# The Effect of LTV-Based Risk Weights on House Prices: Evidence from an Israeli Macroprudential Policy*

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**Abstract**

This paper studies the link between bank mortgage lending and house prices by asking whether macroprudential policies that impose higher risk weights on high LTV mortgages can slow house price growth. For housing units likely to be purchased using mortgages subject to the higher risk weights, we estimate that prices are lower by about 2 to 3 percent than they would have been without the policy. We find that the policy has larger effects in more expensive areas of the country and, in particular, in lower quality neighborhoods within these more expensive areas. Combining our results with previous estimates of the effect of this policy on mortgage interest rates, we derive an estimate for the semi-elasticity of house prices with respect to mortgage rates that is consistent with the upper range of estimates reported in the literature.

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# 1. Introduction

House prices and residential mortgages play central roles in credit cycles generally, and were especially important in the credit cycle that sparked the 2007-2009 global financial crisis. As a result, many of the macroprudential policies (MPPs) imposed in the wake of the crisis have specifically focused on banks' provision of mortgage credit. Among such policies, limits on loan-to-value (LTV) ratios and policies that assign higher risk weights to high-LTV mortgages are the two most common (Alam et al., 2019).

Macroprudential policies that reduce banks' origination of high LTV mortgages serve two purposes. First, and most directly, because residential mortgages make up a significant fraction of bank assets, policies that discourage banks from originating these riskier mortgages can reduce bank losses during economic downturns (Krznar and Morsink, 2014; Lim et al., 2013). A second potential benefit of these MPPs is the possibility that they could limit the build up of financial imbalances by moderating the growth in house prices. A large literature has found that that an easing of mortgage credit leads to stronger house price growth (e.g. Mian and Sufi (2009); Favara and Imbs (2015); Di Maggio and Kermani (2017)). We might therefore expect that MPPs that *limit* mortgage credit could reduce price growth.

This question has also received considerable attention in the literature, but with mixed conclusions. Some studies do find that these MPPs do slow house price growth (e.g. Igan and Kang (2011); Galati and Moessner (2013); Akinci and Olmstead-Rumsey (2018), while others fail to find any such effects (e.g. Wong et al. (2011); Kuttner and Shim (2016); Cerutti et al. (2017)). For the most part, these papers rely on evidence from cross-country panel regressions to identify the effect of MPPs that are implemented in different countries at different times. The identification challenges facing these studies are significant. MPPs are frequently implemented in combination with other policies and often around times of large macro-economic events. In addition, implementation of these policies is certainly highly endogenous.

The goal of this paper is to obtain a more cleanly identified measure of the effect of these policies on house prices by studying an MPP imposed by the Bank of Israel in 2010. In response to strong housing boom, the Bank of Israel passed a series of MPPs between 2010 and 2014. The first of these, implemented in October 2010, increased the risk-weight factor for mortgages with LTV ratios of at least 60 percent, but crucially for our identification scheme, only for mortgages

with values larger than NIS 800,000 (approximately USD 200,000). Previous work has shown that this policy increased interest rates on high-LTV mortgages and caused borrowers to reduce their leverage and shift their purchases towards units that were smaller, farther from the country center, and located in less desirable neighborhoods (Tzur-Ilan, 2016).

Because this MPP only applied to mortgages over a certain size, we can measure its effect on house prices by comparing price growth in different segments of the Israeli housing market. In particular, our approach is motivated by the observation that only for housing units above a certain purchase price would a mortgage with a given LTV ratio be larger than the NIS 800,000 threshold. As an illustrative example, assume that buyers always use an LTV ratio of 75 percent. Then only for units with transaction prices above NIS 1.06M (which equals 800,000 divided by 0.75), would the mortgage be larger than the NIS 800,000 threshold. For less expensive units, a mortgage with a 75 percent LTV ratio would be smaller than NIS 800,000 and therefore not affected by the policy. We could therefore use a difference-in-differences approach to compare units with prices above and below this NIS 1.06M threshold, before and after the policy was implemented. This situation, in which the mortgage market is dominated by a single LTV ratio, would be similar to Adelino et al. (2012). In that paper, the authors study the effect on house prices in the United States caused by the ability of the Government Sponsored Enterprises (Fannie Mae and Freddie Mac) to purchase mortgages below a certain size (the "conforming loan limit"). In that context, the authors argue that one can safely assume that the marginal buyer will use an 80 percent LTV loan.

The setting we study is somewhat more complicated, as the Israeli housing market is not dominated by a single LTV ratio. We therefore construct a more general treatment measure that uses the observed distribution of LTV ratios to capture the likelihood that a particular unit would be purchased using a mortgage affected by the policy, given the transaction price. Using this modified treatment effect, we then perform the difference-in-differences estimation described above.[1]

Under a range of model specifications, we find that after the policy was passed, units that would be purchased using a mortgage large enough to be affected by the policy sell for about 2 to 3 percent less than units that are not affected by the policy. While this effect is relatively small compared to overall rate of house price appreciation during this period, these findings nevertheless represent new

[1]We also consider the simpler cases where we assume that all borrowers are using LTV ratios of 60, 70 and 75 percent, all common LTV ratios in this market.

evidence that these MPPs do have some effect on house prices.

Our methodology also allows us to measure the different effects of the MPP on house prices in different parts of Israel. We find that the policy has larger effects in more expensive areas, such as Tel Aviv (the business capital of Israel), and the central region of the country. In particular, we find that the policy has the largest effect in the lower quality neighborhoods within these more expensive areas. These results suggest that such policies may have the largest impact on households struggling to afford housing in the locations that offer access to the best employment opportunities.

Finally, we use our results to produce a new estimate for the semi-elasticity of house prices with respect to interest rates by combining our estimates on house price growth with earlier research (Tzur-Ilan, 2016) studying the effect of this policy on interest rates. Using estimates from several different model specifications, we find a semi-elasticity of house prices with respect to interest rates in the range of 6-10, consistent with the upper range of estimates reported in the literature.[2] This analysis provides new quantitative evidence for the impact of credit markets on house prices and, in particular, supports the interpretation that these MPPs affect house prices through their effects on mortgage interest rates.

Our paper contributes to a large literature that has tried to measure the effects of LTV limits on house prices. For example, Duca et al. (2010) find that a 10-percentage-point reduction in the strict LTV limit for first-time buyers is associated with a 10 percentage point decline in house price appreciation rate. Crowe et al. (2013) find that a 10 percentage point decrease in the maximum LTV limit for all borrowers is associated with a 13 percentage point decline in house price. Igan and Kang (2011) find that house prices appreciation rates decrease by a monthly rate of 0.5 percent against a historical monthly change of 0.4 percent in response to a strict LTV limit. Akinci and Olmstead-Rumsey (2018) find that strict LTV caps reduce quarterly house price growth by nearly 1.5 percentage points and LTV-based risk weights by 1 percentage point. Krznar and Morsink (2014) find that strict LTV and LTV-based risk weights limit appear to have an effect on house price growth of 2.5 percentage points. A recent paper by Armstrong et al. (2019) also find an effect of approximately 2.5 percentage points and Alam et al. (2019) find a 1 percentage point effect. As noted above, other studies including Kuttner and Shim (2016), fail to find any effect of MPP tools on housing prices.

[2]For example, Adelino et al. (2012) estimate a semi-elasticities in the range from 1 to 9 and Pinto et al. (2018) estimate a value of 3.4. Kuttner (2014) provides a survey of this literature and Anenberg and Kung (2017) offer a discussion of why empirically estimated values may be lower than those predicted by economic models.

Unlike most of the above papers, which generally examine strict LTV limits, this paper studies a policy that allows high-LTV mortgages but forces banks to hold additional capital against them. According to the literature, these LTV-based risk weights limits are quite popular: Jacome and Mitra (2015) study six economies (Brazil, Hong Kong SAR, Korea, Malaysia, Poland, and Romania) and show that they all use, in addition to strict LTV limits, higher risk weights on high-LTV mortgages. Lim et al. (2011) examine 39 countries and show that many of those countries use risk weights based on LTV ratios. Finally, according to Akinci and Olmstead-Rumsey (2018), the higher risk weights on high-LTV mortgages is the second most popular MPP tool among 57 advanced and emerging economies (where a strict LTV limit is the most common one). Among these papers, only Akinci and Olmstead-Rumsey (2018) report separate results for the effect of these LTV-based risk weights on house prices. Therefore, in addition to using a stronger identification strategy, our paper adds to a very small literature looking specifically at MPPs that impose higher risk weights on high-LTV mortgages.

Finally, our paper contributes to a nascent literature that uses variation at the micro level to draw conclusions about the effects of credit limits on house prices. Anenberg et al. (2017) construct "loan frontiers" that characterize largest obtainable mortgages by different types of borrowers. Using variation in these frontiers across time and metropolitan areas of the United States, the authors show that borrowing constraints have significant effects on house prices. In somewhat similar work using Irish data, Kelly et al. (2018) construct a measure of credit availability at the borrower level and find that a ten per cent increase in available credit leads to a 1.5 percent increase in property values. In both cases, the authors must infer information about credit limits from the distribution of originated mortgages, while our setting allows us to observe the actual policy changes directly.

The rest of the paper proceeds as follows. Section 2 provides background on the housing market and housing finance in Israel. Section 3 describes the data used in our analysis. Section 4 analyzes the effects of this MPP on the distribution of observed LTV ratios and describes how we identify which transactions were more likely to be affected by the policy. Finally, section 5 presents our results, including a series of robustness checks and placebo tests. Section 6 concludes the paper.

