The Lock-In Effect of Housing Transfer Taxes: Evidence from a Notched Change in D.C. Policy

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Abstract

This paper estimates the medium-term lock-in effect of residential real estate transfer taxes by studying notched tax rate changes in Washington D.C., exploiting both a price and time notch as identifying variation. We provide evidence that there is substantial manipulation of the sales price in the region around the price notch, but there is little manipulation around the time notch. Ignoring this manipulation would lead us to substantially overstate the lock-in effect. Because using an instrument is not a satisfactory econometric solution in this context, we propose an alternative way of addressing the selection into treatment. We find that a 1 percent increase in the transfer tax rate decreases the rate of sales by 0.20 percent.

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1 Introduction

As of 2006, 34 states and the District of Columbia had a transfer tax on residential real estate transactions. Washington D.C. had the highest transfer tax rate of all, even before in 2006 it increased its tax rate from 2.2 to 2.9 percent of the sale price for home buyers with house sale prices above $400,000. Some counties and municipalities also impose transfer taxes. As with other transfer taxes such as capital gains levies, this type of tax makes selling a house more costly, and therefore may affect how often houses are bought and sold—the “lock-in” effect—as well as house value. Compared to capital gains taxes, transfer taxes based on a house’s selling price have received relatively little attention in the economics literature, but their analysis has some natural advantages as a way to learn about the lock-in effect, the focus of this paper.

To learn about the lock-in effect, this paper will examine recent changes in transfer tax policy in Washington D.C.\textsuperscript{1} In particular, we focus on two tax changes that introduced a “notch”—a discontinuous jump in tax liability—into the rate schedule. Prior to January 1, 2003, the transfer tax rate was 2.2 percent of the sale price. Beginning on January 1, 2003, the rate increased to 3 percent, but only for houses with a reported transaction price greater than or equal to $250,000. This created a tax notch because the higher tax rate applied to the entire sales price of the house if the house sold for $250,000 or more, so that increasing the sales price by one dollar, from $249,999 to $250,000, increased the tax due by $2,000. For homes valued in the region of the tax notch, this created a substantial incentive for the official selling price to not exceed $249,999 beginning on January 1. The higher 3 percent tax rate, and therefore this notch, was eliminated on October 1, 2004, but a new tax notch of $2,800 was introduced on October 1, 2006, which increased the tax rate to 2.9 percent on homes selling above $399,999. This notch still exists today.

\textsuperscript{1}In D.C., there are technically two separate transfer taxes of equal magnitude: a deed recordation tax and a deed transfer tax. It is standard for the purchaser to remit the former and the seller to remit the latter. Going forward, we refer to the transfer tax rate as the combined deed recordation tax and deed transfer tax rate.
We estimate the medium-term effect of the transfer tax on house sales and also address two related margins of behavioral response. Using the 2006 tax reform as an example, the related margins are: i) the acceleration of the date of sale of high-price (i.e., price \( \geq 400,000 \)) homes to before October 1, 2006, and ii) the manipulation of the sales price from above to below \$400,000 for sales made after October 1, 2006. The simultaneous presence of a discontinuous tax penalty based on both sale price and time provides an unusual and potentially insightful empirical challenge, what we call a “two-notch” problem.\(^2\) The price notch occurs at \$400,000 and the time notch occurs on October 1, 2006. We document the degree of manipulation on each notched dimension. We find that there is substantial manipulation at the price notch, but little evidence of manipulation at the time notch.

We estimate the lock-in effect using a difference-in-differences empirical strategy, exploiting the fact that all individuals faced the same proportional disincentive to sell when the tax rate was flat, but once a tax notch were implemented, those with houses above the price notch faced higher proportional disincentives to sell. Because the applicable tax rate (in other words, the treatment) is a function of the potentially manipulable house value, estimating the lock-in effect assuming that the observed tax rate is exogenously assigned would lead us to conclude that the higher tax rate induced a much larger lock-in effect than actually occurred. Using an instrument for the tax rate is not a satisfactory solution in this context, for reasons we discuss below. Instead, we redefine treatment in a way that eliminates the selection bias that would otherwise exist because of manipulation. We find that a 1 percent increase in the transfer tax rate decreases the rate of sales by 0.20 percent.

The paper proceeds as follows. Section 2 provides a brief review of related literature. Section 3 provides a model that motivates and provides an interpretation for the empirics in the sections that follow. Section 4 discusses the data used in this paper. Section 5 examines the manipulation at each notch. Section 6 estimates the lock-in effect. Section 7 concludes.

\(^2\)In the future, we plan to examine the discontinuity in tax treatment at the Washington D.C. border, posing a “three-notch” problem.
2 Related Literature

2.1 Capital Gains Taxes

A large literature has examined the impact of capital gains taxes on the volume of sales of assets subject to the tax—the lock-in effect—and on asset prices. An early literature (Feldstein et al., 1980) attempted to measure this using cross-sectional tax-return data, but the difficulties of identification in this context meant that subsequent empirical analysis focused either on aggregate time series analysis or panel data analysis. A difficult and recurring problem in this literature is to distinguish a short-term response to an anticipated tax change from a long-term response to a tax rate change perceived to be permanent or at least very long-lasting. Prominent examples are Burman and Randolph (1998), who use panel data and differences in state tax rates to attempt to separate the effects of permanent and transitory tax rate changes on capital-gains realization behavior, and Auerbach (1988), who examines aggregate annual U.S. capital gains realizations from 1954 to 1985.

Most of these studies focused, explicitly or implicitly, on capital gains from public-traded stock, and only a small minority of these studies have focused on housing markets. Because the nature of both the market itself and the tax treatment differs substantially between stock and housing markets, the empirical results about the former cannot be presumed to carry over to housing markets. Major changes in the U.S. income tax treatment of housing capital gains brought about by the Tax Relief Act of 1997, such as eliminating the “age-55” rule\(^3\) and excluding capital gains of up to $500,000 from taxable income, spurred much recent analysis, such as Cunningham and Engelhardt (2008) and Shan (2011). Shan (2011) finds that the sales rate of houses rose from between 19 to 24 percent for houses with price appreciation up to $500,000, but found no effect on house sales for houses with larger gains. Subsequent changes in the capital gains tax, in 2001 and 2003, are estimated to have reduced sales by between 6 and 13 percent for each $10,000 increase in capital gains taxes. Notably, Shan

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\(^3\)The age-55 rule allowed individuals to claim a one-time exclusion of $125,000 if they were over age 55.
(2011) finds that the short-term effect was much larger than the long-term effect, mirroring a common finding in analyses of corporate stock sales. This suggests that many previously locked-in homeowners took advantage of the tax rules immediately after its taking effect.

### 2.2 Transfer Taxes

Transfer taxes are similar to capital gains taxes in that tax liability is generally triggered by a transaction, and so arguably will induce asset owners to hold on to assets that they otherwise would want to sell. The key difference between the two taxes is that transfer tax liability does not depend on the appreciation of the asset since acquisition. O’Sullivan, Sexton, and Sheffrin (1995) observe that the effective annual transfer tax rate declines as the years of ownership increase. They find the inefficiencies associated with a transfer tax are substantially higher than with a simple property tax or acquisition-value property tax.

According to Dachis et al. (2012), as of 2004, 34 U.S. states, most Canadian provinces, Australian states, as well as the United Kingdom and France levy land transfer taxes and raise non-trivial revenue from these taxes. These authors examine the impact of a transfer tax on the housing market by studying the imposition in 2008 by Toronto of a 1.1 percent transfer tax, and find it caused a 15 percent decline in the number of sales of houses and a decline in housing prices about equal to the tax increase. Their methodology relies on a difference-in-differences regression discontinuity design—comparing the changes in house sales (and house values) within and outside the Toronto border, before and after the tax change.

### 2.3 The Econometrics of Notches and Regression Discontinuity Designs

The regression discontinuity (RD) research design, laid out in Imbens and Lemieux (2008), relies on two critical assumptions. The first is that, absent a change in policy, the dependent
variable would change continuously at the policy discontinuity cut-off. The second is that what determines which side of the cut-off a particular observation falls in—that is, which treatment an observation receives—is not subject to choice or manipulation. To a public finance economist, the second assumption of the RD design is a bit disconcerting, as generally the behavioral response to policy is the principal object of interest. Thus the RD design works as intended when the assignment to a policy treatment is random and unmanipulable, at least locally, near the discontinuity of treatment.

