Days on Market and Home Sales

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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 properties' "days on market" through relisting. We study the effect of this new policy on home sales along the Massachusetts-Rhode Island border, using homes in Rhode Island, which did not change its relisting policy, as the control group. We find that Massachusetts homes that were on the market at the time of the policy change suffered an average reduction of $16,000 in sale price relative to their Rhode Island counterparts. Homes that were revealed to be slow-moving suffered a greater reduction, but fresher listings only had a small increase in sale price. One reason is that some buyers were unaware of sellers' manipulation of days on market and were thus unable to recognize home listings that were authentically fresh. A direct homeowner survey confirmed that buyer awareness was indeed lacking. Sellers reacted to the new policy by lowering their initial listing price to sell fast. However, in towns where listing price history was more transparent, sellers set a higher listing price to dampen the negative signal of slow sales.

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1 Introduction

Home buyers rarely have complete information about the quality of a property in the market. However, each buyer may receive some private information. For example, a buyer can visit the property in person and find out whether it has a convenient floor plan, or hire a home inspector to check whether the basement has been flooded. On the basis of this private information, the buyer may decide against purchasing the home. Therefore, if a home has spent too many days on the market, this is usually interpreted as a negative quality signal, as buyers speculate that there are maybe flaws which made the property hard to sell to previous buyers (Banerjee, 1992; Bikhchandani, Hirshleifer, and Welch, 1992; Taylor, 1999).

As a result, the days on market statistic in real estate listings has been a target of rampant manipulation. Sellers regularly pull sluggish listings off the market, and relist them immediately, resetting their days on market to zero. This practice has been criticized as "resetting the odometer on a used car" (Blanton, 2005). To deter the manipulation of days on market, in April 2006, the real estate listing service in Massachusetts changed its policy governing home listings. Under the new policy, the days on market measure for a house was forced to be an accurate cumulative total which carried over from previous listings.

The new policy could bring significant changes to the real estate market. Before the policy reform, slow-moving homes were able to "pool" with newly listed homes, thus diluting the information content of the days on market statistic (Sobel and Crawford, 1982; Farrell and Rabin, 1996). After the policy change, the release of the true days on market figure would allow buyers to draw better inferences of home quality. Buyers' willingness to pay for slow-moving homes should decrease. However, buyers' willingness to pay for freshly listed homes could be more subtle. In particular, if a buyer was unaware of sellers' tendency to manipulate days on market, she would be unable to distinguish between freshly listed homes under the new policy and relisted, fresh-looking homes under the old policy. As a result, the policy

change might not boost the buyer's willingness to pay for homes that displayed a short time on market.

The policy reform could further affect home sellers' listing strategies. No longer able to conceal their homes' days on market, sellers would want to act to avoid the negative consequence of slow sales. There are two ways to do so: lower the listing price to sell fast, or increase the listing price in the hope that buyers would attribute the slow sale to the high listing price rather than home quality. Which effect dominates is an empirical question. However, the latter effect is relevant only if buyers can observe the history of listing prices; otherwise, buyers could simply dismiss claims of high listing prices as untrustworthy cheap talk (Taylor, 1999).

We study the policy effects on home sales using a natural experiment. While Massachusetts switched to the new policy, the neighboring state of Rhode Island continued allowing days on market to be reset through relisting, and thus serves as the control group. We collect home sales data from twenty towns on the border of these two states. These towns fall within the same primary metropolitan statistical area, exhibit similar time trends in home sales, and were identified by realtors as being comparable in home buyers' perception.

A unique feature of the data is that there is an "Interim Group" of homes that were on the market when the new policy was announced. Sellers of these homes set initial listing prices without anticipating the new policy. However, the sudden policy change discretely shifted the information value of displayed days on market. This feature allows us to identify the causal effect of true days on market information on home sale prices. As hypothesized, Interim Group homes in Massachusetts which displayed above-average days on market at the time of the policy change suffered a $32,000 decline in sale price relative to similar Rhode Island homes. However, the effect was asymmetric; home listings that were truly fresh on the market enjoyed a smaller and less significant increase in sale price.

We investigate the behavioral mechanisms behind buyer reactions. A direct mail survey

of homeowners in our data suggests that a significant portion of buyers were indeed unaware of sellers' tendency to manipulate days on market. We further ask whether buyers were inferring home quality from days on market or merely gravitating towards recent, salient listings. We find evidence of the quality inference effect—displaying longer days on market mainly hurt homes in towns with older housing stock and higher flood risks, and towns where the real estate market was more liquid. That is, buyers were able to make savvy use of the information contained in days on market, but some were unaware of the improved informativeness of days on market brought by the new policy. As a result, the policy punished homes that were revealed to be slow-moving, but failed to sufficiently reward home listings that were authentically fresh to the market. The net effect was an overall $16,000 decrease in sale price for Massachusetts homes caught in the middle of the policy change.

To study the policy effect on sellers' listing strategies, we compare homes that sold or expired before the policy change (the "Before Group") and homes that were listed after the policy change (the "After Group"). Moreover, we obtain data on each town's observability of listing price history, as measured by the presence of realtor websites with searchable listing price history and the existence of reputable brokers. Our findings support Taylor's predictions. Massachusetts homes listed after the policy change on average cut listing prices by $16,000 compared with their Rhode Island counterparts. However, in towns where homes' price histories were readily available, sellers turned out to charge a higher listing price. These seller strategies seemed effective: Massachusetts homes in the After Group only had an insignificant decrease in sale price compared to the Before Group.

The relationship between days on market and sale prices has attracted significant attention in the real estate literature, but the empirical findings are mixed. For instance, Miller (1978) uses days on market as an independent variable to explain sale price, and finds a positive effect. Belkin et al. (1976) argues that days on market is largely unrelated to pricing conditional on housing market segment. Kalra and Chan (1994) suggest a simultaneous

relationship between days on market and sale prices. This simultaneity problem, coupled with the fact that both days on market and sale prices are affected by factors such as home quality, listing strategy, or seller motivation, makes causal inferences even more complicated (Yavas and Yang, 1995; Glower, Haurin, and Hendershott, 1998; Rutherford, Springer, and Yavas, 2005; Levitt and Syverson, 2008; Hendel, Nevo, and Ortalo-Magne, 2009). Our paper contributes to this literature by using an exogenous policy change to identify how the availability of true days on market information affects home sales.

This study also contributes to the information economics literature on "herding," or imitative behaviors among decision-makers (Banerjee, 1992; Bikhchandani, Hirshleifer, and Welch, 1992).[1] Taylor (1999), in particular, develops a theory of herding among home buyers: the decision to not purchase a home appears contagious among buyers because slow sale signifies inferior home quality. To our best knowledge, our study is the first empirical evaluation of Taylor's theory. The findings support Taylor's predictions: buyers were able to infer home value from days on market; sellers seemed to understand the implications of days on market to buyers and took active measure to manage its consequences.

Our research also has important implications for policy makers. The new policy in Massachusetts was intended to facilitate reliable transmittal of information in the housing market. Home buyers' willingness to pay should increase with the amount of information (Milgrom and Weber, 1982). However, we find a decrease of home sale prices following the new policy, although the effect diminishes in the long term as sellers adjusted their listing strategies. This was because home buyers might not be aware of sellers' manipulation of days on market prior to the policy change, and thus failed to appreciate home listings that were truly fresh on the market. These findings emphasize the need not only for policies that

[1]The herding effect has been documented in a number of markets. For example, Chevalier and Ellison (1999) find that career-concerned mutual fund managers herd into popular sectors; Cai et al. (2009) show through a field experiment that restaurant customers tend to order popular dishes; Zhang (2010) finds that kidney transplant candidates are less likely to accept an organ declined by other patients. There is also a broad literature on social learning (Duflo and Saez, 2003; Sorensen, 2006). See Cai et al. (2009) for a review.

improve the amount of information available to the public, but also for concerted efforts to ensure that the public learns that information availability has increased.

2 Market Context and Data

To mediate the real estate market, realtors have collectively developed proprietary databases that store information about properties for sale. In most of the United States, seller-side agents enter information about the homes they are selling into a database that is maintained by the local Multiple Listing Service (MLS).[2] There are around 900 such MLSes across the US. They are generally self-regulated, and the rules governing home listings vary across MLSes.[3]

A widely used real estate listing variable is "days on market," which tracks the number of days a property has been listed on the market. Some home sellers are known to cancel their listing and create a new one immediately for the same property. In this way, these sellers are able to reset a home listing's days on market and restore its fresh appeal, a practice criticized as "resetting the odometer on a used car" (Blanton, 2005).

