The Real Consequences of LTV Limits on Housing Choices

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Abstract

This paper provides novel evidence on the effects of LTV limits on housing choices. I exploit the 2010 and 2012 introduction of LTV limits in Israel using a detailed loan-level dataset. I find that the LTV limits, which were designed to lower borrowers' risk, resulted in borrowers choosing more affordable housing units, farther from the central business district, and in lower socioeconomic neighborhoods. In addition, those LTV limits increased interest rates and reduced loan amounts. My evidence suggests that macroprudential policies, which focus on the stability of the financial system, thus have micro-implications on location choices, commuting costs, and moving to less-advantaged areas.

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More than a decade after the Great Recession of 2008, drivers and implications of the crisis are coming into better focus. Mortgage lending and housing markets played central roles in the financial crisis: they contributed to the drop in consumer spending and in output growth, led to higher unemployment, and slowed the recovery (Mian and Sufi (2011), Mian, Rao and Sufi (2013), Mian, Sufi and Verner (2017)). As a result, policy makers in many advanced economies have implemented macroprudential policies (MPPs) aimed at limiting household leverage in order to slow down the feedback loop between credit market and home prices. Despite the increasing adoption of MPPs, chiefly in implementing loan-to-value (LTV) limits,[1] and considerable empirical evidence demonstrating that LTV limits reinforce bank stability by reducing risks from borrowers in case of sharp housing price declines (Akinci and Olmstead-Rumsey (2018), Cerutti, Claessens and Laeven (2017)), the transmission channels of LTV limits on housing choices are not well explored.

This paper advances our understanding of the effect of financial regulation on the real economy. Combining a unique property-level characteristics dataset with detailed loan-level data that includes borrower characteristics, I study the Bank of Israel's introduction in 2010 and 2012 of two types of loan-to-value limits. The 2010 limit, which increased risk weights on a certain LTV threshold (soft LTV limit), operated as an implicit tax on lenders when they originated loans with high LTV ratios. The 2012 policy limits the LTV ratio to three different thresholds, according to buyer type (strict LTV limit).

I find that due to the LTV limits, affected borrowers bought cheaper houses that were smaller, farther from Tel Aviv central business district (CBD), and in lower socioeconomic neighborhoods. These macroprudential policies, which focus on the stability of the banking system, thus have micro implications for the housing market. When home buyers move farther from the center of Israel to lower-graded neighborhoods, the implications on commuting costs, education levels, the labor market, and other economic factors are substantial. I also find that Israel's LTV limits increased economic burden: the higher costs imposed on lenders in the soft LTV limit were passed to borrowers in the form of higher interest rates.[2] This increase in the interest rate induced some

[1] Alam et al. (2019) collect data from 1990 to 2016 on the adoption of 17 types of MPPs in 134 countries and find that LTV limits are the most widely used tool in advanced economies (adopted by 60 countries).

[2] Other ex-ante restrictions on household leverage, such as qualified mortgages program in the United States, studied by DeFusco, Johnson and Mondragon (2020) operate similarly by penalizing lenders for issuing loans with certain risky characteristics.

borrowers to reduce their leverage, leading to a decline in the amount borrowed.

The purpose of imposing an LTV limit is to increase borrower resilience and lower bank losses during downturns by requiring a higher equity stake and lower leverage (see, for example, Campbell and Cocco (2015) for theoretical arguments and Demyanyk, Koijen and Van Hemert (2011) for empirical evidence). The initial limitation, in 2010, increased the capital provision for mortgages with an LTV limit of greater than 60% (soft LTV limit). This guideline did not apply to housing loans originally amounting to less than NIS 800,000 (approximately USD 200,000). The 2012 policy limits the LTV ratio according to buyer type (strict LTV limit): to 75% for those acquiring a first home (henceforth "first-time home buyers"), 70% for borrowers needing to sell their existing home within 18 months ("upgraders"), and 50% for borrowers buying a home for investment purposes ("investors").

To identify the effects of the LTV limits on the credit and housing markets, I use a difference-in-differences (DID) matching research design. In the soft LTV limit, ex post, a borrower could have taken an LTV of greater than 60% and paid a higher interest rate, or he or she could have chosen to buy a different asset with an LTV of less than 60%. To estimate the effects, I compare identical households whose loan amounts are "just below" or "just above" the NIS 800,000 mortgage constraint. The two groups were equally affected by macroeconomic events, but only one group was affected by the LTV limit. In the strict LTV limit, ex post, the borrower could not borrow above the cutoff. To overcome the challenge of identifying the affected borrowers, I predict the LTV ratio that the borrower would have chosen in the absence of the limitation. To identify the treated borrowers, I examine households that are (slightly) below the LTV cutoff after the policy and match them the closest household from the period before based on borrower age and income. Then, I divided borrowers into two groups: the control group, households that chose the same LTV ratio before the policy, slightly below the cutoff, and the treatment group, households that chose to be above the LTV cutoff before the limitation.

I find that both types of LTV limits increased mortgage prices. However, using the variation across the 60% LTV threshold, I can better identify the effect on the interest rate in the soft LTV limit than in the strict LTV limit. I do this using the difference in the interest rate paid by two identical borrowers, that is having similar observable characteristics, one with an LTV ratio slightly

below 60% and the other with a ratio slightly above this threshold. Before the 2010 regulation these two borrowers paid the same interest rate. After it, the interest rate paid by a borrower with an LTV ratio just above 60% was, on average, 0.21-0.36 percentage points higher than the interest rate paid by a borrower with identical characteristics just below the limit. In terms of interest rate payments, the soft LTV limit increased the yearly payment, on average, by 2,700-3,250 NIS (4% of average household gross yearly income). This increase in the interest rate may have induced some borrowers to reduce their leverage. In line with this conjecture, the distribution of LTV ratios moved significantly toward lower values after the imposition of the LTV limit, suggesting that some borrowers decided to lower their LTV ratios, mainly borrowers with higher income. In terms of the strict LTV limit, after the limitation there was high density just below the value of the cutoff, for each borrower type.

I argue that the increase in interest rates for high LTV loans primarily reflects the pass-through to borrowers of lenders' increased origination costs. However, another interpretation is that the results reflect borrower selection. In other words, does the distribution of borrowers' characteristics change after the imposition of the LTV limits, or do banks now avoid giving loans to certain households? In the Israeli case of implementing soft and strict LTV limits, there is no support for credit rationing, that is, there is no significant change in the distribution of borrowers' age and income after the imposition of each LTV limit.

My results show that the LTV limits caused some borrowers to buy lower quality assets: cheaper, smaller, farther from the CBD, and in lower socioeconomic neighborhoods. The 2012 strict LTV limit had a much greater impact on borrowers' choices in the credit and housing markets than the 2010 soft LTV limit, probably because the strict limit did not give them a choice to stay highly leveraged.[3] This is especially true for investors, who changed their choices much more than the other types of borrowers. These results are promising in terms of financial stability: if investors have a higher impact on housing prices during a housing boom, MPPs will not stop them from

[3]I assume that the housing supply is inflexible, at least in the short term. I also examine only the choices in the housing market of the affected borrowers, which is a subgroup of the buyers with a mortgage (which in turn is a subgroup of home buyers). Hence, such questions as who bought the remaining dwellings in the center of Israel (which the treatment group could not afford because of the limit), what happened to the prices of those dwellings, and whether the LTV limit had an effect on the supply of housing (e.g., did developers start to build more homes in the periphery or smaller homes) are not within the scope of this paper.

entering the housing market but may lead them to lower their leverage and buy cheaper houses. This is in line with Igan and Kang (2011), who suggest that investors are more flexible in their purchasing decisions than first-time home buyers, who usually have limits that require them to purchase properties in particular locations, such as the need to be near their parents or workplace.

To understand the magnitude of the changes in housing choices resulting from the imposition of the 2010 and 2012 LTV limits, we need to understand that 55% of the Israeli population lives in the center of the country, within a 40-kilometer (km) radius of Tel Aviv. In the first 6-10 months after the imposition of the respective LTV limits, affected borrowers moved, on average, 3.8 km under the soft limit and 7.1 km under the strict limit, farther from Tel Aviv to significantly lower-rated neighborhoods. Although the average move was 4.3-7 km from Tel Aviv, almost 20% of borrowers moved 5-10 km, and almost 17% moved 10-20 km farther from the center; overall 70% of the borrowers moved farther from the center of Israel. Moreover, within each subgroup of distance from Tel Aviv, more borrowers moved to lower-graded neighborhoods than to higher-graded areas. In addition, I use Google Location History data and measured commuting time from different locations in Israel to the CBD. According to Google Maps, moving farther from the center by 4.3-7.1 km adds, on average, 40 minutes to 1 hour of daily commuting time.

However, the imposition of a strict LTV limit on new contracts yielded counterintuitive results in the credit market. Although it was supposed to lower borrowers' risk and therefore improve their credit conditions, the results show that affected borrowers paid a higher interest rate and increased their term to maturity. This outcome has three possible explanations. The first explanation is taken from de Araujo, Barroso and Gonzalez (2020), that the banks may have changed their risk perception due to the strong signal from the macroprudential supervisory authority about a buildup of systemic vulnerability among highly leveraged borrowers, which in turn affected banks' risk pricing. Second, because the affected borrowers bought riskier assets (i.e., farther from the center), the interest rate on those loans was higher. Third, because of the LTV limit, the affected borrowers may have borrowed money from other sources, or used other credit such as consumer credit, which increased their monthly loan payment. To keep the monthly mortgage payment constant, the affected borrowers needed to increase their term to maturity, which in turn caused the mortgage interest rate to rise. Regarding the last explanation, MPPs typically target large

financial intermediaries (e.g., banks) by enforcing LTV limits. This focus may be too narrow if households have access to alternative, unregulated credit channels that allow them to increase leverage by shifting the demand to consumer credit (Buchak et al. (2018)).[4] To the best of my knowledge, Braggion, Manconi and Zhu (2018) is one of a very few studies that uses loan-level data to examine the shift from mortgages to other credit channels following the imposition of LTV limits.[5] Consumer credit in Israel is not subject to the same scrutiny as mortgages, and therefore may pose new risks to the financial system. In particular, it may lead to an excessive buildup of household leverage, as happened in the US subprime crisis.

This paper contributes to a large literature evaluating the effects of financial regulation on the real economy and, more specifically, emphasizing the unintended consequences of LTV limits in terms of pushing borrowers farther from the city center to less desirable neighborhoods. It also contributes to the growing literature on MPPs by providing a credible estimate of the impact of LTV limits, the most common MPP tool, on borrowers' reactions. Most studies use macroeconomic and cross-country data, and face problems of identification, controlling for country characteristics, and assessing the distributional effects (e.g., Crowe et al. (2013), Lim et al. (2011)). This paper is one of a few to use a loan-level dataset merged to a housing characteristics dataset to show that MPPs have micro effects on borrowers' choices in the housing market (e.g., Igan and Kang (2011), Han et al. (2017), Van Bekkum et al. (2019)). This study is the first to examine the impact of an LTV limit on housing unit characteristics other than price, such as asset size and location. Finally, LTV limits are occasionally criticized for preventing groups that need more access to credit markets from obtaining loans. The household-level database I use enables me to evaluate the impact of an LTV limit on different segments of the population, especially on those with limited access to credit. As such, my study is also related to the literature on the broader regulation of consumer financial products and consumer protection in household finance (Campbell et al. (2011), Posner and Weyl

[4] Cizel et al. (2019) show evidence of leakages to the shadow banking sector. Using cross-country data, they show that when macroprudential measures apply only to banks, they may be circumvented by nonbank lending, hence the necessity of extending MPP beyond banking.

[5] They study the impact of peer-to-peer lending on household leverage and exploit two policy interventions in the market for real estate mortgages in a number of major Chinese cities, which at first increase (in 2013) and later reduce (in 2015) the demand for peer-to-peer lending, while leaving overall credit demand unchanged. They find that peer-to-peer lending led to excessive levels of household debt and undermined policy interventions in the credit market after the intervention.

(2013)).

The rest of the paper proceeds as follows. Section I provides background on the housing market and housing finance in Israel. Section II discusses data sources and summary statistics. Section III describes the identification approach. Section IV reports results on the impact of LTV limit on borrowers' choices in the credit and housing markets. Section V discusses other potential effects of LTV limits and Section VI concludes.

I. Background

A. Housing and Credit Markets in Israel

Israel's financial system was not markedly affected by the recent Great Recession. However, Israel's inability, as a small and open economy, to disassociate itself from the low level of global interest rates contributed to a trend of rising asset prices, especially housing values.[6] Between 2007 and 2016, nominal housing prices rose by 128%, and real housing prices rose by 95%. At the same time, the volume of housing loans increased by 95%, raising concerns among policy makers. Housing prices and mortgages tend to move together and influence each other in a two-way feedback loop, a phenomenon widely described in the literature (Crowe et al. (2013)). In view of these trends, between 2010 and 2014 the supervisor of banks in Israel adopted a number of MPPs to maintain financial stability and to address the development of systemic risk in the housing market. These measures were intended not only to prevent households from overleveraging when purchasing homes, which could affect buyers' ability to make future repayments, but also to slow the pace of home price increases.

Figure 1 shows the rate of change in housing prices in Israel and, depicted in vertical lines, the various MPP tools employed (see Appendix A for a detailed timeline). This paper focuses on a short period of time before and after each LTV limit, colored in red in the graph. There was a slower rate of increase in housing prices around the soft LTV but the challenge is to isolate the

[6] Other factors contribute to the increase in housing prices, such as demographics, rising household income, bureaucratically constrained rates of construction, etc. During the same period, many countries that had not experienced a housing price boom in the pre-crisis era also saw a rise in housing prices (e.g., Germany, Switzerland), but the degree of the increase in housing prices observed in Israel stands out (Cerutti, Dagher and Dell'Ariccia (2017)).

impact of the MPPs on the housing market from other macroeconomic events that occurred around the same time.