# 2. Background: The Housing Market and Housing Finance in Israel

We begin with a few facts about the Israeli housing and credit markets:
1. Israeli households are not very indebted. The ratio of total household debt to Gross Domestic Product in Israel averaged 42 percent between 2006–2013, low compared to other developed countries (e.g. 92 percent in the UK and 89 percent in the US). The average Israeli household debt-to-income ratio is lower than that of Germany, which is the lowest among major countries, and far below that of the United States. Further, LTV ratios on mortgages are low compared to other developed countries, averaging about 53 percent.[3]
2. Mortgages in Israel are recourse loans, so that in the event of nonpayment, the lender can pursue other assets of the borrower beyond the house itself.
3. The Israeli government's role in housing finance is more limited than in most other countries, with the government providing almost no upfront subsidies to first-time or other buyers.[4] On the tax side, mortgage interest payments are not tax deductible.
4. Over 93 percent of all mortgage loans in Israel are made by banks, with the rest coming mostly from insurance companies. The mortgage industry is highly concentrated with the three largest banks holding over 80 percent of all mortgages. Nevertheless, other rules of the banking sector make the industry function in a highly competitive manner. There is no secondary market for Israeli mortgages.
5. The home ownership ratio in Israel is close to 70 percent, somewhat higher than the 63 percent average home ownership rates among OECD countries.
6. Housing supply in Israel is particularly inelastic as a result of government ownership and control of most undeveloped land,[5] a centralized planning process, and extensive bureaucracy at all stages of construction.[6] According to a recent report, it takes an average of 11 years from the time the government decides to develop a plot of land until a building permit is finally issued.[7] As a result, positive demand shocks would be expected to have large price effects.

The Israeli financial system weathered the global financial crisis relatively well. Several factors helped mitigate the effect of the crisis, including the timing

[3]For more information, see Cerutti et al. (2017).
[4]Government mortgages account for less than 2 percent of the market.
[5]The state controls 93 percent of the land in Israel, and a government agency, the Israel Land Administration (ILA), manages and allocates this land.
[6]See Bank of Israel Annually report (2012).
[7]See Bank of Israel Annually report (2015).

of the crisis, which followed five years of rapid growth. Certain features of the economy and the financial system also helped reduce the impact of the crisis, including a tightly regulated banking system, a conservative mortgage market and the virtual absence of complex financial assets. Another contributing factor was the conservative conduct of households, who maintained a high rate of saving and avoided over-leveraging in general and with respect to mortgages in particular. These factors helped prevent a real estate bubble from forming in Israel during the years prior to the crisis. Israel's financial institutions, including the banks, showed resilience relative to the intensity of the crisis, and they remained stable with none collapsing.[8]

Notwithstanding its favorable state, Israel is a small open economy, which means that its domestic financial conditions are strongly affected by global interest rates. In 2008, when global rates declined after the burst of the financial crisis, Israeli domestic interest rates declined as well and housing prices in Israel began to rise. In other countries, the rise in house prices during this period led to a sharp increase in the rate of residential construction. In Israel, however, though the rate of construction increased, the increase was not sufficient to meet the rise in demand for housing, and prices continued to rise. From 2008 to 2016, nominal house prices in Israel climbed by 128 percent, and real house prices grew by 95 percent (see Figure 1 for international comparison). Over the same period, rents increased 52 percent in nominal terms and approximately 34 percent in real terms. Meanwhile, the volume of mortgages increased by 95 percent, raising concerns among policy makers about the stability of the commercial banks holding these loans.

In response to these rising concerns, Israel, like many other advanced economies, implemented a series of MPP tools between 2010 and 2014. These policies were intended to limit household borrowing and ultimately reduce the risk that losses in the financial sector might spill over into the real economy. Figure 2 depicts the timing of the implementation of the various MPPs, together with the rate of change of Israeli house prices during this period.[9] While the graph shows a slowing rate of increase in house prices around the time the policies were implemented, the challenge remains to isolate the impact of the MPPs on house prices from other macroeconomic events that occurred around the same time.

[8]See Bank of Israel Annually report (2018).

[9]For a summary of the various MPP tools, see Tzur-Ilan (2016).

### 2.1. *The LTV Limit*

The analysis in this paper focuses on the first of these MPPs, whereby, in October 2010, the Israeli Supervisor of Banks issued a directive requiring banks to increase capital provisions for residential mortgages with high LTV ratios. As a result of this directive, required capital provisions for mortgages with variable interest-rate portions of 25 percent or more and LTV ratios greater than 60 percent rose from the existing 35–75 percent (depending on the loan characteristics) to 100 percent. Importantly for our analysis, the guidelines applied only to mortgages larger than NIS 800,000 (approximately USD 200,000), which represented roughly 30 percent of outstanding mortgages in 2010 (see Tzur-Ilan (2016)). Since these LTV-based risk weights forced banks to hold more capital against these loans, borrowers wanting to take a loan with an LTV of greater than 60 percent now faced higher interest rates. As a result, this policy led to a shift in the distribution of LTV ratios among originated loans, with fewer mortgages originated above the 60-percent threshold following the implantation of the policy (figure 3). Unlike subsequent attempts to limit LTV ratios, the market appears not to have anticipated this initial October 2010 policy.[10]

## 3. Data

The main dataset used in this paper is administrative data on the universe of household purchases of residential properties, obtained from the Israel Tax Authority via the Bank of Israel. These data are used by the Israeli Central Bureau of Statistics (CBS) to construct the official Prices of Dwellings Index. Transactions that do not meet criteria set by the CBS to construct the index were dropped.[11] For each transacted property, we have the date and price of the transaction and information on the unit including the size (in square meters), number of rooms, location, and year of construction.[12] Our analysis focuses on the period from January 2010 to April 2011 (the red area in Figure 2), a relatively narrow time

[10] According to Google Trends, the number of searches for the word "equity" in Israel increased by 50 percent during the period between the imposition of the second and third LTV limits (Oct. 2010 - Dec. 2012).

[11] The most important of these criteria are the following: (1) the number of rooms is between 1.5 and 5.0 (the share of properties outside this range is negligible); (2) the ratio between property area and the number of rooms is within a certain range; (3) the price per square meter is within a certain range (determined separately for each locality).

[12] The dataset does not contain information on seller and buyer characteristics. Also, since the housing market in Israel is ethnically segregated, the analysis in this paper excludes Arab localities.

window centered on the October 2010 limit. This time frame is relatively free of external shocks and excludes events such as the introduction of additional variable interest rate limits in May 2011 and the outbreak of social protests in July 2011. Our sample includes 90,332 observations, 48,985 observations before the higher risk weights were imposed and 41,347 observations afterwards. While the main analysis in this paper uses only the housing transaction dataset, we also merge the housing transaction dataset to loan-level mortgage data from the Bank of Israel. The mortgage dataset covers all housing loans issued by the seven commercial banks in Israel and includes information on mortgage contracts term (interest rate, LTV, bank, duration, value and location of acquired property etc.) and borrower characteristics (age and income).[13] We are able to merge approximately one-third of all the observations from the loan-level dataset. Although this merge rate is fairly low, analysis in Tzur-Ilan (2016), suggests that the merging procedure does not cause any bias (e.g., the observations in the merged dataset, mortgages and housing unit level data are similar in character to those in the complete dataset of all mortgage observations). The merged data and its construction are explained in detail in Tzur-Ilan (2016). In summary, the housing transaction dataset was merged with approximately 27 thousand observations from the mortgages dataset (16,100 observations before the LTV limit and 11,224 observations after).

### *3.1. Summary Statistics*

As background for our analysis, we first divide the dataset into two periods: before and after the imposition of the restriction in October 2010. Table 1 shows descriptive statistics from the housing transaction dataset, divided into periods before and after the imposition of the LTV-based risk weights. The average transaction value rises from 1,016 thousand NIS before the limit to 1,098 after the limit, an increase of 8 percent, reflecting the rapid increase in home prices during that period. Similarly, the average price per square meter climbs from NIS 12 thousand before to 12.9 after. The average size of the units increases slightly from 84.1 square meters to 84.9. Houses have, on average, 3.6 rooms, both before and after the policy. Following the policy, buyers tend to buy houses that are, on average, newer and farther from Tel Aviv, the business capital of Israel.[14] The last variable we show in Table 1 is the neighborhood socioeconomic index, published

[13] Together, these seven lenders account for roughly 95 percent of all mortgage loans in Israel.

[14] Observations with distance from Tel Aviv above 80km were dropped from our analysis in order to focus only on the most populated areas in Israel. Around 70 percent of the Israeli population lives within this 80km radius from Tel Aviv.

by the Israeli Central Bureau of Statistics, which combines 16 different variables, including demography, education, employment, income, and standard of living, into a single measure. All neighborhoods in Israel are then classified into one of twenty clusters, with 1 being the lowest socioeconomic status and 20 being the highest. The final row of Table 1 shows that homes sold after the LTV limit was introduced tended to be in neighborhoods of slightly lower quality than homes sold before the policy.

# 4. The Effectiveness of the LTV Limit

The LTV-based risk weights required banks to set aside more capital against high-LTV loans, increasing the banks' overall costs associated with originating these loans. Tzur-Ilan (2016) finds that a portion of these costs were passed onto borrowers: after the LTV-based Risk Weights limit, the interest rate paid by borrowers with LTV ratios just above 60 percent were 0.31–0.36 percentage points higher than the interest rate charged to an identical borrower just below the 60 percent threshold. The higher interest rate for more highly leveraged loans incentivized borrowers to lower their leverage and to choose loans with LTV ratios of less than 60 percent. Figure 3 shows the change in the LTV distribution before and after the limit. After the LTV limit, we see a high density of loans clustered just below the 60 percent LTV cutoff.[15] In addition, Tzur-Ilan (2016) finds that the policy shifted purchases towards units that were smaller, farther from country center, and in less desirable neighborhoods.