2.1 Policy Change

On April 1, 2006, MLS Property Information Network (MLS-PIN), the major electronic real estate database for Massachusetts, announced a new policy that modified the way in which the days on market statistic was calculated in its 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, when a property is relisted, it would still appear as a new listing, but its days on market would be displayed as a cumulative total. There were two exceptions to this rule. First, if the time between

[2]We do not distinguish the decisions made by home sellers versus their realtors. See Rutherford, Springer, and Yavas (2005) and Levitt and Syverson (2008) for studies of agency issues among realtors, and Jia and Pathak (2011) for a study of entry dynamics among real estate agents.

[3]See Hendel, Nevo, and Ortalo-Magne (2009) for a study of MLS versus For-Sale-By-Owner platforms.

cancelation and subsequent relisting was more than 90 days, days on market is reset to zero. Second, if the property was put under agreement but the transaction does not close, the days when it was off the market are excluded from the days on market calculation.

All MLS-PIN listings created since 1992 were subject to the new policy. In particular, properties that were canceled, relisted, and were on the market before April 1, 2006 had their cumulative days on market revealed, although these sellers were not forewarned about the policy change. Table 1 shows such a home listing from the MLS-PIN database. The property first went on the market on February 2, 2005. It was relisted three times until it sold on October 20, 2006. Under the old policy, the days on market would have been reset to zero at the time of each relisting. However, after the policy change, the true cumulative numbers were displayed.

Table 1: An Example of Real Estate Listings from the MLS-PIN

List Date	Status	Sq. Feet	Beds	Baths	Acre	DOM before Policy Change	DOM after Policy Change	Listing Price	Sale Price	Sold Date
2/2/2005	CAN	1456	3	2	0.22	147	147	$379,900		
6/29/2005	CAN	1456	3	2	0.22	93	240	$379,900		
9/30/2005	CAN	1456	3	2	0.22	160	400	$335,000		
3/9/2006	SLD	1456	3	2	0.22	225	625	$309,900	$300,000	10/20/2006

Notes: In the Status column, CAN means canceled, and SLD means sold. The Days on Market (DOM) columns are updated each day by definition. The numbers displayed in the table reflect the days on market when the listing was canceled or sold. For example, "147 days" reflects the duration between February 2, 2005 and June 29, 2005.

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.[4] In addition, NAR has developed an Internet Data Display 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

[4]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.

on market without the aid of MLS information.

One concern about studying any policy effect is that the policy change may have been provoked by certain market conditions. It would complicate our study if MLS-PIN tightened its policy in anticipation of changing demand. However, background interviews with MLS-PIN realtors and reviews of earlier news coverage suggest that MLS-PIN instituted the policy change mainly to avoid lawsuits by Boston-based home buyers.[5] We also verified with the news aggregating service Factiva that there were no significant variations in Massachusetts or Rhode Island real estate regulations during the period studied.

2.2 Data

We obtained listings data for residential properties on the market between January 2005 and June 2007 from two MLSes: the Multiple Listing Service Property Information Network (MLS-PIN) which serves Massachusetts, and the State-Wide Rhode Island Multiple Listing Service which serves the neighboring state of Rhode Island.[6] 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, homes in Rhode Island 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. Sample selection follows two criteria, in consultation with a local real estate agency (whose identity is removed for privacy concerns). First, included towns should fall within the same

[5]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 the West San Gabriel Valley changed its relisting policy. In 2006, RE InfoLink, which operates in Santa Cruz, Santa Clara, and San Mateo 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.

[6]Earlier versions of this paper focused on single-family homes. However, the definition of single-family and multi-family homes is not always consistent across the border. Therefore, we now expand the sample to include all residential properties.

official census Metropolitan Statistical Area (MSA) and must attract a similar set of buyers. Housing market dynamics in these towns would have likely shared the same time trend had there been no policy intervention. Our ability to use Rhode Island properties as the control group relies on this common-trend assumption.

Second, included towns must fall outside the central orbit of the Massachusetts MLS (with the center being Boston); they must also feature an home inventory different from typical homes in Boston. This is because the policy change was believed to target new realtors that gravitated towards the hot market of condos in Boston, who were perceived as being less scrupulous than established realtors when it came to churning listings (Blanton, 2005). By choosing towns sufficiently far away from Boston, we aim to minimize the chance that the policy change be endogenous to the characteristics of the housing market we study.

Table A1 in the Appendix presents the names of the selected towns and key demographic variables from the year 2000 census. Figure A1 displays town locations.[7] The median age of homes is around 40 years. While most of the towns are similar in terms of household size and income—variables that are likely to affect home sales—there are a few towns such as Fall River in Massachusetts where median income is noticeably lower than average. Property tax rates are relatively similar across the two states (1.17% in MA and 1.35% in RI). None of the towns changed property taxes significantly in the period we study. Average education spending per student is slightly lower in Rhode Island ($11,731) than in Massachusetts ($13,356). We will capture these differences with town fixed effects in subsequent analysis.

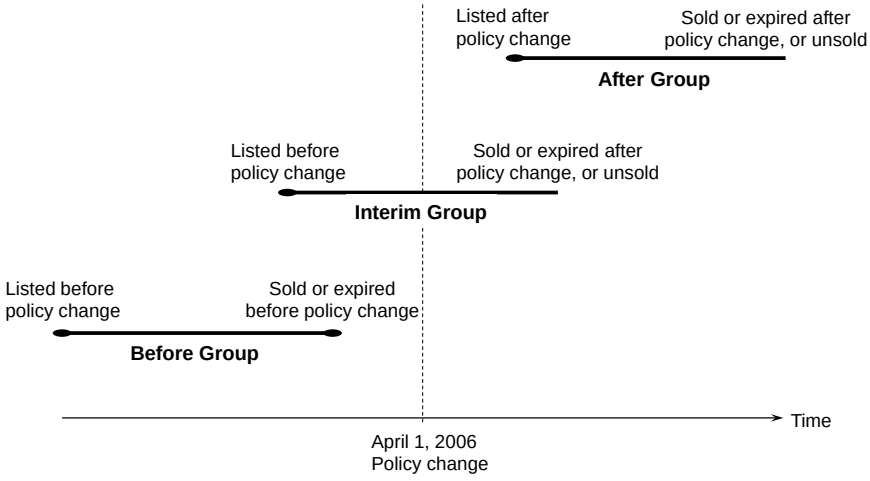
2.3 Three Groups of Homes

Our empirical identification benefits from the existence of three home groups in our sample based on their exposure to the new policy. Figure 1 summarizes the classification. Each group includes homes in both states. The Before Group consists of 5,271 home that sold or

[7]These towns all belong in the Providence-New Bedford-Fall River MSA with the exception of North Attleboro, which is included because it is similar to other towns in the sample and because its "sister town" Attleboro, 4.6 miles away, does fall in the focal MSA.

expired before the policy change. Both buyer and seller behaviors in this group were driven by the old policy. The Interim Group contains 5,718 properties that were initially listed before the policy change but were still on the market when the new policy was announced. Massachusetts buyers in this group could access and react to the cumulative days on market information, but Massachusetts sellers, not forewarned about the change in policy, had set initial listing prices without anticipating buyer reactions. Finally, the After Group includes 5,540 homes initially listed after the policy change. Massachusetts buyers and sellers in this group were both exposed to the new policy.

Figure 1: Three Groups of Homes Differently Exposed to the Policy Change



3 Descriptive Statistics

In this section, we present the descriptive statistics of the data and a set of "model-free" empirical evidence of the policy impact. Table 2 reports the summary statistics by state and by the three home groups. The average house in our sample has 1,948 square feet of space, a lot size of 0.617 acres (roughly 27,000 square feet), 3 bedrooms, and 2 bathrooms. Homes in Massachusetts on average are slightly larger, and have more bedrooms and bathrooms. We will control for these home attributes in subsequent analysis. Homes in the Interim Group

are larger, possibly due to seasonality of the housing market. Reassuringly, however, this pattern appears consistent across the two states.

Homes in Massachusetts were listed at lower prices except in the After Group. For both states, the Interim Group had the highest listing price, again potentially due to seasonality. The incidence of price discounts also peaked in the Interim Group for both states. After the policy change, Massachusetts homes seemed less likely to sell; those in the Interim Group took longer to reach final sale while those in the After Group took fewer days.[8] Relative to the baseline level in the Before Group, Massachusetts homes sold at lower prices in the Interim Group, although the comparison in the After Group is less clear.