B. The LTV Limits

B.1. Soft LTV Limit

The soft LTV limit was the first of these MPPs. 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, required capital provisions for mortgages with variable interest-rate portions of 25% or more and LTV ratios greater than 60% rose from the existing 35-75% (depending on the loan characteristics) to 100%.[7] Importantly for my analysis, the guidelines applied only to mortgages larger than NIS 800,000 (approximately USD 200,000). I focus on the period between January 2010 and May 2011 (the first red area in Figure 1), when the Banking Supervision Department applied another important MPP tool ("Variable Interest Rate" in Figure 1). The time span thus includes the 10 months before the soft LTV limit and the seven months after. Since the soft LTV limit forced the banks to tie up more capital against these high LTV loans, borrowers wishing to take out a loan with an LTV greater than 60% faced higher interest rates, which incentivized them to lower their leverage. As Figure 2 shows, some borrowers continued to be highly leveraged (and paid a higher interest rate), whereas others, due to the increase in the interest rate, had their leverage lowered to 60% (or less). Figure 2 therefore presents the LTV distribution after the limit with two peaks, one around an LTV ratio of 75% and the other around an LTV ratio of 60%.

B.2. Strict LTV Limit

The strict LTV limit was implemented in November 1, 2012. It established that a banking corporation could not approve a mortgage with an LTV ratio of more than 70% excluding a housing loan granted to a borrower for the purpose of acquiring a first home, to which a maximum LTV ratio of 75% would apply. In addition, the directive established that a banking corporation could

[7] Israeli households are not very indebted, and LTV ratios on mortgages are relatively low: in 2010, the average LTV ratio was 52%.

not approve a loan with an LTV ratio above 50% to a borrower for the purpose of purchasing an investment home.[8] The directive was intended to reduce the significant effects of the realization of a crisis in the real estate market, by reducing the risks inherent in the housing credit portfolio and in taking out a housing loan with a high LTV ratio. Importantly, the new directive was unexpected from the standpoint of market participants, because regulators in Israel had never before mandated strict LTV limits. Moreover, prior regulation had strongly favored regulatory capital measures using risk weights (soft LTV limit). I focus on the period between January 2012 and September 2013 (the second red area in Figure 1), when the Banking Supervision Department applied another important MPP tool ("PTI+Duration" in Figure 1). The time span includes the 10 months before the new LTV limit and the 10 months after it. Figure 3 illustrates the distribution of LTV ratio of new housing loans granted before the LTV regulation (January 2012 to October 2012) and after it (November 2012 to September 2013). Before the LTV regulation, the distribution of the LTV ratio was highly concentrated in the 70-75% range due to the adoption of the Basel II Standard Approach Guidelines in 2008, and at 60% due to the implementation of the soft LTV limit in October 2010, which increased the capital requirement for loans with LTV ratios above 60%. Figure 3 clearly shows how the 2012 LTV limit changed the LTV ratio distribution: after the strict LTV limit there is a high concentration of density around the three LTV limits: 75%, 70%, and 50%. According to the Kolmogorov-Smirnov test, the two distributions are statistically significantly different.

II. The Database

A. Data Construction

This study uses loan-level data provided by the Bank of Israel. They contain information on all housing loans issued by seven commercial banks in Israel. Together, these lenders account for roughly 95% of all mortgage loans in Israel, making this a rich source of data. Information is provided on current mortgage balance, date of issue, date of acquired asset, bank, interest rate, duration, LTV ratio, and value and location of acquired property. Certain borrower characteristics

[8] An investment home is defined as a second home in accordance with reports to the Israel Tax Authority, as well as any dwelling acquired by a nonresident.

are also reported in the data, such as whether the application is a joint or single assessment and the borrower's age and monthly income.

I focus on the time framing the soft LTV limit and strict LTV limit (colored in red in Figure 1). My motivation for observing a short time period around the limitation is to test a time frame that is relatively free of external shocks that might influence the results, such as macroeconomic events or additional MPPs imposed afterward (as shown in Figure 1).

I merge the loan-level dataset with another dataset, CARMAN, from the Israel Tax Authority which contains information on all home sale transactions and their characteristics. In the CARMAN dataset, each housing unit has a unique ID. Because the recording of the IDs in the mortgage dataset is distorted, I perform not a direct merger but a statistical one, using other remaining fields in the two datasets: price, date, and city of the purchased asset (see Appendix B for a description of the merging process). In summary, I merge the original mortgage dataset with one-third of observations from the CARMAN dataset. Following preliminary tests to determine that the observations in the mortgage dataset that are matched to the CARMAN dataset are similar in character to the entire class of mortgage observations, and that these matched observations are similar in character to all the observations in the CARMAN dataset (Appendices C and D), a Kolmogorov-Smirnov test of equality distributions shows no significant difference between the merged observations and the overall dataset.

B. Summary Statistics

The dataset is divided into two periods: before and after the imposition of each LTV limit. There are 16,100 observations before the soft LTV limit (January 2010 to October 2010) and 11,224 observations after (to May 2011). There are 17,260 observations before the strict LTV limit (January 2012 until October 2012) and 16,761 observations after it was imposed (until August 2013). Table I shows descriptive statistics of mortgage contracts, borrower characteristics, and home purchase transactions before and after the imposition of the soft LTV limit. Some of the main results in this paper are evident in these sample statistics. The average interest rate increased after the LTV limit, whereas the average LTV ratio decreased. At the same time, the age of the borrowers decreased, whereas income increased slightly. The statistics also show that borrowers

bought assets that were cheaper (in real terms), smaller (not significantly), and farther from Tel Aviv (the business capital of Israel) after the LTV limit.

The Israel Central Bureau of Statistics (CBS) publishes a socioeconomic index of neighborhoods consisting of 16 variables, such as demographic, education, employment, income, and standard of living. These 16 variables are compiled into an index, and all neighborhoods in Israel are classified into one of 20 clusters, with 1 being the lowest socioeconomic status and 20 being the highest. Table I shows a decline in the socioeconomic level of neighborhoods after the LTV limit. Households moved farther from the CBD to lower-graded neighborhoods. The econometric challenge in this paper is to attribute these changes in housing preferences to the LTV limit.

Table II presents summary statistics on mortgage contracts, borrower characteristics, and home purchase transactions before the imposition of the strict LTV limit for each borrower type: first-time home buyers, upgraders (who need to sell their existing home within 18 months), and investors (who own two or more homes). In general, first-time home buyers buy cheaper assets, have a lower income, are younger, have a higher LTV ratio, and have a longer loan duration than the other two groups. Investors buy smaller assets that are farther from Tel Aviv. Table II shows the socioeconomic level of neighborhoods for all three groups, and as expected, the upgraders bought assets in higher-graded neighborhoods.

On average, investors take out smaller loans with a shorter duration and have a lower payment-to-income ratio, compared to first-time home buyers. Investors have a higher income (75% are from the income deciles 6 to 10) and are older (the average age in these income deciles is 47). Although investors are less risky borrowers in all of those risk indicators (leverage, loan duration, age, and income), they pay a higher average interest rate than the other two groups. This is consist with the disagreement in the literature regarding the risk inherent in real-estate investors (compared first-time home buyers); Agarwal, Badarinza and Qian (2018) find that investors have a better risk profile ex ante: relative to first-time home buyers, investors are older, are better educated, earn higher incomes, and have longer tenures with the bank. In contrast, Haughwout et al. (2011) find that investors are riskier because they are the first to dampen their homes in a case of a bust, as seen in the Great Recession, where in US states that experienced the largest housing booms and busts, at the peak of the market almost half of purchase mortgage originations were associated with

investors and those investors contributed to higher rates of default.

Appendix E presents summary statistics for leveraged borrowers before strict LTV limit was imposed: first-time home buyers with an LTV ratio above 75%, upgraders with an LTV ratio above 70%, and investors with an LTV ratio above 50% limit. Overall, the leveraged borrowers account for 19% of borrowers: 12.4% of first-time home buyers and 12.3% of upgraders have LTV ratio above these limits before the policy intervention, while among investors the rate is 60.1%.[9] On average, affected borrowers of each borrower type bought cheaper assets, had lower income, and were younger, compared to the average borrower. Turning from borrower characteristics to housing choices, the table shows that affected borrowers of each type bought smaller houses, farther from the center, and in lower-graded neighborhoods, compared to the average borrower.

III. Identification Approach

A. Soft LTV Limit: Difference-in-Differences Matching

There is no information in the dataset about the decisions of households before and after the imposition of the soft LTV limit, just at one point in time following it. Hence, the challenge is to find and compare households with similar characteristics before and after the imposition of the limit and compare their choices in the housing and credit markets.

Let Y_i denote the choice that a household or borrower (i) made, such as the price, size, or location of the housing unit. Let T_i denote the treatment defining all households after October 2010 as being subject to the treatment (until the end of April 2011), that is T_i. In the case of households that borrowed before October 2010, T_i=0. Where Y_i is the expected difference between the choices of households under the limit and the choices made without being subject to the limit. For example,

$$Y_1 = E(Y_{i1} \mid T_i = 1) - E(Y_{i0} \mid T_i = 1). \tag{1}$$

The Average Treatment Effect (ATE) is useful in evaluating the expected effect that randomly assigning individuals in the population to the treatment has on the outcome. Therefore, the naive

[9] As can be seen in Appendix E, the average LTV ratio for investors is 52.5%, above the limit cutoff.

estimator will be:

$$Y_1 = E(Y_i \mid T_i = 1) - E(Y_i \mid T_i = 0) \tag{2}$$

Abadie and Imbens (2011) note that the ATE might not be relevant for policy makers because it includes the above effect on persons for whom the program was never intended. The Average Treatment Effect on the Treated (ATT) is useful in evaluating the above effect on those for whom the program is actually intended. In what follows I will consider the ATT the parameter of interest in most evaluation studies.

Because choosing housing assets and getting a mortgage are not random decisions but are correlated with the household's means and the affordability of the dwelling, this can cause a bias in estimating the impact of LTV limits on the choices households make when purchasing a home. The matching method helps solve this problem by assigning each observation in the treatment group to the closest observation in terms of observable characteristics in the control group. First, I match the treated household with a similar household that is not treated based on observable characteristics, and then I compare the results for the paired households by estimating the average effect of the treatment group to obtain the ATT for those in the treatment group (Atanasov and Black (2016)).

The pairs of households before and after the imposition of LTV limits are matched on the basis of their observed characteristics. The variable X denotes age and income at the initial stage. I focus the ATT parameter on the treatment group for an individual with characteristics X:

$$ATT = E(Y_1 - Y_0 \mid T = 1, X) \tag{3}$$

Y_1 and Y_0 are the outcome variables for households that are treated and those that are not treated, respectively (Abadie and Imbens (2006), Abadie et al. (2004)).[10] For each i, matching

[10]The matching method rather than propensity score matching is used when the database is large and there are a small number of observable variables, similar to the situation here. The calculations were made using STATA software employing the command Nnmatch (Nearest-neighbor matching), explained in detail in Abadie et al. (2004). The Nnmatch command developed in Abadie and Imbens (2006) allows for matching with replacements, which can be termed the Abadie-Imbens variable. This lowers the bias and leads to greater similarity between the observations, although it does increase the variance. In addition, when doing matching with replacement, the order in which the observations are matched is not important. I assume unconfoundedness (selection of observables or conditional

estimators impute the missing outcome by finding other individuals in the data whose covariates are similar but who are exposed to the other treatment. In this way, differences in outcomes between this well-selected and hence adequate control group and the treatment group can be attributed to the treatment.

To determine an exact match, or at least a close one for a given unit, I set a distance matrix that quantifies the differences between pairs of observations, such as between unit i from the treatment group and j from the control group, according to the observed characteristics. The greater this difference, the less similar those observations will be in one or more of the characteristics. The estimate of Abadie-Imbens minimizes the Mahalanobis distance of the observed characteristics vector between the control group and the treatment group. This estimate finds exact pairings on categorical variables, but the pairings according to the continuous variables will not be exact, although they will be very close. I recognize this issue and implements a bias-correction component to the outcome variables.[11]

The soft LTV limit required banks to increase capital provisions only for mortgages exceeding NIS 800,000. Accordingly, I examine two groups of borrowers: those who borrowed NIS 600,000 to 700,000, just below the threshold (the untreated group), and those who borrowed NIS 900,000 to 1,000,000, just above the threshold (the treatment group). Even though there is a low probability of overlap between the two groups, the groups are not very different in terms of observable characteristics.[12]

Figure 9 shows that the soft LTV limit affect the treated group, resulting in a change in the distribution of LTV ratios to significantly lower values according to the Kolmogorov-Smirnov test, while there is no change in the distribution of LTV ratios in the control group.

"Before the limit" is the time period between the beginning of 2010 and October 2010 (when the soft LTV limit was implemented), and "after the limit" is between November 2010 and April 2011 (before the next MPP tool was implemented). Then, the matching process is compared between

independence). The intuition for unconfoundedness is that if the decision to take the treatment is purely random for individuals with similar values of pre-treatment variables, then I can use the average outcome of some similar individuals who were not exposed to the treatment.

[11] For more details, see Abadie et al. (2004).

[12] Other groups have also been tested, such as NIS 650,000-750,000 versus NIS 850,000-950,000, and I find similar results, but because of the trade-off between observing similar groups while rolling out overlapping between the groups, I elected to stay with the NIS 600,000-700,000 versus NIS 900,000-1,000,000 groups.

the treatment group and control group. So the ATT now is calculated as:

$$ATT = (After - Before)_{treated} - (After - Before)_{control}. \quad (4)$$

The treatment group includes 1,498 observations (844 observations before the imposition of the LTV limit and 654 observations after), and the untreated group includes 3,462 observations (2,023 observations before the imposition of the LTV limit and 1,439 observations after). The control group, comprising those observations that are matched to the treatment group by observable characteristics, includes 1,498 observations (895 observations before the LTV and 603 after). The outcome variables are: real home price (in NIS), home size (in square meters), the number of rooms in the home, the distance of the property from Tel Aviv (km), and the socioeconomic level of the neighborhood (scale of 1 to 20).

Table III presents the characteristics of the two groups before the matching process. Borrower characteristics differ significantly between the treated groups. The initial goal is to demonstrate that the matching process is effective in matching the treatment group to the control group (which is the result of matching the treated group to the untreated group) along the dimensions of the explanatory variables. According to the results presented in Table III, the matching process results in insignificant differences in the observed characteristics between the groups. There is thus no significant difference between the treatment group and the control group.

A major concern in the matching process is that certain population groups will be omitted from the sample. Namely, if the matching is done by age and income, and the age and income of borrowers changed over time, income groups could be omitted from the sample because only half of the control group is matched.

