

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 macroprudential regulation and house prices by asking whether policies that impose higher risk weights on high-LTV mortgages can slow house price growth. Studying a 2010 Israeli policy, we find that the prices on housing units likely to be purchased using mortgages subject to higher risk weights are reduced by about 2 1/2 percent. 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 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.

Keywords: House prices, Macroprudential Policy, LTV-based risk weights, Mortgages
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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 aimed to build greater financial resilience by controlling the amount of mortgage lending in the economy. These policies have largely focused on limiting banks' provision of riskier mortgages, such as those with higher loan-to-value (LTV) ratios. Among such policies, limits on LTV ratios and policies that require banks to hold more capital against high-LTV mortgages have been the two most common (Alam et al., 2019).

Macroprudential policies that reduce banks' origination of high-LTV mortgages can potentially improve financial stability through three distinct mechanisms. First, policies that discourage banks from originating these high-leverage mortgages can slow the total growth of mortgage credit during a credit boom (Akinci and Olmstead-Rumsey, 2018). Second, because residential mortgages make up a sizeable fraction of bank assets, such policies can significantly reduce bank losses during economic downturns (Krznar and Morsink, 2014; Lim et al., 2013). A final 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 easing 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.

The question of whether macroprudential policies reduce house prices has received considerable attention in the literature, but with mixed conclusions. Some studies 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 the 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 220,000). 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 NIS 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 inability of the Government Sponsored Enterprises (Fannie Mae and Freddie Mac) to purchase mortgages above 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.

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 1/2 percent less than units that are not affected by the policy. In aggregate, we estimate that this policy reduced prices for the most expensive properties by around 1 percent and for the market as a whole by 0.6 percent. While this effect is relatively small compared to the overall rate of house price appreciation during this period, these findings nevertheless represent new 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), Jerusalem (the capital city), 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 7-9, consistent with the upper range of estimates reported in the literature.¹ 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.

This paper contributes to a nascent literature that uses micro-data to identify the effects of housing-focused macroprudential policies on house prices and other outcomes. Within this literature, our paper is most similar to [Acharya et al. \(2019\)](#), who show that LTV and LTI limits introduced in Ireland in 2015 led to a reallocation of mortgage credit across markets and slowed house price growth in markets more affected by the new policies. Meanwhile, [Armstrong et al. \(2019\)](#) exploit exemptions from 2013-2016 LTV restrictions implemented in New Zealand in order to show that these restrictions were effective at reducing house prices. Other authors have also used micro data to measure the effect of LTV limits, but look at outcomes other than house prices. For example, [Gaborro et al. \(2019\)](#) study a 2011 limit on LTV ratios imposed in the Netherlands and find that the first-time homebuyers most affected by this policy responded by taking on less mortgage debt.

Together with some of the studies cited above, our paper also falls within a larger literature that has tried to measure the effects of LTV limits on house prices, mostly using macro data. 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 the 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 price 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 limits and LTV-based risk weights appear to have an effect on house price growth of 2.5 percentage points. The paper described above by [Armstrong et al. \(2019\)](#) (using micro data) also finds an effect of approximately 2.5 percentage points, and [Alam et al. \(2019\)](#) find a 1 percentage point effect. As noted earlier, other studies including [Kuttner and Shim \(2016\)](#), fail to find any effect of MPP tools on housing prices.

Unlike most of the above papers, which generally examine strict LTV limits,

¹For example, [Adelino et al. \(2012\)](#) estimate a semi-elasticities in the range from 1 to 9 and [Davis et al. \(2020\)](#) 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.

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 \(2018\)](#), 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, several other researchers have used variation at the micro level to draw conclusions about the effects of credit limits on house prices, though without directly examining macroprudential policies. [Anenberg et al. \(2017\)](#) construct “loan frontiers” that characterize the largest obtainable mortgages by different types of borrowers. Using variation in these frontiers across time and across 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 percent 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 1 provides background on the housing market and housing finance in Israel. Section 2 describes the data used in our analysis. Section 3 analyzes the effects of this MPP on the distribution of observed LTV ratios and describes how we identify which transactions are more likely to be affected by the policy. Finally, section 4 presents our results, including a series of robustness checks and placebo tests. Section 5 concludes the paper.

1. 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.²

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.³ 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 rate in Israel is close to 70 percent, somewhat higher than the 63 percent average home ownership rate among OECD countries.

6. Housing supply in Israel is particularly inelastic as a result of government ownership and control of most undeveloped land,⁴ a centralized planning process, and extensive bureaucracy at all stages of construction.⁵ 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.⁶ 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 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.⁷

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

²For more information, see Cerutti et al. (2017).

³Government mortgages account for less than 2 percent of the market.

⁴The state controls 93 percent of the land in Israel, and a government agency, the Israel Land Administration (ILA), manages and allocates this land.

⁵See Bank of Israel Annually report (2012).

⁶See Bank of Israel Annually report (2015).

⁷See Bank of Israel Annually report (2018).

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 the first quarter of 2007 to the first quarter of 2016, nominal house prices in Israel climbed by 135 percent, and real house prices grew by 103 percent (see Figure 1 for an international comparison). Over the same period, rents increased 56 percent in nominal terms and approximately 35 percent in real terms. Meanwhile, the volume of mortgages increased by 93 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.⁸ 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.

1.1. 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 220,000), 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 loans with LTV ratios 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.⁹

⁸For a summary of the various MPP tools, see Tzur-Ilan (2016).

⁹According to Google Trends, the number of searches for the words “equity”, “down payment”, “loan”, or “Loan-to-Value” in Israel did not change in the months leading up to the October 2010 policy change.

2. 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. 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.¹⁰ We drop transactions that do not meet criteria set by the CBS to construct the price index.¹¹ We also drop the observations at the upper and lower ends of the distributions of price and price per square meter (which will be our dependent variables), specifically the top and bottom 0.125 percent of the distributions of each of these two variables, totaling 0.5 percent of all observations. Our analysis focuses on the period from January 2010 to April 2011 (the red area in Figure 2), a relatively narrow time window centered on the October 2010 policy implementation. 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 89,900 observations, 48,719 observations before the higher risk weights were imposed and 41,180 observations afterwards.

While the main analysis in this paper uses only the housing transaction dataset, we also use a dataset from Tzur-Ilan (2016) in which the housing transactions have been merged with 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).¹² Tzur-Ilan (2016) was 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 (16,073 observations before the LTV limit and 11,134 observations after).

¹⁰The 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.

¹¹The 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.

¹²Together, these seven lenders account for roughly 95 percent of all mortgage loans in Israel.

2.1. Summary Statistics

As background for our analysis, Table 1 shows descriptive statistics for our dataset, which we divide into the 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 1,092 after the limit, an increase of 7.5 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 84.2 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.¹³ The last variable we show in Table 1 is the neighborhood socioeconomic index, published 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.