Before analyzing these observations, we first want to assess the magnitude of the policy treatment in terms of displayed days on market. Table 3 reports this policy impact. Among Massachusetts homes in the Interim Group, 35% were affected; they suddenly displayed a longer days on market statistic than they had previously. This difference is 86 days on average and in one case is as large as 1294 days. In Rhode Island, 37% of homes would have displayed a different days on market if the policy had been implemented. This percentage is close to its MA counterpart, suggesting that there were no marked differences in seller manipulation of days on market which might have prompted the policy change in Massachusetts.

Another question is whether the policy change has indeed deterred home sellers from manipulating days on market by relisting. We examine whether there was a change in listing cancelation rates. Rhode Island had an increase in listings cancelations of around 2%, while Massachusetts saw a decrease of around 6%. The difference in the changes between the two states is significant at the $p = 0.01$ level. We take the drop in listing cancelations as a first indicator that the policy did affect the housing market in Massachusetts.

Our identification strategy requires that real estate markets in Massachusetts and Rhode Island exhibit the same time trend had there been no policy change. To statistically verify

[8]Note that the *Whether Sold* dummy is truncated for After Group homes.

Table 2: Summary Statistics by State and by Home Group

	MA-Before	MA-Interim	MA-After	RI-Before	RI-Interim	RI-After
Home Attribute Variables						
Square Footage (1,000)	1.88	2.30	2.03	1.70	1.88	1.77
	(1.01)	(3.57)	(1.11)	(0.70)	(0.88)	(0.80)
Acreage	0.59	0.76	0.75	0.45	0.62	0.44
	(1.88)	(2.11)	(2.32)	(2.44)	(3.42)	(1.33)
Bedrooms	3.69	4.14	3.88	3.04	3.10	3.08
	(2.10)	(3.55)	(2.68)	(0.79)	(0.83)	(0.85)
Bathrooms	1.90	2.16	2.03	1.66	1.84	1.74
	(0.83)	(1.90)	(0.85)	(0.67)	(0.76)	(0.72)
Home Transaction Variables						
Initial Listing Price	344.02	389.82	363.44	374.45	412.79	353.28
	(182.74)	(198.65)	(235.17)	(204.41)	(343.57)	(307.64)
# Price Discounts	1.53	1.61	1.17	1.33	1.63	1.20
	(2.23)	(1.65)	(0.58)	(0.84)	(1.28)	(0.61)
Whether Sold (Dummy)	1.00	0.31	0.26 †	1.00	0.45	0.55 †
	(0.00)	(0.46)	(0.44)	(0.00)	(0.50)	(0.50)
Actual Days on Market until Sale	121.30	513.15	152.38	113.55	456.04	185.23
	(74.15)	(221.29)	(111.82)	(71.34)	(229.16)	(108.92)
Sale Price	324.40	337.30	311.83	317.48	348.01	305.75
	(121.74)	(142.15)	(148.49)	(194.88)	(257.28)	(184.68)
Interaction Variables (Town-Specific)						
Year Built	1957.79	1957.45	1957.49	1947.82	1950.36	1948.46
	(18.99)	(19.77)	(19.25)	(16.91)	(16.90)	(17.13)
Higher Flood Risk (Dummy)	0.46	0.44	0.44	0.66	0.67	0.64
	(0.50)	(0.50)	(0.50)	(0.47)	(0.47)	(0.48)
Searchable Price History (Dummy)	0.28	0.28	0.30	0.48	0.45	0.47
	(0.45)	(0.45)	(0.46)	(0.50)	(0.50)	(0.50)
Reputable Broker (Dummy)	0.23	0.24	0.25	0.51	0.51	0.52
	(0.42)	(0.43)	(0.43)	(0.50)	(0.50)	(0.50)
Observations	2625	3179	3349	2646	2539	2191

Notes: for each variable, we report its mean value and standard deviation (in parentheses) for each home group in either state. The Before Group consists of home that were sold or expired before the policy change. The Interim Group contains homes that were initially listed before the policy change but were still on the market when the new policy was announced. The After Group includes homes initially listed after the policy change. † The Whether Sold dummy in the After Group is truncated.

the common-time-trend assumption for the towns studied, 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 policy change ($p = 0.62$). As another evaluation of the common-time-trend assumption, Figure A2 in the Appendix plots the Office of Federal Housing Enterprise Oversight (OFHEO) House Price Indexes for the two states.[9] Both states experienced a similar general

[9]Based on the modified weighted-repeat sales methodology developed by Case and Shiller (1989), the

Table 3: How the New Policy Affected Displayed Days on Market (MA-Interim Group)

	Mean	Std Dev	Min	Max
Displayed Days on Market at Policy Change	125.02	113.87	0	862
Actual Days on Market at Policy Change	211.48	164.56	0	1413
Discrepancy Indicator	0.35	0.48	0	1
Change in Displayed Days on Market at Policy Change	86.45	150.52	0	1294

decrease in home prices during the period we study (January 2005 to June 2007).

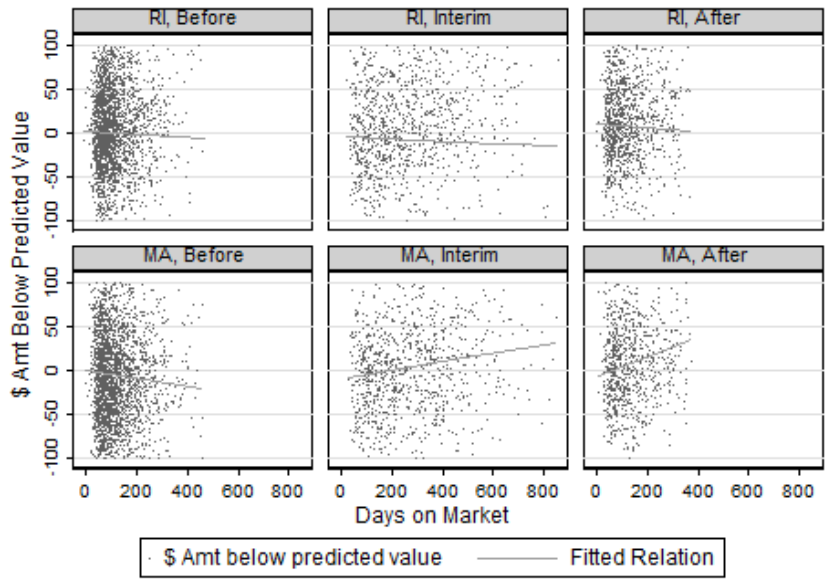
Figure A3(a) of the Appendix displays the hedonic price index for each state controlling for home characteristics.[10] There was a noticeable decrease in the hedonic price index of Massachusetts relative to Rhode Island following the policy change, a decrease that appeared to last longer than other previous fluctuations. However, the gap between the two states diminished approximately six months into the new policy. Figure A3(b) similarly reports the hedonic index for actual days to sell of homes in our data. Relative to Rhode Island, Massachusetts homes experienced a sharp increase in days to sell months after the policy change, but not in the long run.

Finally, we are interested in the relationship between home sale prices and days on market, and how the policy affected this relationship. Using the same hedonic-price-index method, we regress sale prices of all homes in our data on home characteristics and monthly dummies to construct each home's predicted sale price. Figure 2 plots the amount by which a home's sale price deviated from prediction against its actual days on market at the time of sale. We also plot a line that represents the fitted linear relation between these two variables. We perform the same analysis separately for each state and each of the three home groups. For the Interim and After Groups in Massachusetts, a longer time to sell was associated with

OFHEO House Price Index is estimated using repeated observations of values of single-family homes on which at least two mortgages were originated and subsequently purchased by either the Federal Home Loan Mortgage Corporation or the Federal National Mortgage Association since January 1975. Because the use of repeat transactions on the same home helps control for differences in home quality, the OFHEO House Price Index is regarded as a "constant quality" house price index (source: http://www.fhfa.gov).

[10]We regress sale prices of all homes in our data on home characteristics and a set of monthly dummies. The coefficients of the monthly dummies constitute the hedonic price index (Wallace, 1996).

Figure 2: Relationship between Actual Days on Market until Sale and the Deviation of Sale Prices from Predicted Values



Notes: Predicted sale prices are based on hedonic regression of sale prices of all homes in the data on home characteristics and monthly dummies.

a larger price reduction below the predicted value, although the opposite was true for the Before Group and all homes in Rhode Island.

These empirical regularities suggest that in Massachusetts the new policy changed the relisting tendency, days on market, home sale prices, and the relationship between days on market and sale prices. In the next section we explore the policy effects in detail.