B. Strict LTV Limit: Predicted LTV Ratio Using DID Matching

This study focuses on the LTV policy's effect on the subset of borrowers constrained by the limit. However, the treatment status can be observed only before the policy intervention. After the intervention, it is no longer possible to distinguish easily between affected and non-affected borrowers. In the case of the strict LTV limit, it is even harder to distinguish between the two

groups: all borrowers are below the limit after the intervention, and we do not know if the borrower wished to borrow more but could not do so because of the intervention. As Abadie (2005) notes, one way around this missing data issue (i.e., what LTV ratio the borrowers would have chosen in the absence of the LTV limit) is to determine the treatment status of the post-program sample from "some individual characteristic observed in both periods."[13]

The DID matching approach identifies the affected borrowers using prediction of the LTV ratio that the borrower would have chosen without the limitation. By this method, only households that are (slightly) below the cutoff[14] after the policy are examined, and the closest household from the period before the policy is matched with each borrower, based on observed characteristics (age and income).[15] Then, the leverage choices of the matched borrowers, their actual choices before and after the policy, can be examined. Observations with an LTV ratio, before the policy, that is far from the cutoff are excluded, because the objective is to examine two groups that are near the cutoff (local treatment effect). Then, only two groups remain: households that chose the same LTV ratio before and after the policy, which is slightly below the cutoff (the control group), and households that chose to be above the LTV cutoff before the policy (the treatment group).

The outcomes in the empirical application refer to mortgage contract terms (interest rate, maturity, default rate) and to the characteristics of the housing unit bought with the mortgage (price, size, location, and neighborhood socioeconomic level). To obtain the outcomes, I use the Abadie and Imbens (2011) estimator, enables me to match households based on their income and age; since income and age are continuous variables, they will probably not yield an exact match (though it should be close). To overcome this difficulty, I apply a "bias-correction" component to the interest rate estimates, by focusing on the average treatment effect on the treated[16] (ATT) for an individual with characteristics X (age and income):

[13] See also Manski and Pepper (2013).

[14] The assumption here is that those who wish to borrow above the cutoff after the intervention will borrow slightly below the cutoff. See, e.g., Igan and Kang (2011).

[15] de Araujo, Barroso and Gonzalez (2020) use income as the only predictor of the LTV ratio. Other borrower characteristics including number of family members and previous place of residence (as indicators of socioeconomic status and residential preferences, respectively) have been used to predict the LTV distribution, but income and age have been found to have greater explanatory power.

[16] See Abadie et al. (2004) for a review.

$$ATT = E(Y1 - Y0)|T = 1, X) \tag{5}$$

The outcome variables for households that receive treatment and those that do not are $Y1$ and $Y0$, respectively.[17]

After the matching procedure, I examine the difference between the choices of the treated borrowers and those of the untreated borrowers, using the DID method. To control for unobservable or other macroeconomic events that might affect the elasticity results, I use traditional DID estimation. This method compares the outcomes of two groups: one group above the LTV cutoff and the other below the LTV cutoff (treated and control, respectively), both before and after the LTV limit. The outcome of the control group will show the unobservable or other macroeconomic events that occurred concurrently (I assume that the housing supply is inflexible, at least in the short term). Although the treatment group cannot be observed after the policy restriction, it can be identified by means of the predicted distribution. This is because the basic assumption[18] in DID estimation is that the treatment and control groups will both be near the cutoff (Abadie (2005)). This paper examines groups that are near the cutoff, while ruling out transitions between the groups (local treatment effect).[19]

The interpretation of the outcome variables is based on the post-treatment outcome gaps between the treatment and control groups. Thus, the ATT is calculated as ATT = (after/before) treatment group - (after/before) control, and it is calculated separately for each outcome variable and borrower type.

IV. Results

A. The Effect of LTV Limits on Credit Choices

The soft LTV limit required banks to set aside more capital against risky loans (that is, loans with an LTV ratio above 60%). I examine whether the limit was binding and how banks reacted to

[17] See Abadie and Imbens (2011), Abadie et al. (2004), and Heckman, Ichimura and Todd (1997).

[18] See Abadie (2005).

[19] Another important assumption is that there is no anticipatory response by those in the treatment group. As mentioned, chances are that this policy was not anticipated.

this policy. Figure 4 shows that the banks increased the average interest rate charged from risky borrowers (borrowers with an LTV ratio above 60%). Because Figure 4 presents the interest rate charged at two time periods (before and after the soft LTV limit), so I also compare the interest rate paid by two similar borrowers (matched by income, age, bank, and loan duration) just above and below the 60% LTV limit (61% versus 59% in the first test and 61-65% versus 55-59% in the second test). Since the prime interest rate[20] changes between the periods, which could bias the results, I also examine the spread of the interest rate (over the prime rate). Before the regulation, there was no significant difference in the interest rate paid by borrowers above and below the 60% LTV threshold (with a 0.01-0.03 percentage point difference in their interest rate). After the LTV limit, the interest rate paid by a borrower with an LTV just above 60% was 0.21-0.36 percentage points higher than the interest rate charged to an identical borrower just below the LTV limit (Table IV).

The increased interest rate caused by the soft LTV limit incentivized borrowers to lower their leverage. As Figure 2 shows, some borrowers continued to be highly leveraged (and paid a higher interest rate), whereas others, due to the higher interest rate, lowered their leverage to 60% (or less). Figure 2 thus presents the LTV distribution after the soft limit with two peaks, one around LTV ratio of 75% and around LTV ratio of 60%. These results suggest that the soft LTV limit might incentives credit constraint borrowers to lower leveraged. In the same time, it is also possible that the credit constraint borrowers had no choice but to stay highly leveraged and to pay a higher interest rate. Appendix (H) shows that credit constraints borrowers, i.e. lower income borrowers, younger households and first-time home buyers, chose to continue to be highly levered and pay a higher interest rate, and those that chose to lower leverage are mainly borrowers with higher income, especially investors and upgraders.[21]

The strict LTV limit produced similar results. Figure 5 shows the change in the LTV distribution before and after the strict LTV limit for each borrower type. The dotted line in the figure is the LTV limit cutoff. The percentage of observations that are above the cutoff before the limitation

[20] The prime interest rate is the annual interest rate that banks and financial institutions use to set interest rates for variable-rate mortgages, which are based on the short-term interest rate set by the central bank.

[21] One possible explanation is that those borrowers are more informed to the new police rules, compare to lower income borrowers and first time home buyers.

was 12% for first-time home buyers, 12.5% for upgraders and 60% for investors. After the LTV limit was imposed, there is high density around the value of the cutoffs, for each borrower type. Figure 5 also shows the high impact of the strict LTV limit on the LTV distribution of investors. Overall, for each buyer type, there is a significant change in the LTV distribution after the new limit, according to the Kolmogorov-Smirnov test.

B. Credit Rationing

An important question in the literature that usually arises from changes in credit constraints is whether imposing an LTV limit affects the distribution of borrower characteristics. That is, are the same types of borrowers present before and after the imposition of the restriction, or does the LTV limit push out certain types of borrowers, perhaps those with limited access to the credit market? In the phenomenon known as "credit rationing" (Stiglitz and Weiss (1981)), banks limit the supply of additional credit to borrowers based on their characteristics, even if the borrowers are willing to pay higher interest rates.

I examine the distribution of borrower age and income before and after the imposition of both soft and strict LTV limits (Figures 6 and 7). A Kolmogorov-Smirnov test shows that there is no significant change in the distribution of borrower age and income.

Using aggregate data from the Bank of Israel, I examine whether households left the credit and housing markets because the strict LTV limit (under the soft limit they can stayed highly leveraged and simply pay a higher interest rate). Figure 8 presents changes in the activity in the credit and housing markets, before and after the strict LTV limit, for each borrower type. The vertical line denotes the time of the strict limit imposition. As can be seen, households did not leave either market following the restrictions.[22]

[22]No significant change in the homeownership rate following the restrictions.

C. The Effect of LTV limits on Housing Choices

C.1. Soft LTV limit

Because of the LTV limit, which increased interest rates and reduce loan amounts, borrowers had with less credit to purchase housing. Table V compares the outcome variables of housing units between the treatment and control groups before and after the imposition of the soft LTV limit (as shown in equation (4)). The DID matching estimator shows a significant decline of NIS 68,000 between the treatment and control groups. In terms of distance from Tel Aviv, the results indicate a clear and significant distancing of 4.3 km from the city in the treatment group compared with the control group.[23] The DID matching results also show that the treatment group moved to significantly lower-graded neighborhoods: the treated group versus the control group shows a decline of 2.2 points (12%) in neighborhood quality.

To help explain the magnitude of these changes, Figure 10 shows the population distribution of Israel. Fifty-five% of the population lives in the center of Israel, up to 40 km from Tel Aviv. In the first six months after the imposition of the soft LTV limit, affected borrowers moved, on average, 4.3 km (14.7%) farther from Tel Aviv to significantly lower-graded neighborhoods.

I assume that the housing supply is inflexible, at least in the short term. I also examine only the housing choices of the affected borrowers, which is a subgroup of buyers with a mortgage (which in turn is a subgroup of home buyers). Hence, such questions as who bought the remaining dwellings in the center of Israel (that the treatment group could not afford because of the limit), what happened to the prices of those dwellings, and whether the LTV limit affected the supply of housing (e.g., whether developers began to build smaller homes, in the periphery) are not within the scope of this study.

An interesting question that arises from the results of the matching method is, what kind of neighborhoods did the borrowers move to? More specifically, did some borrowers moved farther from Tel Aviv but improved their welfare by increasing the socioeconomic level of their neighborhood? There are, of course, very high graded suburbs of Tel Aviv. Figure 11 shows the distribution of changes in neighborhood grading by distance from Tel Aviv. The changes are divided into three

[23] The variable distance from Tel Aviv was censored at 40 km, to focus only on the most populated areas in Israel, as shown in Figure 10.

groups: upgrade (green), downgrade (red), and no change in neighborhood socioeconomic level (yellow). First, although the average move was of 4.3 km from Tel Aviv, almost 20% of the borrowers moved 5-10 km and almost 17% moved 10-20 km, which is a significant change. Moreover, within each subgroup of distance from Tel Aviv, more borrowers moved to lower-graded neighborhoods than to higher-graded areas (a statistically significant difference).

In Appendix F, I examine the differential effect of the soft LTV limit on subsegments of the population, particularly young and low-income borrowers. This test sheds light on the question of whether LTV limits make it difficult for households in need of credit to purchase property. I find that older adults and investors were affected more by the LTV limit and changed their choices in the housing market more than the other two groups. These results may be attributable to the possibility that older adults and investors are more flexible in their purchasing decisions and can either delay purchasing housing or compromise on the type of assets, as opposed to younger adults or first-time home buyers who may have different limitations and constraints that require them to purchase specific properties at particular locations, such as near their parents or their work.

C.2. Strict LTV limit

To control for unobservable or other macroeconomic events that might affect the results, I use a DID estimation to examine the local treatment effect of the policy intervention. This allows me to use the reaction of the control group as a symbol of other macroeconomic events that might have occurred at the same time. Recall that we do not know what the borrowers' leverage choices would have been without the policy intervention. We know only what the actual choices were, and so we use the borrowers' characteristics and leverage choices before the policy to infer their leverage choices after the policy through the matching method. Tables VI and VII present the results of the second estimation strategy, DID matching. First, the matching method is used to predict which borrower type borrowed (slightly) below the limit after the policy intervention, which type borrowed (slightly) above the limit before the intervention (the treatment group), and which type borrowed (slightly) below the limit before the intervention (the control group). The results for each type, before and after the policy intervention, are as follows. For first-time home buyers, the control group has an LTV ratio of 70-75, and the treatment group has an LTV ratio of 75-80.

For upgraders, the control group has an LTV ratio of 65-70, and the treatment group has an LTV ratio of 70-75. For investors, the control group has an LTV ratio of 45-50, and the treatment group has an LTV ratio of 50-55. Then, a DID estimation is performed to examine the difference between the credit and housing choices of the control and treatment groups before and after the policy.

Table VI presents the results in absolute terms, and Table VII presents the rate of change in choices in the credit and housing markets. In terms of housing characteristics, affected borrowers bought lower-quality assets: cheaper, smaller, farther from the center, and in lower-graded neighborhoods. I obtain counterintuitive results on the effect of the imposition of a strict LTV limit on new contracts in the credit market. The LTV limit should lower the LTV ratio, thereby lowering the risk of the loan and leading to a lower interest rate and shorter term to maturity. However, for each buyer type there is an increase in the interest rate and in the term to maturity (discussed in Section V). As Table VII shows, investors changed their choices more than the other two groups, probably because they are more resilient to changes and care more about maximizing their yield and less about the socioeconomic level of the neighborhood. Another possible reason for the higher increased in the interest rate for investors is that the new policy led them to buy riskier assets, and in particular assets farther from the CBD (discussed in Section V).

Overall, the most significant change is in the location of the housing unit: borrowers purchased homes farther from the center of Israel. The next most important change is a reduction in the socioeconomic level of the neighborhoods: the size of the housing unit changed the least. I find that the 2012 strict LTV limit had a much greater impact on borrower choices in the credit and housing markets, than the 2010 soft LTV limit. This is especially true for investors, who moved farther from the center (24%), to lower-graded neighborhoods (18%), and into smaller houses (14%), compared with 15%, 9%, and -2%, respectively, in response to the 2010 soft LTV limit. This is probably because the 2012 strict LTV limit reduced the LTV distribution by almost half. In addition, after the 2012 limit the greatest change was in the location of borrowers' housing units, whereas after the 2010 limit the greatest change was in the socioeconomic level of the neighborhood. The final difference between the two limits is that after the 2012 limit the borrowers changed the size of their housing unit, whereas after the 2010 limit there was no such change.

Appendix I analyzes the elasticity impact of the strict LTV limit on each borrower type, taking

into account that different buyer types are limited by different LTV cutoffs. Even when controlling for the different LTV cutoffs, I find that investors exhibit the highest elasticity in each variable, contrary to the criticism that MPPs might be pushing lower-income households out of those markets. These results are promising also in terms of MPPs: if investors have the highest impact on housing prices during a housing boom (Kuttner and Shim (2016)), MPPs will not stop them from entering the credit and housing markets, but it will affect their housing choices.