## *4.1. Identifying Affected Transactions*

The first challenge in identifying the effect of this policy on house prices is determining which transactions were affected by the policy. Because the applicability of the policy is determined by the size of the mortgage rather than the price of the unit, assessing whether a particular sale is affected by the policy depends on choice of LTV ratio. For example, consider a unit sold for NIS 1.07 million, which equals NIS 800,000 divided by 0.75. If the purchaser uses a mortgage with an LTV ratio of at least 75 percent, that mortgage would be larger than NIS 800,000 and therefore subject to the higher risk weights. However, if the purchase were financed using a mortgage with an LTV ratio less than 75 percent, that smaller

[15]The change in the LTV distribution after the limit is statistically significant according to the Kolmogorov–Smirnov test.

mortgage would be smaller than NIS 800,000 and not affected by the policy. As we consider transactions at higher prices, a wider the range of LTV ratios would place the purchase mortgage above the NIS 800,000 threshold. For example, a transaction of NIS 1.33 million (NIS 800,000 divided by 0.6) would require a mortgage affected by the policy as long as the borrower uses a loan with an LTV ratio of at least 60 percent. Our conclusion from this analysis is that transactions with higher prices are more likely to be affected by the policy. We can observe these differences directly by examining the change in LTV ratios induced by the policy change. Figure 4 shows the distribution of LTV ratios before and after the policy on mortgages used to purchase housing units of different values. In panel A, we show LTV ratios for sales between NIS 900,000 and NIS 1 million, a price range low enough that units are very unlikely to be affected by the policy. For these transactions, there is little change in the LTV ratios after the policy. The shift in LTV ratios becomes larger for moderately higher priced transactions (Panels B and C). Finally, for transactions above NIS 1.4 million (Panel D), where all mortgages with LTV ratios above 60 percent would be subject to the policy, we see a notable shift in the distribution of LTV ratios and an increase in the density of LTV ratios around 60 percent.

## *4.2. Construction of the Treatment Effect*

The observation that higher priced transactions are more likely to be affected by the policy motivates the construction of our treatment effect. In particular, to quantify the degree to which the policy affects transactions of various prices - our treatment variable - we first assume that the "desired" distribution of LTV ratios matches the observed distribution in the year before the policy was enacted. Then, using this distribution of LTV ratios (shown in figure 5), we compute the treatment effect as the probability that the unit would be purchased with a mortgage that had both a value above NIS 800,000 and an LTV ratio above 60 percent. Formally, for a transaction at price $p$:

$$Treat(p) = \sum_{LTV=0.6}^{1} I(p * LTV > NIS800,000) * f(LTV), \tag{1}$$

where $I$ is in indicator function, $p * LTV > NIS800,000$ is the condition that a mortgage with LTV ratio "$LTV$" would be larger than NIS 800,000 for price $p$, and $f(LTV)$ is the fraction of units purchased in the previous year using a mortgage with that LTV ratio. At the lowest extreme, transactions under NIS 800,000 receive

a treatment of zero because the NIS 800,000 mortgage size could only be achieved with an LTV ratio over 100 percent, which we do not observe in the data (i.e. p* LTV is never above NIS 800,000). At the upper extreme, for all transaction prices above NIS 1.33 million (which equals NIS 800,000 divided by 0.6), any mortgage above the policy's 60 percent threshold would be larger than NIS 800,000 (i.e. p*LTV is always above NIS 800,000). For these sales, the treatment is simply:

$$Treat(p) = \sum_{LTV=0.6}^{1} * f(LTV), \tag{2}$$

i.e. the probability of having a LTV ratio over 60 percent, which we estimate to be 0.45. The resulting treatment function is illustrated in Figure 6. As described above, transactions below NIS 800,000 never involve mortgages larger than NIS 800,000 and therefore receive zero treatment. As we consider higher prices, lower LTV ratios will produce mortgages above the NIS 800,000 threshold. For example, at prices above NIS 1.14 million (which equals NIS 800,000 divided by 0.7), any sale with a LTV ratio of at least 70 percent will involve a mortgage over NIS 800,000. Because 70 percent is the most common LTV ratio observed in the data, purchases above this price are significantly more likely to use an affected mortgage and therefore receive a notably higher treatment effect. The treatment reaches its maximum value of 0.45 when the price exceeds NIS 1.33 million (NIS 800,000 divided by 0.6), at which point any mortgage with a LTV ratio above 60 percent will be above NIS 800,000, as described above.

# 5. Results

## *5.1. Empirical Methodology*

Our empirical methodology uses a difference-in-differences approach, comparing the differences in prices between treated and untreated properties, before and after the policy was implemented. Using our micro-level data on the characteristics of the housing units, we also control for observable features of the units that affect the purchase prices, such as location and size. We then estimate the following hedonic regression:

$$\ln(PPSM_{it}) = \alpha + \hat{\beta} X_i + Area_i + \Gamma * \theta_t + \delta * Treat(p) + \sigma * Treat(p) * \theta_t + \epsilon_{it} \tag{3}$$

where $ln(PPSM_{it})$ is log price per square meter for unit $i$ sold at time $t$. The control variables included in the vector of property characteristics $X$: number of rooms (in groups: 1.5-2, 2.5-3, 3.5-4, 4.5-5), and log age of the building. The treatment, $Treat(p)$, is a function of the transaction price as described above, $\theta_t$ is a time dummy equal to zero before the policy was implemented (01/2010-10/20101) and one afterwards (11/2010-05/2011), and $\epsilon_{it}$ is a well-behaved error term clustered at the locality statistical area level. Our primary interest is in the coefficient $\sigma$, which captures the difference in pricing for treated and untreated properties after the LTV limit is imposed, relative to the difference before the policy.

The identifying assumption underlying this approach is that more and less treated properties would have appreciated at the same rate in the absence of the policy change. While we can't test this assumption directly, we can offer evidence that price trends for the two groups were similar in the period leading up to the policy's enactment. In particular, because the degree of treatment depends on the price of the unit, we can compare price growth in more and less expensive market segments in the months before the policy went into effect. In figure 7, the left panel shows the evolution of average PPSM for 3-room, 4-room and 5-room units, while the right panel shows the hedonic price index for groups of cities classified as inexpensive, moderate and expensive. In neither graph do we see significant differences in the rate of appreciation between the different market segments that would explain any differences in price growth that we identify after the policy was enacted.

## *5.2. Main Results*

The first column of Table 2 presents the results from the specification described above. As expected, units with larger number of rooms have a lower price per square meter. Age enters positively, likely reflected that older properties were built in more desirable locations. Units selling in the second half of the period sold at higher prices, consistent with the general rise in house prices during this period. Finally, our main finding from this exercise, as shown in bold, is that after the policy was implemented, treated units sold for 4 percent less than untreated units.

One natural concern with this approach is that transactions that are more likely to be treated may be located in areas with different rates of house price appreciation. Such a pattern could lead us to improperly attribute some of these differences in prices to the policy change. To account for this, we introduce

geographic fixed effects interacted with our "after" dummy variable.[16] We include these interacted fixed effects in all of the remaining specifications.

Column (2) of table 2 presents our preferred baseline specification, which uses the model described in equation 3, plus the geographic fixed effects. With the addition of the fixed effects, the coefficients on the indicators for units with more rooms become smaller, suggesting a selection effect whereby local price levels affected the size of the units constructed in each area. The coefficient on unit age flips from positive to negative, consistent with our interpretation that older units appeared more valuable because they were built in more desirable areas. Finally, we turn to the question of what effect these fixed effects have on our main results. As shown in bold, including the geographic fixed effects yields a slightly smaller estimate for our main parameter of interest, which falls from 4.0 percent to 3.1 percent, but remains statistically significant. This supports our earlier conjecture that some of the measured effect in the original specification could reflect treated properties being in locations with slightly slower price growth. However, the quantitative effect of this issue appears small. Our main result, then, is that after the policy was implemented, units that would be purchased using a mortgage large enough to affected by the policy sold for 3.1 percent less than units not affected by the policy.

As an alternative to our baseline specification, column (3) of table 2, shows results where we use price instead of PPSM as our dependent variable. In this specification, we find that the policy reduced prices by 2.4 percent, slightly smaller but statistically indistinguishable from our baseline estimate.

One potential concern with these specifications is that the price of the units is both the outcome variable and also the input used to compute the treatment effect. Consider, for example, some source of measurement error that increases the price on units selling after the policy's enactment. Affected transactions will have higher reported prices (or PPSM) and we will also conclude that they are more likely to be treated, creating a positive bias in our estimate. In order to address this concern, we estimate an alternative treatment variable that does not directly use the observed price of the transactions. Instead, we compute a predicted price ($\hat{p}$) for each unit based on its hedonic characteristics and use this predicted price to compute a treatment effect $Treat(\hat{p})$ (see the appendix for more details). Results using the treatment effect calculated from these predicted prices are shown in columns (4) and (5) of table 2 and are very similar to our baseline estimates. To

[16] We include fixed effects for each "sub-district," which is a large geographical unit defined by the Israeli CBS. Israel proper is divided into 6 districts and 15 sub-districts.

calculate the standard error for this estimate, we bootstrap the entire process using a stratified re-sampling procedure that randomly samples transactions with replacement in a way that ensures that the number of properties sampled stays the same.