4 Regression Analyses of Policy Effects

4.1 Buyer-Side Responses to Policy

Prior to the policy change, seller relisting compromised the information value of the days on market statistic (Sobel and Crawford, 1982; Farrell and Rabin, 1996). After the policy change, the true days on market statistic would be more diagnostic of home quality. The existence of the Interim Group of homes, which were caught in the middle of the policy

change, allows us to investigate how home buyers reacted to the discrete improvement in information at the time of the policy change. §4.1.1 below studies the effect of true days on market information on home sales. §4.1.2 investigates whether this effect is symmetric between slow-moving homes and freshly listed homes. §4.1.3 explores the behavioral mechanisms behind buyer-side responses to the new policy.

4.1.1 Effects of True Days on Market Information

Displaying a home's days on market could affect its sale price. The new policy might further shift this effect because the displayed days on market information is now of better quality. For a clean test of this policy impact, we take a snapshot of Interim Group homes at the time of the policy change, record their displayed days on market, and study the effect on sale prices. We estimate the following specification of sale prices ($1,000) for the Interim Group:[11]

$$SalePrice_i = \alpha_1 MA_i \times PriorDOM(Norm)_i + \alpha_2 PriorDOM(Norm)_i + X_i\beta + Town_i + MonthSold_i + \epsilon_i.$$

MA_i is an indicator variable which equals 1 if home i is located in Massachusetts and 0 if it is located in Rhode Island. $PriorDOM(Norm)_i$ is home i's days on market displayed at the time of the policy change, normalized relative to all homes with the same number of bedrooms in the same town. Normalization helps control for potential differences in home liquidity driven by property size or township, factors that might affect what amounts to "slow sale" in buyers' perception. The interaction term $MA_i \times PriorDOM(Norm)_i$ reflects whether Massachusetts homes in the Interim Group were affected differently by displayed prior days on market than their Rhode Island counterparts.

The vector X_i contains home characteristics, including square footage, acreage, and dum-

[11] We estimate a similar specification for whether a home ends up being sold. As Table A2 of the Appendix shows, displayed days on market at the policy change does not significantly affect homes' likelihood of sale. Therefore, the rest of the buyer-side exploration will focus on reporting the policy effect on home sale prices.

mies for the number of bedrooms and bathrooms. The vector $Town_i$ contains town fixed effects that capture cross-town variations in factors related to the housing market, such as school and neighborhood quality. (The main effect MA_i is not separately identified from the town fixed effects.) To control for variation in home prices over time, we include a vector of monthly dummies $MonthSold_i$ for the month in which the property was sold. We cluster the error term ϵ_i by neighborhood (defined by which street the house is on) to control for potential serial correlation across time.

Column (1) of Table 4 reports the estimation results. The incremental effect of prior days on market for Massachusetts homes in the Interim Group is negative and significant at the $p = 0.01$ level—those homes with actual days on market one standard deviation above average at the time of the policy change suffered a $16,000 decline in sale prices relative to their Rhode Island counterparts. This result is consistent with the herding hypothesis that sluggish sales hurt a home's perceived value when actual days on market information is publicized. The effect of $PriorDOM(Norm)$ is positive and significant at the $p = 0.05$ level, which echoes the positive relationship between days on market and sale prices Figure 2 suggests. One interpretation is that sellers choose to stay on the market for a longer time to wait for a better sale price (Levitt and Syverson, 2008), especially when buyers cannot readily access the actual days on market information.

The effects of home characteristic variables are as expected. Sale prices increase with both square footage and acreage, although the benefits are diminishing. 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, which could reflect a distaste for smaller bedrooms.

As a robustness check, we use a nearest-neighbor matching estimator to evaluate the population average treatment effect of the policy (Abadie and Imbens, 2006). The advantage of this estimation procedure is that it allows us to compare homes that are similar in

Table 4: Displayed DOM at the Policy Change Affects Sale Prices: Interim Group

	(1) OLS	(2) OLS	(3) Matching	(4) Matching	(5) OLS
MA × Prior DOM (Norm)	-15.93***				
	(5.725)				
Prior DOM (Norm)	8.886**				
	(4.214)				
MA × Above Mean Prior DOM (Dummy)		-32.05***	-26.70**	-19.68*	
		(10.78)	(10.83)	(10.83)	
Above Mean Prior DOM (Dummy)		15.56*			
		(8.036)			
MA × Top 1/3 Prior DOM (Dummy)					-21.61**
					(10.55)
MA × Bottom 1/3 Prior DOM (Dummy)					15.07*
					(8.603)
Top 1/3 Prior DOM (Dummy)					21.35**
					(8.759)
Bottom 1/3 Prior DOM (Dummy)					-1.891
					(6.018)
Square Footage (1,000)	120.4***	120.5***			120.3***
	(8.983)	(8.918)			(9.024)
Square Footage (1,000) Squared	-4.172***	-4.191***			-4.192***
	(0.547)	(0.534)			(0.543)
Acreage	21.43***	21.39***			21.35***
	(6.356)	(6.369)			(6.351)
Acreage Squared	-0.151***	-0.151***			-0.150***
	(0.0485)	(0.0486)			(0.0484)
Bedroom Dummies	Yes	Yes	No	No	Yes
Bathroom Dummies	Yes	Yes	No	No	Yes
Town Fixed Effects	Yes	Yes	No	No	Yes
Month Sold Fixed Effects	Yes	Yes	No	No	Yes
Observations	2128	2128	2128	2128	2128
R-Squared	0.585	0.585	—	—	0.586

Notes: The dependent variable is home sale price ($1,000). Sample: Interim Group homes that were listed before the policy change but were still on the market at the policy change. OLS estimates in columns (1), (2) and (5). Nearest-neighbor matching estimators for average treatment effects in columns (3) and (4). Matching variables for column (3) are prior DOM, square footage, acreage, bedroom and bathroom dummies, longitude, latitude, school spending, property taxes, average town household size, average median income of town, average proportion of families with children, and town population. Column (4) additionally includes border fixed effects as matching variables. Robust standard errors clustered at neighborhood level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

observable characteristics across the state border, which helps address some of the unevenness in property characteristics between the two states that is visible in Table 2. Moreover, this procedure allows us to match houses across the state border on the basis of latitude and longitude, and in doing so compare homes that are likely perceived to be similar by buyers. Since these matching estimators have been developed to evaluate binary treatment variables, we convert $PriorDOM(Norm)_i$ into a binary variable *Above Mean Prior DOM (Dummy)*$_i$ which equals 1 if home i, at the time of the policy change, displayed days on market above

the mean value among homes of the same number of bedrooms and in the same town.[12]

To facilitate comparison, we first report in column (2) an OLS specification where we replace *Prior DOM (Norm)*$_i$ with *Above Mean Prior DOM (Dummy)*$_i$. Displaying an above-mean number of days on market had a negative effect on sale prices of Massachusetts homes relative to Rhode Island homes, consistent with column (1). Column (3) reports the results using the aforementioned matching estimator, which again indicates a negative treatment effect of displaying longer days on market on home sale prices in Massachusetts. As a further robustness check, we repeat the matching estimator procedure but use dummies for each MA-RI border pairing. As reported in column (4), the effect of displaying longer days on market was also negative although less significant. Across columns (2) to (4), the point estimate is large; relative to Rhode Island, Massachusetts homes suffered a \$20,000 to \$32,000 reduction in sale prices as the new policy revealed that they had accumulated an above-average number of days on market among homes with the same number of bedrooms in their town.

4.1.2 Asymmetric Effects of True Days on Market Information

We have seen that longer days on market, when truly revealed under the new policy, decreased sale prices on average. We further explore whether the new policy differently affected home listings that have been on the market for a long time versus those fresh to the market. There are two possibilities. First, under the old policy, most buyers understood that sellers would manipulate their displayed days on market through relisting. Therefore, buyers would not take the claimed days on market at its face value, and would evaluate home quality based on their prior perceptions of how long a home could have actually been on the market. As the actual days on market information became available at the policy change, buyers' evaluation of home quality would be polarized relative to their prior perceptions—homes with longer days on market compared with buyers' prior expectations would sell for less, whereas those with shorter days on market should sell for more.

[12]The same qualitative conclusions hold when we classify homes based on a median split of prior DOM.

The second possibility is that there was a substantial mass of consumers who were unaware of sellers' manipulation of days on market prior to the policy change. These buyers, who might be the marginal consumers for homes that had disguised long days on market, drew negative inferences about home quality when the days on market information was suddenly revealed. However, unaware of the improved informativeness of the days on market statistic after the policy change, these buyers did not reward homes with shorter actual days on market.