V. Additional Analysis on the Effectiveness of LTV Limits

Section IV shows that imposing a strict LTV limit on new contracts yielded counterintuitive results in the credit market. Although a strict LTV limit was supposed to have lowered borrowers' risk and therefore improved their credit conditions, affected borrowers actually paid a higher interest rate and increased their term to maturity. There are three plausible explanations for this outcome. First, banks may have changed their risk perception due to the strong signal from the macroprudential supervisory authority about a buildup of systemic vulnerability among highly leveraged borrowers, which in turn affected their risk pricing (de Araujo, Barroso and Gonzalez (2020)). Two other explanations are possible: either the affected borrowers bought riskier assets, that is, farther from the center, which may have increased the interest rate on those loans; or, Third, because of the LTV limit, the affected borrowers may have borrowed money from other sources or used other credit such as consumer credit, which increased their monthly loan payment. To keep the monthly mortgage payment constant, the affected borrowers needed to increase their term to maturity, which in turn caused the mortgage interest rate to rise.

A. Are Housing Assets Farther from the Center Riskier?

A central finding thus far that the strict LTV limit lowered the prices of the purchased assets, especially by forcing borrowers to move farther from the center of Israel (Tel Aviv). Moving farther from the city might increase borrowers' risk, in that there is less demand to live in the periphery and asset prices there are more volatile. This section examines whether assets in the periphery are indeed riskier than those in the center by using a monocentric city model (DiPasquale and

Wheaton (1996)). This model tries to explain the spatial distribution of a population in a city or country. The main mechanism is the relation among commuting costs, house prices, and housing consumption. The basic assumption in the model's framework is that all jobs are located in the city center(CBD). Households want to live near the CBD, since there is more economic activity there, which means higher wages and a lower unemployment rate. There are also more cultural activities in the CBD than in more distant locations. For these reasons, prices are assumed to be higher in the CBD. Eckstein, Tolkovsky and Tzur-Ilan (2012) show that housing prices are highest in Tel Aviv and decline as one moves farther from the city.

I examine the monocentric city model in terms of the investor's risk and explore whether the risk factor changes as we move farther from the center. I use data from the YAD2 website, which publishes information and advertisements about homes for sale, and I examine the price gap between the final and first asking prices of each ad (between the years 2013 and 2016). Figure 12 shows that the price gap decreases as we approach the center of Israel, and even becomes positive in Tel Aviv itself, which suggests that it is harder to sell properties in the periphery.

Figure 13 presents two more risk indicators that compare between housing assets in the center and the periphery of Israel. The first (blue line) is the average listing time for selling a home between the years 2013 and 2016. Like the indicator in Figure 12, it shows the greater difficulty of selling a property that is farther from the center: the average listing time for selling a home shortens the closer one gets to the center of Israel. The second risk indicator (red line) is the volatility (or standard deviation) in the change in the average price of homes along the Israeli coastal plain between the years 1998 and 2017. The volatility in housing prices decreases as one approaches to the center of Israel.

All three indicators suggest that the risk associated with housing assets increases the farther one moves from the center. Therefore, the strict LTV limit, which is theorized to lower borrower risk, might actually increase risk by forcing borrowers to move farther from the center, to riskier areas.

B. Shifts in Demand for Consumer Credit

Following the imposition of a strict LTV limit, borrowers who still wish to buy a home but lack the equity to do so can try to raise money from other resources. First, they can raise money from their family (which is cheaper than credit from other sources), but my assumption is that they have already borrowed whatever they could from their family before making the decision to buy a home. Therefore, restricting their LTV ratio should not greatly affect their ability to raise more money from their family in the short term (and in terms of housing prices, even a 5% in the LTV ratio equals tens of thousands of dollars, which is a significant amount to raise in a short time). Second, borrowers can withdraw money from their liquid financial resources, but my assumption, again, is that they have already decided how much they would like to spend out of their financial resources before deciding to buy a property (this is especially relevant for first-time home buyers). Figure 14 shows withdrawals from several financial resources before and after the LTV limit was imposed. We can see that withdrawals were not higher after the limit. Third, borrowers could still choose to buy the same property they wanted before the limit, but also take out other, unregulated credit, such as consumer credit. There are no loan-level data on consumer credit in Israel. Without such data, I use macrodata and some indicators from existing loan-level data to examine whether there was a shift in demand for consumer credit following the LTV limit.

First, I estimate the decrease in the amount of mortgages after the LTV limit. For each borrower type, I calculate the amount of mortgages that were above the cutoff before the policy intervention and find that this amount is around NIS 10.3 billion[24] (36% belong to first-time home buyers, 33% for upgraders, and 31% for investors). However, some of those borrowers did not exit the market; instead, they lowered their loan amount. Using the method in Subsection III.B, I match households that were supposed to be above the cutoff with their actual LTV choices and examine the change in their loan amount. Overall, borrowers with an LTV ratio above the cutoff before the policy intervention took out NIS 3.6 billion in mortgages after the intervention. Therefore, about NIS 6.7 billion were excluded from the mortgage market. Some of the borrowers bought less expensive house-the overall change in the value of properties due to the limit is NIS 3.2 billion. The findings

[24] I assume that the demand for mortgages remains the same after the policy intervention. This calculation is an underestimate because housing prices increased in the following years.

thus suggest that certain borrowers continued to buy similar houses, with lower mortgages. At the same time, housing prices increased by 6%, the number of transactions in the real estate market did not change, and the population of borrowers also did not change. How did borrowers raise this additional amount of money in such a short time?

Figure 15 shows the shift in demand from mortgages to consumer credit after the imposition of the LTV limit. Although demand for consumer credit increased after the LTV limit, the question is how much of this increase was due to the regulation. To answer this, I run two ordinary least-squares (OLS) regressions. First, I examine the impact of the strict LTV limit on the rate of change in mortgages (controlling for interest rate, housing transactions, real estate taxes, and rent prices). Second, I explore the impact of the strict LTV limit on the rate of change in consumer credit (controlling for vehicle imports, interest rate, private consumption, and taxes). I find that the LTV limit dummy has a significantly negative effect in the first regression and a significantly positive effect in the second regression, which implies that the shift in the demand for consumer credit might be caused by the strict LTV limit instead of mortgages.

Although the loan-level data do not reveal the amount of consumer credit for each borrower, I do have information about each borrower's amount of equity. This equity can come from the borrower's savings, from the family, or from consumer credit he or she took out from other financial institutions. Figure 16 shows the change in the equity distribution for the affected borrowers, that is those who had an LTV ratio above the cutoff before the policy intervention. For each (affected) borrower type, we can see an increase in the equity distribution after the policy intervention (between 2012 and 2013), relative to the equity distribution before the policy intervention (between 2011 and 2012), when there was no change. A Kolmogorov-Smirnov test confirms this.

The increase in equity for the affected borrowers is NIS 3.2 billion, or almost half of the amount of mortgages withdrawn from the market. Again, it is hard to believe that borrowers could raise such large sums only a few months after the limit was imposed. It is thus unlikely that the funds came from their families.

Instead, the decline in mortgage loans was likely replaced in part by consumer loans. Such loans are riskier than mortgages, both for the banks and for the households. They are not backed by collateral. Moreover, the average consumer credit loan has a duration around 4.5 years, compared

to 22 years on average for mortgages, and hence makes the debt payment relative to income ratio high, increasing the borrower's probability of default. This leads us to ask whether the LTV limit actually lowers the borrower's risk or merely shifts that risk to unregulated institutions or products, thereby increasing the borrower's household leverage, monthly debt payments, and overall exposure to risk of recession and unemployment.

VI. Conclusion

Since the 2008 global financial crisis, MPPs have attracted considerable attention, and the literature on this issue is growing rapidly. However, despite the importance of the wide use of MPPs by numerous countries, including Israel, the literature still lacks information on the benefits and costs of such policies.

This paper estimates the effects of LTV limits on loan terms, especially on borrower behavior in the housing market. Although LTV limits typically target banks, they may cause borrowers to pay higher interest rates and to move to lower-graded housing, farther away from the population center, and into neighborhoods with lower socioeconomic ratings. This study leaves some interesting but unanswered questions, mainly the welfare implications of borrowers moving farther from the city center to lower socioeconomic neighborhood, due to credit constraints. These are fruitful topics that I hope to pursue in future research.

Another important finding is that the two policies, soft and strict LTV limits, are in fact very similar and generate similar effects on household choices in the credit and housing markets. In magnitude, the strict LTV limit generates much larger results than the soft LTV limit, probably because of the higher costs associated with circumventing a strict LTV limit in comparison with a soft one. Under the strict LTV limit, borrowers choosing to stay highly leveraged can only do so by using nonmortgages credit, which is more expensive compared to bank interest rates on highly leveraged mortgages (above the soft LTV limit). Another way to summarize the effects of the strict LTV limit studied here is to divide them into a reduction in the value of acquired properties (around NIS 3.2 billion in total) and an increase in the use of non-mortgage consumer credit that

is roughly equal in magnitude. Therefore, the strict LTV limit may pose more risk to the financial system because it induces borrowers to use risky, costly, and unregulated credit. Notwithstanding the lack of data on the consumer credit markets which may limit the generality of the findings, this study suggests that future LTV measures should take into account nonbank sources of credit as part of the LTV limitations.

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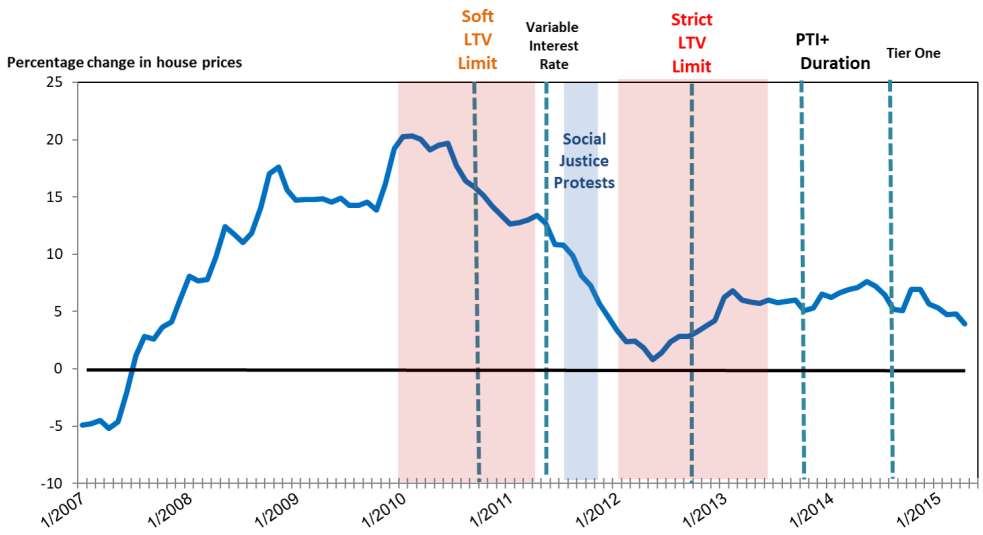


Figure 1. The Rate of Change in Housing Prices in Israel, 2007-2015. The line represents the monthly change in home prices (in annual terms). MPP tools are shown on the vertical lines. This paper focuses on a short period of time before and after each LTV limit, colored in red in the graph. Source: Israel Central Bureau of Statistics.

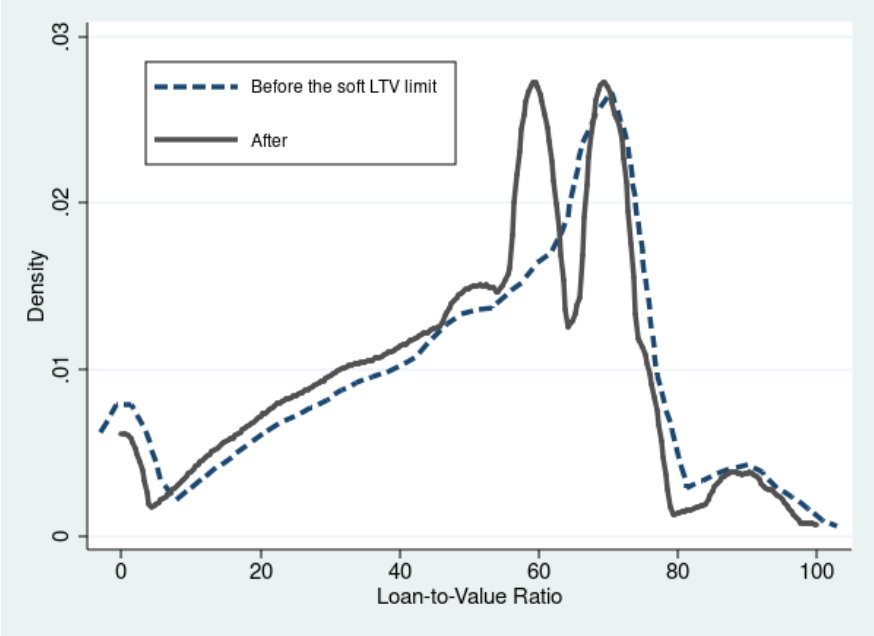


Figure 2. LTV Distribution Before and After the Soft LTV Limit. This figure plots the Loan-to-Value ratio distribution for two periods: before the soft LTV limit, i.e. between January 2010 to October 2010 (N=16,100), and after the policy, between November 2010 until May 2011 (N=11,224). A Epanechnikov kernel distribution with bandwidth of 2.9601 is used in both figures.

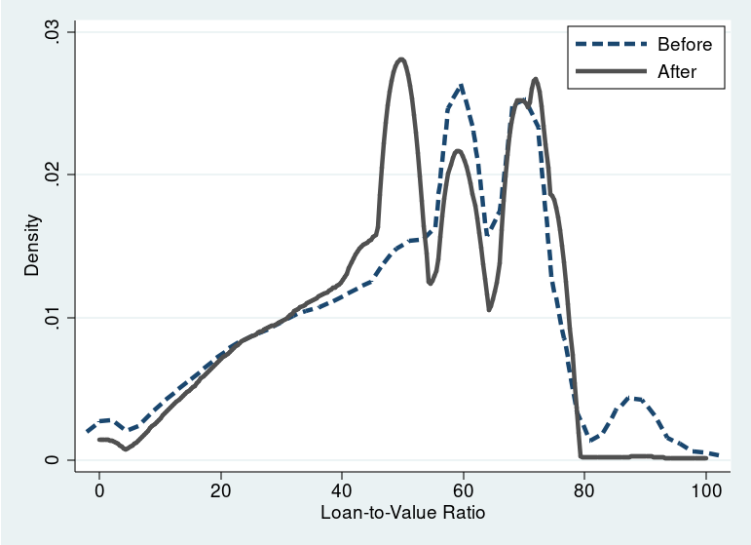


Figure 3. LTV Distribution Before and After the Strict LTV Limit. This figure plots the Loan-to-Value ratio distribution for two periods: before the strict LTV limit, i.e. between January 2012 to October 2012 (N=17,260), and after the policy, between November 2012 until August 2013 (N=16,761). A Epanechnikov kernel distribution with bandwidth of 2.1121 is used in both figures.

