The effect of public property valuation*

Simon Stehle†

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†University of Konstanz, Department of Economics, Postbox 147, D-78457 Konstanz, Germany, phone: +49 7531 88 3620, e-mail: simon.stehle@uni.kn.

Abstract

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Homeowners' property tax payments are commonly calculated as a fraction of their homes' estimated market values (EMVs). These EMVs should impact trading prices through two counteracting channels. First, an increase in EMV implies increased tax payments, which should negatively affect a home's trading price (tax channel). Second, EMVs are a potential reference price, which should lead to a positive effect (anchoring channel). In a quasi-experimental setting that exploits geographic variation in timing of EMV publications and revaluation frequencies, I show that a higher EMV leads to a lower trading price, i.e., that the tax channel dominates.

JEL Classification Codes: R38, H29

Key Words: Housing market, micro-structure, property tax, real estate, assessed values

1 Introduction

For many governments worldwide, property taxes are a major source of income. In 2019, for instance, US homeowners alone paid more than 616 billion USD in property taxes, accounting for almost 40% of state and local tax revenue.[1] To account for wealth disparities among homeowners, authorities tax each home by a fraction of its officially estimated market value (henceforth EMV). These EMVs constitute an important factor for homeowners, as they are not only central for the calculation of individual tax bills, but also provide information about the current value of their underlying homes. Thus, EMVs have the potential to affect and distort transaction prices each home within entire markets. Yet, to which extent this is the case remains largely unexplored.

In this paper, I investigate the causal impact of EMVs on transaction prices, which should be driven by two counteracting channels. On the one hand, when negotiating sales prices, a home's EMV serves as a potential reference price. An increasing EMV should therefore increase the trading price of the corresponding home (anchoring channel). On the other hand, an increasing EMV typically implies a higher tax burden, which should in turn negatively affect a home's trading price (tax channel). I identify the dominating channel with Differences-in-Differences (DiD) regressions relying on a novel identification setup that exploits the timing of EMV publication as well as geographic variation in the frequencies at which EMVs are reassessed. My results document that the tax channel prevails: An increasing EMV is associated with a decline in the corresponding transaction price. A back-of-the-envelope calculation indicates that this effect is economically sizable, leading to a change in 0.9% of an average home's initial sales price.

The empirical analysis is based on two datasets that contain historical tax records and individual transactions of homes, respectively, of which each covers on average more than 99 percent of the US population. I focus on single-family homes in New York State for several reasons. First, within each municipality, EMVs are published annually at a particular date, allowing me to compare pre- and post-publication periods. Second, homes are reassessed at different frequencies, such that transaction of homes in municipalities, that publish unchanged EMVs in the given year, can be used as control group. Third, EMVs are not defined as forecasts, but have to reflect home values in the past. Fourth, unlike, e.g., in California, there is no post-transaction adjustment of an EMV once the trade of a home is completed, but EMVs are collectively generated by a local assessor. Together, these characteristics provide the opportunity to use a novel DiD setting that allows for causal inference.

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

To identify the effect of EMVs on transaction prices, a measure other than raw sales prices is necessary. That is the case as treatment (i.e., publication of updated EMVs) is assigned on the municipality-tax-year level, the treatment effect is heterogeneous on the individual home level. For instance, homes with unexpectedly low EMVs face opposite price changes compared to homes with unexpectedly high EMVs, regardless of which channel prevails. Thus, it would be possible to observe little to no price change on the aggregate level, even if individual homes within the treated area were substantially affected by their updated EMVs. I therefore investigate the absolute ratio between sales price and EMV. In a theory section, I show that this variable should be decreased through anchoring and increased by the tax channel, allowing to infer about the dominating mechanism. Additionally, this variable should change into the same direction for all homes within a treated municipality, whether EMVs are over- or underestimating a home's value, thus solving the problem of effect heterogeneity.

My results suggest that EMVs negatively affect transaction prices, in line with the tax channel. In particular, the absolute sales-price-EMV ratio increases by about 1.1 percentage points after updated EMVs are published. A back-of-the-envelope calculation shows that this translates to a value-based taxation induced change of the initial sales price of an average home of about 0.9%. The results are robust to the inclusion of local fixed effects (such as municipality-tax-year or zip-code-tax-year), as well as month fixed effects. Consistent with the tax channel, units with higher effective tax rates are stronger affected than units with lower rates. Right after EMV publication, no immediate effect is observed, suggesting that when EMVs are most salient (e.g., sent to homeowners by mail) 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. This assumption requires that the homes in the control group follow the same trend treated units would have followed if EMVs had remained constant. As sale dates are known by day, it is possible to investigate pre-publication trends of both groups. 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 pre-publication trends in several subsamples. I additionally run placebo tests on pre-publication observations, which indicate no significant difference in trends.

Second, it is possible that buyers and sellers postpone transactions until updated EMVs are published to reduce uncertainty. This should be of minor concern, as waiting in the housing market is costly 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 EMVs are changed after their initial publication, I make use of a dataset from the New York City government on EMV-revision letters ("Notices of Revised Property Values") sent to homeowners after initial EMV publication. The data shows that EMVs of about 6,000 single-family homes have been changed after initial publication. These homes account for only 0.9% of the single-family homes found in my New York City dataset for the subsequent tax year, suggesting that the possibility to contest EMVs is only of minor importance.

This work contributes to a growing strand of literature on the effect of property taxation on trading prices. Bai, Li, and Ouyang (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 EMV) 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 documenting a new effect that is heterogeneous even within the same treated location.

I further contribute to the literature on inequity in property taxation by revealing an additional source of inequitable outcomes. So far, other work focused on the inequity through tax payments resulting from a misspecified assessment process, such as Goolsby (1997), Allen and Dare (2002), Sirmans, Gatzlaff, and Macpherson (2008), and Hodge, McMillen, Sands, and Skidmore (2017). I extend this strand of literature by showing that in addition to the documented inequity in tax payments, misspecified assessments distort sales prices themselves. As my results indicate that increased EMVs reduce sales prices, my paper provides evidence that valuation errors increase injustice among homeowners in the form of a double-punishment. Homeowners with unjustified high EMVs do not only have to pay an excessive amount of taxes, but also suffer from a reduced price when selling their home.

By investigating EMVs 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. 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, Badarinza, Liu, Marx, and Ramadorai (2020) estimate a structural model of listing decisions and identify the nominal purchase price as a reference point for homeowners. Fischer, Füss, and Stehle (2020) 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, Cao, Kuchler, and Stroebel (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 assessed values (here, EMVs), Jones (2020) shows that homeowners confronted with an increase in their EMV have a higher propensity to contest their home's assessment, which can be linked to loss aversion. Considering the EMVs as an anchor for valuations instead, Cypher and Hansz (2003) do not find anchoring on assessed values in an experimental setting. In contrast, Levy, Dong, and Young (2016) find homeowners to be influenced by home values that are used for property taxation, but are not necessarily market value estimates, in Wellington, New Zealand. While these studies investigate EMVs primarily as anchor, this study expands this view by studying the interplay between tax and anchoring channel.

The remainder of this paper is structured as follows. The following section briefly introduces the property tax system in New York State. Afterwards, in Section 3, I theoretically motivate the channels through which EMVs should influence trading prices. Section 4 describes the empirical approach followed in this paper. The data used in this work and the validity of the methodology applied is discussed in Section 5. Results and robustness checks are presented in Sections 6 and 7, respectively. Section 8 concludes.

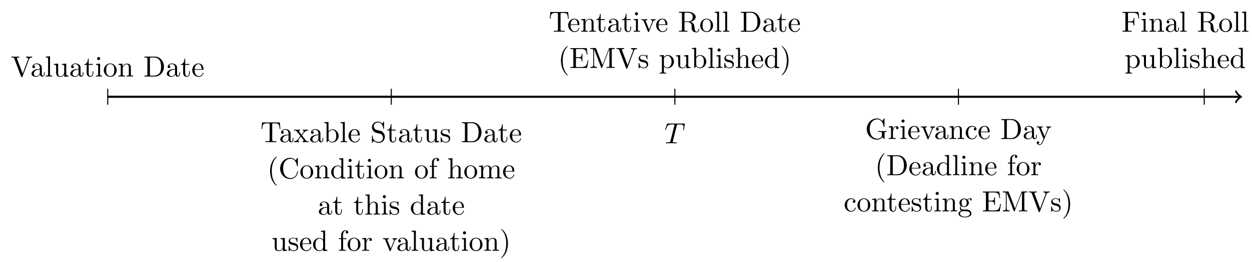
2 The Property Tax System in New York State

This section gives a brief overview of the New York State property tax system. Figure 1 illustrates the official property tax calendar that is followed by all municipalities in the state.[2]

In all municipalities, EMVs are published annually at the "tentative roll date", T, as shown in the center of Figure 1. Beginning at T, the new information contained in the updated EMVs 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

[2]An overview of important dates 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.

Figure 1
A stylized 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". EMVs are published at the "tentative roll date" T. EMVs can be challenged until the "grievance date". The "final roll" is published at the beginning of the new tax year.