When the policy treatment changes budget sets (as opposed to administering a discrete treatment such as a job training module), there are two possible types of treatment. In the first, the slope of the budget set changes discretely, creating (or altering) a kink in the budget set. In the second type, the budget set changes discontinuously, producing a notch in the budget set. (If the two pieces of the discontinuous budget set have different slopes, there is an element of both notch and kink). Tax policy that changes at a point in time creates a time notch, because the budget possibilities change discretely whether an action is taken just before the policy change or just after. Property taxes and transfer taxes often feature a price or value notch, in which a flat tax rate that applies to all prices or property valuations and then jumps discontinuously at a certain property value.\(^4\) In this case, too, the budget set changes discontinuously at the cut-off value. If the policy varies across jurisdictions, border notches arise as well.\(^5\)

One key problem with applying the RD design to tax policy notches is that, in many but not all contexts, there is a behavioral response in the variable subject to the policy discontinuity both due to the varying treatment but also due to the fact that people try to escape the higher-tax treatment entirely. For time notches, people can speed up taxable transactions when taxes are going up, and postpone them when taxes are declining; in the capital gains tax literature, this type of response has been labeled the “short-term” effect

\(^4\)Another property tax notch example cited in Slemrod (2010) is the Israeli municipal property tax, which has separate tax rates per square meter for different size categories.

\(^5\)Slemrod (2010) discusses other kinds of notches.
of anticipated tax changes. For value notches, they can reduce or under-report the value to be assigned the lower tax rate. For border notches, they can move their purchases, business locations, or even residences from one side of the border to the other. These behavioral responses are of interest in and of themselves, because they affect revenue and cause deadweight loss. Moreover, in many contexts, no response of the “running variable” is a strong indication that we should expect to find no response to the treatment. However, in principle, this response of the running variable invalidates the pure RD approach, and requires other methods that account for the manipulation while retaining as much as possible the advantages of the RD design.

The second key problem with applying the RD design to tax policy notches is that the expected treatment often does not change discretely at the notch. In particular, because the treatment and outcome are realized simultaneously, individuals may not know with certainty whether they are in the treatment or comparison group until they receive their treatment, by which point they may have already make key choices; thus, as the time notch date approaches, the expected treatment on each side of the notch will converge. For example, in the house transfer tax context, individuals do not know for sure whether their house will sell before or after the time notch until it has sold. We discuss the implications of this uncertainty in this context in more detail in Section 3.3.

3 Model

Whether or not a house is sold is a function of the transfer tax rate. The transfer tax is a transaction-based tax so, all else equal, it provides a disincentive to buy or sell a house and this disincentive gets larger as the tax rate increases.6 Lastly, a relatively high transaction tax in Washington D.C. will make houses just outside of D.C. more attractive. The degree of lock-in is determined in a buyer-seller equilibrium.

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6Note that this is slightly different than a lock-in effect due to capital gains taxation, because the transfer tax is based on the gross sales price, whereas the capital gains tax is based on the appreciation in price since the house was last purchased.
We describe the decision primarily from the perspective of the seller and, until Section 3.3, assume there is a buyer for each house at all prices less than or equal to the true value; we relax this assumption in Section 3.3 and consider what effect this has on what sellers know when deciding whether to sell their houses. The transaction tax presumably also affected the equilibrium price of housing, based on the expected tax liability over the lifetime of the house, which in turn depends on expected turnover and the relative elasticity of the supply and demand for housing in the Washington D.C. area.\textsuperscript{7} We assume that such market effects occurred at the time of announcement, not upon implementation.

In Section 3.1, we model the seller’s problem assuming that the seller cannot understate the true sale price. In Section 3.2, we relax this assumption, and allow for price manipulation and side payments. Section 3.3 introduces uncertainty and the buyer’s decision.

### 3.1 No Price Understatement

Each homeowner has a reservation utility level of staying in their house, $U$, and will move if by doing so she can obtain a higher utility level. In this section, we assume that if individuals sell their houses, they receive its true value, $V$, minus their share of the tax remitted, where that share is assumed to be exogenous\textsuperscript{However, individuals can alter the value of their house before they sell it, by choosing the amount of home improvements, $I (I \geq 0)$, to make before they sell their house (e.g., putting in new carpet).\textsuperscript{8} They can also alter the time at which they sell their house, $T$. The maximization problem faced by an individual considering selling their house is given by:

$$
\max_{I,T} \max U(Y_I, T), U,
$$

\text{(1)}$

\textsuperscript{7}For simplicity, the possibility of the sale in question is assumed to be in addition to the expected future sales that determine the tax capitalization upon announcement.

\textsuperscript{8}They might, for example, expend resources to “stage it”, i.e., put in temporary new furniture and other interior decorating to make it appear more attractive.
where \( Y_I \) is the after-tax proceeds from selling the house. Let \( Y_I \) be given by:

\[
Y_I = V(V_0, I) \cdot [1 - \gamma \tau(\rho V(V_0, I), T)] - b(I),
\]

(2)

where \( V_0 = V(I = 0) \) is the valuation with no improvements, \( \tau(\cdot) \) is the tax rate given the choice of \( I \) (through its effect on \( V \)) and \( T \), and \( b(I) \) is the cost of making improvements, such that \( b \geq 0, b(0) = 0, b'(\cdot) > 0 \) and \( b''(\cdot) > 0 \). The parameter \( \gamma \) is the share of the tax \( \tau \) that is borne by the seller and \( \rho \equiv 1/(1 - \tau(1 - \gamma)) \) is a multiplicative factor increase in the price paid by the buyer on which the tax rate is based. Note that \( \tau(\cdot) \) is discontinuous at the value notch, \( V_n \), and the time notch, \( T_n \). The first-order condition with respect to \( T \) for an interior solution implies:

\[
[\partial T :] \frac{\partial U}{\partial T} / \frac{\partial U}{\partial Y_I} = \gamma \frac{\partial \tau}{\partial T} \cdot V.
\]

(3)

The left-hand side of equation 3 defines the slope of the indifference curves between net proceeds and time of sale, as shown in Figure 1. The slope is negative before \( T^*_{nt} \); it equals zero at \( T^*_{nt} \), and it is positive thereafter, where \( T^*_{nt} \) is the optimal time of sale in the absence of tax incentives to postpone or accelerate the sale. The term \( \partial \tau / \partial T = 0 \) everywhere, except at \( T_n \), where it is infinite.

Figure 1 depicts the choice of \( T \) for a given \( I \), where \( V_0 \geq 400,000 \); that is, the seller cannot alter the house value to get it below the price notch. The figure shows that everyone with \( T^*_{nt} \) between \( T_n \) and \( T_n + dT_n \) will sell their house at time \( T_n \). As individuals have weaker sale date preferences (i.e. their indifference curves get flatter), more and more individuals will “bunch” at \( T_n \); that is, they will move up their sale date to \( T_n \), creating excess mass at that date.

Individuals sell if \( U \geq \bar{U} \), where \( \bar{U} \) varies across people. This condition is met for fewer individuals after the higher tax rate is imposed. For example, in Figure 1, all individuals with sale date preferences depicted by \( U_H \) and a reservation utility between \( U_H \) and \( \bar{U}_H \) will
no longer sell their house after the tax notch is imposed. This is a depiction of the lock-in effect. Note that, for all individuals with sale date preferences between those depicted by $U_L$ and $U_H$, the extent of lock-in will be lower. This is because the utility loss from the tax notch is mitigated by the option of bunching at $T_n$. We term the amount of lock-in in this region the short-run lock-in effect and we term the lock-in effect in the region above $T_n + dT_n$ (where individuals always choose sale date $T_{ni}$) the medium-term lock-in effect.

The first-order condition with respect to $I$ for interior solutions implies:

$$\left[\partial I :\right] \frac{\partial V}{\partial I} \left[1 - \gamma (\tau + \rho V \cdot \frac{\partial V}{\partial \cdot})\right] = \frac{\partial b}{\partial I}. \quad (4)$$

The term $\frac{\partial \tau}{\partial V}$ is equal to zero everywhere except at $V_n$, where it is infinite. So, equation (4) is defined everywhere except at $V_n$ and therefore can be simplified for interior solutions to:

$$\left[\partial I :\right] \frac{\partial V}{\partial I} \cdot [1 - \gamma \tau] = \frac{\partial b}{\partial I} \quad \forall V \neq V_n. \quad (5)$$

Figure 2 depicts the terms of the choice of $I$ for a given $T$, where $T > T_n + dT_n$, so that moving up the date of sale is not an attractive option. Note that the objective is to maximize net proceeds, so the relevant indifference curve in the bottom panel is horizontal. The individual whose $Y_I(\cdot)$ curves are depicted in the bottom panel of Figure 2 is exactly indifferent between bunching at $I(V_0, V_n)$, so that $V = V_n$, and choosing his optimum without bunching $I(V_0, V_n) + dI(V_0, V_n)$. Individuals only consider bunching when $V_n$ is below (but near) the house value at their optimal choice of $I$ without bunching. How near $V_n$ must be to the optimum without bunching to induce individuals to bunch at $V_n$ depends on the shape of $Y_I(\cdot)$. As the curvature of $Y_I(\cdot)$ gets steeper, the number of individuals located at $V_n$ decreases. For a given $T$, let $Y_I^*$ be defined as the amount of net proceeds required to obtain the seller’s reservation utility, $U(Y_I^*, T) = \bar{U}$.

