

# Unintended Consequences of LTV Limits on Credit and Housing Choices\*

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December 17, 2019

## Abstract

This paper investigates the implications of credit constraints on housing decisions. I exploit the introduction of LTV limits in Israel in the years 2010 and 2012 and find that LTV limits increase interest rate and reduce loan amount. Although borrowers continue borrowing and purchase apartments, the LTV limits induced them to buy cheaper homes and to move farther from high demand areas to lower socio-economic neighborhoods. Buy-to-let investors were more affected by the LTV limit than first-time home buyers. My evidence suggests that LTV limits - a commonly used macro-prudential policy tool - affects not only financial stability but also affects borrowers' decisions in the credit and housing markets.

*Keywords: Macroprudential policy, LTV, mortgage, housing market, household finance*  
*JEL Classification: E43, E58, E61, G21, G28, R28, R51*

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\*I thank Yishay Yafeh, Efraim Benmelech, Nathan Sussman, David Genesove, Martin Eichenbaum, Narat Charupat, Jagdish Tripathy, Robert Kelly, Lucas Teixeira, Ángel Estrada, Stephanie Johnson, two anonymous referees, as well as seminar participants at the Bank of Israel Research Department, Bar-Ilan University, Hebrew University of Jerusalem, Faculty of Agriculture, Food and Environment at the Hebrew University, Federal Reserve Board, IMF Macro-Financial Division, Bundesbank Research Seminar, University of Wisconsin–Madison, University of Illinois at Chicago, Northwestern University, 2016 Annual IFID Centre Conference, RiskLab /BoF/ ESRB Conference on Systemic Risk Analytics, Joint ECB & Central Bank of Ireland research workshop, The 35th annual conference of the Israel Economics Association, CEBRA 2018, CEPR conference on Systemic Risks and Macroprudential policy, Second Conference on Financial Stability of Banco de España and 2020 American Economic Association Annual Meetings for their helpful comments and insightful discussions. This paper was part of my Ph.D. dissertation at the Hebrew University of Jerusalem. This paper was awarded for the Sapir Prize and for the Gaathon Prize for outstanding research in Israel's economy. All errors are my own. The views expressed herein are those of the author and do not necessarily reflect the views of the Bank of Israel.

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# 1. INTRODUCTION

Ten years after the financial crisis of 2008, some of the drivers and implications of the crisis are coming into better focus. Most observers agree that mortgage lending and housing markets played central role during the financial crisis and contributed to the drop in consumer spending and in output growth, to higher unemployment and slowed the recovery from the Great Recession (Mian and Sufi (2011), Mian et al. (2013), Mian et al. (2017)). As a result, Policymakers in most of the advanced economies have recently implemented macroprudential policies (MPPs) aimed at limiting household leverage so as to slow down the feedback loop between credit and house prices.

While there is an increasing global adoption of such policies, mainly in implementing LTV limits,<sup>1</sup> and considerable empirical evidence demonstrating that LTV limits reinforce the stability of banks by reducing potential risks from borrowers in case of sharp declines in housing prices,<sup>2</sup> the transmission channels of LTV limits at the borrower level are not well explored in the literature. In particular, LTV limits may influence the credit and housing choices of affected borrowers.

This paper aims to advance our understanding of the effect of financial regulation on the real economy. Combining unique property-level characteristics dataset with detailed loan-level dataset that includes borrower characteristics, I study the introduction by the Bank of Israel of two types of loan-to-value (LTV) limits introduced in 2010 and 2012. The 2010 limit increases risk weights on a certain LTV threshold (Soft LTV limit), operates as an implicit tax on lenders when originate loans with high LTV ratios. The 2012 policy limit the LTV ratio to three different thresholds, according to the buyer's type (Strict LTV limit). By studying the borrowers response to LTV limits, this paper sheds new light on the direct and indirect effect of most widely used type of macroprudential regulations.

I find that the LTV limits increase the interest rate paid by affected borrowers, meaning that the higher costs imposed on lenders end up being economically born by borrowers in the form of higher interest rates. Many other ex-ante restrictions on household leverage, such as Qualified Mortgages program in the US, study by DeFusco et al. (2019), operate in a similar fashion by penalizing lenders for issuing

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<sup>1</sup>Alam et al. (2019) collected data from 1990 to 2016 on the adoption of 17 types of macroprudential policies in 134 countries and find that LTV limits are the most widely used tool in advanced economies (have been adopted by 60 countries).

<sup>2</sup>See Akinci and Olmstead-Rumsey (2018) and Cerutti et al. (2017a).

loans with certain risky characteristic. This increase in the interest rate induced some borrowers to reduce their leverage, leading to decline in loan amount. Overall, there wasn't any significant change in the distribution of borrowers' age and income after the imposition of each LTV limit, nor a decline in number of housing transactions. So borrowers continue purchasing housing units, but with different characteristics: I find that due to the LTV limits affected borrowers bought cheaper houses, smaller, farther from the business center, and in lower-graded (in terms of socio-economic indicators) neighborhoods. Therefore, I find evidence that those macroprudential policies, who focus on the stability of the banking system, have micro implications in the sense that it might induce borrowers to change (mainly) the location and the socio-economic level of their housing unit. Moving farther from the center of Israel, to lower graded neighborhoods probably have crucial implications on commuting costs, level of education, labor market etc.

An LTV limit is designed to increase borrower resilience and to lower bank losses during downturns by requiring a higher equity stake and lower leverage; see, e.g., [Campbell and Cocco \(2015\)](#) for theoretical arguments and [Demyanyk et al. \(2011\)](#) for empirical evidence. The 2010 limitation increases the capital provision for mortgages with an LTV limit of greater than 60 percent ("soft LTV limit"). This guideline did not apply to housing loans originally amounting to less than NIS 800,000 (approximately US \$200,000). Then, in 2012, the Bank of Israel limit the LTV ratio according to the type of buyer ("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 effect of the LTV limits on the credit and housing markets outcomes, I use a difference-in-differences matching research design. In the soft LTV limit, Ex post, the borrower could have taken an LTV of greater than 60 percent and paid a higher interest rate, or she could have chosen to buy a different asset with an LTV of less than 60 percent. To estimate these 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 cut-off. To overcome the challenge of identify the affected borrowers, this paper tries to 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 cut-off after the policy, and I match them the closest household from the period

before based on age and income of the borrowers. Then, I divided borrowers to two groups: the control group, households that chose the same LTV ratio before the policy, slightly below the cutoff, and treatment group, households that chose before the limitation to be above the LTV cut-off.

I find that both types of LTV limits increase the price of the mortgage. However, in the soft LTV limit, using the variation across the 60 percent LTV threshold, I can better identify the effect on the interest rate, than in the strict LTV limit. This is done using the difference in the interest rate paid by two identical borrowers, i.e. with similar observable characteristics, one with an LTV ratio slightly below 60 percent and the other with one slightly above this threshold. Before the regulation there was no difference in the interest rate paid by these two borrowers. After the regulation, the interest rate paid by a borrower with an LTV ratio just above 60 percent was, on average, 0.21–0.36 percentage points significantly higher than the interest rate paid by the borrower with identical characteristics just below the LTV limit. In terms of interest rate payments, the soft LTV increases the yearly interest rate payments, 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 introduction of the LTV limit, suggesting that some borrowers decided to lower their LTV ratios. In terms of the strict LTV limit, After the limitation there was high density around the value of the cut-offs, 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 due to the LTV limit. However, an alternative interpretation is that the results reflect borrower selection. That is, does the distribution of borrowers characteristics changes due to the imposition of the LTV limits or, instead, do banks avoid giving loans to certain households? In the Israeli case of implementing soft and strict LTV limits, there are no support for the credit rationing, i.e. no significant change in the distribution of borrowers' age and income after the imposition of each LTV limit.

The results of the paper show that due to the LTV limits affected borrowers bought lower quality assets: cheaper, smaller, farther from the business center, and in lower socio-economic graded 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.<sup>3</sup> This is especially true for investors, who changed

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<sup>3</sup>This paper assumes that the housing supply is inflexible, at least in the short term. It also

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, MPP will not stop them from entering the housing market but may lead them to lower leverage and buy cheaper houses. This is inline with [Igan and Kang \(2011\)](#) paper, which suggests that investors are more flexible in their purchasing decisions than first-time home buyers, who usually have different limits that require them to purchase specific properties in particular locations, such as close to their parents or their workplace.

