# The Effects of Macroprudential Mortgage Insurance Regulation During a Housing Boom: Evidence from Canada

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**Abstract**

In this paper, we seek to empirically assess the impact of a recent macroprudential regulation on housing market in the midst of a housing boom in Toronto, Canada's largest housing market. We exploit a natural experiment arising from the 2012 law change that limits Mortgage Insurance (MI) to homes with a purchase price of less than 1 million Canadian dollars. We find that, in the single-family-house market, the limitation of MI insurance caused a 0.5 percent decline in the annual growth of houses listed above $1M and a 0.2 percent decline in the annual growth of houses sold above $1M. Both estimates are statistically significant. Turning to the condominium market, these effects are much smaller, with a significant estimate of 0.14 and an insignificant estimate of 0.03, respectively. We view the main contribution of this paper as providing the credible estimation of the impact of a recent macroprudential regulation on the housing market, and discuss related policy issues.

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## 1. Introduction

Since the crash of global financial markets caused by the great housing bubble in the 2000s, macroprudential policies, a nascent field of policy responses, has attracted considerable attention around the world for its potentially important role in mitigating the risks associated with a housing boom. These policies aim to create a buffer in a boom to ensure that "shocks from the housing sector do not spill over and threaten economic and financial stability" (IMF Speech, 2014).[2] They include cyclical changes in capital requirements, risk weights, maximum loan-to-value ratio, and mortgage insurance rules, etc. Being still in their infancy, these policies are controversial. For example, the Bank of International Settlements (BIS) suggested in 2014 that central banks need to use tighter monetary policy to counter domestic financial booms.[3] In marked contrast, Janet Yellen, the Chair of the Federal Reserve, responded by contending that macroprudential regulation, and not monetary policy, should be used to control the risks associated with large asset price expansions.[4] Despite all the attention and diverse views from policy makers and economists, there has been little systematic evidence on the consequence of macroprudential policies.[5] As noted in **?**, a major obstacle to assessing the effectiveness of macroprudential policies is that these policies are "typically used in combination with macroeconomic policy and direct interventions...to the housing market...complicating the challenge to attribute outcomes to specific tools."

In this paper, we seek to empirically assess the impact of a recent macroprudential regulation on housing market in the midst of a housing boom in Toronto, Canada's largest housing market. We exploit a natural experiment arising from the 2012 law change that limits Mortgage Insurance (the transfer of credit and mortgage risk from lenders and originators to insurers; henceforth, MI) to homes with a purchase price of less than 1 million Canadian dollars. Under the new policy, anyone who purchases a home with a sales price greater than or equal to 1 million dollars must contribute at least 20 percent to a down payment to secure an uninsured mortgage. Given that the average home purchase price for Toronto in 2012 was 601,000 Canadian Dollars,[6] the law is aimed to calm the higher-end housing market sector that experienced marked price appreciation in major Canadian cities such as Toronto.[7] Figures 1-2 present empirical patterns that motivate our

[2]See "Managing House Price Boom: The Role of Macroprudential Policies," December, 2014. https://www.imf.org/external/np/speeches/2014/121114.htm.

[3]See "Bank for International Settlements 84th Annual Report." http://www.bis.org/publ/arpdf/ar2014e.pdf. P. 73 and 94-96.

[4]In July 2014, Janet Yellen stated that "monetary policy faces significant limitations as tool to promote financial stability" and that "a macroprudential approach to supervision and regulations needs to play the primary role." See "Monetary Policy and Financial Stability" http://www.federalreserve.gov/newsevents/speech/yellen20140702a.htm.

[5]For example, Ben Bernanke in 2015 noted that the "the macroprudential approach remains unproven."

[6]The average value of the US dollar-Canadian dollar exchange rate in 2012 was approximately 1.00, suggesting that the average home in Toronto had a purchase price of 601,000 US dollars.

[7]Indeed, Jim Flaherty, the Canadian Finance Minister who designed and implemented the regulation made the

analysis.

Figure 1 plots smoothed percentage changes in sales volume before and after the change in MI policy by sale price. To create this figure, we count the number of sales in price bins of $10,000 and perform a local linear regression on data points below and above $1M, separately, where the pre-treatment period is the first six months of 2012 and the post-treatment period is the first six months of 2013.[8] The figure shows that, as price moves from $700,000 to $1,300,000, changes in sales volume increase in a roughly monotonic fashion until the million dollar threshold, followed by a sharp decline at the threshold, and then monotonically declines thereafter. Thus, there is substantial evidence of a jump at this price threshold in the wake of the policy change.

Figure 2 plots weighted sales volume for houses within a $100,000 bandwidth above and below $1M by month against a variable representing the number of months to the enactment of the MI policy. In constructing this figure, we calculate weighted sales volume using a quartic weighting kernel, which is symmetric about $1M and assigns lower weights to sales further away from $1M. As expected, there is considerable seasonality in the data. Before the enactment of the policy, the two series follow one another very closely and then diverge after the policy date and towards the end of the series. There is a noticeable spike in the sales volumes difference across the $1M threshold in the aftermath of the MI regulation.

Figures 1-2 suggest that the MI policy caused two discrete changes in the Greater Toronto Area (GTA) housing market: one in the month when the MI policy was imposed, and the other at the million dollar threshold. Exploiting these two sources of variations, we first estimate the effect of the MI policy on housing sales using a differences-in-differences approach combined with the regression discontinuity design. In particular, we treat sales slightly above the million dollar threshold as a "quasi-treatment" group and sales slightly below the threshold as a "quasi-control" group, with houses priced further away from the threshold receiving less weight in both groups. Controlling for business cycle and seasonality effects, we find that the MI policy reduces the number of houses listed in the $1M to $1.1M range by about 20% and the number of houses sold at the same range by 12%. These estimates are robust when we include group-specific trends, which absorb the influence of any other macro factors that might have a differential impact on houses below and above the million dollar threshold. Taken alone, these results seem to suggest that the MI policy has desired effects in calming the higher end of the housing market.

However, categorizing housing sales by price thresholds is potentially problematic as the MI policy could have affected house prices themselves. As a result, there is no exogenous distinction between the *control* and *treatment* groups as in the standard differences-in-differences analysis. In

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following statement in 2012 regarding substantial house price appreciation and the corresponding law change: "I remain concerned about parts of the Canadian residential real estate market, particularly in Toronto...[and] we need to calm the...market in a few Canadian cities." See "Canada Tightens Mortgage-Financing Rules." *Wall Street Journal*. June 21, 2012.

[8]By using the same interval in both 2012 and 2013, we remove such seasonal effects.

addition, the regression discontinuity approach above is not informative about the spillover effects of the MI policy on the house sales at other points of the price distribution, making it difficult to assess the overall consequences of the MI policy. For example, one may be concerned that although the MI limit slowed the boom in above million-dollar housing market, it may have also spurred a boom for homes below the million-dollar mark.

To address these problems, in our main analysis, we employ a distribution regression approach first proposed by Foresi and Peracchi (1995) and estimate the *joint* effects of the MI policy on the distribution of house prices and sales at different points along the house price distribution. Unlike the differences-in-differences approach that has focused on the effects of the MI policy on the sales around the million dollar threshold, the distribution approach models the impact the MI policy on the proportion of houses listed (sold) above any given price within each postal code. When the price is very low, this proportion is simply the fraction of all houses sold. When the price is set at $1M, this proportion becomes the fraction of houses sold above 1 million dollars. As such, the distribution approach not only nests the differences-in-differences approach above but also allows us to examine the spillover effects at other points of the price distribution.

As noted above, a key challenge to estimating the effect of the MI policy is that the policy is accompanied by a number of other prudent lending regulations as well as a booming market.[9] Under the distribution regression approach, we first estimate the before-after estimator along different points of the price distribution and then examine whether these estimates have a discontinuity at the million dollar threshold. Thus, our identifying assumption is that the impact of other macro forces on house sales is continuous along the price distribution around the time when the MI policy is implemented. Under this assumption, evidence of discontinuity at the million dollar threshold lends support to the idea that the MI policy affects the joint distribution of house sales and prices. To further isolate the effect of the MI policy from other potential sources of a discontinuity at the $1M, such as psychological bias, we also perform a "falsification" test by repeating the same analysis before the implementation of the policy, between 2011 and 2012. Finally, we combine the estimates from the falsification test with the estimates of the MI policy to yield a double-differences regression discontinuity estimator.

We find that, in the single-family-house market, the limitation of MI insurance caused a 0.5 percent decline in the annual growth of houses listed above $1M and a 0.2 percent decline in the annual growth of houses sold above $1M. Both estimates are statistically significant. Turning to the condominium market, these effects are much smaller, with a significant estimate of 0.14 and an insignificant estimate of 0.03, respectively. We go on to investigate the channels through which the MI policy might affect the housing sales by examining the estimated spillover effect along other points of the price distribution. Our results demonstrate that the MI policy generates not only a sharp decline in houses listed right above $1M, but also a spike in houses listed right below $1M. The spike in the latter segment is accompanied by a higher fraction of the sales over asking price

[9] See Section II for details.

and a shorter seller time on the market, both of which are associated with the MI policy. This suggests that sellers of million dollar homes respond to the policy actively by pricing below the threshold. However, the underlying housing market was in the midst of a demand boom, with very few buyers constrained by the 20% down payment and hence the new MI limitation.[10] Thus, the under-listing by sellers actually ignites bidding wars among homebuyers, which not only speed up housing sales but also push the sales price well above the asking price. This positive bidding effect indirectly induced by the policy largely offset the policy's direct dampening impact on the asking price, yielding a small net effect on the final sales price.

Since the MI policy should not affect sales of houses priced well below the million dollar threshold, finding an effect of the MI policy at the lower end of the housing market would suggest a spurious correlation between the MI policy and other macro-prudential or monetary policies, confounding the interpretation of our estimates. The spillover estimates show that indeed the MI policy has little impact on sales below $900,000 or above $1,100,000. This lends further credibility to our main estimates.

We view the main contribution of this paper as providing the credible estimation of the impact of a recent macroprudential regulation on the housing market. Despite all the debates and controversies that macroprudential policies have brought in the policy arena, their effectiveness has been understudied. A few recent studies have considered macroprudential tools in the face of house price booms.[11] However, restricted by the low frequency, aggregate data they use, these studies are largely descriptive. To the best of our knowledge, our paper represents the first to exploit micro-level house transaction data to assess causal effects the MI macroprudential regulation during a house price boom.[12]

Further, in the case of the MI policy, standard differences-in-differences estimates of the policy effects can be quite misleading as it requires the strong assumption that the policy does not affect the price threshold. Our approach not only deals with the price endogeneity directly but also provides a comprehensive picture of what happens to listing, bidding, and sales at different points of the price distribution under the MI policy.

These findings also have important policy implications. While macroprudential policies are typically targeted at a narrow segment of the market, our results show that they can have non-trivial spillover effects on the nearby segments. Depending on the housing market conditions, these spillover effects could largely mitigate the government's original effort to cool down the targeted

[10]See "Targeting high-end mortgages more politics than protection, experts say." *The Globe and Mail.* June 22, 2012.

[11]See Crowe et al. (2011); Crowe et al. (2013); Elliott et al. (2013); Ariccia et al. (2012); Lim et al. (2013), and Krznar and Morsink (2014).