The bunching at $I(V_0, V_n)$ shown in Figure 2 directly implies that there will be bunching at $V_n$ because individuals will choose not to make an additional marginal improvement.
around the notch, leading to an excess sale of homes whose value is exactly \( V_n \). In this diagram, lock-in increases after the reform to the extent that \( \bar{Y}_I \) is between the \( Y_I \) chosen before and after the tax notch is introduced. We can distinguish a lower local lock-in effect in the region of the notch where individuals choose to bunch at \( V_n \) and obtain a higher \( Y_I \) than would otherwise be possible. Once individuals no longer gain by bunching at \( V_n \), the full medium-run lock-in effect can be measured. The analysis in this subsection has assumed a constant sales price for a given \( V \), and therefore only considered a static mechanism for the seller to alter \( V \). If we relax this assumption to let houses appreciate (depreciate) with time for a given \( V \), individuals may pick an earlier (later) time to sell so that the house value when they sell is less than \( V_n \).

### 3.2 With Price Understatement

Now we introduce the possibility that a selling price below the true selling price, gross of tax, is reported. We consider two variations. First, sellers may simply choose to accept a price below what the market would bear. Second, a seller may make an agreement with a buyer such that the reported sale price will be lower than the market price and below the price notch. As part of the agreement, the buyer will make a side payment to the seller.

Without side payments, are their circumstances in which it is optimal for the seller to sell the house for a price less than what the market would bear to avoid the higher tax? Figure 3 shows that it may be optimal to do so but, because sellers have some control over \( V \) by choosing the amount of improvements, they only want to sell their house for \( R < \rho V \) if, with \( R = \rho V_0 \), they are between \( R_n \) and \( R_n + dR_n \) in Figure 3.

Next we introduce the possibility of understating the reported selling price and combining this with some form of side payment. For example, suppose the pre-reform market value—and by assumption the price—of a house is $405,000. The seller could agree to “officially” sell the house to the buyer for $399,999 to avoid the higher tax rate, combined with a side payment to the seller for some fraction of the $5,001 by which the official sales price fell
short of the true value. Note that a side payment could take the (illegal) form of simply exchanging more money under the table, but could also take on (possibly) legal forms of tax avoidance, such as shifting the remittance responsibility of the brokerage fees or closing costs to the buyer. Reducing the reported transaction price decreases tax liability and therefore increases the after-tax proceeds. The increase in after-tax proceeds is a continuous function of the understatement except if the understatement moves the seller from above to below the price notch \( V_n \), where at the margin it saves \( (\tau_1 - \tau_0)V \). There is a cost of understating, which is likely exacerbated by the need to coordinate the understatement with the buyer and presumably the real estate agent. In addition, for illegal understatement, it includes the expected cost of detection and punishment as well as a possible psychic cost from being dishonest or displaying a lack of civic-mindedness. We assume that the cost of understatement is borne by the seller in the same fraction as any other cost, \( \gamma \). Given that the seller bears the whole cost, there is always a buyer willing to make side payments. We denote this cost as \( c(\rho V(I, V_0) - R) \), where \( c(0) = 0 \), \( c'(\cdot) > 0 \), \( c''(\cdot) > 0 \), and \( \frac{\partial c}{\partial V} = \frac{\partial c}{\partial R} \); thus the cost increases, at an increasing rate, with the amount of understatement. Note that if all evasion were costless everyone would report selling their house for a price of $0, and the seller would be compensated using side payments. Below, we introduce the notation \( s \) to denote the fraction of the understated value \( V(V_0, I) - R \) the buyer pays to the seller as a side payment.

Now the seller chooses the level of improvements, time of sale, and also the reported price to maximize:

\[
\max_{I,R,T} \left[ \max_{U} U(Y_{IR}, T), U \right],
\]

for a given \( V_0 \) and \( U \). Let \( Y_{IR} \), the net proceeds from a house sale in the presence of price manipulation, be defined as:

\[
Y_{IR} = R \cdot [1 - \tau(R, T)] + s \cdot [\rho V(V_0, I) - R] - b(I) - \gamma c(\rho V(V_0, I) - R).
\]
The choice of $T$ is the same as before except that now sellers are potentially interested in moving up the sale date to $T_n$ if the reported transaction price is above the price notch, $R > R_n$ (rather than $V > V_n$). The first-order conditions for the interior solutions of $I$ and $R$ are given by:

\[
\begin{align*}
[\partial I] & : \rho (s - \gamma c') \cdot \partial V / \partial I = \partial b / \partial I. \\
[\partial R] & : 1 - s + \gamma c' = \tau + R \cdot \partial \tau / \partial R.
\end{align*}
\]

Combining these two first-order conditions gives:

\[
\rho \partial V / \partial I \cdot [1 - (\tau + R \cdot \partial \tau / \partial R)] = \partial b / \partial I,
\]

and because $\partial \tau / \partial R = 0$ at every point except $R_n$, where it is infinite, we can further simplify this equation to get:

\[
\rho \partial V / \partial I \cdot [1 - \tau] = \partial b / \partial I \quad \forall \ R \neq R_n.
\]

Up to this point, we have analyzed the choice of $T$ assuming $R$ is far above $R_n$ and the choice of $I$ and $R$ assuming $T$ is far above $T_n$; that is, we have examined cases where $t$ is a function of $I$ and $R$ or $T$, but not both. Now, we consider how individuals respond if they are in the neighborhood of both $T_n$ and $R_n$. Let the individual’s choice in $(Y_{IR}, R)$ space be depicted in the top panel of Figure 4. It is clear that, holding $T$ fixed above $T_n$, individuals facing these tradeoffs should reduce the reported selling price of their house to $R_n$. Holding $R$ fixed above $R_n$, the pre- and post-tax reform values of $Y_{IR}$ are given by the thick blue and gray horizontal lines in the bottom panel of Figure 4. If the individual decreases $R$ to $R_n$, the post-tax reform lines change to the thinner yellow and green lines in the bottom panel of Figure 4. Given the black solid net proceeds time-of-sale indifference curve depicted in the bottom panel of Figure 4, this individual would have chosen to shift up to the red-dashed indifference curve and bunch at $T_n$, holding $R$ fixed above $R_n$. Now that $R$ has been decreased to $R_n$ the individual is indifferent between choosing the pair $(R_{nt}, T_n)$.
and the pair \((R_n, T_{nt})\). Conditional on the depicted sale date preferences, all individuals whose realized gain from decreasing to \(R_n\) is greater than depicted in this figure will choose the pair \((R_n, T_{nt})\); that is, they will bunch in the \(R\), but not the \(T\) dimension. All individuals whose realized gain from decreasing to \(R_n\) is less than depicted in this figure will bunch in the \(T\), but not the \(R\), dimension. Conditional on the depicted gain from shifting to \(R_n\), all individuals whose sale date indifference curves are steeper or for whom \(T_{nt}\) is further to the left will choose the pair \((R_n, T_{nt})\); that is, they will bunch in the \(R\), but not the \(T\), dimension. Those whose sale date indifference curves are flatter or for whom \(T_{nt}\) is further to the right will bunch in the \(T\), but not the \(R\), dimension.

As this example illustrates, the number of individuals who bunch around each notch will decrease for individuals located near both notches. Which notch will experience the largest decrease in bunching depends on the gain from moving to \(R_n\) relative to the gain from moving to \(T_n\). The presence of the two notches will decrease the short-run lock-in effect in both the time of sale and sale price dimensions because individuals for whom it is relatively costly to avoid the higher tax in one dimension may choose to avoid the higher tax by adjusting their behavior along the other dimension instead.

### 3.3 Effective After-Tax Sales Price Smoothing and Uncertainty

The price notch introduces a discrete increase in the transfer tax rate for houses sold at a price above the notch (and the same is true for the time notch), but it would be misleading to assume in the subsequent empirical analysis that, in practice, the notches introduce a discrete increase in expected effective tax liability for the seller at the notched value.\(^9\) The reason is two-fold. First, to the extent that sellers choose \(R < V\) and are not compensated by a side payment from the buyer, the average effective tax rate on \(V\), the sales price of the house absent the tax notch, is between the tax rates above and below the notch. In the limit approaching \(R_n\), the effective tax rate above and below \(R_n\) is the same.