## *5.3. Robustness Checks*

An additional concern about our identification is that our treated transactions differ from untreated transactions for reasons that are unconnected to the macro-prudential policy being studied. We perform several robustness checks to address this issue.

### *5.3.1. Placebo Tests*

The most precise statement of our identification problem is the concern that any nonlinear function of price that resembles our treatment effect might also predict a lower price per square meter, independent of the policy. To address this concern, we run a series of placebo tests where we construct placebo treatment effects using the probability of having a mortgage over NIS 700,000 or over NIS 900,000 (as opposed to over NIS 800,000), values that have no particular significance in this context. The results from this exercise are illustrated in Figure 8, which plots our main coefficient of interest from each placebo test.[17] In each of the placebo cases, we estimate insignificant effects from the alternative treatment. In other words, after including appropriate controls, transactions that are more likely to have a mortgage with a value above some random size after the policy is implemented do not have significant differences in prices per square meter. However, transactions that are more likely to have a mortgage with a value above the NIS 800,000 threshold specified in the policy do have significantly lower prices per square meter. We regard this as relatively strong evidence that the policy itself is responsible for the lower prices, rather than simply the functional form of our treatment effect.

### *5.3.2. Isolating Particular LTV Ratios*

In our baseline specification, we consider a treatment measure that accounts for the range of LTV ratios used in housing purchases. As a robustness check we

[17]The figure also shows results from placebo tests when treatment is probability of having a mortgage over NIS 750,000 or over NIS 850,000.

consider simpler treatment effects that each assume buyers are using a specific LTV ratio. In particular, we consider LTV ratios of 0.6, 0.7 and 0.75, which imply that the policy affects transactions above NIS 800,000/0.6, 800,000/0.7 and 800,000/0.75 respectively. Using the hedonic regression, we again control for housing characteristics and examine the change in home prices after accounting for differences in unit quality. Then, we will focus on the difference between houses above the cutoff to below these cutoff, before and after the policy. This approach uses an estimating equation of the form:

$$\ln(PPSM_{it}) = \alpha + \hat{\beta} X_i + \Gamma * \theta_t + \delta * AboveThreshold(p) + \sigma * AboveThreshold(p) * theta_t + \epsilon_{it} \quad (4)$$

The results are shown in Table 3. Columns (1) and (2) show results, where we assume that borrowers use mortgages with LTV ratios of 60 percent and the policy therefore only affects housing units above the cutoff-price of NIS 1.33M. Columns (3) and (4) assume a LTV ratio of 0.7 and columns (5) and (6) an LTV ratio of 0.75. Within each pair of columns, the first column uses the full sample of 90,000 transactions, while the second column restricts the sample to a smaller and more homogenous subsample of units with four or five rooms.

We focus our discussion on the results assuming an LTV ratio of 60 percent (columns (1) and (2)) and note the results in the other columns look similar. As in our main specification, most of the coefficients have the expected sign. Units with more rooms have a lower price per square meter, reflecting the lower marginal value of additional rooms. Newer units are slightly more valuable and units with a higher overall price also have higher prices when measured per square meter. Units selling after the policy were passed trade at higher prices, reflecting the strong house price growth in the Israeli housing market during this period.

Most importantly, as shown in bold, the interaction between having a price above the NIS 1.33M threshold and selling after the MPP was enacted enters with a negative sign. Looking at results from the full sample (column (1)), we conclude that after the policy was passed, units that would be purchased using a mortgage large enough to affected by the policy sell for 3-4 percent less than units that are not affected by the policy. When we restrict our analysis to the more homogeneous sample of four and five room units (column (2)), the size of the effect shrinks somewhat to roughly 1 ½ percent. This reduction in the estimate suggests that either (1) part of this effect reflects unobserved heterogeneity in the larger sample that is correlated with our treatment or (2) the effect of the policy is smaller for the subsample of larger units. In either case, our estimate remains

statistically significant and provides corroborating evidence that this MPP has a small negative effect on house prices.[18]

We also examine a broader time period around the LTV limit, from the beginning of 2009 until the end of 2012. In these specifications (not shown), the interaction between having a price above the threshold and selling after the MPP was enacted again enters with a negative sign, and the estimated effect for each of the three different thresholds is even larger than for the more narrow time period.

### *5.4. Differential Effects by Neighborhood Quality*

We next test whether these price effects are larger in particular types of neighborhoods. As described in Section 3, the Israeli Central Bureau of Statistics publishes a socioeconomic index of neighborhoods quality for each neighborhood in Israel. This index combines 16 different variables, including education, employment, income, family size and standard of living into a single index. Neighborhoods are then classified into one of twenty clusters, 1 being the lowest socioeconomic status and 20 being the highest. The median quality is 10. For our analysis, we divide neighborhoods into two groups: low-quality areas, those neighborhoods that are graded from 1 to 10, and high-quality areas, neighborhoods that are graded from 11 to 20. We repeat our main estimation separately on these two groups of neighborhoods. As shown in Table 4, we find that the effects of higher risk weights on house prices is notably larger in low-quality neighborhoods. One possible explanation is that households who buy homes in these neighborhoods are more credit constrained and therefore more sensitive to the higher interest rates caused by the policy.

We next examine these results more carefully by considering the effects by neighborhood quality for different parts of Israel. For this purpose, the CBS divide the country into six regions: the cities of Jerusalem, Tel Aviv and Haifa, and for locations outside of these cities, the northern, central and southern areas of the county. Within each of the six regions, we again consider high and low socioeconomic neighborhoods. As shown in Table 5, the effects of the MPP are statistically insignificant in the northern and southern regions of Israel, where housing is less expensive. Conversely, the effect is largest in Tel Aviv and the central region, the most expensive areas of the country. Further, within each of the more expensive regions, the effects are notably larger in the low quality neighborhoods

[18]Using a single LTV ratio closely mirrors the approach of Adelino et al. (2012).When we use the exact same specification as in the Adelino et al. (2012) the results are little changed.

within the region. In other words, the effects of the policy are largest in the lower quality neighborhoods of the more expensive areas of the country. These may be places where relatively poorer households stretch their income to afford housing close to good employment opportunities. According to our results, such households are the most impacted by the government's macroprudential policies.

### *5.5. Direct observation of high-LTV mortgages*

In all the specifications so far, we constructed a treatment measure using the transaction price to estimate the probability that a buyer would use a mortgage affected by the policy, but deliberately ignored whether the buyer actually does use such a mortgage to purchase the unit. In this section, we instead identify the treatment status of each property using information about the mortgage itself. While we would not rely on these estimates as an accurate measurement of the effects of the MPP, these results nevertheless provides useful intuition about the identification challenges and selection effects that may be operative in our setting.

In order to observe information about the mortgages used in these transactions, we merge the housing transaction dataset to loan-level data from the Bank of Israel, which includes information on all housing loans issued by the seven commercial banks in Israel (as explained in Section 3). We are able to merge one-third of all the observations from the loan-level dataset (around 30,000 observations). Earlier work (Tzur-Ilan, 2016) suggests that the set of transactions we can successfully merge is representative of the larger sample.

With this merged dataset, we can directly identify buyers using mortgages affected by the policy, i.e. loans above NIS 800 thousand and with LTV ratios above 60 percent. Using this measure as our treatment effect, we rerun our main regression. While these results reflect a more direct application of policy, they are of course subject to selection concerns due to the fact that borrowers endogenously select their LTV ratio and loan amount. In addition, borrowers who use smaller mortgages in order to avoid loans subject to the policy are still being *affected* by the policy, even though they will be categorized as *un*treated in this specification.

As shown in Table 6, this exercise would suggest that the LTV limit caused a decrease in house price of 6.9-7.5 percent, notably larger than our baseline results using only the housing transactions data.

It is unsurprising that we find larger effects of the policy when we identify affected purchases as those that actually use mortgages subject to the higher risk weights. One reason is that our main treatment effect is likely an imprecise

measure of which units are actually treated. This will create an attenuation bias, resulting in estimated effects that are lower than the true effect. In addition, we would expect that the selection effects in the more direct measurement described in this section would result in artificially lager estimates. For example, if more credit constrained buyers are more reliant on high-LTV loans and also more sensitive to interest rates, then we would expect to see larger price effects from purchases where the buyers chose to use high-LTV loans despite the higher interest rate on such loans. The estimates in our main specification do not suffer from such a selection effect.

### *5.6. Semi-Elasticity with respect to Interest Rates*

The mechanism underlying our results is that banks charge higher interest rates on these high-LTV loans because of the additional capital they are required to hold against them. Earlier research (Tzur-Ilan, 2016) has found that interest rates on mortgages affected by this policy were higher by 0.31-0.36 percentage points. When we combine these estimates of the interest rate effect with our baseline estimate of the effect on house prices, we produce an estimate for the semi-elasticity of house prices with respect to interest rates in the range of 6-10, consistent with the upper range of the results reported in the literature (e.g. Adelino et al. (2012), who estimate values between 1 and 9). Although it is generally not possible to compare the quantitative impact of various MPPs that differ significantly in their particularities, this computation lets us place our numerical results in the context of a broader literature that has tried to measure the effect of mortgage financing conditions on house prices. We find that our results are quite consistent with this literature, supporting our interpretation that this MPP largely affected house prices through its effects on mortgage interest rates.

## 6. Conclusion

In recent years, many countries have implemented MPPs aimed at limiting LTV ratios on residential mortgages. While there exists a substantial literature studying the effects of these policies on house prices, we argue that this literature is constrained by significant identification challenges. In this paper, we take advantage of a policy implemented by the Bank of Israel in 2010 that we believe offers a rare opportunity to more credibly demonstrate the ability of such policies to affect house prices. Using our approach, we present robust evidence that this

policy had a moderate effect on house prices during the boom.