May 1, but other dates are used, e.g., January 15 in New York City. Although EMVs are published every year, EMVs are not necessarily updated in the same frequency. In some years, published EMVs either remain unchanged or are collectively adjusted by the same percentage, e.g., to adjust for inflation.[3] The existence of pre-and post-publication periods as well as years without reassessment in some municipalities yields the key ingredients for the DiD analysis that is described in Section 4.

Prior to 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 EMVs have to reflect home values at this particular day. EMVs are thus not meant as a forecast, but reflect pre-publication prices. Valuation of residential real estate is typically done with a comparable sales approach. EMVs are thus based on past sales prices of units that are similar to the home underlying. The second relevant date before T is the "taxable status date". The EMV 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 EMV returns.

At T, EMVs are not final. Until the "grievance day", typically four weeks after T, homeowners have the chance to contest their home's valuation. This is a potential concern for the empirical analysis, since the values are not necessarily fixed at T, raising endogeneity concerns. In Section 5.2, I show that successful contests are relatively rare, affecting less than 1% of all single-family homes, in a dataset from New York City and thus are likely

[3]As discussed in Section 5.1, I exclude municipality tax-year clusters in which EMVs have been collectively adjusted bx the same percentage, as the corresponding observations can be neither assigned to treatment nor control group.

to be of minor concern. The "final roll" is published at the beginning of the new tax year and contains the finalized EMVs. Based on these values, taxes are calculated as a fraction of the EMVs. Until the next reassessment, this fraction depends on the local tax rate, the local assessment ratio (i.e., how much of the EMV is taxable), the local budgeting, as well as personal exemptions, leading to a substantial variation in individual effective tax rates.[4] EMV contests that have not been accepted are not necessarily final, but homeowners have to apply for judicial review, making further changes rather difficult.

3 Theoretical Considerations

In this section, I provide a simple model that first, theoretically motivates anchoring and tax channel, and second, illustrates the implications of anchoring and tax channel for the causal impact of EMVs on transaction prices, respectively.

3.1 A simple model

I consider a model similar to Landvoigt, Piazzesi, and Schneider (2015), extended by a second period and property taxation. 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 $n \geq 0$.

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 EMV, $\tau EMV(n)$, in which $EMV(n) = \hat{p}n$ 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 EMV, $\hat{p}$, as the local assessor is uncertain about the true model and can thus only provide a best estimate for $\dot{p}$. The parameter $0 \leq \tau \leq 1$ is the household's effective tax rate, which is the product of the local assessment ratio (the proportion of the EMV that can be taxed), and the individual tax rate (local tax rate after individual exemptions).

For simplicity, further assume that the household 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 frictionless transfer of wealth to the second period, the

[4] In New York State, assessed values are defined as EMV times assessment ratio and thus indicate the taxable value of each home. Thus, if the assessment ratio equals 100% (i.e., the full market value of the home is taxable), EMVs and assessed values coincide. Consequently, to avoid confusion between the terms "assessed value and "market value", often interchangeably used, I continue to use the abbreviation "EMV".

household solves the optimization problem

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

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

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

$$\frac{1}{c_1} = \beta\frac{1}{c_2} \qquad \text{and} \tag{3}$$

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

Equation (4) shows that, as in addition to the closed-form model of Landvoigt, Piazzesi, and Schneider (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 the marginal tax payment at n, $\hat{p}$. 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

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

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

The rational choice model described above does (so far) not include the anchoring channel. That is the case as anchoring itself is not rational. For instance, Northcraft and Neale (1987) document that professional real estate agents adjust their appraisals towards randomly assigned ask prices. Similarly, Black and Diaz III (1996) document random adjustments of an ask price to influence offering price as well as final transaction price in an experimental setting.[5] 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

[5]In 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.

must result in a deviation from the optimal choice (unless the EMV perfect prediction of the sales price before anchoring).

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

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

in which $0 \leq \alpha \leq 1$ is the 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 EMV.

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

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

which indicates that the tax channel dominates, i.e., an increasing EMV decreases the transaction price, if $\alpha - (1-\alpha)\tau < 0$, and the anchoring channel dominates, i.e., an increasing EMV 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 individual effective tax rate τ. Accordingly, it should hold that anchoring should be highest when EMVs are most salient, i.e., right after publication. Furthermore, the tax channel should be most pronounced for units associated with high effective tax rates. Both of these hypotheses are tested in Section 6.

3.2 Measuring the impact of EMVs on transaction prices

So far, the model illustrated that whether a sales price is positively or negatively influenced by an EMV depends on two factors, first, the dominating channel and, second, whether the EMV 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 by EMVs. For instance, while over- and undervalued homes should be affected in the 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 EMV to sales price relationship.

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

$$AD(n) = \Big|P(n) - EMV(n)\Big| = n\Big|\dot{p}^{anch} - \hat{p}\Big|. \tag{8}$$

Importantly, the true EMV (or the true per-unit price, $\hat{p}$) in Equation (8) that is subtracted is known after publication, but not before. That is, when constructing the empirical measure AD later, I rely on ex-post knowledge of the EMVs.

In the empirical analysis, I make use of a standardized version of AD, as illustrated in Section 4. For simplicity, I illustrate the effects from anchoring and tax channel on the absolute measure AD first. It is then straightforward to show that the same advantages of AD also hold for the relative measure.

In the remainder of this section, I show that post publication, AD should increase for all units in treated municipalities, relative to the control group, if the tax channel dominates, and decrease for all units in treated municipalities relative to the control group if the anchoring channel dominates. For the sake of clarity, both channels are investigated separately.

3.2.1 Anchoring channel

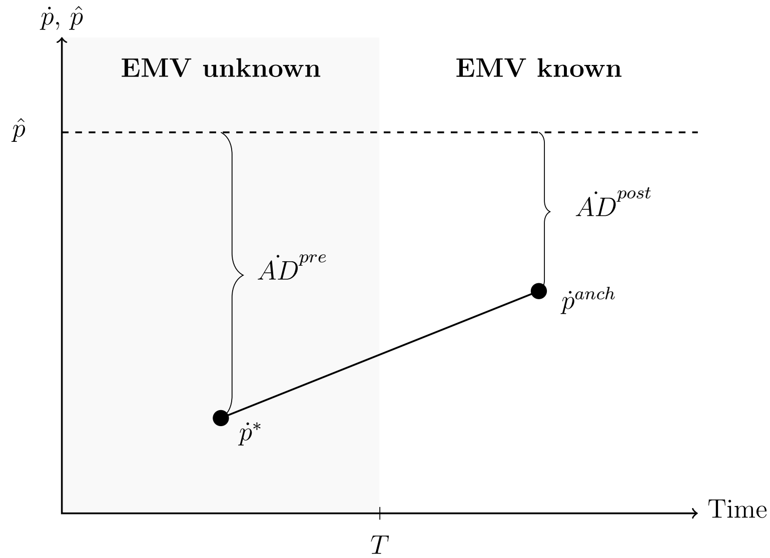
In this section, I show that the anchoring channel reduces the absolute distance between sales price and EMV, AD. For the sake of simplification, define $\dot{p}^* \equiv \frac{W\theta}{(1+\theta)\bar{n}}$. Then, the change in the absolute EMV-sales-price difference from pre- to post EMV publication, given that only the anchoring channel is present, can be described with

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

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

Figure 2 illustrates the anchoring channel for a particular home based on prices per unit of housing n. Here, the per-unit sales price, $\dot{p}$, depends on whether the home is sold before or after EMV publication at T. In the former case, highlighted in gray, the per-unit EMV, $\hat{p}$, is unknown, such that $\dot{p}^*$ remains unaffected. The corresponding distance between $\dot{p}^*$ and $\hat{p}$ is labeled $\dot{AD}^{pre}$. In the latter case, instead, $\hat{p}$ is known and serves as anchor. The per-unit sales price, $\dot{p}$, therefore adjusts towards the per-unit EMV, $\hat{p}$, reducing $\dot{AD}^{pre}$ to $\dot{AD}^{post}$.

Figure 2
Illustration of the anchoring channel



This graph illustrates the anchoring channel through which the per-unit sales price of a home, $\dot{p}$, is affected by its corresponding per-unit EMV, $\hat{p}$, once $\hat{p}$ is known, i.e., was published at T. The effect of $\hat{p}$ on $\dot{p}$ is measured by the change in $\dot{AD}$, which is the absolute distance between both quantities, as defined in Equation (8). The graph illustrates a pre- (*gray-shaded*) and a post-publication scenario for the same home, such $\hat{p}$ takes on the same value in each, as indicated by the dashed line. Before T, the transaction is realized without available anchor at $\dot{p}^*$. The corresponding EMV-price distance is $\dot{AD}^{pre}$. In the right-hand scenario, $\hat{p}$ is known and anchoring thus possible. The per-unit sales price, $\dot{p}$, thus adjusts towards the per-unit EMV, $\hat{p}$, as indicated by the black solid line, resulting in $\dot{AD}^{pre} > \dot{AD}^{post}$.