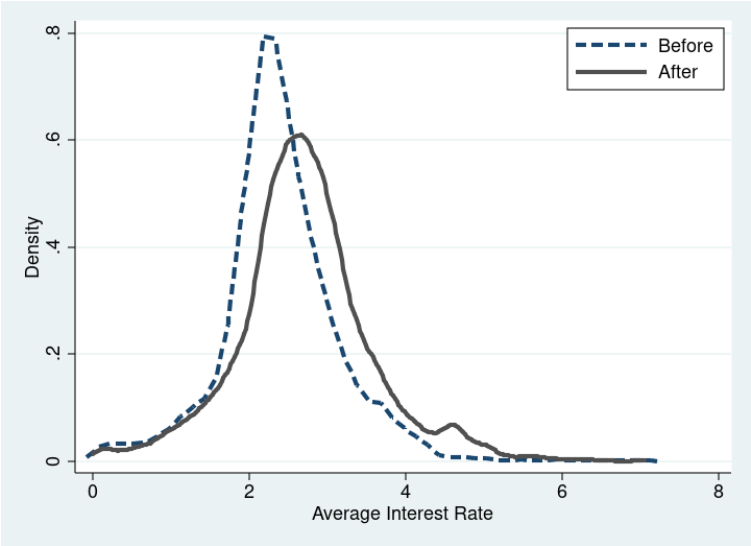
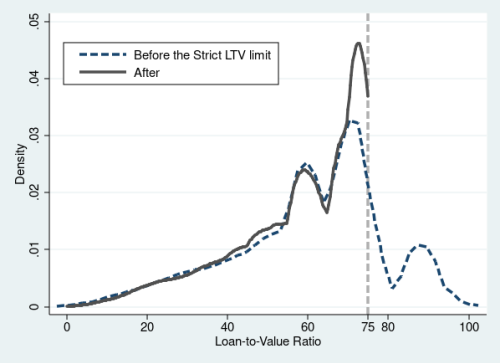
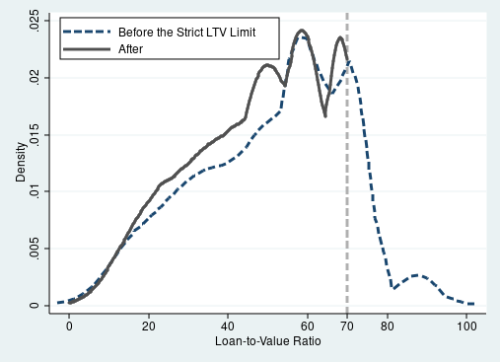


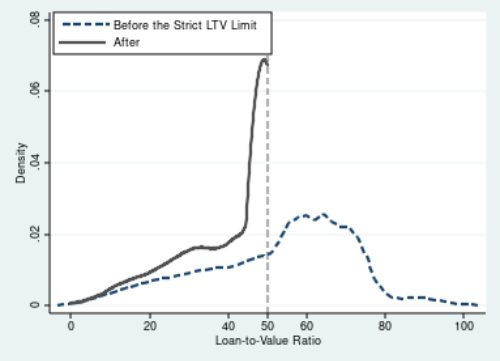
Figure 4. Change in Average Interest Rate for Risky Borrowers (LTV>60%). This figure plots the average mortgage rate interest rate for borrowers with LTV above 60% for two periods: before the soft LTV limit, i.e. between January 2010 to October 2010 (N=16,100), and after the policy, between November 2010 until May 2011 (N=11,224). A epanechnikov kernel distribution with bandwidth of 0.0606 is used in both figures.



Panel A: First-Time Home Buyers

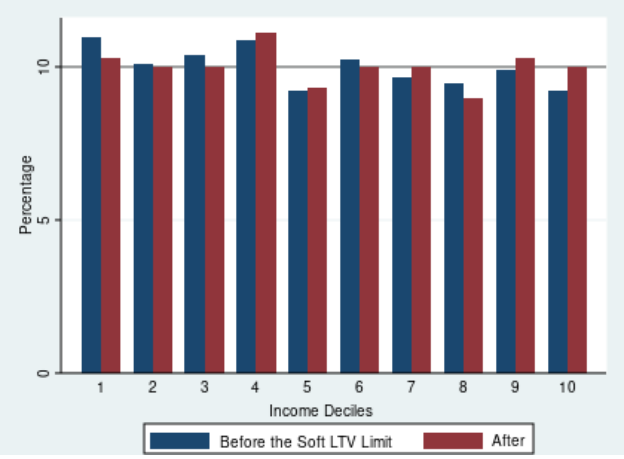


Panel B: Upgraders

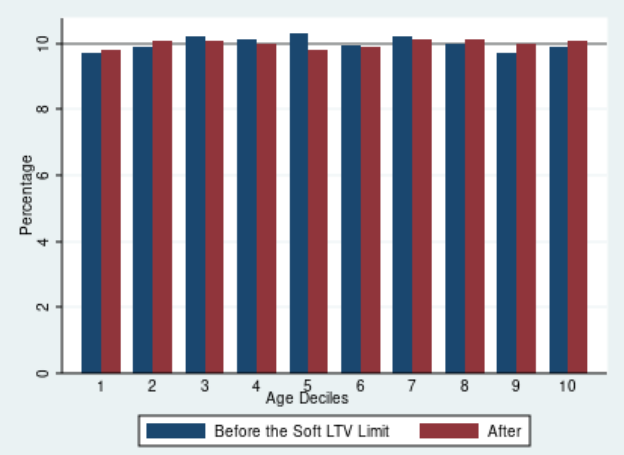


Panel C: Investors

Figure 5. LTV Distribution by Buyer Type, before and after the Strict LTV Limit. This figure plots the change in the LTV distribution before and after the strict LTV limit for each borrower type. The vertical dotted line in the figure is the LTV limit cutoff: 75% for first-time home buyers, 70% for upgraders, 50% for investors. A epanechnikov kernel distribution with bandwidth of 0.0606 is used in both figures.

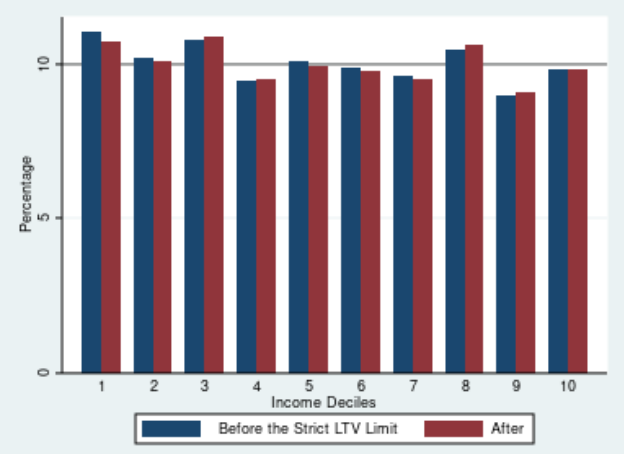


Panel A: Income Distribution Before and After the soft Limit

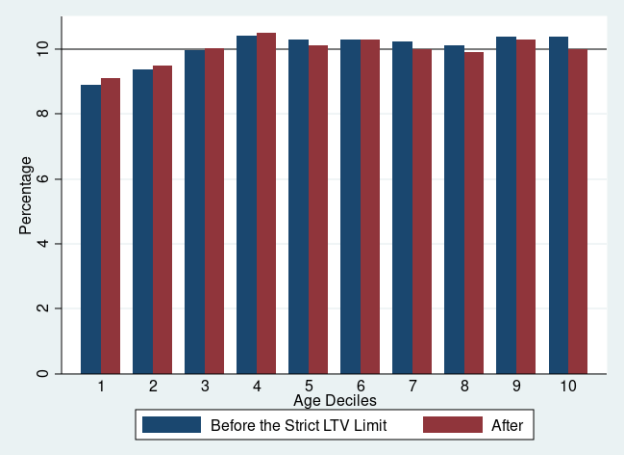


Panel B: Age Distribution Before and After the soft Limit

Figure 6. Soft LTV limit: Change in Distribution of Borrower Characteristics.

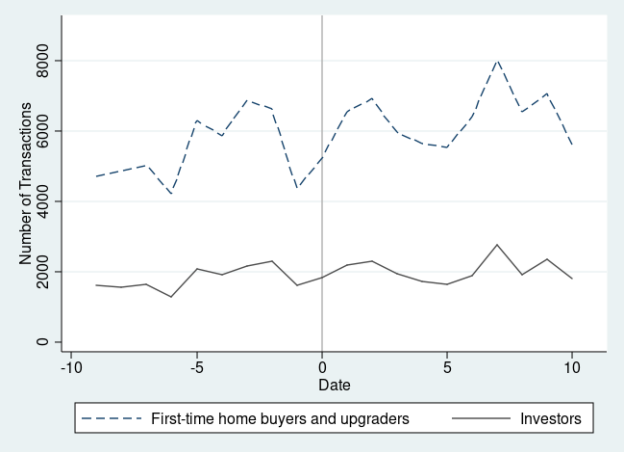


Panel A: Income Distribution Before and After the Strict Limit

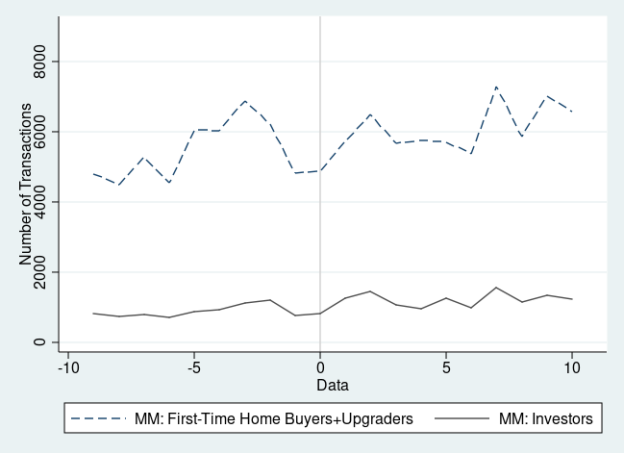


Panel B: Age Distribution Before and After the Strict Limit

Figure 7. Strict LTV limit: Change in Distribution of Borrower Characteristics.

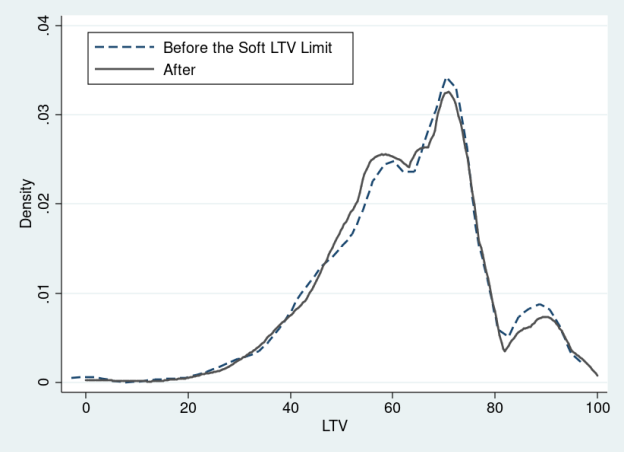


Panel A: Real Estate Transactions, by Buyer Type, January 2012 - August 2013

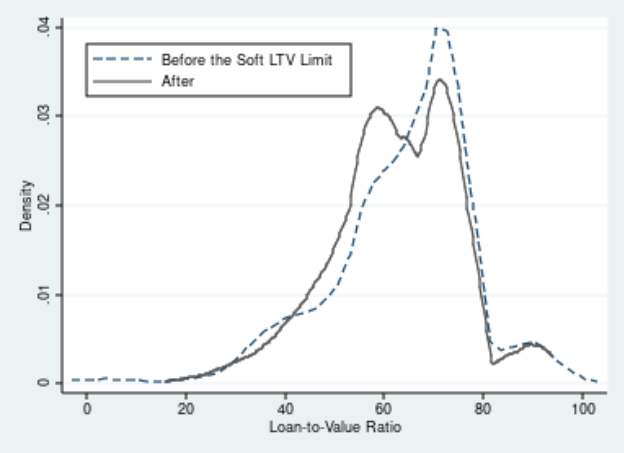


Panel B: Mortgages Transactions, by Buyer Type, January 2012 - August 2013

Figure 8. Strict LTV limit: Activity in the Credit and Housing Markets, by Borrower Type. This figure plots the amount of transactions in the mortgage and housing markets before and after the strict LTV limit, by Buyer Type. The vertical line denotes the time of the strict limit imposition. Source: Israel Tax Authority for housing transactions and Bank of Israel for mortgage transactions.



Panel A: LTV Distribtuion of the Control Group



Panel B: LTV Distribtuion of the Treatment Group

Figure 9. LTV Distribution before and after Imposition of the LTV Limit: Treatment and Control Groups. This figure plots the change in the LTV distribution before and after the soft LTV limit for the treatment and control groups. Control group: those who borrowed NIS 600,000 to 700,000, just below the threshold. Treatment group: those who borrowed NIS 900,000 to 1,000,000, just above the threshold. A epanechnikov kernel distribution with bandwidth of 2.9472 is used in both figures.

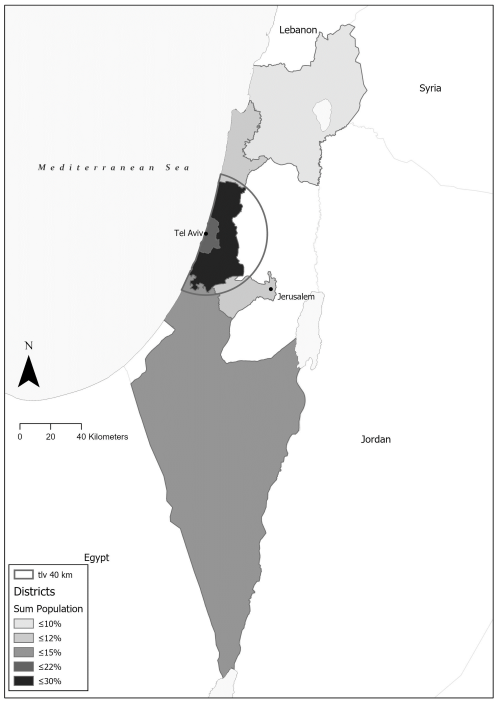


Figure 10. Israeli Population Distribution. The Israeli population distribution is calculated according to the population that is relevant to the sample. The variable distance from Tel Aviv was censored at 40 km, to focus only on the most populated areas in Israel, as shown in the figure. Source: Israel Central Bureau of Statistics.