To explore which effect dominates, we create the dummy variable *Top 1/3 Prior DOM*$_i$ which equals 1 if home i, at the time of the policy change, displayed days on market in the top 1/3 of homes with the same number of bedrooms and in the same town. We similarly create the dummy *Bottom 1/3 Prior DOM*$_i$ to indicate homes with the bottom 1/3 *Prior DOM*. Columns (5) of Table 4 reports the results when we regress sale prices on these two dummy variables, on their interactions with the MA dummy, and on home characteristics. The effect of days on market was indeed asymmetric—although there was a strong negative effect for Massachusetts homes with the top 1/3 days on market, there was a smaller and less significant positive effect for those with the bottom 1/3 days on market. These results are suggestive of the second effect, that a mass of home buyers did not understand that the policy change made shorter days on market an informative sign of better home quality and consequently did not reward fresh home listings adequately.

4.1.3 Mechanisms behind Buyer-Side Responses

In this section, we explore the behavioral mechanisms behind buyer-side responses to the new policy. First, we want to further assess the conjecture that some buyers were unaware of seller manipulation of the days on market statistic. To do so, we sent out direct mail surveys to Interim Group homeowners, asking the following questions:

1. *Can home sellers in your state cancel a home listing and then relist it? - Yes/No*

2. *If a home listing is cancelled and then relisted, how does the home's number of days on market change in the listing?*
   - *The number is reset to zero, so that the home seems like a fresh listing.*
   - *The number carries over from the previous listing.*

We received 43 responses, 20 from Massachusetts and 23 from Rhode Island. For the first question, 27 respondents answered "yes", 13 from Massachusetts and 14 from Rhode Island. For the second question, 27 respondents chose "the number is reset to zero", 14 from Massachusetts and 13 from Rhode Island. That is, only 62.8% of respondents were aware of seller manipulation of days on market. This fraction is indeed significantly lower than 100% ($t = 4.99$, $p < 0.001$). The degree of awareness seems similar between the two states.[13]

When some buyers were unaware of seller's manipulation of days on market yet inferred home quality from this statistic, we would expect the asymmetric policy effect on home sale prices as reported in column (5) of Table 4. It remains to explore whether buyer did infer home quality from days on market. Table 4 itself cannot answer this question. A main competing explanation of the results presented therein is salience (Cai et al., 2009). For instance, a real estate website might provide sorting tools that prioritize fresher home listings. We disentangle the quality inference versus salience roles of days on market by testing whether home quality uncertainty moderates the policy effect. If the quality inference role dominates, days on market information should affect subsequent buyers' quality valuation only when home quality is uncertain. If the salience role dominates, days on market information should affect buyer valuation regardless of home quality uncertainty.

We obtain two measures of home quality uncertainty: *Older Home* is an indicator variable for whether the town, according to the U.S. Census Bureau, had a median age of housing stock above the average for our sample; *Higher Flood Risk* is an indicator variable for whether

[13]There are 5,718 homes in the Interim Group. A total of 4,140 mail surveys were successfully delivered; the rest failed to reach the intended household for reasons such as address updates. The response rate is thus around 1%, which is low and reflects a common phenomenon with mail surveys (Kaplowitz et al., 2004). The small number of respondents does not allow for further regression analysis. Nevertheless, the data indicate that a non-negligible fraction of buyers were indeed unaware of sellers' manipulation of days on market.

the town was exposed to higher-than-average flooding potential (and thus greater possibility of having water damage). A town's flooding potential decreases with its elevation and increases with the number of bodies of water nearby, as documented in Federal Emergency Management Agency Maps (Roberts, 2011). The bottom panel of Table 2 reports the summary statistics of town-specific home age and the flood risk indicator. Older age and higher flooding potential are normally believed to be associated with greater uncertainty in home quality.[14] Hence we expect the policy effect to be more relevant to towns with older homes and higher flooding potential, if buyers were indeed inferring quality from days on market.

We examine the interaction effects of *MA* × *Prior DOM (Norm)* with *Older Home* and *Higher Flood Risk* on home sale prices, respectively. As column (1) of Table 5 shows, *Older Home* has a negative interaction effect, whereas the remaining effect of *MA* × *Prior DOM (Norm)* is insignificant. These results suggest that the policy effect on home sale prices was mainly driven by towns with older homes. Column (2) shows a similar pattern—longer days on market decreased home sale prices under the new policy, but mainly in towns with higher flood risk. Results from both columns are consistent with the hypothesis that days on market information affects sale prices through the quality inference mechanism.

An additional test between the quality inference and salience effects is to examine whether market liquidity moderates the effect of days on market. If buyers were making quality inferences rather than simply chasing salience, they would interpret days on market in light of market liquidity—they would realize that slow sale in a sluggish market do not necessarily reflect quality flaws detected by their predecessors, and would be less pessimistic about the home's value. As a measure of market liquidity, we construct the dummy variable *Liquid Town*, which equals 1 if the town had below-average days to sell before the policy change. Column (3) of Table 5 shows the moderating effect of *Liquid Town* on prior days on market

[14]See http://answers.yahoo.com/question/index?qid=20091020153921AAEP8UU for a discussion thread that illustrates home buyers' uncertainty.

in Massachusetts. The interaction effect is negative, and the remaining effect of *MA* × *Prior DOM (Norm)* is insignificant. That is, a home's prolonged stay on the market carried a stigma only in liquid towns, consistent with the quality inference effect of days on market.

Table 5: Mechanisms behind Buyer-Side Responses: Interim Group

	(1)	(2)	(3)
MA × Prior DOM (Norm) × Older Home	-14.95*		
	(8.559)		
MA × Prior DOM (Norm) × Higher Flood Risk		-22.31**	
		(9.519)	
MA × Prior DOM (Norm) × Liquid Town			-31.93***
			(10.68)
MA × Prior DOM (Norm)	-4.346	-1.584	-1.235
	(6.575)	(5.662)	(5.195)
Prior DOM (Norm) × Older Home	17.23**		
	(6.961)		
Prior DOM (Norm) × Higher Flood Risk		20.17***	
		(5.580)	
Prior DOM (Norm) × Liquid Town			24.65***
			(8.941)
Prior DOM (Norm)	-3.641	-4.323	-2.681
	(5.073)	(3.532)	(3.104)
Square Footage (1,000)	120.8***	120.8***	120.6***
	(9.122)	(9.023)	(9.049)
Square Footage (1,000) Squared	-4.197***	-4.195***	-4.181***
	(0.547)	(0.547)	(0.557)
Acreage	21.84***	21.87***	21.50***
	(6.399)	(6.413)	(6.420)
Acreage Squared	-0.154***	-0.154***	-0.151***
	(0.0489)	(0.0490)	(0.0489)
Bedroom Dummies	Yes	Yes	Yes
Bathroom Dummies	Yes	Yes	Yes
Town Fixed Effects	Yes	Yes	Yes
Month Sold Fixed Effects	Yes	Yes	Yes
Observations	2128	2128	2128
R-Squared	0.586	0.586	0.587

Notes: The dependent variable is home sale price ($1,000). Sample: Interim Group homes that were listed before the policy change but were still on the market at the policy change. OLS estimates. Robust standard errors clustered at neighborhood level. * p <0.10, ** p <0.05, *** p <0.01.

In summary, we explore the behavioral mechanisms behind buyer-side responses by collecting auxiliary data. We find that a significant fraction of home buyers might indeed be unaware of seller manipulation of days on market under the old policy, and thus undervalued the informativeness of days on market under the new policy. Despite the lack of perfect awareness, however, buyers did seem to be sophisticated when interpreting the days on market information. They seemed to be able to draw active inferences of home quality

and adjust their inference based on market liquidity.

4.2 Policy Effects on Interim versus After Groups

So far we have focused on the policy effect *conditional* on homes' prior days on market at the time of the policy change. Our next question is how the new policy affected sale prices of Interim Group homes overall *unconditional* on their prior days on market. Moreover, we ask whether the new policy affected sale prices of After Group homes differently given that After Group sellers could adjust their initial listing strategies in response to the new policy. To answer these questions, we include all homes in our data and estimate the following pooled regression:

$$SalePrice_i = \alpha_1 MA_i \times InterimGroup_i + \alpha_2 MA_i \times AfterGroup_i + X_i\beta + Town_i + MonthSold_i + \epsilon_i$$

To specifically test whether the policy affected the Interim and After Groups differently, we include $MA_i \times InterimGroup_i$ and $MA_i \times AfterGroup_i$ within the same regression, using the Before Group as the common benchmark. Column (1) of Table 6 reports the estimation results. Relative to their Rhode Island counterparts, Massachusetts homes that were caught in the middle of the policy change experienced a significant $\$16,000$ decline in sale prices overall; Massachusetts homes that were listed after the policy change had a smaller and insignificant decline in sale prices, although the difference between these two effects are insignificant ($p = 0.145$) partly due to imprecise estimation of the latter effect.

