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Abstract

In April 2006, the real estate listing service in Massachusetts adopted a new policy that prohibits home sellers from resetting their property's "days on market" to zero through relisting. We study the effect of this new policy on single-family home sales along the Massachusetts-Rhode Island border, using homes in Rhode Island, which did not change its policy, as the control group. We find that the policy change leads to a relative sale price reduction of around $11,000 for affected homes in Massachusetts. Homes caught in the middle of the policy change are the hardest hit; the sudden release of the cumulative days on market information lowers the average sale price by $21,500. Sellers respond to the new policy by reducing the listing price to shorten their property's days on market.

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1 Introduction

In April 2006, home sellers in Massachusetts experienced a sudden change in how their property's "days on market" measure was displayed to home buyers. Before the policy change, each time a property was "relisted", its days on market would be reset to zero. Under the new policy, the days on market measure for a house is shown as a cumulative total and is no longer reset by relisting.

In the real estate industry, days on market, or the number of days since the property was first listed, is an important statistic. Too many days on market are usually interpreted as a negative quality signal, as buyers speculate that there are hidden flaws which make the property hard to sell (Taylor, 1999). As a result, the days on market statistic has been a target of rampant manipulation. Sellers regularly pull sluggish listings off the market, make slight adjustments, and then resubmit them. This resets the days on market counter and removes the stigma attached to a slow-selling house, a practice similar to resetting the odometer of a used car (Blanton, 2005).

It is unclear how a new policy that prevents the manipulation of days on market information would affect home sales. On the one hand, this new policy alleviates information asymmetry between home sellers and buyers. If all sellers can claim few days on market, the statistic becomes uninformative (see also Sobel and Crawford (1982); Farrell and Rabin (1996)). By making the true days on market information transparent, the new policy may increase a risk-averse buyer's willingness to pay. On the other hand, if slow sales raise doubt over property value, a higher days on market statistic will reinforce itself, making stagnant homes even harder to sell. By asymmetrically punishing these homes, the new policy may reduce mean sale prices in the market.

We study how home sales react to the new policy. We seek empirical identification from a natural experiment: while Massachusetts switched to the new policy, the neighboring state of Rhode Island did not change its policy which allows days on market to be reset through relisting. We focus on single-family homes in twenty towns that lie on the state border of Massachusetts and Rhode Island. These towns fall within the same primary metropolitan statistical area, and exhibit similar time trends in home sales. We examine the policy impact on sale prices and days on market of Massachusetts properties relative to Rhode Island properties that were on the market at the same time.

We first compare homes listed before the policy change with those listed after the policy change. In addition, to control for the fact that we only observe the sale price for sold homes, we use the Heckman selection model, whereby we separately identify a property's chance of being sold through input mistakes in listings. We find consistent evidence that sale prices of Massachusetts homes decreased by around $11,000 after the policy change and the time on market shortened by 18 days, relative to that of Rhode Island homes.

We further investigate how home buyers reacted to the policy change. There is an "Interim Group" of homes that were listed before the policy change but were still on the market when the new policy was announced. The sellers of these properties set initial listing prices without knowing the policy change, which was not announced in advance, but the buyers were exposed to the new policy. Therefore, the existence of this Interim Group offers an opportunity to isolate buyer reactions to the policy change. We find that the new policy caused Massachusetts home sellers a $21,500 reduction in sale prices and lengthened their homes' days on market by 20 days, relative to similar Rhode Island sellers.

Last, we explore how the policy change affected sellers' decisions to set the initial listing price. The direction of this effect is conceptually ambiguous. On the one hand, sellers may

become less aggressive in pricing. This is because high listing prices are likely to prolong the days on market which, when displayed under the new policy, cast doubt on property value. On the other hand, sellers may use a higher listing price as an excuse for a slow sale, thus "dampening" the negative signal of a long time on market (Taylor, 1999). Comparing homes listed before and after the policy change, we find that Massachusetts home sellers lowered the listing price by around $12,000 under the new policy relative to their Rhode Island counterparts.

The rest of the paper is arranged as follows. Section 2 reviews the related literature. Section 3 introduces the context for the policy change, and Section 4 presents the data, summary statistics, and preliminary findings. Section 5 assesses the policy impact on sale prices and days on market. Section 6 further disentangles the policy impact by exploring the buyer side and seller side reactions to the policy change respectively. Section 7 concludes the paper with discussions for future research.

2 Related Literature

Real estate markets are characterized by information asymmetry. Buyers typically have limited information about the property's value. Meanwhile, each buyer may be privately informed about certain features of the property through, for example, home inspection. If buyers can freely observe the decisions made by other potential buyers in the past, they may use such observations to infer the value of a property. Taylor (1999) constructs a theoretical model and demonstrates that buyers can infer the quality of a house from the amount of time it spends on the market. Sellers in response should either set a low initial price to sell fast, or charge a high initial price as an excuse for slow sale.

The real estate economics literature has found mixed findings on the empirical relationship

between days on market and sale price. For instance, using days on market as one of the independent variables to explain sale price, Miller (1978) finds that sale price increases with days on market. On the other hand, Belkin et al. (1976) consider days on market as the dependent variable and provide evidence that if sale price is higher, then days on market are longer. Meanwhile, Kalra and Chan (1994) suggest a simultaneous relationship between days on market and the sale price whereby the two variables influence each other. This simultaneity problem, coupled with the fact that both days on market and sale prices are affected by unobserved factors such as the quality of a property or the seller's motivation, makes causal inferences even more complicated (see Glower et al. (1998); Levitt and Syverson (2008); and Hendel et al. (2009)). Our paper differs from previous research in that we are able to use an exogenous policy change to identify how the availability of days on market information actually affects home sales.

A seller also faces a trade-off between shortening the time it takes to sell a house and increasing the sale price (Hendel et al. (2009) and Levitt and Syverson (2008)). Anglin et al. (2003) study the impact of the list price set by sellers and link it to the days on market and sale price. By setting a high initial price, it takes longer to sell the property. Conversely, if the initial price is too low, the seller may accelerate sale but at a lower sale price. In this paper, we will investigate the sellers' responses to the policy change, so that we can quantify the impact of information availability on both sellers and buyers that is reflected in combination in the final sale price.

Our study is also related to the literature on observational learning. The pioneering theoretical works of Banerjee (1992) and Bikhchandani et al. (1992) show how decision makers draw quality inferences from observing others' choices, and how such observational learning may lead to uniform mass behavior. However, it is challenging to document observational learning empirically, due to its coexistence with other behavioral mechanisms underlying

correlated social behaviors (Manski, 1993).[1] One exception is Cai et al. (2009), who find through a randomized field experiment that restaurant customers are more likely to order popular courses. Another exception is Zhang (2009), who studies how patients on a waiting list adopt kidneys for transplant, relying on the variation in observational learning across different queue positions as the source of identification. Our study contributes to the observational learning literature by studying the impact of a public policy that exogenously changes the extent of observational learning available to home buyers—by releasing the days on market information, the new policy allows home buyers to observe their predecessors' "no purchase" decisions. We study how such observations affect buyer behaviors, seller reactions, and home sales.

