Abstract

This paper estimates the behavioral response to 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 manipulation of the sales price to the lower-tax-rate region around the price notch, and use this manipulation to show that there was significant awareness of the tax changes and the incentives they created. We find some less compelling evidence of a change in the timing of house sales to beat the tax increase. Finally, we construct difference-in-difference estimates to examine whether there is a lock-in effect in the volume of house sales away from the price and time notches; we find no evidence of a lock-in effect in this setting.
1 Introduction

As of 2012, 35 states and the District of Columbia had a transfer tax on residential real estate transactions. Washington D.C. had one of the highest transfer tax rates of all, even before 2006 when 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. The mean state housing transaction tax rate in 2012 for states that had this tax was 0.58% in the U.S. (and the standard deviation was 0.54%). 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 until very recently received relatively little attention in the economics literature, but their analysis has some natural advantages as a way to learn about the effects of transaction taxes and real estate taxes more generally.

To learn about this issue, this paper will examine changes in transfer tax policy in Washington D.C. 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

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1 States with flat fees of $5 or less are not included in this calculation.
2 This calculation is based on the lowest possible tax rate in each state. Some states have higher tax rates for homes above a certain value. This does not include any county or local tax rates.
3 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.
October 1, 2004, but a new tax notch 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.

We develop a Nash bargaining model to analyze how price notches in the schedules of taxes remitted by both home buyers and sellers affect behavior and the buyers’ and sellers’ surpluses. This model generalizes existing models in the literature (Kopczuk and Munroe 2015; Best and Kleven 2015; Besley et al. 2014) by allowing both the buyer and seller to bear remittance responsibility, which, in contrast to standard models, matters because the notch is defined in terms of the amount that buyers pay sellers. This extension highlights that we have a strictly dominated region—a region in price space where neither the buyer nor the seller wish to transact—in our setting, which we can exploit to speak to the degree of optimization frictions and awareness of the tax change in this market. We then extend the model by allowing the seller to upgrade the quality of the home prior to matching. We also relax the assumption that the date of sale is exogenously fixed and analyze bargaining over the sale date with tax schedules that may include time notches or combined price and time notches. This model motivates our empirical analysis.

We estimate the effect of the transfer tax on the density of house sales and also address two related margins of behavioral response: manipulation of house sales to avoid the higher taxes above the price or time notches. We provide empirical evidence that there is manipulation of the sales price to avoid the higher-tax-rate region above the price notch. We use this manipulation to show that there was significant awareness of the tax changes and the incentives they created. For the 2006 reform, we estimate that about two-thirds of individuals were aware of the tax change, and for the 2003 reform, we show that about one-third of individuals were aware. We cannot reject the null hypothesis that the excess mass below the price notch is equal to the hole in the density of house sales above the price notch, so we cannot reject the null that bargaining power is split equally between the buyer and seller.

We also do not observe compelling evidence of a substantial adjustment of the timing of house sales in response to the time notch. This suggests that sellers are more likely to adjust
the sale price of their house than they are to speed up the sale date to avoid the tax notch, which speaks to the relative cost of these two choices.

We then use this information to develop difference-in-difference estimates of the lock-in effect that are consistent in the presence of the observed house sales price manipulation. We exploit the fact that all individuals faced the same proportional disincentive to sell when the tax rate was uniform, but once a price notch was in place, those with houses above the price notch faced higher proportional disincentives to sell. We find no evidence of a housing lock-in effect in our setting, one in which the average tax change we consider is about the same as the average housing transaction rate at the state level. This research therefore suggests that introducing or eliminating a housing transaction tax will have little effect on the volume or timing of house sales.

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 the behavior around the price notch. Section 6 examines the the behavior around the time notch. Section 7 conducts the difference-in-differences analysis to estimate the lock-in effect. Section 8 concludes.

2 Related Literature

There is an existing literature on the effects of capital gains taxes on lock-in and housing lock-in in particular, as well as a burgeoning literature on the effects of housing transfer taxes on lock-in and other associated margins of adjustment. However, the existing literature gives somewhat mixed predictions about the size of the lock-in effect we should expect. Our paper seeks to settle this question. It also builds on existing models in the housing transaction tax literature to provide new and important results. Lastly, our research informs and is informed by research on the underlying microstructure of housing markets. This section provides a
brief overview of the existing work on capital gains taxes, housing transaction taxes, and the housing market microstructure as it relates to this paper.

2.1 Capital Gains Taxes

Most capital gains tax studies have focused, explicitly or implicitly, on capital gains from publicly traded stock, while only a small minority of these studies focused on housing markets. The single most important question in this literature is how do capital gains taxes affect when individuals realize their gains, or the inverse of this, how locked-in are individuals to their current asset portfolio due to taxes. Because both the market itself and the tax treatment differ substantially between the 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" and excluding capital gains of up to $500,000 from taxable income, spurred much recent analysis on the housing market, such as Cunningham and Engelhardt (2008) and Shan (2011).

Shan (2011) finds that the latter change caused the sales rate of houses to rise from between 19 to 24 percent for houses with price appreciation up to $500,000. Notably, Shan (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. She estimated that 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. Given average annual home prices reported in her paper, $10,000 represents about a 1.4 percentage point increase in the tax rate, which is twice as large as the tax change we consider.

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4The age-55 rule allowed individuals to claim a one-time exclusion of $125,000 if they were over age 55.
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 et al. (1995) observe that in this case the effective annual transfer tax rate declines as the years of ownership increase. For this reason, we might expect estimates of lock-in to be larger in our setting than in settings, such as Shan (2011) that consider capital gains tax rate changes.

As of 2012, 35 states and the District of Columbia had a transfer tax on residential real estate transactions. Washington D.C. had one of the highest transfer tax rates of all, even before 2006 when 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. The mean state housing transaction tax rate in 2012 for states that had this tax was 0.58% in the U.S. (and the standard deviation was 0.54%). Some counties and municipalities in the U.S. also impose transfer taxes. Most Canadian provinces, Australian states, as well as the United Kingdom and France also levy land transfer taxes and raise non-trivial revenue from these taxes.

Other recent papers examine the effects of house transaction taxes, all in contexts in which the buyer bears all of the statutory remittance responsibility (Kopczuk and Munroe, 2015; Best and Kleven, 2015; Besley et al., 2014). Our context and model are unique in that we analyze a setting in which both the buyer and seller bear remittance responsibility. The seller’s remittance responsibility creates a region above the notch strictly dominated by sales at the notch in terms of both the buyer’s and seller’s surplus from the sale. In the limit as the buyer bears all of the remittance responsibility, this strictly dominated

\footnote{As a result, they find the inefficiencies associated with a transfer tax are substantially higher than with a simple property tax or acquisition-value property tax.}

\footnote{States with flat fees of $5 or less are not included in this calculation.}

\footnote{This calculation is based on the lowest possible tax rate in each state. Some states have higher tax rates for homes above a certain value. This does not include any county or local tax rates.}

\footnote{Both Kopczuk and Munroe (2015) and we show that remittance responsibility can have real effects, contrary to the standard model.}
region disappears. Examining whether transactions occur in the strictly dominated region provides direct evidence of the extent of optimization frictions in general, and of awareness of the buyer and seller in particular, because the cost of leaving the strictly dominated region by adjusting the sale price of the house is approximately zero. Assigning a higher fraction of the remittance responsibility to the seller expands the strictly dominated region and reduces the number of transactions that can bunch. Our model of continuous-time bargaining over the date of sale with a time notch and of bunching with both time and price notches approaches the timing of transactions differently from the two-period market-level model Best and Kleven (2014) use. Our model is the only one to examine the implications of a notch for sellers’ decisions to improve the quality of their houses prior to sale, which Kopczuk and Munroe (2015) discuss empirically but do not model. We depart from Kopczuk and Munroe (2015) by fully developing a model of proportional taxation and not assuming a functional form for the counterfactual distributions of prices.

Both Best and Kleven (2015) and Kopczuk and Munroe (2015) examine behavior around a price notch and find results that are somewhat different from each other and from the results in our paper, highlighting how the microstructure of different local housing markets can vary as well as the role of remittance responsibility. In particular, Best and Kleven (2015) find the width of their "hole" in the distribution of sales above the price notch that extends to five times the size of their tax liability. This is about three times as large as the hole width we find, which may be partly due to increased tax avoidance in their setting given that nearly four out of ten transactions are in cash (Pickford, 2015). The hole width in Kopczuk and Munroe (2015) extends even further—it is about 15 times the size of the increase in tax liability. Both Best and Kleven (2015) and we find that the amount of bunching below the notch approximately matches the volume of "missing" transactions above the notch. We find much less bunching than do Best and Kleven (2015), which is partly due to a smaller hole width and also partly due to the fact that the seller bears a

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9Both Best and Kleven (2015) and Kopczuk and Munroe (2015) were written concurrently with this paper.
larger portion of the remittance responsibility. Kopczuk and Munroe (2015) find a much larger volume of "missing" transactions above the notch, relative to the amount of bunching they find. This leads them to conclude that the market "unravels" in the neighborhood of the notch: its presence provides strong incentive for buyers and sellers in the proximity of the threshold not to transact.

Both Dachis et al. (2012) and Best and Kleven (2015) implicitly or explicitly exploit transfer tax notches in property price, sale date, or geography to identify the permanent lock-in effect in Canada and the U.K., respectively. Both find permanent lock-in effects of a much larger magnitude than the lock-in effects found in this paper; in particular, Dachis et al. (2012) find that a one percentage point increase in the housing transaction tax leads to a 13.6 percent decrease in house sales and Best and Kleven (2015) find that the same increase leads to a 12 percent decrease in house sales. As noted in the last paragraph, we have evidence that different markets behave differently on a number of dimensions. Notably, both of these studies, unlike our study, identify a lock-in effect local to one of the notches and then make some assumptions to determine the permanent response. Dachis et al. (2012) compares sales just on either side of the Toronto city border, so that their estimates do not overstate lock-in only if sales do not increase just outside of Toronto at the same time they are falling within Toronto (which may be expected if those who would have bought inside Toronto are now purchasing in the suburbs instead). Best and Kleven (2015) estimate their lock-in effect from a temporary sales tax holiday and then back out a measure of the permanent lock-in effect as the extent to which increased sales during the tax holiday are not subsequently offset by decreased sales after the reform. It is also worth noting that Besley et al. (2014) examine the same setting as Best and Kleven (2015) using a different methodology, and get a substantially smaller short-run lock-in effect; Besley et al. (2014) do not attempt to disentangle the short-run and long-run lock-in effects.
2.3 Housing Market Microstructure

This paper also contributes to the literature on housing market microstructure, surveyed in Han and Strange (2014a), by deepening our understanding of the role that information and frictions, including transaction taxes, play in the housing market. Our findings about transaction taxes are likely informative about other frictions in the housing market, including broker fees that average five to six percent of the sale price in the U.S. (Han and Strange, 2014a). We focus on a bargaining model that abstracts from the search process portion of the random search models Han and Strange (2014a) discuss; however, we will discuss the search process when it provides insightful context for our results.

Our finding that many transactions occur even in the strictly dominated price range contributes to a growing strand of literature on optimization failures in the housing market. Genesove and Mayer (2001) find that loss aversion drives sellers to seek higher prices and accept a longer time on the market in return for those prices, and Han and Strange (2014b) find evidence that bidding wars, in which houses are sold for prices above list price, are more common in housing markets with larger post-boom price drops, which may indicate irrational behavior.

We find that tax changes that incentivize sales to occur before a cutoff date do not cause substantial manipulation of the date of sale, in contrast to some related existing literature that finds that price incentives do have a significant effect on the timing of housing market transactions in certain situations. Hausrin (1988) shows that sellers of atypical houses have optimal stopping rules that imply longer marketing time, and that atypical houses do indeed tend to spend more time on the market. Zuehlke (1987) finds that stronger incentives to sell vacant houses leads to positive duration dependence for these houses, that is, vacant houses’ probability of sale rises with the time the house spends on the market.

Sass (1988) finds that optimal, and observed, pricing behaviors decline more quickly over time in thinner markets (with fewer potential customers), and where the seller has less information about demand. This, combined with the thinness of the market for houses that
are worth about one million dollars may help explain why Kopczuk and Munroe (2015) finds
house sales unravel near the price notch among houses around the million dollar mark, while
we do not find such a result at lower house price thresholds.

3 Model

We analyze how price notches in the schedules of taxes remitted by both home buyers and
sellers affect behavior and the surpluses of matched buyers and sellers in a Nash bargaining
model. For simplicity, our base model takes the matches between buyers and sellers,
the reservation values of both the buyer and the seller, and the sale date as given. This
model generalizes the models in Kopczuk and Munroe (2015), Besley et al. (2014), and the
appendix of Best and Kleven (2014) by allowing both the buyer and seller to bear remittance
responsibility. We then further extend the model by allowing the seller to upgrade the
quality of the home prior to matching. We also relax the assumption that the date of sale
is exogenously fixed and analyze bargaining over the sale date with tax schedules that may
include time notches or combined price and time notches.