To understand the magnitude of the changes in housing choices, 55 percent of the Israeli population lives in the center of the country, within a 40-kilometer radius of Tel Aviv. In the first six months after the imposition of the LTV limit, affected borrowers moved, on average, 3.8 km in the soft limits and 7.1 km in the strict limit, farther away from Tel Aviv, to significantly lower-rated neighborhoods. Although the average move was of 4.3-7 km from Tel-Aviv, almost 20 percent of the borrowers moved 5–10 km and almost 17 percent moved 10–20 km farther from the center, and overall 70 percent of the borrowers moved farther away 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. According to Google Maps, in terms of commuting, moving farther from the center by 4.3-7.1 KM means between 40 minutes to 1 hour more on the road each day.

However, imposing a strict LTV limit on new contracts yield counterintuitive results in the credit market. Although a strict LTV limit 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. There are three possible explanations for this outcome. First, 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 the risk pricing of the banks (this possible explanation was taken from [De Araujo et al. \(2016\)](#)). This paper add two other possible explanations: Second, the affected borrowers bought riskier assets, i.e., farther from the center, which may have increased the interest rate

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examines 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, questions such as, who bought the remaining dwellings in the center of Israel (that the treatment group could not afford due to the limit), what happened to the prices of those dwellings, and did the LTV limit have an effect on the supply of housing (for example, did developers start to build more homes in the periphery, or smaller homes), are not within the scope of this paper.

on those loans. Third, due to 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 go up. Regarding the last explanation, MPPs typically target large financial intermediaries (e.g., banks) by enforcing the LTV ratio. 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). To the best of my knowledge, Braggion et al. (2018) is one of a very few studies that use loan-level data to examine the shift from mortgages to other credit channels following the imposition of LTV limits.<sup>45</sup> Consumer credit in Israel is not subject to the same scrutiny as mortgages, and therefore it may pose new risks to the financial system. In particular, it may lead to an excessive buildup of household leverage, as happened in the U.S. 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 that LTV limits in terms of pushing borrowers farther from city center to lower graded neighborhoods. This study 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 borrower's reactions. Most of the 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 loan level dataset merged to housing characteristics dataset showing that macroprudential policies have micro effects on borrower's choices in the housing market. To my knowledge, there are only a few existing studies using microeconomic data to examine the effects of MPP tools on household choices in the credit and housing markets (e.g., Igan and Kang (2011); Han et al. (2017); Acharya et al. (2017); DeFusco et al. (2019)). The present study is the first to examine the impact of an LTV limit on housing unit characteristics other than price, such as asset size

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<sup>4</sup>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.

<sup>5</sup>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 macroprudential policy beyond banking.

and location. Finally, LTV limits are occasionally criticized for preventing groups needing more access to credit markets from obtaining a loan. The household-level database used here enables to evaluate the impact of an LTV limit on different segments of the population, especially those with limited access to credit. As such, this paper 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 (2013)).

The rest of the paper proceeds as follows. Section 2 provides background on the housing market and housing finance in Israel. Section 3 discusses data sources and summary statistics. Section 4 describes the identification approach. section 5 reports results on the impact of LTV limit on borrowers' choices in the credit and housing markets. Section 6 discusses other potential effects of LTV limits and Section 7 concludes.

## 2. BACKGROUND

### 2.1. *The Housing and Credit Markets in Israel*

Israel's financial system was not markedly affected by the recent global financial crisis. However, the relatively healthy condition of the economy and its 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 in Israel, especially housing prices.<sup>6</sup> Between 2007 to 2016, nominal housing prices have risen by 128 percent, and real housing prices have risen by 95 percent. At the same time, the volume of housing loans has increased by 95 percent, raising concerns among policymakers. 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 intended 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 their ability to make future repayments, but also to slow the pace of home price increases.

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<sup>6</sup>Other factors contribute to the increase in housing prices, such as demographic, increase in households' 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 an increase in housing prices (e.g., Germany, Switzerland), but the degree of the increase in housing prices observed in Israel stands out from the most of them (Cerutti et al., 2017b).

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

## 2.2. *The LTV Limits*

### 2.2.1. *Soft LTV Limit*

The soft LTV limit is the first of these MPPs, whereby, in October 2010, the Israeli Supervisor of Banks issued a directive requiring banks to increase capital provisions for residential mortgages with high LTV ratios. As a result of this directive, required capital provisions for mortgages with variable interest-rate portions of 25 percent or more and LTV ratios greater than 60 percent rose from the existing 35–75 percent (depending on the loan characteristics) to 100 percent.<sup>7</sup> Importantly for the analysis, the guidelines applied only to mortgages larger than NIS 800,000 (approximately USD 200,000). This paper focuses 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). Thus, the time span includes the 10 months before the soft LTV limit and the 7 months after. Since the soft LTV limit would force the banks to tie up more capital against these loans, borrowers wanting to take a loan with an LTV greater than 60 percent faced higher interest rates, which incentive them to lower 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 percent (or less). Therefore, Figure 2 presents the LTV distribution after the limit with two picks, one around LTV ratio of 75 percent and around LTV ratio of 60 percent.

### 2.2.2. *Strict LTV Limit*

The strict LTV limit was implemented in November 1, 2012. It established that a banking corporation could not approve mortgage with an LTV of more than

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<sup>7</sup>Israeli households are not very indebted and LTV ratios on mortgages are relatively low: in 2010, the average LTV was 52 percent.

70 percent—excluding a housing loan granted to a borrower for the purpose of acquiring a first home, to which a maximum LTV of 75 percent would apply. In addition, the directive established that a banking corporation could not approve a loan with an LTV above 50 percent to a borrower for the purpose of purchasing an investment home.<sup>8</sup> The directive was intended to reduce the significant effects of the realization of a crisis in the real estate market, by reducing the risk inherent in the housing credit portfolio and reducing the risk inherent in taking out a housing loan with a high LTV ratio. Finally, it is important to highlight that the new directive was unexpected from the standpoint of market participants, as regulators in Israel had never used strict LTV limits. Moreover, prior regulation had strongly favored regulatory capital measures using risk weights (soft LTV limit). This paper focuses 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). Thus, the time span includes the 10 months before the new LTV limit and the 10 months after it. Figure 3 illustrates the distribution of LTVs 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 percent range due to the adoption of the Basel II Standard Approach Guidelines adopted in 2008, and at 60 percent due to the implementation of the soft LTV limit in October 2010, which increased the capital requirement for loans with LTV ratios above 60 percent. Figure 3 clearly shows how the 2012 LTV limit changed the LTV distribution: after the 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.

### 3. THE DATABASE

#### 3.1. *Data Construction*

The data used in this study come from 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 seven lenders account for roughly 95 percent of all mortgage loans in Israel, making this a rich source of data. Information is

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<sup>8</sup>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.

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 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.

This study focuses on a limited time period framing the soft LTV limit and strict LTV limit (the period that is coloured in red in Figure 1). The 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 macro-economic events or additional MPPs that were imposed afterwards (as shown in figure 1).

This dataset was merged with another dataset on housing unit characteristics (CARMAN) from the Israel Tax Authority, containing information on all home sale transactions and their characteristics. In the CARMAN dataset, each housing unit has its own ID. It is important to note that the recording of the IDs in the mortgage dataset is distorted. Therefore, the merger was not a direct merger but a statistical one, using other remaining fields in the two datasets: price, date, and city of the purchased asset. Appendix B shows the process of how these two datasets were merged. In summary, the original mortgage dataset was merged with one-third of all 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 (Appendix C and D), a Kolmogorov–Smirnov test of equality distributions showed no significant difference between the merged observations to the overall dataset.

### 3.2. *Summary Statistics*

The dataset was 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 until October 2010) and 11,224 observations after (until May 2011). There are 17,260 observations before the strict LTV limit was imposed (January 2012 until October 2012) and 16,761 observations after it was imposed (until August 2013). Table 1 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 already 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 Israeli Central Bureau of Statistics (CBS) publishes a socioeconomic index of neighborhoods consisting of 16 different variables, including demographic, education, employment, income, and standard of living. These 16 variables are compiled into a single index, and all neighborhoods in Israel are classified into one of twenty clusters, 1 being the lowest socioeconomic status and 20 being the highest. Table 1 shows a decline in the socio-economic level of neighborhoods after the LTV limit. Hence households moved farther away from the business center to lower-graded neighborhoods. The econometric challenge in this paper will be to attribute these changes in housing preferences to the LTV limit.