3 Market Context and the Policy Change

To mediate the real estate market, realtors have collectively developed proprietary databases that store information about properties for sale. These databases allow buyer-side agents to identify homes for their clients. Although the public can view abbreviated home listings at web sites such as Realtor.com, only licensed real estate agents with paid memberships in particular databases can fully access the raw data. In most of the United States, seller-side agents are responsible for entering information about the homes they are selling into a database that is maintained by the local Multiple Listing Service (MLS). There are around 900 such MLSs across the US. They are generally self-regulated, and the rules governing home listings vary across MLSs.

A widely used real estate listing variable is "days on market", which tracks the number

[1] There is a broader social learning literature which documents socially correlated choices. For example, Duflo and Saez (2003) find evidence of social effects in retirement plan choices through a randomized experiment. Sorensen (2006) identifies social learning by exploiting the panel nature of the data to control for the common unobservables. See Cai et al. (2009) for a review of this literature.

of days a property has been listed on the market. As slow sale increases buyers' doubts about property value, some home sellers and agents cancel their listing and create a new one immediately for the same property. In this way, sellers reset a property's days on market and restore its fresh appeal, a practice criticized as "resetting the odometer on a used car" (Blanton, 2005). The tactic can frustrate buyers by providing misleading information about a property's value and gain undeserved market exposure at the expense of other properties.

On April 1, 2006, MLS Property Information Network (MLS-PIN), the major MLS for Massachusetts, announced a new policy that modified the way in which the days on market statistic was tracked and totaled in its electronic system. Before the policy change, each time a listing was entered into the system, its days on market would reset regardless of how long the property had been available for sale. After the policy change, days on market is displayed as a cumulative total; when a property is relisted, it would still appear as a new listing, but the calculation of days on market is picked up from its prior listing.[2] All MLS-PIN listings created since 1992 are subject to the new policy. In particular, properties that were canceled, relisted, and were on the market on April 1, 2006 would also have their cumulative days on market revealed, although these sellers were not forewarned about the policy change.

Table 1 shows a sample listing from the MLS-PIN database. The property first went on the market on February 2, 2005. It took 18 months to sell, and was relisted three times during that period. The first relisting maintained the initial list price but reset the days on market. The second relisting displayed a price reduction of $44,900, and the third relisting further lowered the price. Under the old policy, the days on market (until the property went under agreement) would have been displayed as 192 days for the final relisting. However, after the

[2]There were two exceptions to this rule. First, if the time between cancelation and subsequent relisting is more than 90 days, days on market is reset to zero. Second, if the property is put under agreement but the transaction does not close, the days it is off the market are excluded from the days on market calculation.

Table 1: An example of real estate listings from the MLS-PIN.

List Date	Status	Sq. Feet	Beds	Baths	Acre	Days on Market before Policy Change	Days on Market after Policy Change	List Price	Sale Price	Sold Date
2/2/2005	CAN	1456	3	2	0.22	120	120	$379,900		
6/29/2005	CAN	1456	3	2	0.22	90	210	$379,900		
9/30/2005	CAN	1456	3	2	0.22	150	360	$335,000		
3/9/2006	SLD	1456	3	2	0.22	192	552	$309,900	$300,000	10/20/2006

policy change, the true cumulative days on market of 552 days were displayed.

This new policy may bring significant changes, because for many home buyers MLS is the only source that provides accurate information on days on market. By National Association of Realtors (NAR) regulation, access to the full listings data is almost universally restricted to real estate agents.[3] In addition, NAR has developed an Internet Data Display (IDX) policy, which regulates what information from the MLS can be displayed online. In particular, MLS-PIN restricts real estate agents from releasing address information or providing maps that might identify the property. As a result, it is difficult for home buyers to track a property's days on market without the aid of MLS information provided to them by their realtors.

One concern about studying any policy change is that the change may have been provoked by and thus be endogenous to the variables of interest. It would be problematic for our study if MLS-PIN tightened the relisting policy in anticipation of changing market dynamics in Massachusetts. For example, suppose MLS-PIN decided to display cumulative days on market because it believed that demand was on the rise which would accelerate the sale for all properties, so that displaying the cumulative days on market would not hurt the market. This endogenous motivation, if it exists, may lead the researcher to spuriously attribute the rising demand to the policy change. However, background interviews with MLS-PIN realtors and reviews of the news coverage suggest that MLS-PIN instituted the policy change mainly

[3]The extent of this control was recently challenged by the U.S. Justice Department (see antitrust case *United States v. National Association of Realtors* (2008)). NAR settled in 2008 and now allows internet brokerages the same access to real estate listings as traditional brokerages.

to avoid lawsuits by home buyers who may find relisting practices unethical.[4]

4 Data

4.1 Data Source

We obtained listings data for single-family homes on the market between January 2005 and June 2007 from two MLSs, one of which is the Multiple Listing Service Property Information Network (MLS-PIN) which serves Massachusetts, and the other is the State-Wide Rhode Island Multiple Listing Service which serves the neighboring state Rhode Island. During the period analyzed, the Rhode Island MLS maintained the old policy which allowed a property's days on market to be reset through relisting. Therefore, single-family homes in Rhode Island naturally serve as the control group to assess the treatment effect of the new policy in Massachusetts.

The data span 20 towns located on either side of the Massachusetts-Rhode Island border. The names of these towns alongside their key demographic variables from the year 2000 census are listed in Table 2. Figure 1 displays their locations. While most of these towns are similar in terms of household size and income (variables which are likely to affect the housing market), there are a few towns such as Fall River in Massachusetts where median income is much lower than average. These cross-town differences will be captured by town fixed

[4]MLS-PIN is not the only MLS to have tightened up relisting regulations, although it is one of the first outside of California. In May 2004, the iTech MLS service which serves West San Gabriel Valley changed its relisting policy. In 2006, RE InfoLink, which operates in Santa Cruz, Santa Clara and San Mateo Counties in Northern California, similarly announced a more stringent policy. Northwest MLS, a regional MLS in Washington state, on September 1, 2006 issued a notice to members that canceling and relisting would only be permitted when there has been a substantial change in the quality or condition of the property. Also in September 2006, the Silicon Valley MLS introduced a "continuous days on market" field which measures the listing time across all relistings. (Source: "Crackdown on Relisting Homes," *Altos Research Real Estate Insights,* September 21, 2006.) In most of these cases, a fear of being sued was mentioned as the primary motivation for the policy change.

Table 2: Demographics of towns included in this study.