[12]Other papers that use micro-level date in the study of housing markets include Garmaise (2013); Anenberg and Kung (2014); and Landvoigt et al. (2015).

segment. For example, we find that MI limitation on the million-dollar homes effectively reduces sellers' asking price, resulting in fewer listings in this segment. But in a market where buyers are not cash-constrained, this also encourages buyers to bid more aggressively in the segment right under the million dollar threshold, resulting in little actual changes in the sales price and volume for million dollar homes. Thus, understanding how the market participants respond to the policy, not only in the targeted segment but also from the overall population, is crucial for developing an appropriate policy response framework.

Finally, we want to emphasize that the lessons learnt from this paper are important not only for Canada, but also for other nations around the world. Indeed, in the United States for example, over 1.1 trillion US dollars of mortgages are insured by the government-backed Federal Housing Administration (FHA) and the US Congress is reviewing proposals that would make the US MI system similar to that used in Canada.[13] Further, MI is also a key part of the housing finance systems the United Kingdom, the Netherlands, Hong Kong, France, and Australia.

## 2. Background

### 2.1. Overview of the Canadian Housing Market

Since the early 2000s, Canada has experienced one of the world's largest modern house price booms. Indeed, between 2000 and 2014 Canadian home prices have surged nearly 150 percent and, in contrast to other large housing markets like those in the United States, home values in Canada only suffered minor price depreciation during the recent financial crisis. Figure 3 plots the national house price indices for the United States and Canada since 2000.[14] In the early 2000s, house prices increased markedly in both the US and Canada in midst of the global housing boom. Then starting in 2007, US home values plummeted with the onset of the financial crisis, while those for Canada suffered only a minor episode of price deterioration. Following the crisis, a low interest rate environment and a well functioning housing finance system further facilitated large price increases in the Canadian housing market (Crawford et al. 2013). Then, from 2009Q1 to 2013Q4, Canadian home values jumped by 33 percent, the 2nd largest instance of price appreciation in all OECD countries over this time period.[15] As home prices continued to accelerate in the post-recession era, the Canadian government became increasingly concerned that this large price appreciation would

[13] Option 3 in "Reforming America's Housing Finance Market, A Report to Congress." February 2011. The US Treasury and the US Department of Housing and Urban Development.

[14] The house price indices are quarterly seasonally adjusted data from the Bank of International Settlements.

[15] Of the 32 OECD countries, only Turkey experienced a larger increase in house prices at 40 percent over this period. The house price indices are quarterly seasonally adjusted data from the Bank of International Settlements. See the data appendix for more details. The list of OECD countries is from the OECD's website: http://www.oecd.org/about/membersandpartners/list-oecd-member-countries.htm

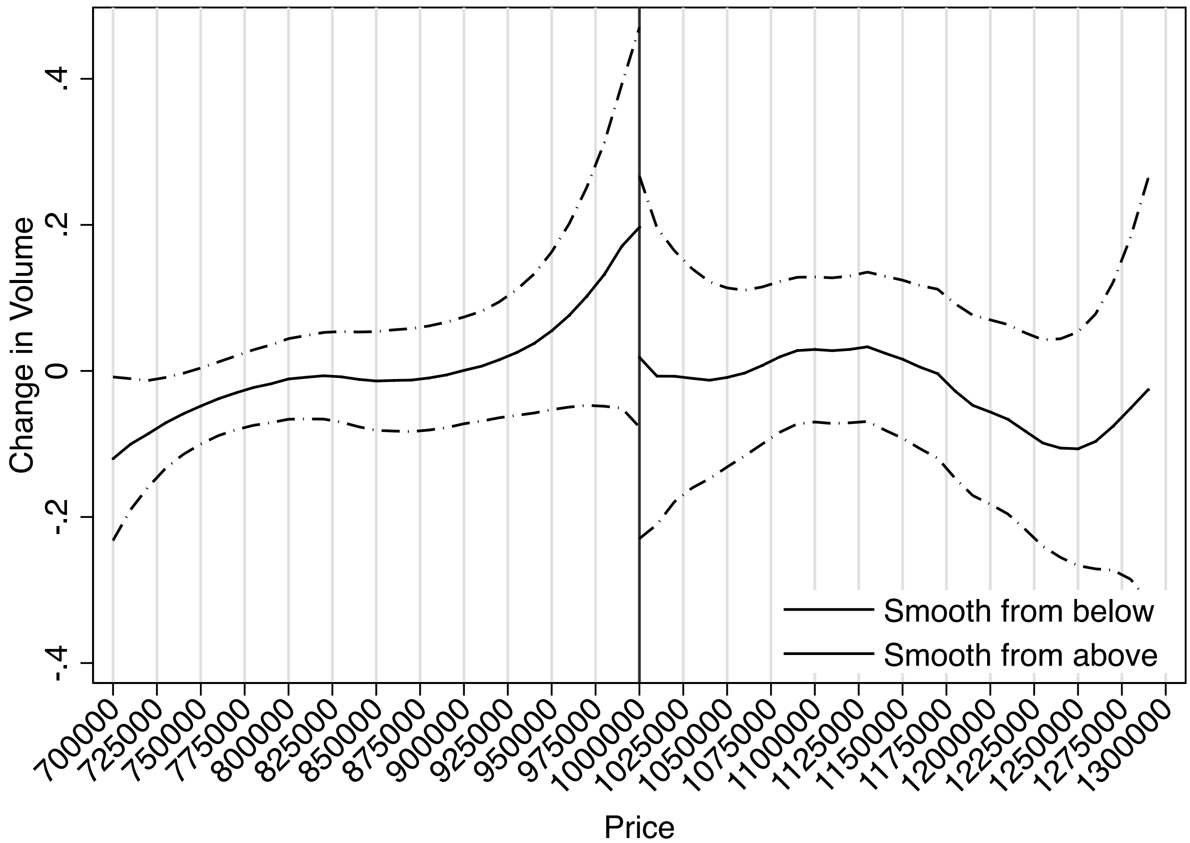


Fig. 1.— Smoothed Change in Volume by Sales Price

***Note*:** The sample includes all sales between \$700,000 and \$1,300,000. Volume is calculated by summing all transactions in sales price bins of \$10,000 in the first 6 months of 2012 and 2013. We calculate the log change in volume in each bin by comparing 2012 to 2013. The solid line is the fitted values from a local linear regression, run separately, on data points above and below \$1M. Confidence bands are produced by bootstrapping the procedure 100 times.

lead to an eventual adverse and severe housing market correction. Indeed, Jim Flaherty, Canada's Minister of Finance from February 2006 to March 2014, said in 2013 that, "We [the Canadian government] have to watch out for bubbles - always - ...including [in] our own Canadian residential real estate market, which I keep a sharp eye on."[16] Further, when talking about the Canadian housing market, Robert Shiller suggested in 2012 that "...what is happening in Canada is kind of a slow-motion version of what happened in the US."[17]

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[16]"Jim Flaherty vows to intervene in housing market again if needed." *The Globe and Mail.* November 12, 2013.

[17]"Why a U.S.-style housing nightmare could hit Canada." *CBCNews.* September 21, 2012.

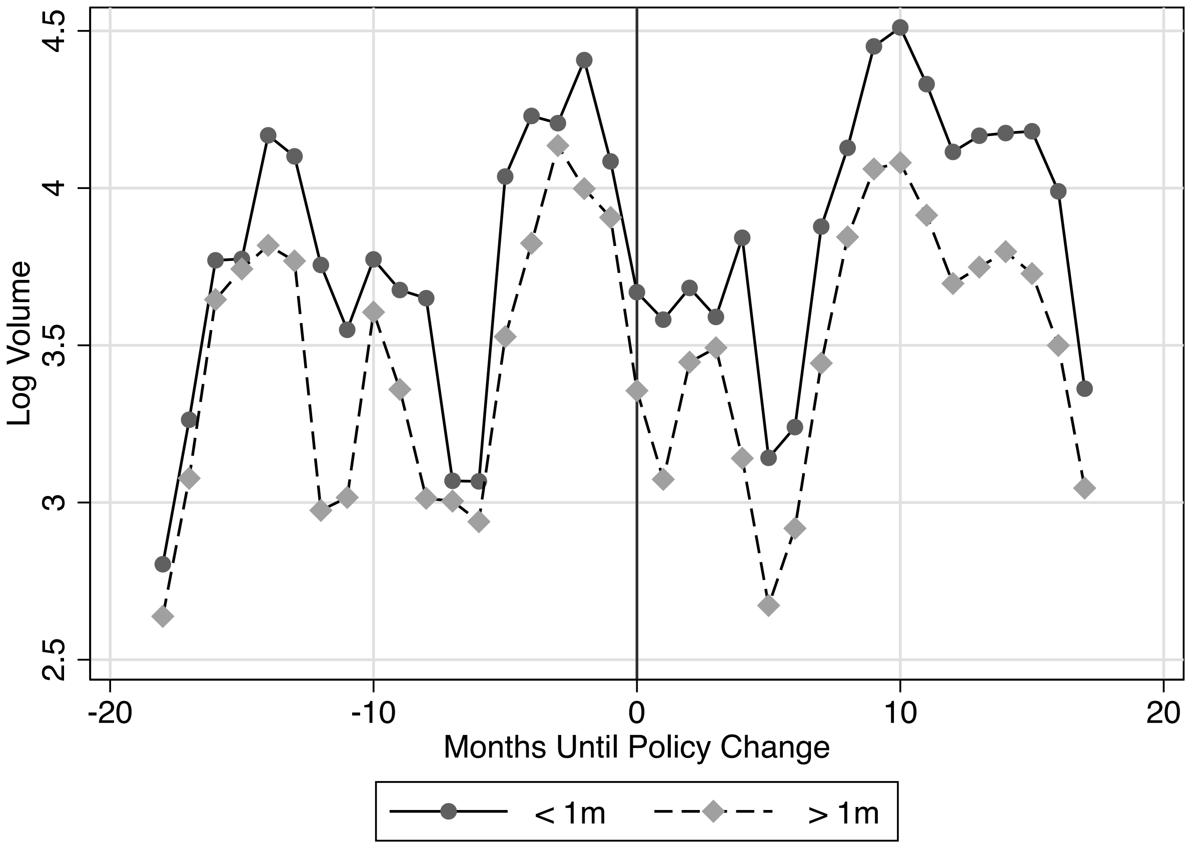


Fig. 2.— Percentage difference in Volume between homes below and above $1M

***Note*:** The sample includes all sales between $900,000 and $1,100,000. Volume is calculated by summing all transactions below and above 1 million dollars, using a quartic weighting function that gives less weight to sales further away from 1 million. The two series are the log of weighted sales volume month.

Thus, to counter the potential risks associated with this house price boom, the Canadian national government implemented four major rounds of housing market macroprudential regulation between July 2008 and and July 2012.[18] Over this time period, these interventions included increasing the minimum down payment for a mortgage (2008, 2010); reducing the maximum amortization period for new home loans (2008, 2010, 2012); reducing the maximum amount that can be borrowed during a refinancing (2010, 2011); increasing homeowner credit standards (2010, 2012); and limiting government-backed mortgage insurance to homes with a purchase price of less than $1M,

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[18]For a summary of events, see "The Fourth Round of Mortgage Tightening, One Year Later." The Quebec Federation of Real Estate Boards; 2013.

the focus of this paper. We describe the MI market in Canada and other countries in sections 2.2 and 2.3; section 2.4 gives a detailed overview of the Canadian MI regulation of 2012; and in section 4, we use a novel micro-level dataset to assess the effects of the 2012 MI regulation on the housing market in Toronto.