\(^9\)We continue to assume that nominal incidence and real incidence coincide and the discussion that follows considers the price notch, but the same analysis also applies to the time notch.
Second, up to this point, we have ignored the cost of initiating the process of selling a house, i.e. “putting it on the market.” Sellers will only incur this cost if they believe it likely that a sale will provide additional utility, such that:

$$
\mathbb{E} [U(Y_{IR} - z, T)] > \bar{U},
$$

where $z$ is the cost of initiating a sale. If we continue to assume that there is always a buyer at $R \leq V$ (and the seller knows $V$), there is no uncertainty upon initiation of the sale. However, if we relax this assumption, there is uncertainty about a potential buyer’s reservation-price time-of-purchase frontier. Regarding the choice of $T$, which is now the purchase date, a buyer’s tradeoffs can also be depicted by Figure 1, where $Y$ is now the cost of purchasing a house of a given value $V$. Figure 2 also accurately depicts the buyer’s decision regarding the value of $I$, where $b(I)$ is now the cost of acquiring a home with additional home improvements relative to $V_0$. It is important that the cost of acquiring a home with these improvements is less than the cost of making them himself; that is, the seller bears part of the burden of the costs of making these renovations before selling the house. Otherwise, in the region of the notch, the buyer would always prefer to buy a home at $V_0$ and make the renovations himself. As we mentioned before, once price understatement is introduced, the buyer is always willing to engage in price understatement if the entire cost is born by the seller. If the costs are split between the two parties, then the buyer will be willing to engage in this activity sometimes, and the decision is modeled analogously to the seller’s decision.

Now that sellers must form an expectation over the utility they will gain by selling their house to determine whether the sale is expected to provide utility exceeding $\bar{U}$, they must form an expectation about the after-tax sales price of the house, $\mathbb{E}[R(1 - \tau(R))]$, where $\tau(R)$ is the transfer tax rate. To the extent that individuals do not reverse their decision once they have incurred the fixed cost $z$, the relevant treatment determining whether a given house sells is $\mathbb{E}[R(1 - \tau(R))]$. This expectation is arguably smooth around the price notch.
Therefore, individuals with house selling prices below $400,000 face an expected tax rate that is above the comparison group tax rate and individuals just above $400,000 face an expected tax rate that has not yet reached the treatment group tax rate. The same analysis also applies to the time notch.

For these reasons, in the neighborhood of the notch, the expected effective treatment above and below the notch is approximately the same. Because there is neither a discrete change nor a change in the slope of the expected effective treatment at the notch, neither a regression-discontinuity nor a regression-kink design are feasible designs to learn about the medium-run lock-in effect, even if we could observe the price absent manipulation.

4 Data

The model of the decision processes of buyers and sellers presented in Section 3 does not quantify the short-term or medium-term lock-in effect, which depends on the share and distribution of indifference curves and reservation utilities. To measure these effects, we now turn to empirical analysis. This section describes the data used in the empirical analysis in Sections 5 and 6. The data are merged together from two sources and provide detailed information on housing characteristics and transactions.

The first source is housing characteristics data for all residential properties in D.C. These data were provided by the D.C. Office of Tax and Revenue, and contains property characteristics and property tax assessment data for all residential properties in D.C in 2005 and 2010. We exclude houses on which there are no housing characteristics, which eliminates less than 0.29 percent of houses that we otherwise observe because they sold at least once. The housing characteristics we use in this paper include: living area, number of rooms, number of stories, number of bedrooms, number of full and half bathrooms, type of flooring, type of roof, type of exterior, number of fireplaces, type of heat, whether there is air conditioning, condition, grade, and year built. We also observe the assessor valuation. For now, when we use housing
characteristics in our analysis in Sections 5.2 and 6, we exclude condominiums (22.3 percent of the data) because the available set of housing characteristics for condominiums is very different.\textsuperscript{10}

The second source, which can be linked to the first by a unique house identifier, provides data on the sales price for all residential housing transactions in D.C. in the years 1999 to 2010. This data set is purchased from CoreLogic, a firm that specializes in real estate data and analysis. This source also has some housing characteristics; in particular, we use neighborhood location and acreage in our analysis. We exclude all houses that apparently sold more than once per day because these are in general duplicate observations, not actual additional transactions, or one of the two sales is a non-arms length transaction at $0 dollars; in this case we always drop the observation with a sales price of $0.

Table 1 provides summary statistics for all covariates used in the analysis (the unit of observation in these summary statistics is a house).\textsuperscript{11} In the empirical analysis that follows, we exclude houses that never sell because we never observe a sales price for these houses, which we need in order to conduct our analysis.\textsuperscript{12} Thus, this analysis is really about a medium-run lock-in effect: when within a 12-year period a house sells. The left panel displays all the summary statistics for homes that sold at least once during the period 1999 to 2010 and the middle panel displays the same characteristics for homes that did not sell in this period. The right panel displays the difference in means of the two groups and reports the degree of significance of this difference. Not surprisingly, homes that did not sell in this period are different in a variety of dimensions relative to those that sold. For this reason, generalizing the results we find in this paper to homes that never sold is not necessarily appropriate. However, because these homes never sell, excluding them in all years from the regression analysis we conduct below does not bias our estimate of the medium-run lock-in

\textsuperscript{10}We do include condominiums in the analysis in Section 5.1 because covariates are not needed in the analysis. The results are very similar with and without the inclusion of condominiums.

\textsuperscript{11}At this time, we do not include summary statistics of neighborhoods because there are 56 neighborhoods, which is too many to be informative as simple summary statistics. We will include a map in future drafts.

\textsuperscript{12}As we discuss in more detail in Section 6, there is an alternative procedure that would not require this restriction, but we expect that this method will substantially increase noise in the estimates.
Figure 5 displays the median price of houses that sold by year for all houses that are in our sample based on the sample restrictions described above. The figure highlights that prices changed substantially over the period 1999-2010. Most strikingly, the median house sale price more than doubled from 2001 to 2005. At the time the price notch was introduced in 2003, the notch was placed roughly at the median sale price and by 2006 when a notch was re-introduced at a point $150,000 higher, it was also located approximately at the current median sale price. After reaching its peak in 2007, the median price declined until 2009. We will consider the changes in average price across time when conducting our empirical analysis.

If there were other tax changes during the same time period that affected those above and below both the price and time notches differently, these changes would bias our difference-in-differences estimates of the lock-in effect due to the changes in the housing transfer tax. Also, if any of these taxes changed discretely at the price notch, then the estimates of the bunching would be biased. The DC First-Time Homebuyer Credit provides a non-refundable income tax credit to first-time DC homebuyers. This credit is determined by adjusted gross income (AGI), so it does not change discretely at the price notches and this credit did not change over time. The property tax changed over time, but applies uniformly to all houses in our sample. The Homestead Deduction exempts assessed value up to a given threshold from property tax. The exemption amount changed over time, but it applies uniformly to all houses in our sample. Also, the exemption amount is low, so no houses in our analysis will be exempted from paying property taxes because of the Homestead Deduction. The Assessment Price Cap puts an annual limit on the appreciation of assessed value on which property taxes can be assessed. The existence of the cap may create some lock-in effects of its own, and it also changes around the same time as the 2006 tax notch was introduced. If all houses appreciated at the same rate, then it wouldn’t matter, but if more expensive homes appreciated faster then it may matter. At the median house price, the change doesn’t
matter because by 2007 prices had flattened out and were declining by 2008 so the cap was not binding. But, perhaps it mattered for some homes that experienced more extreme appreciation. We will consider this possibility more carefully in the future.

5 Examining the Response around the Price and Time Notches

In this section, we look at the degree of bunching induced in the region surrounding each notch, as well as other changes in the price-to-value distribution of house sales after the tax reform. This analysis is both informative in its own right, and it will also motivate the empirical strategies employed in the next section to estimate the lock-in effect.

5.1 Bunching Analysis

This section examines the degree of bunching induced in the region surrounding each notch. We can observe the volume and distribution of sales prices before the tax change, during the post-announcement but pre-implementation period, and also after the tax change has been implemented. The non-uniformity of this density would make it difficult to analyze without the actual pre-tax-change counterfactual density.

After the price notch is implemented, we expect the density of house sales newly taxed at the higher rate to decline for two reasons: i) some individuals will manipulate the sales price of their house to a price below the price notch, and ii) the higher tax rate above the notch is expected to discourage some sales due to an increased lock-in effect. Similarly, we expect the density to the right of the time notch (i.e. after the tax increase) to decline for two reasons: i) some individuals will manipulate the timing of their house sale to a point before the time notch, and ii) the higher tax rate above the price notch is expected to increase the lock-in effect.