3. 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 identical borrowers with LTV ratios 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.¹⁴ 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 the country center, and in less desirable neighborhoods.

¹³Observations 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 80 km radius from Tel Aviv.

¹⁴Data for all the figures showing the distribution of LTV ratios are from the Bank of Israel mortgage dataset.

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

3.1. Identifying Affected Transactions

The first challenge in identifying the effect of this policy on house prices is determining which transactions are 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 the 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 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 at or just below 60 percent.

3.2. 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 :

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

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:

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

i.e. the probability of having an 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 an 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 an LTV ratio above 60 percent will be above NIS 800,000, as described above.

4. RESULTS

4.1. 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:

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

where $\ln(P_{it})$ is either the log price or log price per square meter (PPSM) for unit i sold at time t . The control variables included in the vector of property characteristics X are the 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, θ_t is a time dummy equal to zero before the policy was implemented (01/2010-10/2010) and one afterwards (11/2010-05/2011), and ϵ_{it} is a well-behaved error term clustered at the locality statistical area level. Our primary interest is in the coefficient σ , which captures the difference in pricing for treated and untreated properties after the LTV limit is imposed, relative to the difference before the policy.

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 η interacted with our “after” dummy variable.¹⁶ The resulting equation is:

$$\ln(P_{it}) = \alpha + \beta X_i + \delta * Treat(p) + \lambda * \theta_t + \sigma * Treat(p) * \theta_t + \eta_{0,j(i)} + \eta_{1,j(i)} * \theta_t + \epsilon_{it} \quad (4)$$

where $j(i)$ indexes the geographic region of unit i .

The identifying assumption underlying our 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 top panel shows the evolution of average PPSM for 3-room, 4-room and 5-room units, while the bottom 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. More formal tests of the parallel pre-trends assumption are included in the robustness section below.

4.2. Main Results

The first column of Table 2 presents the results from the basic specification without geographic fixed effects (equation 3). As expected, units with larger number of rooms have a lower price per square meter. Age enters positively, likely reflecting the fact that older properties were built in more desirable locations. Units selling in the second half of the period had higher prices, consistent with the general rise

¹⁶We 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.

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.

The remaining columns of Table 2 show results from the specification that includes the interacted geographic fixed effects (equation 4). With the addition of these fixed effects, as shown in column (2), 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 produces a smaller estimate for our main parameter of interest, which falls from 4.0 percent to 2.9 percent, but remains statistically significant. This supports our earlier conjecture that some of the measured effect in the previous specification could reflect treated properties being in locations with slightly slower price growth. However, the quantitative effect of this issue appears small. An alternative specification where we use price instead of PPM (as shown in column (3)) produces very similar estimates for the effect of the policy.

One potential concern with the above 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). With this substitution, we reach our main specification.

Our main results, using both the geographic fixed effects and treatment effects calculated from these predicted prices, are shown in columns (4) and (5) of Table 2.¹⁷ When we use PPSM as the dependent variable (column 4), we find that treated properties have prices that are 2.5 percent lower. When we use price as the dependent variable (column 5), we get a slightly larger but statistically indistinguishable estimate of 2.7 percent. Overall, we conclude that properties likely to be purchased using mortgages subject to higher risk weights have prices that are 2 1/2 percent lower than properties not affected by the policy.

¹⁷Because these estimates use a covariate that is itself the output of a first stage regression, we calculate standard errors by bootstrapping the entire process, using a stratified re-sampling procedure with replacement so that the number of observations stays the same in each replication.

4.3. Robustness Checks

A potential concern about our identification is that our treated transactions differ from untreated transactions for reasons that are unconnected to the macroprudential policy being studied. We perform several robustness checks to address this issue.

4.3.1. 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.¹⁸ 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.

We also perform standard tests for parallel pre-trends, running each of our specifications on the data with all dates moved one year earlier. In these regressions, the pre-period is the 10 months from January to October 2009 and the post-period is November 2009 through May 2010, so that all observations are taken from before the October 2010 policy implementation. As shown in the first two columns of Table 3, we find no significant results in any of these specifications.¹⁹ These findings lend additional support to our assumption that there are no other significant differences between house price growth in the more and less treated segments of the market.

4.3.2. Allowing for Substitution Between Treated and Untreated Properties

Our estimated effect reflects the difference in prices between treated and untreated properties induced by the policy change. However, if the policy caused some

¹⁸The figure also shows results from placebo tests where the treatment is the probability of having a mortgage over NIS 750,000 or over NIS 850,000.

¹⁹We also repeat this exercise with the pre-and post-periods moved back an additional year and find no significant effects, as shown in columns 3 and 4 of Table 3.

buyers to shift their demand from the treated properties to the untreated ones, this extra demand for untreated properties would have raised the price of these units relative to what the price would have been without the policy. This would mean that the control group (of untreated properties) would not have actually been insulated from the policy change. In this case, our measured effect might actually be best interpreted as the *difference* between the policy's (negative) effect on the more expensive "treated" properties and the policy's (positive) effect on these less expensive "untreated" properties. The aggregate effect on prices would be the combination of these two offsetting effects and would therefore be smaller than the effect we measure above.

In order to address this concern, we consider a control group that should be less affected by this demand substitution. In particular, we restrict our sample to a treatment group of units with prices above NIS 1.3 million and a control group of units with prices below NIS 800 thousand, dropping units with prices in the intermediate range. In this exercise, we assume that units with prices below NIS 800 are of sufficiently different quality from the higher priced units that they are not subject to the concerns about substitution described above. In Table 4, we show results from these regressions, using several different price ranges to define the high-priced treatment group and the low-priced control group. In each case, we indeed find effects that are slightly smaller, but statistically indistinguishable from our baseline estimates.²⁰

Conversely, this argument also implies that limiting the sample to a narrower range of prices should produce larger estimates compared to our baseline. In this case, the "control" group of relatively lower priced units is very close to the treatment group and should benefit more from the shift in housing demand away from the treated units. Indeed, we find evidence supportive of this interpretation. Table 5 shows the same specification as in Table 2 but using samples limited to transactions with prices close to the range where the policy becomes effective. Specifically, we consider three different samples limited to i) sales with prices from NIS 600-1400 thousand, ii) sales from NIS 700-1500 thousand, and iii) sales from NIS 800-1400 thousand. In all three specifications we find a larger effect of the LTV limit on house prices compared to our baseline estimates. These findings are consistent with the substitution effects described above. Alternatively, the stronger results could also reflect a higher sensitivity to interest rates as we focus the analysis away from the most expensive sales, which presumably involve the most financially well off buyers.²¹

These exercises offer empirical support for the argument that the policy may have shifted demand from more expensive to less expensive units. In the end, we think that the most appropriate way to capture the overall effects of the policy is to

²⁰We caution the reader that, by construction, the two groups being compared in this exercise are quite different from each other, reducing the ability of our control variables to properly account for all the differences between them.