3.2.2 Tax channel

The tax channel, in contrast, generally implies an opposite, increasing effect on AD, as shown 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 EMVs are published, households purchase their home under the expected future EMV, and replace expectations with the true value post-publication. Thus, when considering the tax channel isolated from anchoring, it holds that

$$AD^{pre}(n) - AD^{post}(n) = n\left|\dot{p}^* - \tau E[\hat{p}] - \hat{p}\right| - n|\dot{p}^* - \tau\hat{p} - \hat{p}|\,. \tag{10}$$

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

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

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

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

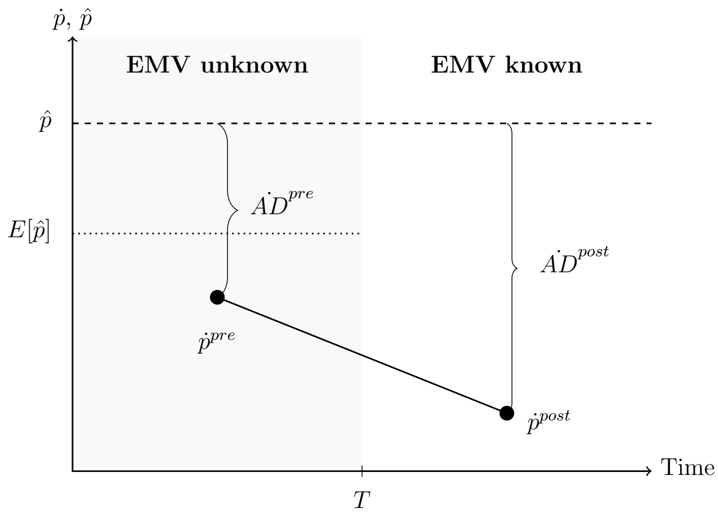
Panels A and B of Figure 3 illustrate this idea as well as the resulting effect on the per-unit EMV-sales-price distance, $\dot{AD}$. As before, each panel shows two scenarios, respectively. In the first, highlighted in gray, the home under consideration is sold before publication date, T, when the true EMV is unknown. In the second, the EMV has been published. Again, as the same home is analyzed for both scenarios, the per-unit EMV, $\hat{p}$, is the same for both cases, as illustrated by the dashed line. The resulting distance under expectations between $\dot{p}^{pre}$ and the true $\hat{p}$ is then $\dot{AD}^{pre}$. Once the EMV is published, the new information about taxes is capitalized in the sales price (instead of the expectations), leading to a relatively higher or lower $\dot{p}^{post}$, depending on whether expectations have been too low or too high.

In Panel A, expectations are lower than the actual EMV, which implies that market participants in the (gray-shaded) pre-publication scenario expected lower future tax payments. Thus, once the higher per-unit EMV, $\hat{p}$, is published, the implied increase in taxes negatively affects the per-unit sales price, $\dot{p}$. In Panel B, the opposite is the case, as higher taxes (through a higher EMV) are expected in the (gray-shaded) pre-publication scenario. Once the relatively lower $\hat{p}$ becomes available for market participants, the sales price thus increases. For both Panels A and B, as illustrated in the graph, the per-unit absolute EMV-sales-price distance $\dot{AD}$ (and thus AD) increases ($\dot{AD}^{post} > \dot{AD}^{pre}$).

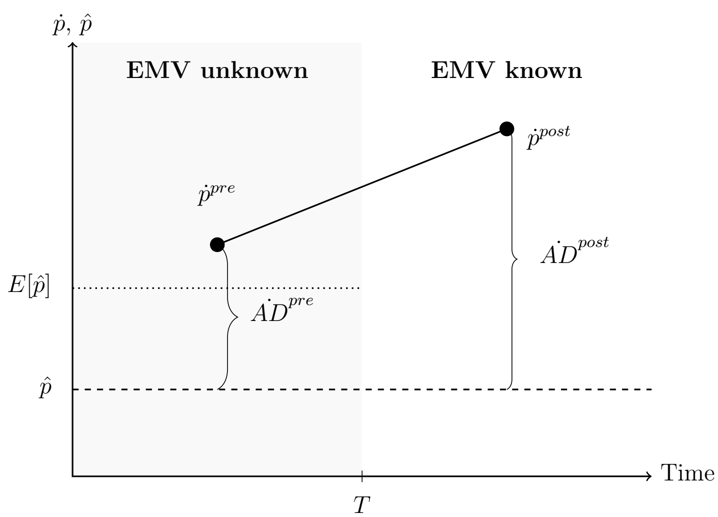
In sum, Figures 2 and 3 illustrate that anchoring and tax channel should have opposing effects on the easily measured variable AD and each effect is distinctive for both over- and undervalued homes. Finally, as the change in AD can be directly observed, it is not necessary to observe the expectations formed by homeowners. Before turning into testing which of these channels is dominating the other with a DiD approach, I present my methodology in the following section.

Figure 3
Illustration of the tax channel

Panel A: Expectations about the EMV are too low



Panel B: Expectations about the EMV are too high



Panel A and B of this figure illustrate the tax channel through which the per-unit sales price of a home, $\dot{p}$, is affected by either its expected per-unit EMV, $E[\hat{p}]$, if sold before EMV publication date T (*gray-shaded* area), or the true per-unit EMV, $\hat{p}$, if sold after T. The effect of $E[\hat{p}]$ or $\hat{p}$, respectively is measured by $\dot{AD}$, the absolute per-unit EMV-sales-price difference, as defined in Equation (8). In Panel A, the expected EMV is lower than the true EMV, i.e., $E[\hat{p}] < \hat{p}$. Once the true EMV is published, $\hat{p}$ decreases due to the unexpected increase in tax payments. In Panel B, the future EMV is overestimated, i.e., $E[\hat{p}] > \hat{p}$. After T, the unexpectedly lower tax payments thus increase $\hat{p}$. In both situations shown in Panel A and B, the tax channel implies an increase in $\dot{AD}$, i.e., $\dot{AD}^{pre} < \dot{AD}^{post}$.

4 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 that reassessed homes (i.e., publish previously unknown, updated EMVs) with homes in municipalities that did not reassess homes (i.e., publish unchanged EMVs).

As my dataset contains multiple years of data for a large amount of locations, I define municipality-tax-year clusters c. Around the corresponding publication dates, T_c, I define symmetric time-windows of ± 150 days.[6] I do not consider longer time-spans to strictly avoid overlaps with upcoming (and previous) tax-years.

In the previous section, I proposed a simple, absolute measure, AD, 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 AD, to prevent that higher-priced homes drive the regression results. Leaving the theoretical framework from Section 3, I calculate for each home i, transacted within municipality-tax-year cluster c, the absolute ratio between P_{ic} and EMV_{ic} as

$$ARD_{ic} = \left| \frac{AD_{ic}}{EMV_{ic}} \right| = \frac{|P_{ic} - EMV_{ic}|}{EMV_{ic}}. \tag{13}$$

It is straightforward to see that, as $EMV_{ic} > 0$ is fixed for home i within c, the conditions derived for AD in theory Section 3 hold for ARD_{ic} as well. That is, post-publication, the tax channel should lead to an increase in ARD for treated municipalities. In contrast, the anchoring channel should decrease ARD of treated units post-publication. In other words, after the EMVs are published, the anchoring channel moves prices towards EMVs, reducing the EMV-sales-price distance, whereas the tax channel drives prices away from EMVs, increasing the distance between both quantities. Note that the EMV that is matched to the sales price is always the one published at T_c. Thus, if the common trend assumption holds, what changes between treatment and control group after T_c is only the knowledge about the EMV, allowing to disentangle the causal EMV impact on prices from the naturally existing positive EMV-sales-price correlation.

It is necessary to standardize by EMV_{ic} instead of P_{ic}, as the EMV_{ic} remains constant before and after T by definition, whereas the transaction price is, as illustrated above, affected by anchoring and tax considerations. Consequently, dividing AD_{ic} by the transaction price, P_{ic}, instead of EMV_{ic} would yield an unstable and endogenous measure.

Furthermore, it is important to note that investigating the absolute value of a relative

[6] The results are robust for alternative time-windows of 90 and 120 days, respectively, as shown in Table 4.

measure makes the implicit assumption that an overvaluation of 50% is as equally important as 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. I therefore remove extreme ratios when cleaning the data. Furthermore, in the robustness section, I show that my results quantitatively hold when investigating the non-standardized measure AD_{ic}, and second, are robust to setting a conservative upper bound for ARD_{ic}.

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

$$ARD_{itc} = \alpha + \beta_1 Treatment_{ic} + \beta_2 Post_{ic} + \gamma Treatment_{ic} \times Post_{ic} + \delta_c + \nu_t + \epsilon_{itc}, \quad (14)$$

in which ARD_{itc}, as defined in Equation (13), is the absolute ratio between P_i and EMV_i, α is an intercept, δ_c and ν_t denote municipality-tax-year and time-dependent fixed effects, respectively, Treatment $\times$ Post is the interaction between the dummies for treated and post groups, and ϵ_{itc} is a nuisance term. The coefficient of interest is γ, which measures the treatment effect on ARD due to EMV updates. As illustrated in Section 3, 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 identification in the following section.

5 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. The exact data cleaning process, as well as how transactions are merged can be found in Appendix B. After the cleaned data is described, I discuss the validity of my identification strategy.