In order to provide a sense of the data, we begin by presenting histograms of house sales
in the region surrounding the price notch for the 2006 tax reform. Figure 6 shows house sales histograms in the region around the notch at $400,000. The figure on the left includes all house sales for one year prior to announcement of the reform (April 1, 2005 to April 1, 2006). The figure on the right includes all house sales for one year post-reform (October 1, 2006 to October 1, 2007). The bin size is $1,420. Recall that we should expect no seller to be in the first bin to the right of $400,000 because every seller would be better off by simply decreasing the price to $399,999. However, there are 41 individuals in this bin, 38 of whom sold their house for exactly $400,000. Even with the 41 apparently irrationally priced transactions, one can observe a “hole” in the density of house sales just above $399,999 in the post-reform year relative to the pre-reform year—41 houses compared to 114 houses. The hole in the density of house sales prices does not end at $401,420. In particular, there are only 2 or 3 individuals in each of the next two post-reform bins, compared to 8 and 21 pre-reform.

Figure 7 makes the pre-post reform comparison more formally. It plots the difference in the house sales density after the reform relative to before the reform using a smoothed local polynomial. Using this estimated density, we investigate whether there is a significant break at each notch a procedure proposed by McCrary (2008). There is an 85.3 percent decline in the density going from just below to just above the notch (in the limit) and is significant at the 1 percent level. To the extent that all price manipulation occurs within a narrow region of the notch, this can be interpreted as a 42.65 percent decline in the density just above the price notch and a 42.65 percent increase in the density just below. This approach is valid to the extent that there would be same density of houses in the region of a sale price of $400,000 in the two years of the difference absent the introduction of the tax notch. To the extent that there is a large change in house prices between these two years, this assumption may be violated. This concern might be addressed by adjusting prices so that they are in real, rather than nominal dollars. However, in this context such an adjustment would then

\[^{13}\text{The months from April to October, 2006 are not part of the analysis because during this period there was anticipation of the upcoming tax change. This time period will be examined separately below.}\]
make the bunching at round numbers no longer line up across years. For this reason, the bunching analysis presented in this section relies on the 2006 reform because prices did not change much in the year before and after this reform was introduced (see Figure 5). That said, we have also examined the 2003 reform and there is significant evidence of bunching in response to that tax notch, too.

Figure 8 plots the density of house sales price in a larger region around the price notch one year prior the announcement of the 2006 reform (Panel A), one year after the reform (Panel B), and two years after the reform (Panel C). The shift in the density from just above to just below $400,000 between Panel A and B is consistent with the evidence presented in Figure 7. However, other than an adjustment in prices right around the notch, there is no clear evidence of a change in the extent of lock-in because there is no clear decline in the density above $400,000. Recall from Section 3 that significant bunching at the price notch provides information regarding individuals’ willingness to manipulate their price below the notch, but does not necessarily reveal much information about the medium-term or long-term lock-in effect. However, Panel C, which looks at the density in the second year after the implementation of the tax notch, provides clear evidence of an increase in the extent of lock-in—ignoring the manipulation in the immediate region around $400,000, there is a statistically significant decline of houses selling for a price just above the notch at $400,000. We could estimate the jump in the density excluding sales in the immediate vicinity of the notch, but we know from the RD literature that when there is substantial heaping—natural bunching at round numbers (Barreca et al., 2011)—the RD estimate will be biased. Moreover, housing transaction data is imperfectly heaped in such a way that dropping the heaped data and estimating an RD parameter from the remaining continuous data (the method proposed in Barreca et al., 2011) is not feasible. For the same reason, it is likely not possible to control for the bunching within the regression (the method proposed in Kleven and Waseem, 2012). Therefore, we take this evidence as suggestive of a lock-in effect, but do not pursue this analysis further.
In Figure 9, we examine the anticipation period. In Panel A, we look at the window from May 10, 2006 to October 1, 2006 in which arguably the tax rate change was anticipated, but not yet implemented. The sales in this time period are compared to sales for May 10, 2005 to October 1, 2005. Notably, there was a decline in the three bins just above $400,000 relative to the other nearby bins, suggesting that individuals may have began bunching during the anticipation period. Panel A of Figure 10, which was constructed in the same manner as Figure 7, further corroborates this story, showing that there was not a large change in lock-in during this period, but bunching begins to be evident. In Panel B of Figures 9 and 10, we compare a two-month window before and after the time notch. These figures suggest that there was a decline in house sales just above $400,000 after the reform was implemented, and that there was an increase in bunching after the price notch was introduced.

Figure 12 applies the same technique to the time notch. Here we plot the smoothed difference in the frequency of house sales around the time notch relative to the year before. This figure includes all house sales above $350,000. We include some houses that sold at a price below the price notch (for whom the time notch is irrelevant) because if we excluded those just below $400,000, it would appear that there was a decline in house sales after the reform because of the price manipulation that occurs after the reform. This figure does not provide substantial evidence of manipulation around the time notch. Perhaps part of why we do not observe substantial bunching in a narrow region around the time notch is because there is a fixed supply of real estate agents, lawyers, deed specialists, and other professionals needed to complete transactions. Additionally, there is an important sequence of events in house buying and selling, which may make shifting the sale before time notch difficult and requires substantial advance planning.

5.2 Hedonic Analysis

Recall from Section 3 that the price manipulation we observe could be due either to individuals adjusting the value of their home $V$ below the price notch by, for example, altering...
the amount of home improvements done before selling the house, or, alternatively, by leaving $V$ fixed and decreasing $R$. To assess the degree to which the manipulation is due to a manipulation of $V$ or $R$, we estimate a hedonic value for $V$, which we term $\hat{H}$, where $\hat{H}$ is the predicted value from a regression estimated using all reported sale prices $R$ one year before the notched 2006 tax reform (April 1, 2005 to April 1, 2006) and year 2005 housing characteristics $X$, both of which should be unaffected by price manipulation incentives that exist once the price notch is introduced the following year, as follows:

$$R_i = \beta_0 + \beta_1 X_i + u_i. \quad (12)$$

so that

$$\hat{H}_{it} = \hat{\beta}_0 + \hat{\beta}_1 X_i. \quad (13)$$

Figure 11 plots the difference in the estimated hedonic house valuation after the reform (October 1, 2006 to October 1, 2007), relative to before the reform (April 1, 2005 to April 1, 2006). The increase in the difference in valuation below the notch suggests that after the notched reform, the true value of houses sold at a price just under than $400,000 was substantially higher than before the reform (and substantially higher than just above or below that region). This provides some evidence that individuals kept their sale price below the price notch by manipulating $R$, but leaving $V$ relatively fixed. Additionally, the large magnitude of the increase in the difference in valuation below the notch suggests that some individuals have true house values that are relatively far from the price notch (and therefore are presumably engaging in some sort of side payment between the buyer and the seller).
6 Estimating the Medium-Term Lock-in Effect Parametrically

The previous section provided substantial evidence of manipulation around the price notch and a bit of suggestive evidence of medium-term lock-in, particularly after the reform had been in place for a year. This section focuses on a parametric estimation of the medium-term lock-in effect controlling for other factors that could have biased the non-parametric estimates presented above. In our empirical analysis in this section, we take into account the fact that there is substantial manipulation of the applicable tax rate by altering the sale price and (to a lesser extent) time of sale in the region near the notches.

We define the dependent variable $\text{sell}_{it}$ as a one-zero indicator for whether house $i$ is sold in month $t$. The increase in the lock-in effect from the tax notches we examine is the amount by which $\text{sell}_{it}$ changes, on average, in response to the increases in tax liability above the notches. To identify the lock-in effect, we use a difference-in-differences research design that exploits the fact that the tax rate differed by house price and time.

Suppose we had access to data on only one tax reform and wanted to estimate the treatment using a binary treatment indicator $1[P_{it} > P^n]$, which equals one when house $i$ is above the price notch in month $t$ and equals zero otherwise. Because the unit of observation is a house-month but we only observe $P_{it}$ in month-years in which the house sells, we must impute $P_{it}$ for all other month-years. Note that $P_{it}$ is the nominal price (as it must be given that $P^n$ is defined in nominal dollars). We construct imputed values for $P_{it}$ as follows. If a house sells only once in our data, we scale the observed selling price by the monthly Washington D.C. Case-Shiller Price Index to get a nominal $P_{it}$ for each month-year. If a house sells twice, for all month-years before the first time it sells (after the second time it sells), we impute the price using the first sale (second sale) and the Case-Shiller Price Index as before. In the month-years between the two house sales, we compute the monthly house-specific appreciation rate and apply this monthly rate to get $P_{it}$ for each month-year. We
use an analogous method to calculate $P_{it}$ for all homes that sell three or more times. While this is a reasonable way to calculate $P_{it}$, we acknowledge that it is not the only reasonable way to proceed.