Table 2 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 longer loan duration than the other two groups. Investors buy smaller assets that are farther from Tel Aviv, the business capital of Israel. Table 2 shows the socio-economic 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, which are of shorter duration and have a lower payment-to-income ratio, compared to first-time home buyers. Investors have higher income (75 percent of them are from the income deciles 6 to 10) and are older (the average age in those income deciles is 47). Although investors are less risky borrowers in all of those risk indicators (leverage, loan duration, age and income), the average interest rate paid by investors is higher than the other two groups. This is consistent with the disagreement in the literature regarding the risk inherent in real-estate investors (compare to first time home buyers): [Agarwal et al. \(2018\)](#) find that investors have a better risk profile ex ante: relative to first-time home buyers, investors are older, better educated, earn higher incomes, and have longer tenures with the bank. In contrast, [Haughwout et al. \(2011\)](#) find that investors are more risky because they will be the first to dampen their homes in a case of a bust, as seen in the GFC, where in 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 contribute to higher rates of default.

Appendix E presents summary statistics for leveraged borrowers before the LTV limit was imposed: first-time home buyers with an LTV above 75%, upgraders with an LTV above 70%, and investors with an LTV above 50% limit. Overall, the leveraged borrowers account for 19 percent of all borrowers: 12.4 percent of first-time home buyers and 12.3 percent of upgraders had LTVs above these limits before the policy intervention, while among investors the rate was 60.1 percent.<sup>9</sup> 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.

## 4. IDENTIFICATION APPROACH

### 4.1. *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 LTV limit, but only 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 LTV 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), i.e.,  $T_i$ . 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 limits and the choices made without imposing the limit. For example,

$$Y_1 = E(Y_{i1} | T_i = 1) - E(Y_{i0} | T_i = 1) \quad (1)$$

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 naïve estimator will be

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<sup>9</sup>As can be Appendix E, the average LTV for investors was 52.5 percent, above the limit cutoff.

$$Y_1 = E(Y_i | T_i = 1) - E(Y_i | T_i = 0) \quad (2)$$

Abadie and Imbens (2011) notes that ATE might not be of relevance to 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 we will consider ATT, the parameter of interest in most evaluation studies.

Because choosing housing assets and taking a mortgage are not random 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 was not treated based on observable characteristics, and then compare the results for the paired households by estimating the average effect of the treatment group to obtain the average treatment effect (ATT) for those in the treatment group (Atanasov and Black, 2016).

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

$$ATT = E(Y_1 - Y_0 | T = 1, X) \quad (3)$$

$Y_1$  and  $Y_0$  are the outcome variables for households that were treated and those that were not treated, respectively (Abadie and Imbens (2006), Abadie et al. (2004)).<sup>10</sup> <sup>11</sup> <sup>12</sup> For each  $i$ , matching estimators impute the missing outcome by

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<sup>10</sup>The matching method is used rather than propensity score matching when the database is large and there are a small number of observable variables, similar to the situation in this study.

<sup>11</sup>The calculations were made using STATA software employing the command `Nnmatch` (Nearest-neighbor matching), which is explained in detail in Abadie et al. (2004). The `Nnmatch` command developed in the article by Abadie and Imbens (2006) allows for matching with replacements, which can be referred to as 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.

<sup>12</sup>I assume unconfoundedness (selection of observables or conditional independence). The intuition for unconfoundedness is that if the decision to take the treatment is purely random for

finding other individuals in the data whose covariates are similar but who were 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. This study recognizes this issue and implements a bias-correction component to the outcome variables.<sup>13</sup>

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). The reason for choosing these levels of loan amounts is that there is a low probability of overlap between the two groups, but at the same time the groups are not very different in terms of observable characteristics.<sup>14</sup>

Figure 9 shows that the LTV limit affected the treated group, resulting in a change in the distribution of LTV to significantly lower values according to the Kolmogorov–Smirnov test, while there was no change in the distribution of LTV in the untreated group.

“Before the limit” is the time period between the beginning of 2010 and October 2010 (when the 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 the treatment group and control group. So the ATT now is calculated as:

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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.

<sup>13</sup>For more details, see Abadie et al. (2004).

<sup>14</sup>Other groups have been tested as well, such as NIS 650-750 thousands versus NIS 850-950 thousands, and I find similar results, but due to the trade-off between observing similar groups while rolling out overlapping between the groups, I decided to stay with the NIS 600-700 versus NIS 800-900 thousands groups.

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

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 were 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 (in kilometers), and the socio-economic level of the neighborhood (scale of 1 to 20).

Table 3 presents the characteristics of the two groups before the matching process. Borrower characteristics differed significantly between the treated and the untreated 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 3, the matching process results in insignificant differences in the observed characteristics between the groups. This leads to the conclusion that there is 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, it could be the case that income groups will be omitted from the sample because only half of the control group was matched. Appendix F examines the differences between the borrowers' characteristics in the control group that were matched to the treatment group compared to the borrowers in the control group. The borrowers' age decreased between the two periods and therefore it can be expected to see a larger representation of young people. Because income increased between the two periods, it can be expected to see a representation of borrowers with higher incomes. Appendix D also shows that both groups purchased relatively similar properties and that there were no significant changes in the location and size characteristics of the properties. The Kolmogorov–Smirnov test confirms these results.

#### 4.2. *Strict LTV limit - Predicted LTV Ratio Using DID Matching*

This paper focuses on the policy's effect on the subset of borrowers constrained by the LTV limit. However, the treatment status can be observed only before the policy intervention. After the intervention, it is no longer possible to easily distinguish 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 wanted to borrow more but did not have the option, due to 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".<sup>15</sup>

The DID matching approach tries to identify the affected borrowers using prediction of the LTV ratio that the borrower would have chosen in the absence of the limitation. By this method, only households that are (slightly) below the cutoff<sup>16</sup> 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).<sup>17,18</sup> Then, the leverage choices of the matched borrowers, their actual choices before the policy and their choices after the policy, can be examined. Observations with an LTV ratio, before the policy, that is far from the cutoff is excluded, because the objective is to examine two groups that are close to the cutoff (local treatment effect). Then, only two groups remain: households that choose the same LTV ratio before and after the policy, which is slightly below the cutoff (the "control group"), and households that choose to be above the LTV cutoff, before the policy (the "treatment group").

The outcomes in the empirical application will 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 the socio-economic level of the neighborhood). To obtain the outcomes, I use the [Abadie and Imbens \(2011\)](#) estimator,<sup>19</sup> which makes it possible to match households based on their income

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<sup>15</sup>See also [Manski and Pepper \(2013\)](#).

<sup>16</sup>The assumption here is that those who want to borrow above the cutoff after the intervention will borrow slightly below the cutoff. See, e.g., [Igan and Kang \(2011\)](#).

<sup>17</sup>Other borrower characteristics include 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.

<sup>18</sup>[De Araujo et al. \(2016\)](#) use income as the only predictor of the LTV ratio.

<sup>19</sup>The calculations were made using STATA software employing the Nnmatch (nearest-neighbor

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<sup>20</sup> (ATT) for an individual with characteristics  $X$  (age and income):

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

The outcome variables for households that receive treatment and those that do not are  $Y1$  and  $Y0$ , respectively.<sup>21</sup>

After the matching procedure, I examine the difference between the choices of the treated borrowers and those of the untreated borrowers, using the difference-in-differences method. To control for unobservable or other macroeconomic events that might have affected the elasticity results, this paper uses traditional difference-in-differences (DID) estimation. DID estimation compare the outcomes of two groups: one group above the LTV cutoff and one group below the LTV cutoff (treated and untreated, respectively), before and after the LTV limit. The outcome of the untreated group will show the unobservable or other macroeconomic events that occurred concurrently (this paper assumes that the housing supply is inflexible, at least in the short term). Although the treated group cannot be observed after the policy restriction, it can be identified by means of the predicted distribution. Because the basic assumption<sup>22</sup> in DID estimation is that the treatment and control groups will both be very close to the cutoff [Abadie \(2005\)](#). This paper examines groups that are very close to the cutoff, while ruling out transitions between the groups (local treatment effect).<sup>23</sup>

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  $ATT = (\text{after/before}) \text{ treatment group} - (\text{after/before}) \text{ control}$ , and it is calculated separately for each outcome variable and for each borrower type.

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matching) command, which is explained in detail in [Abadie et al. \(2004\)](#). The `Nnmatch` command developed in the article by [Abadie and Imbens \(2011\)](#), called the Abadie–Imbens variable, allows for matching with replacements. This command lowers the bias and leads to greater similarity between observations, but increases the variance. In addition, when matching with replacements, the order in which the observations are matched is not important.

<sup>20</sup>See [Abadie et al. \(2004\)](#) for a review.

<sup>21</sup>See [Abadie and Imbens \(2011\)](#), [Abadie et al. \(2004\)](#) and [Heckman et al. \(1997\)](#).

<sup>22</sup>See [Abadie \(2005\)](#).

<sup>23</sup>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.