Town	County	State	Population	% Households with children under 18	Average household size	Median household income
Attleboro	Bristol	MA	42,068	33.4	2.57	$50,807
Blackstone	Worcester	MA	8,804	38.2	2.71	$55,163
Douglas	Worcester	MA	7,045	43.1	2.85	$60,529
Fall River	Bristol	MA	91,938	29.9	2.32	$29,014
North Attleboro	Bristol	MA	27,143	36.0	2.60	$59,371
Plainville	Norfolk	MA	7,683	33.4	2.53	$57,155
Seekonk	Bristol	MA	13,425	35.7	2.77	$56,364
Somerset	Bristol	MA	18,234	28.1	2.57	$51,770
Swansea	Bristol	MA	15,901	31.1	2.67	$52,524
Uxbridge	Worcester	MA	11,156	29.2	2.79	$61,855
Westport	Bristol	MA	14,183	29.0	2.62	$55,436
Bristol	Bristol	RI	22,469	28.2	2.45	$62,575
Burrillville	Providence	RI	15,796	36.6	2.75	$52,587
Cumberland	Providence	RI	31,840	33.2	2.59	$54,656
East Providence	Providence	RI	48,688	27.1	2.33	$39,108
North Smithfield	Providence	RI	10,618	22.4	2.61	$58,602
Pawtucket	Providence	RI	72,958	30.5	2.41	$31,775
Tiverton	Newport	RI	15,260	29.6	2.51	$49,977
Warren	Bristol	RI	11,360	27.4	2.36	$41,285
Woonsocket	Providence	RI	43,224	31.2	2.37	$30,819

Source: Year 2000 Census.

effects in subsequent analysis. Most of these towns fall in the official Census Metropolitan Statistical Area (MSA) of Providence-New Bedford-Fall River, and were further selected due to their immediate proximity to the state border.[5] The advantage of choosing towns in the a similar MSA is that any economic shock to the stabiltiy of the region would impact properties on both sides of the state border. These towns have similar housing inventories of single-family homes which are composed almost entirely of older housing stock, the median age of houses being around 40 years. Also, none of these towns changed their property taxes significantly in the period we study. Therefore, housing market dynamics in these towns would have been likely to share the same time trend if there had been no policy intervention. Our empirical identification relies on this common trend assumption, such that we can use Rhode Island properties as the control group to study the treatment effect of the policy change on Massachusetts properties.

4.2 Summary Statistics

For each property listed we have data on its attributes, including address, square footage, acreage, and the number of bedrooms and bathrooms. We also have data on each property's initial listing price, listing date, days on market,[6] whether it sold, and sale date and sale price if it sold.

Table 3 displays the summary statistics for each state. The average house in our dataset has 3 bedrooms, 2 bathrooms, 1800 square feet, and a a lot-size of 0.55 acres (roughly 25,000 square feet). Homes in Massachusetts on average are slightly larger, have more bedrooms and bathrooms, are higher priced and stay on the market longer.

[5]One exception is North Attleboro, which does not fall in the official borders of the Providence MSA but is included in the study. Its neighboring town Attleboro, 4.6 miles away, does fall in the Providence MSA.

[6]Days on market is calculated as the length of time for which a property is an active listing in the MLS records. Days on market may be right-censored for homes listed near the end of the observation window. Later analysis will take this into account.

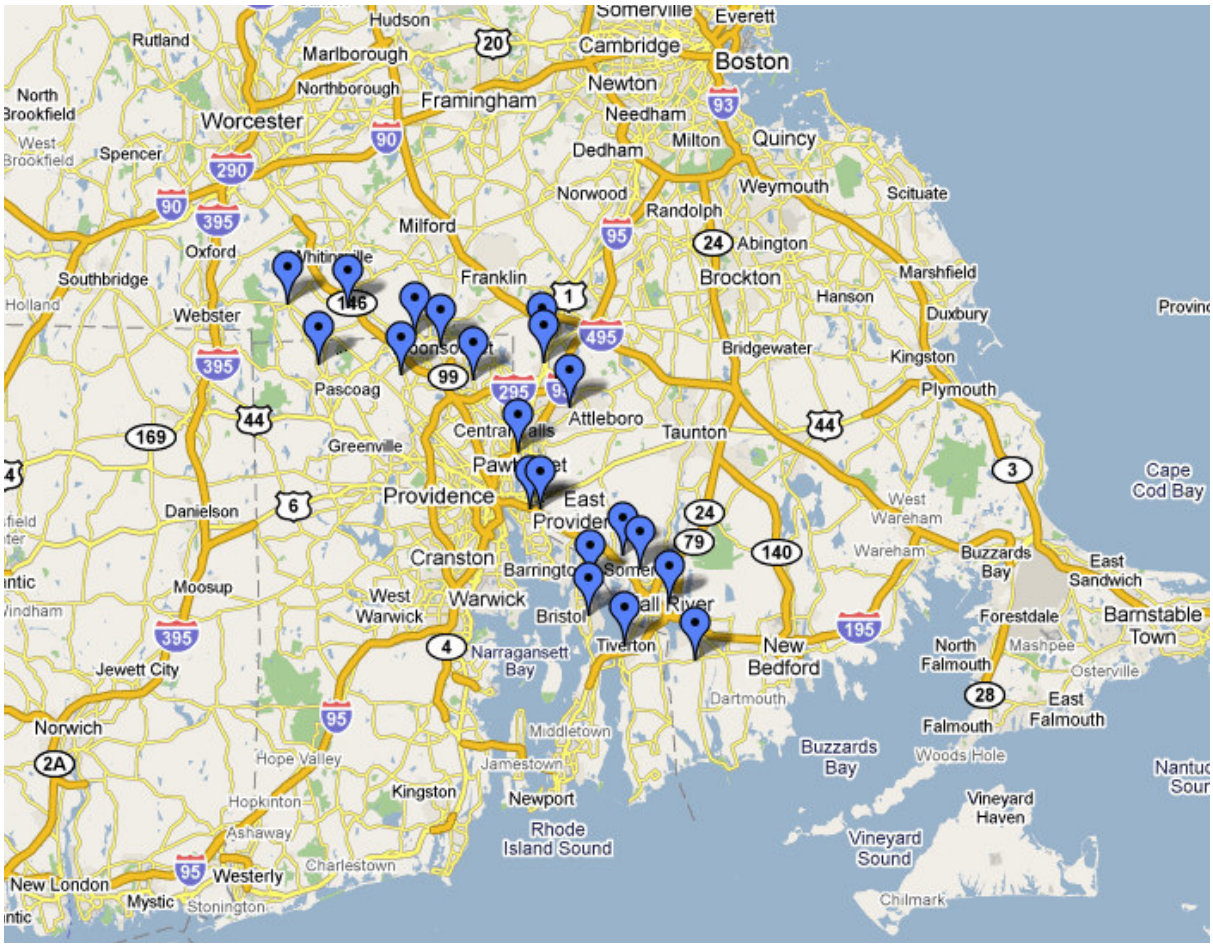

Figure 1: Location of towns included in this study. These towns are clustered along the Massachusetts-Rhode Island border.

Table 3: Summary statistics.