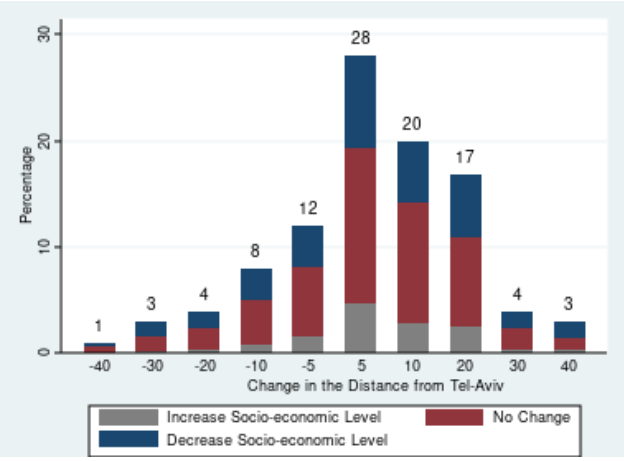


Figure 11. Distribution of Change in Neighborhoods' Socioeconomic Level, by Distance from Tel Aviv. The changes in neighborhoods' socioeconomic level are divided into three groups: increase in the socioeconomic level, decrease in the socioeconomic level, and no change in neighborhood socioeconomic level.

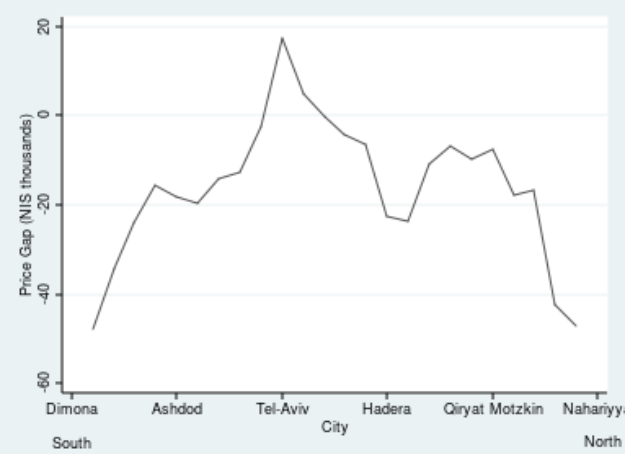


Figure 12. Price Gap between First Asking Price and Final Asking Price along the Israel Coastline. This figure plots the change in risk indicator along the Israel coastline, i.e. between the center and the periphery (south and north) of Israel. Source: Listing data from "YAD2" website, which publishes information and advertisements about homes for sale, for years 2013-2016.

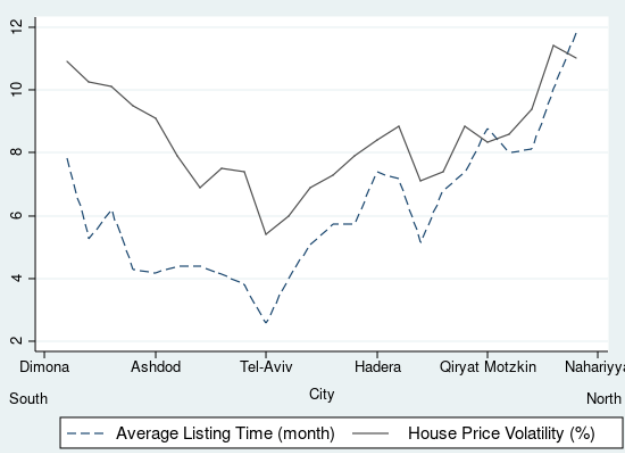


Figure 13. Risk Measure of Property Assets along the Israeli Coastline. This figure plots the change in risk indicators along the Israel coastline, i.e. between the center and the periphery (south and north) of Israel. Source: Listing data from "YAD2" website, which publishes information and advertisements about homes for sale, for years 2013-2016. House Price Volatility was calculated using Israel Tax Authority data and according to monthly standard error of home prices between years 1989-2016.

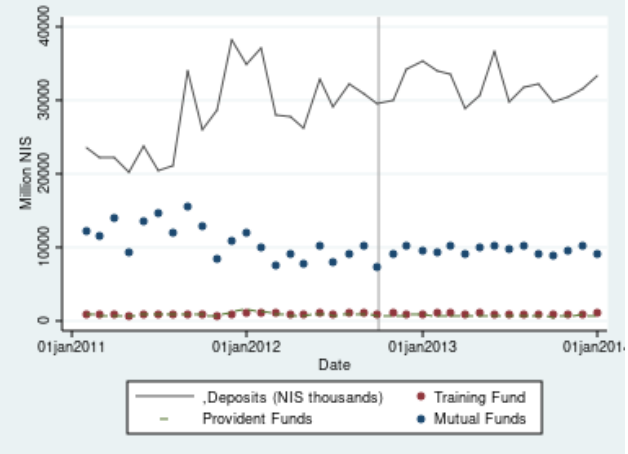


Figure 14. Withdrawals from Several Financial Resources, 2011-2013. This figure plots withdrawals from several financial resources before and after the LTV limit was imposed. Training Funds is a provident fund that was originally intended to be used by employees to fund training courses but is now generally used as a medium-term savings channel that provides tax benefits in Israel. Mutual Funds: professionally managed investment fund that pools money from many investors to purchase securities. Provident Funds is a compulsory, government-managed retirement savings scheme used in Israel. Deposits: financial term that means money held at a bank, including checking and saving accounts. The vertical line is the implementation date of the strict LTV limit. Source: Bank of Israel.

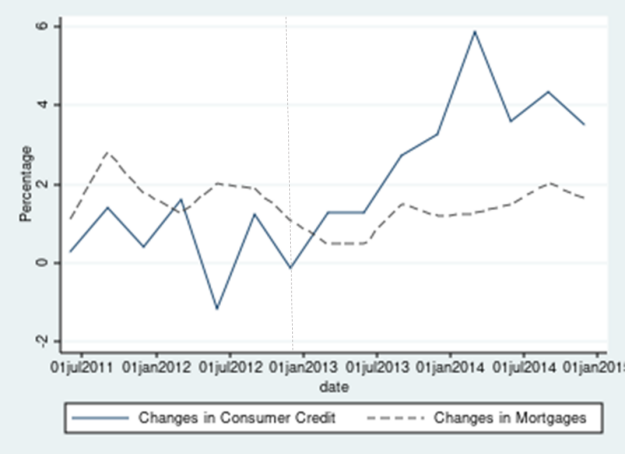
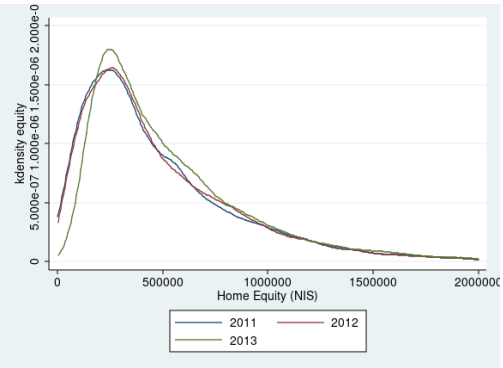
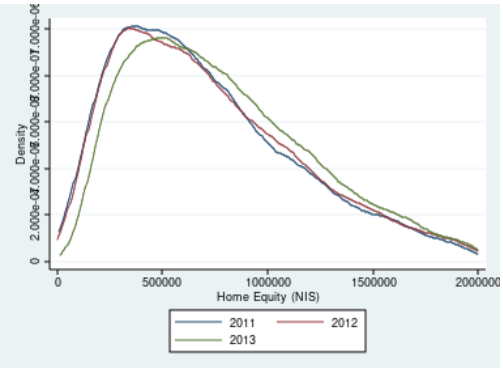


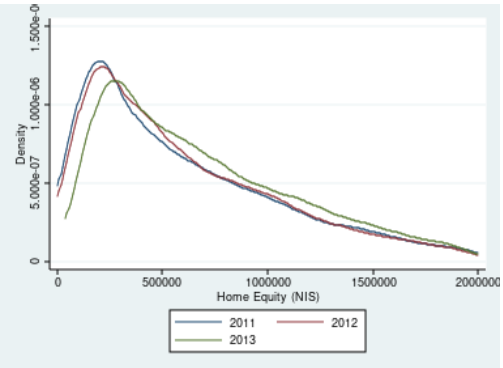
Figure 15. Changes in Mortgages and Consumer Credit over Time. This figure plots the quarterly growth rate in mortgages and consumer credit before and after the strict LTV limit using two ordinary least-squares (OLS) regressions. For the changes in mortgages, the OLS regression controls for interest rate, housing transactions, real estate taxes, and rent prices. For the changes in consumer credit, the OLS regression controls for vehicle imports, interest rate, private consumption, and taxes. The vertical line is the implementation date of the strict LTV limit. Source: Bank of Israel.



Panel A: Change in Downpayment Distribution for First-Time Home Buyers



Panel B: Change in Downpayment Distribution for Upgraders



Panel C: Change in Downpayment Distribution for Investors

Figure 16. Changes in Down Payment Distribution Before and After the Strict LTV Limit. This figure plots the change in the equity distribution for the affected borrowers, that is those who had an LTV ratio above the cutoff before the policy intervention, for each (affected) borrower type. The years 2011-2012 are before the implementation of the policy, which occurred in the end of 2012, and the year 2013 is after the strict LTV limit.

Table I
Summary Statistics: Soft LTV limit

Dataset	Variable	Before the LTV Limit (N=16,100)		After the LTV Limit (N=11,224)		Difference	
		Mean	S.D.	Mean	S.D.	Coef	S.E.
Mortgage contracts	Loan (thousands)	554	346	565	348	11**	4.3
	Interest rate	2.41	0.67	2.71	0.97	0.3***	0
	LTV	56.7	19.7	55.9	18.9	-0.8***	0.2
	Duration (months)	245	79.9	254	82.1	9***	0.9
Borrowers	Income (thousands)	14.17	8.24	14.76	8.45	0.59***	0.1
	Average age	41.68	9.95	41.47	10.2	-0.21*	0.1
Home	Nominal house prices (thousands)	1,078	601	1,106	614	28***	7.4
	Real house prices (thousands)	1,026	572	968	537	-58***	6.8
Purchase	Rooms	3.98	1.09	3.97	1.1	0.0	0.0
	Area (m^2)	97.3	48.7	96.9	79.3	-0.4	0.8
Transactions	Distance from Tel Aviv (km)	45.2	45.7	47.8	45.8	2.6***	0.5
	socioeconomic level of Neighborhoods	11.9	3.61	10.4	3.5	-1.5***	0.0

Source: Data on mortgages from the Bank of Israel; data on purchase transactions from the Israeli Tax Authority. Real house prices is inflated by the overall (monthly) change in house prices. Socioeconomic level of Neighborhoods - index of neighborhoods consisting of 16 variables, including demographics, education, etc. Neighborhoods are classified into one of 20 clusters, 1 being the lowest socioeconomic status. The sample covers the period January 2010-May 2011. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively

Table II
Summary Statistics: Strict LTV limit

First-Time Home Buyers					% Observation = 46
	Mean	Sd. dev.	25%	50%	75%
House price (NIS)	1,002,977	463,532	720,000	960,000	1,270,000
Borrower monthly income (NIS)	13,272	6,539	9,250	12,100	16,000
Borrower age	36.4	8.4	30.0	34.5	40.1
LTV (%)	60.2	18.7	50.0	61.2	72.0
Average interest rate (%)	2.95	0.78	2.50	2.89	3.36
Loan duration (years)	22.2	6.5	18.3	23.8	27.2
Area (m^2)	87.4	32.7	65.0	84.0	103.0
Rooms	3.7	0.9	3.0	4.0	4.0
Distance from Tel Aviv-Jaffa (km)	38.4	29.5	12.7	28.8	52.9
Socioeconomic level of neighborhood	10.6	3.5	9.0	11.0	13.0

Upgraders					% Observation = 39
	Mean	St. dev.	25%	50%	75%
House price (NIS)	1,330,092	684,012	920,000	1,260,000	1,660,000
Borrower monthly income (NIS)	15,913	7,776	11,000	14,420	19,173
Borrower age	42.9	9.6	36.2	41.2	48.5
LTV (%)	51.0	19.0	37.0	54.1	66.0
Average interest rate (%)	2.90	0.76	2.45	2.82	3.27
Loan duration (years)	21.2	7.3	15.7	22.2	26.1
Area (m^2)	110.9	53.8	82.0	104.0	129.0
Rooms	4.4	1.1	4.0	4.0	5.0
Distance from Tel Aviv-Jaffa (km)	39.9	30.2	12.7	29.9	59.8
Socioeconomic level of neighborhood	11.3	3.8	9.0	12.0	14.0

Investors					% Observation = 14
	Mean	St. dev.	25%	50%	75%
House price (NIS)	1,031,760	712,695	590,000	995,000	1,500,000
Borrower monthly income (NIS)	19,928	11,674	12,300	17,500	24,000
Borrower age	44.7	10.9	36.5	43.1	52.0
LTV (%)	52.5	18.8	40.0	58.0	68.0
Average interest rate (%)	2.96	0.85	2.46	2.89	3.42
Loan duration (years)	18.5	7.7	12.9	20.0	25.0
Area (m^2)	87.2	56.4	55.0	75.0	106.0
Rooms	3.6	1.2	3.0	3.0	4.0
Distance from Tel Aviv-Jaffa (km)	42.7	33.0	9.9	40.7	69.0
Socioeconomic level of neighborhood	10.6	3.7	8.0	11.0	13.0

Source: Data on mortgages from the Bank of Israel; data on purchase transactions from the Israeli Tax Authority. The table reports summary statistics for the sample, which is distributed into three groups according to type of buyer: first-time home buyers, upgraders (who seek to upgrade their housing situation), and investors (who own more than one residential property). Each panel reports details about the loan, the house bought using the loan, and borrower characteristics, for the period Jan 2012-Oct 2012 (i.e., before the LTV limit). Number of observations: 34,021 borrowers. Real house prices is inflated by the overall (monthly) change in house prices. Socioeconomic level of Neighborhoods - index of neighborhoods consisting of 16 variables, including demographics, education, etc. Neighborhoods are classified into one of 20 clusters, 1 being the lowest socioeconomic status. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

Table III
Sample Statistics before the Matching Process

Average, per household	Before Matching			After Matching		
	Treated	Untreated	P-Value	Treated	Untreated	P-Value
Total income (NIS)	17,982	14,710	0.00	17,982	17,845	0.93
Average age	42.28	40.49	0.00	42.28	42.29	0.98

"Treated" - those that borrowed from 900,000 to 1,000,000 NIS. "Untreated" - those that borrowed 600,000 to 700,000 NIS.