²¹These models also pass the same pre-trend tests that we perform for our baseline model (not shown).

use the broader sample we rely on for our baseline estimates, where treated and non-treated units have greater differences from each other and these substitution effects should be less of a concern.

4.4. Differential Effects by Neighborhood Quality

We next test whether these price effects are larger in particular types of neighborhoods. As described in Section 2, the Israeli Central Bureau of Statistics publishes a socioeconomic index of neighborhood 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 6, 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 divides 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 7, 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 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.

4.5. Direct observation of high-LTV mortgages

In all the specifications so far, we have used a treatment measure based on 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 use a dataset from (Tzur-Ilan, 2016), where the housing transactions data have been merged with loan-level data from the Bank of Israel (as described in Section 2). 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 the policy, they are of course subject to selection concerns due to the fact that borrowers endogenously select their LTV ratios and loan amounts. 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 *untreated* in this specification.

As shown in Table 8, this exercise would suggest that the LTV limit caused a decrease in house price of 5.5-7.5 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. 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.

4.6. *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 7-9, 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.

4.7. *Interpreting the Effects on Prices*

Finally, we address the question of what effect this macroprudential policy had on the overall level of Israeli house prices. The quantitative analysis described so far estimates the impact of the policy on a theoretical property that receives a treatment effect of one. This estimate would be accurate for a property that, for whatever idiosyncratic reason, had a pool of potential buyers that planned to use a mortgage subject to the higher risk weights. More generally, this would be the effect of a policy that raised risk weights on mortgages overall (at least for segments of the market where buyers could be assumed to be using mortgages to finance their purchases).²² However, in the specific case of the Israeli housing market, the policy was only applied to large, high-LTV mortgages. What then was the total effect of this policy on Israeli house prices?

For the most expensive properties (those with prices over NIS 1.33 million), any mortgage with an LTV ratio over 60 percent will be larger than the NIS 800,000 minimum size. However, as described above, only 45 percent of purchases use mortgages with LTV ratios above the 60 percent threshold. This implies that even for the most expensive segment of the market, where the policy has its largest effect, the impact on prices will only be 45 percent of our estimated effect, or roughly 1 percent. To measure the effect on the market as a whole, we multiply our estimated price effect by the average treatment effect across all properties in our sample (weighted by transaction price). Performing this exercise, we conclude that, in aggregate, this policy lowered Israeli house prices by about 0.6 percent.

5. 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 recent run-up in Israeli house prices.

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

²²For example, if such a policy were applied in the United States to loans guaranteed by Fannie Mae and Freddie Mac, this estimate should capture the effect of the policy on the FHFA house price index, which is based on transactions that use these mortgages.

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.²³ 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.²⁴ 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 examine had its largest effect on house prices in lower quality neighborhoods within the more expensive areas of the country. 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.

²³This 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.

²⁴[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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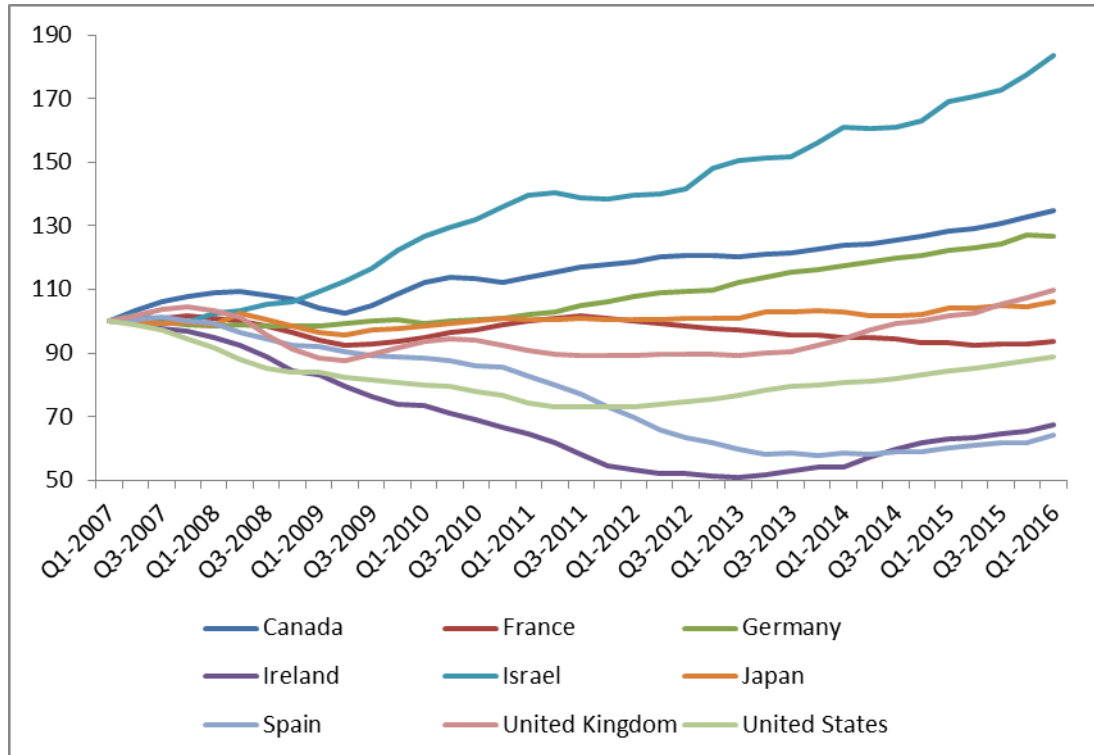


Figure 1
Housing prices - various countries. Real house prices. All indexes normalized to 100 in 2007 Q1. Source: OECD.