5.1 Data cleaning

I merge property transactions of single-family homes with historical tax records from 2007 to 2017 that contain property assessments for each property for up to eleven years. Both datasets are obtained from the data vendor CoreLogic who provides a US property record coverage of more than 99 percent.[7] I focus on New York State properties out of several reasons. First, the municipality-dependent publication dates T_c are easily accessible through the municipal profile webpage provided by the state government.[8] Second, the revaluation frequencies across municipalities differ, allowing for a control group. Third, EMVs 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 datasets by following **?** in dismissing all observations that are not classified as "arms length", have a missing sales price, is associated with a foreclosure, or identified as a duplicate. Additionally, I dismiss all transactions without an assessor's parcel number (APN), which is used to merge the transactions with the tax records. Afterwards, I follow Bollerslev, Patton, and Wang (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 each EMV_{ic} to the respective home i whenever possible.

Further matching the respective one-year EMV lag is necessary for two reasons. First, to filter out observations with unusually large valuation changes. Second, to employ a data-driven way to identify treated and control clusters c. I do this by investigating whether at least 75% of the remaining observations have the same one-year EMV return (rounded up to the third digit to rule out that small deviations confound the classification). If at least 75% of returns have a one-year EMV return of zero, I assign all observations in c as control units (and dismiss all observations with a return different than 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 EMVs in the given year, I dismiss all observations in this c, as they can neither be considered treated nor control units. The remaining observations are then assigned treatment status.

After filtering out further extreme returns based on ARD_{ic} and AD_{ic}, 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 152,901 observations. Appendix B describes the technical details of each cleaning step described above.

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 untreated municipality-

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

[8]To be found at http://orps1.orpts.ny.gov/cfapps/MuniPro/, last retrieved on August 25, 2020.

tax-year clusters. From the total 151,624 observations, about 60% are treated units. For them, the average sales price is about 386,000USD, and the mean EMV only slightly lower with about 385,000USD. For both groups, a larger share of transactions are realized after the publication date, with 54% for the treated and 60% for the control units. A potential reason is that most municipalities publish the new EMVs at the beginning of the year and in spring (January 15 for New York City, May 1 for most of New York State), and turnover is typically highest in summer (e.g., Ngai and Tenreyro, 2014). For the treated group, the average return of the EMV is 1.2%, reflecting the general increase of house prices over the sample period.

Figure 4 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, in the very west of Long Island) are all treated units, as here, homes are reassessed annually. Other cities, such as Buffalo (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 comparably little 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 EMVs of both groups should be similarly precise and distributed, as the local assessors should remain more or less constant over time.

5.2 Identification

The key underlying assumption of the DiD analysis is that the trend of the dependent variable ARD_{ic} would have been the same for treated and control group in the absence of treatment. 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 evidence how the dependent variable ARD_{ic} of the treated homes would have developed if EMVs had not been updated.

To check the validity of the common trend assumption, I plot pre-publication trends of the dependent variable ARD_{ic} for both groups in Panels A and B of Figure 5. Both panels show the means of ARD_{ic} for two periods prior to publication, one for 150 to 76 days, and one for 75 to 1 day before publication of EMVs. Panel A shows trends conditional on municipality-tax-year fixed effects, the base case controls, for both groups, respectively. ARD_{ic} is upwards sloping for both groups at a highly similar magnitude. Likewise, Panel B

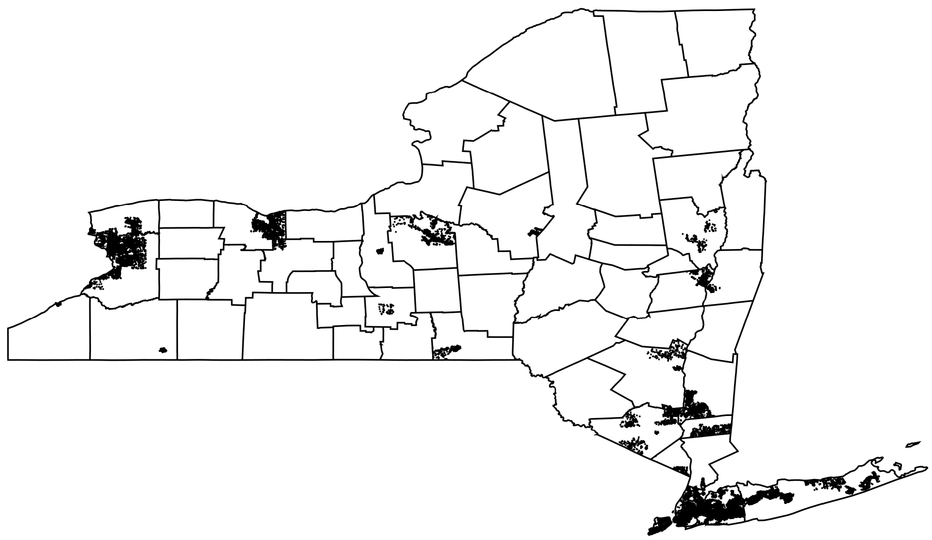
Table 1
Summary statistics

Panel A: Treated units						
	Observations	Mean	Std	1st Quantile	Median	3rd Quantile
Sales price	91,530	386,144	293,271	190,000	350,000	495,000
EMV	91,530	385,023	281,337	200,000	353,000	493,100
Abs. price-EMV ratio	91,530	0.173	0.155	0.059	0.129	0.239
Abs. price-EMV diff.	91,530	62,017	70,450	15,100	38,000	82,700
One-year return EMV	91,530	0.012	0.089	-.038	0	0.050
Post	91,530	0.539	0.498	0	1	1
Crisis (2007-2010)	91,530	0.225	0.418	0	0	0
Effective tax rate seller	76,196	0.017	0.011	0.008	0.013	0.025
Effective tax rate buyer	72,795	0.019	0.011	0.010	0.016	0.027
Panel B: Control units						
	Observations	Mean	Std	1st Quantile	Median	3rd Quantile
Sales price	61,371	257,053	212,599	123,000	206,700	335,000
EMV	61,371	249,383	199,201	120,100	210,000	325,400
Abs. price-EMV ratio	61,371	0.173	0.161	0.059	0.125	0.237
Abs. price-EMV diff.	61,371	40,591	48,976	10,000	23,100	51,723
One-year return EMV	61,371	0	0	0	0	0
Post	61,371	0.601	0.490	0	1	1
Crisis (2007-2010)	61,371	0.229	0.420	0	0	0
Effective tax rate seller	55,714	0.026	0.010	0.021	0.027	0.032
Effective tax rate buyer	48,824	0.029	0.010	0.023	0.029	0.035

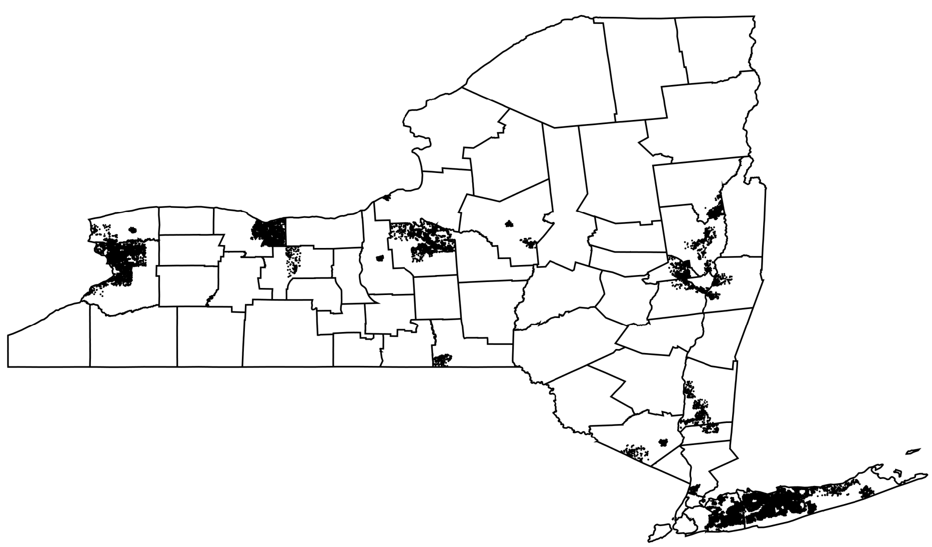
This table provides summary statistics for the variables used in the empirical application. Panel A shows summary statistics for the homes located in treated municipalities (i.e., in which updated EMVs were published). Panel B shows the same statistics for the control units, i.e., sales of homes within municipality-tax-year clusters that did not reassess homes. "Sales price" denotes the nominal transaction price, the EMV is the publicly available market value estimate provided by local assessors, which is known if the unit was sold before the publication date T_c, and known afterwards. "Abs. price-EMV ratio" is the absolute value of the ratio between nominal sales price and EMV (ARD). "Abs. price-EMV diff" is the absolute value of the difference between nominal sales price and EMV (AD). "One-year return EMV" is the relative change in EMV with respect to the prior year. "Post" is a dummy indicating whether a property was sold before or after the publication of EMVs. "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 the contemporaneous EMV. For the effective tax rate of the seller, the tax amount prior to the sale year is used, for the buyer, the contemporaneous sale year.