## 5. RESULTS

### 5.1. *LTV limits Increase Interest Rate and Lower Loan Amount*

The soft LTV limit required banks to set aside more capital against risky loans (i.e. loans with an LTV ratio above 60 percent). I examine whether the limit was binding and the reaction of the banks to this policy. Figure 4 shows that the banks increased the average interest rate charged from risky borrowers (borrowers with LTV ratio above 60 percent). Because Figure 4 presents the interest rate charged at two different time periods (before and after the LTV limit), I also compare the interest rate paid by two similar borrowers (matched by income, age, bank, and duration of loan) just above and below the 60 percent LTV limit (61 percent versus 59 percent in the first test and 61–65 percent versus 55–59 percent in the second test). Because the prime interest rate<sup>24</sup> changes between the periods, which could bias the results, I examine also the spread of the interest rate (over the prime). Before the regulation, there was no significant difference in the interest rate paid by borrowers above and below the 60 percent 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 percent was 0.21–0.36 percentage points higher than the interest rate charged to an identical borrower just below the LTV limit (Table 4).

Due to the increase in the interest rate due to the soft LTV limit, borrowers were incentivized to lower 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 percent (or less). Therefore, Figure 2 presents the LTV distribution after the limit with two picks, one around LTV ratio of 75 percent and around LTV ratio of 60 percent.

The strict LTV limit produce 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 cut-off and the number is the percentage of observations that were above the cut-off before the limitation. After the LTV limit there was high density around the value of the cutoffs, for each borrower type. Figure 5 also shows the high impact of the LTV limit on the LTV distribution of investors. Overall, for each buyer type, there was a significant change in the

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<sup>24</sup>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.

LTV distribution after the limit, according to the Kolmogorov–Smirnov test.

## 5.2. *No Evidence of Borrower Selection*

An important question in the literature that usually arises from changes in credit constraints is whether the 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 borrowers with limited access to the credit market? The literature calls this phenomenon “credit rationing” (Stiglitz and Weiss, 1981), which mainly refers to a situation where banks limit the supply of additional credit to borrowers based on their characteristics, even if the latter are willing to pay higher interest rates.

Therefore, this paper examines the distribution of borrower age and income before and after the imposition of each type of the LTV limits (figures 6 and 7). A Kolmogorov–Smirnov test shows that there was no significant change in the distribution of borrower age and income.

Using aggregate data from the Bank of Israel, this paper examines whether households left the credit and housing markets due to the LTV limit, i.e. the strict one, because in the soft one they can stay highly leveraged and just 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 dotted line in the time of the strict limit. As can be seen, apparently households did not leave those markets following the restrictions.

## 5.3. *LTV Limit Affect Housing Choices*

### 5.3.1. *Soft LTV limit*

This paper shows that due to the LTV limit, which increases the interest rate and reduce loan amount, borrowers left with less credit in order to purchase housing units. Table 5 compares the outcome variables of the housing units between the treatment group and the untreated group before and after the imposition of the LTV limit (as shown in equation 4). The difference-in-differences matching estimator shows that there was 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 Tel Aviv in the treated group compared with

the untreated group.<sup>25</sup> The difference-in-differences matching results indicate also a decline in the socio-economic level of the neighbourhoods after the LTV limit in the treated group versus the untreated group of 2.2 points (a 12 percent decline). Hence, the treatment group moved to significantly lower-graded neighbourhoods.

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

This paper assumes that the housing supply is inflexible, at least in the short term. It also examines 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, questions such as, who bought the remaining dwellings in the center of Israel (that the treatment group could not afford due to the limit), what happened to the prices of those dwellings, and if the LTV limit had an effect on the supply of housing (for example, if developers start to build smaller homes, in the periphery), are not within the scope of this paper.

An interesting question that arises from the results of the matching method is, what kind of neighborhoods did the borrowers move to? More specifically, one wonders whether some borrowers moved farther away from Tel Aviv but actually improved their welfare by increasing the socio-economic level of their neighborhood. There are, of course, very high-graded suburbs of Tel Aviv. Figure 11 shows the distribution of changes in neighborhoods grading by distance from Tel Aviv. The changes are divided into three groups: upgrade (green), downgrade (red), and no change in socio-economic level of their neighborhood (yellow). First, although the average move was of 4.3 km from Tel-Aviv, almost 20 percent of the borrowers moved 5–10 km and almost 17 percent moved 10–20 km farther from the center, which is a very large change for those borrowers. 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 G I examine the differential effect of the LTV limit on sub-segments 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

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<sup>25</sup>The variable distance from Tel-Aviv-Yafo was censored at 40 km, to focus only on the most populated areas in Israel, as shown in Figure 10.

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 decisions 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 close to their parents or their work.

### 5.3.2. *Strict LTV limit*

To control for unobservable or other macroeconomic events that might affect the results, we use difference-in-differences (DID) estimation to examine the local treatment effect of the policy intervention. DID estimation allows to use the reaction of the untreated 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 in the absence of the policy intervention. We only know 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, this will be done using the matching method. Tables 6 and 7 present the results of the second estimation strategy, difference-in-differences 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 of 70–75 and the treatment group has an LTV of 75–80. For upgraders, the control group has an LTV of 65–70 and the treatment group has an LTV of 70–75. For investors, the control group has an LTV of 45–50 and the treatment group has an LTV of 50–55. Then, a DID estimation were performed, in order to examine the difference between the credit and housing choices of the control and treatment groups before and after the policy.

Table 6 presents the results in absolute terms, and Table 7 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. We obtain coun-

terintuitive results on the effect of the imposition of a strict LTV limit on new contracts in the credit market. The LTV limit was supposed to lower the LTV ratio, thereby lowering the risk of the loan and leading to a lower interest rate and term to maturity. However, in the credit market I receive counter-intuitive results: for each buyer type there is an increase in the interest rate and in the term to maturity and will be discussed in Section 6. As Table 7 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 socio-economic level of the neighborhood. Also, one possible reason for the higher increase in the interest rate for investors is that the new policy led them to buy riskier assets, and in particular assets farther from the center (will be discussed in Section 6).

Overall, the most significant change is in the location of the housing unit: borrowers moved farther from the center of Israel. This is followed by a change in the socio-economic level of the neighborhoods, while the size of the housing unit changed the least. It can be seen that the 2012 strict LTV limit had a much greater impact on borrower choices in the credit and housing markets, compared to 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 socio-economic level of the neighborhood. The final difference between the two limits is that here the borrowers changed the size of their housing unit, whereas after the 2010 limit there was no such change.

Appendix H analyses 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 cut-offs, I find that investors exhibit the highest elasticity in each variable, contrary to the criticism that MPP might be pushing lower-income households out of those markets. These results are promising also in terms of MPP: if investors have the highest impact on housing prices during a housing boom [Kuttner and Shim \(2016\)](#), MPP will not stop them from entering the credit and housing markets, but it will affect their housing choices.

## 6. ADDITIONAL PERSPECTIVES ON THE EFFECTIVENESS OF LTV LIMITS

Section 5 shows that imposing a strict LTV limit on new contracts yielded counter-intuitive results in the credit market. Although a strict LTV limit 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. To the best of my knowledge, There are three plausible explanations for this outcome. First, 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 the risk pricing of the banks (this possible explanation was taken from [De Araujo et al. \(2016\)](#)). This paper had two other possible explanations: Second, the affected borrowers bought riskier assets, i.e., farther from the center, which may have increased the interest rate on those loans. Third, due to 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 go up.

### *6.1. Are Housing Assets Farther from the Center Riskier?*

One of the main findings so far are 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 center might increase the borrower's risk, in the sense that there is less demand to live in the periphery and asset prices there are more volatile. This section examines if 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 relationship between commuting costs, house prices, and housing consumption. The basic assumption in the model's framework is that all jobs are located in the center of the city (called the central business district or CBD for short). 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 et al. \(2012\)](#) show that housing prices are highest in Tel Aviv and decline as one moves farther away from it.

This section examines the monocentric city model in terms of the investor's risk, and whether the risk factor changes as we move farther from the center. This section uses data from the YAD2 website, which publishes information and advertisements about homes for sale and examine the price gap between the last asking price and the first asking price of each ad (between the years 2013 and 2016). Figure 12 shows that the price gap decreases as we get closer to 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 declines the closer we get 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 we get closer to the center of Israel.

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

## *6.2. Shifts in Demand for Consumer Credit*

Following the imposition of a strict LTV limit, borrowers who still want to buy a home but do not have enough 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 borrow 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 five percent cut in the LTV ratio equals tens of thousands dollars, which is a significant amount to raise in a short period of time). Second, they 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 making a decision 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. We can see that withdrawals were not higher after the limit. Third, they can still choose to buy the same property they wanted before the limit, but to take out other, unregulated credit, such as consumer credit. There are no loan-level data on consumer credit in Israel. In the absence of such data, this section uses macro-data and some indicators from existing loan-level data to examine whether there was a shift in demand for consumer credit due to the LTV limit.