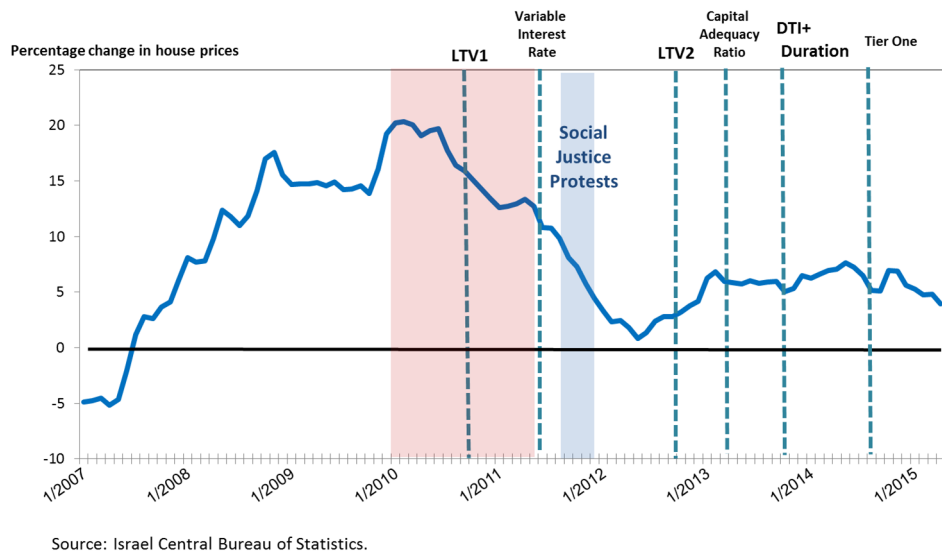


Figure 2
Israeli House Prices and Implementation of MPPs. Note: The figure shows the monthly change in home prices (at an annual rate). Vertical lines show the implementation of MPPs (See [Tzur-Ilan \(2016\)](#) for additional details). The red area is our sample period while the blue area shows a period of social protests directed at the rising costs of living, especially housing.

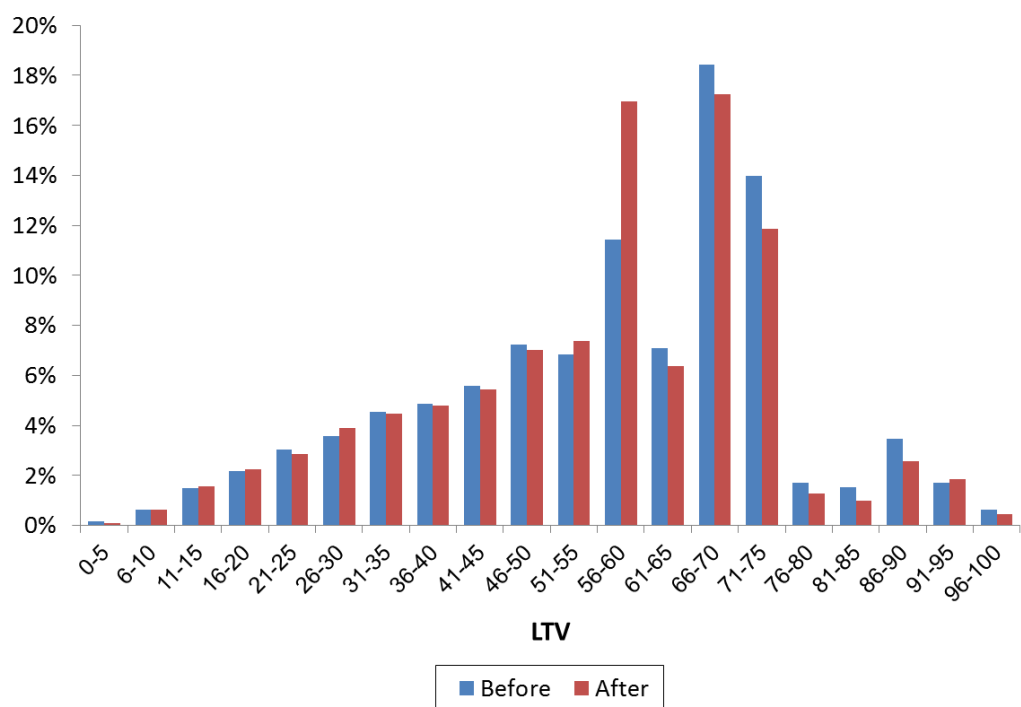


Figure 3
Distribution of LTV Ratios Before and After LTV Limit. *Source: Bank of Israel.*

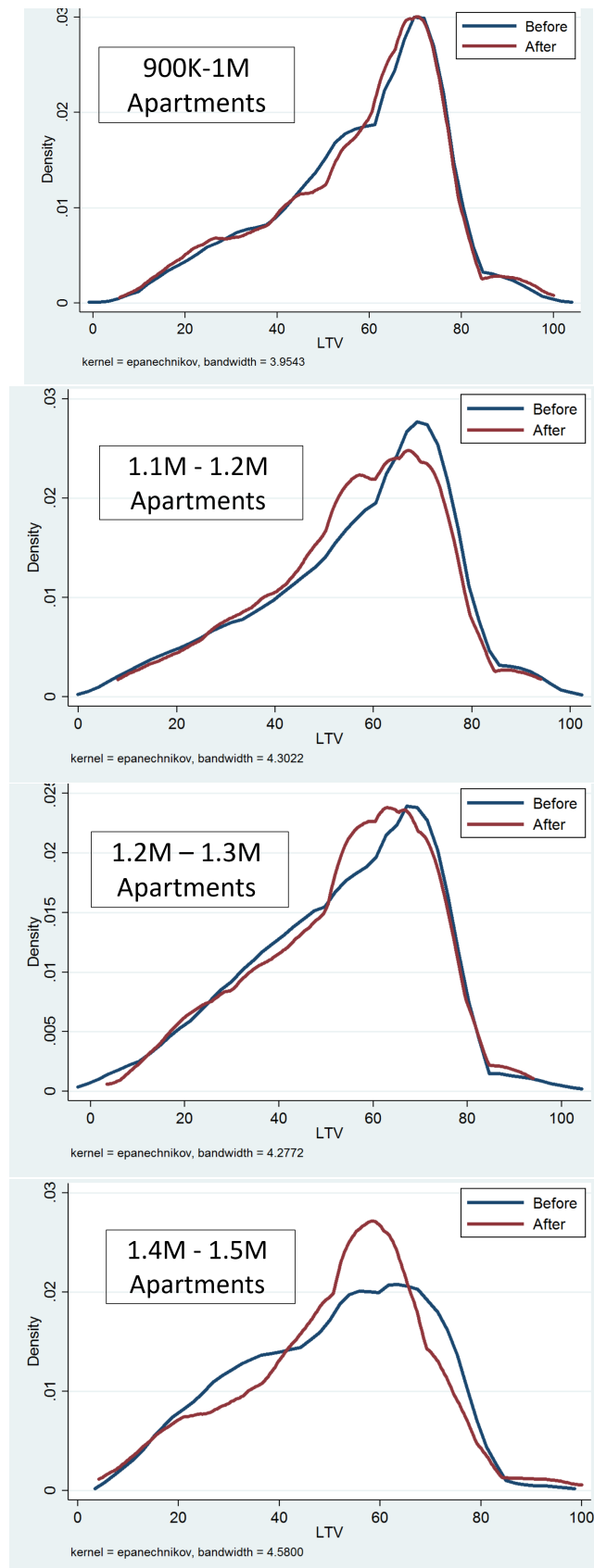


Figure 4

Distribution of LTV Ratios Before and After LTV Limit, by Sale Price. *Note: Distributions smoothed using Gaussian kernel. (The appearance of an increase in the share of loans with LTVs slightly above 60 percent after the policy is a result of this smoothing.) Source: Bank of Israel.*