Figure 4
Geographic dispersion of transactions

Panel A: Treated units



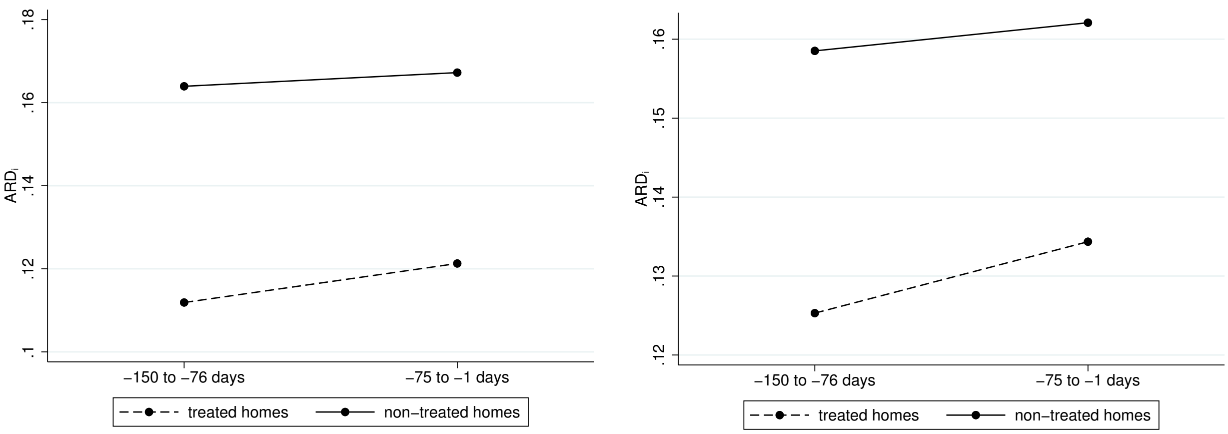
Panel B: Control units



Panels A and B show the geographic dispersion of the observations in the cleaned dataset within New York State. Panel A shows the distribution of observations assigned to the treated group, i.e., sales that took place in a time window of ± 5 months around the publication of updated EMVs. Panel B shows the geographic distribution of homes in the control group, i.e., ones that were transacted ± 5 months around EMV publication in tax-years in which the EMVs were not updated (i.e., coincide with last year's EMV). The solid lines indicate county and state borders. County and state border data was obtained from data.gov.

Figure 5
Trends of ARD_{ic} before treatment

Panel A: Base case controls *Panel B: Simple controls*



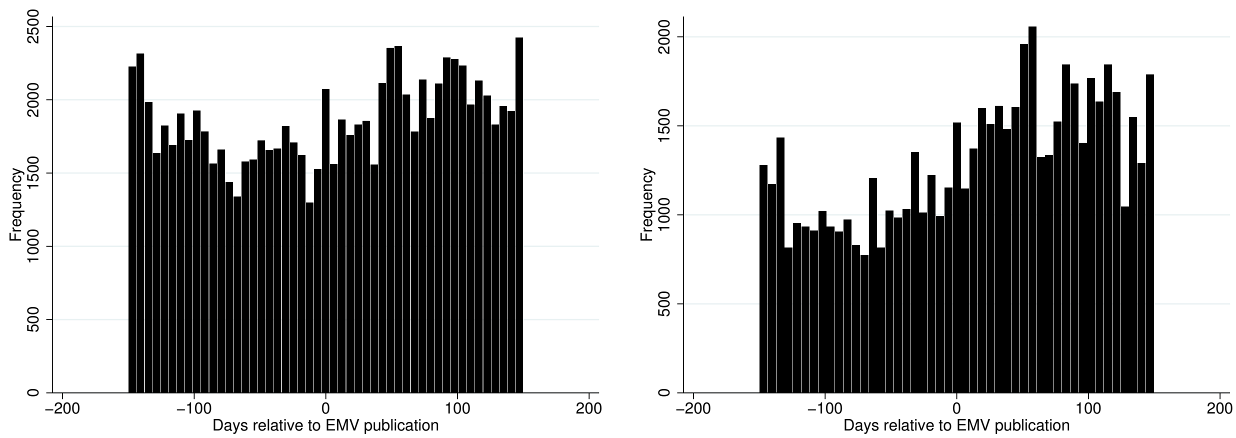
This figure shows the pre-publication development of the average absolute sales-price-EMV ratio, ARD, defined in Equation (13), for both treated (dashed line) and control units (solid line). The points displayed indicate means for two subperiods of 75 days each. Panel A displays trends conditional on municipality-tax-year fixed effect dummies. The means in Panel B are conditional on separate sets of municipality as well as tax-year fixed effect dummies.

shows the trends of ARD_{ic} for both groups when conditioning on fixed effects on separate sets of municipality and tax-year level, respectively. Even under the much simpler controls, both means are upwards 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. Note that the absolute level of each point is uninformative as the level is relative to a base cluster. Yet, this does not constitute a problem, as only the trend needs to be common in DiD regressions. Additional pre-publication trends for subsamples can be found in Appendix C, e.g., for samples based on different effective tax rates. Several placebo tests are presented in Section 7.

A further potential issue is that homeowners wait for publication until they sell their homes. Figure 6 shows the frequency of transactions relative to EMV publication for both treated (Panel A) and non-treated units (Panel B). For the treated group, no change in trend is visible around day zero, indicating no immediate effect on liquidity. With increasing amount of days relative to 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 EMVs 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 EMVs are mostly published in spring, and turnover is typically highest in summer. Additionally, waiting for EMV updates should

Figure 6
Amount of sales relative to the event date

Panel A: Treated units *Panel B: Control units*



The histograms displayed in Panel A and B show the number of observations in the dataset relative to EMV publication in a time window of ±150 days. Panel A shows the frequency for treated units, i.e., units for which the EMV 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) EMV is not revalued and thus known before and after day zero.

not be a concern in the housing market, as it is costly due to interest payments as well as maintenance 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 EMV. To investigate how frequently EMVs are changed, I utilize data from the New York City government, which provides such information for the year of 2016.[9] Investigating the subsample for single-family homes, I find that only about 6,000 EMVs 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 contesting the tentative market values successfully is rarely happening, the estimates published at T_c can be viewed as quasi-fixed, thus mitigating 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

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

affected, depending on whether the updated EMV is relatively high or low. That is, within a municipality, some homes can be positively, and some negatively affected. As this implies no shift in the aggregate price level within a municipality-tax-year cluster, no spillover effects across units due to EMV publication should be expected. Having discussed the validity of the identification strategy, the following section presents the empirical results.

6 Results

The aim of this work is to show the impact of EMVs on sales prices. To identify this effect, I investigate ARD_{ic}, the absolute ratio between the sales price of a home i and the corresponding EMV, and exploit the timing of publication at time T_c in a time window of ± 150 days. The sign of this effect is ex-ante not clear. As illustrated in Section 3, the tax channel should lead to an increase in ARD_{ic} through an induced change in tax payments. In contrast, anchoring should reduce ARD_{ic}. Using municipality-tax-year clusters in which homes are not reassessed as a control group, I run DiD regressions as defined in Equation (14).

6.1 Base case

Table 2 shows results for OLS regressions with ARD_{ic}, the absolute sales-price-EMV ratio, as dependent variable. The causal effect of EMVs on transaction prices that is measured by ARD_{ic} is given by the interaction between dummies *Treatment* and *Post*. Standard errors are clustered over counties and are shown in parentheses below.[10] Column (1) shows estimates when including municipality fixed effects only. The effect of the interaction between treatment and post-publication is statistically significant at the 0.1% level and positive, suggesting that knowledge about new EMVs is influencing ARD_{ic} positively, in line with the tax channel.

Column (2) shows the base case model, including municipality-tax-year fixed effects. The common pre-publication trend when using this specification was shown in Panel A of Figure 5. The result is significant at the 0.1% level and suggests a positive causal effect of EMVs on ARD_{ic} of 1.1 percentage points. Adding month fixed effects to address seasonalities does not alter the results, and neither does adding the more fine-grained zip-code-tax-year fixed effects as shown in Columns (3) and (4). Together, the positive estimates indicate that the

[10] An inspection showed that the results are robust to alternative clustering of standard errors, such as on municipality or zip-code level, as well as two-way clustering (Cameron, Gelbach, and Miller, 2011) with county and tax year.

tax channel is dominating and increasing the absolute sales-price-EMV ratio by about 1.1 percentage points.

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

$$\Delta P_{ic} = \frac{\gamma EMV_{ic}}{P_{ic}^c}, \tag{15}$$

in which P^c is the price under control conditions, ΔP the relative change in the sales price between treated and control state. The coefficient γ is the estimated treatment effect and is estimated to be 1.1% in the base case.

Applying Equation (15) to the sample means of the treated group, it is possible to derive an effect on transaction prices. Plugging-in the average EMV of the treated group (about 385,000USD), and using the average difference between sales price and the EMV of 62,000 USD (which must be added to the EMV to get P^c fulfill the underlying assumption), the change in P due to treatment is 0.9%. This back-of-the-envelope calculation indicates that value-based property taxation leads to price distortions of about one percent of the non-treatment sales price.