The ability to perform similar analyses of MPPs implemented in other countries is of course limited by the details of how those policies are constructed. In most cases, limits on LTV ratios or higher capital requirements for high-LTV mortgages are imposed on an entire market, leaving no cross-sectional variation that could be used for identification. There are, however, several exceptions where additional study might be possible. In Canada, for example, a 2012 law restricting access to mortgage insurance effectively eliminated the availability of loans with LTV ratios above 80 percent, but only for purchases over 1 million Canadian dollars.[19] In another example, changes made by the government of Hong Kong in 2009 and 2010 to LTV limits on "luxury" properties above a certain price may also present additional opportunities to better measure the effects of these limits on house prices.[20] Other countries may impose macroprudential policies in ways that create other types of variation that could be used for identification. Given the important role of MPPs in stabilizing the financial system and the prevalence of these policies, we think such exploration would be a fruitful avenue for additional research.

In addition to identifying the overall effect of MPPs on house prices, our paper sheds light on which areas may be more affected by these policies. We find that the policy we examined had its largest effect on house prices in lower quality neighborhoods within the more expensive areas of the country. Because these neighborhoods likely contain relatively poorer families living in high-cost areas, they are places where concerns about housing affordability may be the most pronounced. The welfare implications of these findings are complicated. On the one hand, lower house prices in hard-to-afford areas sounds like a positive development. On the other hand, these lower prices likely reflect a particularly sharp reduction in demand from the families who want to live in these areas. This suggests that these policies may fall particularly hard on households already struggling to meet their housing needs. A better understanding of these issues strikes us as another important area for future research.

[19] This policy is the focus of work by Han et al. (2017), who look at volumes of sales and listings around the 1 million dollar threshold, but don't reach conclusions about the total effect on prices.

[20] Wong et al. (2011) study the effects of these policies using time series analysis but don't take advantage of the cross-sectional variation they create

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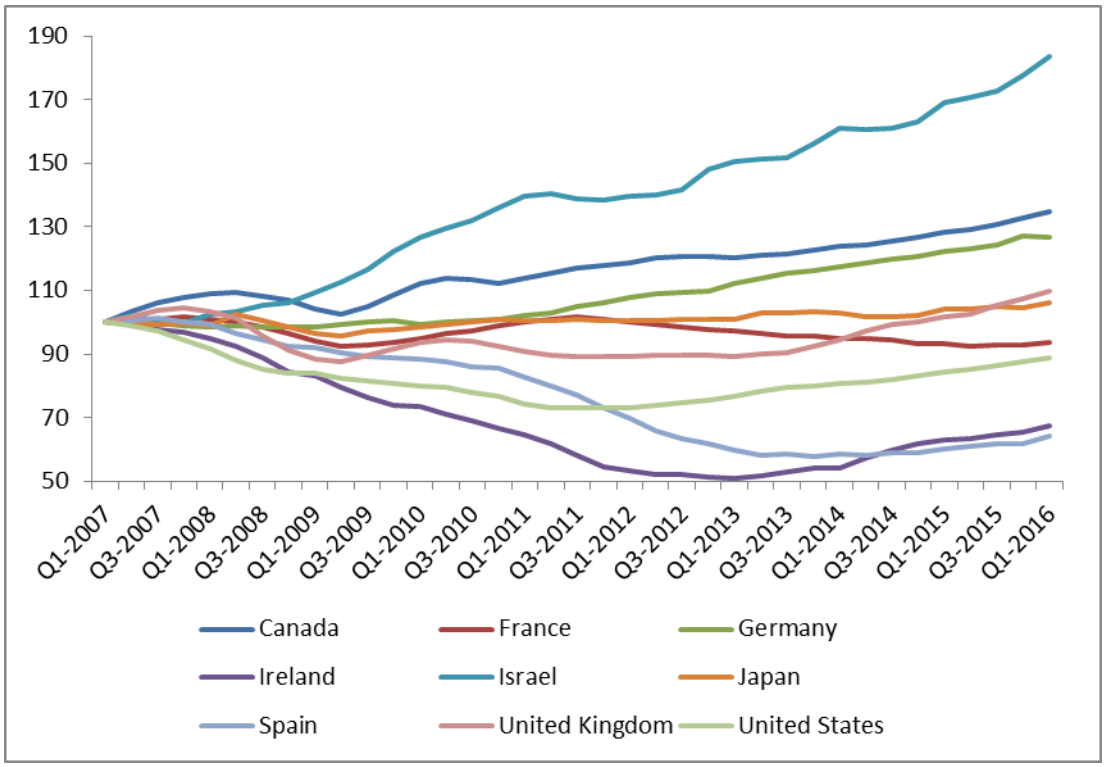
**Figure 1**

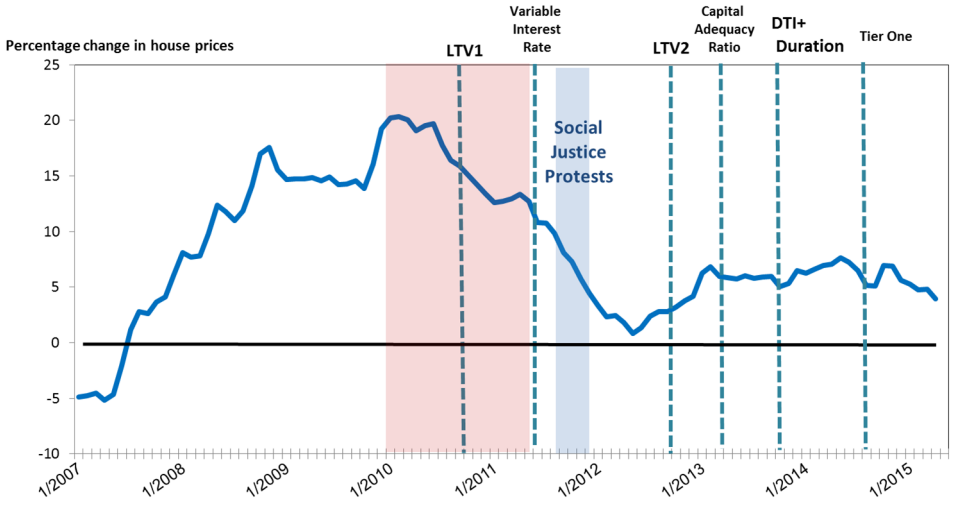
*Housing prices – various countries*




*Note*: Real house prices. All indexes normalized to 100 in 2007 Q1.

**Figure 2**

*Israeli House Prices and Implementation of MPPs.*

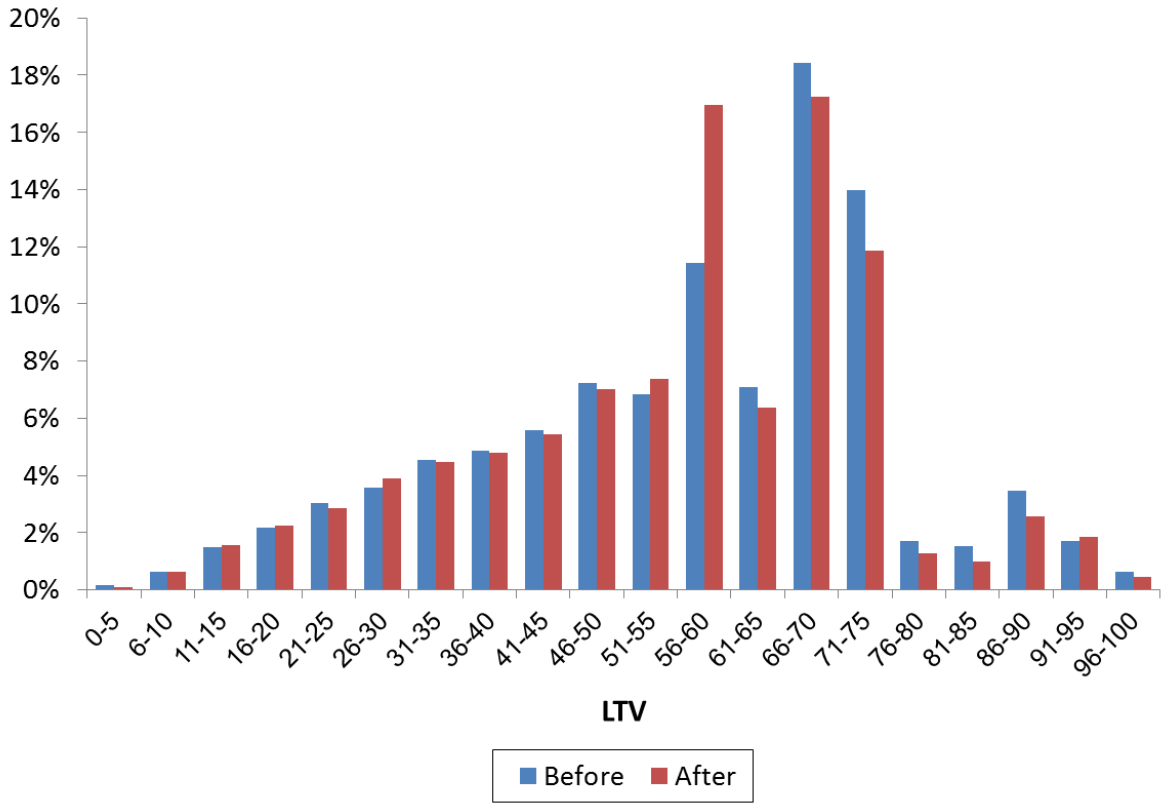



Source: Israel Central Bureau of Statistics.