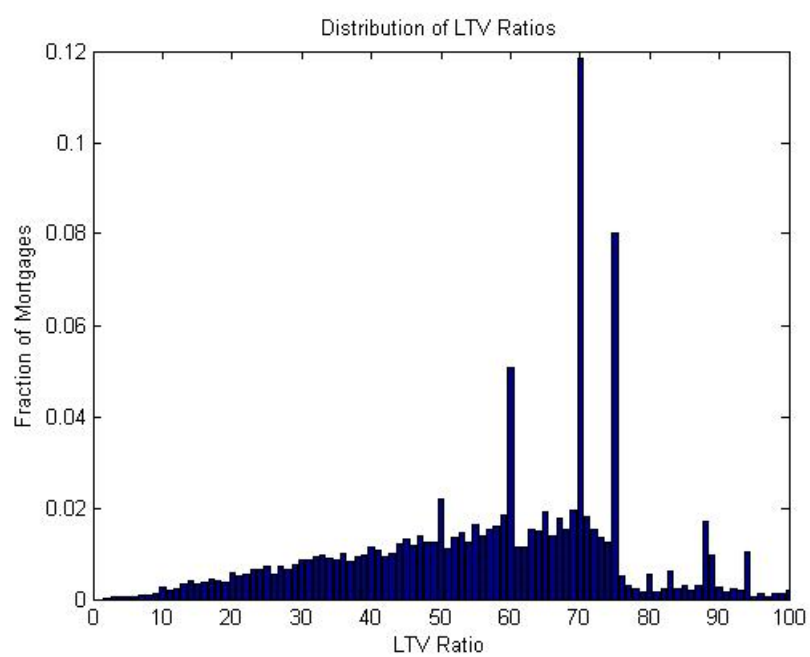


Figure 5
Distribution of LTV Ratios Before LTV Limit. *Source: Bank of Israel*

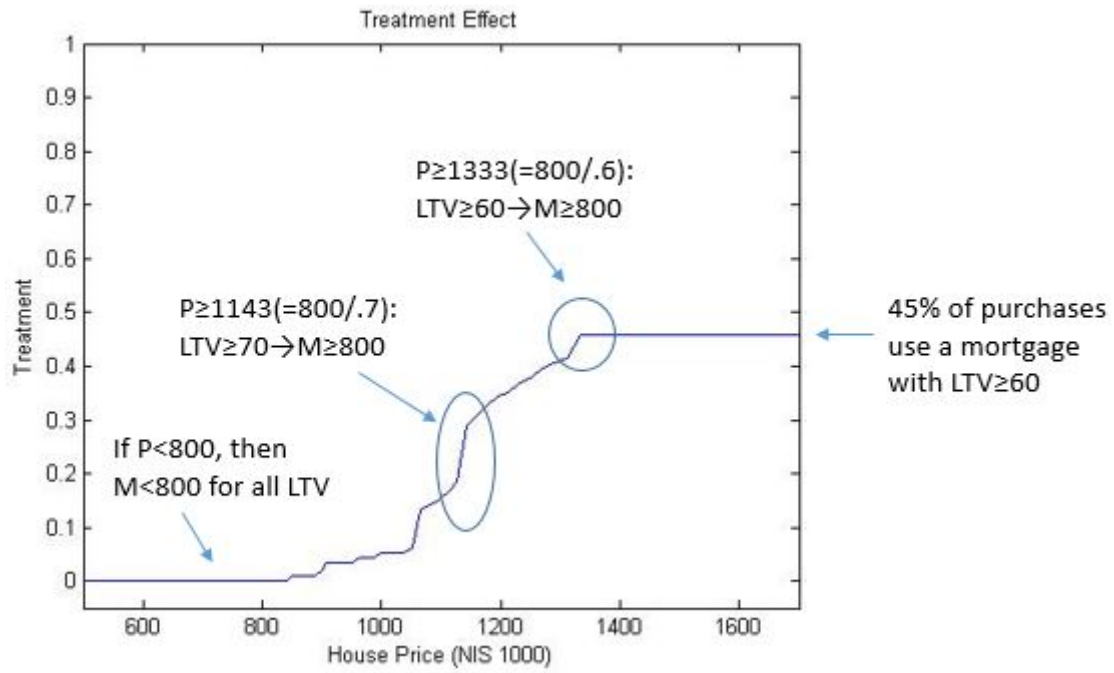


Figure 6

Construction of the Treatment Effect. *Note: Figure illustrates construction of treatment effect, as described in section 3. Labels indicate prices (P) at which having an LTV ratio (LTV) above the indicated value would result in a mortgage size (M) larger than NIS 800,000.*

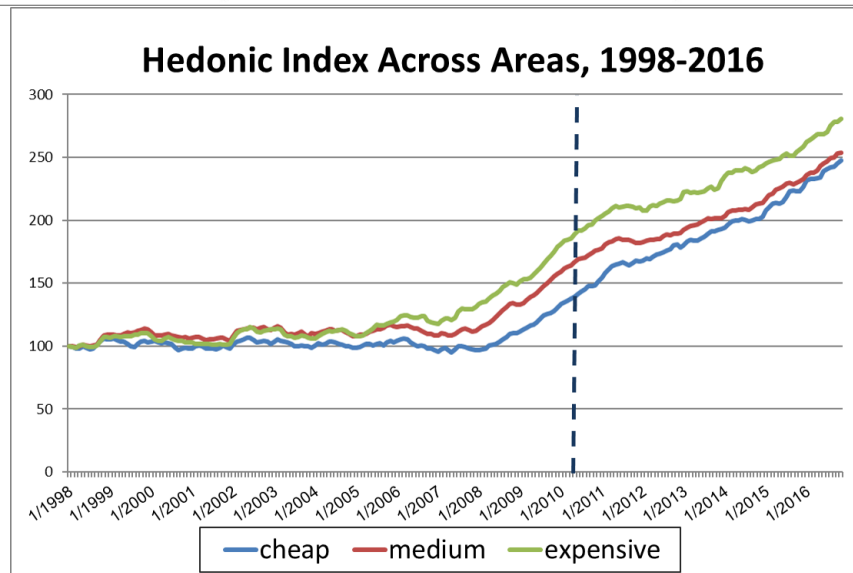
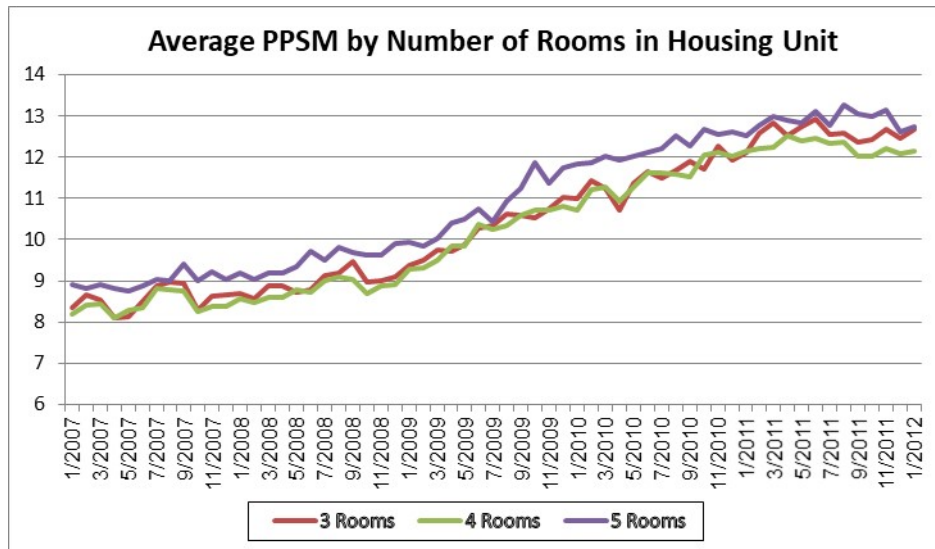