3.1 Base Model: Proportional Taxation

We consider an exogenous match between the seller of a home of fixed quality, whose after-
tax reservation value is $S$, with a buyer, whose after-tax reservation value is $B$. The date
of sale is exogenously fixed, and $h$ is the purchase price arrived at by Nash bargaining, in

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10 Rubinstein and Wolinsky (1985) develop the theory of equilibrium in markets with Nash bargaining,
and Binmore et al. (1986) lay out the basics of its application to economic modelling. Because the housing
market has large search costs, research on housing markets often employs a bargaining framework either
as a component of random matching models like the example provided in Han and Strange (2014a), or on
their own, as in Harding et al. (2003). We abstract from several other features of the housing market’s
microstructure that are discussed in detail in Han and Strange (2014a), including but not limited to
the search and matching process, liquidity, asking prices, the role of real estate agents and their incentives in
the housing market, features of the market that may induce cyclicality, and auctions and bidding wars.

11 In a random matching model of the type developed in Han and Strange (2014a), the reservation values
$B$ and $S$ depend on the cost of search, market tightness, the probability that a match results in a sale, and
surplus conditional on a sale. We do not model these effects, although we discuss how they contribute to
lock-in.
Sale takes place.  Sale does not take place.

Figure 1: Given tax remittance responsibilities, the highest price the buyer is willing to pay is $B/(1 + \alpha t)$ and the lowest price the seller is willing to accept is $S/(1 - (1 - \alpha) \cdot t)$. A sale takes place at a price $h$ in the bolded interval where both parties will accept the price. When $B$ and $S$ are close enough, there is no price that both parties are willing to accept, given their tax remittance responsibilities, and the sale does not take place.

which the surplus resulting from the match is divided between the seller and the buyer with exponential weights $\beta$ on the buyer’s surplus and $1 - \beta$ on the seller’s surplus. The total tax rate is $t$, levied on the purchase price $h$. The buyer remits a fraction $\alpha$ of the total tax, while the seller remits the remaining fraction $1 - \alpha$ of the total tax.

The seller is willing to sell whenever $h \cdot (1 - (1 - \alpha) \cdot t) - S \geq 0$, and the buyer is willing to buy whenever $B - h \cdot (1 + \alpha t) \geq 0$. There is a price at which both parties are willing to transact whenever the total after-tax surplus is non-negative, i.e. whenever

$$\frac{B}{(1 + \alpha t)} - \frac{S}{(1 - (1 - \alpha) \cdot t)} \geq 0.$$  \hspace{1cm} (1)

As Figure 1 illustrates, tax remittance responsibilities create wedges between the buyer’s and seller’s reservation prices and the purchase prices they will each accept, which may be large enough that the match fails to satisfy \hspace{1cm} (1) and no sale takes place.

The purchase price\footnote{Appendix A.1 uses this price to find the buyer’s and seller’s surpluses and to analyze the relationships between the bargaining power weights, the tax rate, remittance responsibility, and the buyer’s and seller’s surpluses.} that maximizes the weighted surplus $W = (B - (1 + \alpha t) \cdot h)^\beta \cdot (h \cdot \ldots)$
\[(1 - (1 - \alpha) \cdot t) - S)^{1-\beta}\] is

\[h = \frac{B \cdot (1 - \beta)}{(1 + \alpha t)} + \frac{S \beta}{(1 - (1 - \alpha) \cdot t)}. \quad (2)\]

### 3.1.1 Remittance Responsibility, Bargaining Power, and Incidence

The real incidence each side bears as a result of a small increase in the tax rate is proportional to that side's bargaining power as shown in Appendix A. Proportional taxation (unlike lump-sum taxation) makes it less costly for the buyer to directly remit taxes than for the buyer to compensate the seller for the seller’s remittance responsibility by paying a higher purchase price, because the tax base is defined to be the price the buyer pays to the seller. Remittance responsibility affects real incidence to the extent that this makes it more costly to provide the seller with surplus—both the buyer and seller are best off when the buyer bears all of the remittance responsibility \((\alpha = 1)\), as equation (A.19) in Appendix A implies. As a result, for given remittance responsibility shares there is no value of the bargaining power parameter such that the shares of real incidence generally coincide with the remittance responsibility shares, as shown in equation (A.25).

### 3.2 Price Notch

If there is an upward discontinuity, or notch\(^{13}\) \(\Delta t\) in the tax schedule at cutoff price \(\bar{h}\), sales at prices at or above the cutoff price incur additional tax liability. Buyers remit \(h \alpha \cdot (t + \Delta t \{h \geq \bar{h}\})\) and sellers remit \(h \cdot (1 - \alpha) \cdot (t + \Delta t \{h \geq \bar{h}\})\). The price maximizes weighted surplus, which is now

\[W = \left(\frac{B - h \cdot (1 + \alpha \cdot (t + \Delta t \{h \geq \bar{h}\}))}{(1 - \beta)}\right) ^\beta \cdot \left(h \cdot (1 - (1 - \alpha) \cdot (t + \Delta t \{h \geq \bar{h}\})) - S\right)^{1-\beta}. \quad (3)\]

\(^{13}\)The notch \(\Delta t\) is in terms of the tax rate, not in dollars.
3.2.1 Strictly Dominated Region

The notch creates a strictly dominated region in which no transaction provides surplus to both parties. This strictly dominated region is the interval from \( \bar{h} \) to

\[
h^{SD} = \bar{h}(1 - (1 - \alpha) \cdot t)/(1 - (1 - \alpha) \cdot (t + \Delta t)).
\]  

(4)

Both the buyer and the seller are better off if the sale takes place at \( \bar{h} \) than if the sale takes place at a price \( h' \in (\bar{h}, h^{SD}) \): the buyer gets the house for a lower purchase price and remits less tax, while the seller receives \( \bar{h} \cdot (1 - (1 - \alpha) \cdot t) > h' \cdot (1 - (1 - \alpha) \cdot (t + \Delta t)) \) after remitting taxes. The size of the strictly dominated region increases with the seller’s remittance responsibility \( 1 - \alpha \). If the buyer bears all of the remittance responsibility (\( \alpha = 1 \)), then there is no strictly dominated region.

3.2.2 Bunching

Some buyer-seller matches that would have resulted in sales at prices above the cutoff price if there were no notch instead avoid incurring increased tax liability by selling at a price just below the cutoff price \( \bar{h} \). We call this behavior bunching. For bunching to occur, the seller must be willing to sell at a price just below the cutoff price, \( S < (1 - (1 - \alpha) \cdot t) \cdot \bar{h} \).

If the seller is willing to transact at a price just below the notch, bunching occurs when selling just below the cutoff price \( \bar{h} \) provides greater weighted welfare than any lower price and, given the increased tax rate, greater weighted welfare than any higher price. If the sale price absent the notch \( h \) is greater than \( \bar{h} \), then weighted welfare is greater just below the cutoff price than at any lower price. The weighted welfare from a sale at \( \bar{h} \) exceeds the weighted welfare from \( h^* = \frac{(1-\beta)B}{(1+\alpha(t+\Delta t))} + \frac{\beta S}{(1-(1-\alpha)(t+\Delta t))} \), the best price given the higher tax.
rates, when

\[ \left( B - (1 + \alpha t) \cdot \bar{h} \right)^{\beta} \cdot \left( \bar{h} \cdot (1 - (1 - \alpha)t) - S \right)^{1-\beta} > \left( B - (1 + \alpha \cdot (t + \Delta t)) \cdot h^* \right)^{\beta} \cdot \left( h^* \cdot (1 - (1 - \alpha) \cdot (t + \Delta t)) - S \right)^{1-\beta}. \] (5)

Bunching provides greater weighted welfare than transacting at \( h^* \) when \( h^* \leq \bar{h} \) or in the interval \([\bar{h}, h^U]\), where \( h^U \) is implicitly defined by Equation (5) and so varies across matches. Ignoring the constraint that \( S < (1 - (1 - \alpha) \cdot t) \cdot \bar{h} \) for bunching to occur, this interval would create a hole in the observed price distribution.

The size of the hole and amount of bunching first increases and then decreases as the buyer’s bargaining power \( \beta \) increases due to conflicting effects. A higher value of \( \beta \) increases the weighted welfare from lower prices, which are closer to the seller’s participation condition. The welfare from transacting at \( \bar{h} \) grows relative to the welfare from transacting at \( h^* \) above the notch \( \text{[14]} \) which expands the interval \([\bar{h}, h^U]\) where bunching generates greater weighted welfare than the best price above the notch. This effect tends to produce a larger hole and more bunching. However, \( h^* \) moves towards the seller’s participation constraint, so fewer matches in \([\bar{h}, h^U]\) can bunch, which reduces bunching and the size of the hole. The value of \( h \) also falls, so some matches may switch from bunching to \( h < \bar{h} \). For intermediate values of \( \beta \), the hole in the price distribution can extend beyond the strictly dominated region, but will not for the \( \beta = 0 \) and \( \beta = 1 \) polar cases.

If the seller has all the bargaining power \( (\beta = 0) \), the seller captures all surplus and thus bears all of the real incidence. Each house that sells does so at the highest price the buyer is willing to accept given the buyer’s remittance responsibility, except when that price is in the strictly dominated region. Sales that would take place in the strictly dominated region instead bunch at \( \bar{h} \) if they take place, so all bunching matches come from the strictly dominated region, which is both the interval \([\bar{h}, h^U]\) and the hole in the price distribution.

If the buyer has all the bargaining power \( (\beta = 1) \), the buyer captures all surplus and

\[ \text{[14]} \text{see Proposition A.1 in Appendix A.2 for a proof.} \]
thus bears all of the real incidence. Each house that sells does so at the lowest purchase price the seller is willing to accept given remittance responsibility. Bunching provides greater weighted welfare than a sale at any price above the notch, so the interval $[\bar{h}, h^U]$ extends to infinity. However, this interval is not the hole in the price distribution. If the seller will accept a price below the cutoff price, then $h = S/[1 - (1 - \alpha) \cdot t] < \bar{h}$, so there is no bunching, as any sale that can bunch can and does take place at a lower price. Otherwise, the sale price shifts upward to offset the seller’s increased remittance responsibility, which makes the strictly dominated region the hole in the price distribution.

3.2.3 Incidence and Bunching

The incidence of the notch for matches that bunch differs from the incidence of a proportional tax increase - the notch is akin to a minimum wage, where the way a nominal threshold is defined affects incidence. For a given value of the notch, because the notch is defined in terms of the price received by the seller before any tax she owes is due, increasing the fraction of remittance responsibility borne by the seller is analogous to increasing a minimum wage - increasing $(1 - \alpha)$ makes the seller’s participation condition for bunching $(1 - (1 - \alpha) \cdot t) \cdot \bar{h} > S$ less likely to hold, so fewer matches bunch and fewer sales take place.

If a match bunches, the introduction of the notch costs the seller $(h - \bar{h}) \cdot (1 - (1 - \alpha) \cdot t)$, where $h$ is the sale price absent the notch, and gives the buyer $(h - \bar{h}) \cdot (1 + \alpha t)$. Note that increasing the buyer’s bargaining power $\beta$ decreases $h$ and thus reduces the seller’s loss and buyer’s gain. Remittance responsibility also affects the price, and so has an ambiguous effect on the surplus changes for matches that bunch.

3.2.4 Lock-in

The increased tax rate for sales at prices above the notch causes an extensive margin response we call lock-in: some matches that would sell absent the notch no longer sell, and as a result fewer sales take place in equilibrium. We do not model the mechanism by which this reduces
equilibrium sale volumes, although in a random matching model like that described in Han and Strange (2014a) reducing the fraction of matches that sell increases expected search costs and time-to-market, which leads fewer buyers and sellers to choose to enter the market in equilibrium.\footnote{Given the substantial fixed costs incurred in listing a house, it is unlikely that a transaction tax of the size we consider leads many houses already listed on the market to de-list. Instead we expect that any lock-in effect occurs through fewer sellers choosing to list their houses and fewer buyers choosing to begin searching for a house.}

If the seller is not willing to sell the house at a price below the notch, \( S > (1 - (1 - \alpha) \cdot t) \cdot \bar{h} \), then the sale must take place at a price above the notch, if it takes place at all, and so the notch is equivalent to an increase in the tax rate from \( t \) to \( t + \Delta t \). For a sale to take place at a price above the notch, it must be the case that

\[
\frac{B}{(1 + \alpha t + \Delta \alpha t)} - \frac{S}{(1 - (1 - \alpha) \cdot (t + \Delta t))} > 0, \tag{6}
\]

so some sales that would take place at the tax rate \( t \) no longer take place at the tax rate \( t + \Delta t \). Bargaining power determines the division of surplus, but does not affect whether there is total surplus to divide, so lock-in does not depend on bargaining power, as is evident in condition (6) (i.e. \( \beta \) does not enter).

Sales that would, absent the notch, take place at prices far above the notch are unlikely to bunch. Any match that bunches has \( S < (1 - (1 - \alpha) \cdot t) \cdot \bar{h} \), and the buyer’s willingness to buy at the previous price \( h \) implies \( h \cdot (1 + \alpha t) < B \), so that any match that bunches has \( B - S > h \cdot (1 + \alpha t) - \bar{h} \cdot (1 - (1 - \alpha) \cdot t) \), regardless of bargaining power. For a fixed tax rate, if \( h \) is much larger than \( \bar{h} \), bunching requires a large gap between the buyer’s and seller’s reservation prices. We expect that both reservation prices are strongly correlated with the quality of the house, which makes large differences in reservation prices unlikely. Changes in transaction volume at purchase prices far above the notch are therefore due almost entirely to lock-in, and not to bunching. This enables us to use transaction volume away from the notch to construct difference-in-difference estimates of the lock-in effect.
3.3 Improvements Prior to Matching

Suppose that before a match occurs, the seller may improve the quality of the house including, but not limited to, in real estate terms, “staging” the house. Let \( c(I) \) be the cost of improvements that raise the (anticipated) buyer’s and seller’s reservation prices proportionally from \( B \) and \( S \) to \( B \cdot (1 + I) \) and \( S \cdot (1 + I) \). Improvements that increase the reservation prices proportionally do not change whether a sale takes place unless the improvements change the tax rate that applies to the sale. The purchase price is then \( h(I) \), a function of the amount of improvements. Assume that \( c(I) \) is convex and increasing, with \( c(0) = 0 \) and \( c'(0) = 0 \).