To investigate whether the policy effects were asymmetric, we stratify the sample based on whether a home had above- or below-median total days to sell among homes in the same town and with the same number of bedrooms. Column (2) and (3) repeat the same regression of column (1) on these two sub-samples separately. For homes with above-median days to sell, there was a large negative policy effect on sale prices for both the Interim Group and the After Group, although the difference between these two effects is not significant ($p = 0.221$).

Table 6: Policy Effects on Sale Prices: Interim Group versus After Group

	(1) Entire Sample	(2) > Median Days to Sell	(3) < Median Days to Sell
MA × Interim Group (α_1)	-15.72**	-39.98**	-10.90
	(6.287)	(15.51)	(6.809)
MA × After Group (α_2)	-5.692	-21.75***	5.707
	(4.662)	(7.685)	(4.299)
Interim Group	10.31*	14.10	9.986
	(5.712)	(13.40)	(7.287)
After Group	11.34**	12.64	14.92**
	(4.560)	(15.44)	(6.621)
Square Footage (1,000)	102.4***	112.3***	94.87***
	(6.747)	(12.29)	(6.387)
Square Footage (1,000) Squared	-2.651***	-3.023*	-2.499**
	(0.985)	(1.650)	(1.087)
Acreage	19.15***	19.83***	20.73***
	(4.140)	(5.041)	(6.580)
Acreage Squared	-0.159***	-0.210***	-0.155***
	(0.0400)	(0.0525)	(0.0499)
Bedroom Dummies	Yes	Yes	Yes
Bathroom Dummies	Yes	Yes	Yes
Town Fixed Effects	Yes	Yes	Yes
Month Sold Fixed Effects	Yes	Yes	Yes
Difference between α_1 and α_2 (p value)	0.145	0.221	0.0107
Observations	9471	3608	5863
R-Squared	0.549	0.531	0.581

Notes: The dependent variable is home sale price ($1,000). Column (1) includes all homes in the data. Columns (2) and (3) include homes that have above-median versus below-median days to sell among homes in the same town and with the same number of bedrooms, respectively. OLS estimates. Robust standard errors clustered at neighborhood level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In contrast, for homes with below-median days to sell, the policy effect was negative for the Interim Group but positive for the After Group. Although neither effect is significantly different from zero, the difference between them is significant at the $p = 0.01$ level. One explanation of these differences is that After Group sellers in Massachusetts were able to alter their listing strategies in response to the policy change. Our newxt section explores this possibility in detail.

4.3 Seller-Side Responses to Policy

We first study the policy effect on sellers' initial listing prices ($1,000). We estimate the following specification using homes from the Before and After Groups:[15]

[15]We exclude Interim Group homes for a clean test of changes in seller responses. Nevertheless, the inclusion of the Interim Group does not affect the qualitative conclusions. Moreover, comparing the Before

$$InitialLisingPrice_i = \alpha MA_i \times AfterGroup_i + X_i\beta + Town_i + ListingMonth_i + \epsilon_i$$

The interaction term $MA_i \times AfterGroup_i$ captures the overall policy effect on the initial listing price of Massachusetts homes listed after the policy change. As column (1) of Table 7 shows, the policy led to a significant $16,000 decline in Massachusetts homes' listing prices relative to Rhode Island.[16]

There are two opposing mechanisms for a seller in setting listing prices when buyers observe days on market (Taylor, 1999). On the one hand, the seller may want to post a low initial price in order to sell early and reduce any negative quality inferences buyers may draw from slow sales. On the other hand, there is a more nuanced signal dampening effect from raising the listing price as buyers attribute slow sales to the high listing price instead of to low home quality. The overall policy impact on listing prices depends on which effect dominates. However, the latter effect is relevant only if buyers can observe the listing price history, otherwise the seller cannot credibly refer to a high listing price as an excuse for a slow sale. Therefore, variation in the observability of the listing price history can help disentangle the seller-side mechanisms. We expect initial listing prices to be higher in towns where buyers have greater access to listing price histories.

As noted by Taylor (1999), "dated advertisements" and "use of reputable brokers" are two commonly used ways for buyers to find out the price history of a home. We obtain two corresponding measures of price observability. We first construct the town-specific dummy

Group and the Interim Group reveals no significant changes in seller strategies, which is consistent with the fact that Interim Group sellers were not forewarned of the policy change.

[16]We estimate a quartile regression to explore whether the result is robust to outliers. Figure A4(a) of the Appendix plots the quartile regression coefficients. Listing prices did drop across the board after the policy change in Massachusetts. As a further robustness check, we reran the quartile regression comparing homes in the Before Group and the Interim Group. Figure A4(b) plots the quartile regression coefficients. There was no significant change in initial listing prices, which is expected as Interim Group sellers did not anticipate the policy change.

variable *Searchable Price History*, which equals 1 if there was a searchable listing price history on one of the town's realtor websites. This is an internet version of whether buyers had access to dated advertisements. We also create the dummy variable *Reputable Broker*, which equals 1 if a town had a realtor who was an "Accredited Buyer Representative Manager (ABRM)".[17] The bottom panel of Table 2 reports the summary statistics of these two variables.

We interact these two variables, respectively, with "*MA* × *After Group*" to explore the incremental effect of high price history observability on Massachusetts homes listed after the policy change. As column (2) of Table 7 shows, the interaction with *Searchable Price History* is positive—if a town had a website archiving searchable listing price histories, sellers were indeed more prone to ask a high listing price, consistent with the signal dampening incentive. Similarly, as column (3) shows, greater price observability through the presence of reputable brokers encouraged sellers to start off with a higher listing price, although the effect is less precisely estimated.

Columns (4)-(6) extend our analysis to explore the policy effect on another seller-side decision variable—the total number of price discounts that a seller posted since the initial listing. This follows analysis by Yavas and Yang (1995), who argue that the listing price serves a dual role of influencing both days on market and subsequent bargaining, and Merlo and Ortalo-Magne (2004), who use complete data on listing price changes to illuminate bargaining over real estate transactions.[18] Our results indicate that after the policy change, the number of price discounts decreased in Massachusetts relative to Rhode Island. The interactions with the two measures of price observability are both positive, although the interaction with *Reputable Broker* is again less precisely estimated. These results are di-

[17]The ABRM qualification is the "only buyer representation designation for managers, brokers and owners affiliated with the National Association of Realtors." A realtor with the ABRM qualification would specialize in buyer-oriented services, catering to the needs of real estate buyers rather than sellers. The idea of this qualification is that such agents will "provide the quality of service and degree of fidelity to buyers that sellers have customarily enjoyed."

[18]See Merlo, Ortalo-Magne, and Rust (2008) for a dynamic model of homeowners' optimal selling strategy.

rectionally consistent with Taylor's signal dampening hypothesis. In general, though, the overall drop in the number of price discounts is suggestive evidence that, after the policy change, Massachusetts sellers were less likely to adopt a strategy of a high initial price with multiple successive reductions to fish for buyers, now that days on market were truthfully revealed.