First, this section estimates the decrease in the amount of mortgages due to the LTV limit. For each borrower type, the amount of mortgages that were above the cutoff before the policy intervention was calculated, and find that this amount is around NIS 10.3 billion<sup>26</sup> (36 percent belong to first-time home buyers, 33 percent for upgraders, and 31 percent for investors). However, some of those borrowers did not exit the market; instead, they lowered the amount of their loans. Using the method in Subsection 4.2, households that were supposed to be above the cutoff were matched with their actual LTV choices and examine the change in their loan amount. Overall, borrowers with an LTV 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 houses– the overall change in the value of properties due to the limit is NIS 3.2 billion, therefore the findings suggest that there is still a group of borrowers that continued to buy the same houses, with less amount of mortgages. At the same time, housing prices increased by 6 percent, the number of transactions in the real estate market did not change, and the population of borrowers also did not change after the policy intervention. The question is, how did borrowers raise this additional high amount of money in such a short period of time?

Figure 15 shows the shift in demand from mortgages to consumer credit after the imposition of the LTV limit. While there was an increase in demand for consumer credit after the LTV limit, the question is how much of this increase was due to the new regulation. To answer this question, I run two OLS regressions. First, the impact of the strict LTV limit on the rate of change in mortgages is examined (controlling for interest rate, housing transactions, real estate taxes, and

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<sup>26</sup>The assumption here is that the demand for mortgages stays the same after the policy intervention, and therefore calculation is an underestimate because housing prices increased in the years following the intervention.

rent prices). Second, the impact of the strict LTV limit on the rate of change in consumer credit is examined (controlling for vehicle imports, interest rate, private consumption, and taxes). I find that the LTV limit dummy had 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 does not reveal the amount of consumer credit each borrower had, I do have information about the amount of equity each borrower had. This equity can come from the borrower's savings, from his family, or from consumer credit he took out from other financial institutions. Figure 16 shows the change in the equity distribution for the affected borrowers, i.e., those who had an LTV 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 total increase in equity for the affected borrowers amounts to NIS 3.2 billion, which is almost half of the amount of mortgages that were withdrawn from the market. Again, it is hard to believe that the borrowers succeeded in raising such a huge amount of money only a few months after the limit was put in place. Therefore, the likelihood that this money came from their families is low.

To sum up, it is likely that the decline in mortgage loans was partly replaced by consumer loans.<sup>27</sup> Such loans are riskier than mortgages, both for the banks and for the households. They are not backed by any collateral: the average loan duration of consumer credit is around 4.5 years, compare to 22 years on average for mortgages and hence makes the debt payment relative to income ratio high, increasing the borrower probability of default. This leads to the question of whether the LTV limit actually lowers the borrower's risk or just shifts that risk to unregulated institutions/products, and by that increasing household leverage, monthly debt payments, and overall exposure to risk of recession and unemployment.

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<sup>27</sup>For example, in Slovakia banks offered “other housing loans” to enable borrowers to bypass the limits on housing loans, thereby undermining the intent of the policy.

## 7. CONCLUDING REMARKS

Since the 2008 global financial crisis, MPP has 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.

The main contribution of this paper is the estimation of the effect of an LTV limit on loan terms and especially on borrower behavior in the housing market. While LTV limits typically target the banks, they may cause borrowers to pay higher interest rates and to move to lower-graded housing, farther away from the center, and into neighborhoods with lower socioeconomic ratings. The paper 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 is an interesting question I hope to pursue in future research.

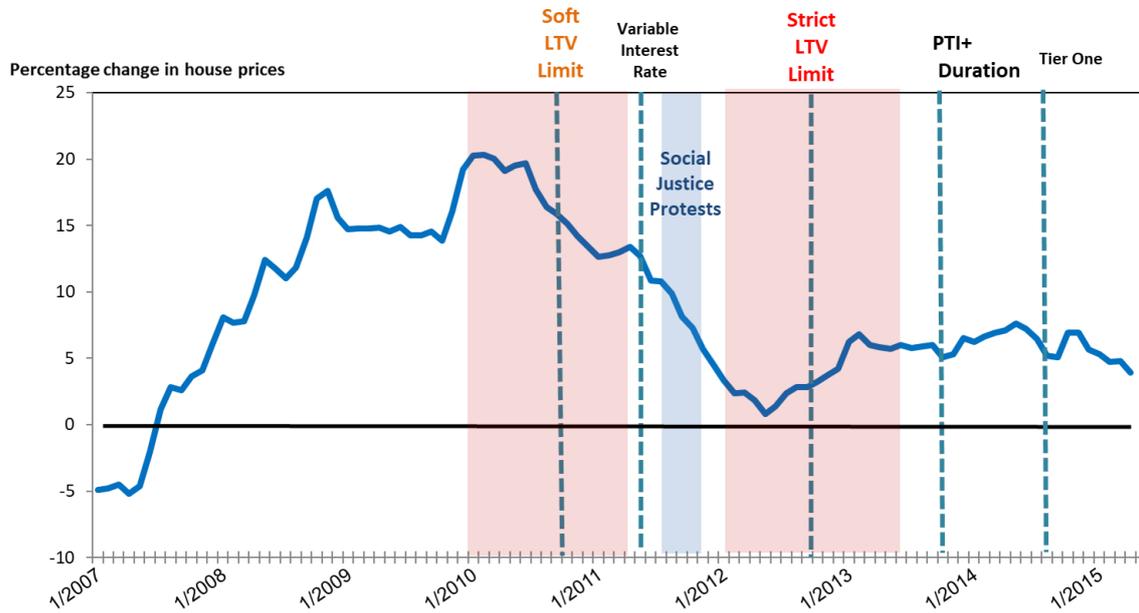
Another important finding of the present paper is that the two policies, soft and strict LTV limits, are in fact very similar and generate similar effects on household choices in the credit and housing markets. In magnitude, the strict LTV limit generate much larger results than the soft LTV limit, probably 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 non-mortgages 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 billions in total) and an increase in the use of non-mortgage consumer credit which 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 about the consumer credit market which may limit the generality of this paper's findings, the study suggests that future LTV measures should take into account non-bank sources of credit as part of the LTV limitations.

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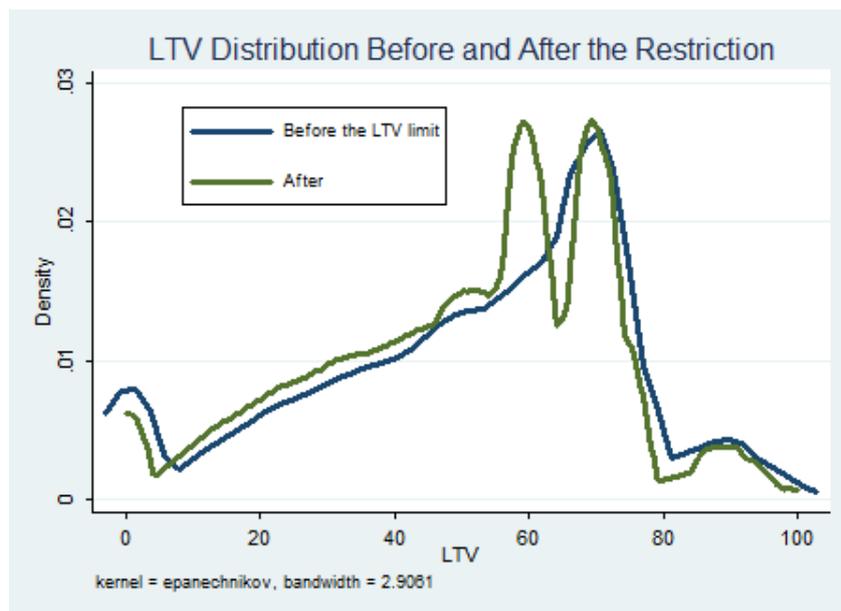
**Figure 1**  
*The Rate of Change in Housing Prices in Israel, 2007-2015*



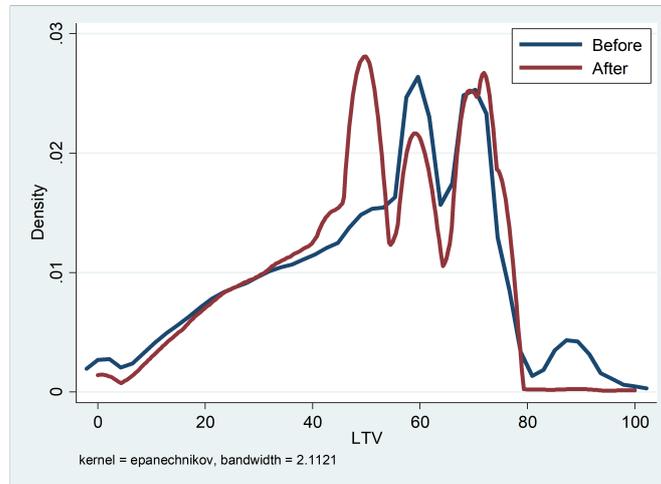
Source: Israel Central Bureau of Statistics.