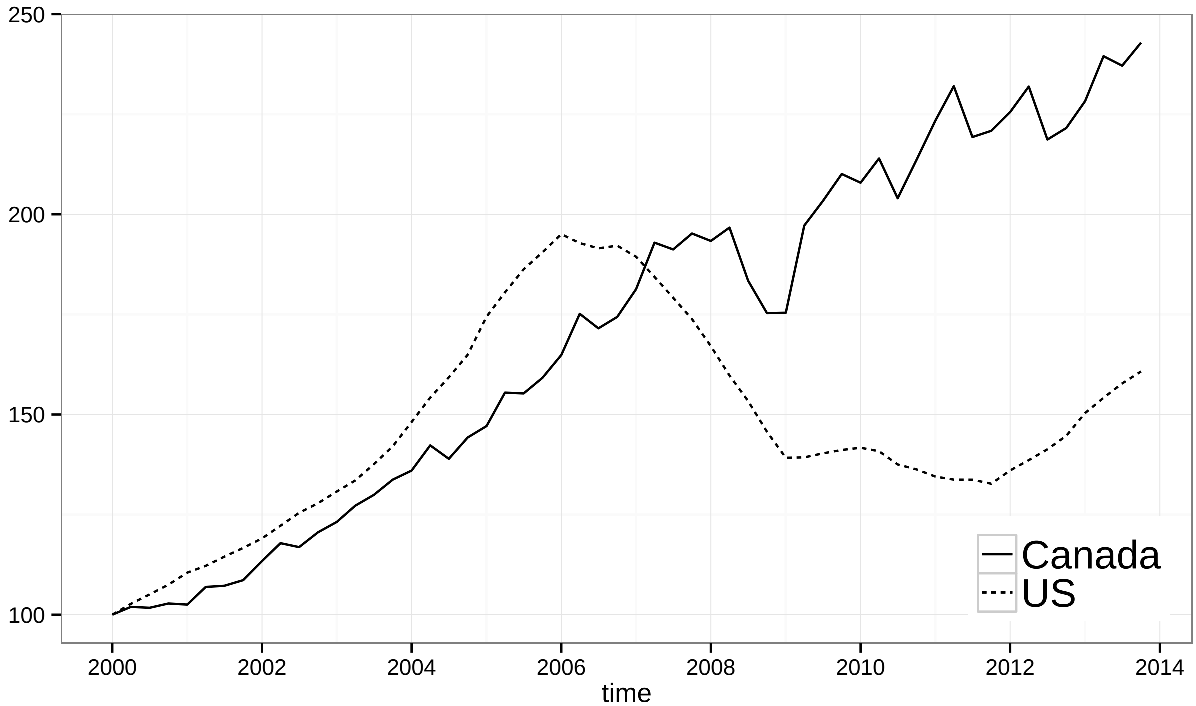


Fig. 3.— House Price Indices for the U.S. and Canada

### 2.2. Mortgage Insurance in Canada

Mortgage Insurance (henceforth, MI) is a financial instrument used to protect lenders and the suppliers of funds by transferring mortgage default risk from lenders and originators to insurers. **?** contends that MI facilitates flexibility in the housing finance system by allowing those with lower levels of equity (e.g. lower initial down payments) the ability to purchase a home. Specifically, MI extends the so-called "underwriting envelope" along the loan-to-value (LTV) dimension so that less qualified borrowers are able to obtain mortgage credit. In Canada, regulated financial institutions are required to purchase mortgage insurance on any loan with an LTV higher than 80 percent.[19]

[19]The 12 largest financial institutions originate over 90 percent of all home mortgages in Canada. There is a small unregulated housing finance sector that accounts for approximately five percent of all Canadian mortgage loans. For more details, Crawford et al. (2013) and "Mortgage insurance: market structure, underwriting cycle and policy

The mortgage insurance premiums vary based on the LTV and are made as a single upfront payment at the time of loan origination that covers the entire amortization period. Typically, lenders pass the mortgage insurance premiums completely on to the mortgage borrower, so that the mortgage insurance premiums are included in the home loan.

In Canada, the MI market is dominated by three main players: The government owned Canada Mortgage and Housing Corporation (CMHC) and two private insurers, Genworth Financial Mortgage Insurance Company Canada (Genworth) and Canada Guaranty. CMHC commands a 70 percent market share, while Genworth and Canada Guaranty make up just 25 and 5 percent of the market, respectively.[20] Home mortgages insured through CMHC have an explicit guarantee from the Canadian federal government that provides 100 percent coverage on the full extent of losses including interest and reasonable expenses. To facilitate competition between CMHC, Genworth Financial, and Canada Guaranty, the Canadian government guarantees 90 percent of the obligations for private mortgage insurers. As all three institutions benefit from guarantees provided by the Canadian government, both the government and privately-owned mortgage insurers are subject to financial market regulation through the Canadian Office of the Superintendent of Financial Institutions (OFSI). Thus, neither CMHC nor its two private sector competitors can provide MI on mortgage loans for home purchases with a sale price greater than or equal to $1M and with an LTV over 80 percent.[21]

Although statistics regarding the distribution of MI across geographies and prices are largely unavailable, we do know that approximately three-fifths of Canadian mortgage loans require MI (Krznar and Morsink 2014). Thus, the majority of homeowners do not make a 20 percent down payment. Yet some anecdotal evidence suggests that in more speculative Canadian housing markets, like that of Toronto, a larger portion of home buyers make a 20 percent or more down payment, especially at the higher end of the house price spectrum.[22]

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implications." *Basel Committee on Banking Supervision Joint Forum.* August 2013.

[20]"Mortgage insurance: market structure, underwriting cycle and policy implications." *Basel Committee on Banking Supervision Joint Forum.* August 2013.

[21]See Crawford et al. (2013), Krznar and Morsink (2014), and "High-end mortgage changes seen as return to CMHC's roots." *The Globe and Mail.* June 23, 2012. We would also like to thank Jie Zhou of the Bank of Canada for further clarification on this point. Note also that insurers may still provide portfolio or "bulk" insurance on mortgage loans where the LTV is less than or equal to 80 percent even if the home purchase price was over $1 million.

[22]"Flaherty's million dollar mortgage change hits only 0.1% of buyers; Buyers of home worth $1-million don't really need taxpayer help." *The Globe and Mail.* June 25, 2012.

## 2.3. Mortgage Insurance in other Countries

In addition to Canada, MI plays an important role in the housing finance systems of Australia, France, Hong Kong, the Netherlands, the United Kingdom, and the United States.[23] In particular, the MI system in Canada is substantially similar to that used in the United States: The US MI market is dominated by a large government-backed entity, the Federal Housing Administration (FHA), and MI is required for all loans with an LTV of less than 80 percent.[24] Indeed, the FHA, which operates with an implicit guarantee from the US Government, insures a portfolio of $1.1 trillion of mortgages and insured over 4.8 million mortgages alone in 2010.[25] Moreover, in line with the Canadian housing finance system, the US Congress is considering a proposal to guarantee the MI policies written by private insurers in order to facilitate competition between the FHA and the private market.[26] In total, given the international importance of MI across housing markets, our analysis in this paper may have important implications not only for Canada, but also for other major housing markets in North America, Europe, and Australia.

## 2.4. The Canadian Mortgage Insurance Regulation of 2012

In an attempt to cool the higher end of the market during the recent boom, the Canadian federal government, in June of 2012, passed a law that changed Canada's mortgage insurance system.[27] In particular, the government limited the availability of mortgage insurance to homes with a purchase price of less than 1 million Canadian dollars.[28] In other words, for homes with a minimum purchase price of 1 million Canadian dollars, the law change required the borrower must make at least a 20 percent down payment. Hence, the aim of the regulation was to increase

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[23]"Mortgage insurance: market structure, underwriting cycle and policy implications." *Basel Committee on Banking Supervision Joint Forum.* August 2013.

[24]"U.S. Mortgage Insurer Sector Remains Negative – And The Clock's Ticking." Standard & Poor's, 2012. "Mortgage insurance: market structure, underwriting cycle and policy implications." *Basel Committee on Banking Supervision Joint Forum.* August 2013.

[25]See "F.H.A. Hopes to Avoid a Bailout by Treasury." *New York Times.* November 16, 2012; and "F.H.A. Audit Said to Show Low Reserves." *New York Times.* November 14, 2014.

[26]Option 3 in "Reforming America's Housing Finance Market, A Report to Congress." February 2011. The US Treasury and the US Department of Housing and Urban Development.

[27]The law change in June 2012 was the fourth main instance of housing macroprudential regulation pursued by the Canadian government. For a chronology of events, see "The Fourth Round of Mortgage Tightening, One Year Later." The Quebec Federation of Real Estate Boards; 2013.

[28]This law change also reduced the maximum amortization period from 30 years to 25 years; limited the amount that households can barrow when refinancing to 80 percent (from 85 percent); and limited the maximum gross debt service ratio to 39 percent (down from 44 percent) where the gross debt service ratio is the sum of annual mortgage payments and property taxes over the gross family income. "Harper Government Takes Further Action to Strengthen Canada's Housing Market." Department of Finance Canada. June 21, 2012.

borrower creditworthiness at the higher end price distribution, especially in the more speculative housing markets of Toronto and Vancouver. The law was announced on 21 June 2012 and went into effect on 9 July 2012. Further, anecdotal evidence suggests that the announcement of the law change was largely unexpected by market participants.[29]

## 3. Data

The data source is transaction records at the Multiple Listing Service (MLS) from a large North American Metropolitan area. The data cover all the transactions in this metropolitan area from 2011 to 2013. For each transaction, we observe asking price, sales price, time on the market, transaction date, location, as well as detailed housing characteristics. These transactions can be segmented into four property types: Single-family-houses, condominiums, semi-detached houses, and town houses.

## 4. Results

### 4.1. Differences-and-Differences with Weighted Prices

We exploit each source of variation in a regression framework and combine both the over-time variation (the date the policy was implemented) and the threshold effect (the $1M threshold). First, with the MI policy, there is no natural 'treatment' and 'control' group. Indeed, all sales above the $1M threshold are affected, but the impact is likely less important for sales that are further away from the price threshold. Likewise, while the policy does not directly affect sales below $1M, there may be indirect effects. For example, demand for houses just below the threshold may increase if the MI policy makes houses above the threshold unattractive for buyers who would prefer to make a down payment of less than 20 percent and purchase the corresponding mortgage insurance.

Hence, our econometric strategy is to exploit the price threshold by treating sales near $1M as more likely to be impacted and thus assigning them more importance. To facilitate this approach, we create an indicator for sales above $1M and calculate the sales volume for this group using a quartic weighting kernel that assigns a weight of 1 to a $1M sale and then down-weights higher priced sales. Similarly, we treat sales below the threshold symmetrically and assign lower weights to sales further away from the cut-off. Thus, our quasi-treatment and -control groups are sales above and below the price threshold, where sales closer to the threshold receive higher weights. In implementing this procedure, one must decide the bandwidth to use. As a baseline bandwidth, we choose $100,000 but assess the sensitivity to this choice.

With the groups defined as above, we run a simple differences-in-differences specification.

[29]"High-end mortgage changes seen as return to CMHC's roots." *The Globe and Mail.* June 23, 2012.