Table IV
Changes in the Interest Rate for Matched Borrowers above and below the LTV Limit

	61% VS 59%				61-65% VS 55-59%			
	Average Rate	Average Rate	Spread	Spread	Average Rate	Average Rate	Spread	Spread
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	.358***	.251***	0.213*	0.258**	.312***	.297***	0.251***	0.259***
	(.078)	(.081)	(.110)	(.129)	(.065)	(.063)	(.086)	(.079)
Total income	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Average age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Duration	No	Yes	No	Yes	No	Yes	No	Yes
No. of obs.	349	349	349	349	1,937	1,937	1,937	1,937

Note: Heteroskedasticity-consistent standard errors are in parentheses. ***, **, * - significance at 1, 5, and 10 percent levels, respectively. Spread - the interest rate over the PRIME. ATT is the Abadie-Imbens bias corrected average treated effect. Treated- who borrow above 60% LTV threshold. Borrowers were Matched by income, age, bank and duration of the loan.

Table V
Soft LTV Limit - Diff-in-Diff Matching

Dep. Variable:	Real Home Prices (NIS)	Size (sq.m.)	Rooms	Distance from Tel Aviv (km)	Neighborhoods Ranking
ATT	-67,789***	0.41	-0.03	4.3***	-2.2***
	(36,135)	(2.7)	(0.05)	(1.7)	(0.8)
ATT (%)	-4.7%	-1.1%	-0.9%	14.7%	-12.3%

Treated borrowers are defined as those borrowing 900,000 to 1,000,000 NIS. The untreated borrowers are those borrowing 600,000 to 700,000 NIS. There are 1,498 treated borrowers and 1,498 borrowers in the control group. ATT is the Abadie-Imbens bias-corrected average estimator. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

Table VI
DID Matching Estimation of the Effect of Strict LTV Limit on Credit and Housing Choices

	First-Time Home Buyers	Upgraders	Investors
	70-75 v. 75-80	65-70 v. 70-75	45-50 v. 50-55
Home prices (NIS thousands)	-78,504***	-48,760**	-182,722***
	(15,252)	(16,901)	(27,522)
Size (m^2)	-8.05***	-3.1*	-14.9***
	(2.19)	(2.42)	(3.01)
Distance from	7.1***	3.3**	12.0***
Tel Aviv (km)	(1.61)	(1.57)	(2.97)
Neighborhoods	-1.2***	-0.4	-2.0***
Quality	(0.39)	(0.43)	(0.57)
Interest rate (p.p.)	0.41***	0.15	0.62***
	(0.13)	(0.14)	(0.22)
Maturity (years)	1.8***	0.5	1.5***
	(0.45)	(0.42)	(0.59)
Default (p.p.)	-0.2***	-0.15***	0.06
	(0.06)	(0.05)	(0.07)
N	3,229	1,714	628

This table reports the average treatment effect on the treated (ATT) for each variable and borrower type. The first stage uses an Abadie-Imbens estimator to match borrowers in order to estimate which borrowers would have borrowed above the limit before the policy, conditional on borrower income and age. The second stage involves using a DID estimation to identify the effect of the LTV limit on the treatment and control groups before and after the policy intervention. Standard errors take into account prediction stage estimation uncertainty. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

Table VII
The Effect of LTV Limit on Credit and Housing Choices Using DID Matching (Percentage Change)

	First-Time Home Buyer	Upgraders	Investors
	70-75 v. 75-80	65-70 v. 70-75	45-50 v. 50-55
Home prices (NIS thousands)	-0.10***	-0.05**	-0.22***
Size (square meters)	-0.09***	-0.03**	-0.14***
Distance from Tel Aviv (km)	0.14***	0.06**	0.24***
Neighborhoods quality	-0.12***	-0.04	-0.18***
Interest rate (p.p.)	0.41***	0.15	0.62***
Maturity (years)	0.07***	0.02	0.09***
Default (p.p.)	-0.2***	-0.15***	0.06

This table reports the average treatment effect on the treated (ATT) for each variable and borrower type, in percentage change. The first stage uses an Abadie-Imbens estimator to match borrowers in order to estimate which borrowers would have borrowed above the limit before the policy, conditional on borrower income and age. The second stage involves using a DID estimation to identify the effect of strict LTV limit on the treatment and control groups before and after the policy intervention. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

Appendix A. MPPs Used in Israel, in Chronological Order

MPPs	Date	Type of MPP
Soft LTV limit	October 2010	Loans with an LTV ratio above 60%, which have been weighted at 35 to 75% of weighted capital, must provide a 100% allocation. This does not apply to housing loans less than NIS 800,000.
Variable Interest Rate	May 2011	The share of the variable interest rate of the housing loan will be limited to one third of the total loan for up to five years.
Strict LTV limit	November 2012	LTV will be limited as follows: 75% for a single housing unit; 50% for investors; 70% for improvers.
PTI + Duration	August 2013	PTI limited to 50% of HH net income, risk weights for capital adequacy requirements on PTI¿40% raised to 100%, share of variable-rate loans limited to two-thirds for all loan periods, loan period limited to 30 years.
Tier One	September 2014	Additional Tier 1 capital requirement equal to 1% of total outstanding housing credit portfolio. Gradual implementation with final target to be reached by 1 January 2017.

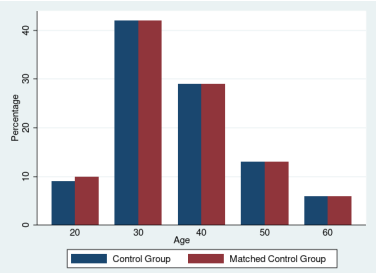
Appendix B. Merging the Mortgages Database to the Real Estate Database (CARMAN)

The mortgages file is merged with the CARMAN file through the following fields common to the two files: date of transaction, transaction price, city of property, and block and parcel numbers. As mentioned in the text, the recording of the block and parcel numbers in the mortgages file is distorted: 36% of the records are blank, and others showing only partial information. As a result,

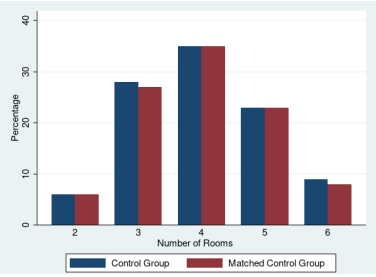
the block and parcel number field is used only if no adjustment could be made using the other fields. The first step in merging the mortgages file to the CARMAN file is a full matching using the three fields of city, date, and price of purchased assets. Such a match is found in approximately 65,000 records (step 1). In cases where there is more than one match in the mortgages file, the block-parcel field is also used, leading to the identification of 2,000 additional observations (step 2). Sometimes the registration date of the transaction in the CARMAN file is distorted. In cases where there is a blank date field in the mortgages file with one match to city and price, there are 500 matched observations (step 3). When the match is not complete and a unique match is made possible by using the block-parcel field, 4,000 observations are obtained (step 4). When the mortgages file has a date that is not compatible with that in the CARMAN file (gap of up to 20 days), but there is a match using the block-parcel field, 2,290 observations are obtained (step 5). When there is no block or parcel number, but there is a single adjustment in the date range of up to five days, 160 paired observations are obtained (step 6). Finally, cases in which there is a city but not a price match are examined. If the date and the city match but there is a range in the price of up to NIS 100,000, and there is a match using the block-parcel field, 14,600 observations are obtained (step 7). When a match is made by locality and date, with a gap of up to NIS 1,000, 400 paired observations are obtained (step 8). In cases where there is a price adjustment, as well as matches in the date and block-parcel fields, but there is no information on the city, 40 observations are obtained (step 9). In cases where there is no unique detection and the block-parcel field provides a unique identification, 23 paired observations are obtained (step 10).

Steps	Exact City	Exact Price	Exact Date	Single Match	Block, Parcel, and Sub-parcel	Range	Number of Identified Observations	Comments
1	+	+	+	+	-	-	**65,000**	
2	+	+	+	-	+	-	**2,000**	
3	+	+	-	+	-	-	**500**	date missing
4	+	+	-	-	+	-	**4,700**	date missing
5	+	+	-	-	+	+	**2,290**	
6	+	+	-	+	-	+	**160**	
7	+	-	+	-	+	+	**14,668**	
8	+	-	-	-	-	+	**400**	
9	-	+	+	+	-	-	**40**	
10	-	+	+	-	+	+	**23**	

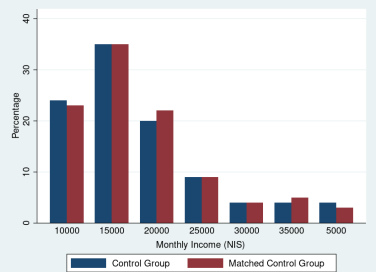
Appendix C. Distribution of the Key Variables in the Control Group versus the Control Group Match to the Treatment Group



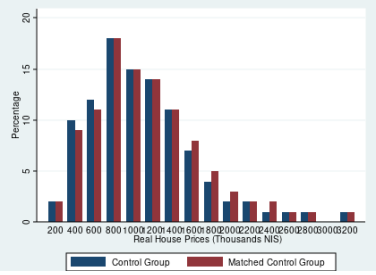
Panel A: Distribution of Age in the Control Group versus the Control Group Match to the Treatment Group



Panel A: Distribution of Rooms in the Control Group versus the Control Group Match to the Treatment Group



Panel A: Distribution of Borrowers Income in the Control Group versus the Control Group Match to the Treatment Group



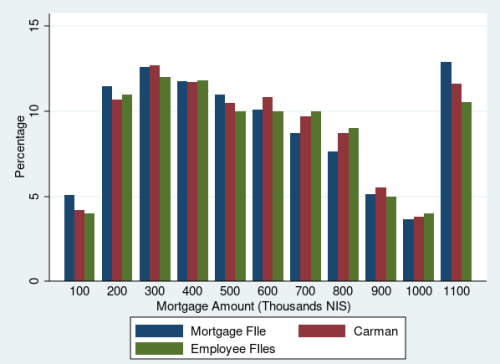
Panel A: Distribution of Real Home Prices in the Control Group versus the Control Group Match to the Treatment Group

Appendix D. Distribution of Key Variables in the Mortgages and CARMAN Files: Separate versus Merged Sample

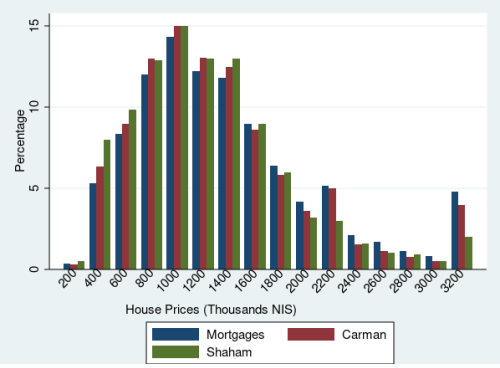
After the mortgages file is merged with the CARMAN file, which contains information on property characteristics, the question of whether the observations of the merged file indeed reflect the observations in the CARMAN file must be considered. One advantages of combining the mortgages file with the CARMAN file is the potential to identify the reason for the acquisition. This field distinguishes among first-time home buyers, upgraders, and investors. Because this information is incomplete in the mortgages file, the CARMAN file is particularly useful, as it provides accurate information about the reason for the purchase. Below is a comparison of reasons for the purchase in the CARMAN file versus in the mortgages files and in the employee file between early 2010 and May 2011. The differences between the two samples can also be attributed to the fact that the mortgages file contains data only about those who have taken out mortgages, which does not necessarily represent the entire population of home buyers.

Cause of Purchase	Carman	merged - mortgage file	employee files
First Home Buyers	34%	42%	43%
Improvers	37%	40%	42%
Investors	29%	18%	15%

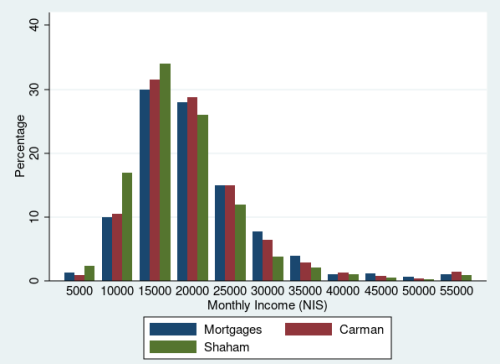
CARMAN is linked to the employee file obtained from the Israel Tax Authority, containing demographic and income information on a random sample of about 10% of the employees in Israel. A Kolmogorov-Smirnov test of equality in the distribution of mortgage amounts, house prices, and borrower income shows no significant differences among the three resources.