Figure 7

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 inexpensive, moderate and expensive. All indexes normalized to 100 in January 1998. Source: CBS and Bank of Israel*

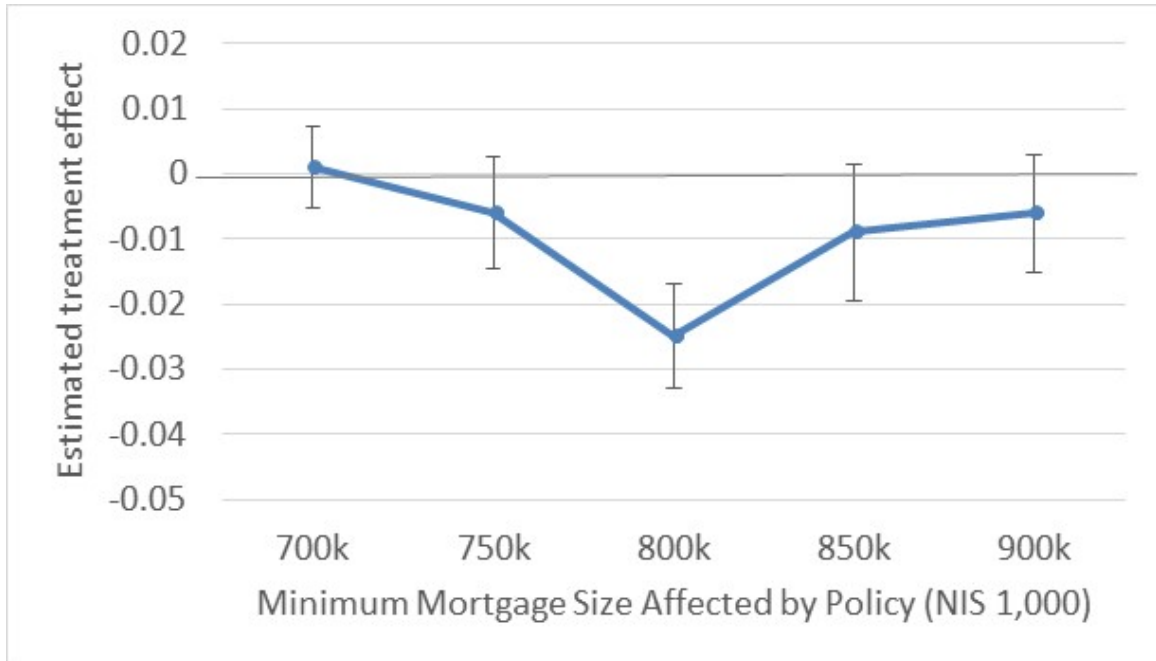


Figure 8
Placebo Treatment Effects.

Note: Figure shows estimated effect on house prices when we make different assumptions about what size mortgages are 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

Average	Summary Statistics		Difference
	Before the LTV Limit	After the LTV Limit	
Home price (NIS thousand)	1015.7	1091.5	75.8***
Price per SM (NIS thousand)	12.0	12.9	0.9***
Rooms	3.56	3.64	0.08**
Area (square meters)	84.2	84.9	0.7***
Building Age	29.1	28.4	-0.7***
Distance from Tel Aviv (KM)	44.8	45.5	0.7***
Neighborhood Ranking	10.3	10.1	-0.1***

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Number of observations: 89,900 (48,719 before the LTV limit, 41,180 after the LTV limit. Source: Israel Tax Authority.

Table 2

The Estimated Effect of LTV limit on Housing Prices

	PPSM	PPSM	PRICE	^{predicted price} PPSM	PRICE
	(1)	(2)	(3)	(4)	(5)
3.rooms_group	-0.240*** (0.006)	-0.109*** (0.006)	0.053*** (0.006)	-0.111*** (0.001)	0.092*** (0.005)
4.rooms_group	-0.431*** (0.009)	-0.191*** (0.008)	0.113*** (0.007)	-0.051*** (0.008)	0.322*** (0.007)
5.rooms_group	-0.612*** (0.009)	-0.255*** (0.009)	0.143*** (0.008)	-0.056*** (0.009)	0.675*** (0.007)
lnage	0.002 (0.002)	-0.003*** (0.001)	-0.011*** (0.001)	-0.006*** (0.002)	-0.008*** (0.001)
Treatment	0.112*** (0.005)	0.421*** (0.017)	0.634*** (0.011)	0.343*** (0.017)	0.682*** (0.016)
After	0.057*** (0.005)	0.096*** (0.004)	0.093*** (0.004)	0.068*** (0.005)	0.053*** (0.008)
TreatmentAfter	-0.043*** (0.012)	-0.029*** (0.009)	-0.024*** (0.008)	-0.025*** (0.008)	-0.027*** (0.005)
Geographic FE	NO	YES	YES	YES	YES
Geographic FEAfter	NO	YES	YES	YES	YES
Constant	2.287*** (0.030)	2.393*** (0.006)	3.650*** (0.011)	2.331*** (0.063)	4.232*** (0.006)
Observations	89,899	89,899	89,899	89,899	89,899
R-squared	0.839	0.895	0.911	0.907	0.918

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. Bootstrapped standard errors are shown in columns 4 and 5. Significance levels 10%, 5%, and 1% are denoted by *, **, and *** and are determined based on the assumption that the bootstrap distribution is normally distributed.