Variable	Mean	(Std. Dev.)	N
Massachusetts			
Days on Market	204.02	(193.398)	4560
Initial List Price	376.919	(157.315)	4560
Sale Price	342.348	(130.215)	2528
Whether Sold	0.554	(0.497)	4560
Bedrooms	3.214	(0.726)	4560
Bathrooms	1.81	(0.704)	4560
Square Footage	1.805	(0.759)	4560
Acreage	0.681	(0.935)	4560
Rhode Island			
Days on Market	182.158	(174.63)	4676
Initial List Price	327.835	(156.701)	4676
Sale Price	295.365	(121.847)	3117
Whether Sold	0.667	(0.471)	4676
Bedrooms	3.098	(0.695)	4676
Bathrooms	1.665	(0.664)	4676
Square Footage	1.736	(0.695)	4676
Acreage	0.414	(0.794)	4676

4.3 Listing Cancelations

Among all properties in the dataset, 78 percent were listed only once, and 22 percent posted multiple listings. Altogether, there are 15,863 listings for the 9,236 houses in our data, 8,485 for Massachusetts and 7,378 for Rhode Island.

A home might be relisted for several reasons. For example, if a house is put under a purchase agreement but the buyer withdraws from the sale, then the house could be relisted to reenter the market. Also, if a seller changes realtor, she could submit a new listing which is managed by the new realtor. Another possibility is foreclosure, although no properties were marked as having been foreclosed in the period we study. Finally, a seller or her realtor may relist a property to reset its time on the market. One indication of such manipulation is that the seller side voluntarily cancels a listing (Blanton, 2005).

To see whether the policy change has actually deterred home sellers from manipulating days

on market by relisting, we examine whether there is a change in listing cancelation rates. Figure 2 compares the average number of cancelations before and after the policy change in the two states.

Rhode Island exhibits an increase in listings cancelations of around 2 percent, while Massachusetts shows a decrease of around 6 percent. The difference in the changes between the two states is significant at the 1 percent level. We take the drop in listing cancelations as a first evidence that the policy did affect the housing market in Massachusetts.

Figure 2: Proportion of listings canceled in MA and RI before and after the policy change.

4.4 Trend in Sale Prices

Our identification strategy requires real estate markets in the two states to exhibit the same time trend had there been no policy change. According to Table 3, the modal number of

bedrooms in our data is 3. Figure 3 displays the average sale prices for 3-bedroom houses which represent 58.2% of houses in our data in the two states over time. There is no clear discrepancy in the sales trend between MA and RI before the policy change. Statistically, we regress sale prices of all homes in our data on a set of monthly dummies and a set of interactive terms between monthly dummies and the MA state dummy. A t-test of the joint significance of these interactive terms fails to reject the null hypothesis that sale price trends are the same across states prior to the experiment ($p = .66$).

Noticeably, there is a decrease in sale prices in MA relative to RI after the change in policy. It is interesting to note that this decrease in sale prices was worse in MA soon after the policy change, but that prices recovered fairly quickly, if not entirely to their previous level. In Section 6 we present some evidence suggesting that this initial decline came from houses caught in the middle of the policy change. In other words, houses that were listed before the policy change but were forced by the new policy to display the truthful days on market seem to be the hardest hit. In the next section we explore the effects of the policy change in detail.

5 Effects of the Policy Change

The decrease in listing cancelations in Massachusetts suggests that the policy change achieved its primary goal, which was to deter home sellers and realtors from relisting old properties as new. For the remainder of this paper, we study the impact of the policy change on real estate market outcomes. To test the impact of the policy change, we first compare the market outcomes of properties listed before the policy change with those listed after the policy change.

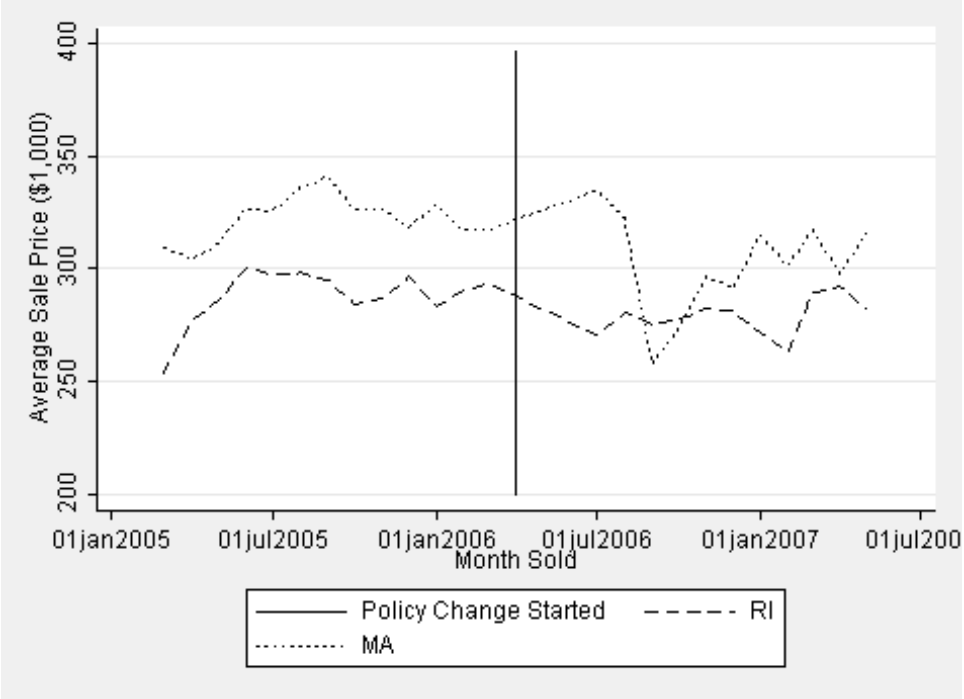

Figure 3: Trend of sale prices in MA and RI before and after the policy change (3-bedroom homes).

5.1 Policy Impact on Sale Prices

We start by investigating the policy impact on properties' sale prices by comparing the properties listed before and after the policy change. We estimate the following differences-in-differences specification:

$$SalePrice_i = \alpha \cdot ListedAfterPolicy_i * MA_i + X_i\beta + Town_i + Month_i + \epsilon_i \qquad (1)$$

$ListedAfterPolicy_i$ is an indicator variable which equals 1 if property i is listed after the policy change and 0 otherwise. MA_i is an indicator variable which equals 1 if the property is located in Massachusetts and 0 if it is in Rhode Island. The interaction term $ListedAfterPolicy_i * MA_i$ reflects whether the property is fully (from listing to sale, if

it sells) subject to the policy change. The associated coefficient α captures the level effect of the policy change on sale prices, provided that Rhode Island and Massachusetts have the same time trend in sale prices absent the policy change.