If there is no notch, the seller chooses the amount of improvements to maximize the after-tax sales price net of improvement costs, which we call the seller’s utility, \( U(I) = (1 - (1 - \alpha) \cdot t) \cdot h(I) - c(I) \). Without the notch, \( h(I) = (1 + I) \cdot h(0) \), where \( h(0) \) is the purchase price without improvements defined in (2) above. The seller chooses to make the quantity of improvements that satisfies the first-order condition, which is

\[
I^* = \left[ c'((1 - (1 - \alpha) \cdot t) \cdot h(0)) \right]^{-1}.
\]

Taking the comparative static with respect to the tax rate \( t \), increasing the tax rate reduces \( I^* \).

If there is a price notch at \( \bar{h} \), then the seller still chooses the amount of improvements to maximize the after-tax sales price net of improvement costs \( U(I) = [1-(1-\alpha)\cdot(t+\Delta t\{h(I) \geq \bar{h}\})] \cdot h(I) - c(I) \). Now, however, the notch makes \( h(I) \) a discontinuous function of \( I \), where a small change in the amount of improvements can lead a match to bunch, to transact at a price above the notch instead of bunching, or to fail to transact due to lock-in. If the purchase price after improvements absent the notch, \( h(I^*) \), is below the cutoff price, then

---

\( ^{16} \)Our data do not permit us to empirically examine the effect of the notch on improvements, although the model is informative.

\( ^{17} \)See Appendix A.3 for details.
the notch has no effect. If, with the notch, the sale takes place at a price above the cutoff price even if the seller makes no improvements, then the notch is equivalent to an increase in the tax rate, which reduces the quantity of improvements the seller chooses to make.

In cases where the cutoff price lies between the price without improvements and the price, absent the notch, if the seller makes the optimal quantity of improvements, \( h(0) \leq \bar{h} \leq h(I^*) \), or where the match would bunch if the seller made no improvements, the seller takes into account how improvements affect whether the match bunches. Sellers’ ability to make improvements adds matches with \( h(0) \leq \bar{h} \leq h(I^*) \) absent the notch to the set of matches that may bunch. For these sellers, making the minimum amount of improvements needed for the sale to bunch is a local maximum of \( U(I) \), as the price increases outweigh the costs until the match bunches, at which point the price ceases to increase.

The ability to make improvements also gives sellers the choice to pay for improvements to move the purchase price from the bunching price to a price above the notch, which can reduce the number of matches that bunch. Sellers choose between making the minimum amount of improvements such that the match bunches and making enough improvements that the sale takes place at a price above the cutoff price\(^{18}\) trading off increased improvement costs for increased after-tax proceeds. If the higher tax rate above the notch would result in lock-in, then the seller does not improve the house beyond the minimum amount needed to bunch. As seller’s ability to make improvements both increases the number of matches that can bunch and provides sellers with the option to pay to avoid bunching, the net effect of sellers’ ability to make improvements on the number of matches that bunch is ambiguous.

### 3.4 Time Notch

In the policy episode we will consider later, there is a combined price and time notch, that is, the price notch was instituted as of a cutoff date. To understand the effects of this policy,

\(^{18}\)The seller may choose to continue to make improvements beyond the minimum quantity of improvements such that the sale price jumps discontinuously up from the cutoff price if the version of (7) replacing \( t \) with \( t + \Delta t \) holds only by doing so.
we first analyze the effects of a tax schedule with only a time notch, and then the effects of a tax schedule with a combined price and time notch.

Nash bargaining determines both the purchase price $h$ and the sale date $T$, which is determined well in advance of the actual sale. The buyer’s ideal sale date is $T_b$, while the seller’s ideal sale date is $T_s$. The willingness to pay of the buyer and seller to move the transaction from date $T$ to their ideal dates are represented by strictly convex, continuously differentiable cost functions $k_b(|T - T_b|)$ and $k_s(|T - T_s|)$, with $k_b(0) = k_s(0) = 0$ and $k_b'(0) = k_s'(0) = 0$. The weighted surplus from a transaction at purchase price $h$ and sale date $T$ is

$$W(T, h) = (B - k_b(|T - T_b|)) - h \cdot (1 + \alpha t) \cdot (1 - \beta) \cdot (h \cdot (1 - (1 - \alpha) \cdot t) - S - k_s(|T - T_s|))^{1 - \beta}.$$  (8)

At an interior solution, $T$ is uniquely defined by the condition that represents tangency of the buyer’s and seller’s indifference curves in time-price space,

$$-\frac{\partial k_b(|T - T_b|)}{\partial T} \cdot (1 + \alpha t) = \frac{\partial k_s(|T - T_s|)}{\partial T} \cdot (1 - (1 - \alpha) \cdot t),$$  (9)

which implies that the two parties cannot gain by trading a change in the purchase price for a change in the sale date. Given $T$, the first-order condition for $h$ implies that

$$h = \frac{(1 - \beta) \cdot (B - k_b(|T - T_b|))}{(1 + \alpha t)} + \frac{\beta \cdot (S + k_s(|T - T_s|))}{(1 - (1 - \alpha) \cdot t)}.$$  (10)

If there is a time notch, sales that take place after a cutoff date $\bar{T}$ are subject to tax at rate $t + \Delta t$. Sales that would take place before the cutoff date absent the notch are unaffected. Other sales either bunch just before the cutoff date or are subject to the higher tax rate and take place after the cutoff date, whichever generates higher surplus $W(T, h)$. The sale does not take place if neither bunching nor selling at the higher tax rate after the cutoff date generates positive surplus.
If the sale takes place after the cutoff date, the notch is equivalent to an increase in the tax rate \( t \). The increased tax rate increases the cost in total surplus of using the price to give surplus to the seller, while the cost in total surplus of moving the sale date toward the seller’s preferred date is unchanged. Formally, condition (9) implies that at a higher tax rate \( T \) is closer to the seller’s preferred sale date. The purchase price adjusts to take account of the higher tax rate and adjusted sale date. Accordingly, the lost surplus is divided between the buyer and seller.

If the sale bunches at \( T = \bar{T} \), condition (9) no longer holds with equality, and the transaction price is

\[
h = \left( 1 - \beta \right) \cdot \left( B - k_b(|\bar{T} - T_b|) \right) \left( 1 + \alpha t \right) + \beta \cdot \left( S + k_s(|\bar{T} - T_s|) \right) \left( 1 - \left( 1 - \alpha \right) \cdot t \right) .
\]  

(11)

Relative to the case without the notch, both parties lose surplus in proportion to bargaining power. The party that would prefer to transact later may benefit from the price change but suffers due to the earlier sale date, while the party that would prefer to transact earlier may benefit from the earlier sale date but suffers from the price change. Here the time notch differs from a price notch with fixed sale date, where bunching at the cutoff price pins down the price, so there is no margin along which the reduction in surplus can be shared, and the seller alone loses surplus due to bunching.

Weaker time preferences correspond to more bunching at the cutoff date and a larger hole in the sale date distribution after the cutoff date. Note that if \( k_b(|\bar{T} - T_b|) \) is approximately equal to \( k_b(|T - T_b|) \), where \( T \) is the optimal sale date absent the notch, and \( k_s(|\bar{T} - T_s|) \) is approximately equal to \( k_s(|T - T_s|) \), then bunching at the time notch has approximately no effect on the price, and thus does not affect substantially either the buyer’s or seller’s surplus. As a consequence, the closer \( T \) is to \( \bar{T} \), the more likely a match is to bunch for given \( k_s() \) and \( k_b() \) functions. Additionally, weaker time preferences make the welfare cost of bunching smaller, while the welfare cost of transacting after the sale date is always substantial due
to the increased tax liability for both parties, so weaker time preferences expand the range of times $T$ that bunch and thus the size of the hole in the sale date distribution. In the limit as both parties are indifferent across transaction times, the hole is infinite, because the constraint that $T = \bar{T}$ does not affect welfare.

In contrast to the price notch with fixed sale date, the adjustment in price ensures that if the total joint surplus from bunching just before the cutoff date is positive, both the buyer and seller are always willing to transact. Lock-in is thus limited to those matches that generate negative total joint surplus at any price at the cutoff date or at the higher tax rates incurred by transacting at a price above the notch.

Matches that, absent the time notch, take place long after the cutoff date are highly unlikely to bunch. For any match, a sufficiently early $\bar{T}$ is far enough before either party’s preferred sale date that the joint surplus from bunching at $\bar{T}$ must be negative, which rules out bunching. Therefore the distribution of sales at dates sufficiently long after the cutoff date changes only due to lock-in, and not bunching.

### 3.4.1 Time Notch in a Random Matching Model

A time-notched tax has similar implications in a random matching model (of the type discussed in Han and Strange 2014a) where the seller’s reservation price is endogenously determined but the buyer’s access to other markets makes her reservation price exogenous. A (lump-sum) tax increase causes the seller’s reservation price to fall by less than the amount of the tax, and reduces market tightness (the number of buyers per seller). Conditional on contact, a sale is less likely, so seller time on the market rises while the effect of the tax on buyer time on the market is ambiguous.

Given an anticipated future tax increase, like the time notch described above, those sellers likely to be subject to the tax increase have an incentive to reduce their reservation prices slightly to increase the likelihood that they sell before the tax increase takes effect. This tends to produce time bunching before the cutoff date, as the probability of a sale conditional
on a contact rises before the cutoff date. Fewer sellers choosing to enter the market would attenuate this time bunching; however, to the extent that sellers’ fixed costs of entry are sunk prior to the announcement of the time notch, the notch will not affect seller entry.

### 3.5 Combined Time and Price Notch

Suppose there is a combined price and time notch, in which sales are subject to discretely higher tax liability if they take place both after a cutoff date and above a cutoff price, as occurs in our empirical application. The combined notch does not affect transactions that would, absent the notch, take place before the cutoff date, at a price below the cutoff price, or both. If a sale would take place above the cutoff price and after the cutoff date, either it is subject to the higher tax rate and takes place at an adjusted purchase price and sale date, or, if bunching can provide higher weighted surplus, it bunches. If neither provides positive total surplus, then the sale does not take place.

A sale that bunches takes place either at the cutoff date and a price above the cutoff price or at the cutoff price and a date after the cutoff date, whichever provides greater weighted surplus. For a sale that bunches at the cutoff date $T = \bar{T}$, the analysis is exactly as in section 3.4 above. A sale that bunches at the cutoff price $h = \bar{h}$ takes place at a sale date uniquely determined by the first-order condition

$$
\frac{\partial k_b(|T - T_b|)}{\partial T} = \frac{\partial k_s(|T - T_s|)}{\partial T} = \frac{(1 - \beta)}{\beta} \left[ B - k_b(|T - T_b|) - \bar{h}(1 + \alpha t) \right] \left[ \bar{h} \cdot (1 - (1 - \alpha) \cdot t) - k_s(|T - T_s|) - S \right].
$$

That is, the sale date moves towards the seller’s ideal date, which partially offsets the seller’s surplus lost from reducing the price to $\bar{h}$. The buyer and seller share in the loss of surplus to the combined notch, and, as in section 3.4, if the total joint surplus is positive, then both the buyer and seller are willing to transact.

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19 A sale that would, absent the combined notch, take place above the price notch and after the cutoff date must, if it bunches, have either $h = \bar{h}$, $T = \bar{T}$, or both. Otherwise there exists a small change in the purchase price and sale date that is Pareto-improving.

20 This adjustment process may result in the corner solution $T = T_s$, which limits the scope for adjustment.
3.5.1 Time Preference Intensity and Bunching

The intensity of the buyer’s and seller’s time preferences determines whether sales bunch at the cutoff price or at the cutoff date. If time preferences are weak, sales are more likely to bunch at the cutoff date rather than at the cutoff price. If time preferences are intense, sales are more likely to bunch at the cutoff price than at the cutoff date.

When time preferences are intense, both parties to the sale insist on a sale date close to their most preferred sale date. Sales happen only if $T_b$ and $T_s$ are approximately equal, at sales dates in the small interval between the two. Only matches with $T_b$ and $T_s$ in a neighborhood of $\bar{T}$ obtain positive surplus from bunching at $\bar{T}$. See Proposition A.2 in Appendix A.4 for a formal statement and proof of this result. Bunching at the cutoff price is more likely to provide positive surplus because it does not impose a constraint on $T$.

Even holding the sales date fixed at its optimum in the interval between $T_s$ and $T_b$, the set of matches that can bunch at the cutoff price is defined by the price notch model in (3.2) (redefining $B = B - k_b(|T - T_b|)$ and $S = S + k_s(|T - T_s|)$, which does nothing for matches with $T_b = T_s$), and if the sales date is only approximately fixed, any sales date adjustment that accompanies bunching at the cutoff price must produce a welfare improvement (and thus more matches able to bunch) relative to the model in (3.2).