*Note*: The line represents the monthly change in home prices (at an annual rate). Vertical lines show the implementation of MPPs (See Tzur-Ilan (2016) for additional details). The red area is our sample period while the blue area shows a period of social protests directed at the rising costs of living, especially housing.

**Figure 3**

*Distribution of LTV Ratios Before and After LTV Limit*

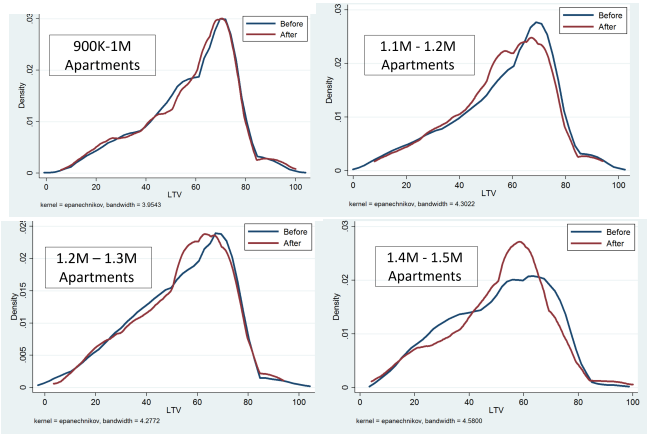





Source: Bank of Israel

**Figure 4**

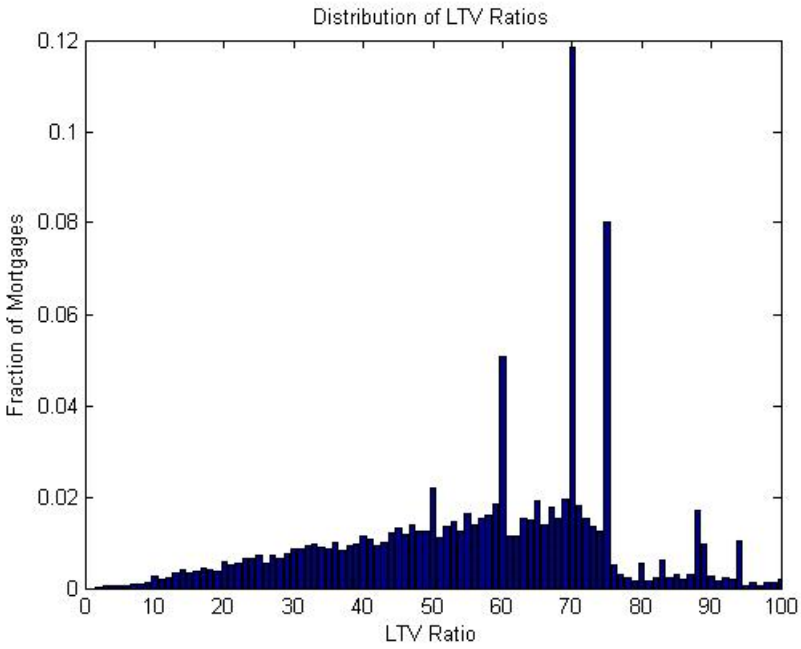
*Distribution of LTV Ratios Before and After LTV Limit, by Sale Price*




Note: Distributions smoothed using Gaussian kernel

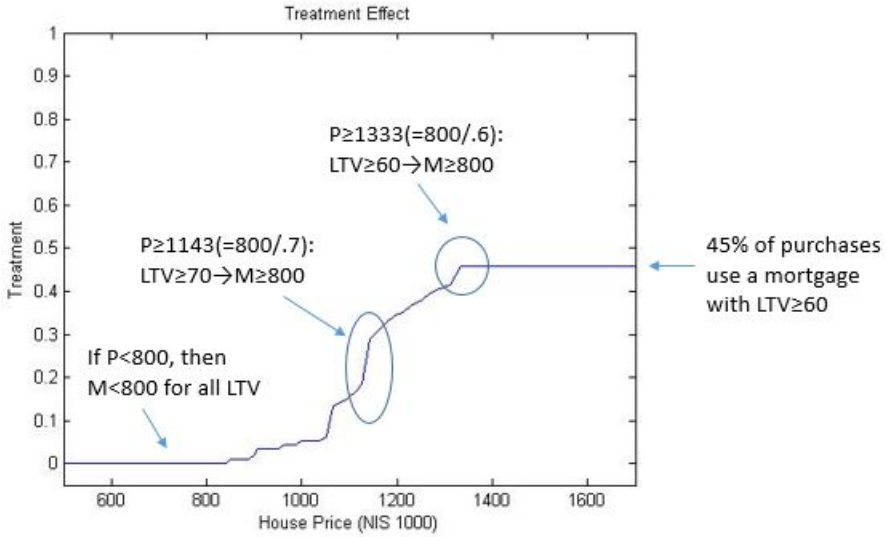
**Figure 5**

*Distribution of LTV Ratios Before LTV Limit*




Source: Bank of Israel

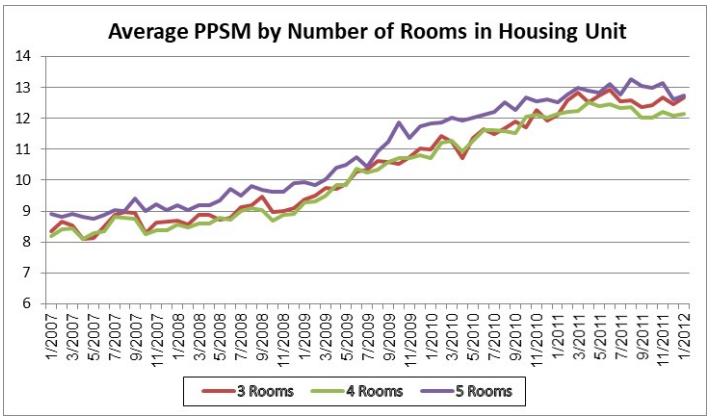
**Figure 6**
*Construction of the Treatment Effect*

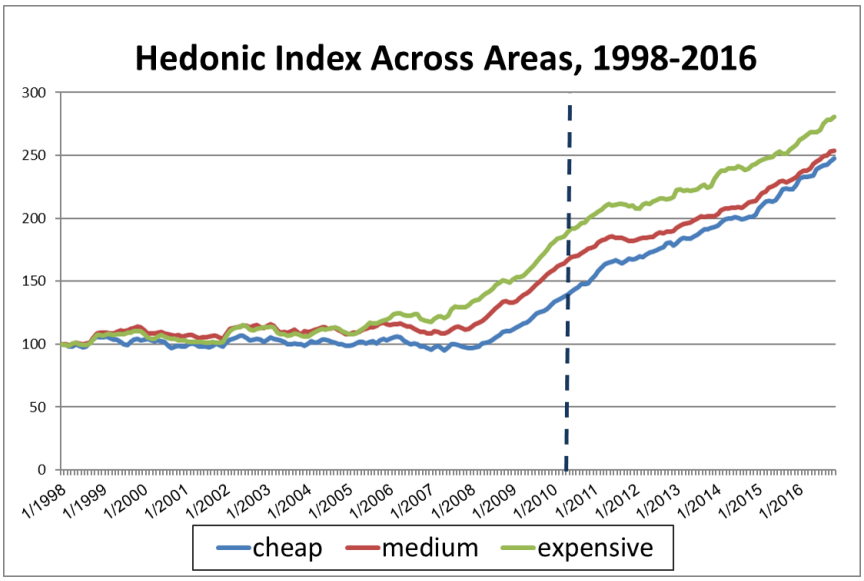



Note: Figure illustrates construction of treatment effect, as described in section 4.2. Labels indicate prices (P) at which having a LTV ratio (LTV) above the indicated value would result in a mortgage size (M) larger than NIS 800,000.

**Figure 7**

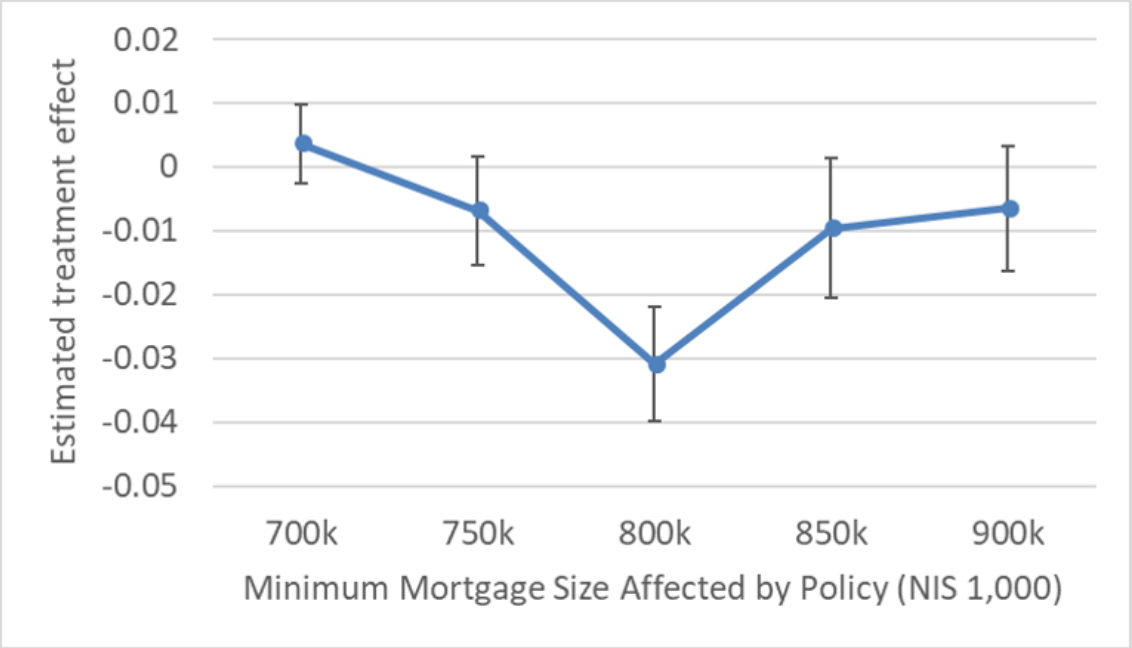
*House Price Growth by Market Segment*




*Note: top panel shows average monthly PPSM for housing units with different numbers of rooms. Lower panel shows monthly hedonic price indexes for cities classified (by the CBS) as as inexpensive, moderate and expensive. All indexes normalized to 100 in January 1998. Source: CBS and Bank of Israel*