Consider the following equation:

$$\ln(v_{gt}) = \delta_t + \alpha \cdot D_g + \gamma \cdot (D_g \cdot T_t) + \epsilon_{gt}$$

where we define

$$D_g = \begin{cases} 1 & \text{if price} > 1\text{M} \\ 0 & \text{otherwise} \end{cases},$$

$$T_t = \begin{cases} 1 & \text{date} > \text{July 9, 2012} \\ 0 & \text{otherwise} \end{cases}$$

$\delta_t$ represents a set of time dummy variables to account for seasonal effects or the post-intervention period; the dependent variable, $\ln(v_{gt})$, is the log of sales volume, calculated using the weights as described above; and the coefficient of interest is $\gamma$, which can be interpreted as a difference-in-difference estimate of the effect of the policy change on sales volume near $1M.

Table 1: The Effect of MI on Volume of Transactions

| | 1M +/− 1000000 | | | 1M +/− 50000 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post × > 1M | -0.098 | -0.20* | -0.19* | -0.10 | -0.17* | -0.14 |
| | (0.21) | (0.061) | (0.059) | (0.22) | (0.072) | (0.073) |
| > 1M | -0.30* | -0.25* | | -0.11 | -0.078 | |
| | (0.15) | (0.053) | | (0.16) | (0.059) | |
| Post | 0.16 | | | 0.20 | | |
| | (0.15) | | | (0.15) | | |
| Year | No | Yes | Yes | No | Yes | Yes |
| Month | No | Yes | No | No | Yes | No |
| Month× >1M | No | No | Yes | No | No | Yes |
| Observations | 70 | 70 | 70 | 70 | 70 | 70 |
| $R^2$ | 0.16 | 0.92 | 0.94 | 0.059 | 0.88 | 0.91 |

Standard errors in parentheses

* $p < 0.05$

Table 1 presents the estimation. The table contains two panels, each pertaining to a different bandwidth of the weighting function. In the first panel, we consider a bandwidth of $100,000. Thus, the sample includes all sales within the interval of $0.9M and $1.1M. Each column of each panel contains several different specifications. In column (1), we approximate the time function ($\delta_t$) with a simple post-policy dummy and include a group and group-time interaction. The coefficient on the interaction, Post $\times$ >1M, is of interest as it captures the effect of the policy change. The results indicate that the policy reduced the sales volume of units above \$1M by about 10 percent relative to units below \$1M, although the effect is very imprecisely estimated and not statistically different form zero at the 10 percent level of significance. In the second column, we allow for unrestricted month and year dummies to approximate the time function to account for business cycle effects and the seasonality in housing markets. This specification suggests that volume falls by about 20 percent. Finally, in column (3), we allow for month-group interactions to allow for different price-seasonality. Again, our results indicate that the policy led to a reduction in the volume for the home sales above the \$1M threshold fell by nearly 20 percent. Next, in columns (4) through (6), we repeat the above analysis using \$50,000 price bins. Generally, the results are similar, but the coefficient on the interaction, representing the effect of the policy, is not significant in column (6) when consider month-group interactions. This latter result is not surprising as the narrower bins yield less precise point estimates. Overall, the findings from this section indicate that the MI policy change led to lower sales volume for home priced above the \$1M threshold.

### 4.2. Estimating the Joint Distribution of Sales and Prices

The analysis above presents suggestive evidence of an impact of the MI policy on the volume of sales around the \$1M threshold. However, while the approach used is vary transparent, there are potential problems with the identification strategy. First, there is no clear way to define a *treatment* and a *control* group as the MI policy may have affected sales volumes at different points along the housing price distribution. Second, the MI policy could have impacted housing prices themselves; thus, categorizing houses by price is potentially problematic. In this section, we provide additional evidence regarding the impact of the MI policy using an approach that addresses both of these drawbacks to our previous estimation scheme. More Specifically, we use a distribution regression approach to estimate the *joint* effect of the MI policy on the distribution of housing prices and sales at different points along the housing price distribution. The distribution regression approach was first developed by Foresi and Peracchi (1995) to estimate the conditional distribution of excess returns. This approach has gained more interest recently, and the properties of this approach have been examined by Leorato et al. (2012), Chernozhukov et al. (2009) and Rothe and Wied (2013) among others.

Our distribution regression approach models the impact of the MI policy on the probability that a house is sold above a certain price. Define $S(p|\mathbf{x})$ as the *survivor* function or the probability that a house is sold above price $p$, conditional on a vector $\mathbf{x}$ that includes, for now, Year $\times$ Month $\times$ district. The distribution regression approach chooses a finite set of cut-offs, $p_1, \ldots, p_J$ and estimates the conditional mean of binary indicators $D_j = 1[p > p_j]$, for $j = 1, \cdots J$. For example, one could specify $S(p|\mathbf{x}) = \Lambda(\mathbf{x}'\beta(p))$ where $\Lambda(\cdot)$ is a known link-function, such as a the logistic. Alternatively, working with grouped data, one can estimate $S(p|\mathbf{x})$ directly as a linear function of $\mathbf{x}$

using OLS. In our baseline empirical work, we choose the latter for simplicity and transparency.[30]

We empirically implement this approach as follows. We calculate the survivor function by constructing a set of variables corresponding to different cut-off points, $p_j$:

$$S(p_j)_{itm} = \frac{1}{N_{itm}} \cdot \sum_i 1\left[p_{itm} \geq p_j\right] \quad \text{for } p_j = p_1, \ldots, p_J \tag{1}$$

where $N_{itm}$ is the number of houses sold in year $t$ and month $m$ in district $i$, and $1\left[\cdot\right]$ is an indicator function that is turned on for houses with $p_{itm}$ greater than the fixed cut-off point $p_j$. We then model this function by running second-step distribution regressions:

$$S(p_j)_{itm} = \beta(p_j)_0 + \delta(p_j)_m + \alpha(p_j)_i + \mu(p_j) \cdot \text{Post}_{itm} + \epsilon(p_j)_{itm} \quad \text{for } p_j = p_1, \ldots, p_J. \tag{2}$$

Month and district fixed effects are captured by $\delta(p_j)_m$ and $\alpha(p_j)_i$, respectively. These effects are both allowed to vary at each $p_j$, providing a considerable amount of flexibility to fit the underling housing price distribution. $\mu(p_j)$ is the coefficient on a dummy variable representing the post-policy period, which, again, varies by $p_j$, and $\epsilon(p_j)_{itm}$ is an error term.

When (2) is estimated with two periods of data, a pre- and post-period, $\mu(p_j)$ captures the before-after estimate of the policy effect. We construct a set of $\mu(p_j)$ estimates at different values of $p_j$ in order to capture before-after estimates of the policy effect along the $p_j$ distribution. Since there is no variation in policy across districts at a given time, we are unable to exploit the difference-in-differences approach used above as, given this policy, there is no natural control group. Hence, our distribution regression approach yields only a before-after estimator, exploiting the fact that our policy variation only has a time dimension.

The before-after estimator obviously confounds policy effects with other common (to district) macro forces that may affect housing sales. However, our key conjecture asserts that the impact of these macro forces on housing sales should be continuous along the housing price distribution. Therefore, our approach is to estimate our distribution regression before-after estimator along different points of the housing price distribution and then examine if these estimates have a discontinuity at the \$1M threshold using regression-discontinuity methods. Evidence of discontinuity lends support to the idea that the MI policy had an impact on the joint distribution of housing sales and prices.

Our distribution regression estimates from (2) provide $\hat{\mu}(p_j)$ for $p_j$, $p_j = p_1, \cdots p_J$, which we model as smooth functions of $p$ and allowing for a discontinuity around $p = \$1M$:

$$\hat{\mu}(p_j) = \gamma_0 + f_l(p_j - \$1M) + D \times f_r(p_j - \$1M) + \gamma_1 \cdot D + \varepsilon(p_j), \tag{3}$$

[30] With grouped data, one may model the dependent variable as $\log S(p|\mathbf{x}) - \log(1 - S(p|\mathbf{x}))$ or the log-odds ratio, which is consistent with a logistic link function. However, if $S(p|\mathbf{x})$ contains zeros for some $\mathbf{x}$, which is the case for us, then this approach does not work directly. Using OLS directly on $S(p|\mathbf{x})$ also avoids having to specify a link function.

where $D = 1$ if $p \geq \$1M$ and 0 otherwise, and the functions $f_l(\cdot)$ and $f_r(\cdot)$ are smooth functions of $p$ that we allow to vary to the left and right of the cut-off. The coefficient of interest is $\gamma_1$, which captures any discontinuities in the before-after estimates from the distribution regressions. This is our estimate of the effect of the MI policy.

In order to further isolate the effect of the MI policy from other potential sources of a discontinuity at $1M, such as psychological biases at this threshold, we also perform the same analysis on two periods of data *before* the implementation of the policy as a 'falsification' test. Finally, we combine our results from the falsification test with our estimates of the policy effect in order to perform a double-difference, regression discontinuity estimator.

### 4.3. Implementation and interpretation

Our data on housing sales contains two measures of prices, the asking price, ($p^A$), and the actual price at which the house is sold, ($p^S$). We implement our approach separately using each price measure. This allows us to analyse different types of behaviour. First, we examine whether there is a response to the MI policy in terms of asking price behaviour. Then, using sales prices, we assess the market response to the policy.

The MI policy took effect in July of 2012.[31] We define the pre- and post-period by taking the first six months in each calendar year. That is, the pre-period consists of January to June in 2012 and the post-policy period consists of those same months in 2013. We define the two periods in this way for two main reasons: (1) due to a large degree of seasonality in housing sales, we want to compare the same calendar months in the pre- and post-period, and (2) we perform a falsification test by using a pre-reform period between 2011 and 2012 when no MI policy changes took place. At this time, we only have data dating back until 2011. Yet we have assessed the sensitivity of our results using the 11 calendar months before and after the policy as the pre- and post-periods; this alternative approach does not change our results substantially.

In order to implement our distribution regressions, we must define cut-off points $p_j$ at which to estimate the distribution regressions. We do so by defining $p_j$ in $5,000 intervals. Later, we assess the sensitivity to different sized intervals. Smaller bins allow more flexibility in estimating the underlying housing price distribution while larger bins allow for more precise estimates. All of our results are robust to reasonable deviations from the $5,000 interval.

When estimating (2), we normalize the coefficients on the district dummy variables, $\delta(p_j)$, so they average to zero and omit June from the set of monthly dummy variables. Thus, the constant, $\beta(p_j)_0$, can be interpreted as the value of the $S(p_j|\mathbf{x})$ function for an 'average' district in June before the MI policy. Therefore, $1 - \widehat{\beta(p_j)}_0$ gives the value of the CDF of the housing price distribution at

[31] The announcement date was June 21, 2012 and the implementation date was July 9, 2012. See 2 for more details.

$p_j$. The $\widehat{\mu(p_j)}$ then gives the difference in the survival function between the pre- and post-period for each $p_j$. Once we recover these estimates, we can also compute the entire PDF of the housing distribution in each period, which we will exploit in a later section.

Our housing data contains information on 4 different housing types. These include detached, semi-detached, row houses, and condominiums. In our baseline empirical work, we focus on detached, single family homes, since these are typically more expensive and we believe to be more generally affected by the MI policy. In a later section, we break down our main results by housing type.