Table 7: Seller-Side Responses to the Policy Change: Before Group versus After Group

	Initial Listing Price			# Price Discounts		
	(1)	(2)	(3)	(4)	(5)	(6)
MA × After Group	-15.68**	-32.02***	-26.50***	-0.123***	-0.178***	-0.132**
	(6.339)	(9.867)	(9.184)	(0.0367)	(0.0538)	(0.0506)
MA × After Group × Searchable Price History		37.09***			0.152*	
		(13.52)			(0.0852)	
MA × After Group × Reputable Broker			24.48			0.0227
			(15.17)			(0.0630)
After Group × Searchable Price History		-29.87***			-0.0585**	
		(10.58)			(0.0270)	
After Group × Reputable Broker			-17.81			-0.0120
			(11.95)			(0.0397)
Square Footage (1,000)	102.4***	102.2***	102.3***	0.0149	0.0137	0.0148
	(14.26)	(14.24)	(14.19)	(0.0197)	(0.0200)	(0.0198)
Square Footage (1,000) Squared	-0.613	-0.604	-0.607	-0.00111	-0.00105	-0.00110
	(0.689)	(0.691)	(0.691)	(0.00103)	(0.00104)	(0.00103)
Acreage	32.61***	32.65***	32.58***	0.0106	0.0107	0.0105
	(6.148)	(6.144)	(6.139)	(0.00912)	(0.00910)	(0.00911)
Acreage Squared	-0.395***	-0.396***	-0.395***	-0.000171	-0.000172	-0.000171
	(0.0976)	(0.0973)	(0.0974)	(0.000132)	(0.000131)	(0.000132)
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
Observations	10761	10761	10761	10761	10761	10761
R-Squared	0.406	0.406	0.406	0.0957	0.0959	0.0957

Notes: The dependent variable is initial listing price ($1,000) for columns (1)-(3), and # price discounts for columns (4)-(6). Sample: Before Group and After Group homes. OLS estimates. Robust standard errors clustered at neighborhood level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5 Concluding Remarks

It is a common practice for home sellers to reset a property's "days on market" statistic by withdrawing the property and relisting it. We study the effect of a policy that prevented Massachusetts home sellers from manipulating days on market through relisting. We investigate the policy impact by comparing homes in Massachusetts to homes in the neighboring state of Rhode Island, which maintained the old policy.

We find that Massachusetts homes caught in the middle of the policy change were the hardest hit: the sudden release of true days on market information led to a $16,000 reduction in sale prices on average. The reduction mainly occurred where there was greater uncertainty in home quality and where the real estate market was more liquid, which suggests that buyers actively drew quality inference from days on market. However, a direct mail survey of households in our data indicates that a significant fraction of buyers might be unaware of sellers' tendency to manipulate days on market. The net effect was a sharp decrease in sale price for slow-moving homes but a smaller and less significant increase for freshly listed homes. After the policy change, sellers reacted by lowering their initial listing price to speed up sale, although in towns with greater transparency of listing price histories sellers turned out to charge a higher listing price to serve as an excuse for slow sale. 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.

The new policy was intended to improve the transparency of the Massachusetts real estate market. Home buyers' willingness to pay should increase with the amount of information (Milgrom and Weber, 1982). However, this study documents a decrease of home sale prices following the new policy, although the effect diminished in the long term when sellers could change their listing strategies in response. The reason is not that buyers did not know how to use the new information; buyers did seem to make sophisticated inference of home quality from days on market. However, buyers might not be fully aware of the degree of information asymmetry which had triggered the policy change; some buyers were unaware of sellers' manipulation of days on market, and thus did not sufficiently value home listings that were authentically fresh to the market. These findings suggest that policy makers should inform the public of the existence of information asymmetry while trying to reduce information asymmetry through policy reform.

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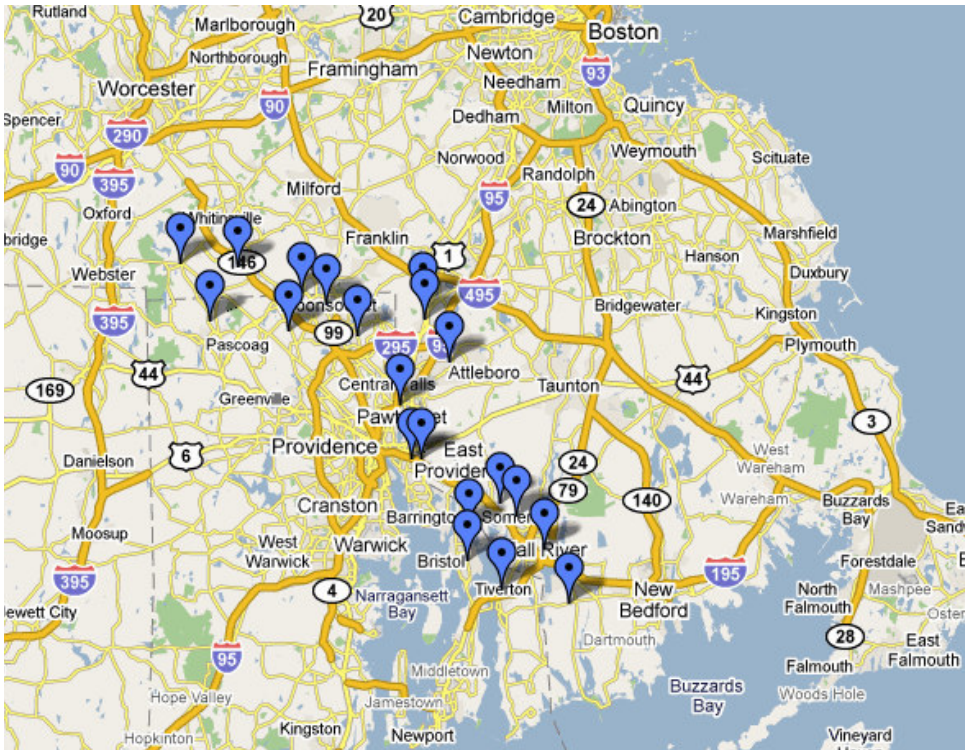
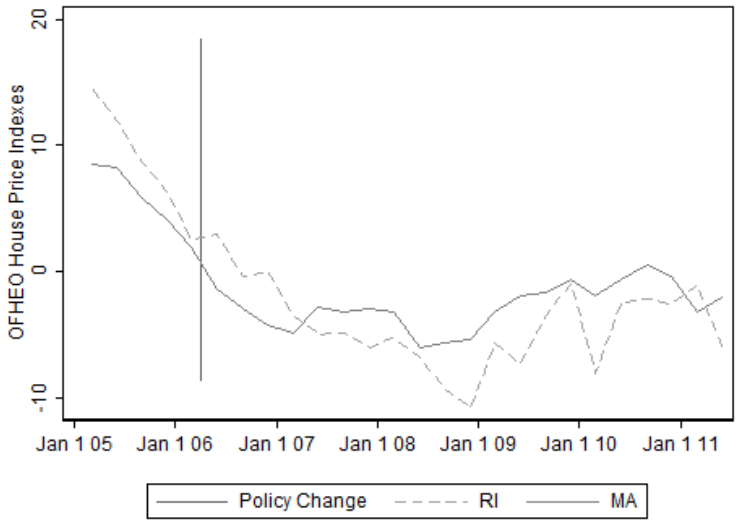
Online Appendix—Days on Market and Home Sales

Figure A1: Location of Towns Included in This Study



Notes: This figure plots the location of the towns included in this study. These towns are clustered along the Massachusetts-Rhode Island border.

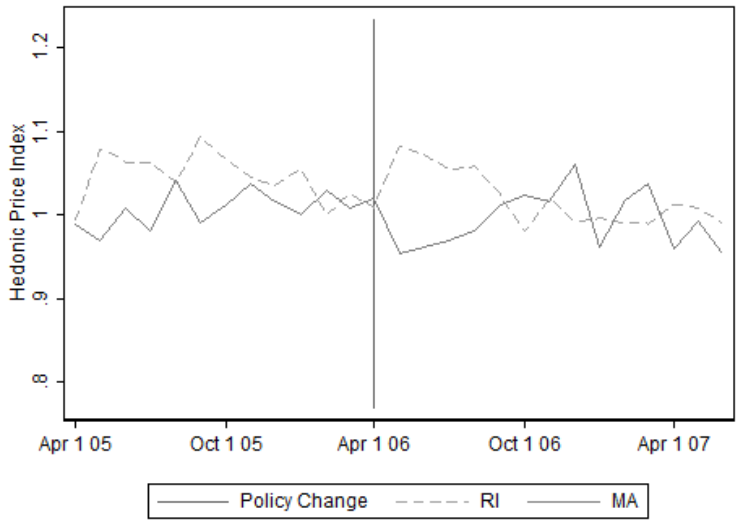
Figure A2: OFHEO House Price Indexes for Massachusetts and Rhode Island



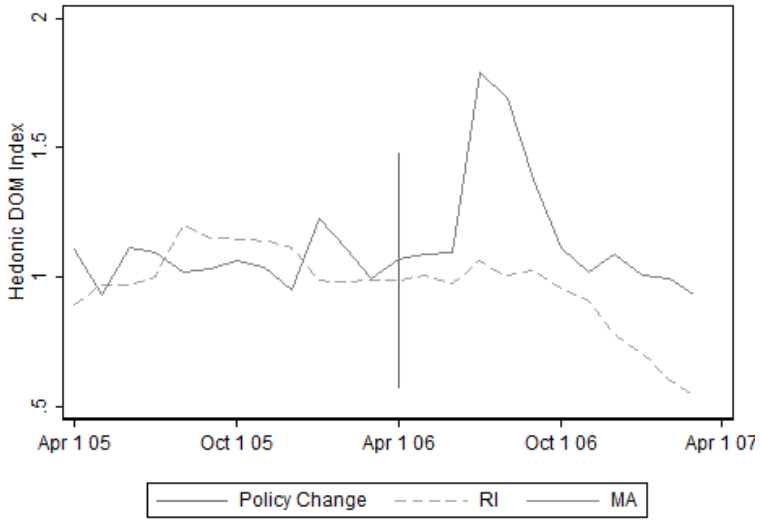
Notes: This figure plots the long-run Office of Federal Housing Enterprise Oversight (OFHEO) House Price Indexes for Massachusetts and Rhode Island. The two states exhibited similar aggregate real estate price trends during the period we study (January 2005 to June 2007).