**Figure 8**
*Placebo Treatment Effects*




*Note: Figure shows estimated effect on house prices when we make different assumptions about what size mortgages were affected by the policy. The actual mortgage size specified in the policy was NIS 800,000, while other values represent counter-factual placebo tests. Vertical bars show two standard errors around the estimates.*

**Table 1**
*Summary Statistics*

| Average | Before the LTV Limit | After the LTV Limit | Difference |
|---|---|---|---|
| Home price (NIS thousand) | 1,016.4 | 1,097.7 | 81.3*** |
| Price per SM (NIS thousand) | 12.0 | 12.9 | 0.9*** |
| Rooms | 3.6 | 3.6 | 0.0*** |
| Area (square meters) | 84.1 | 84.9 | 0.8*** |
| Building Year | 29.2 | 28.4 | -0.8*** |
| Distance from Tel Aviv (KM) | 44.8 | 45.5 | 0.7*** |
| Neighborhood Ranking | 10.3 | 10.2 | -0.1*** |

*** p<0.01, ** p<0.05, *p<0.1
Number of observations: 90,332 (48,985 before the LTV limit, 41,347 after the LTV limit).
Source: Israel Tax Authority.

**Table 2**

*The Estimated Effect of LTV limit on Housing Prices*

| | | | | predicted price | |
|---|---|---|---|---|---|
| | PPSM | PPSM | PRICE | PPSM | PRICE |
| | (1) | (2) | (3) | (4) | (5) |
| 3.roomsgroup | -0.183*** | -0.101*** | 0.233*** | -0.0591*** | 0.170*** |
| | (0.00598) | (0.00603) | (0.00660) | (0.00699) | (0.00950) |
| 4.rooms_group | -0.345*** | -0.179*** | 0.441*** | -0.0789*** | 0.525*** |
| | (0.00775) | (0.00815) | (0.00930) | (0.00913) | (0.00967) |
| 5.rooms_group | -0.490*** | -0.241*** | 0.570*** | -0.0811*** | 0.804*** |
| | (0.00851) | (0.00927) | (0.0107) | (0.0157) | (0.0120) |
| lnage | 0.00371*** | -0.00346*** | -0.0126*** | -0.00741*** | -0.0254*** |
| | (0.000810) | (0.000887) | (0.00111) | (0.00469) | (0.00672) |
| Treatment | 0.156*** | 0.744*** | 1.010*** | 0.744*** | 1.012*** |
| | (0.00623) | (0.0175) | (0.0176) | (0.0231) | (0.0193) |
| After | 0.0812*** | 0.0998*** | 0.0940*** | 0.0959*** | 0.0846*** |
| | (0.00360) | (0.00362) | (0.00380) | (0.00674) | (0.00699) |
| **TreatmentAfter** | **-0.0404*** ** | **-0.0309*** ** | **-0.0235*** ** | **-0.0212*** ** | **-0.0287*** ** |
| | (0.0108) | (0.0119) | (0.0092) | (0.0231) | (0.0090) |
| Geographic FE | NO | YES | YES | YES | YES |
| Geographic FEAfter | NO | YES | YES | YES | YES |
| Constant | 2.113*** | 2.319*** | 5.517*** | 2.324*** | 6.288*** |
| | (0.00977) | (0.0619) | (0.183) | (0.0965) | (0.0152) |
| Observations | 90,332 | 90,332 | 90,332 | 90,332 | 90,332 |
| R-squared | 0.891 | 0.902 | 0.919 | 0.902 | 0.893 |

[a] *Note: Table shows estimated effects of LTV limit on housing prices. Column 1 presents the results from the specification using the treatment function. Column 2 presents our preferred baseline specification which uses the results from the specification of the treatment function plus the geographic fixed effects. As an alternative to our baseline specification, column 3 shows results where we use price instead of price per square meter (PPSM) as our dependent variable. Column 4 and 5 show an alternative treatment variable that uses predicted price for each unit based on its hedonic characteristics and use this predicted price to compute a treatment effect. Bootstrap standard errors were calculated for columns 4 and 5. Significance levels 10%, 5%, and 1% are denoted by *, **, and *** and were determined based on the assumption that the bootstrap distribution is normally distributed.*

**Table 3**

*The Estimated Effect of LTV limit on Housing Prices: Individual LTV Ratios*

| | Cutoff: price> (800/0.60) | | Cutoff: price> (800/0.70) | | Cutoff: price> (800/0.75) | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| 3.rooms | -0.233*** | | -0.230*** | | -0.229*** | |
| | (0.00529) | | (0.00536) | | (0.00540) | |
| 4.rooms | -0.444*** | | -0.437*** | | -0.434*** | |
| | (0.00648) | | (0.00661) | | (0.00663) | |
| 5.rooms | -0.610*** | -0.182*** | -0.606*** | -0.182*** | -0.605*** | -0.179*** |
| | (0.00707) | (0.00305) | (0.00710) | (0.00309) | (0.00711) | (0.00305) |
| lnage | 0.00507*** | 0.00410*** | 0.00483*** | 0.00412*** | 0.00481*** | 0.00412*** |
| | (0.000462) | (0.000336) | (0.000481) | (0.000333) | (0.000488) | (0.000329) |
| price | 0.00120*** | 0.000815*** | 0.00118*** | 0.000777*** | 0.00115*** | 0.000774*** |
| | (2.63e-05) | (2.05e-05) | (3.11e-05) | (1.81e-05) | (3.21e-05) | (1.71e-05) |
| price sq' | -1.64e-07*** | -8.42e-08*** | -1.65e-07*** | -7.80e-08*** | -1.62e-07*** | -7.75e-08*** |
| | (7.22e-09) | (4.43e-09) | (8.30e-09) | (3.92e-09) | (8.52e-09) | (3.75e-09) |
| abovethr | -0.107*** | 0.00609 | -0.0684*** | 0.0321*** | -0.0470*** | 0.0424*** |
| | (0.00678) | (0.00383) | (0.00670) | (0.00409) | (0.00703) | (0.00470) |
| 1.after | 0.0435*** | 0.0271*** | 0.0486*** | 0.0297*** | 0.0511*** | 0.0337*** |
| | (0.00212) | (0.00255) | (0.00241) | (0.00363) | (0.00253) | (0.00446) |
| **abov.after** | **-0.0333*** ** | **-0.0173*** ** | **-0.0360*** ** | **-0.0160*** ** | **-0.0374*** ** | **-0.0192*** ** |
| | (0.00339) | (0.00314) | (0.00339) | (0.00409) | (0.00333) | (0.00481) |
| Constant | 1.754*** | 1.634*** | 1.768*** | 1.653*** | 1.780*** | 1.644*** |
| | (0.0158) | (0.0176) | (0.0178) | (0.0152) | (0.0182) | (0.0144) |
| Obs. | 90,332 | 29,564 | 90,332 | 29,564 | 90,332 | 29,564 |
| R-squared | 0.920 | 0.919 | 0.918 | 0.919 | 0.918 | 0.920 |

[a]

[a] *Note: Table shows estimated effects of LTV limit when we assume that treated properties are simply those with purchase prices above the indicated value. Columns 1,3 and 5 use the full sample, while columns 2,4 ad 6 use the more homogeneous sample of of only four and five room units.Significance levels 10%, 5%, and 1% are denoted by *, **, and ****

**Table 4**

*The Estimated Effect of LTV Limit on Housing Prices by Neighborhood Quality*

| | Low-Quality Areas | High-Quality Areas |
|---|---|---|
| | (1) | (2) |
| 3.roomsgroup | -0.220*** | -0.270*** |
| | (0.00768) | (0.00524) |
| 4.roomsgroup | -0.611*** | -0.536*** |
| | (0.00345) | (0.00517) |
| 5.roomsgroup | -0.549*** | -0.691*** |
| | (0.0108) | (0.00563) |
| lnage | -0.0109*** | -0.0126*** |
| | (0.000713) | (0.00111) |
| Treatment | -0.129*** | 1.820*** |
| | (0.00397) | (0.00865) |
| After | 0.744*** | 0.0334*** |
| | (0.0175) | (0.00389) |
| **TreatmentAfter** | **-0.0571*** ** | **-0.0274*** ** |
| | (0.00380) | (0.00362) |
| Geographic FE | YES | YES |
| Geographic FEAfter | YES | YES |
| Constant | 2.254*** | 2.566*** |
| | (0.00502) | (0.00510) |
| Observations | 38,585 | 51,747 |
| R-squared | 0.897 | 0.875 |