Table 3
*Pre-Trends Test: The Estimated Effect of Constructed Treatment on Housing
Prices One and Two Years Before the Policy*

	01/2009 - 05/2010		01/2008 - 05/2009	
	predicted price		predicted price	
	PPSM	PRICE	PPSM	PRICE
	(1)	(2)	(3)	(4)
3.rooms_group	-0.139*** (0.009)	0.574*** (0.005)	-0.172*** (0.005)	0.237*** (0.002)
4.rooms_group	-0.093*** (0.007)	0.293*** (0.005)	-0.329*** (0.008)	0.448*** (0.009)
5.rooms_group	-0.234*** (0.009)	0.109*** (0.012)	-0.204*** (0.002)	0.574*** (0.004)
lnage	-0.003*** (0.000)	-0.008*** (0.001)	-0.008*** (0.000)	-0.019*** (0.001)
Treatment	0.218*** (0.017)	0.091*** (0.013)	0.082*** (0.013)	0.121*** (0.024)
After	0.056*** (0.004)	0.043*** (0.007)	0.032*** (0.003)	0.028*** (0.008)
TreatmentAfter	0.001 (0.003)	-0.004 (0.004)	-0.005 (0.004)	0.006 (0.005)
Geographic FE	YES	YES	YES	YES
Geographic FEAfter	YES	YES	YES	YES
Constant	1.719*** (0.014)	1.742*** (0.017)	1.596*** (0.015)	2.362*** (0.017)
R-squared	0.901	0.909	0.871	0.910

*Note: Table shows the results of test for parallel pre-trends in the more and less treated samples. In columns 1 and 2, the pre-period is the 10 months from January to October 2009 and the post-period is November 2009 through May 2010, one year earlier than in our baseline specification. In columns 3 and 4, the pre-period is the 10 months from January to October 2008 and the post-period is November 2008 through May 2009, two years earlier than in our baseline specification. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***. Significance levels are determined based on the assumption that the bootstrap distribution is normally distributed.*

Table 4
The Estimated Effect of LTV Limit on Housing Prices: Comparing Transactions with Less Demand Substitution

	Price: NIS 700-800 vs 1333-1400		Price: NIS 600-800 vs 1333-1500		Price: below 800 vs above 1300	
	PPSM	PRICE	PPSM	PRICE	PPSM	PRICE
	(1)	(2)	(3)	(4)	(5)	(6)
3.rooms_group	-0.254*** (0.011)	0.006*** (0.002)	-0.247*** (0.008)	0.028*** (0.003)	-0.112*** (0.007)	0.043*** (0.006)
4.rooms_group	-0.471*** (0.012)	0.011*** (0.002)	-0.460*** (0.010)	0.057*** (0.005)	-0.184*** (0.009)	0.037*** (0.009)
5.rooms_group	-0.622*** (0.015)	0.017*** (0.003)	-0.604*** (0.011)	0.076*** (0.005)	-0.248*** (0.009)	0.058*** (0.010)
lnage	-0.003*** (0.001)	-0.000 (0.000)	-0.003*** (0.001)	-0.001*** (0.000)	-0.003*** (0.001)	-0.011*** (0.001)
Treatment	0.723*** (0.027)	0.686*** (0.004)	0.834*** (0.024)	0.413*** (0.008)	1.052*** (0.024)	0.913*** (0.025)
After	0.027*** (0.004)	0.006*** (0.001)	0.036*** (0.004)	0.013*** (0.002)	0.042*** (0.004)	0.064*** (0.004)
TreatmentAfter	-0.022 (0.015)	-0.011*** (0.003)	-0.014** (0.006)	-0.016*** (0.005)	-0.018* (0.010)	-0.010** (0.004)
Geographic FE	YES	YES	YES	YES	YES	YES
Geographic FEAfter	YES	YES	YES	YES	YES	YES
Constant	2.644*** (0.010)	6.612*** (0.002)	2.591*** (0.007)	6.522*** (0.003)	2.263*** (0.007)	6.089*** (0.007)
Observations	9,058	9,058	18,876	18,876	58,907	58,907
R-squared	0.901	0.991	0.886	0.971	0.881	0.928

*Note: Table shows estimated effects of LTV limit on housing prices, for house prices between NIS thousand 700-800 and 1333-1400 (columns 1 and 2), and house prices between NIS thousand 600-800 and 1333-1500 (columns 3 and 4). Significance levels 10%, 5%, and 1% are denoted by *, **, and *** and are determined based on the assumption that the bootstrap distribution is normally distributed.*

Table 5
The Estimated Effect of LTV limit on Housing Prices, for narrow price ranges

	Price: NIS 600-1400k		Price: NIS 700-1500k		Price: NIS 800-1400k	
	PPSM	PRICE	PPSM	PRICE	PPSM	PRICE
	(1)	(2)	(3)	(4)	(5)	(6)
3.rooms_group	-0.220*** (0.006)	0.064*** (0.004)	-0.237*** (0.006)	0.034*** (0.003)	-0.247*** (0.007)	0.015*** (0.002)
4.rooms_group	-0.400*** (0.007)	0.131*** (0.005)	-0.436*** (0.007)	0.074*** (0.004)	-0.453*** (0.008)	0.040*** (0.002)
5.rooms_group	-0.519*** (0.009)	0.178*** (0.007)	-0.568*** (0.008)	0.102*** (0.005)	-0.595*** (0.009)	0.054*** (0.003)
lnage	-0.003*** (0.001)	-0.003*** (0.001)	-0.001*** (0.000)	-0.002*** (0.000)	-0.003*** (0.000)	-0.001*** (0.000)
Treatment	0.082*** (0.016)	0.072*** (0.011)	0.041*** (0.013)	0.035* (0.017)	0.013 (0.011)	0.020* (0.010)
After	0.051*** (0.003)	0.035*** (0.002)	0.041*** (0.002)	0.024*** (0.002)	0.033*** (0.003)	0.032*** (0.001)
TreatmentAfter	-0.051*** (0.003)	-0.035*** (0.002)	-0.044*** (0.006)	-0.021*** (0.004)	-0.061*** (0.011)	-0.037*** (0.009)
Geographic FE	YES	YES	YES	YES	YES	YES
Geographic FEAfter	YES	YES	YES	YES	YES	YES
Constant	2.660*** (0.036)	6.582*** (0.032)	2.722*** (0.030)	6.708*** (0.018)	2.790*** (0.025)	6.788*** (0.013)
Observations	46,344	46,344	44,037	44,037	33,354	33,354
R-squared	0.834	0.849	0.848	0.904	0.851	0.920