When time preferences tend toward indifference, bunching at the cutoff date incurs weighted welfare losses that tend to zero, while bunching at the cutoff price incurs weighted welfare losses bounded away from zero for all but a select few matches, so matches are more likely to bunch at the cutoff date than the cutoff price. If the buyer and seller are nearly indifferent across all sale dates, then bunching at the cutoff date provides almost the same surplus as selling at the date the sale would have taken place at without the notch. In the limit, the time preference terms drop from the model and the constraint requiring that $T = \bar{T}$ does not affect the weighted surplus. Bunching at the cutoff price incurs losses of weighted surplus bounded away from zero for matches that would, absent the notch, transact at prices outside a neighborhood of $\bar{h}$ and where bunching at the cutoff price provides both parties
with weighted surplus bounded away from zero, by Proposition A.3 in Appendix A.4.

In short, the combined price and time notch leads some sales to bunch at the cutoff date at prices above the price cutoff or at the price cutoff on sales dates after the cutoff date. Stronger time preferences result in more price bunching and less time bunching. There is also a lock-in effect: as above, some houses that would sell absent the notch no longer sell because bunching at either the cutoff date or price or selling at a price and date subject to the higher tax rate would generate negative total joint surplus.

3.6 Relation to Empirics

The model predicts that the combination of a time notch and a price notch should result in time bunching at the cutoff date at prices above the cutoff price, and in price bunching at the cutoff price at dates after the cutoff date. More flexible time preferences result in more bunching at the cutoff date relative to the cutoff price. If we observe no bunching at the cutoff date, we can infer that sales dates are effectively held fixed by inflexible time preferences.

Fixing the total tax rate and all other parameters, increasing the fraction of the total tax payment the seller is responsible for remitting reduces the number of sales that can bunch. As a result, for a given total tax rate and change in tax rate at the notch, we would expect to observe less bunching in the empirical context we study, where the seller is responsible for remitting half of the total tax, than in the empirical settings of Best and Kleven (2013) and Kopczuk and Munroe (2014), where the buyer is responsible for remitting the full amount of the total tax. Additionally, in our context there is a strictly dominated region when the seller bears some of the remittance responsibility, in which no match optimally takes place—any matches that take place in the strictly dominated region are due to optimization frictions in general, and a lack of awareness in particular.

The model also predicts that some sales that would have occurred absent the introduction of the tax notch will not happen once the tax notch is in place—a lock-in effect. With fixed
sale date and a price notch, any decline in the number of sales at prices far above the notch (also near the notch if the bunching region is included above, rather than below the notch) is due almost entirely to lock-in, and not to bunching. Similarly, with bargaining over both sale date and purchase price, any decline in the number of sales long after the cutoff date at prices far above the cutoff price is due almost entirely to lock-in. Thus, we expect that our estimates of the amount of lock-in using changes in the distribution of sales away from the notches will accurately capture the amount of lock-in.

4 Data

We have data on the sales price for all residential housing transactions in D.C. in the years 1999 to 2010.\textsuperscript{21} This data set was purchased from CoreLogic, a firm that specializes in real estate data and analysis. We drop apparent house sales duplicates (i.e., we keep only one observation if the data shows the same house selling twice in one day for the same price). We also exclude all transactions in which the house sold twice or more on the same day (or multiple sales of the same house were recorded on the same day) for different prices both greater than $50,000 because, although it is clear that only one of these observations should be kept, it is ambiguous which price is the relevant, arms-length transaction price. These exclusions apply to a very small portion of the data and have no noticeable effect on the estimates we present below.

The D.C. Office of Tax Revenue has also provided us with some characteristics of all houses that were assessed in D.C. in 2005 and 2010. We use 2005 values unless not available, in which case we use the 2010 values. These characteristics are summarized for each house that sells in Table A.1. The housing characteristics for all houses include: condition, grade\textsuperscript{22} year built, living area, number of rooms, number of bedrooms, number of full and half baths, neighborhood location (which we have collapsed for the purposes of summary statistics into

\textsuperscript{21}For this reason, we cannot explore the cross-border effect of the transfer tax.

\textsuperscript{22}Grade is very detailed ranking of the overall quality/condition of the house with too many categories to make it worth listing in the summary statistics.
Southeast, Northeast, Downtown, and Northwest D.C.), acreage, and building type. The housing characteristics for all houses except condos include: number of stories, number of fireplaces, type of flooring, type of roof, type of exterior, type of heating, whether or not there is air conditioning. The housing characteristics available only for condos include the type of parking and the number of parking spaces.

Figure 2 displays the Case-Shiller price index of houses in Washington D.C. by quarter. The figure highlights that prices changed substantially over the period 1999-2010. House prices more than doubled between January 1, 1999 and the peak in house prices in the second quarter of 2006. It is also interesting to note that the median house sale price in our data was actually approximately $250,000 at the time of the 2003 policy that imposed the price notch at that point, 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. We cannot confirm whether or not this was a coincidence, but it seems unlikely to be one. It is also worth noting that Han and Strange (2014b) find that in D.C. during the boom years of 2003-2006, as many as 29% of sales reported in a survey had bidding wars, while only 12 percent had bidding wars during the bust years 2007-2010. Given that bidding wars are an indicator of high seller bargaining power, we might expect to find lower seller bargaining power in our context than in other contexts in which there were more bidding wars.

If there were other tax changes during the same time period that affected potential sellers above and below both the price and time notches differently, these changes could bias our difference-in-differences estimates of the lock-in effect by attributing these other changes to the change in the housing transfer tax. Four approximately concurrent policies are worth considering. The D.C. First-Time Homebuyer Credit provides a non-refundable income tax credit to first-time D.C. 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. Washington D.C.’s property tax changed over time, but applies uniformly to all houses in our sample. The Homestead Deduction exempts from property tax assessed value up to
This figure plots the average Case-Shiller Price Index in Washington D.C. by quarter. The first vertical dashed line marks the quarter in which the 2003 tax notch was implemented, and the second vertical dashed line marks the quarter in which the 2006 tax notch was implemented.

a given threshold. The exemption amount changed over time, but it also 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. This will only matter in the analysis that follows to the extent that it had a larger effect for those above the price notch relative to below. This would bias our lock-in effect estimates upward.

5 Examining Selling Behavior around the Price Notch

In this section, we examine selling behavior around the price notch. We can observe the volume and distribution of sales prices both before and after the price notch has been im-
implemented. Based on the model we developed in Section 3, there are two possible reasons the density of house sales newly taxed at a higher rate after the price notch is implemented will decline: (i) some sellers will alter the sales price of their house to a price below the price notch, and (ii) the higher tax rate above the notch will discourage some sales due to an increased lock-in effect.

Examining behavior in the region of the price notch can be informative along two main dimensions: (i) it allows us to speak to the bargaining power of the buyer ($\beta$), (ii) and it provides evidence regarding the degree to which individuals understood the reform, the incentives created by the reform and whether this knowledge changed over time. Both have important implications for the lock-in analysis we conduct in Section 7.

Figure 3: Pre and Post Reform Sales by House Price

![Figure 3: Pre and Post Reform Sales by House Price](image)

This figure plots the count of house sale prices sold pre- (yellow line) and post-reform (blue line) above and below the price notch. The data are in $5,000 bins. The area above the price notch is shaded in grey. The entire comparison group series is adjusted so that the mean level of house sales for both groups are the same pre-reform.

We begin our analysis of the selling behavior around the price notch with a raw plot of house sale counts on either side of the price notch. Figure 3 plots the counts of house sales in $5,000 bins for houses that sold pre- (yellow line) and post-reform (blue line) for both the 2006 reform (left-panel) and the 2003 reform (right panel). As we will see below, the

\[23\] All dates in this paper are based on the recording date for each house sale. This is the date that determines the applicable tax rate.

27
analysis of the 2006 reform is slightly more straightforward, so throughout this section we will always begin with the discussion of this reform. For the 2006 reform, the pre-period is three years before the 2006 reform (October 1, 2003 to September 30, 2006) and the post-period is three years after the 2006 reform (October 1, 2006 to September 30, 2009). The only clear change we see after the reform in the left panel of Figure 3 is that many more houses sell for just below $400,000 and many less sell just above $400,000, suggesting that a significant number of sales that otherwise would have occurred at prices just above the notch move below the notch to avoid facing the higher average tax rate above the notch.

The right panel of Figure 3 provides the same plots for the 2003 reform. Because this tax change was only in place for seven quarters we cannot construct the figures using three years of data pre- and post-implementation. Moreover, average sale prices were rising quite a lot over this period and never decreased, so our comparison group pre-reform has greater potential to be suspect. We compare the seven quarters during the reform (January 1, 2003 to September 30, 2004) to the eighteen months before the tax was introduced (July 1, 2001 to December 31, 2002) and the eighteen months after the tax was eliminated (October 1, 2004 to March 31, 2006). The shift of the density from above to below $250,000, where this price notch was located, is smaller after the 2003 reform was implemented. This is perhaps unsurprising given that newspaper coverage of the price notch in this reform was scarce—there was only one article that mentioned the price notch, and it described its location at $250,000 instead of $249,999—in major Washington D.C. newspapers (see Appendix E). This is in contrast to the 2006 reform, for which there were three newspaper articles that mentioned the price notch, two of which described the location of the notch properly.

We turn now to a method that will allow us to pin down this behavior precisely. The first step often is to estimate a polynomial in the region of the notch after the price notch is implemented, as done by [Best and Kleven (2015)] and [Chetty et al. (2011)]. The pooled
cross-section regression is given by:

\[ c_{it} = \gamma_0 + \sum_{j=1}^{5} \gamma_{1j} b_{jt} + \sum_{k=1}^{100} \gamma_{2k} \text{below}_{kt} + \sum_{l=1}^{50} \gamma_{3l} \text{above}_{lt} + \sum_{m=1}^{5} \gamma_{4m} d_{mt} + \nu_{mt} \]  

(13)

where \( c_{it} \) is the count of house sales in bin \( i \) at time \( t \). The prices in each bin range from \([b_{it}, b_{it} + 100]\), so the first term on the right-hand side is a fifth-order polynomial in the house sales price\(^{24}\). The variables \( \text{below}_{kt} \) are a set of indicator variables for being in bin \( k \) within $10,000 below the price notch and the variables \( \text{above}_{lt} \) are a set of indicator variables for being in bin \( l \) within $5,000 above the price notch. The indicator variables \( d_{mt} \) are a set of indicators for sales prices at the following round-number endings: 500, 1,000, 5,000, 10,000, and 25,000. We estimate this regression within $100,000 on either side of the price notch\(^{25}\).

We modify this regression and estimate it in differences comparing these coefficients after the tax was implemented to before the tax was implemented. We do this because when we estimate equation (13) we find bunching around the notch both pre- and post-reform because even our polynomial and controls for round number bunching do not fully capture the non-uniformity of the density across sales prices in this setting. The estimating equation in differences is given by:

\[ c_{it} = \gamma_0 + \sum_{j=1}^{5} \gamma_{1j} b_{jt} \cdot \text{post}_{it} + \sum_{k=1}^{100} \gamma_{2k} \text{below}_{kt} \cdot \text{post}_{kt} + \sum_{l=1}^{50} \gamma_{3l} \text{above}_{lt} \cdot \text{post}_{lt} + \sum_{m=1}^{5} \gamma_{4m} d_{mt} \cdot \text{post}_{it} \\
+ \sum_{j=1}^{5} \gamma_{5j} b_{jt} + \sum_{k=1}^{100} \gamma_{6k} \text{below}_{kt} + \sum_{l=1}^{50} \gamma_{7l} \text{above}_{lt} + \sum_{m=1}^{5} \gamma_{8m} d_{mt} + \gamma_9 \text{post}_{it} + \nu_{mt}. \]  

(14)

For the 2006 reform, \( \text{post} = 0 \) for the three years before price notch was introduced and \( \text{post} = 1 \) for the three years after it was in place. For the 2003 reform, \( \text{post} = 0 \) for the

\(^{24}\)The results are qualitatively the same with a third- or seventh-order polynomial.

\(^{25}\)We have tried narrower regions, but the wider region yields very similar results and allows us to pin down the polynomials and round number effects more precisely.
18 months before price notch was introduced and the 18 months after the price notch was eliminated and post = 1 for the 21 months during which the price notch was in place. In this setting, $\sum_{k=1}^{100} \gamma_{2k}$ captures the excess bunching below the price notch when the price notch is in place and $\sum_{t=1}^{50} \gamma_{3t}$ captures the hole in the distribution above the price notch when the price notch is in place.

**Figure 4: Manipulation Around the Price Notch**

This figure is constructed so that the estimated count in each bin is relative to the estimated count in the period before the implementation of the price notch. Specifically, this figure graphs the predicted values from:

$$\sum_{j=1}^{50} \gamma_{1j} \cdot b_{jt} \cdot \text{post}_{jt} + \sum_{k=1}^{100} \gamma_{2k} \cdot \text{below}_{kt} \cdot \text{post}_{kt} + \sum_{l=1}^{50} \gamma_{3l} \cdot \text{above}_{lt} \cdot \text{post}_{lt}.$$ 

The data are grouped in $2,500$ bins for the region within $50,000$ of the price notch. The area above the price notch is shaded in grey.