X_i contains property attributes such as square footage, acreage, and dummies for the number of bedrooms and bathrooms. β is the associated coefficient. The vector $Town_i$ contains town fixed effects that capture variations in school and neighborhood quality across towns. The main effect MA_i cannot be separately identified from the town fixed effects. To control for variation in home prices over time, we include a vector of monthly dummies $Month_i$ for the month in which that the property was first listed.[7] The $ListedAfterPolicy_i$ dummy cannot be separately estimated from the listing month dummies either. We cluster the error term ϵ_i by the street the property is situated on to control for potential serial correlation across time.

The results of this specification when estimated by OLS are reported in the first column of Table 4. The policy change causes around a $11,000 reduction in sale prices of affected Massachusetts homes relative to Rhode Island properties. The estimates for the house characteristic variables in X_i are as expected. Sale prices increase with both square footage and acreage, although there is a diminishing benefit from having large lot sizes. The (unreported) bathroom and bedroom dummies suggest that sale prices increase strongly in the number of bathrooms, but decline weakly and linearly in the number of bedrooms given the same square footage and acreage of a property.

One potential issue with the OLS specification is that it does not capture the different impact the policy change may have on a $0.5 million property versus a $2.5 million property (the highest priced property in our data). To address this issue, we run an alternative specification

[7] We have also run our regressions including fixed effects for the month the property was sold, with similar results.

Table 4: Effect of the policy change on sale prices and days on market.

	(1) Sale Price ($1,000) OLS	(2) Sale Price Log	(3) Sale Price ($1,000) Heckman	(4) Days on Market
Main Equation				
Listed After Policy*MA	-10.80**	-0.0235*	-10.75**	-18.35***
	(4.847)	(0.0121)	(4.701)	(5.585)
Square Footage	86.57***	0.257***	86.61***	22.18***
	(17.29)	(0.0217)	(4.608)	(7.070)
Square Ft Squared	-2.028	-0.0138***	-2.028***	-0.452
	(4.235)	(0.00442)	(0.774)	(1.088)
Acreage	49.96***	0.135***	49.96***	3.779
	(4.788)	(0.0105)	(3.495)	(6.024)
Acreage Squared	-5.231***	-0.0140***	-5.230***	0.709
	(0.816)	(0.00183)	(0.580)	(0.989)
Bedroom Dummies	Yes	Yes	Yes	Yes
Bathroom Dummies	Yes	Yes	Yes	Yes
Town Fixed Effects	Yes	Yes	Yes	Yes
Listing Month Fixed Effects	Yes	Yes	Yes	Yes
Selection Equation				
Listed After Policy*MA			-0.0975	
			(0.0600)	
Error in listing			-0.223	
			(0.254)	
Non-Conforming Address			-0.270***	
			(0.0585)	
Square Footage			-0.146**	
			(0.0626)	
Square Ft Squared			0.00161	
			(0.00959)	
Acreage			-0.0224	
			(0.0444)	
Acreage Squared			-0.00481	
			(0.00701)	
λ			-0.4754	
			(3.8627)	
Bedroom Dummies	Yes	Yes	Yes	Yes
Bathroom Dummies	Yes	Yes	Yes	Yes
Town Fixed Effects	Yes	Yes	Yes	Yes
Listing Month Fixed Effects	Yes	Yes	Yes	Yes
Observations	5645	5645	9236	9236
R-Squared	0.658	0.659	0.658	0.240

Standard errors in parentheses.
Robust standard errors clustered by street.
Column (3): R-Square is for the main equation. All observations used for the selection equation.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

where the dependent variable is the log of the sale price. The estimation results are reported in the second column of Table 4. On average, Massachusetts properties experience a 2.35 percent decrease in sale prices after the policy change relative to Rhode Island homes.

5.2 Adjusting for Selection Bias

The above estimates are censored in the sense that we only observe sale prices for houses that actually sold. More than 44 percent of houses in Massachusetts and 33 percent in Rhode Island did not sell. Such censoring could bias our estimates if the policy affects the chance of sale.

We employ the Heckman two-stage method to correct for the potential selection bias problem (Heckman, 1979). We first specify a probit "selection equation" that reflects the probability of a house being sold:

$$\text{Prob}(Sold_i|Z_i) = \text{Prob}(Z_i\gamma \geq u_i) = \Phi(Z_i\gamma) \tag{2}$$

where the variable $Sold_i$ equals 1 if house i sells, and 0 otherwise. Z_i is a vector of property characteristics variables that determine its probability of being sold, and γ is the vector of parameters to be estimated. u_i is an error term following the standard normal distribution, and Φ is the standard normal cumulative distribution function.

We use parameter estimates from the first stage probit regression $\hat{\gamma}$ to construct a correction factor $S(Z_i\hat{\gamma}) = \phi(Z_i\hat{\gamma})/\Phi(Z_i\hat{\gamma})$, where ϕ is the standard normal probability distribution function. We then correct for sample selection bias by including the correction factor as an additional explanatory variable in the main equation of the sale price:

$$E[SalePrice_i|Sold_i] = \alpha \cdot ListedAfterPolicy_i * MA_i + X_i\beta + Town_i + Month_i + \lambda S(Z_i\hat{\gamma}) \tag{3}$$

where λ is the coefficient associated with the correction factor to be estimated.

To apply the Heckman selection method, we need to find a plausible set of Z_i variables for the selection equation, where there is at least one variable that can be excluded from the sale price equation. It is difficult to identify a property listing characteristic that influences the likelihood of sale without also influencing the sale price. We meet this challenge using the mismatch between actual home specs and those provided in the MLS listing. For example, one single-family house in our dataset is listed as having three bedrooms but zero bathrooms, while tax assessment data shows that it has two bathrooms. Altogether, 0.5 percent of properties in our sample are listed as having zero bedrooms or bathrooms, or report a square footage less than 100. Cross-references with tax assessment data confirm that these are input mistakes rather than highly unusual property features. Another type of mistake is misspelling of the property address. For example, Griffin Street is misspelled as Griffen Street for one house. In our sample, 6 percent of houses have addresses not recognized by the USPS computerized address database.

These mistakes make the property harder to find. Typically, realtors would conduct a computerized search screened by pre-determined parameters such as a minimum number of bathrooms or bedrooms. If a listing contains errors, the property may not appear on a search. Similarly, many realtors prepare for their clients internet maps of an area's houses for sale. If the address is incorrectly entered into the database, the house may not show up on the map. Since the National Association of Realtors requires the address data for a property listing to be kept confidential, there is no easy feedback mechanism for such address mistakes to be corrected.

We construct two dummy variables "Error in Listing" and "Non-Conforming Address" to capture whether a listing contains information inconsistent with the tax assessment data,

and whether it supplies an address unrecognized by the USPS address database. These two variables would affect the likelihood of the buyer finding the house and therefore the probability of the house selling. However, they are less likely to affect the sale price since they do not reflect the true characteristics of the property.[8] In addition to these two "excluded variables" (meaning that they are excluded from the main price equation), we also include the right-hand side variables of Equation (1) in the vector Z_i, since all these factors that affect sale prices may also affect the probability of sale.