Our key identifying assumption is that the pre- vs post-policy difference in the survivor function will be smooth in prices in the absence of the MI policy. This is a standard regression discontinuity assumption of the continuity of potential outcomes in the running variable – in our case, prices. The continuity assumption may not be plausible if agents are able to manipulate prices (McCrary 2008). We believe it is both plausible and likely that agents can manipulate asking prices, since they have precise control over the posted price. Essentially, this violates the regression discontinuity assumption that assignment near the cut-off is 'as good as random.' What this implies, given that the MI policy was announced and likely known to sellers upon its implementation, is that sellers can sort themselves based on the perceived effect of the policy (Lee and Lemieux 2010). For instance, if the MI policy is perceived to limit the number of available buyers for million dollar homes, once could act strategically by setting an asking price just under $1M to attract more potential buyers. If this is the case, it implies that there should be heaping in the density on one side of the cut-off of the forcing variable. In fact, this insight forms the basis of a test for manipulation developed by McCrary (2008) that tests for discontinuities in the density of the running variable, which we implement below.

On the other hand, we believe it less likely that the seller has *precise* control of the actual sales price. As long as agents only have *imprecise* control over the actual sales price, outcomes around the threshold can still be viewed as 'good as random' as long as there is stochastic error around the final selling price (Lee and Lemieux 2010). An example of this would be the existence of bidding wars for houses, where sellers can choose a price from a set of offers, but do not have control over which prices are offered – and thus cannot perfectly sort on either side of the cut-off.

Manipulation of the running variable is generally viewed as problematic in RD designs, since it implies that economic agents sort based on self-interest. However, in many cases, and, we believe, in our situation, sorting can be viewed as evidence of a policy effect. Since sellers have control over asking prices, evidence of sorting suggest that sellers are responding to economic incentives and this behavioural response is interesting in its own right, even if it does not fit the standard RD framework.

### 4.4. Results - Detached Homes

The first step in our procedure is estimating the distribution regression. We present the results of this exercise graphically in Figure 4 for $p_j = 500,000, \ldots, 1,400,000$ for asking prices. In panel A, we plot the estimated survivor function for the pre- and post-policy period. The post-period survivor, indicated by the solid line, lays everywhere above the pre-period survivor, indicating that the probability of selling a home at any asking price increased over this time or that there was a general improvement in the housing market. In panel B, we plot the difference between the two survivor functions, which is actually our estimates of $\widehat{\mu(p_j)}$. Our key identifying assumption is that in absence of the MI policy, this difference would be a smooth function of asking price. As shown in the figure, the $\widehat{\mu(p_j)}$ generally falls in price, but there is larger fall at the \$1M threshold. This jump forms the basis for our regression discontinuity investigation. Our interpretation of this result is that, while sales grew year-over-year at any asking price, the growth was not uniform across asking prices. In particular, the growth exhibited a discrete jump at the \$1M threshold. For later reference, when we turn to the regression discontinuity estimator, we will focus on a price 'window' around the \$1M cut-off. The largest such window we consider is \$100,000 on either side of the cut-off, and is depicted in Figure 4 by the dashed lines.

Figure 5 displays the same estimates for the selling price. One key difference between these results and the asking price survivor functions in Figure 4 is that the selling price survivor function is much smoother. This is a result of the fact that there is a fair degree of heaping in the asking price distribution. The lack of smoothness in the asking price survivor may be viewed as suggestive evidence that sellers can manipulate or have more precise control over the asking price. Before we begin our RD investigation, it is worth assessing the possibility of manipulation of the running variables, or prices.

Note that once the survivor functions are estimated, we can construct estimates of the conditional density of the housing price distribution. For asking prices, we construct $\widehat{S(p^A_{j-1}|\mathbf{x})} - \widehat{S(P^A_j|\mathbf{x})}$ for each $p^A_j$. We use this estimated density to perform a test of manipulation developed by McCrary (2008). The first step in this test involves plotting an under-smoothed histogram of the estimated density, which we present in Figure 6. Panels A and B of this figure focus on asking prices. Panel A shows the estimated density for 2012, the pre-policy year, and panel B shows the density for 2013. Inspection of these panels suggest that there is substantial heaping at certain price points, and in particular, right before the \$1M threshold. Recall that we aggregate the housing data in pricing intervals that are \$5,000 wide. Thus, the largest spike observed in both 2012 and 2013 occurs in the interval $[\$995,000 - \$1M)$. This suggests that, even *before* the MI policy, agents seem to sort to the left of \$1M, although the this effect appears to be smaller in 2012 compared to 2013, which may suggest that the policy induced more sorting. Panels C and D show the estimated densities for selling prices. These histograms show less evidence of heaping, and no visual evidence of heaping around \$1M.

The second step in McCrary (2008)'s test is to smooth the histogram using a local linear

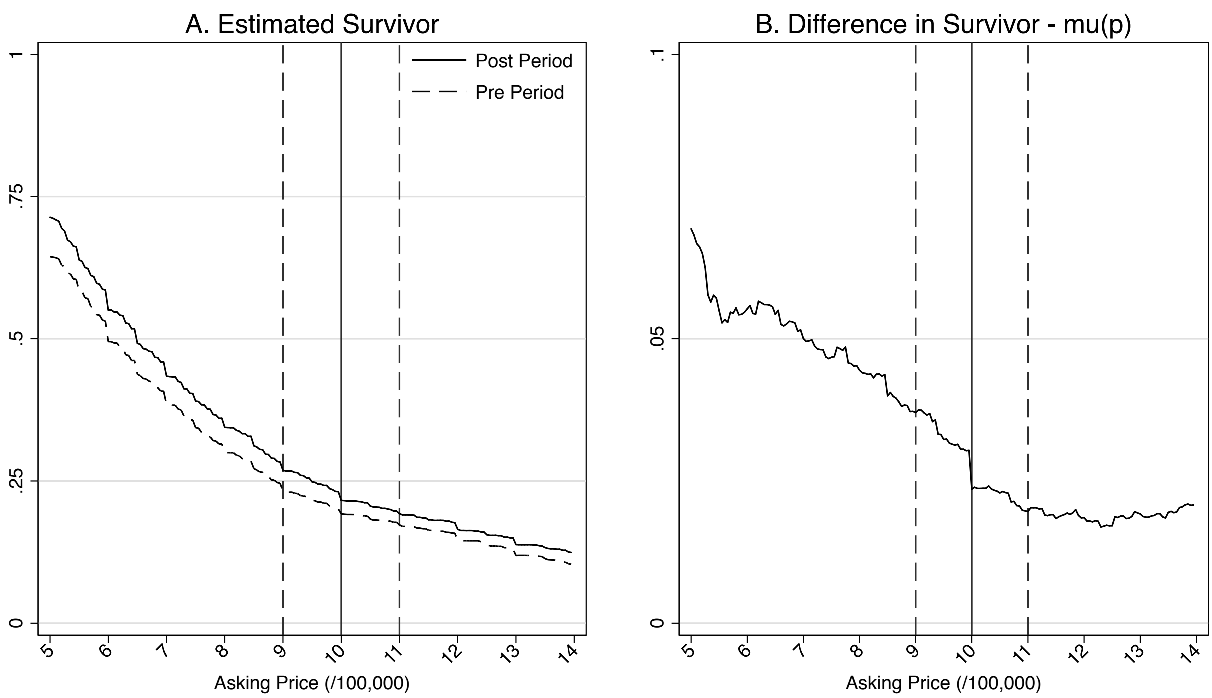


Fig. 4.— Estimates of the Survivor function and $\hat{\mu}(p)$

***Note*:** The sample includes all detached homes. Panel A shows the pre- and post-period survivor function in asking price corresponding to the first six months in 2011 and 2013, respectively. Panel B shows the difference in the pre- and post survivor functions.

regression, separately on each side of the threshold. These smooths are indicated by the dashed lines in the figure, and are constructed by running a weighted regression using the midpoints of the pricing bins to explain the hight of the histogram, and placing more weight on points nearer to \$1M.[32] In both 2012 and 2013, there appears to be a substantial drop in the density as one moves left to right over the \$1M threshold. For selling prices, in panels C and D, there appears to be little or no substantial discontinuity in the smoothed plots. McCrary (2008) purposes estimating

[32] In practice, we run these regressions using the `lpoly` command in STATA. We use half the optimal bandwidth recommended by STATA, a polynomial order of one, and a triangular kernel.

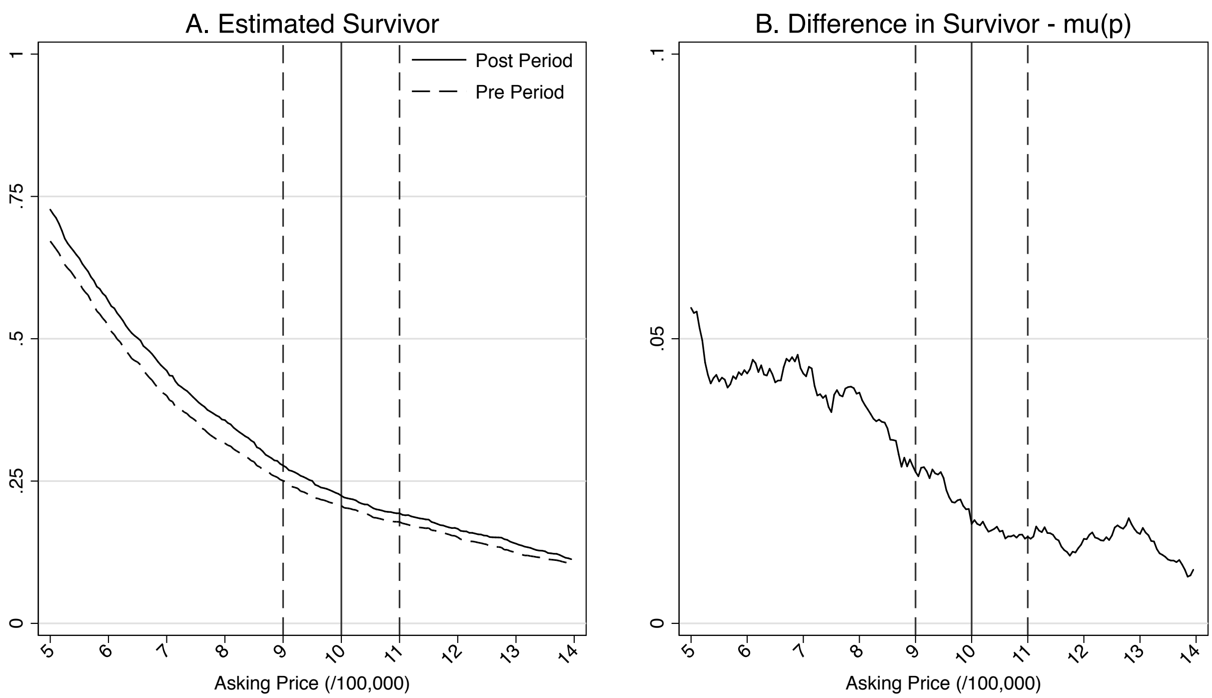


Fig. 5.— Estimates of the Survivor function and $\hat{\mu}(p)$

***Note*:** See the notes for 4. Here, we plot the survivor functions in sales price.

the following parameter to detect manipulation:

$$\theta = \lim_{p^A \downarrow \$1M} \ln f(p^A) - \lim_{p^A \uparrow \$1M} \ln f(p^A) \equiv \ln f^+ - \ln f^-. \tag{4}$$

We present estimates of this parameter in Table 2, which is constructed using the log difference of the smoothed density estimates just to the right and left of \$1M. In the first two columns, we present evidence of the discontinuity in the density of asking prices (column 1) and selling prices (column 2) in 2013 after the MI policy. For asking prices, we estimate a large, negative and significant discontinuity, suggesting that sellers set prices to sort themselves just under \$1M. For selling prices, we find no evidence of sorting. Columns (3) and (4) show the same estimates for 2012. Again, there is evidence of sorting in asking prices to the left of \$1M even before the MI policy was implemented. The estimated size of the discontinuity is smaller than in 2013, however. In column (5), we estimate the difference in the asking price density discontinuity (that is, column (5) =

column (1)-column (2)), and estimate a significant increase in the discontinuity, which is consistent with visual evidence from Figure 6, panels A and B.[33] Thus, this is evidence that the MI policy induced additional sorting in asking prices just under \$1M. We find no evidence of manipulation for selling prices.