<sup>1</sup> The line represents the monthly change in home prices (in annual terms). MPP tools are shown on the vertical lines.

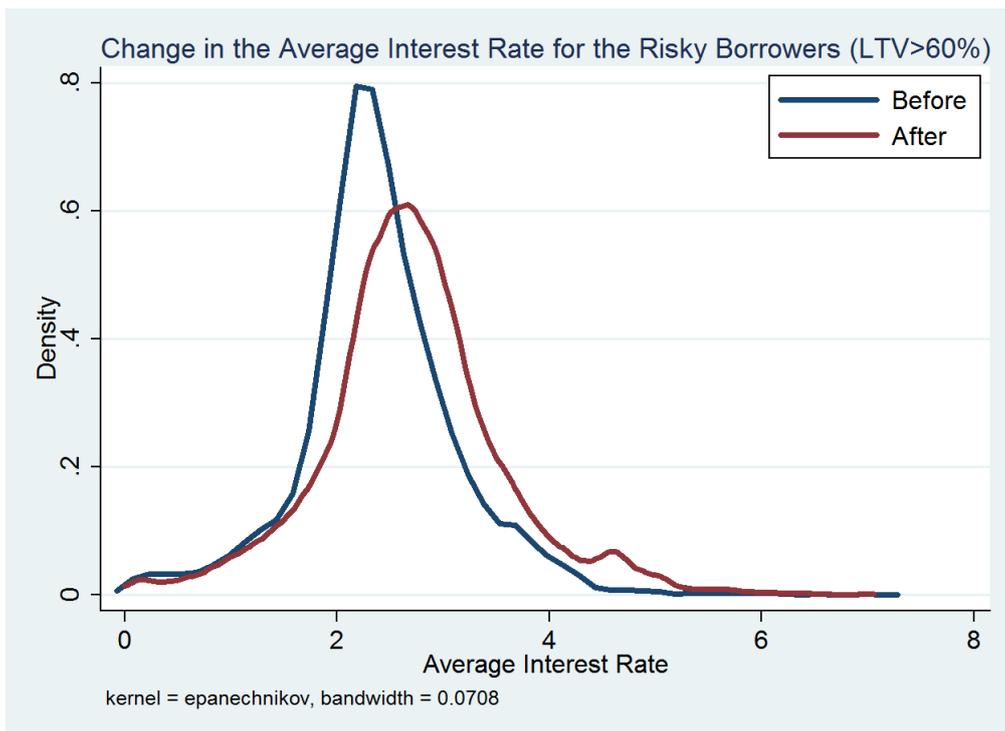
**Figure 2**  
*Soft LTV limit*



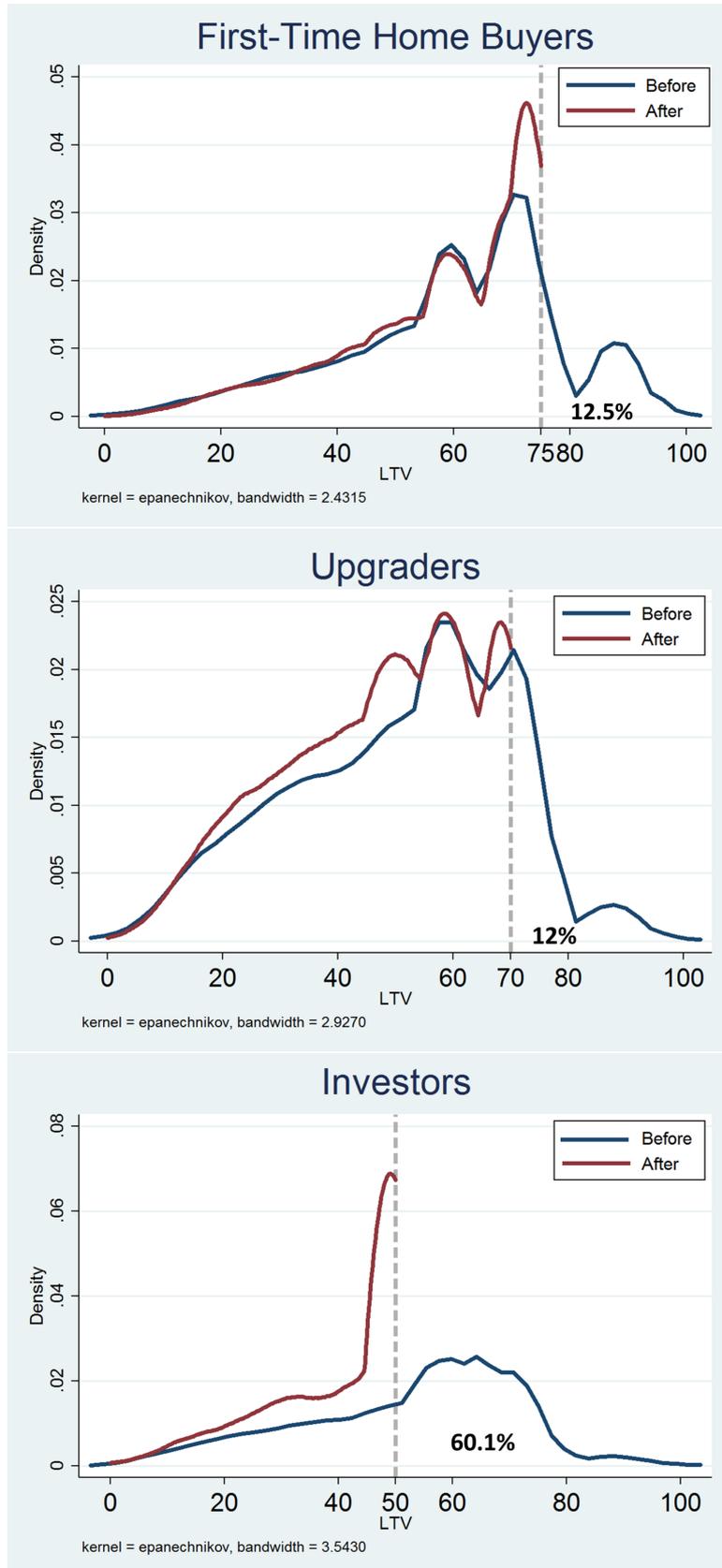
**Figure 3**  
*LTV Distribution before and after the Strict LTV Limit*