The third column of Table 4 displays the estimation results using the Heckman correction method. The coefficients for the main equation are close to those obtained without adding the Heckman correction term. The λ coefficient is insignificant, suggesting that sale prices are not subject to significant selection bias. The estimates for the two excluded variables are as expected, although it seems that misspelled addresses have a more negative effect on the probability of sale than errors in listings. We speculate that this is because a buyer would be more likely to catch a mistake that a realtor makes on the number of rooms than a misspelled address, since the address information is proprietary to the MLS and hence less exposed to scrutiny.

5.3 Policy Impact on Days on Market

In addition to sale prices, we also explore how the new policy affects the average time a property spends on the market. We estimate a specification identical to Equation (1), but using cumulative days on market as the dependent variable instead. The days on market measure is right-censored for unsold homes. However, such censoring is captured by the month times and is further "differenced out" between Massachusetts and Rhode Island.

[8] If we suppose that the potential buyers submit offers sequentially, and the seller accepts the first offer that meets his reserve price, the sale price would not be affected by the number of buyers showing interest in the house directly.

Column (4) of Table 4 displays the results. It appears that the policy change shortens Massachusetts homes' days on market by about 18 days. Multiple explanations exist for this observation. It could be that the release of the cumulative days on market information has sped up home sales by increasing buyers' willingness to pay. It could also imply the opposite, that home owners try to sell fast precisely because longer time on market weakens demand. We will further disentangle the policy effects by examining buyer and seller reactions separately in Section 6.

5.4 Heterogeneity of the Policy Impact

If slow sales raise doubt over property value, we expect that the effect of policy change would be worse for houses that took longer to sell. We stratify our regressions in Table 4 by how long each house took to sell. We summarize the estimation results by OLS and Heckman selection models for houses sold within 120 days, 180 days and 240 days respectively. For the OLS regression, we include houses listed and sold prior to the policy change as control and use houses listed and sold after the policy change to identify the treatment effect. The first column of Table 5 reports results for sale prices of houses that took less than 120 days to sell. For these quick-selling houses, the policy change in Massachusetts has little effect. Column (2) of Table 5 reports the results for a Heckman specification, where selection is defined as whether the house is sold within 120 days. In the selection model, we exclude houses that were listed within 120 days before the policy change, and houses that were listed within 120 days before the end of our sample period, so that all transactions can be observed if the houses were sold within 120 days after the initial listing. Similarly to the results for the OLS regression, there is little effect on quick-selling houses. Columns (3) and (5) present the results of the OLS specification for houses that sold within 180 days and 240 days. It is noticeable that the negative effect of the policy change on sale prices is more severe for slow-

selling homes (houses sold within 240 days) than for those that sold within either 180 or 120 days. This suggests that, as expected, the policy change disproportionately affects houses that take longer to sell. Columns (4) and (6) present results from the two-step Heckman estimation. The results again suggest that the decrease in sale prices is the largest for homes that sold within 240 days and the smallest for homes sold within 120 days.

6 Buyer and Seller Reactions to the Policy Change

We have shown that the inability to reset days on market to zero in Massachusetts leads to lower sale prices and shorter days on market. These effects may be driven by buyer reactions to the policy change, or seller reactions, or a combination of both. For example, lower sale prices under the new policy could reflect buyers' pessimism in valuing a property after finding out about its long history on the market; they could also reflect sellers' conservatism in pricing a property knowing that they can no longer reset its days on market through relisting. In this section we disentangle the policy effects by investigating the buyer side and seller side reactions respectively.

6.1 Buyer Reactions to the Policy Change: Before vs. Interim Group

To isolate the policy impact for buyers, we need to ensure that sellers are not aware of the policy when they list their house. This is challenging in many markets where buyers and sellers respond simultaneously to market changes. However, we benefit here from the existence of an "Interim Group" of properties listed before the policy change but still on the market when the new policy was announced.

Figure 4 shows how properties in the sample can be classified into three groups based on their exposure to the new policy. The "Before Group" consists of 4,118 properties that were

Table 5: Heterogeneity of the policy impact on sale prices.

	Homes sold within 120 days		Homes sold within 180 days		Homes sold within 240 days	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Heckman	OLS	Heckman	OLS	Heckman
Main Equation	Listed After Policy*MA	-1.623	-1.566	-11.50*	-7.868	-16.83**
Listed After Policy*MA	-1.623	-1.566	-11.50*	-7.868	-16.83**	-14.08*
	(7.291)	(7.303)	(6.751)	(7.087)	(7.705)	(8.013)
Square Footage	96.04***	92.95***	91.37***	98.46***	97.86***	99.52***
	(9.897)	(13.82)	(14.03)	(14.28)	(13.25)	(13.23)
Square Ft Squared	-4.855**	-4.627**	-3.446	-3.930	-4.229	-4.342
	(2.193)	(2.295)	(3.378)	(3.376)	(3.068)	(3.059)
Acreage	45.25***	43.69***	44.91***	49.42***	48.81***	50.99***
	(7.004)	(8.366)	(5.987)	(6.261)	(6.763)	(6.800)
Acreage Squared	-4.968***	-4.871***	-4.885***	-5.173***	-5.291***	-5.390***
	(1.455)	(1.484)	(1.132)	(1.127)	(1.208)	(1.200)
λ		69.75		-188.3*		-101.2
		(188.0)		(111.0)		(102.2)
Bedroom Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Bathroom Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Town Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Listing Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Selection Equation	Listed After Policy*MA		-0.0237		-0.0976	
Listed After Policy*MA		-0.0237		-0.0976		-0.142*
		(0.0678)		(0.0700)		(0.0788)
Error in listing		-0.759***		0.137		0.190
		(0.280)		(0.296)		(0.339)
Non-Conforming Address		-0.138**		-0.172***		-0.275***
		(0.0653)		(0.0665)		(0.0727)
Square Footage		-0.252***		-0.201***		-0.0871
		(0.0641)		(0.0680)		(0.0730)
Square Ft Squared		0.0194**		0.0135		0.00512
		(0.00949)		(0.0105)		(0.0109)
Acreage		-0.120**		-0.124**		-0.124**
		(0.0478)		(0.0481)		(0.0524)
Acreage Squared		0.00712		0.00784		0.00558
		(0.00757)		(0.00737)		(0.00791)
Bedroom Dummies	No	Yes	No	Yes	No	Yes
Bathroom Dummies	No	Yes	No	Yes	No	Yes
Town Fixed Effects	No	Yes	No	Yes	No	Yes
Listing Month Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2948	8598	3516	7916	3245	6939
R-Squared	0.670	0.670	0.646	0.647	0.644	0.644

Standard errors in parentheses.
Robust standard errors clustered by street.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

sold or expired before the policy change. The "Interim Group" contains 2,096 properties that were initially listed before the policy change but were not sold or expired prior to the new policy being announced. The "After Group" includes 3,022 properties that were initially listed after the policy change.