The results above are important for the interpretation of our RD estimates that follow. The estimates in Table 2 support the idea that sellers are able to, and actually do, manipulate asking prices. Thus, this violates the standard RD assumption that potential outcomes are continuous in the forcing variable. However, we believe this sorting provides interesting information on economic behaviour in its own right, since it demonstrates that sellers may have reacted to the MI policy by manipulating asking prices. Indeed, anecdotal evidence after the imposition of the policy suggested that the policy created a 'hot market' for homes just under \$1M. However, evidence of manipulation suggests variation around \$1M in asking prices can not be thought of generating quasi-local random variation. On the other hand, sellers have less control over sales prices and we fail to detect any evidence of manipulation in Table 2, suggesting that we can view outcomes around the \$1M selling price threshold as being as 'good as randomly assigned' Lee and Lemieux (2010).

Table 2: Density Discontinuity Estimates of Forcing Variables

| | 2013 | | 2012 | | Difference | |
|---|---|---|---|---|---|---|
| | (1) Asking | (2) Selling | (3) Asking | (4) Selling | (5) Asking | (6) Selling |
| $\ln f^{+} - \ln f^{-}$ | -3.83* | -0.26 | -2.66* | 0.46 | 1.17* | -0.20 |
| | (0.50) | (0.19) | (0.38) | (0.24) | (0.54) | (0.26) |

Standard errors in parentheses

$^{*}$ $p < 0.05$

Our regression discontinuity estimates focus on housing prices near the \$1M cut-off. To do this, we restrict our attention to a range of housing prices near \$1M by selecting various bandwidth windows. We also need to select functional forms for the smooth functions $f_l(\cdot)$ and $f_r(\cdot)$ in equation 3. We deal with these choices as pragmatically as possible by estimating a variety of specifications with different windows and smoothing functions. Table 3 contains the results of this exercise. This table contains two panels; the first panel shows the results when the asking price is the running variable, while the second panel is for the sales price. Each column of the table shows the results for a separate bandwidth window, while each row shows the estimated $\hat{\gamma}_1$ from separate regressions where the $f_l(\cdot)$ and $f_r(\cdot)$ functions are approximated by low-order polynomials, where the order is indicated in the left-most column.

In the first four columns of Table 3 contains the results for the asking price. In the first entry,

[33] The standard errors in column 5 and 6 are calculated assuming independence of the 2013 and 2012 estimates.

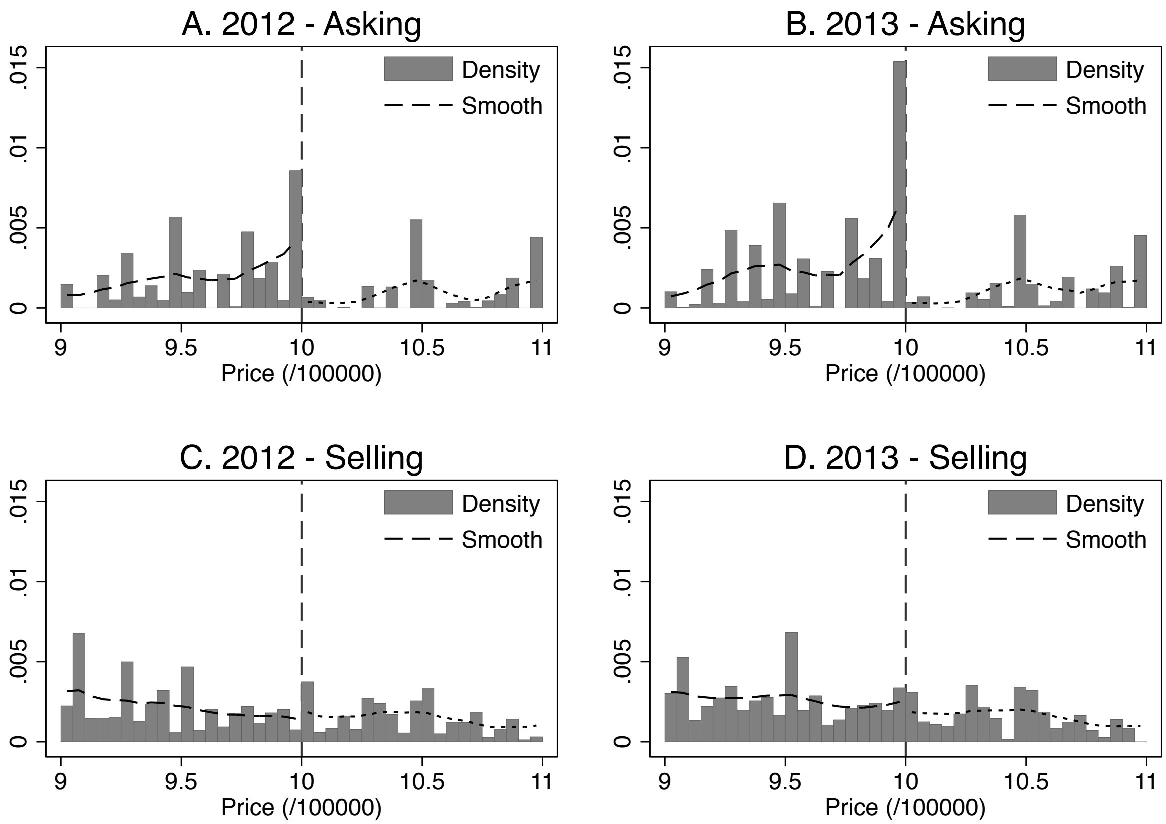


Fig. 6.— Density of Forcing Forcing Variables – Asking and Selling Prices

***Note*:** The sample includes all detached homes. Each panel shows the estimated density for the indicated year within $100,000 of $1M. The dashed lines represent local linear smooths of the underlying density, estimated separately to the right and left of the cut-off.

column (1), the bandwidth window contains 5 pricing bins, where the bins are at $5,000 intervals. Therefore, the estimated coefficient is from a local linear regression for houses within $25,000 of $1M. The estimated coefficient is $-0.64$, indicating that the probability of selling a house with an asking price just to the right of $1M fell by 2/3rds of a percentage point.[34] This estimate is fairly robust to different bandwidth windows and orders of the polynomial functions. We consider four different bandwidth windows. In columns 1, 2, and 4 we use bandwidths of 5, 10, and 20 price bins, respectively, while in column 3 use use the cross-validation method to choose the bandwidth.[35]

[34]In order to ease the presentation of the results, we multiply the $\widehat{\mu(p_j)}$s by 100 so as to present them in percentage point terms.

[35]We implement the cross validation method by first restricting the largest possible bandwidth window to be 20

In the last row of the table, we present the 'optimal order' of the smoothing functions, chosen by fixing the bandwidth window (looking down one column) and finding the specification that minimizes Akaike's Information Criterion (Lee and Lemieux 2010).[36] Among the optimal order estimates chosen in this way, the estimated $\hat{\gamma}_1$ ranges from $-0.62$ to $-0.67$.

The last four columns of Table 3 shows the estimates when the sales price is the running variable. Again, the estimates of $\hat{\gamma}_1$ are fairly robust to bandwidth windows and polynomial order. For the optimal order estimates, the estimated $\hat{\gamma}_1$ ranges from $-0.18$ to $-0.24$, indicating that sales just to the right of the \$1M cut-off fell by about 1/5th of a percentage point. These estimates are statistically significant at conventional significance levels. Figure 7 presents the results from Table 3, columns (4) and (8), graphically. As can be seen by inspection of this figure, the forth order polynomial functions fit the data quite well, and show a distinct discontinuity at the policy threshold for both asking and selling prices.

Table 3: Regression Discontinuity Estimates: Policy Period

| | Asking Price | | | | Sales Price | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | bw(5) | bw(10) | bw(13) | bw(20) | bw(5) | bw(10) | bw(10) | bw(20) |
| One | -0.64* | -0.62* | -0.57* | -0.44* | -0.20* | -0.15* | -0.15* | -0.21* |
| | (0.027) | (0.018) | (0.035) | (0.042) | (0.044) | (0.048) | (0.048) | (0.041) |
| Two | | -0.67* | -0.69* | -0.63* | | -0.24* | -0.24* | -0.058 |
| | | (0.020) | (0.026) | (0.035) | | (0.071) | (0.071) | (0.063) |
| Three | | -0.66* | -0.64* | -0.74* | | -0.18* | -0.18* | -0.24* |
| | | (0.027) | (0.038) | (0.034) | | (0.074) | (0.074) | (0.051) |
| Four | | | -0.69* | -0.62* | | | | -0.30* |
| | | | (0.032) | (0.037) | | | | (0.068) |
| Optimal Order | 1 | 2 | 4 | 4 | 1 | 3 | 3 | 3 |

Standard errors in parentheses

* $p < 0.05$

One might be concerned that the discontinuity we estimate is really just picking threshold effects in pricing that are caused, for example, by psychological biases around the \$1M threshold rather than the MI policy itself. For instance, the asking price distribution is quite lumpy, with

(corresponding to \$100,000 widow), and then choosing the window which minimizes the CV function. This procedure is implemented using the `bwselect` STATA code provided by ?.

[36]In order to avoid over fitting the models, we do not estimate polynomials that in an estimation of equation (3) would use up more than 60% of the degrees of freedom. This is why we only estimate a local linear regression (order one) in column (1) when the bandwidth window is only 5.