Figure A3: Hedonic Indices: Massachusetts versus Rhode Island

(a) Hedonic Price Index



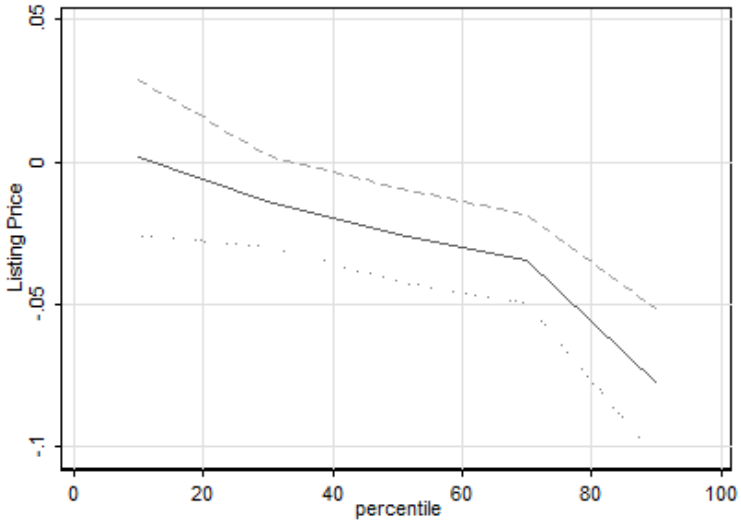
(b) Hedonic Actual Days on Market until Sale Index



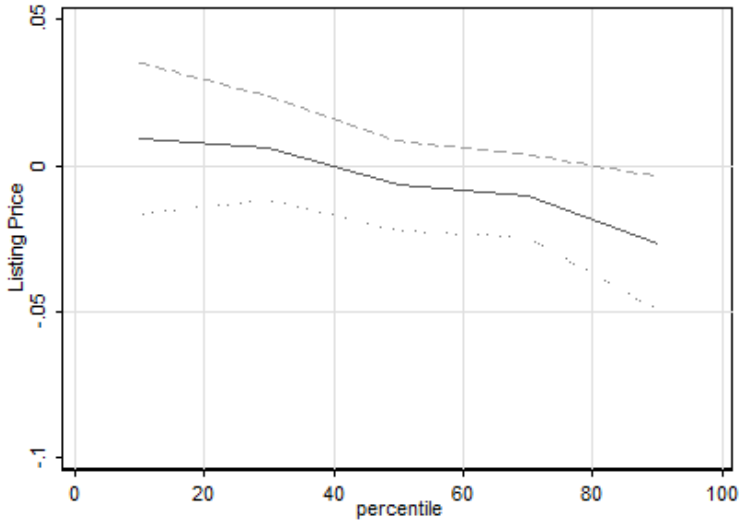
Notes: The horizontal axis denotes the month during which a home was sold. We regress sale prices of all homes in our data on home characteristics and a set of monthly dummies. The coefficients of the monthly dummies constitute the hedonic price index. We obtain the hedonic days to sell index similarly.

Figure A4: Quartile Regression of Initial Listing Prices

(a) Change in Initial Listing Prices: Before Group versus After Group



(b) Change in Initial Listing Prices: Before Group versus Interim Group



Notes: The horizontal axis denotes the percentile of initial listing prices. The solid line plots the quartile regression parameter estimate, which captures the proportional change in initial listing prices for Massachusetts homes in the After and Interim Groups, respectively, relative to the Before Group in the corresponding percentile of listing prices. The dashed lines denote the 95% confidence bounds.

Table A1: Demographics of Towns Included in This Study

Town	County	State	Population	% HHs with Children under 18	Average HH Size	Median HH Income	Property Tax Rate	Spending per Student
Attleboro	Bristol	MA	42,068	33.4	2.57	$50,807	0.9	$12,599
Blackstone	Worcester	MA	8,804	38.2	2.71	$55,163	1.3	$10,969
Douglas	Worcester	MA	7,045	43.1	2.85	$60,529	1.5	$9,555
Fall River	Bristol	MA	91,938	29.9	2.32	$29,014	0.9	$14,157
North Attleboro	Bristol	MA	27,143	36.0	2.60	$59,371	1.3	$11,162
Plainville	Norfolk	MA	7,683	33.4	2.53	$57,155	1.4	$10,936
Seekonk	Bristol	MA	13,425	35.7	2.77	$56,364	1.3	$11,444
Somerset	Bristol	MA	18,234	28.1	2.57	$51,770	0.9	$12,906
Swansea	Bristol	MA	15,901	31.1	2.67	$52,524	1.2	$11,790
Uxbridge	Worcester	MA	11,156	29.2	2.79	$61,855	1.4	$12,675
Westport	Bristol	MA	14,183	29.0	2.62	$55,436	0.8	$10,854
Bristol	Bristol	RI	22,469	28.2	2.45	$62,575	1.0	$14,789
Burrillville	Providence	RI	15,796	36.6	2.75	$52,587	1.8	$11,924
Cumberland	Providence	RI	31,840	33.2	2.59	$54,656	1.8	$10,791
East Providence	Providence	RI	48,688	27.1	2.33	$39,108	1.2	$14,297
North Smithfield	Providence	RI	10,618	22.4	2.61	$58,602	1.6	$11,782
Pawtucket	Providence	RI	72,958	30.5	2.41	$31,775	1.3	$12,668
Tiverton	Newport	RI	15,260	29.6	2.51	$49,977	1.0	$12,263
Warren	Bristol	RI	11,360	27.4	2.36	$41,285	1.8	$14,789
Woonsocket	Providence	RI	43,224	31.2	2.37	$30,819	1.0	$12,494

Source: Year 2000 Census. HH stands for household.

Table A2: Displayed DOM at the Policy Change Does Not Significantly Affect Likelihood of Sale: Interim Group

	(1) Whether Sold	(2) Whether Sold	(3) Whether Sold
MA × Prior DOM (Norm)	-0.00950		
	(0.0140)		
Prior DOM (Norm)	-0.112		
	(0.113)		
MA × Above Mean Prior DOM (Dummy)		-0.00802	
		(0.0294)	
Above Mean DOM Prior (Dummy)		0.0203	
		(0.107)	
MA × Top 1/3 Prior DOM (Dummy)			-0.0323
			(0.0290)
MA × Bottom 1/3 Prior DOM (Dummy)			-0.0358
			(0.0371)
Top 1/3 Prior DOM (Dummy)			0.0674***
			(0.0229)
Bottom 1/3 Prior DOM (Dummy)			0.00443
			(0.0342)
Square Footage (1,000)	-0.00932	-0.00936	-0.00935
	(0.00654)	(0.00657)	(0.00670)
Square Footage (1,000) Squared	0.0000613	0.0000622	0.0000627
	(0.0000669)	(0.0000672)	(0.0000685)
Acreage	-0.0107***	-0.0106***	-0.0105***
	(0.00296)	(0.00297)	(0.00295)
Acreage Squared	0.000103***	0.000102***	0.000100***
	(0.0000294)	(0.0000295)	(0.0000292)
Bedroom Dummies	Yes	Yes	Yes
Bathroom Dummies	Yes	Yes	Yes
Town Fixed Effects	Yes	Yes	Yes
Listing Month Fixed Effects	Yes	Yes	Yes
Observations	5718	5718	5718
R-Squared	0.115	0.115	0.115

Notes: The dependent variable is whether a home ends up being sold. Sample: Interim Group homes that were listed before the policy change but were still on the market at the policy change. Probit estimates. Robust standard errors clustered at neighborhood level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.