Results from equation (14) are reported in Table 1. The standard errors are clustered by month-year to allow for correlation of bins within a month-year. The top panel of the table reports results for the 2006 reform and the bottom panel does the same for the 2003 reform. Column (1) reports our baseline results, in which p-values are in square brackets. Figure 4 provides a visualization of the analysis for this specification where we show the excess bunching and the corresponding hole in the distribution of transaction prices relative to the estimated counterfactual. The data in the figure are aggregated to $2,500$ bins. The

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26These are the most conservative standard errors, which makes sense as it is the month-year that is superfluous in this analysis (i.e., we could estimate this aggregated to the pre-post level and get approximately the same estimates). One might alternatively be concerned about serial correlation of bins over time; however, the standard errors from addressing this correlation are strictly smaller than the standard errors reported in Table 1.
figure is constructed so that the estimated count in each bin is relative to the estimated count in the period before the implementation of the price notch (so if counts were the same pre- and post-implementation for a given bin, this will show up as a zero in the figure).

The figure highlights several important results: (i) as expected, the excess mass below the notch is concentrated in the $2,500 bin immediately below the price notch; (ii) for the 2006 reform, the hole above the price notch is concentrated in the $2,500 bin immediately above the price notch, whereas it is more spread out for the 2003 reform; and (iii) the density does not decline above the price notch, which is consistent with no lock-in effect.\(^{27}\)

For the 2006 reform, we estimate in Table 1 Column (1) that an excess of 5.5 houses per month-year bunch below the price notch and this estimate is significant at the one percent level. This amounts to a 28 percent increase in sales in the bunching region. This represents a 10 percent increase in sales per $1,000 increase in tax liability, which is slightly less bunching than in Best and Kleven (2015), who find a 13 to 17 percent increase per $1,000 increase in tax liability. This is not surprising given that the model predicts there will be less bunching when the seller bears more of the remittance responsibility, and may also be a result of more tax avoidance in Best and Kleven (2015), which we will discuss in more detail below.

We find that there are 3.2 houses per month-year less that sell in the region $5,000 above the price notch (also significant at the one percent level). While we find that, on average, the excess mass below the notch is larger than the hole above the notch, this difference is not significant. Thus, based on a generalized version of the bargaining power formula from Kopczuk and Munroe (2015) in Appendix B, we cannot reject the null that \( \beta = \alpha = 0.5 \), which contrasts strongly with their result.

We find that about two-thirds of the hole in the price distribution above the notch comes from the strictly dominated region and the remaining one-third comes from the region above the strictly dominated region; this one-third estimate is statistically significant at the five percent level. Consistent with our estimate of \( \beta \) of 0.5, this rules out the two special cases...\(^{27}\)It does eventually decline above the price notch for the 2003 reform, but as discussed above, this might be due to a poor comparison group, rather than a true lock-in effect.
in which either the buyer or the seller has all the bargaining power, because it was shown in Section 3 that in both these extreme cases the hole of above the price notch is confined to the strictly dominated region.

The theory also predicts that if there are no optimization frictions, there would be no sales with transaction prices in the strictly dominated region after the price notch was implemented. Unlike other settings that have considered a strictly dominated region due to

### Table 1: Bunching Analysis

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<td>114,057</td>
<td>114,057</td>
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<td>-2.16***</td>
<td>-2.16***</td>
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<td>% decrease in SD region relative to no price notch:</td>
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<td>67.70%</td>
<td>67.70%</td>
<td>67.70%</td>
<td>65.67%</td>
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<td>144,072</td>
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<td>30K above</td>
<td>15K below</td>
<td>2 Years</td>
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</table>

p-values from clustered standard errors by month-year are in square brackets. Asterisks denote significance at the 1% (***)**, 5% (**), and 10% (*) levels.
a tax notch (e.g. Kleven and Waseem, 2012), moving out of the strictly dominated region by decreasing the sale price (even at the last minute just before the sale is finalized) is approximately costless. Hence, individuals who remain in the strictly dominated region in our setting generally reflect a particular type of optimization friction: a lack of awareness. Using equation (4) from Section 3, the strictly dominated region for the 2006 reform is \([\$400000, \$401421]\) (rounded to the nearest dollar). Similarly, for the 2003 reform, the strictly dominated region is \([\$250000, \$251015]\). Given that our data is grouped in $100 bins, for this analysis we will define our strictly dominated region rounded to the nearest $100. For the 2006 reform, we find that sales in the strictly dominated region decline by only 68 percent, suggesting that both parties are unaware of the price notch for about one-third of transactions.

One thing to note is that not all of the newspaper articles leading up to the implementation of each notch got the exact location of the price notch correct; one describes the notch as if one would still owe the lower tax rate at a sales price of exactly $400,000, but would be subject to the higher tax rate for any price above that. All the newspaper articles in major Washington D.C. newspapers are listed in Appendix E in 2006, there were three newspaper articles that mentioned the price notch, two of which described the location of the notch properly. To the extent that individuals were misinformed about the location of the price notch (within $1) from these newspaper articles, and this reflects a general confusion among the public, our estimates of awareness will be biased towards zero. However, when we re-run the estimates placing $400,000 on the other side of the price notch, we obtain very similar results, so this appears not to bias our estimates in practice.

We find very similar results for the 2003 reform with one main exception: the missing mass within the strictly dominated region is much lower. Related to this, there is only a 3.3 percent decline in house sales in the strictly dominated region above the price notch. This is consistent with less awareness of this reform, as suggested by less media attention. Additionally, no news articles described the location of the notch exactly correct. When we
re-run the estimates placing $250,000 on the other side of the price notch, we obtain similar results overall, but much higher awareness (39 percent instead of 3.3 percent), suggesting that there was substantive confusion about the precise location of this notch.

Table 1 Columns (2) through (6) provide a variety of sensitivity checks. We find that our estimates are quite robust. Column (2) includes a seventh-order, rather than fifth-order polynomial in prices and finds almost identical results. Column (3) expands the region in which the indicator variables above the price notch are estimated from $5,000 to $30,000. The main purpose of this exercise is to ensure that we capture in our estimates all of the sales missing above the notch because they shifted below the notch are captured in our estimates. For the 2006 reform, the estimate of the decrease in house sales above the notch is almost identical. For 2003, it is larger, but this difference is far from statistically significant. The fact that the hole above the price notch is so narrow rules out the possibility of extensive tax avoidance or tax evasion schemes in response to the price notch. In particular, we might imagine that the price notch induces a change in who pays the closing costs or some form of side payments between the buyer and seller. To the extent that these behaviors are going on, they are occurring within a very narrow region above the notch. We find a much smaller hole than Best and Kleven (2015); in particular, our hole extends $5,000 above the price notch, which is about 1.8 times the size of the tax liability. Best and Kleven (2015) find a hole that extends to 5 times the size of their tax liability, which may be due to increased tax avoidance in their setting because nearly four out of 10 transactions are in cash (Pickford 2015). This might facilitate side payments and thus bunching.

We expect that all shifting of house sales from above to below the price notch should move into a narrow region just below the price notch; however, to verify that we have fully captured this movement, we can expand the region in which the indicator variables below the price notch are estimated from $10,000 to $15,000 in Table 1 Column (4). We find the estimate of the region below the price notch is quantitatively very similar and not statistically

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28 All estimates between $5,000 and $30,000 also yield similar estimates, and are available from the authors upon request.
different from the baseline estimates.

By including three years of data pre- and post-implementation for the 2006 reform and three years of comparison group data (split before implementation and after elimination) for the 2003 reform, we increased our statistical precision. However, for our estimates to be valid, we must assume that all behavior we capture in the pre-period would be the same in the post-period if not for the implementation of the price notch. This assumption is potentially less tenable as we increase the number of years included in our estimation. For this reason, we decrease our three-year window to two years in Column (5) and one year in Column (6). The results in both these columns are similar to our baseline estimates. They also highlight that awareness did not change significantly over time. This suggests that there was not meaningful learning over time; if there was, we would expect the estimates of the excess mass below the price notch to increase as the post-implementation window increases.

To summarize, in this section we have learned that there was significant manipulation of house prices around the notch, implying that the buyer and seller have approximately equal bargaining power. We find less bunching in our setting than did Best and Kleven (2015), which is likely due both to the seller bearing more remittance responsibility in our setting and more tax avoidance in their setting. This section has also provided compelling evidence that about two-thirds of individuals were aware of the 2006 reform, while awareness was lower for the less salient 2003 reform. Hence, when we estimate the lock-in effect in Section 7 we should not attribute a lack of response, particularly for the 2006 reform, to an overall unawareness of the tax reform.

6 Examining Selling Behavior Around the Time Notch

This section examines the response to the change in tax regime—the time notch. By looking at the behavior in the immediate vicinity of the time notch, we can learn whether individuals

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29It would be interesting to look at learning over a shorter time horizon, such as the first several months after the reform, but we don’t have the statistical power to conduct this analysis with sufficient precision.
decided to re-time their house purchases to avoid the higher tax. This section will speak both to the sale-time preferences of buyers and sellers, as well as the degree to which this tax affects the time a house sits on the market.

To examine the time notch, we pursue the same strategy as in the last section, but now we are looking for changes in the mass of house sales on either side of the time notch, rather than on either side of the price notch. Our regression specification is as follows:

\[
c_{it} = \gamma_0 + \sum_{j=1}^{5} \gamma_{1j} date_{it}^j \cdot treat_{it} + \sum_{k=1}^{60} \gamma_{2k} before_{kt} \cdot treat_{kt} + \sum_{l=1}^{60} \gamma_{3l} after_{lt} \cdot treat_{lt} \\
+ \sum_{j=1}^{5} \gamma_{4j} date_{it}^j + \sum_{k=1}^{60} \gamma_{5k} before_{kt} + \sum_{l=1}^{60} \gamma_{6l} after_{lt} + \gamma_7 treat_{it} + \sum_{m=1}^{11} \gamma_{8m} mth_{mt} \\
+ \sum_{q=1}^{6} \gamma_{9q} dow_{qt} + \sum_{r=1}^{31} \gamma_{10r} dom_{rt} + \sum_{i=1}^{19} \gamma_{11i} bin_{it} + \nu_{mt}. \tag{15}
\]

where \( c_{it} \) is the count of house sales in bin \( i \) at time \( t \). Each bin \( i \) is $5,000 wide for date \( t \). The term \( date_{it}^j \) is a fifth-order polynomial based on the recording date. The variables \( before_{kt} \) are a set of indicator variables for being in bin \( k \) within two months before the tax notch was implemented and the variables \( after_{lt} \) are a set of indicator variables for being in bin \( l \) within two months after the tax notch was implemented. The indicator variables \( mth_{mt} \) are a set of month indicators, \( dow_{qt} \) are a set of day of the week indicators, \( dom_{rt} \) are a set of day of the month indicators, and \( bin_{it} \) are a set of indicators for being in bin \( i \); these are included to capture the lumpiness in house sales across different months, days of the week, days of the month, and sale prices. We estimate this regression within $50,000 on either side of the price notch. We define \( treat = 0 \) for prices $10,000 below the price notch and \( treat = 1 \) for prices above this cut point. We split the treatment and comparison

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30 Because we are looking across time rather than prices, the width of these bins does not matter; the results would be equivalent if we again used $100 bins as for the price notch analysis. Round-number indicator variables are also not important in this context; our results are almost identical when they are included.

31 The recording date is the date that matters for the tax treatment of the house sale.

32 We will consider some robustness to different windows below, but we choose this as our baseline to match the analysis in Appendix C which uses a very similar setting to estimate the lock-in effect over time.

36
group at $10,000 below the price notch instead of at the price notch, so that we do not capture the movement from the strictly-dominated region to the region just below the notch that we documented in the previous section. For the 2006 reform, our time window is three years before and after the implementation of the tax notch. For the 2003 reform, the time window begins 18 months before price notch was introduced and ends 18 months after the price notch was eliminated. In this setting, $\sum_{k=1}^{60} \gamma_{2k}$ captures the excess sales before the price notch was implemented and $\sum_{l=1}^{60} \gamma_{3l}$ captures the hole in the distribution after the tax notch was implemented. Note that for the 2003 reform, there is a second set of before and after indicators capturing the behavior around the second time notch when the tax notch was eliminated.

**Figure 5: Manipulation Around the Time Notch**

This figure is constructed so that the estimated count in each bin is relative to the estimated count in the period before the implementation of the price notch. Specifically, this figure graphs the predicted values from: $\sum_{j=1}^{5} \gamma_{1j}\text{date}_{it} \cdot \text{treat}_{it} + \sum_{k=1}^{60} \gamma_{2k}\text{before}_{kt} \cdot \text{treat}_{kt} + \sum_{l=1}^{60} \gamma_{3l}\text{after}_{lt} \cdot \text{treat}_{lt}$. The data are grouped in $2,500$ bins for the region within $50,000$ of the price notch. The area above the price notch is shaded in grey.