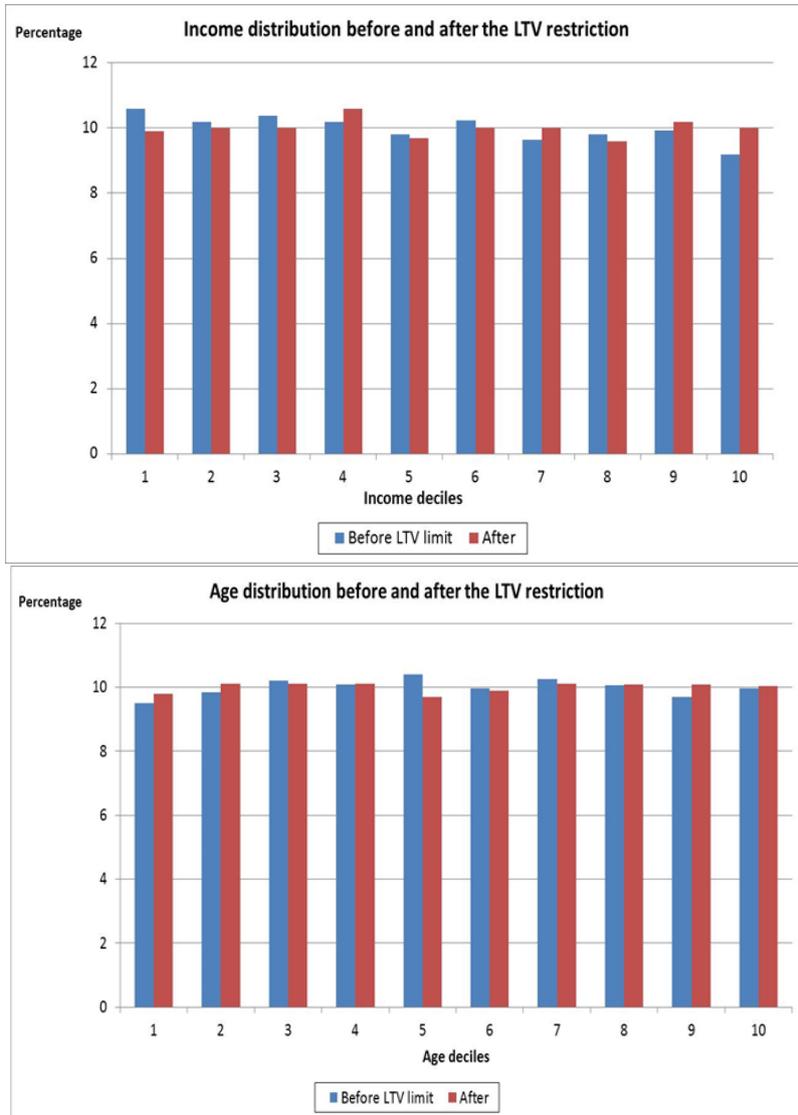
**Figure 4**



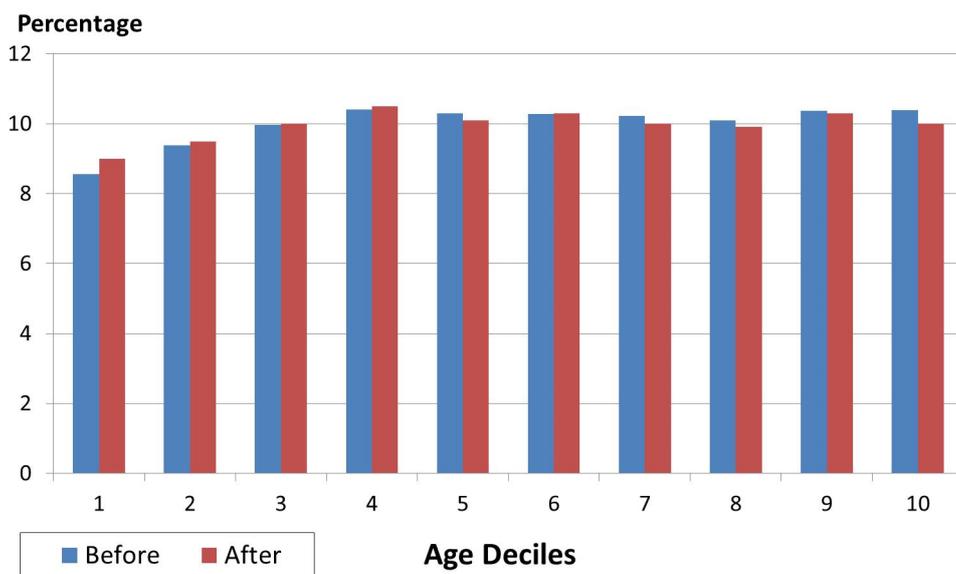
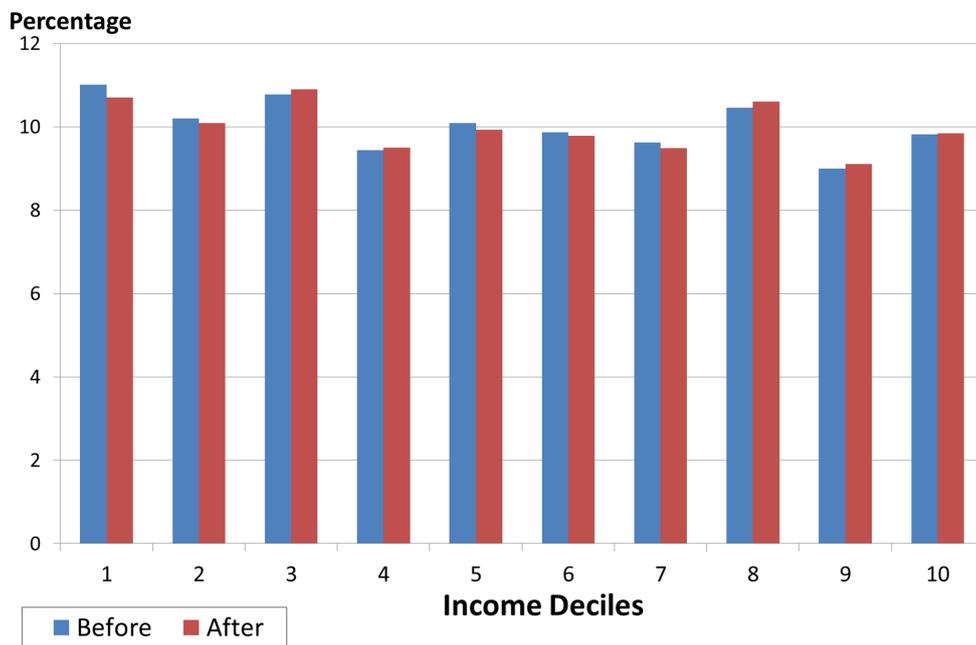
**Figure 5**  
*LTV Distribution by Buyer Type, before and after the LTV Limit*