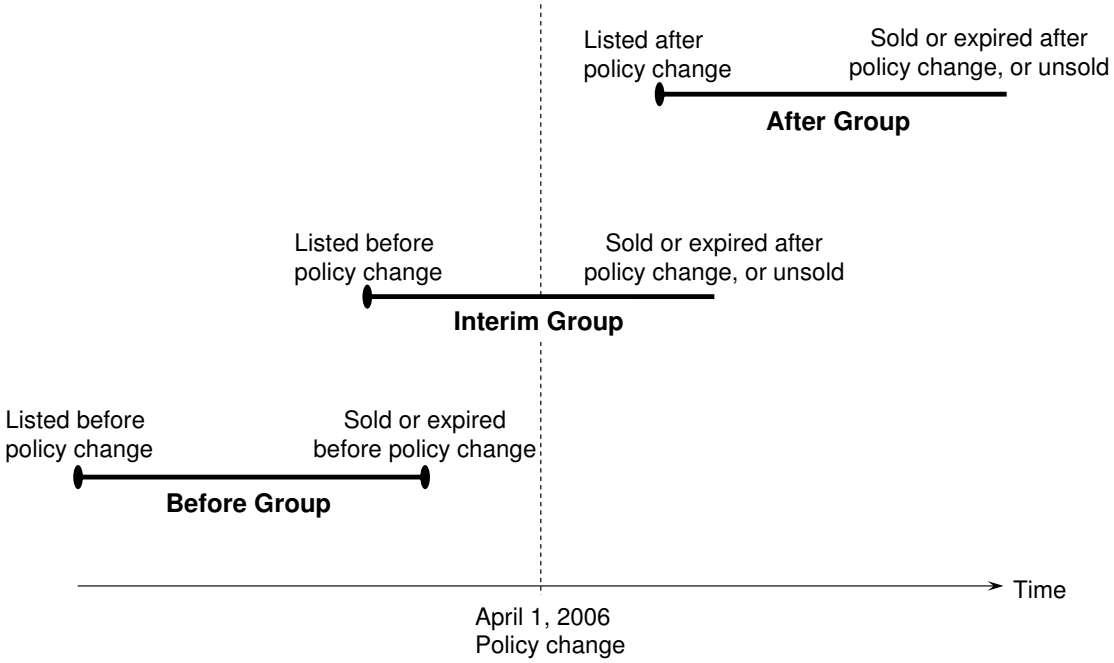

Figure 4: Three groups of properties differently exposed to the policy change.

For the Interim Group of properties, buyers can access and react to the cumulative days on market information, while sellers, not forewarned about the change in policy, have set initial listing prices without anticipating such buyer reactions. In contrast, properties in the Before Group have no exposure to the new policy. By comparing sale prices between the Before Group and Interim Group and across the two states, we can measure the policy impact on sale prices driven by buyer reactions. Note that it is possible that among properties listed at the same time the worse ones take longer to sell and thus are still on the market at the time of the policy change. These properties may subsequently sell at relatively lower

prices. However, this sampling difference is controlled for by comparing the *relative* sale price change between Massachusetts and Rhode Island. Specifically, we again estimate a differences-in-differences equation:

$$SalePrice_i = \alpha \cdot UnsoldBeforePolicy_i * MA_i + X_i\beta + Town_i + Month_i + \epsilon_i \qquad (4)$$

where $UnsoldBeforePolicy_i$ equals 1 if property i is still on the market yet unsold at the time of the policy change and 0 otherwise. When the sample consists of the Before Group and the Interim Group, $UnsoldBeforePolicy_i$ indicates whether home i belongs in the Interim Group. Unlike in Equation (1), $UnsoldBeforePolicy_i$ can be separately estimated from the listing month dummies. Similar to the previous section, we also estimate a specification with log $SalePrices_i$ as the dependent variable, correct for any selection bias using the Heckman method, and estimate the policy impact on cumulative days on market.[9]

We use an ordered Probit model to correct for the selection bias. We have three ordered categories: the property is sold before the policy change; the property is unsold at the time of the policy change but sold after the policy change; and the property is not sold until the end of our sample period. Through this ordered Probit model, we not only address the differences between the properties sold and unsold, but also the differences among the properties sold in different time periods.

Table 6 displays the results. The policy change shows a larger impact on sale prices than indicated by Table 4. When the seller side set initial prices unaware of the policy change, properties on average sold for around $21,500 less under the new policy. This result corresponds to the sale price drop following the policy change as shown in Figure 3. This finding is consistent with the herding conjecture that buyers infer low property value when they find

[9]Note that $UnsoldBeforePolicy_i$ and $UnsoldBeforePolicy_i * MA_i$ should be excluded from the selection equation of the Heckman model.

out that the property has stayed on the market for a long time (Taylor, 1999). In comparison, Table 4 shows the overall policy impact on sale prices; its smaller magnitude suggests that sellers *after* the policy change may have taken measures to counterbalance the negative impact of cumulative days on market. We investigate this possibility in Section 6.2.

In contrast to Table 4 which shows a decline in days on market for the After Group, the comparison of the Before and Interim Groups suggests that the sudden release of cumulative days on market further lengthens Massachusetts homes' days on market by 20 days relative to Rhode Island homes. This result is consistent with the "bandwagon effect" predicted by observational learning theories, whereby buyers' doubt over home value makes stagnant homes even harder to sell (Banerjee (1992); Bikhchandani et al. (1992)). The shortened days on market for the After Group therefore suggests that sellers, after the policy change, have taken actions to accelerate sale and avoid the detrimental bandwagon effects.

Table 6 can also be seen as a robustness check of the results in Section 5. This is because if the new policy affects sale prices, properties that were caught in the middle of the policy change would have received the most impact. If results in Section 5 were driven by worsening long run sale prices for Massachusetts homes, then these same properties should be less affected, since they are closer in time to the policy change than the After Group. In this sense, Table 6 further confirms the existence of the policy impact.

6.2 Seller Reactions to the Policy Change

We now investigate whether sellers adjust their listing strategies in response to the new policy. In particular, we study how initial listing prices differ before and after the policy change. According to Taylor (1999), there are two opposing incentives for the seller. On the one hand, the seller may want to post a low initial price in order to sell early and reduce

Table 6: Buyer reactions to the policy change (Before Group vs. Interim Group).

