}{2}. \quad (\text{A13})$$

Proceeding with similar calculations and arguments (A10) and (A9), it follows that (A1) holds if

$$\hat{p} \geq E[\hat{p}] \geq \dot{p}^{pre} - \frac{|\epsilon|}{2}. \quad (\text{A14})$$

Hence, inequalities (A13) and (A14) are sufficient conditions for (A1).

B.2 Derivations for the back-of-the-envelope calculations

Defining the price after treatment as P^t and the price under control conditions as P^c , per definition, the estimated treatment effect γ is the difference in DAR between treatment and control condition, given as

$$\left| \frac{P^t - AV}{AV} \right| - \left| \frac{P^c - AV}{AV} \right| = \gamma. \quad (\text{A15})$$

Note that the AV is the same for both scenarios, mirroring the empirical application in which the contemporaneous AV is used to derive the absolute sales-price-AV ratio. For simplicity, assume that P^t and P^c are larger than AV. Then, it follows from Equation (A15) that

$$P^t - AV = P^c - AV + \gamma AV, \quad (\text{A16})$$

which can be rearranged to

$$\Delta P_{ic} = \frac{P^t - P^c}{P^c} = \frac{\gamma AV}{P^c}. \quad (\text{A17})$$

To see that these calculations fit to the model presented in Section 2, note that the assumption made that P^t and P^c are smaller than the AV is consistent with condition (12), which implies that the sales price is increasing as well. Similarly, when assuming that P^t and P^c are smaller than AV instead, it follows that

$$\frac{P^t - P^c}{P^c} = -\frac{\gamma AV}{P^c}, \quad (\text{A18})$$

which is again in line with condition (11).

C Data handling

I merge the tax record database with the housing transactions as follows. For each municipality tax-year cluster c , I define a symmetric time-window of 360 days around the publication date T_c . For all transactions that are observed for each c , I identify the particular property-specific AV that was published at the corresponding date T_c . Thus, the AV that is matched to a transaction i was unknown if i was sold in the 180 days before T_c , and known if i was sold in the 180 days afterwards instead. The merging process described is applied to all transactions that remain after the cleaning procedure that is described below.

I focus on single-family homes as they are associated with a unique assessor’s parcel number (APN) which is used as a key for the matching process. Starting out with 1,253,756 arms-length transactions from December 2006 (the earliest possible date to identify a matching AV for) to December 2017 with non-missing sales price and available APN, I follow several steps of DeFusco et al. (2020). First, I remove all foreclosure related transactions (1,184,960 remain). Second, I remove duplicates according to two criteria. If names of buyers and sellers, as well as sales prices are identical for more than one observation, I keep only the earliest recorded transaction. If there are multiple transactions for a particular property at the same day, all but one transaction is dismissed (1,129,881 remain). Similar to Bollerslev et al. (2016), I set fixed bounds on the nominal values of 5,000 USD and 10,000,000 USD. Thus the high right-skewness of the price distribution is mitigated, and scaling is more appropriate for the regression analysis. This leaves me with a cleaned transaction sample of 1,126,596 observations.

The cleaned transaction data is matched to the publication dates obtained from the Municipal Profile webpage of the New York State government. The general publication date is May 1, but some municipalities choose to deviate from this date, e.g., to match other budget related purposes. I use the Statewide Information System (SWIS) codes that uniquely identify municipalities to match the respective publication dates to the observations.¹⁷ Thus, observations for which the SWIS code is missing cannot be used and must be dismissed (1,125,338 observations remain). Finally, the transactions are matched to the tax records. Here, each transaction is assigned to the up-to-date AV that is (yet) unknown if the property transacted before publication (i.e., in the time window -180 to -1 days), and known afterwards (day 0 to 180). The matched dataset contains 747,254 observations.

For the analysis, the one-year AV lag is necessary for two reasons. First, to identify the control group, which requires that homes have not been reassessed in the given tax year.

¹⁷For New York City, I use the county FIPS codes. For most of the remaining counties, the SWIS code is part of the assessor’s parcel number. For the few counties that do not follow this coding procedure, SWIS codes are given in another variable of the dataset that is identified manually.

Second, extreme changes in valuation are possible as the condition of a home could have changed dramatically, e.g., through fire damage. Accordingly, I dismiss observations for which the prior year's AV is either missing or for which the one-year AV return exceeds the bounds of the second and 98th percentile. I chose the second and 98th percentile, as trimming based on the first and 99th percentiles did leave extreme outliers in the sample. After these steps, 516,165 observations remain.

I identify treatment and control group in a data driven way. For each of the remaining observations, I calculate the one-year AV return and round it up to the third decimal place to correct for small differences in returns. I identify three different types of municipality-tax-year clusters based on how many observations within a cluster have the same return. First, clusters for which the mode return makes up less than 75% of returns is assigned treatment status. Second, clusters for which the return of at least 75% of observations equals zero is assigned control status. Third, clusters in which the mode return makes up more than 75% of observations, but the mode return is different from zero, is assigned the market- or inflation-adjusted status.

Based on these definitions, I dismiss observations in the control group with returns different from zero, as these are likely to be caused by unusually large physical changes, and AV adjustments are likely to be expected for such properties. I further dismiss all observations in the market- or inflation-adjusted group as they are neither fully treatment nor control group. Additionally, observations in municipalities (SWIS codes) that never reassess homes in the sample are also dismissed to increase the comparability of the control group. After these steps, 315,402 observations remain. Once the observations from the market- or inflation-adjusted group are removed, I deal with extreme assessment ratios which are commonly observed in housing markets (e.g., McMillen, 2013). To rule out that my findings are driven by such outlier properties, I dismiss trades of homes for which either the ratio of sales price and AV or the absolute distance between both quantities is extreme (again 2nd and 98th percentile). The latter is done for both treated and control group separately to account for disparities in the price distribution. After this step, 288,546 observations remain.

To increase comparability between control and treatment group, I conduct a final step. As treated municipalities are typically larger than the control group ones, I adjust both groups such that at least 100 observations per municipality-tax-year cluster are required. The final dataset consists of 193,494 observations.