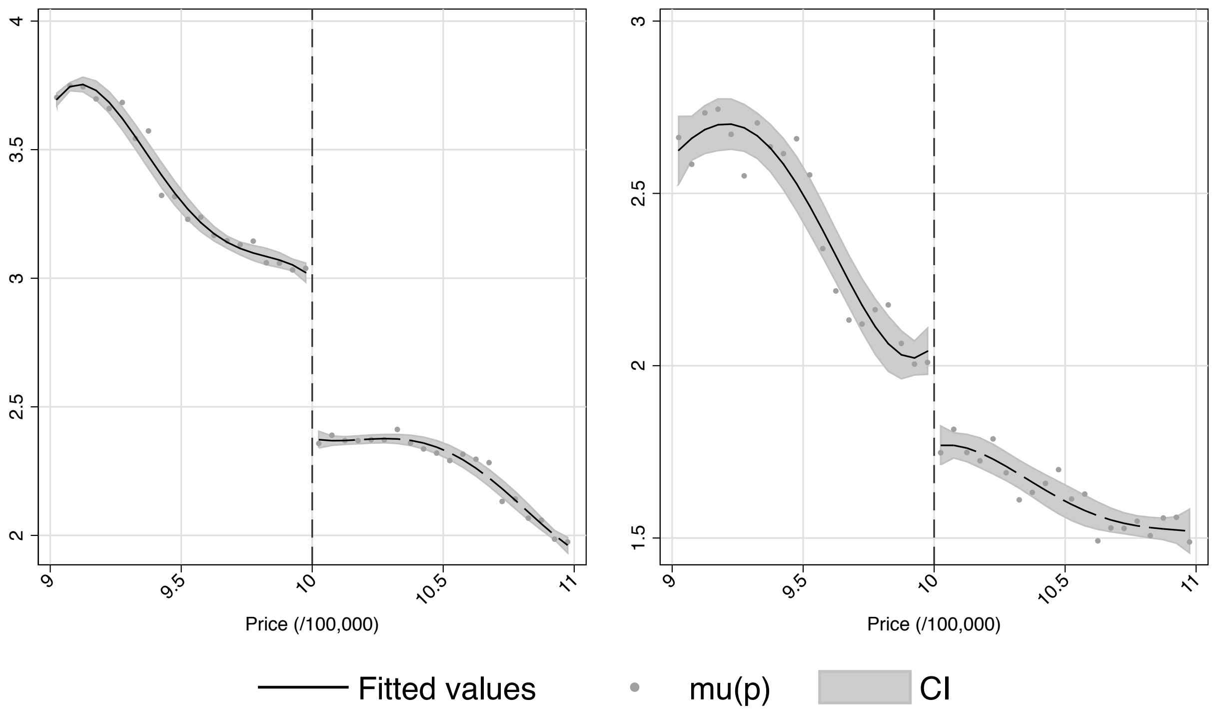


Fig. 7.— Estimates of the Survivor function and $\hat{\mu}(p)$

***Note*:** The sample includes all ...

asking prices often heaping on certain values. We assess this idea in two ways. First, we conduct the same analysis for data covering the years 2012-2011 before the MI policy. In this pre-policy period, one would not expect to find any discontinuity in the $\widehat{\mu}(p_j)_{pre}$ estimates, under our identifying assumptions. Second, we examine a set of alternative, arbitrary cut-offs that should not have been affected by the MI policy.

Table 4 shows the results for the pre-policy period. The table is formatted in the same way as Table 3. For the asking prices, there are a few specifications that show a significant discontinuity in the pre-program period, but the sign of the discontinuity is opposite to what we would expect. In the optimal order specifications, the results are mostly small and insignificant. For sales prices, the results generally indicate no significant effects. Figure 8 shows the results from column (4) and (8) graphically, were visual evidence of any discontinuity in the pre-policy period is very weak. Finally, in Table 5, we combine the results from our estimates in the pre- and post-policy period.

Specifically, we take the difference in $\hat{\mu}(p)_{post} - \hat{\mu}(p)_{pre}$ as the dependent variable in estimations of equation (3). This specification is designed to difference away any permanent threshold effect at \$1M, that may not be caused by the MI policy. The results are very similar to Table 3, indicating that our results are robust to permanent threshold effects at \$1M.

Finally, we investigate whether we are just picking up arbitrary threshold effects at at key price points in the data. To investigate this phenomenon, we create 'false' cut-off points at \$25,000 intervals from \$700,000 to \$900,000 and estimate our regression discontinuity estimator using these as price points as cut-offs. These price intervals are far enough away from \$1M that we do not expect the MI policy to impact them, nor do we expect that these cut-offs would produce the same patterns as documented above for the \$1M threshold. Table **??** displays these results, where the last row shows our estimates with the \$1M cut-off for reference. Each entry in the table is from a separate regression. The left most column shows the chosen cut-off price. The bandwidth window is chosen using the data driven CV procedure outlined above, and the order of the polynomial something function is chosen optimally by the AIC procedure outlined above. For most of the estimates, the result are statistically insignificant. However, there are some economically large and statistically significant estimates, particularly with asking prices. One such estimate is at the \$850,000 threshold for asking prices. However, for the most part these results are supportive of the notion that the estimates documented in Tables 3 to 5, and in the last row of this table, are isolating the an effect caused by the MI policy.

Table 4: Regression Discontinuity Estimates: Pre Policy Period

| | Asking Price | | | | Sales Price | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | bw(5) | bw(10) | bw(10) | bw(20) | bw(5) | bw(10) | bw(10) | bw(20) |
| One | 0.071* | 0.12* | 0.12* | 0.071 | 0.067 | 0.084 | 0.084 | 0.12* |
| | (0.029) | (0.030) | (0.030) | (0.037) | (0.062) | (0.057) | (0.057) | (0.041) |
| Two | | 0.073* | 0.073* | 0.075* | | 0.083 | 0.083 | 0.036 |
| | | (0.033) | (0.033) | (0.035) | | (0.051) | (0.051) | (0.077) |
| Three | | -0.0054 | -0.0054 | 0.14* | | 0.011 | 0.011 | 0.12* |
| | | (0.019) | (0.019) | (0.051) | | (0.046) | (0.046) | (0.057) |
| Four | | | | 0.032 | | | | 0.048 |
| | | | | (0.038) | | | | (0.045) |
| Optimal Order | 1 | 3 | 3 | 4 | 1 | 3 | 3 | 4 |

Standard errors in parentheses

* $p < 0.05$

Table 5: Regression Discontinuity Estimates: Double Difference

| | Asking Price | | | | Sales Price | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) bw(5) | (2) bw(10) | (3) bw(17) | (4) bw(20) | (5) bw(5) | (6) bw(10) | (7) bw(10) | (8) bw(20) |
| One | -0.71* | -0.74* | -0.53* | -0.51* | -0.26* | -0.24* | -0.24* | -0.33* |
| | (0.013) | (0.022) | (0.054) | (0.053) | (0.063) | (0.076) | (0.076) | (0.069) |
| Two | | -0.74* | -0.81* | -0.71* | | -0.32* | -0.32* | -0.092 |
| | | (0.028) | (0.043) | (0.040) | | (0.062) | (0.062) | (0.11) |
| Three | | -0.66* | -0.78* | -0.89* | | -0.19* | -0.19* | -0.35* |
| | | (0.028) | (0.066) | (0.078) | | (0.096) | (0.096) | (0.063) |
| Four | | | -0.62* | -0.66* | | | | -0.35* |
| | | | (0.047) | (0.044) | | | | (0.078) |
| Optimal Order | 1 | 3 | 4 | 4 | 1 | 3 | 3 | 3 |

Standard errors in parentheses

* $p < 0.05$

## 4.5. Other Results

### *4.5.1. Sales Spread*

Our distribution regression approach allows us to investigate other features of the joint distribution of house sales and prices. Here we investigate the impact of the MI policy on the likelihood a home is sold above asking.

We empirically implement this approach as follows. We calculate a rescaled survivor function by constructing a set of variables corresponding to different cut-off points, $p_j$:

$$RS(p_j)_{itm} = \frac{1}{N_{itm}} \cdot \sum_i 1\left[p^S_{itm} \geq p^A_{itm} \text{ and } p^A_{itm} \geq p_j\right] \quad \text{for } p_j = p_1, \ldots, p_J \tag{5}$$

where $N_{itm}$ is the number of houses sold in year $t$ and month $m$ in district $i$, and $1[\cdot]$ is an indicator function that is turned on for houses with $p_{itm}$ greater than the fixed cut-off point $p_j$. We then model this function by running second-step distribution regressions:

$$RS(p_j)_{itm} = \beta'(p_j)_0 + \delta'(p_j)_m + \alpha'(p_j)_i + \mu'(p_j) \cdot \text{Post}_{itm} + \epsilon'(p_j)_{itm} \quad \text{for } j = 1, \ldots, J. \tag{6}$$

Month and district fixed effects are captured by $\delta'(p_j)_m$ and $\alpha'(p_j)_i$, respectively. $\mu'(p_j)$ measures any shift in the joint distribution over time. These shifts will combine possible shifts in both the conditional and marginal distribution. Analysing (6) estimates the joint distribution of housing sales and sold over asking. Equation (2) gives the marginal distribution of housing sales. We have

Table 6: Regression Discontinuity Estimates: Alternative Cut-offs

| | Policy | | Pre-Policy | | Difference | |
|---|---|---|---|---|---|---|
| | (1) Asking | (2) Selling | (3) Asking | (4) Selling | (5) Asking | (6) Selling |
| 700000 | -0.094* | -0.087 | -0.094 | 0.15* | 0.025 | -0.23 |
| | (0.041) | (0.099) | (0.096) | (0.067) | (0.11) | (0.13) |
| 725000 | 0.010 | -0.17 | 0.083 | 0.15 | -0.089 | -0.48* |
| | (0.051) | (0.14) | (0.14) | (0.14) | (0.22) | (0.14) |
| 750000 | -0.090 | -0.21 | -0.31* | 0.033 | 0.22 | -0.44 |
| | (0.080) | (0.18) | (0.061) | (0.11) | (0.12) | (0.32) |
| 775000 | -0.071 | 0.042 | -0.082 | 0.029 | -0.078 | -0.033 |
| | (0.11) | (0.089) | (0.049) | (0.12) | (0.13) | (0.18) |
| 800000 | 0.084 | 0.089 | -0.40* | 0.21* | 0.35* | -0.16 |
| | (0.13) | (0.050) | (0.059) | (0.11) | (0.089) | (0.16) |
| 825000 | -0.090 | -0.093 | 0.13* | 0.038 | -0.053 | -0.045 |
| | (0.063) | (0.069) | (0.043) | (0.10) | (0.11) | (0.11) |
| 850000 | -0.32* | -0.14 | -0.10* | -0.018 | -0.24* | -0.022 |
| | (0.050) | (0.084) | (0.029) | (0.11) | (0.049) | (0.12) |
| 875000 | -0.049 | -0.070 | -0.098 | 0.024 | 0.017 | -0.22 |
| | (0.071) | (0.11) | (0.096) | (0.082) | (0.15) | (0.16) |
| 900000 | 0.0082 | -0.14 | -0.56* | 0.27 | 0.57* | -0.51* |
| | (0.034) | (0.082) | (0.017) | (0.15) | (0.045) | (0.25) |
| 1000000 | -0.69* | -0.18* | -0.0054 | 0.011 | -0.62* | -0.19* |
| | (0.032) | (0.074) | (0.019) | (0.046) | (0.047) | (0.096) |