[a] *Note: Table shows estimated effects of LTV limit on housing prices by neighborhood quality. The socioeconomic index of neighborhoods quality combines 16 different variables, including education, employment, income, family size and standard of living into a single index. Neighborhoods are then classified into one of twenty clusters, 1 being the lowest socioeconomic status and 20 being the highest. The median quality is 10. For our analysis, we divide neighborhoods into two groups: low-quality areas, those neighborhoods that are graded from 1 to 10, and high-quality areas, neighborhoods that are graded from 11 to 20. We repeat our main estimation separately on these two groups of neighborhoods. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***.*

**Table 5**

*The Estimated Effect of LTV Limit on Housing Prices by Region and Neighborhood Quality*

| | Low-Graded Areas | High-Graded Areas |
|---|---|---|
| **Jerusalem** | **-0.0255**** | **-0.02077***** |
| | (0.0113) | (0.00679) |
| **North** | **-0.0102** | **0.00974** |
| | (0.00999) | (0.00715) |
| **Haifa** | **-0.0137***** | **-0.00970** |
| | (0.00348) | (0.0113) [a] |
| **Center** | **-0.0761***** | **-0.0454***** |
| | (0.00742) | (0.00703) |
| **Tel-Aviv** | **-0.0667***** | **-0.0324***** |
| | (0.00670) | (0.0131) |
| **South** | **-0.00463** | **0.00239** |
| | (0.00487) | (0.00348) |

[a] *Note: Table shows estimated effects of LTV limit on housing prices by neighborhood quality for different regions in Israel. For this purpose, the CBS divide the country into six regions: the cities of Jerusalem, Tel Aviv and Haifa, and for locations outside of these cities, the northern, central and southern areas of the county. Within each of the six regions, we again consider high and low socioeconomic neighborhoods. Significance levels 10%, 5%, and 1% are denoted by *, **, and ****

**Table 6**

*The Estimated Effect of LTV Limit on Housing Prices Using Observed Mortgages*

| | (1) | (2) | (3) |
|---|---|---|---|
| 3.roomsgroup | -0.677*** | -0.840*** | -0.605*** |
| | (0.178) | (0.167) | (0.00703) |
| 4.roomsgroup | -0.686*** | -0.999*** | -0.708*** |
| | (0.180) | (0.170) | (0.00279) |
| 5.roomsgroup | -0.652*** | -1.144*** | -0.519*** |
| | (0.182) | (0.174) | (0.00234) |
| lnage | 0.00495*** | 0.00515*** | 0.00489*** |
| | (0.000484) | (0.000474) | (0.000490) |
| price | | 0.00128*** | 0.00120*** |
| | | (3.81e-05) | (2.63e-05) |
| price sq′ | | | -1.64e-07*** |
| | | | (7.22e-09) |
| Treatment | 0.0764*** | 0.00747 | 0.00831 |
| | (0.0149) | (0.0157) | (0.0106) |
| After | 0.134*** | 0.0533*** | 0.0446*** |
| | (0.00822) | (0.00930) | (0.00310) |
| **TreatmentAfter** | **-0.0693*** ** | **-0.0749*** ** | **-0.0544*** ** |
| | (0.0259) | (0.0238) | (0.0208) |
| Constant | 3.016*** | 2.848*** | 1.809*** |
| | (0.186) | (0.172) | (0.0173) |
| Observations | 33,311 | 33,311 | 33,311 |
| R-squared | 0.514 | 0.550 | 0.586 |

[a]

[a]*Note: Table shows results when we identify treated properties as those actually purchased using a mortgage affected by the policy.Significance levels 10%, 5%, and 1% are denoted by *, **, and ****

# 7. Appendix

In this appendix, we present additional results using a treatment defined by having a transaction price above a single threshold, which would describe the affected transactions if the market were defined by a single LTV ratio.

## 7.1. *Additional Robustness Checks for Individual LTV Ratios*

We first examine a broader time period around the LTV limit, from the beginning of 2009 until the end of 2012 and again. The interaction between having a price above the threshold and selling after the MPP was enacted enters again with a negative sign, and the estimated effect is larger than in the main specification for each of the the three different thresholds (not shown). Next, we examine a very narrow price range around the cutoff. Table 7) shows that the effect is still significant and the sign still negative, but the effect becomes smaller.

**Table 7**

*The Estimated Effect of LTV limit on Housing Prices, January 2010 to May 2011, narrower range around the cutoff*

| | price> (800/0.6) | | price> (800/0.75) | |
|---|---|---|---|---|
| | +/- 20,000 | +/- 50,000 | +/- 20,000 | +/- 50,000 |
| | (2) | (3) | (5) | (6) |
| 3.rooms_group | -0.262*** | -0.265*** | -0.275*** | -0.274*** |
| | (0.00668) | (0.00861) | (0.00476) | (0.00483) |
| 4.rooms_group | -0.492*** | -0.496*** | -0.510*** | -0.507*** |
| | (0.00732) | (0.00942) | (0.00530) | (0.00537) |
| 5.rooms_group | -0.643*** | -0.647*** | -0.668*** | -0.667*** |
| | (0.00787) | (0.0100) | (0.00597) | (0.00604) |
| ln_age | 0.00607*** | 0.00602*** | 0.00609*** | 0.00628*** |
| | (0.000386) | (0.000393) | (0.000407) | (0.000416) |
| Treatment | 0.000579 | 0.00236 | -0.0286*** | -0.0436*** |
| | (0.00338) | (0.00335) | (0.00359) | (0.00345) |
| After | 0.0235*** | 0.0242*** | 0.0247*** | 0.0257*** |
| | (0.00182) | (0.00227) | (0.00193) | (0.00184) |
| **Treatment#After** | **-0.00647**** | **-0.00751**** | **-0.00543**** | **-0.00641**** |
| | (0.00312) | (0.00320) | (0.00268) | (0.00263) |
| Constant | 1.819*** | 1.948*** | 1.682*** | 1.582*** |
| | (0.0284) | (0.0589) | (0.0123) | (0.0105) |
| Observations | 2,983 | 7,074 | 3,590 | 7,876 |
| R-squared | 0.870 | 0.864 | 0.892 | 0.913 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

## 7.2. *Differences in Socioeconomic Area Using Individual LTV Ratios*

In this section, we present results analogous to those from Section 5.4, showing the differential results in high and low socioeconomic areas, but using a treatment effect defined by an individual LTV ratio. The results are shown in table 8. We present the results for the two different cutoffs: (800/0.6) and (800/0.75). In both the low and high quality areas, home price were lower due to the limit. However, the results shows that the LTV limit affected the low quality area more than the high quality area (by approximately 1 percent), suggesting that more credit constrained households may be more affected by the policy.

**Table 8**

*The Estimated Effect of LTV limit on Housing Prices, January 2010 to May 2011, by Socio-Economic Area*

| | Cutoff: price> (800/0.6) | | Cutoff: price> (800/0.75) | |
|---|---|---|---|---|
| | Low-Quality | High-Quality | Low-Quality | High-Quality |
| | (1) | (2) | (3) | (4) |
| 3.rooms_group | -0.238*** | -0.243*** | -0.239*** | -0.239*** |
| | (0.00790) | (0.00650) | (0.00791) | (0.00671) |
| 4.rooms_group | -0.453*** | -0.462*** | -0.448*** | -0.457*** |
| | (0.00976) | (0.00744) | (0.0104) | (0.00763) |
| 5.rooms_group | -0.632*** | -0.622*** | -0.628*** | -0.620*** |
| | (0.0111) | (0.00808) | (0.0114) | (0.00811) |
| ln_age | 0.00789*** | 0.00447*** | 0.00748*** | 0.00444*** |
| | (0.000801) | (0.000527) | (0.000744) | (0.000552) |
| Treatment | -0.138*** | -0.0740*** | -0.130*** | 0.00817 |
| | (0.0153) | (0.00742) | (0.00900) | (0.00801) |
| After | 0.0446*** | 0.0343*** | 0.0482*** | 0.0396*** |
| | (0.00310) | (0.00249) | (0.00358) | (0.00320) |
| **Treatment#After** | **-0.0345*** ** | **-0.0235*** ** | **-0.0340*** ** | **-0.0255*** ** |
| | (0.00844) | (0.00355) | (0.00737) | (0.00376) |
| Constant | 1.539*** | 1.926*** | 1.512*** | 1.980*** |
| | (0.0273) | (0.0208) | (0.0342) | (0.0220) |
| Observations | 38,585 | 51,747 | 38,585 | 51,747 |
| R-squared | 0.905 | 0.915 | 0.906 | 0.912 |

## 7.3. *Construction of the treatment effect using predicted prices*

In this section, we describe the construction of our alternative treatment effect based on a unit's hedonic characteristics rather than on its observed transaction price. In this approach, we compute a predicted price ($\hat{p}$) for each unit based on

its hedonic characteristics using an estimating equation of the form:

$$\ln(\hat{p}_i) = \alpha + \hat{\beta} X_i + month_i + \epsilon_i \tag{5}$$

We use the same specification with both the price of the transaction and the price per square meter as dependent variables. The set of controls $Xi$ includes the size of the unit and the age of the building. We also use monthly indicator variables to account for seasonality in the housing market. We then use this predicted price to compute a treatment effect $Treat(\hat{p})$, analogous equation Equation 1, but using the predicted price ($\hat{p}$) rather than the actual price:

$$Treat(\hat{p}) = \sum_{LTV=0.6}^{1} I(\hat{p} * LTV > NIS800,000) * f(LTV) \tag{6}$$

Results using this alternative treatment effect are shown in columns 3 and 4 of table 2.