The results of estimating equation (15) are reported in Table 2. The standard errors are clustered by bin to allow for correlation within a bin across time.\footnote{These are the more conservative standard errors, which makes sense as it is the bin that is superfluous in this analysis (i.e., we could estimate this aggregated to the treatment-comparison group level and get approximately the same estimates). One might alternatively be concerned about serial correlation of bins over time; however, the standard errors from addressing this correlation are strictly smaller than the standard errors reported in Table 2.
table reports results for the 2006 reform and the bottom panel does the same for the 2003 reform. Column (1) reports our baseline results, in which p-values are in square brackets. Columns (2) to (5) are robustness checks. In Column (2) we include a 7th-order polynomial (in date) rather than a 5th-order polynomial. In Columns (3) and (4) we expand the window around the time notches from two months to three months in case we did not fully capture the manipulation around the time notch in our original specification. We expand the price range we consider from $50,000 to $75,000 in Column (5) to see if we can pin down the effects in the 2006 reform more precisely when we include a wider range of data.

Figure 5 provides a visualization of the analysis for this specification where we show the excess bunching and corresponding hole in the data relative to the estimated counterfactual. The figure plots the predicted values from the first three terms in equation (15), so that the estimated count in each bin is the count relative to the estimated count below the price notch. The patterns in this figure are consistent with the pattern we would expect if there were some response around the time notch to avoid the anticipated implementation of the higher tax rate. For the 2006 reform, there are more sales above the price notch before the time notch and less sales in the same price range after the time notch; the same is true around the implementation of the 2003 reform, except there are also excess sales after the time notch, perhaps because individuals intended to sell their house before the time notch, but didn’t quite make it. Right before the tax notch was removed, sales fell above the price notch, and picked up a bit afterwards. However, most of these results are not statistically significant as seen in Table 2; only the excess sales before the implementation of the price notch in 2003 are statistically significant. When comparing the magnitudes in this table to the shifting around the price notch documented in Table 1, there is apparently less shifting across the time notch in all cases except when the time notch coefficients are statistically significant. Put another way, the main reason the time notch shifting patterns are less statistically significant is because of less shifting, not because of larger standard errors. For example, 5.5 houses shift below the price notch, but only 1.3 houses shift before the time
notch for the 2006 reform. This is a 28 percent increase in house sales in the bunching region of the price notch and only a six percent increase in sales in the bunching region of the time notch.

Table 2: Manipulation Around Time Notch

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<td>[0.77]</td>
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<td></td>
<td></td>
<td>[0.40]</td>
<td></td>
<td>[0.65]</td>
</tr>
<tr>
<td>$\sum_{k=1}^{60} \gamma_{2k} + \sum_{l=1}^{60} \gamma_{3l}$</td>
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<td>6.69**</td>
<td>6.13**</td>
<td>2.41</td>
<td>2.00</td>
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<td>[0.04]</td>
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<td>[0.08]</td>
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<td>-3.27</td>
<td>-3.33</td>
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<td></td>
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<td>-0.75</td>
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<td></td>
<td></td>
<td>[0.84]</td>
<td></td>
<td>[0.98]</td>
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<tr>
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<td>-2.90</td>
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<td>36,435</td>
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<td>53,785</td>
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<td>3 mths after</td>
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p-values from clustered standard errors by price bin are in square brackets. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

To summarize, in this section we find some, but not overwhelming evidence of a response around the time notch. This suggests that sales dates are pinned down by relatively inflexible time preferences, and at the very least, adjusting the sale date seems more difficult than adjusting the sale price in the regions around each of these notches. Moreover, it is likely that in the very local neighborhood of the time notch, it would have been difficult to know whether these houses would sell just before or just after the tax regime changed. So, responses around the time notch are likely capturing, to a large degree, whether, conditional on having
their house on the market, the seller wishes to speed up the sale of their house to avoid the higher tax (and the buyer wishes to hurry up and purchase a house to avoid the higher tax). The evidence in this section suggests that sellers are more likely to adjust the sale price of their house than they are to speed up the sale date to avoid the tax notch, which speaks to the relative cost of these two decisions. Given the relative inflexibility of time preferences we observe here, it is perhaps not surprising that we find no clear evidence of a lock-in effect in the next section, because in practice lock-in generally comes about due to individuals postponing the sale of their house when a match with positive surplus is not available.

7 Estimating the the Lock-in Effect

In this section, we conduct a difference-in-difference analysis to examine changes in sales volume across periods with varying transfer tax rates to assess whether there was a lock-in effect of tax increases on sales. There are two alternative strategies to investigate this issue. The first is to compare the counts of houses sold each month-year in a price range affected by the tax change relative to house sales in a price range not affected by the tax change. This strategy is pursued by Best and Kleven (2015) for houses within £50,000 of the price notch. The second strategy, common in the capital gains lock-in effect literature (e.g., Shan, 2011), estimates the effect of the tax change on the likelihood a house sells. In month-years in which a given house does not sell, the house price is imputed.

The results of the first strategy are detailed in Appendix C. If we examine a price range of data similar to Best and Kleven (2015), we find a lock-in estimate with a very wide 95 percent confidence interval centered near zero. If we expand our analysis to a larger range of the price distribution in an attempt to decrease standard errors, we run into the problem that there are substantial secular changes that affect the treatment and comparison groups differently. This occurs because market prices fluctuated substantially over the course of the periods examined, and the density of houses available to sell is definitely not uniform across
the price distribution; this problem is particularly salient for the 2003 reform.

In light of these issues, this section proceeds with the second identification strategy, which addresses the challenges associated with the first strategy; we can use a wider treatment and comparison group and, if the treatment and comparison groups are chosen properly, we can find compelling counterfactuals across both the price and time notches.

To conduct this analysis, we construct a panel data set in which every house appears every month whether or not it sold; the dependent variable, sell$_{it}$, takes on a value of one if house $i$ sells in month $t$ and zero otherwise. A house is included in the panel only if it sells at least once during our sample. Given this, our regression analysis answers the following question: of the houses that were sold at least once between 1999-2010, how much lower is the likelihood they sell after the reform$^{34}$

The regression is specified as follows:

$$sell_{it} = \gamma_0 + \gamma_1 post_{it} \cdot treat_{it} + \gamma_2 treat_{it} + \gamma_3 X_{it} + \eta_t + u_{it},$$

(16)

where $post_{it}$ is an indicator for whether the month-year of this observation occurs when the tax reform is in place ($post = 1$) or when it is not in place ($post = 0$), $treat_{it}$ is an indicator variable that equals one if the price of the house is in the treated range, $X_{it}$ are covariates, $\eta_t$ are month-year fixed effects$^{35}$ and $u_{it}$ is the error term. This model is estimated using a probit specification. With this method, a house sale is drawn out of the distribution of houses that are valued at a particular price in month-year $t$, eliminating the challenges faced using the method in Appendix E when the number of house sales is not uniform across the nominal price distribution. We will see in Figure 6 below that this is, in fact, a very compelling way to define the treatment and comparison groups.

For month-years in which a house does not sell, we impute the price using the following

---

$^{34}$Alternatively, we could construct a slightly different sample to ask: of the houses that sell at least once before the reform, how much lower is the likelihood they sell again after the tax increase? The results for this specification are similar and available from the authors upon request.

$^{35}$The variable $post_{it}$ is a linear combination of these fixed effects, so it is not included as a separate variable in the regression.
method. 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 price 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 the nominal price for each month-year. We use an analogous method to calculate the price for all homes that sell three or more times.

There will inevitably be some measurement error in \( \text{treat}_{it} \) because prices are imputed in all month-years in which a given house does not sell. However, in contrast with the standard results regarding measurement error in an independent variable, it is unlikely that this measurement error biases our estimates. This is because we never miscategorize houses in month-years in which they sell, so the measurement error only has the potential to adjust the denominator for calculating the selling likelihood in any given month-year and will only do so if the measurement error is systematic. Unless the measurement error is systematic—which we would see in Figure [6] below as a gap between the treatment and comparison groups—and changing over time in a way that is correlated with the tax change, this will not bias our estimates. We do not observe any such systematic bias in Figure [6] below. Put another way, in this particular setting, swapping the prices of one house that does not sell just below the treatment threshold with another house that does not sell just above the treatment threshold does not bias the estimates. The estimates would only be biased if, for example, all the houses just below the treatment threshold were priced correctly, and all houses above the treatment threshold were also priced just below the treatment threshold. Then we would observe too many houses in the comparison group and this would affect the denominator of the likelihood. But we do not see evidence of such systematic bias in Figure [6].

The baseline treatment group for the 2006 reform is $390,000 to $650,000 and the baseline
comparison group is $280,000 to $390,000. Several considerations went into choosing these
treatment and comparison groups. First, we wanted the treatment and comparison groups to
be wide to decrease the size of the confidence interval around our estimate, but not so wide
such that the treatment and comparison groups were no longer comparable in month-years
absent the reform. We show in Figure 6 that the treatment and comparison groups we chose
are well-matched in periods before the tax change took place. Additionally, the comparison
group is constrained from below because two years before the 2006 tax change took place
another price notch still existed at $250,000. By restricting the data to be above this price
notch, the treatment and comparison groups always faced the same tax incentives. Further
we want to restrict it enough above this lower price notch to avoid any shifting to below the
notch, which would bias the comparison group likelihood towards zero. For this purpose,$280,000 seems reasonable. Lastly, we split the treatment and comparison group at $390,000
instead of at the price notch—$400,000—so that we do not capture the movement from the
strictly-dominated region to the region just below the notch that we documented in Section
5. In that section, we also could not reject the null hypothesis that everyone who moved
from above the price notch moved to the region within $10,000 below the price notch. Given
this, we are confident in our choice of the cutoff below the price notch.

We also have a choice to make across time: how many years before and after the tax
changes should be included. As above, more data is better, but we also want the treatment
and comparison groups to be plausible over the whole time period. We choose three years
as our baseline, and present sensitivity analysis to a two-year period in Table 3 below.
We need not be concerned about a response to the time notch in this section because the
responses to the time notch in Section 6 were not large and generally insignificant; ignoring
any meaningful response to the time notch will bias our estimates towards zero.

The baseline treatment and comparison groups were chosen with similar considerations
in mind for the 2003 reform. The baseline treatment group is $150,000 to $240,000 and the
baseline comparison group is $240,000 to $350,000. The treatment group is constrained from
above by the 2006 tax change. In choosing the comparison group, we wanted to include a reasonable range of the price distribution, while keeping the comparison group a good match for the treatment group. We again split the treatment and comparison groups $10,000 below the price notch to avoid shifting from above to below the price notch. We start the analysis three years before the tax change was implemented and end the analysis three years after the tax change was eliminated. We consider two years as an alternative time window in Table 3.

We examine our baseline treatment and comparison groups by presenting some graphical representations of the data in Figure 6. We plot the average of our dependent variable of interest, \( \text{sell}_{it} \) across time, separately for treatment (blue) and comparison (yellow) groups. We fit a fourth-order local polynomial on \( \text{sell}_{it} \) at the month-year level and use a triangle kernel with a bandwidth of eight quarters. The quarters when a price notch is in place are shaded in grey. For the 2006 reform in the left panel, the treatment and comparison groups appear to match each other very closely across time. Before the tax change is implemented this highlights that the treatment and comparison groups are well-matched. Under the assumption that the treatment and comparison group would continue to move together in the absence of a tax reform, our estimates below will be valid. The fact that there is no decline in the treatment group relative to the comparison group after the tax change was implemented, indicates that there is no evidence of a decrease in the likelihood of the treated group to sell during the period in which the higher tax rate was in place. In Appendix D, we conduct a complementary analysis by plotting a three-year time-aggregated version of \( \text{sell}_{it} \) across the price distribution separately for the years before and after the price notch was implemented. This analysis confirms the two main findings from Figure 6: the treatment and comparison groups are well-matched and there is no detectable response to the tax change.

For the 2003 reform, in the right panel, the treatment and comparison groups generally appear reasonable for each other across time, and again there is no evidence of a decrease in the likelihood of the treated group to sell during the period in which the higher tax rate was in place. However, there is a point in 2005 at which the blue line temporarily surges above
Figure 6: Likelihood of House Sale by Time

This figure plots the likelihood a house sells below (yellow line) and above (blue line) the price notch over time. The quarters when the price notch are in place are shaded in grey.

The yellow line when the tax reform is not in place, which may raise some questions about the validity of the comparison group. The analysis of sell$_i$ across the price distribution in Appendix [D] is consistent with these findings.

The probit regression estimates based on equation (16) using the treatment and comparison groups depicted in Figure 6 are given in Table 3, Columns (1) and (2). The table reports mean marginal effects from the probit model and two-way clustered standard errors by house and month-year. The only control variables included in Column (1) are month-year fixed effects; Column (2) adds all house characteristics that we observe in 2005, as described in the data section and listed in Table A.1. We will thus refer to Column (2) as our baseline estimates. The top panel of Table 3 lists estimates for the 2006 reform and the bottom panel lists the same estimates for the 2003 reform. We find that including the covariates does play some role in our estimates; for both the 2006 and 2003 reform, the estimates change by about one standard deviation in the estimate when covariates are included. The estimates for both reforms are not significant, which is not surprising given the figures upon which these estimates are based. The estimate in this column represents a 0.05 percent decrease in
Table 3: Difference-in-Differences Regression Estimates

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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
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<td>2003 Reform</td>
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<td>-</td>
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<td>Frequent</td>
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<td>2,272,005</td>
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<td>0.0140</td>
<td>0.0140</td>
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</table>

This table reports mean marginal effects from a probit specification. Two-way clustered standard errors by house and month-year are in parentheses. The pre-mean row lists the mean of the dependent variable in the periods when the price notch is not in place. Asterisks denote significance at the 1% (***) , 5% (**), and 10% (*) levels.