Standard errors in parentheses

* $p < 0.05$

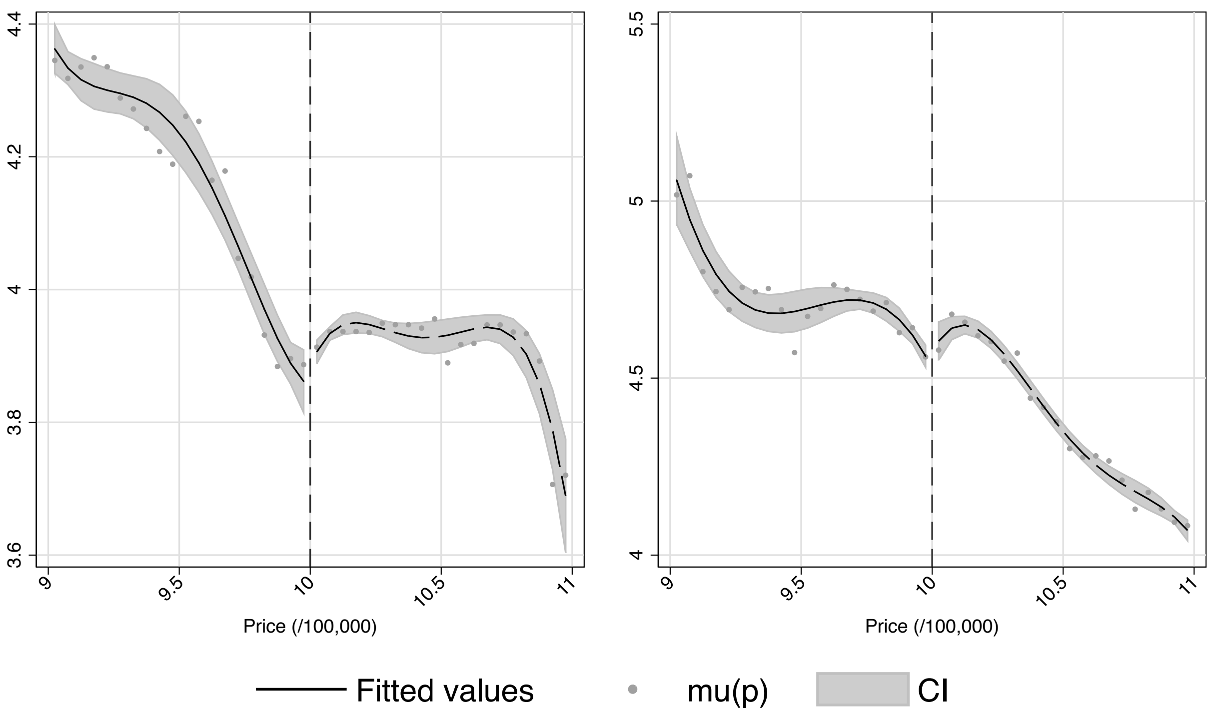


Fig. 8.— Estimates of the Survivor function and $\hat{\mu}(p)$

***Note*:** The sample includes all ...

to combine them to get at the conditional question 'is sales over asking impacted by the MI polcy?'. We can use Bayes' rule to back out the conditional distribution. Consider the constant in (6):

$$\begin{aligned}
\beta'(p) &= P(p^S_{itm} \geq p^A_{itm}, p^A_{itm} \geq p|\mathbf{x}) \\
&= P(p^S_{itm} \geq p^A_{itm}|p^A_{itm} \geq p|\mathbf{x}) \cdot P(p^A_{itm} \geq p|\mathbf{x}) \\
&= P(p^S_{itm} \geq p^A_{itm}|p^A_{itm} \geq p|\mathbf{x}) \cdot \beta(p),
\end{aligned}$$

where $\beta(p_j)$ is estimated in equation (2). Thus, we can combine the constants in (2) and (6) to get a conditional estimate for the base period. Similarly, using the constants, $\mu(p), \mu'(p)$, we can obtain conditional estimates for the second period.

$$\begin{aligned} P(p^S_{itm} \geq p^A_{itm} | p^A_{itm} \geq p_j)_{\text{Post}} - P(p^S_{itm} \geq p^A_{itm} | p^A_{itm} \geq p_j)_{\text{Pre}} \\ = \frac{\beta'(p_j) + \mu'(p_j)}{\beta(p_j) + \mu(p_j)} - \frac{\beta'(p_j)}{\beta(p_j)} \\ = \Gamma(p_j) \text{ for } p_j = p_1, \ldots, p_J. \end{aligned}$$

Therefore estimating equations (2) and (6) allows us to construct $\hat{\Gamma}(p_j)$ for $p_j$, $j = 1, \cdots, J$, which we model as as smooth functions of $p$ and allowing for a discontinuity around $p = \$1M$:

$$\hat{\Gamma}(p_j) = \gamma'_0 + f'_l(p_j - \$1M) + D \times f'_r(p_j - \$1M) + \gamma'_1 \cdot D + \varepsilon'(p_j), \tag{7}$$

where $D = 1$ if $p \geq \$1M$ and 0 otherwise, and the functions $f_l(\cdot)$ and $f_r(\cdot)$ are smooth functions of $p$ that we allow to vary to the left and right of the cut-off. The coefficient of interest is $\gamma'_1$, which captures any discontinuities in the before-after estimates from the conditional distribution sales price over asking price, conditional on asking for at last $p_j$.

The results of this exercise are presented in Panel A of Table **??**. For brevity, we only present a table of these estimates for the post policy period, where we compare 2013 to 2012. However, we present a set of results graphically for the post policy, pre policy, and their difference together in Figure 9.

Table 7: Regression Discontinuity Estimates: Pre vs Post Period

| | Spread | | | | Duration | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | bw(5) | bw(10) | bw(15) | bw(20) | bw(5) | bw(10) | bw(10) | bw(20) |
| One | -1.42* | -1.51* | -1.43* | -1.21* | 1.15* | 1.01* | 1.01* | 0.91* |
| | (0.077) | (0.074) | (0.091) | (0.10) | (0.14) | (0.13) | (0.13) | (0.099) |
| Two | | -1.43* | -1.55* | -1.64* | | 1.12* | 1.12* | 1.03* |
| | | (0.062) | (0.074) | (0.098) | | (0.13) | (0.13) | (0.14) |
| Three | | -1.45* | -1.39* | -1.47* | | 1.43* | 1.43* | 1.15* |
| | | (0.081) | (0.087) | (0.059) | | (0.13) | (0.13) | (0.10) |
| Four | | | -1.52* | -1.39* | | | | 1.20* |
| | | | (0.078) | (0.077) | | | | (0.12) |
| Optimal Order | 1 | 2 | 4 | 3 | 1 | 3 | 3 | 4 |

Standard errors in parentheses

* $p < 0.05$

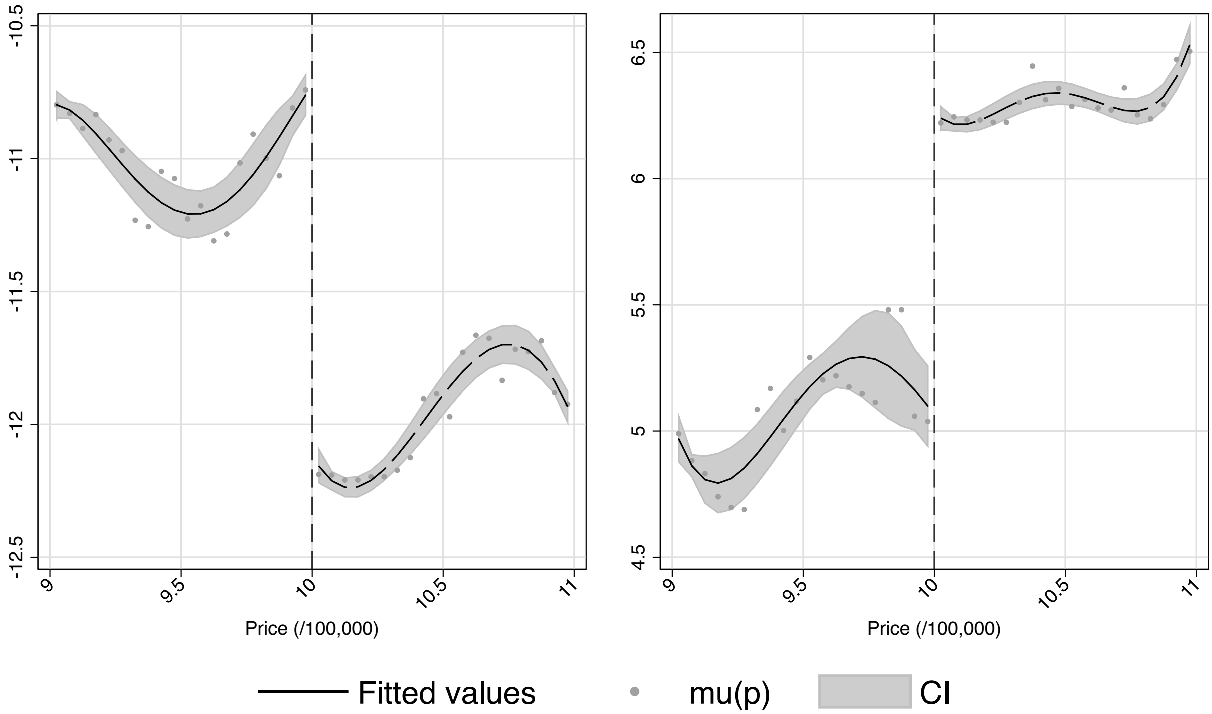


Fig. 9.— Estimates of the Survivor function and $\hat{\mu}(p)$

***Note*:** The sample includes all ...

### *4.5.2. Duration*

We also analyse the effect of the MI policy on the duration a house is on the market, conditional on the asking price. We do this in the same way as sales spread above, but construct the dependent variable as:

$$RS(p_j)_{itm} = \frac{1}{N_{itm}} \cdot \sum_i 1\left[dur_{itm} \geq 14 \text{ and } p^A_{itm} \geq p_j\right] \quad \text{for } p_j = p_1, \ldots, p_J \tag{8}$$

The duration results are presented in Panel B of Table 7. These results are presented in the second panel of Figure 9

### 4.6. Results for other Housing types

In this section, we present results for alternative housing types. We focus separately on condos and all housing types together. In order to preserve space, we present estimates of $\hat{\gamma}_1$ for different bandwidths, but only report the optimal order of the polynomial as chosen by Akaike's Information Criterion.

Table 8 contains the results. Each entry is from a separate regression of equation (3). Each column shows the results for the indicated dependent variable. Each row shows the results for a different bandwidth window around the cut-off.

Table 8: Regression Discontinuity Estimates:

| | Condos - Policy | | Condos - Pre-Policy | | All - Policy | | All - Pre-Policy | |
|---|---|---|---|---|---|---|---|---|
| | (1) Asking | (2) Selling | (3) Asking | (4) Selling | (5) Asking | (6) Selling | (7) Asking | (8) Selling |
| bw(5) | -0.12* | -0.057* | -0.0031 | -0.0098 | -0.31* | -0.099* | 0.0061 | 0.030 |
| | (0.015) | (0.018) | (0.014) | (0.013) | (0.018) | (0.031) | (0.0075) | (0.016) |
| bw(10) | -0.13* | -0.026 | -0.019 | -0.0026 | -0.33* | -0.13* | -0.024* | 0.048* |
| | (0.0091) | (0.030) | (0.021) | (0.0095) | (0.014) | (0.033) | (0.010) | (0.012) |
| bw(15) | -0.13* | -0.026 | 0.020 | -0.010 | -0.33* | -0.10* | -0.027 | 0.053* |
| | (0.0098) | (0.027) | (0.012) | (0.013) | (0.012) | (0.039) | (0.016) | (0.011) |
| bw(20) | -0.14* | -0.076* | 0.023* | -0.0034 | -0.33* | -0.13* | -0.0032 | 0.045* |
| | (0.011) | (0.017) | (0.011) | (0.011) | (0.013) | (0.034) | (0.016) | (0.010) |

Standard errors in parentheses

* $p < 0.05$

## 5. Conclusion

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