	(1) Sale Price ($1,000) OLS	(2) Sale Price Log	(3) Sale Price ($1,000) Heckman	(4) Days on Market
Main Equation				
Unsold before Policy*MA	-21.86***	-0.0671*	-21.37**	19.97***
	(8.344)	(0.0379)	(8.629)	(6.941)
Square Footage	97.61***	0.269***	97.35***	1.547
	(16.26)	(0.0219)	(16.17)	(4.977)
Square Ft Squared	-3.384	-0.0152***	-3.388	0.305
	(3.908)	(0.00412)	(3.907)	(0.552)
Acreage	47.20***	0.129***	47.01***	10.76**
	(4.922)	(0.0110)	(4.758)	(4.727)
Acreage Squared	-4.599***	-0.0126***	-4.590***	-0.798
	(0.806)	(0.00182)	(0.797)	(0.792)
Unsold before Policy	-1.588	-0.0142	-1.780	402.1***
	(6.443)	(0.0191)	(6.414)	(5.659)
Bedroom Dummies	Yes	Yes	Yes	Yes
Bathroom Dummies	Yes	Yes	Yes	Yes
Town Fixed Effects	Yes	Yes	Yes	Yes
Listing Month Fixed Effects	Yes	Yes	Yes	Yes
Selection Equation				
Error in Listing			-0.302	
			(0.266)	
Non-Conforming Address			-0.357 ***	
			(0.0669)	
Square Footage			-0.055	
			(0.0643)	
Square Ft Squared			-0.00629	
			(0.00935)	
Acreage			-0.0691	
			(0.0472)	
Acreage Squared			0.00413	
			(0.00732)	
λ			6.659	
			(38.02)	
Bedroom Dummies	Yes	Yes	Yes	Yes
Bathroom Dummies	Yes	Yes	Yes	Yes
Town Fixed Effects	Yes	Yes	Yes	Yes
Listing Month Fixed Effects	Yes	Yes	Yes	Yes
Observations	4120	4120	6214	6214
R-Squared	0.675	0.653	0.675	0.756

Standard errors in parentheses.
Robust standard errors clustered by street.
Column (3): R-Square is for the main equation. All observations used for the selection equation.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

any negative quality inferences buyers may draw from a long days on market figure. On the other hand, a high listing price may also help the seller by "dampening" the negative signal of slow sale, whereby buyers attribute a long days on market figure to the high listing price instead of low property quality.

To investigate seller reactions to the new policy, we estimate the following equation:

$$ListPrice_i = \alpha \cdot ListedAfterPolicy_i * MA_i + X_i\beta + Town_i + Month_i + \epsilon_i \tag{5}$$

where the variable $ListedAfterPolicy_i$ is defined in the same way as in Equation (1): it equals 1 if the property is listed after the policy change and 0 if listed before. Since all listings are associated with a listing price, there is no need to use the Heckman method to correct for selection bias.

Column (1) of Table 7 displays the results for the entire sample. That is, it compares the listing price between the Before and Interim Groups combined and the After Group. It turns out that Massachusetts home sellers on average cut the initial listing price by around $12,000 after the policy change. This less aggressive pricing strategy seems to have achieved its purpose; it shortens the average days on market by 18 days (Table 4) despite the buyer bandwagon effects (Table 6).

As a robustness check, we estimate the same regression model but exclude houses listed within 180 days before the policy change, because sellers closer to the policy change might have heard whispers that such a change was about to occur. Column (2) of Table 7 report the estimates, which are close to column (1). This result confirms claims by the MLS that the change in policy was not pre-announced.

As a further robustness check, we want to see whether listing prices differ between the Before

Table 7: Seller reactions to the policy change: comparing initial listing prices ($1,000).

	(1) Entire Sample	(2) 180 Days before Policy Removed	(3) Before Group vs. Interim Group
Listed after Policy * MA	-12.13***	-12.40***	
	(4.042)	(4.113)	
Unsold before Policy * MA			-4.817
			(5.918)
Square Footage	100.8***	100.6***	109.0***
	(14.48)	(14.74)	(15.00)
Square Ft Squared	-2.459	-2.468	-3.670
	(3.299)	(3.332)	(3.275)
Acreage	58.54***	59.40***	58.34***
	(5.278)	(5.439)	(6.753)
Acreage Squared	-4.222***	-4.347***	-3.999***
	(0.898)	(0.909)	(1.129)
Unsold before Policy			24.92***
			(4.664)
Bedroom Dummies	Yes	Yes	Yes
Bathroom Dummies	Yes	Yes	Yes
Town Fixed Effects	Yes	Yes	Yes
Listing Month Fixed Effects	Yes	Yes	Yes
Observations	9236	8818	6214
R-Squared	0.642	0.640	0.643

Dependent variable: initial listing prices ($1,000).
Standard errors in parentheses.
Robust Standard Errors clustered by street.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Group and the Interim Group. To do so, we estimate the following equation:

$$ListPrice_i = \alpha \cdot UnsoldBeforePolicy_i * MA_i + X_i\beta + Town_i + Month_i + \epsilon_i \qquad (6)$$

where $UnsoldBeforePolicy_i$ equals 1 if property i belongs in the Interim Group and 0 if it is in the Before Group. We expect no systematic difference in listing prices between the two groups and across states, as sellers did not anticipate the policy change. Column (3) of Table 7 confirms our expectation, where the interactive term $UnsoldBeforePolicy_i * MA_i$ is insignificant, although the average listing prices are higher for the Interim Group.

In summary, this section takes a closer look at the policy effects by investigating buyer and seller reactions separately. Massachusetts home sellers caught unprepared in the middle of the policy change suffer a 20-day delay in sale and a $21,500 loss in average sale prices. Sellers subsequently adapt to the new policy by lowering initial listing prices, which speeds up sale compared to before the policy change, and limits the drop in sale prices to $11,000.

7 Concluding Remarks

It is a common practice for home sellers to reset a property's "days on market" counter through withdrawing the property and relisting it. We study the effect of a policy that prevented Massachusetts home sellers from manipulating days on market by relisting. The new policy can affect home sales in both directions. On the one hand, making the days on market information transparent alleviates home buyers' uncertainty about property value. On the other hand, it may further hinder the sale of stagnant properties and bring average sale prices down.

We investigate the policy impact by comparing single-family home sales in Massachusetts to sales in neighboring Rhode Island, which maintained the old policy. We find that home

sale prices in Massachusetts went down by around $11,000 after the policy change relative to Rhode Island, but that the average days on market shortened by 18 days. Massachusetts homes caught in the middle of the policy change are the hardest hit: the sudden release of days on market information caused unprepared Massachusetts home sellers a $21,500 reduction in sale prices and lengthened their homes' days on market by 20 days. Sellers reacted to the new policy by lowering the listing price to speed up sale, a strategy that limits home buyers' ability to question the value of a slow-moving home.

The findings suggest that a property's days on market is an important statistic for both home buyers and home sellers. Buyers infer property value from days on market, and sellers manage days on market as a strategic variable. Whether publicizing the days on market information improves home sales is therefore a delicate empirical question. Given the slowdown of today's housing markets, it may seem that publicizing properties' long time on the market would further hinder home sales. However, the opposite can be true if home sellers react by lowering the listing price. Public policy makers should therefore take into account possible responses from both home buyers and home sellers.

We expect the impact of policies that publicize days on market information to be moderated by market characteristics, such as differences in home quality, heterogeneity in home buyers' tastes, and the strength of the housing market in general (which moderates the quality implications of days on market). Future research can investigate these factors. Another possible extension is to examine the welfare implications of making days on market transparent. It would be also interesting to investigate the impact of such policies on the agency relationship between home owners and realtors.

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