If we assume there is no manipulation of the sale price or time, we can estimate the medium-term lock-in effect using the following estimating equation:

$$sell_{it} = \beta_0 + \beta_1 P_{it} + \beta_2 T_{it} + \beta_3 X_{it} + myr_t + u_{it},$$ (14)

where $P_{it}$ is the actual or imputed price, $T_{it}$ is a one-zero indicator for whether the house-month observation is after the tax notch was introduced, $X_{it}$ are the house characteristic covariates described in Section 4, $myr_t$ are month-year fixed-effects and $u_{it}$ is the error term.\(^{14}\) Then, we can interpret $\beta_1$ as the decrease in the likelihood a house sells caused by the introduction of the higher tax rate above the notch after the reform.

Because we want to take advantage of the full variation in the tax rate $\tau$ based on the multiple changes of the tax notch at different points in the house price distribution, we want to estimate:

$$sell_{it} = \beta_0 + \beta_1 \log(\tau_{it}) + \beta_2 P_{it} + \beta_3 T_{it} + \beta_4 X_{it} + myr_t + u_{it},$$ (15)

Now, $\beta_1$ can be interpreted as the decrease in the likelihood a house sells in a given month-year for a given percentage increase in the tax rate. One assumption needed for this regression to be valid is that conditional on observables, the likelihood a house sells, absent the tax reform, is the same above and below the price notch. This assumption is much more likely to hold in a region around the notch, so we exclude houses further away from the price notch. For now, we include all houses with $P_{it} \in [\$150,000, \$800,000]$. Later in this section, we consider alternative restrictions. The estimate of $\beta_1$ based on this specification is given in

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\(^{14}\) $1[T_{it} > T^n]$ is not included as a separate term in the regression because month-year fixed effects are included.
Table 2, Column 1. The estimates are mean marginal effects from a probit specification. According to this estimate, a 1 percent increase in the transfer tax (say from 2 percent to 2.02 percent), will decrease the likelihood a house sells in a given month by 0.0000473 and this estimate is significant at the one percent level. The mean likelihood a house sells in a given month is 0.01070 in the years before the notched tax reforms (1999-2002), which means that a 1 percent increase in the transfer tax rate will decrease the likelihood a house sells by 0.44 percent at the mean.\(^{15}\) An increase in the tax rate from 2.2 percent to 2.9 percent (a 27.6 percent increase) would decrease the sales rate by 12.2 percent. Extrapolating much beyond the magnitude of the tax rates we observe is inadvisable.

Recall, though, that we know from Section 5 that there is significant bunching around the price notch, which means that estimating the lock-in effect treating the observed tax rate as exogenously assigned is inappropriate. Because the sales price is manipulable, the observed tax rates, which are a function of this manipulated sales price, are endogenous. In this particular example, we expect that the estimate in Column 1 of Table 2 overestimates the lock-in. This occurs because there are a significant number of individuals who have altered the sale of their house from above to below the price notch. If we define treatment based on the observed price, this manipulation implies that, after the notched reform, there are more house sales below the price notch relative to above even if there is no lock-in effect at all.

The most common approach taken in the tax literature to resolve this type of problem is to use an instrument for the tax rate. In general, this requires an instrument that is independent of manipulation, and the estimate obtained can be interpreted as a Fixed-Bracket Average Treatment Effect as defined in Weber (2012). This means that, with this procedure, the estimated medium-term lock-in effect does not reflect the behavior of those with an incentive to manipulate their house sales price below the price notch. Moreover, the

\(^{15}\) In a proportional hazard model, the mean holding time is simply the inverse of the hazard rate, so if the mean hazard rate is 0.01070, the average holding period is 1/0.01070, or 93.458 months. If a one percent increase in the transfer tax drops the hazard rate to by 0.44 percent, or to 0.01065, the mean holding time increases by also about 0.44%, to 93.873 months.
instrument would need to satisfy the exclusion restriction—conditional on the other right
hand side variables, the instrument does not affect the rate of house sales.

This approach is relatively unsatisfactory in the current context for two reasons. First, we
do not have a baseline covariate that we expect will satisfy the necessary restrictions and thus
be a valid instrument. Second, even if we did have such an instrument, the estimates would
be independent of the behavior of those near the price notch, whose behavioral response
might otherwise provide our most convincing identification (they are, after all, the most
likely to be similar on both observed and unobserved dimensions) or might be interesting
in their own right if we believe their behavior is different from those further away from the
price notch.

Note that defining the tax rate as a function of our hedonic estimate of true value instead
of the actual transaction price is another alternative, and this procedure would allow us to
include all houses in the estimation, not just those that sold during 1999-2010. However,
there are several potential downsides of this method. Most importantly, it would introduce
substantial measurement error into our definition of treatment. Second, because we do not
observe all characteristics before any notch is introduced, it is only valid to the extent that
all the observed price manipulation is done holding $V$ fixed.

We now turn to our baseline estimation procedure, but first we provide some perspective.
In many non-tax contexts, it is crucial to precisely separate those who are treated from those
that are not is because the expected effective treatment changes discontinuously at the policy
notch, regardless of whether there is manipulation to get in or avoid the treatment. Consider,
for example, a job training program. We showed in Section 3 that this is not the case in
this context (and this is often the case in the context of tax kinks and notches). Here we
expect that the response in the region in which the imperfect bunching occurs to be larger
than the response in the comparison group and smaller than the response in the treatment
group because their expected effective tax rate is somewhere between the treatment and
comparison group tax rates.
We proceed as follows. We assume that all individuals who manipulate their price below the notch choose a price within some distance from the notch, so that all individuals who have manipulated their sale price will have a sale price above \( P^l \), where \( P^l < P^n \). This seems to be a reasonable assumption in our case and is consistent with the existing literature on bunching at notches, which always assumes that bunching takes place within a narrow region of the notch (Kleven and Waseem, 2012; Slemrod, 2010). Based on our analysis in Section 5, a reasonable conservative choice for \( P^l \) is $350,000 for the second notch and $200,000 for the first notch. Then we could change the estimating equation from equation (14) by substituting \( P^l \) for \( P^n \), which yields:

\[
sell_{it} = \beta_0 + \beta_1 1[P_{it} > P^l] + \beta_2 1[T_{it} > T^n] + \beta_3 1[P_{it} > P^l] + \beta_4 X_{it} + \text{myr}_t + \epsilon_{it},
\] (16)

Equivalently we can adjust equation (15) as:

\[
sell_{it} = \beta_0 + \beta_1 \log(\tau'_{it}) + \beta_2 1[P_{it} > P^1] + \beta_3 1[P_{it} > P^2] + \beta_4 X_{it} + \text{myr}_t + \epsilon_{it},
\] (17)

where \( \tau' \) increases to the higher notched value for all prices above \( P^l \) for the given tax reform. We can estimate this equation without concern for selection into the comparison group because the estimating equation does not assign a separate treatment based on this selection. The results from estimating equation (17) are presented in Column 2 of Table 2. The estimate for \( \beta_1 \) is -0.00219 and remains significantly different from zero at the 1 percent level. This estimate implies that a 1 percent increase in the transfer tax rate will decrease the likelihood a house sells by 0.20 percent at the mean. An increase from a 2.2 percent to 2.9 percent (a 27.6 percent increase) would decrease the sales rate by 5.7 percent. This estimate is less than half the size of the estimate based on the endogenous measure of treatment. Part of the decline is undoubtedly due to resolving the endogeneity, but part may also be due to the fact that some of the individuals counted as being in the treatment group face a lower effective tax rate than those whose expected effective tax rate was the higher notched value.
We can examine the second story more carefully by estimating the same equation, but using a less conservative choice of $P^l$. The estimates with $P^l$ set to $235,000$ for the first tax reform and $385,000$ are reported in Column 3 of Table 2. To the extent that individuals manipulate to a price below $385,000$, this estimate will be biased upward. The estimate of $\beta_1$ is $-0.00305$, which is higher than the original estimate, but still quite a bit lower than the estimate in Column 1. This estimate implies that a 1 percent increase in the transfer tax rate will decrease the likelihood a house sells by 0.28 percent at the mean. In the future, we will scale the estimates by the expected effective treatment. This will conclusively answer which types of treatment mismeasurement are contributing to the difference between the estimates in Columns 2 and 3.

Even though we did not observe significant manipulation around the time notch in Section 5, some manipulation could still exist and, if it does, the estimates that we have reported so far are biased. We can address the time manipulation in the same way as the price manipulation, by defining a region around which all manipulation occurs. We choose a window of two months and present these results in Column 4 of Table 2. The estimates are very similar to the estimates in Column 2. This suggests both that there was not a lot of manipulation around the time notch and that the response in the region just below the time notch is approximately the same, on average, as above. This is consistent with the evidence presented in Section 5.