*Note: Table shows estimated effects of LTV limit on housing prices, for narrow house price ranges. Columns 1, 2, and 3 present the main specification for house prices between NIS thousand 600-1400, 700-1500, and 800-1400, and for price per square meter (PPSM) and price as our dependent variable. Significance levels 10%, 5%, and 1% are denoted by *, **, and *** and are determined based on the assumption that the bootstrap distribution is normally distributed.*

Table 6

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

	Low-Graded Areas	High-Graded Areas
	(1)	(2)
	PPSM	PPSM
3.rooms_group	-0.121*** (0.009)	-0.096*** (0.008)
4.rooms_group	-0.181*** (0.011)	-0.201*** (0.010)
5.rooms_group	-0.238*** (0.013)	-0.267*** (0.011)
ln_age	-0.006*** (0.002)	-0.002* (0.001)
Treatment	0.195*** (0.030)	0.186*** (0.020)
After	0.106*** (0.005)	0.080*** (0.005)
Treatment#After	-0.040*** (0.009)	-0.016*** (0.005)
Geographic FE	YES	YES
Geographic FE#After	YES	YES
Constant	2.271*** (0.008)	2.502*** (0.009)
Observations	42,094	47,805
R-squared	0.838	0.844

*Note: Table shows estimated effects of LTV limit on housing prices by neighborhood quality, which ranges from 1 to 20. Low-quality areas (column 1) are neighborhoods that are graded from 1 to 10, and high-quality areas (column 2) are those graded from 11 to 20. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***.*

Table 7

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

	Low-Graded Areas	High-Graded Areas
	(1)	(2)
Jerusalem	-0.029*** (0.006)	-0.018*** (0.003)
North	-0.009 (0.011)	0.007 (0.008)
Haifa	-0.014*** (0.002)	-0.003 (0.003)
Center	-0.071*** (0.013)	-0.042*** (0.009)
Tel-Aviv	-0.085*** (0.020)	-0.051* (0.028)
South	-0.002 (0.003)	0.002 (0.003)

*Note: Table shows estimated effects of LTV limit on housing prices by neighborhood quality for each CBS region in Israel. Low-quality areas are neighborhoods that are graded from 1 to 10, and high-quality areas are those graded from 11 to 20. Significance levels 10%, 5%, and 1% are denoted by *, **, and ****

Table 8

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

	(1)	(2)	(3)
3.roomsgroup	-0.677*** (0.178)	-0.840*** (0.167)	-0.605*** (0.007)
4.roomsgroup	-0.686*** (0.180)	-0.999*** (0.170)	-0.708*** (0.003)
5.roomsgroup	-0.652*** (0.182)	-1.144*** (0.174)	-0.519*** (0.002)
lnage	0.005*** (0.000)	0.005*** (0.000)	0.005*** (0.000)
price		0.001*** (0.000)	0.001*** (0.000)
price sq'			-0.001*** (0.000)
Treatment	0.076*** (0.015)	0.007 (0.016)	0.008 (0.011)
After	0.134*** (0.008)	0.053*** (0.009)	0.044*** (0.003)
TreatmentAfter	-0.069*** (0.026)	-0.075*** (0.024)	-0.054*** (0.020)
Constant	3.016*** (0.186)	2.848*** (0.172)	1.809*** (0.170)
Observations	33,311	33,311	33,311
R-squared	0.514	0.550	0.586

*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 ***.*

APPENDIX A 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:

$$\ln(\hat{p}_i) = \alpha + \hat{\beta}X_i + month_i + \epsilon_i \quad (C.1)$$

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 X_i 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 1, but using the predicted price (\hat{p}) rather than the actual price:

$$Treat(\hat{p}) = \sum_{LTV=0.6}^1 I(\hat{p} * LTV > NIS800,000) * f(LTV) \quad (C.2)$$

Results using this alternative treatment effect are shown in columns 3 and 4 of Table 9.

APPENDIX B 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 an additional robustness check we 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, analogous to our main specification, we focus on the difference between houses above and below these cutoffs, before and after the policy was implemented.

This approach uses an estimating equation of the form:

$$\ln(PPSM_{it}) = \alpha + \hat{\beta}X_i + \Gamma * \theta_t + \delta * AboveThreshold(p) + \sigma * AboveThreshold(p) * \theta_t + \epsilon_{it} \quad (C.3)$$

The results are shown in Table 9. 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 an 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 be 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 1.5 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. The fact that these results are smaller than our baseline estimate is as expected. In this robustness test with a single cut-off, we are essentially measuring the effect on prices only from the subset of potential buyers who would choose a particular LTV ratio, whereas in our main results, we measure the effect from all potential buyers. Regardless of the quantitative differences, this exercise shows that a simpler difference-in-difference specification also supports the conclusion that this MPP had a small negative effect on house prices.²⁵

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

²⁵Using a single LTV ratio closely mirrors the approach of [Adelino et al. \(2012\)](#). When we use the exact specification in [Adelino et al. \(2012\)](#) the results are little changed.

the three different thresholds is even larger than for the more narrow time period.

Table 9

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_group	-0.233*** (0.005)		-0.230*** (0.005)		-0.229*** (0.005)	
4.rooms_group	-0.444*** (0.006)		-0.437*** (0.007)		-0.434*** (0.006)	
5.rooms_group	-0.610*** (0.007)	-0.182*** (0.003)	-0.606*** (0.007)	-0.182*** (0.003)	-0.605*** (0.007)	-0.179*** (0.003)
ln_age	-0.003*** (0.000)	-0.006*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.006*** (0.000)	-0.003*** (0.000)
price	0.001*** (0.000)	0.000*** (0.000)	0.0018*** (0.000)	0.000*** (0.000)	0.001*** (0.000)	0.000*** (0.000)
price sq'	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.000*** (0.000)	-0.000* (0.000)	-0.001*** (0.000)
1.above_threshold	-0.107*** (0.007)	0.006 (0.004)	-0.068*** (0.007)	0.032*** (0.004)	-0.047*** (0.007)	0.042*** (0.004)
1.after	0.043*** (0.002)	0.027*** (0.003)	0.049*** (0.002)	0.029*** (0.004)	0.051*** (0.003)	0.034*** (0.004)
1.above_threshold#1.after	-0.033*** (0.003)	-0.017*** (0.003)	-0.036*** (0.003)	-0.016*** (0.004)	-0.037*** (0.003)	-0.019*** (0.005)
Constant	1.754*** (0.002)	1.634*** (0.002)	1.768*** (0.002)	1.653*** (0.002)	1.780*** (0.002)	1.644*** (0.001)
Observations	89,899	29,667	89,899	29,667	89,899	29,667
R-squared	0.912	0.914	0.896	0.917	0.902	0.899

*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 only four and five room units. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***.*