The likelihood a house sells for the 2006 reform and a 0.628 percent increase in the likelihood a house sells for the 2003 reform.

The 95 percent confidence interval for the 2006 reform is [-5.20%, 5.10%], which is almost three times as narrow as the alternative estimation strategy pursued in Appendix C. Our confidence interval includes the lock-in effect implied by Shan (2011), but does not include the substantially larger estimates implied by Dachis et al. (2012) and Best and Kleven (2015).

Note that the regression in Column (2) is different from that of Column (1) in two ways: covariates are added, and houses are excluded if any binary characteristic of the house is not shared by at least one house in the opposite—either treatment or comparison—group. To understand which is more important, we impose the same restriction on the data set in Column (3), but without including any covariates. For the 2006 reform, this is almost the

---

36To calculate these percent changes, we divide the estimated change in the density reported in Table 3 by the mean value of the density when the price notch is not in place. For example, the calculation for the 2006 reform is: 

\[ \frac{-0.000007}{0.0140} = -0.0005 \] 

This is an 0.05 percent decrease.
entire story—once the houses are excluded to match Column (2), the estimates are almost the same as those found in Column (2). For the 2003 reform, this explains about half the change in estimates from Column (1) to Column (2); the rest is explained by the inclusion of covariates.

**Figure 7: Estimate by Different Treatment or Comparison Group Cutoffs**

The point estimates are given by black dots and the 95 percent confidence interval is given by the black line. There is a vertical black line at zero.

Table 3 Column (4) and Figure 7 provide some sensitivity analysis of our estimates. We have argued why we believe we have made a reasonable baseline choice of the upper bound of our treatment group for the 2006 reform and the lower bound of the comparison group for the 2003 reform. Moreover, we showed that these choices provide well-matched treatment and comparison groups. However, these choices remain somewhat arbitrary. Figure 7 compares a wide range of choices of these cutoffs (using the same regression specification as in Table 3 Column (2)), comparing the point estimates and 95 percent confidence intervals that we get from each. This figure shows that the estimates are robust to the choice of these cutoffs. Table 3 Column (4) includes two years instead of three years before and after the tax reforms. The lock-in estimates remain close to zero and highly insignificant, highlighting that the estimates were not sensitive to our original choices of the price range and time span.
Although we find no evidence of a lock-in response for all sellers, it is conceivable that a significant response exists for sellers who are frequently involved in the housing market. We might think that focusing on people who ever sell more than one house in a given month reasonably captures such sellers. Figure 8 plots the same figure as Figure 6 exclusively for houses last sold by a frequent seller. For the 2006 reform in the left panel, the treatment and comparison groups track each other very closely and there is no evidence of a decrease in house sales for the treated group when the higher tax rate was in place.

For the 2003 reform in the right panel, it appears that other than a surge in houses sold by frequent sellers after the higher tax rate was eliminated (followed by similar surge in the comparison group a couple of months later), there is no evidence of a decrease in house sales for the treated group when the higher tax rate was in place. The corresponding point lock-in estimates for both reforms can be found in Table 3 Column (5). These estimates remain insignificant.
8 Conclusion

This paper estimates the behavioral response to residential real estate transfer taxes by studying notched tax rate changes in Washington D.C., exploiting both a property price and sale date notch to obtain identifying variation. To inform the empirical analysis, we first develop a rigorous model in which both the remittance rule and the real incidence matter for the behavioral response, and in which both the timing of the sale and the sales price may be affected. We then use this model to inform our empirical analysis.

We first provide evidence that there was manipulation of the sales price to the lower-tax-rate region around the price notch as well as widespread awareness of the tax changes and the incentives they created: for the 2006 reform, 68 percent of individuals were aware of the tax change. We cannot reject the null hypothesis that the excess mass below the price notch is equal to the hole in the density of house sales above the price notch, which implies that we cannot reject the null that the buyer and seller have equal bargaining power. We observe only a small—and often insignificant—impact on the timing of house sales in response to the time notch, which suggests buyers and sellers are more willing to make a small adjustment to the sale price than they are willing to make a small adjustment to when the house is sold.

We then use this information to develop difference-in-difference estimates of the lock-in effect of the transfer tax that are consistent in the presence of this house sales price manipulation. We find no significant lock-in effect across both reforms and both notches, even among frequent sellers that one might expect to be particularly aware of these taxes. Taken together, our results suggest that the welfare costs of a state introducing or eliminating a housing transaction tax are small.
References


Pickford, J. (October 9, 2015). Cash buyers account for four in ten london property trans-
actions. *Financial Times*.


Table A.1: Summary Statistics for Houses that Ever Sold

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<th>Variable</th>
<th>Obs.</th>
<th>Mean</th>
<th>Std.Dev.</th>
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<th>Obs.</th>
<th>Mean</th>
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This table provides summary statistics on observable covariates in the year 2005 for all houses that we analyze in this paper that ever sold. If 2005 values were unavailable, we use values from 2010.
A Model Appendix

A.1 Base Model

The equilibrium buyer’s surplus and seller’s surpluses (which are both non-negative for any house that sells by the participation condition) are

\[ B - h \cdot (1 + \alpha t) = \beta \cdot \left( B - S \cdot \frac{(1 + \alpha t)}{(1 - (1 - \alpha) \cdot t)} \right) \]  
(A.17)

\[ h \cdot (1 - (1 - \alpha) \cdot t) - S = (1 - \beta) \cdot \left( B \cdot \frac{(1 - (1 - \alpha) \cdot t)}{(1 + \alpha t)} - S \right). \]  
(A.18)

Increasing the buyer’s bargaining power \( \beta \) transfers surplus from the seller to the buyer, with each dollar in surplus lost by the seller providing \( (1 + \alpha t)/(1 - (1 - \alpha) \cdot t) > 1 \) dollars of surplus gained by the buyer. The effect of an increase in the fraction of the tax remitted by the buyer, \( \alpha \), on the purchase price is:

\[ \frac{\partial h}{\partial \alpha} = -t \cdot \left( \frac{(1 - \beta)}{(1 + \alpha t)^2} B + \frac{\beta}{(1 - (1 - \alpha) \cdot t)^2} S \right) < 0. \]  
(A.19)

An increase in the fraction of the tax remitted by the buyer also changes the buyer’s and seller’s surpluses:

\[ \frac{\partial}{\partial \alpha} \left( B - h \cdot (1 + \alpha t) \right) = \frac{\beta t^2}{(1 - (1 - \alpha) \cdot t)^2} S > 0 \]  
(A.20)

\[ \frac{\partial}{\partial \alpha} \left( h \cdot (1 - (1 - \alpha) \cdot t) - S \right) = \frac{(1 - \beta) \cdot t^2}{(1 + \alpha t)^2} B > 0. \]  
(A.21)

Both surpluses increase as the buyer remits a larger share of the tax. When the tax is remitted by the seller, the purchase price adjusts upward, which increases the amount of tax due and hence reduces overall surplus. The effect of a change in the tax rate on the purchase price is

\[ \frac{\partial h}{\partial t} = \frac{-\alpha \cdot (1 - \beta) \cdot B}{(1 + \alpha t)^2} + \frac{(1 - \alpha) \cdot \beta S}{(1 - (1 - \alpha) \cdot t)^2}, \]  
(A.22)
which is positive for values of $\alpha$ between zero and a threshold value $\alpha^*$ and negative for values of $\alpha$ between the threshold value $\alpha^*$ and one. An increase in the tax rate reduces both the buyer’s and seller’s surpluses

$$\frac{\partial}{\partial t} (B - h \cdot (1 + \alpha t)) = \frac{-\beta S}{(1 - (1 - \alpha) \cdot t)^2} < 0 \quad (A.23)$$

$$\frac{\partial}{\partial t} (h \cdot (1 - (1 - \alpha) \cdot t) - S) = \frac{-\beta B}{(1 + \alpha t)^2} < 0. \quad (A.24)$$

Real incidence and remittance responsibility coincide when

$$\frac{\beta S}{(1 - \beta) \cdot B} = \frac{(1 - (1 - \alpha) \cdot t)^2}{(1 + \alpha t)^2 \cdot (1 - \alpha)}, \quad (A.25)$$

which implies that there is not a single value of $\beta$ such that real incidence and remittance responsibility coincide for all matches, which will have different values of $S$ and $B$.

### A.2 Price Notch

**Proposition A.1.** If a match’s best price given the higher tax rates $h^* = \frac{(1 - \beta) \cdot B}{(1 + \alpha \cdot (t + \Delta t))} + \frac{\beta S}{(1 - (1 - \alpha) \cdot (t + \Delta t))}$ is above the dominated region, that is $h^* > \frac{\bar{h}(1 - (1 - \alpha) t)}{[1 - (1 - \alpha) \cdot (t + \Delta t)]}$, then the weighted welfare that match receives from bunching grows more quickly than the weighted welfare that match receives from a sale at $h^*$ as $\beta$ increases. Formally, $\frac{\partial \ln(W(\bar{h}))}{\partial \beta} > \frac{\partial \ln(W(h^*))}{\partial \beta}$.

**Proof.** We have

$$\frac{\partial \ln(W(\bar{h}))}{\partial \beta} = \ln \left( \frac{B - (1 + \alpha t) \cdot \bar{h}}{(1 - (1 - \alpha) \cdot t) \cdot \bar{h} - S} \right), \quad (A.26)$$

and

$$\frac{\partial \ln(W(h^*))}{\partial \beta} = \ln \left( \frac{\beta(1 + \alpha \cdot (t + \Delta t))}{(1 - \beta) \cdot (1 - (1 - \alpha) \cdot (t + \Delta t))} \right). \quad (A.27)$$
The first order condition for $h^*$ implies
\[
\frac{\partial \ln(W(h^*))}{\partial \beta} = \ln \left( \frac{\beta \cdot (1 + \alpha \cdot (t + \Delta t))}{(1 - \beta) \cdot (1 - (1 - \alpha) \cdot (t + \Delta t))} \right) = \ln \left( \frac{B - h^* \cdot (1 + \alpha \cdot (t + \Delta t))}{h^* \cdot (1 - (1 - \alpha) \cdot (t + \Delta t)) - S} \right). \tag{A.28}
\]

The numerator of the right-hand side of (A.27) is larger than the numerator of the rightmost piece of (A.28) as both the price and tax rate are lower in (A.27). The denominator of the rightmost piece of (A.28) is larger than the denominator of the right-hand side of (A.27) for any value of $h^*$ above the strictly dominated region, that is, for any price at which a transaction might take place above the notch. It follows that the log weighted welfare from bunching increases more quickly as $\beta$ rises than the log weighted welfare from a sale at $h^*$ above the strictly dominated region.

\[\square\]

### A.3 Improvements Extension

The amount of improvements the seller chooses without a notch, $I^*$, is defined by
\[
c'(I^*) = (1 - (1 - \alpha) \cdot t) \frac{\partial h(I^*)}{\partial I} = (1 - (1 - \alpha) \cdot t) \cdot \left( \frac{1 - \beta}{1 + \alpha t} B + \frac{\beta}{1 - (1 - \alpha) \cdot t} S \right). \tag{A.29}
\]

Differentiating (A.29) with respect to the tax rate gives
\[
\frac{\partial I^*}{\partial t} = \frac{-(1 - \beta) \cdot B}{c''(I^*)(1 + \alpha t)^2} < 0, \tag{A.30}
\]

because convexity implies $c''(I^*) > 0$. Increasing the transaction tax rate on the sale leads the seller to make fewer improvements.\(^{37}\) Differentiating the seller’s utility at the utility-maximizing amount of improvements with respect to the tax rate shows that increasing the

\[\text{---}\]

\(^{37}\)Improvements in our context are deductible for the purposes of capital gains taxation, so increases in the capital gains tax rate make improvements more, not less, desirable.
tax rate reduces the seller’s maximized utility:

\[
\frac{\partial U(I^*)}{\partial t} = -(1 - \beta) \cdot B \left( 1 + \frac{1}{c'(1 - (1 - \alpha) \cdot t) \cdot h(0)} \right) < 0.
\]  

(A.31)

A.4 Combined Price and Time Notch Extension

**Proposition A.2.** For bounded \( B \) and \( S \), in the limit as \( k''_b(x) \) and \( k''_s(x) \) tend to \( \infty \) for all \( x \), bunching at the cutoff date \( \bar{T} \) only provides positive joint surplus if \( T_s \) and \( T_b \) are both within a neighborhood of \( \bar{T} \).