Given that we do not find any evidence of manipulation around the time notch, we consider Column 2 of Table 2 to be our baseline estimate. The next columns of Table 2 present robustness checks relative to this estimate. The estimates we have reported so far assume that there are no omitted housing characteristics that are biasing the estimates. Column 5 of Table 2 drops all the housing characteristics (which are fixed for a given house across time) and includes house fixed effects instead. This will capture all time-invariant observable and unobservable housing characteristics that determine the date of sale. The estimates increase in absolute magnitude to $-0.00340$ and remain significant at the 1 percent
Columns 6 and 7 consider different definitions of the sample included in estimation. Column 6 includes all individuals in $P_{it} \in [\$150,000, \$600,000]$ and Column 7 includes all individuals in $P_{it} \in [\$200,000, \$800,000]$. The estimates are not statistically different from the baseline estimates.

Up to this point, we have considered nominal price cutoffs for choosing which house-month-years are included in the sample. However, a cutoff that includes or excludes a house for all years based on the price in a base year may be more appropriate. The advantage of this cutoff is that it resolves the potential problem that, if houses appreciate out of our sample and this is correlated with the likelihood they sell and the tax rate, the estimates reported above would be biased. We present these estimates in Table 3. All columns restrict the sample based on their imputed 1999 sales price. Column 1 includes all houses with 1999 sales prices between $100,000 and $600,000. In 2003, 95 percent of the data had prices between $154,128 to $909,262 in nominal dollars and by 2006 the range was $240,576 to $1,425,464. The estimate of $\beta_1$ in Column 1 is -0.00223, which is almost the same and not statistically different from the baseline estimate in Table 2. Given that the likelihood that individuals sell at a given nominal price is changing over time as houses, on average, appreciate or depreciate, a polynomial in the real price or valuation should arguably be included. We include a 3rd-order polynomial in real price and assessor valuation in Columns 2 and 3, respectively. These estimates decline slightly but are not statistically different from the estimates in Column 1.

Columns 4 and 5 of Table 3 consider a narrower restriction of the sample, again based on 1999 imputed sale prices, because the range of houses selected by their 1999 sale prices included in Columns 1-3 is quite large, particularly by the 2006 reform. Column 4 restricts prices in 1999 to $100,000 to $400,000. In 2003, 95 percent of the data had prices between $152,431 to $660,000 in nominal dollars and by 2006 the range was $231,162-$1,026,581. Column 5 restricts prices in 1999 to $100,000 to $300,000. In 2003, 95 percent of the data had prices between $149,960 to $522,648 in nominal dollars and by 2006 the range
was $215,880-$805,754. The estimates in Columns 4 and 5 are similar and not statistically different from the estimates in Column 1.

7 Conclusion

This paper estimates the medium-term lock-in effect of residential real estate transfer taxes by studying notched tax rate changes in Washington D.C., exploiting both a price and time notch as identifying variation. We provide evidence that there is substantial manipulation of the sales price in the region around the price notch, but there is little manipulation around the time notch. Ignoring this manipulation would lead us to substantially overstate the lock-in effect. Because using an instrument is not a satisfactory econometric solution in this context, we propose an alternative way of addressing the selection into treatment. We find that a 1 percent increase in the transfer tax rate decreases the rate of sales by 0.20 percent. The 2006 notched tax reform in Washington D.C. introduced a 27.6 percent increase (from 2.2 percent to 2.9 percent) in the transfer tax rate for all houses above the notch. Our estimates imply that this decreased the house sales rate by 5.7 percent. In future work, we will incorporate data from Maryland to exploit the notched tax rate change at geographical borders. This will sharpen our estimates by providing another type of comparison group and will also generate evidence regarding geographical transfer tax spillovers.
References


This figure depicts the choice of the time of sale, $T$, for a given amount of improvements, $I$, where $V_0 \geq 400,000$; that is, the seller cannot alter the house value to get it below the price notch. The solid blue lines represent the net proceeds available for a given $V$ before and after the time notch (the dashed gray lines depict what they would have been if the policy had not changed at time $T_n$). The seller with sale date preferences depicted by $U_H$ is exactly indifferent between selling at time $T_n$ and selling at time $T_n + dT_n$. Sellers with indifference curves between $U_L$ and $U_H$ will bunch at $T_n$. 

Figure 1: Time Notch
Figure 2: The Effect of the Price Notch on Optimal Improvements

This figure depicts the terms of the choice of home improvements, \( I \), for a given sale date \( T \), where \( T > T_n + dT_n \), so that moving up the date of sale is not an attractive option. The solid blue lines represent the net proceeds available before and after the price notch (the dashed gray lines depict what they would have been if the policy had not changed at the notch). Note that the objective is to maximize net proceeds, so the relevant indifference curve in the bottom panel is horizontal. The individual whose net proceed, \( Y_I(\cdot) \), curves are depicted in the bottom panel of this figure is exactly indifferent between bunching at \( I(V_0, V_n) \), so that \( V = V_n \), and choosing his optimum without bunching \( I(V_0, V_n) + dI(V_0, V_n) \).
This figure depicts the terms of the choice of the reported sale price, $R$, for a given $T$, where $T > T_n + dT_n$, so that moving up the date of sale is not an attractive option. The solid blue lines represent the net proceeds available before and after the price notch (the dashed gray lines depict what they would have been if the policy had not changed at the notch). Note that the objective is to maximize net proceeds, so the relevant indifference curve in this figure is horizontal. The individual whose net proceeds, $Y_t(\cdot)$, curves are depicted in the bottom panel of this figure is exactly indifferent between bunching at $R_n$ and choosing his optimum without bunching $R_n + dR_n$. 
This figure depicts the terms of choice for sale date $T$ and reported sale price $R$ when the seller is in the region of both notches. The solid blue lines represent the net proceeds available before and after the price notch (the dashed gray lines depict what they would have been if the policy had not changed at the notch). If the seller moves from the point $R_{nt}$ to the point $R_n$ in the top panel the net proceeds lines will shift up to the dashed yellow and solid green lines in the bottom panel. This shifts the feasible indifference curve up to the red dashed indifference curve. This seller is now indifferent between bunching in the $R$ and $T$ dimensions. Note that the vertical axis of the bottom panel is not drawn to scale (it should match the vertical axis of the top panel).
This figure plots the median house sale price of houses that sold in a given year. All non-condominium houses on which we have housing characteristics and sell for a price greater than zero are included.
This is a histogram of all houses sold in the region surrounding the price notch. The left histogram include houses sold from April 1, 2005 to April 1, 2006. The right histogram include houses sold from October 1, 2006 to October 1, 2007. The bin size is $1,420. The red vertical line denotes the location of the price notch.
This figure plots the difference in house sales density after the reform (October 1, 2006 to October 1, 2007), relative to before the reform (April 1, 2005 to April 1, 2006). The red vertical line marks the location of the price notch. The log difference of the differenced densities at the notch is -0.8529 (0.2171). The bin size is $1,420. The bandwidth is $15,877.74, which is the mean bandwidth selected by the bandwidth selection method used in McCrary (2008) for the two densities. The dashed lines provide a 95-percent confidence interval.
This figure plots the house sales density for different years before and after the 2006 tax reform. Panel A: before the reform (April 1, 2005 to April 1, 2006). Panel B: 1 year after the reform (October 1, 2006 to October 1, 2007). Panel C: 2 years after the reform (October 1, 2007 to October 1, 2008). The red vertical line marks the location of the price notch. The bin size is $1,420 and the bandwidth is $15,877.74. The dashed lines provide 95-percent confidence intervals.
This is a histogram of all houses sold in the region surrounding the price notch. Panel A examines the price distribution during the anticipation period (May 10, 2006 to October 1, 2006) relative to (May 10, 2005 to October 1, 2005). These figures are on the right and left, respectively. Panel B considers what happens just before (August 1, 2006 to October 1, 2006) relative to just after implementation (October 1, 2006 to December 1, 2006). These figures are on the left and right, respectively. The bin size is $1,420. The red vertical line denotes the location of the price notch.
Figure 10: Difference in the Density of House Sales Prices Post-Announcement

Panel A plots the difference in house sales density after announcement (May 10, 2006 to October 1, 2006), relative to before the announcement (May 10, 2005 to October 1, 2005). Panel B plots the difference in house sales density just after implementation (October 1, 2006-December 1, 2006) to just before (August 1, 2006 to October 1, 2006). The bin size is 1,420. The bandwidth for Panel A is 19967.66 and and 18865.27 for Panel B, which in both cases is the mean bandwidth selected by the bandwidth selection method used in McCrary (2008) for the two densities. The dashed lines provide a 95-percent confidence interval.
This figure plots the difference in hedonic house valuation after the reform (October 1, 2006 to October 1, 2007), relative to before the reform (April 1, 2005 to April 1, 2006). The red vertical line marks the location of the price notch. The bandwidth is $100,000. The dashed lines provide a 95-percent confidence interval.
Figure 12: Frequency of House Sales Around the Time Notch