Importantly, Equation (15) is derived under simplifying assumptions to obtain a simple formula that can applied to assess the economic significance. The underlying assumption is that both transaction prices, under treated and control conditions, are larger than the EMV (see Appendix A.2). Yet, this assumption is in line with condition (12) from theory Section 3, which implies an increase in the sales price post-publication. Doing the same calculations, but assuming the opposite condition (transaction prices smaller than the EMV), in line with condition (11), a decline in the transaction price is implied.

6.2 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 based on the effective tax rates of buyers and sellers. I define the effective tax rate as tax payments made during a particular year, divided by the EMV in the same year. The ratio therefore indicates how many cents per USD increase the homeowner has to pay in taxes. I use the one-year lag of tax payments relative to the sale year to infer about the seller's individual tax rate. I further use the contemporaneous

Table 2
Base case results

	(1)	(2)	(3)	(4)
Treatment	-0.010**			-0.028**
	(0.003)			(0.008)
Post	0.003	0.003*	0.005*	0.005*
	(0.002)	(0.001)	(0.002)	(0.002)
Treatment × Post	0.012***	0.011***	0.011***	0.011***
	(0.002)	(0.002)	(0.002)	(0.002)
Municipality fixed effects	X	-	-	-
Municipality-tax-year fixed effects	-	X	X	-
Month fixed effects	-	-	X	X
Zip-code-tax-year fixed effects	-	-	-	X
Adj. R-sq.	0.054	0.070	0.070	0.099
Observations	152,901	152,901	152,901	152,901

This table provides coefficient estimates on four separate Differences-in-Differences regressions based on Equation (14), with ARD_{ic}, the absolute sales-price-EMV ratio, as dependent variable. "Treatment" is a dummy that equals one if the unit was sold within a municipality-tax-year cluster in which EMVs were updated, and zero otherwise. "Post" is a dummy that equals one if the unit was sold after the EMV was known, and zero otherwise. "×" denotes an interaction between two variables. "X" indicates that the set of control variables is used, whereas "-" indicates the 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.

effective tax rate as a second measure, as the buyer should have been at least partly involved in the payments. The effective tax rate can differ between buyer and seller out of several reasons. First, property tax rates change over time. Second, different households can have different exemptions on their payments (e.g., veterans and senior citizens). Third, the annual increase in payments might be capped. Based on the sample mean of the contemporaneous effective tax rate, I form four subsamples: high tax buyer, low tax buyer, high tax seller, and low tax seller.

Table 3 presents results on DiD regressions for the four subsamples. Importantly, the treatment dummy cannot be displayed as it is absorbed in the municipality-tax-year fixed effects. The corresponding common trends are shown in Figure A1 in the Appendix. The first two columns of Table 3 show results when separating sellers by high and low tax rates.

Comparing the interaction of treatment and post dummy shows that ARD_{ic} of sellers with a higher effective tax rate is affected by about one percentage point more, in line with the idea that taxes matter more for those who have to pay a larger share. This result suggests that sellers have increased incentives to sell a home at a lower price and thus faster when future tax payments are high. Accordingly, this finding is in line with prior literature on forced sales (e.g., Campbell, Giglio, and Pathak, 2011) that documents price discounts when homeowners need to sell more rapidly than usual.

When investigating subsamples based on the contemporaneous effective tax rate, for which should at least partly driven by buyers, a similar pattern holds. The high-tax subsample is associated with a highly significant and precisely measured treatment effect of 1.3%, whereas my estimate for the low-tax buyers at 1.1% is borderline significant at the 5% level. As tax payments for the even more distant future should matter for all buyers, the more balanced results for the two buyer subsamples make intuitive sense. Even if the effective tax rate is relatively low, it might well increase in the more distant future. In contrast, on the seller side, there are should be no immediate long-run considerations concerning taxes.

Having further underpinned that the tax channel is driving sales prices away from EMVs, I turn into investigating the post-publication dynamics of the treatment effect buy studying 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 EMVs, i.e., when the new assessments are most salient. Over time, EMVs become more and more outdated, such that the tax channel should become more and more dominant.

Table 4 shows results on three DiD regressions based on subsamples of time windows with 60, 90, and 120 days before and after the publication date, respectively. The first

Table 3
Treatment effect by effective tax rates

	Seller		Buyer	
	High tax	Low tax	High tax	Low tax
Post	0.003	0.005	0.004**	0.002
	(0.001)	(0.003)	(0.001)	(0.005)
Treatment × Post	0.017***	0.008*	0.013***	0.011*
	(0.003)	(0.004)	(0.003)	(0.005)
Municipality-tax-year fixed effects	X	X	X	X
Adj. R-sq.	0.075	0.060	0.091	0.054
Observations	60,545	71,365	66,919	56,673

This table provides coefficient estimates on four separate Differences-in-Differences regressions based on Equation (14), with ARD_{ic}, the absolute sales-price-EMV ratio, as dependent variable. The subsamples are chosen according to the effective property tax rate of buyer or seller. "Treatment" is a dummy that equals one if the unit was sold within a municipality-tax-year cluster in which EMVs 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 EMV was known, and zero otherwise. Subsamples are chosen according to two effective tax rates, a one-year lag and a contemporaneous one. The effective tax rates are derived as tax amount paid in a given year, divided by the respective EMV. The cut-off value for the subsamples is the average contemporaneous effective tax rate. The "High tax" subsamples include observations with an effective tax rate that is above the mean, "Low tax" below. The "seller" variables are referring to the one-year lag of the effective tax rate. "×" denotes an interaction between two variables. "X" indicates that the set of control variables is used, whereas "-" indicates the set of controls was not used. Standard errors are clustered over counties. *,**, and *** indicate significance on the 5%, 1%, and 0.1% level, respectively.

Table 4
Results for different time windows

	60 days	90 days	120 days
Post	0.000	0.000	0.002
	(0.001)	(0.001)	(0.002)
Treatment × Post	0.005	0.009***	0.011***
	(0.003)	(0.002)	(0.002)
Municipality-tax-year fixed effects	X	X	X
Adj. R-sq.	0.074	0.073	0.071
Observations	59,492	88,768	120,062

This table provides coefficient estimates on three separate Differences-in-Differences regressions based on Equation (14), with ARD_{ic}, the absolute sales-price-EMV ratio, as dependent variable. "Treatment" is a dummy that equals one if the unit was sold within a municipality-tax-year cluster in which EMVs 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 EMV was known, and zero otherwise. The column names indicate the time window relative to the publication date used for the regression: "60", "90", and "120 days" before and after T_c. "×" denotes an interaction between variables. "X" indicates that the set of control variables is used, whereas "-" indicates that the respective set of control variables was not used for the model. Standard errors are clustered over counties. *,**, and *** indicate significance on the 5%, 1%, and 0.1% level, respectively.

column, which covers with a time window of ± 60 days around T_c less than half of the time frame of the base case, shows that there is no immediate overall effect observed, even though the coefficient estimate is positive. As outlined above, this result could suggest that the anchoring channel offsets the tax channel right after the publication date, at which the EMV should be most salient to homeowners.

When investigating 90 days around the publication date, the estimate becomes significant at the 0.1% level and closer to the 1.1% estimate of the base case, suggesting that with increasing time, more and more market participants include the EMV into their price estimates and that anchoring is reduced once the EMV becomes less salient. Expanding the time window to 120 days pre and post-publication, the coefficient estimate is again at the 1.1% as found in the base case analysis with a time window of 150 days. Table 4 thus indicates that first, the base case results are robust to reducing the time-window by 40%, and second, it takes time until the information about new EMVs is capitalized into transaction prices, potentially due to the offsetting anchoring channel. The maximum effect observed for the full sample of 1.1% is almost reached after 90 days, and fully after 120 days. This suggests that after about 120 days, the tax channel is fully dominating, and anchoring is only present when salient (and timely) right after publication.

The dataset used for EMVs starts in 2007, right when the housing bubble began to burst.

Table 5
Treatment effect by subperiods

	Crisis (2007-2010)	2011-2017
Post	0.066**	0.003
	(0.018)	(0.002)
Treatment × Post	0.024	0.014***
	(0.029)	(0.002)
Municipality-tax-year fixed effects	X	X
Adj. R-sq.	0.299	0.059
Observations	34,668	118,233

This table provides coefficient estimates on three separate Differences-in-Differences regressions based on Equation (14), with ARD_{ic}, the absolute sales-price-EMV ratio, as dependent variable. "Treatment" is a dummy that equals one if the unit was sold within a municipality-tax-year cluster in which EMVs 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 EMV was known, and zero otherwise. The column names indicate period according to the publication year on which the regressions are based. "×" denotes an interaction between two variables. "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.

This raises the question whether the subsequent years of turmoil that followed affected the impact of EMVs on trading prices as well. For instance, when prices are more volatile, an anchor such as the EMV could be more relevant for market participants, as it remains unclear where prices are heading. In contrast, the volatility of EMVs 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 EMVs on trading prices.