Panel A: Distribution of Mortgage Amount



Panel B: Distribution of House Prices



Panel C: Distribution of Borrowers Income

Appendix E. Summary Statistics: Strict LTV limit, Leveraged Borrowers

First-Time Home Buyers					% Observation = 12.4
	Mean	Sd. dev.	25%	50%	75%
House price (NIS)	782,739	301,108	572,500	760,000	935,000
Borrower monthly income (NIS)	12,156	5,340	8,711	11,500	15,000
Borrower age	35.2	6.8	30.7	34.3	39.9
LTV (%)	87.9	4.1	86.0	88.0	89.0
Average interest rate (%)	3.51	0.77	2.84	3.11	3.72
Loan duration (years)	26.3	5.0	25.0	28.5	30.0
Area (m^2)	79.4	23.1	62.0	76.0	94.0
Rooms	3.6	0.8	3.0	3.5	4.0
Distance from Tel Aviv-Jaffa (km)	46.1	34.0	16.1	40.7	85.4
Socioeconomic level of neighborhood	10.0	2.6	8.0	10.0	12.0

Upgraders					% Observation = 12.3
	Mean	St. dev.	25%	50%	75%
House price (NIS)	1,119,732	526,471	790,000	1,020,000	1,350,000
Borrower monthly income (NIS)	15,200	8,274	10,625	14,500	20,133
Borrower age	40.1	7.4	34.7	39.2	44.3
LTV (%)	77.7	6.8	74.0	75.0	83.0
Average interest rate (%)	3.26	0.64	2.86	3.10	3.43
Loan duration (years)	26.0	5.4	24.5	26.7	30.0
Area (m^2)	103.6	42.1	78.0	100.0	120.0
Rooms	4.2	1.0	3.5	4.0	5.0
Distance from Tel Aviv-Jaffa (km)	43.9	24.5	21.0	51.0	88.4
Socioeconomic level of the neighborhood	11.1	3.2	9.0	11.0	13.0

Investors					% Observation = 60.1
	Mean	St. dev.	25%	50%	75%
House price (NIS)	1,012,185	753,054	480,000	825,000	1,300,000
Borrower monthly income (NIS)	16,765	11,789	10,100	13,962	19,500
Borrower age	42.9	10.0	35.5	41.6	49.0
LTV (%)	65.0	8.8	59.7	64.0	70.0
Average interest rate (%)	2.98	0.82	2.49	2.89	3.40
Loan duration (years)	20.1	7.2	15.0	20.0	25.0
Area (m^2)	85.7	62.3	55.0	74.0	102.0
Rooms	3.6	1.2	3.0	3.0	4.0
Distance from Tel Aviv-Jaffa (km)	49.3	35.7	12.7	49.2	85.3
Socioeconomic level of neighborhood	10.3	3.6	8.0	10.0	13.0

The table reports summary statistics for the sample, which is distributed into three groups, according to type of buyer. Each panel reports detailed information for each subgroup of affacted borrowers within the group: First-Time Home Buyers - those with LTV above 75%, Upgraders - those with LTV above 70%, Investors - those with LTV above 50%. The sample covers the period January 2010-October 2012 (before the LTV limitation), and the percentage of observations represents the percentage of affected borrowers within the group. Number of observations: 34,021 borrowers.

Appendix F. Effect of LTV Limit on Housing Market, by Buyer Type

The sample is divided into two groups according to the average age of borrowers: young[25] up to age 40 versus older adults. The matching process is carried out for each group individually (as shown in equation (3)). Both groups were affected by the LTV limit, yet older adults were more affected than younger ones. Among the older borrowers, real housing purchase prices dropped by a significantly higher percentage. Also, older borrowers reduced the size of the housing units purchased, albeit not significantly, and moved significantly farther away from the center. These results may be attributable to the possibility that older adults are more flexible in their purchasing decisions and can either delay purchasing or compromise on the type of assets, as opposed to younger adults, who may have different limitations and constraints that require them to purchase specific properties at particular locations, such as near their parents or workplace.

In the second stage, the sample is redistributed into three groups, according to type of buyer: first-time home buyers, home buyers seeking to upgrade their housing situation, and investors (owners of more than one residential property). The matching process is carried out again for each groups separately (as shown in equation (3)). It seems that the investors are more affected by the imposition of the LTV limit. The price of their housing purchases declined sharply, and they purchased assets farther from the Tel Aviv center. Apparently investors are more flexible in their responses to limits because they are not purchasing a primary residence and they are weighing only investment considerations. There is no evidence to suggest that the LTV limit discriminates against weaker population segments.[26]

[25] The median age of mortgage borrowers is 41.5. The percentage of young borrowers is 49 before the imposition of the LTV limit and 51 after.

[26] Igan and Kang (2011) obtain similar results, namely, that older households and investors are more influenced by policy interventions.

Table VIIIEffect of LTV Limit on Housing Market: Matching Procedure by Age Groups

		House Prices (NIS)	Size (sq.m.)	Rooms	Distance from Tel Aviv (KM)	Quality of Neighborhoods
First-time home buyer	ATT	-60,179***	-2.28*	-0.04*	1.85***	-0.6**
		[9,984]	[1.23]	[0.02]	[1.1]	[0.3]
	ATT (%)	-8%***	-3%*	-1%*	2%***	-0.5%**
Upgraders	ATT	-93,021***	-1.43*	-0.02	3.9***	-1.1***
		[12,165]	[1.1]	[0.02]	[1.1]	[0.3]
	ATT (%)	-8%***	-1%*	0%	9%***	11%***
Investors	ATT	-122,680***	-0.13***	-0.08*	5.57***	-1.5***
		[22,940]	[0.04]	[0.04]	[1.9]	[0.41]
	ATT (%)	-12%***	0%***	-2%*	9%***	15%***

Note: Heteroskedasticity-consistent standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively. Treated borrowers are defined as those that borrowed After the LTV limit (October 2010). The un-treated borrowers are those that borrowed before the LTV limit. Control borrowers are a subset of the untreated borrowers selected as the closest match to the treated borrowers based on a set of borrower characteristics: Age and income. ATT is the Abadie-Imbens bias corrected average treated effect matching estimator.

Table IX Effect of LTV Limit on Housing Market: Matching Procedure by buyer type Groups

		House Prices (NIS)	Size (sq.m.)	Rooms	Distance from Tel Aviv (KM)	Quality of Neighborhoods
First-time home buyer	ATT	-60,179***	-2.28*	-0.04*	1.85***	-0.6**
		[9,984]	[1.23]	[0.02]	[1.1]	[0.3]
	ATT (%)	-8%***	-3%*	-1%*	4%***	6%**
Upgraders	ATT	-93,021***	-1.43*	-0.02	3.9***	-1.1***
		[12,165]	[1.1]	[0.02]	[1.1]	[0.3]
	ATT (%)	-8%***	-1%*	0%	9%***	11%***
Investors	ATT	-122,680***	-0.13***	-0.08*	5.57***	-1.5***
		[22,940]	[0.04]	[0.04]	[1.9]	[0.41]
	ATT (%)	-12%***	0%***	-2%*	9%***	15%***

Note: Heteroskedasticity-consistent standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively. Treated borrowers are defined as those that borrowed After the LTV limit (October 2010). The un-treated borrowers are those that borrowed before the LTV limit. Control borrowers are a subset of the untreated borrowers selected as the closest match to the treated borrowers based on a set of borrower characteristics: Age and income. ATT is the Abadie-Imbens bias corrected average treated effect matching estimator.

Appendix G. Changes in Interest Rate for the Affected Borrowers, Event Study Methodology

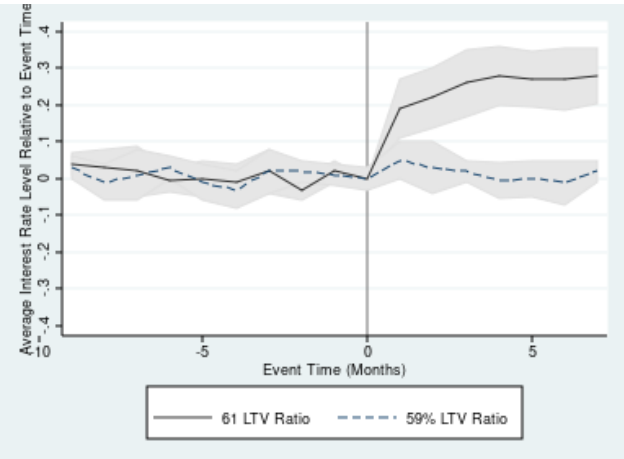
I also adopt a quasi-experimental approach based on event studies around the implementation of the soft LTV limit, between two types of borrowers, a borrower with LTV above the 60% LTV

threshold ("leveraged borrower") and a borrower with LTV just below the threshold ("non-leveraged borrower").[27] For each observation in the data, i.e. loan origination, I denote by t = 0 the month of the implementation and index all months relative to that specific month. The baseline specification considers a balanced of borrowers who I observe every month between 10 month before the soft limit and 7 month after, and so event time t runs from -9 to +7. I study the evolution of the mortgage average interest rate as a function of the event time. Specifically, denoting by y^l_{imt} the interest rate for individual i with leverage l of in month m at event time t, I run the following regression separately for leveraged borrowers and non-leverage borrowers:

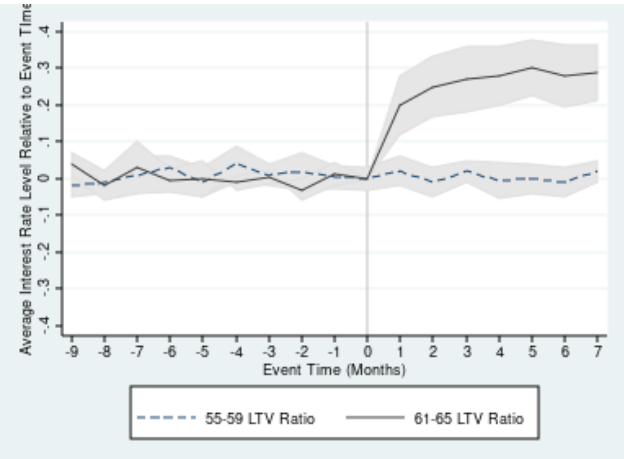
$$y^l_{imt} = \sum_{j \neq 0} a^l_j \times \mathbb{1}[j = t] + \sum_k \beta^l_k \times \mathbb{1}[k = loan_{is}] + \sum_y \gamma^l_y \times \mathbb{1}[y = m] + v^l_{imt} \tag{G1}$$

where I include a full set of event time dummies (first term on the right-hand side), loan and borrowers characteristics dummies (second term) and month dummies (third term). Loan and borrowers characteristics include borrower's age and income, bank and loan duration. I omit the event time dummy at t = 0, implying that the event time coefficients measure the impact of LTV limit relative to the month of the LTV limit implementation. By including a full set of loans and borrowers characteristics dummies I control non-parametrically for underlying difference between individuals, and by including a full set of time dummies I control non-parametrically for time trends such as monetary policy and other macro-economic events. We see in Figure 17 that, once loan and borrower characteristics and time trends are taken out, the interest rate of 61% LTV ratio loan and 59% LTV ratio loan evolve in a parallel way until the implementation of the soft LTV limit (Panel A). But at the precise moment the soft LTV limit implemented, the interest rate of the 61% LTV loan diverges and experience an immediate increase in the average interest rate of 0.2 percentage point, while the 59% LTV loan experience no visible change in the interest rate. The average interest rate increases more in the following month and reaches an increase of 0.3 percentage point. Similar results are shown in Panel B, for the 61-65% LTV ratio loans compare to the 55-59% LTV ratio loans.

[27] I also extend the definition to 55-59% LTV ratio and 61-65% LTV ratio for leveraged and non-leveraged borrowers, respectively.



Panel A: Interest Rate Gap between 59-61 LTV Ratios



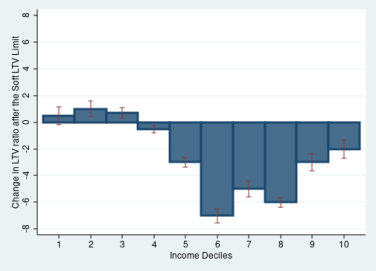
Panel B: Interest Rate Gap between 55-65 LTV Ratios

Figure 17. Impacts of soft LTV limit on Interest Rates. Notes: The graphs show event time coefficients estimated from equation (G1) for leveraged borrowers and non-leveraged borrowers. The Shaded 95% confidence intervals are based on robust standard errors.

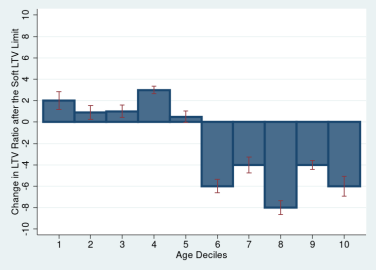
Appendix H. The Effect of Interest Rate on Leverage Choices, By Buyer Type

The soft LTV limit increased interest rate on loans with LTV above the 60% threshold and incentivized borrowers to lower their leverage. This section discusses the characteristics of those that due to the increase in the interest rate, decided to lower there LTV ratio, using the difference in LTV choices before and after the limit. I divided the borrowers according to their income, age and

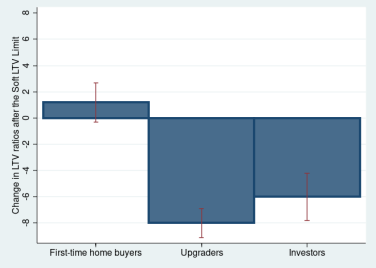
borrower type. Each time I examine their LTV choices before and after the limit, using matching method, explained in Section (III.A).[28] This test suffers form identification challenge as it considers only one difference between the groups and there is no control group that we can compare to, but it gives us a notion that the effect of increase interest rate was not equal between different borrowers.



Panel A: Average Change in LTV Ratio after the Soft Limit, by Income Deciles



Panel B: Average Change in LTV Ratio after the Soft Limit, by Age Deciles



Panel C: Average Change in LTV Ratio after the Soft Limit, by Buyer Type

Figure 18. Average Change in LTV Ratio after the Soft Limit. Notes: The graphs show the average change in LTV ratio after the soft LTV limit, by groups of borrowers characteristics.

[28] i.e., difference in LTV ratio before and after the Limit between two borrowers with similar characteristics

Appendix I. Effect of Strict LTV Limit on Housing Market, By Buyer Type

Here I calculate the elasticity response due to the strict limit for each borrower type (using the predict LTV distribution) and examine which types are most affected by the policy. The elasticity response of the strict LTV limit is calculated differently for each borrower type, according to:

$$\eta = \frac{\Delta Y}{\Delta LTV} \times \frac{\overline{LTV}}{\overline{Y}} \tag{I1}$$

The table shows the elasticity of the change in the choices of borrowers in the credit and housing markets, for each borrower type.

	First-Time Home Buyers	Upgraders	Investors
Home prices	0.61***	0.49***	0.68***
	(0.15)	(0.13)	(0.17)
Size	0.73***	0.51***	0.85***
	(0.23)	(0.18)	(0.25)
Distance from Tel Aviv	0.72***	0.62***	0.82***
	(0.27)	(0.26)	(0.33)
Socioeconomic level of neighborhood	0.62***	0.36*	0.67***
	(0.17)	(0.23)	(0.22)
Interest Rate	0.53***	0.51***	0.75***
	(0.1)	(0.2)	(0.2)
Maturity	0.33***	0.17**	0.23***
	(0.05)	(0.09)	(0.05)
Default	0.93***	0.88***	0.63*
	(0.2)	(0.3)	(0.4)

Elasticity of Demand in the Credit and Housing Markets. This table shows the elasticity of demand in the credit and housing markets for each borrower type. The elasticity of demand is calculated by comparing the change in the borrower's LTV choices to the change in the borrower's choices in the credit and housing markets.