**Proof.** For both parties to be willing to transact at time \( \bar{T} \), we must have the joint surplus condition

\[
\frac{B - k_b(|\bar{T} - T_b|)}{(1 + \alpha t)} - \frac{S + k_s(|\bar{T} - T_s|)}{(1 - (1 - \alpha) \cdot t)} > 0.
\]  

(A.32)

We also have \( k_b(|\bar{T} - T_b|) = \int_{0}^{|\bar{T} - T_b|} k''_b(D) dD \), which goes to \( \infty \) in the limit as \( k''_b(x) \) goes to \( \infty \) for all \( x \) unless \( |\bar{T} - T_b|^2 \to 0 \), and similarly for the seller’s time preference. Thus for any match without both \( T_b \) and \( T_s \) arbitrarily close to \( \bar{T} \), as \( k''_s(x) \) and \( k''_b(x) \) tend to infinity for all \( x \), a sale at \( \bar{T} \) eventually provides negative surplus, and so the match does not bunch at \( \bar{T} \). \( \square \)

**Proposition A.3.** Suppose that absent the combined notch a match would take place at price \( h > \bar{h} \) determined by \( [4] \), and that with the combined notch that match would bunch at the cutoff price \( \bar{h} \). Additionally assume that \( (1 - \alpha)t \leq 1 \). Then in the limit as \( k_b(x) \) and \( k_s(x) \) tend to zero for all \( x \), bunching at the cutoff price \( \bar{h} \) provides weighted surplus less than and bounded away from the weighted surplus provided by the match absent the notch.

**Proof.** In the limit as time preferences tend to indifference, each party’s surplus and the weighted surplus become arbitrarily close to the surpluses without time preferences, so that if the proposition is true without time preferences it is true in the limit as time preferences tend to indifferences.
We prove this proposition without time preferences by the fundamental theorem of calculus, which we use to claim that

\[ W(\bar{h}) - W(h) = \int_{\bar{h}}^{h} \frac{\partial W}{\partial h_{\text{obs}}} dh_{\text{obs}} < -b < 0 \]  

(A.33)

where \( b \) is a constant that does not depend on the strength of time preferences. Note that the derivative of the weighted surplus at any price \( h_{\text{obs}} \) with respect to the price is

\[
\frac{\partial W}{\partial h_{\text{obs}}} = \left[ h - h_{\text{obs}} \right] \frac{(1 - (1 - \alpha) \cdot t) \cdot (1 + \alpha t)}{(B - (1 + \alpha t) \cdot h_{\text{obs}})^{1-\beta}(h \cdot (1 - (1 - \alpha) \cdot t) - S)^\beta}. 
\]  

(A.34)

This derivative is non-negative on the interval \( \bar{h} \leq h_{\text{obs}} \leq h \) and strictly positive on the interior of the interval if \((1 - \alpha) \cdot t < 1\). \( B - (1 + \alpha t) \cdot \bar{h} \leq B - (1 + \alpha t) \cdot h_{\text{obs}} \) for \( \bar{h} \leq h_{\text{obs}} \leq h \) and \( B - (1 + \alpha t) \cdot h \geq 0 \) because the buyer must be willing to buy at \( h \) for a sale to take place at that price. Similarly, \( 0 \leq (1 - (1 - \alpha) \cdot t) \cdot \bar{h} - S \leq (1 - (1 - \alpha) \cdot t) \cdot h_{\text{obs}} - S \leq (1 - (1 - \alpha) \cdot t) \cdot h - S \) because the seller must be willing to accept \( \bar{h} \) for the match to bunch at the cutoff price.

For prices \( h_{\text{obs}} \) in the interval \([ (3\bar{h} + h)/4, (\bar{h} + 3h)/4 ] \), which has fixed length greater than zero, the derivative has value bounded from below by

\[
\frac{h + 3\bar{h}}{4} \frac{(1 - (1 - \alpha) \cdot t) \cdot (1 + \alpha t)}{(B - (1 + \alpha t) \cdot (3\bar{h} + h)/4)^{1-\beta}((\bar{h} + 3h)/4(1 - (1 - \alpha) \cdot t) - S)^\beta}. 
\]  

(A.35)

Then since the argument of the integral in (A.33) is always non-negative, and is bounded away from zero on an interval of fixed length, the value of the integral is bounded away from zero. Bunching at the price notch provides weighted surplus that is less than and does not approach the weighted surplus from selling at \( h \) absent the notch.
B Bargaining Power

In this appendix, we use a generalized version of the method proposed in Kopczuk and Munroe (2015) to estimate the buyer’s bargaining power $\beta$. This method approximates our model of proportional taxation developed above with a model of lump sum taxation, in which $\beta$ is also the fraction of the real incidence of a tax borne by the buyer. As above, $\alpha$ is the fraction of remittance responsibility borne by the buyer and $\bar{h}$ is the price cutoff for the notch. The change in the lump sum tax at the notch is $\Delta t \cdot \bar{h}$. Let $F_0(\cdot)$ denote the counterfactual cumulative distribution function of house prices if instead of the notch there were only a single, lower lump sum tax, and $F_0^{-1}(\cdot)$ denote its inverse. Let $I - G$ denote the mass of sales that bunch minus the mass of sales missing from the gap in the sales price distribution above the notch. In Section 5, we estimate both in counts, which can easily be converted to mass for the purposes of this analysis. In counts, $I - G$ is estimated as $\sum_{k=1}^{300} \gamma_{2k} + \sum_{l=1}^{50} \gamma_{3l}$ and $F_0$ is the cumulative predicted mass from equation (14), excluding the manipulation around the price notch ($\sum_{k=1}^{300} \gamma_{2k} + \sum_{l=1}^{50} \gamma_{3l}$). Note that we do not try to net out the lock-in effect above the price notch in our calculation of $F_0$ because we find no evidence of a lock-in effect in our paper.

Then the conditions under which bunching is optimal in the model with lump-sum taxation imply that

$$\beta = \alpha + \frac{\bar{h} - F_0^{-1}(I - G + F_0(\bar{h}))}{\Delta t \cdot \bar{h}}.$$  \hfill (B.36)

Note that if $I - G$ is close to zero, we expect $\beta$ will be close to $\alpha$ because $F_0^{-1}(I - G + F_0(\bar{h})) \approx \bar{h}$. In Section 5, our estimates of $I - G$ are indeed small and not statistically different from zero. Hence, this formula implies that $\beta$ is approximately 0.5. And, indeed, if we calculate equation (B.36), $F_0^{-1}(I - G + F_0(\bar{h})) = 402.9$, which implies a $\beta$ of 0.501.
C Counts of House Sales

In this appendix, we consider capturing the lock-in effect as the change in the number of sales after the reform above the price notch, relative to before the reform and below the price notch, as given by the following regression:

\[ \text{countsell}_{jt} = \gamma_0 + \gamma_1 \text{post}_{jt} \cdot \text{treat}_{jt} + \gamma_2 \text{treat}_{jt} + \gamma_3 \text{post}_{jt} + u_{it}, \]  

(C.37)

where \( \text{countsell}_{jt} \) is the number of houses that sell in month-year \( t \) in $5,000 bin \( j \), \( \text{post}_{jt} \) is an indicator variable for whether the observation is before or after the time notch (date of policy implementation), and \( \text{treat}_{jt} \) is an indicator variable for whether the observation is above or below the notch. The standard errors are two-way clustered on bin and month-year. The treatment group is defined as the window of houses sold for between $390,000 to $450,000 and the comparison group is defined as $350,000 to $390,000 for the 2006 reform. For the 2003 reform, the treatment group is defined as $240,000 to $300,000 and the comparison group is defined as $200,000 to $240,000. For each group, we begin the treatment group $10,000 below the price notch so that we do not capture the movement from the strictly-dominated region to the region just below the notch that we documented in Section 5. Figure A.2 depicts the average of \( \text{countsell}_{jt} \) separately by treatment (blue darker line) and comparison (yellow lighter line) group aggregated at the quarter level over time. The entire comparison group series is adjusted so that the mean level of house sales for both groups are the same pre-reform (any differences in this mean will be captured by \( \gamma_2 \) in Equation (C.37)). For the 2006 reform, the treatment and comparison groups are well-matched and there appears to be no change in the treatment group relative to the comparison group after the price notch was implemented. Hence, there is no visual evidence of a lock-in effect. There is also no evidence of a response to the time-notch. Moreover, the estimated lock-in effect from equation (C.37) is 0.223 (wrong sign) and highly insignificant for the 2006 reform. This point estimate implies that the 2006 reform increased house sales by 2.4 percent and the
95 percent confidence interval is \([-14.7\%, 19.5\%]\). Note that if we followed Best and Kleven (2015) and only clustered on the month-year, our confidence interval would be about half as wide; however, this is not correct and when the standard errors are constructed in this way, the standard errors increase as the bin size increases. The results are similar for the 2003 reform, although it is much less clear that the treatment and comparison groups are well-matched. The estimates (and confidence intervals) are similar for both reforms if we define the dependent variable as \(\log(\text{countsell}_{jt})\) instead.

**Figure A.1: Counts of Houses that Sell by Time**

This figure plots the likelihood a house sells below (yellow line) and above (blue line) the price notch over time within $50,000 of the price notch. The quarters when the price notch are in place are shaded in grey. The entire comparison group series is adjusted so that the mean level of house sales for both groups are the same pre-reform.

To tighten the confidence interval, we could increase the range of prices that we include in our estimation. Figure A.2 considers the same treatment and comparison groups as considered in Section 7 for our main difference-in-differences strategy. Unfortunately, with these wider treatment and comparison groups, it is very difficult to make the claim that the pre-reform yellow (lighter) line represents a valid comparison group for the post-reform blue (darker) line for either reform. The secular changes in the treatment relative to the comparison groups over time are likely driven by the fact that the house sale distribution is not uniform across all nominal prices and there were substantial changes in house prices, as
documented in Figure 2. Therefore, we do not pursue this research design further.

**Figure A.2: Counts of Houses that Sell by Time**

This figure plots the likelihood a house sells below (yellow line) and above (blue line) the price notch over time over a wider range of prices than in Figure A.2. The quarters when the price notch are in place are shaded in grey. The entire comparison group series is adjusted so that the mean level of house sales for both groups are the same pre-reform.

### D Difference-in-Differences Figures Across Prices

In this appendix, we provide another graphical representation of the baseline treatment and comparison groups we use in our analysis in Section 7. We plot a three-year time-aggregated version of our dependent variable of interest, sell, across the price distribution in Figure C.1 for the 2006 reform, separately for treatment (blue) and comparison (yellow) groups. We fit a fourth-order local polynomial on sell$_{it}$ at the month-year level and use a triangle kernel with a bandwidth of 88. The blue line plots the likelihood a house sells (relative to all houses at that price) during the three years after the higher tax rate was implemented for houses sold at a price above $400,000. Thus, we would expect the likelihood a house sells above $400,000 to fall for this group relative to the comparison group (in yellow). The comparison group is the likelihood a house sells in the three years before the reform takes place. We see no detectable decrease in house sales over this range above $400,000, and the comparison
This figure plots the likelihood a house sells in the three years pre- (yellow line) and three years post-reform (blue line). The area above the price notch is shaded in grey.

We do the same for the 2003 reform in Figure C.2. In both panels, the blue line plots the likelihood a house sells (relative to all houses at that price) during the quarters the higher tax rate was in place above $250,000. In the presence of a lock-in effect the likelihood that
a house sells above $250,000 would fall relative to the comparison group (in yellow). The comparison group in the left panel is the likelihood a house sells in the three years before the reform takes place. On the right, the comparison group is the likelihood a house sells in the three years after the higher tax rate is eliminated. There are two key take-aways from the left figure: (i) both comparison groups appear to be plausible and (ii) we see no detectable decrease in house sales over this range above $250,000. In the right figure, the comparison group does not match the treatment group as well and there is slight evidence for increased lock-in above the price notch after the reform, but these differences are not statistically significant.

E   Newspaper Timeline

This list cites all relevant articles in the Washington Post or Washington Times leading up to or after the introduction of the price notch on January 1, 2003 as well as all the articles leading up to or after its elimination on October 1, 2004:

- September 25, 2002, Washington Post: "District and Maryland Swing the Budget Ax; Mayor’s Plan Acceptable to Council Except for Increase in Income Tax"
- September 25, 2002, Washington Times: "Council, Mayor Divided over Tax Increases"
- September 26, 2002, Washington Post: "Mayor, Council Agree on Budget Fixes; Temporary Income Tax Increase, Favored by Williams, Still a Question"
- September 26, 2002, Washington Times: "Council Puts Traffic Cameras in New Budget; Backs Mayor’s Plan in Move Seen as way to Boost Revenue"
- December 30, 2002, Washington Post: "New Year Rings In At Cash Register; Tobacco, Property Deed Taxes Going Up in D.C."
• March 18, 2003, Washington Times: "Budget Targets Parkers, ‘Wealthy’; Spending Plan sees no Increase"

• March 24, 2004, Washington Post: "Williams’s Budget Would Boost Taxes; Proposal Calls for Spending Increase of 9%"


This list cites all relevant articles in the Washington Post or Washington Times leading up to or after the introduction of the price notch on October 1, 2006:

• February 17, 2006, Washington Post: "Reserves, Deed Tax May Plug D.C. Gap; Deficit Lurks Within Big Schools Outlay"

• March 22, 2006, Washington Times: "Mayor Proposes $62.2 million in Tax Increases; Targets Residential Deed Levy"

• March 22, 2006, Washington Post: Williams Submits His Final Budget; Plan Trims Income Tax, Boosts Schools, Libraries

• April 5, 2006, Washington Times: Cropp Calls for Tax Exceptions; Wants Real-Estate Relief Added to Mayor’s Plan


• **May 10, 2006, Washington Post: Council Approves Increases for Police, Housing

• *May 10, 2006, Washington Times: Council Approves $9 billion Budget

*This articles includes the correct location of the notch.

**This article mentions the notch, but says that the first dollar above the notch is actually below the notch.