Table 5 shows results for two separate DiD regressions based on subsamples related to the year in which the respective EMVs were published. The common trends are, after municipality-tax-year fixed effects, shown in Figure A2 in the Appendix. The first column of Table 5, depicting results for the 2007-2010 crisis subsample, shows that the coefficient estimate is positive and at about 0.024, but insignificant. This could suggest that when prices volatile, homeowners value the EMV as an anchor, outweighing tax considerations. In contrast, the coefficient for the 2010-2017 sample is positive and at about 1.4%, slightly larger than for the base case scenario. This indicates that in normal times, market participants emphasize tax considerations.

7 Robustness

The purpose of this section is to analyze whether the results presented in Section 6 are robust with respect to several choices made throughout the paper. In addition, I present placebo tests to analyze whether there are significant differences in the pre-treatment trend between treatment and control group.

Table 6 summarizes all 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 the base case scenario municipality-tax-year fixed effects.

About 21 percent of properties analyzed in the paper is 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 might behave differently from regions in the control group. I therefore check whether the base case results hold when excluding NYC properties. As shown in Table 6, the coefficient estimate of the interaction term is at 1%, highly similar to the base case. This underpins the external validity of the results: It does not matter whether properties are located in highly urbanized areas or in comparably rural ones, the tax channel remains at a similar order of magnitude.

Similarly, 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 288,000 USD) in the sample. The treatment effect is slightly higher for above-median sales prices, although the difference is only 0.2%. Together, these results document that the tax channel is dominating in a wide range of market segments.

As mentioned earlier in the paper, using the absolute value of a relative measure, such as ARD, might be difficult to use, 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 ARD 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 AD instead. 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 models with a placebo publication date. This placebo date is set to 75 days prior to the actual publication. 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

Table 6
Robustness checks

Panel A: Alternative models and subsamples	Treatment × Post	Observations	Adj. R-sq.
Without NYC	0.010***	120,511	0.086
	(0.002)		
Larger municipalities only	0.016***	51,879	0.022
	(0.002)		
Below median sales price	0.009**	76,444	0.142
	(0.002)		
Above median sales price	0.011***	76,457	0.083
	(0.002)		
$ARD < 0.5$	0.009***	145,502	0.059
	(0.002)		
AD as dependent variable	0.078***	152,901	0.278
	(0.014)		

Panel B: Placebo tests	(1)	(2)	(3)
Post (placebo)	0.003	-0.001	0.006
	(0.002)	(0.004)	(0.007)
Treatment × Post (placebo)	0.006	0.007	0.007
	(0.003)	(0.004)	(0.004)
Municipality-tax-year fixed effects	X	X	-
Month fixed effects	-	X	X
Zip-code-tax-year fixed effects	-	-	X
Adj. R-sq.	0.071	0.071	0.109
Observations	66,686	66,686	66,686

This table documents the results of multiple robustness checks. Panel A shows estimates for the interaction term of treated group and post-EMV publication ("Treatment × Post") that informs about the causal effect of EMV publication on transaction prices, measured by the change in the dependent variable ARD, the absolute value of the sales-price-EMV 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. "$ARD < 0.5$" indicates an upper bound for the dependent variable ARD of fifty percent. For model "Larger municipalities only", only municipality-tax-year clusters with more than 1000 observations have been included. "Below (Above) median sales price" investigates subsamples of transactions with sales prices of less than (more than) the median nominal sample sales price (about 288,000USD). "AD as dependent variable" indicates that the (unstandardized) absolute distance between sales price and EMV was used as dependent variable. For all regressions in Panel A, the base case scenario with municipality-tax-year fixed effects was employed. Each column in Panel B shows a placebo regression with varying control variables. All regressions investigate observations before EMV publication. The "Post (placebo)" dummy was definend to split the pre-publication date into two sales with equal time window (75 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.

group in the dependent variable after the pseudo-publication date. That is, the placebo tests support the observation of common trends documented in Figure 5 and thus the validity of my DiD regressions.

8 Conclusion

Property taxes are commonly based on estimated market values (EMVs) of homes which are generated by local assessors. This paper analyzes whether these EMVs are themselves affecting trading prices of homes. Theoretically, two opposing channels should drive this effect. As EMVs constitute salient reference prices for market participants, an anchoring channel indicates a positive causal effect of EMVs on transaction prices. At the same time, an increase in EMV typically implies an increase in property tax payments. Thus, a tax channel should lead to a negative effect of EMVs on sales prices.

I analyze this ambiguous relationship by exploiting two features of the property taxation practices in New York State with a Difference-in-Differences framework. First, each municipality publishes EMVs at a particular date. Second, the revaluation frequencies across municipalities differ, such that units sold in non-revaluation years serve as control group.

My base case regressions show that, after the knowledge about the new EMVs, the absolute sales-price-EMV ratio increases by 1.1 percentage points. A back-of-the-envelope calculation indicates that this implies a 0.9% change in the transaction price of an average home. The findings are consistent with a theoretical model showing that changes in tax payments, induced by updated EMVs, affect trading prices in negative fashion. Subsamples on units purchased and sold by households with high or low effective tax rates support this hypothesis. The effect is robust to the choice of several subsamples and alternative model specifications. My results 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 sales prices.

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A Derivations

A.1 Derivations for the tax channel

The tax channel increases the absolute sales-price-EMV distance, AD, post EMV publication if

$$AD^{pre}(\bar{n}) \leq AD^{post}(\bar{n}) \tag{A-1}$$
$$\iff \bar{n}\left|p^* - \tau E[\hat{p}] - \hat{p}\right| \leq \bar{n}|p^* - \tau\hat{p} - \hat{p}| \tag{A-2}$$
$$\iff \left|p^* - \tau E[\hat{p}] - \hat{p}\right| \leq |p^* - \tau\hat{p} - \hat{p}|\,, \tag{A-3}$$

holds. The last equivalence follows as the total housing supply is larger than zero, i.e., $\bar{n} > 0$. $\tau E[\hat{p}]$ are the expected tax payments that are replaced with the true value, $\tau\hat{p}$, once the EMV is published (post).

Using $|a| \leq b \iff -b \leq a \leq b$, inequality (A-1) holds if

$$-|p^* - \tau\hat{p} - \hat{p}| \leq p^* - \tau E[\hat{p}] - \hat{p} \qquad \text{and} \tag{A-L}$$
$$|p^* - \tau\hat{p} - \hat{p}| \geq p^* - \tau E[\hat{p}] - \hat{p}. \tag{A-U}$$

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

$$p^* - \tau\hat{p} - \hat{p} \geq \hat{p} - p^* + \tau E[\hat{p}] \qquad \text{or} \tag{A-L1}$$
$$p^* - \tau\hat{p} - \hat{p} \leq p^* - \tau E[\hat{p}] - \hat{p}. \tag{A-L2}$$

Defining $p^{pre} := p^* - \tau E[\hat{p}]$ and $p^{post} := p^* - \tau\hat{p}$, it is easy to show that it follows from inequality (A-L1) that

$$\frac{p^{pre} + p^{post}}{2} \geq \hat{p}, \tag{A-L1'}$$

and from (A-L2) that

$$E[\hat{p}] \leq \hat{p}. \tag{A-L2'}$$

Proceeding similarly, it can be shown that (A-U) is true if either

$$\frac{p^{pre} + p^{post}}{2} \leq \hat{p} \qquad \text{or} \tag{A-U1'}$$

$$E[\hat{p}] \geq \hat{p} \tag{A-U2'}$$

holds. This means that (A-1) is fulfilled when one out of the two inequalities (A-L1') and (A-L2') holds together with either (A-U1') or (A-U2'). This leaves four combinations to check.

Inequalities (A-L1') and (A-U1') hold together if $\hat{p} = E[\hat{p}]$. The same is true for inequalities (A-L2') and (A-U2'). From these two pairs, it follows that AD remains unchanged when expectations match the outcome.

Now consider (A-L1') and (A-U2'). From (A-U2'), it follows that $p^{post} \geq p^{pre}$. Hence, when rewriting (A-L1') to

$$\frac{2p^{pre} + \epsilon}{2} \geq \hat{p}, \tag{L1'-U2'}$$

where $\epsilon := p^{post} - p^{pre}$ is the difference between expected EMV and actual EMV times the effective tax rate, it follows that (A-1) holds if

$$\hat{p} \leq E[\hat{p}] \leq p^{pre} + |\epsilon|/2 \tag{SC-1}$$

Proceeding similarly with (A-U1') and (A-L2'), one can show that (A-1) holds if

$$\hat{p} \geq E[\hat{p}] \geq p^{pre} - |\epsilon|/2. \tag{SC-2}$$

Both inequalities (SC-1) and (SC-2) are sufficient conditions for (A-1).