This figure plots the difference in the frequency of house sales for all sales above $350,000 around the time notch relative to the year prior. The bandwidth is 100 days. The solid vertical red line is at the time notch.
Table 1: Summary Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Houses that Sold</th>
<th></th>
<th>Houses that Never Sold</th>
<th></th>
<th>Diff. in Mean</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Std. Dev.</td>
<td>Min</td>
<td>Max</td>
<td>Mean</td>
<td>Std. Dev.</td>
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<td>776.74</td>
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<td>39</td>
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<tr>
<td># Stories</td>
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<td># Bedrooms</td>
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<td>34</td>
<td>3.28</td>
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<tr>
<td># Baths</td>
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<td>#Half Baths</td>
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<tr>
<td>Carpet</td>
<td>0.03</td>
<td>0.18</td>
<td>0</td>
<td>1</td>
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<td></td>
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<tr>
<td>Built Up</td>
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<tr>
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<tr>
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</tr>
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<td>0.04</td>
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<td>Shingle</td>
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<td>Stucco</td>
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<td>0</td>
<td>1</td>
<td>0.06</td>
<td>0.24</td>
</tr>
</tbody>
</table>

This table reports summary statistics for all houses in our data (the unit of observation is a house). For categorical variables, I exclude all categories that include less than 1 percent of individuals to make the table more accessible. The excluded flooring categories are: Concrete, Parquet, Resiliant, Terrazo, Vinyl Composite, and Vinyl Sheet. The excluded roofing categories are: Clay Tile, Concrete, Concrete Tile, Neoprene, Shingle, Typical, Water Proof, and Wood. The excluded exterior categories are: Aluminum, Concrete, Hardboard, Metal Siding, Plywood, Rustic Log, Plaster, Stone, Other Stone, and Stucco Block. Excluded heating types are Air, Electric Base Board or Radiator, Evaporation Cool, Gravity Furnace, Heat Pump, Industrial Unit, Wall Furnace, and Water Base Board. Excluded condition types are: Default and Poor. Excluded building types are: Town End and Town Inside.
<table>
<thead>
<tr>
<th>Variable</th>
<th>Houses that Sold</th>
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<th>Houses that Never Sold</th>
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<th>Diff. in Mean</th>
<th></th>
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<td>Std. Dev.</td>
<td>Min</td>
<td>Max</td>
<td>Mean</td>
<td>Std. Dev.</td>
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<tr>
<td>Average</td>
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<td>0.01</td>
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<td>-</td>
<td>-</td>
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</tbody>
</table>

Observations 56,555 47,064

This table reports summary statistics for all houses in our data (the unit of observation is a house). For categorical variables, I exclude all categories that include less than 1 percent of individuals to make the table more accessible. The excluded flooring categories are: Concrete, Parquet, Resilient, Terrazo, Vinyl Composite, and Vinyl Sheet. The excluded roofing categories are Clay Tile, Concrete, Concrete Tile, Neoprene, Shingle, Typical, Water Proof, and Wood. The excluded exterior categories are Aluminum, Concrete, Hardboard, Metal Siding, Plywood, Rustic Log, Plaster, Stone, Other Stone, and Stucco Block. Excluded heating types are Air, Electric Base Board or Radiator, Evaporation Cool, Gravity Furnace, Heat Pump, Industrial Unit, Wall Furnace, and Water Base Board. Excluded condition types are: Default and Poor. Excluded building types are: Town End and Town Inside.
Table 2: Lock-In Estimates

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<td>(0.00069)</td>
<td>(0.00067)</td>
<td>(0.00068)</td>
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<td>(T^n)</td>
<td>(T^n)</td>
<td>(T^n) - 2 months</td>
<td>(T^n)</td>
<td>(T^n)</td>
<td>(T^n)</td>
</tr>
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<td>150K-800K</td>
<td>150K-800K</td>
<td>150K-800K</td>
<td>150K-800K</td>
<td>150K-600K</td>
<td>200K-800K</td>
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<td>No</td>
<td>No</td>
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<td>39,413</td>
<td>39,413</td>
<td>39,413</td>
<td>38,216</td>
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</table>

The dependent variable is an indicator for whether the house sells in a given month-year. All columns include house characteristic covariates and month-year fixed effects. Estimates include all month-years in 1999-2010. The sample restrictions are based on the nominal imputed sale price in each month-year. Standard errors clustered by house are in parentheses. The estimates are mean marginal effects from a probit specification.
Table 3: More Robustness Checks

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<td>28,980</td>
<td>25,236</td>
<td>20,395</td>
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The dependent variable is an indicator for whether the house sells in a given month-year. All columns include house characteristic covariates and month-year fixed effects. Estimates include all month-years in 1999-2010. The sample restriction in this table are based on the imputed sale price in 1999. Standard errors clustered by house are in parentheses. The estimates are mean marginal effects from a probit specification.
Appendix 1: Timeline and Relevant excerpts of Washington Post Articles Regarding the 2006 Changes in Deed and Recordation Tax

FEBRUARY 17, 2006: Reserves, Deed Tax May Plug D.C. Gap; Deficit Lurks Within Big Schools Outlay:

“In response to estimates released yesterday by Chief Financial Officer Natwar M. Gandhi, council leaders said they would amend their legislation—scheduled for a second, and final, vote March 7—to tap District reserves for the first year and to increase the deed recordation tax in later years if necessary. The council gave the bill preliminary approval this month..."

“In an interview, Evans said he doesn’t expect amendments to the legislation to cost votes. To school funding advocates, he has been stressing that the disappointing revenue numbers will not affect modernization money, which will come from the first $100 million of sales tax revenue every year. He said that education proponents should view the pledge of increasing the deed recordation tax as proof of the council’s commitment to rebuilding the District’s crumbling schools."

MARCH 27, 2006: Williams Submits His Final Budget; Plan Trims Income Tax, Boosts Schools, Libraries:

“Williams’s budget calls for no broad-based tax increases. However, the mayor is seeking to boost tax revenue on residential home sales by $47 million by raising the deed recordation fee to 1.5 percent, from 1.1 percent..."

“Cropp, who is running to succeed Williams, said she worries that the mayor’s plan to raise the deed recordation tax would hamper first-time home buyers. Cropp said she would urge an exemption to the tax increase for houses that cost less than $500,000."

“It was unclear yesterday how such an exemption would affect revenue from the tax increase, which the mayor proposes to use to fund recommendations from a housing task force, including expanded rental assistance, new housing for middle-class buyers and the homeless, and the designation of a coordinator to oversee housing programs."

APRIL 25, 2006: At Forum, Fenty Vows Not to Raise D.C. Taxes:
“Cropp went one step further, announcing that she will propose to eliminate a portion of a tax increase recently proposed by Mayor Anthony A. Williams (D). Under her plan, Cropp said the deed and recordation tax for residential properties would not go up for first-time home buyers or on houses that sell for less than $400,000.”

MAY 10, 2006: Council Approves Increases for Police, Housing:

“The D.C. Council unanimously approved yesterday a budget of $5.06 billion that provides money to hire 100 additional police officers and boosts funds for public schools and affordable housing programs.”

“Council members vigorously sparred over several changes to the fiscal 2007 funding request submitted by Mayor Anthony A. Williams (D). Most of the debate focused on an increase in the city’s deed recordation and transfer tax...”

“The budget, which requires congressional approval before it can take effect Oct. 1, is a 2 percent increase in local spending over last year. The total city budget, including federal funding, is $9 billion.”

“The council will hold a second and final vote on legislative language supporting the budget June 6...”

“Council members agreed to increase the deed recordation and transfer tax for residential and commercial properties from 1.1 percent to 1.45 percent, with residential properties valued at $400,000 or less exempt from the increase. They allocated $7 million from the tax, which has been a key revenue source during the recent boom in the real estate market, to pay for 100 more police officers. The additional new revenue also went to fund affordable housing initiatives recommended by the city’s housing task force...”

“The city’s dependence on the tax to fund legislative priorities caused concern, too. On Friday, D.C. Chief Financial Officer Natwar M. Gandhi projected lower revenue from the tax because of a cooling off of the housing market, which prompted the council to bump the increase from 1.35 to 1.45 percent.”
2007: When searching for “deed recordation tax,” We found nothing more regarding the implementation of this tax change.