A.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 ARD between treatment and control condition, given as

$$\left|\frac{P^t - EMV}{EMV}\right| - \left|\frac{P^c - EMV}{EMV}\right| = \gamma. \tag{A2-1}$$

Note that the EMV is the same for both scenarios, mirroring the empirical application in which the contemporaneous EMV is used to derive the absolute EMV-sales-price ratio. For simplicity, assume that P^t and P^c are larger than EMV. Then, it follows from Equation (A2-1) that

$$P^t - EMV = P^c - EMV + \gamma EMV, \tag{A2-2}$$

which can be rearranged to

$$\Delta P_{ic} = \frac{P^t - P^c}{P^c} = \frac{\gamma EMV}{P^c}. \tag{A2-3}$$

To see that these calculations fit to the model presented in Section 3, note that the assumption made that P^t and P^c are smaller than the EMV 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 EMV instead, it follows that

$$\frac{P^t - P^c}{P^c} = -\frac{\gamma EMV}{P^c}, \tag{A2-4}$$

which is again in line with condition (11).

B 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 300 days around the publication date T_c. For all transactions that are observed for each c, I identify the particular property-specific EMV that was published at the corresponding date T_c. Thus, the EMV that is matched to a transaction i was unknown if i was sold in the 150 days before T_c, and known if i was sold in the 150 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 1,226,139 arms-length transactions from December 2006 (the earliest possible date to identify a matching EMV for) to December 2017 with non-missing sales price and available APN, I follow several steps of **?**. First, I remove all foreclosure related transactions (1,197,103 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 (1,140,298 remain). Similar to Bollerslev, Patton, and Wang (2016), I set fixed bounds on the nominal values of 5,000USD and 10,000,000USD. This ensures first, that scaling is appropriate for the regression analysis, and second, mitigates the right-skewness of the price distribution. This leaves me with a cleaned transaction sample of 1,136,978 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 chose 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.[11] Thus, observations for which the SWIS code is missing cannot be used and must be dismissed (1,135,873 observations remain). I analyze a time window of about five months around the publication date ($\pm$150 days). Transactions that do not fall in this window are also dismissed (991,763 remain). Finally, the transactions are matched to the tax records. Here, each transaction is assigned the up-to-date EMV that is (yet) unknown if the property transacted before publication (i.e., in the time window -150 to -1 days), and known afterwards (day 0 to 150). The matched dataset contains 620,090 observations.

[11] For 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.

For the analysis, one-year EMV lag is additionally necessary for two reasons. First, to identify the control group, which requires that no change in the market value has been made. Second, extreme changes in valuation are possible as the condition of a home could have changed significantly, e.g., through fire damage. Accordingly, I dismiss observations for which the prior year's EMV is either missing or for which the one-year EMV 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, 428,987 observations remain.

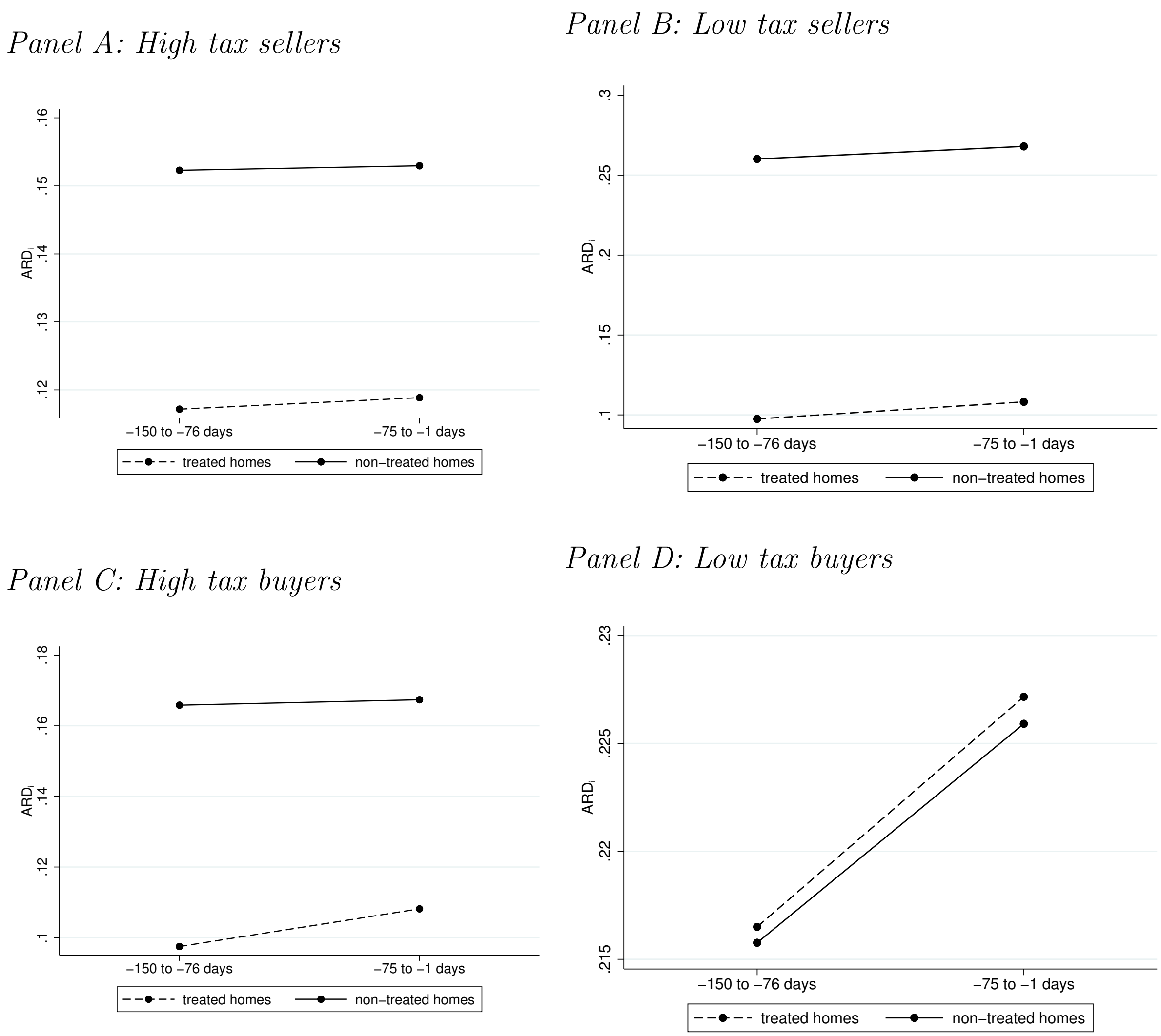
I identify treatment and control group in a data driven way. For each of the remaining observations, I calculate the one-year EMV return and round it up to the third decimal place to correct for small differences in returns. I identify three different types of municipality-tax-year clusters based on 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 EMV 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. After these steps, 260,589 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 EMV 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, 238,746 observations remain.

To increase comparability between control and treated 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 152,901 observations.

C Additional Figures

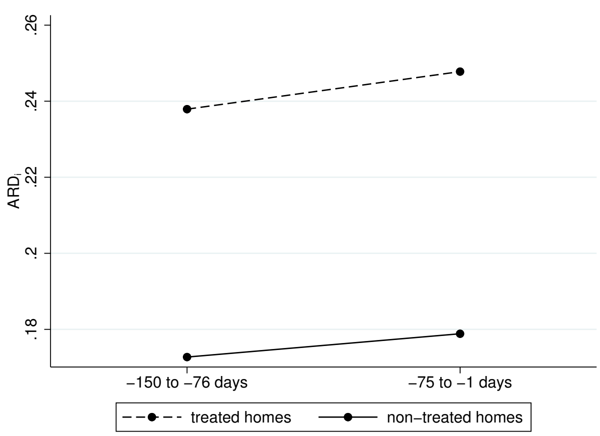
Figure A1
Trends of ARD_{ic} before treatment, subsamples by effective tax rate



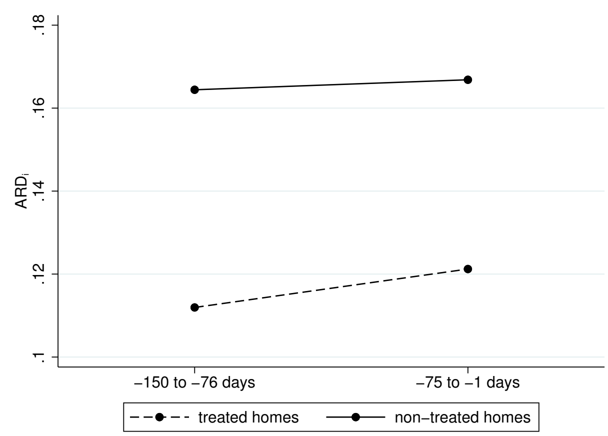
This figure shows the pre-publication development of the absolute sales-price-EMV ratio, ARD_{ic}, defined in Equation (13), for both treated (dashed line) and control units (solid line). Panel A (B) shows pre-publication trends for observations with sellers' effective tax rates above (below) the sample mean of the contemporaneous effective tax rate. Panel C (D) shows pre-publication trends for observations with buyers' effective tax rates above (below) the sample median.

Figure A2
Trends of ARD_{ic} before treatment by subperiods

Panel A: 2007-2010



Panel B: 2011-2017



This figure shows the pre-publication development of the absolute sales-price-EMV ratio, ARD_{ic}, defined in Equation (13), for both treated (dashed line) and control units (solid line). Panel A shows pre-publication trends for homes with EMVs published during the period of 2007 to 2010. Panel B shows pre-publication trends for the period form 2011 to 2017.