**Figure 6**  
*Soft LTV limit- Change in Distribution of Borrower Characteristics*

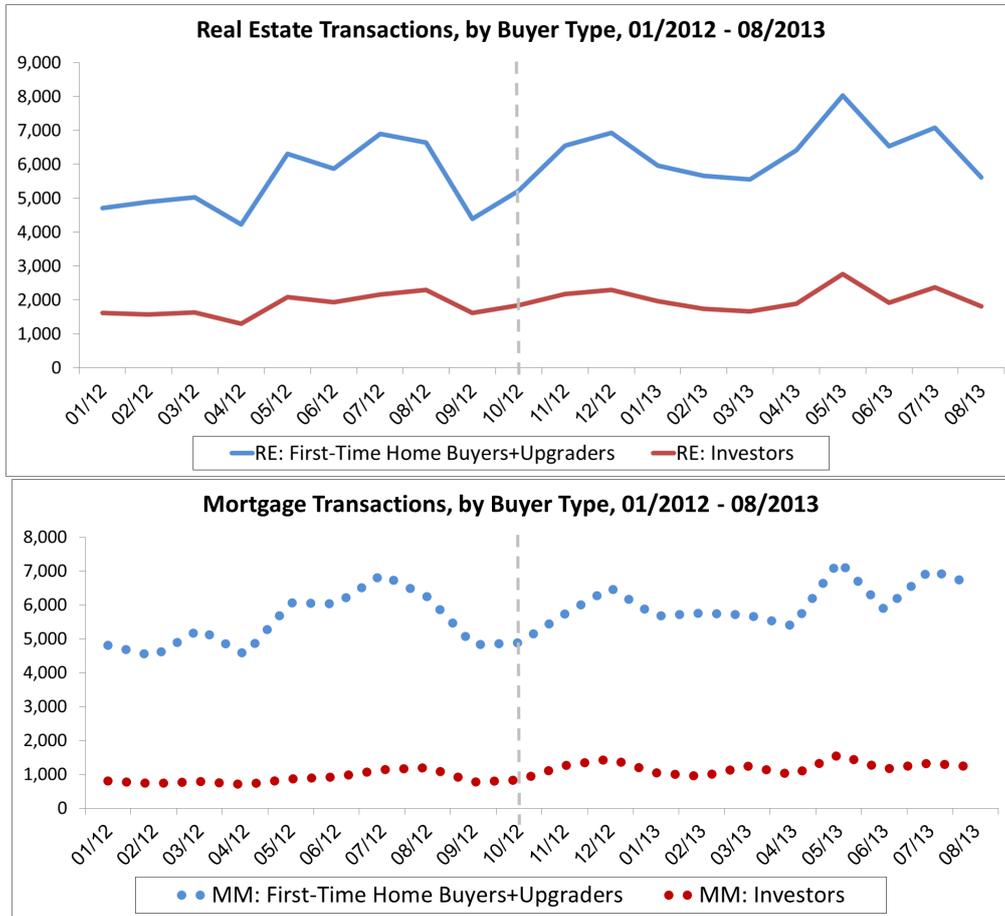


**Figure 7**  
*Strict LTV limit- Change in Distribution of Borrower Characteristics*



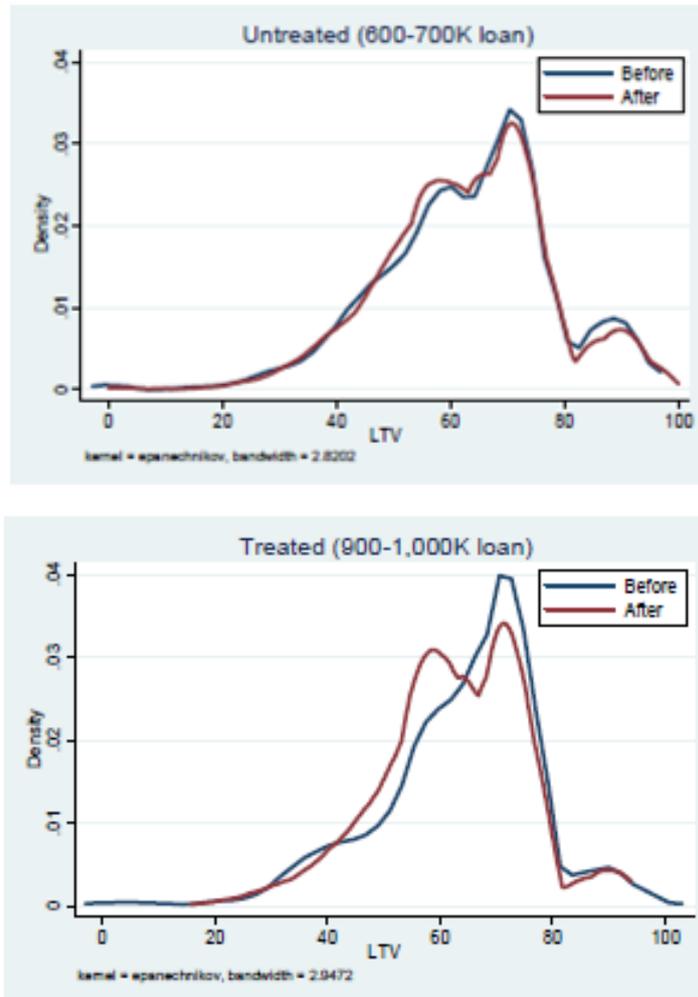
**Figure 8**

*Strict LTV limit- Activity in the Credit and Housing Markets, by Borrower Type*



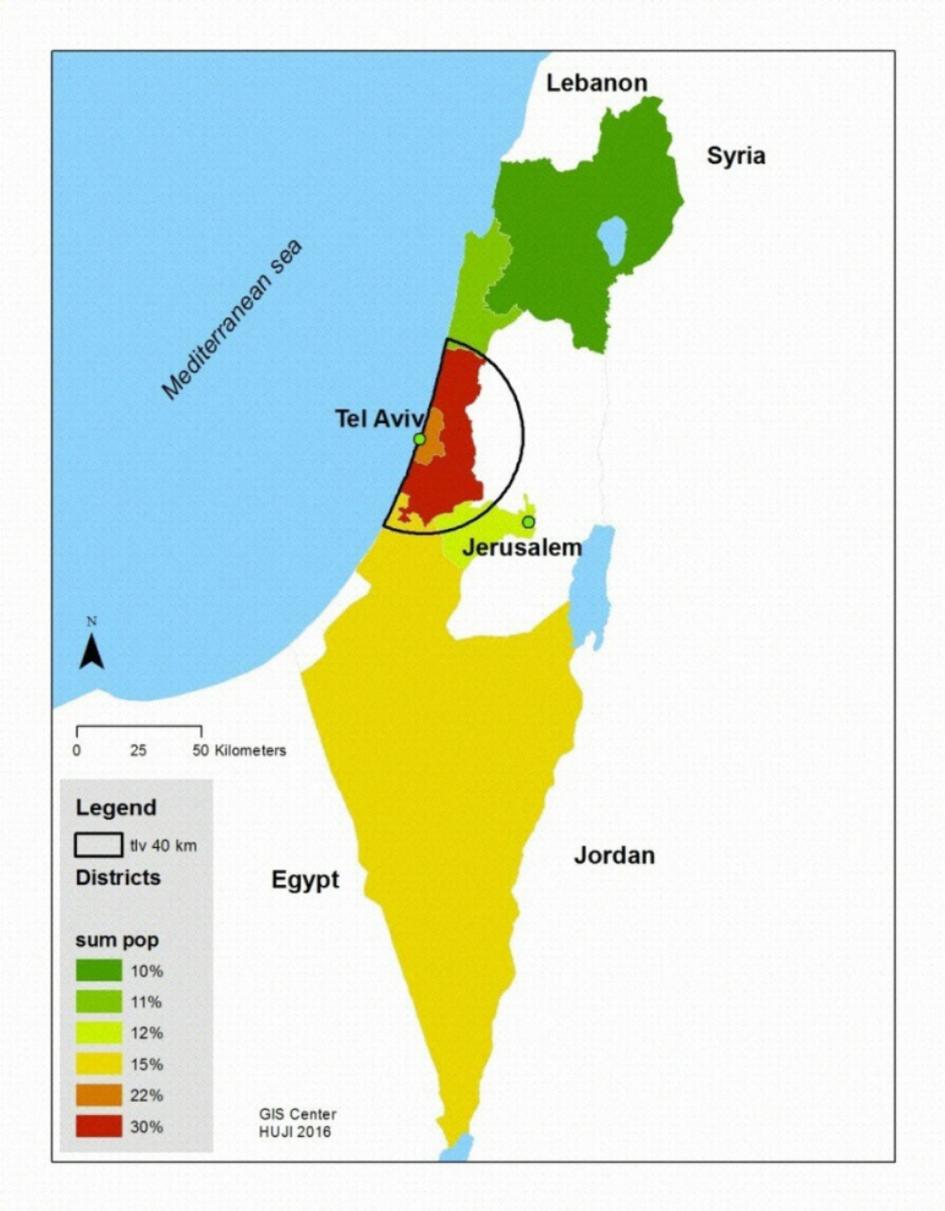
**Figure 9**

*LTV Distribution before and after Imposition of the LTV Limit: Treatment and Control Groups*



**Figure 10**  
*Israeli Population Distribution Map*

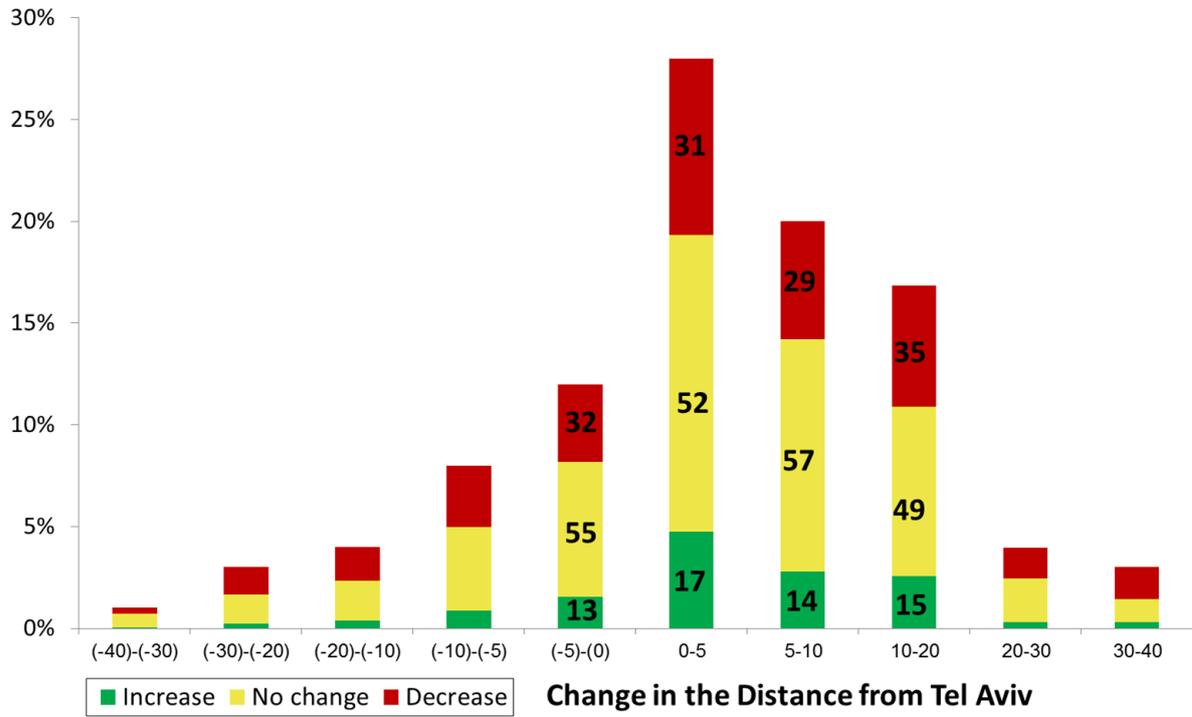
*a*



“Population relevant to the sample, from the Israeli Central Bureau of Statistics.

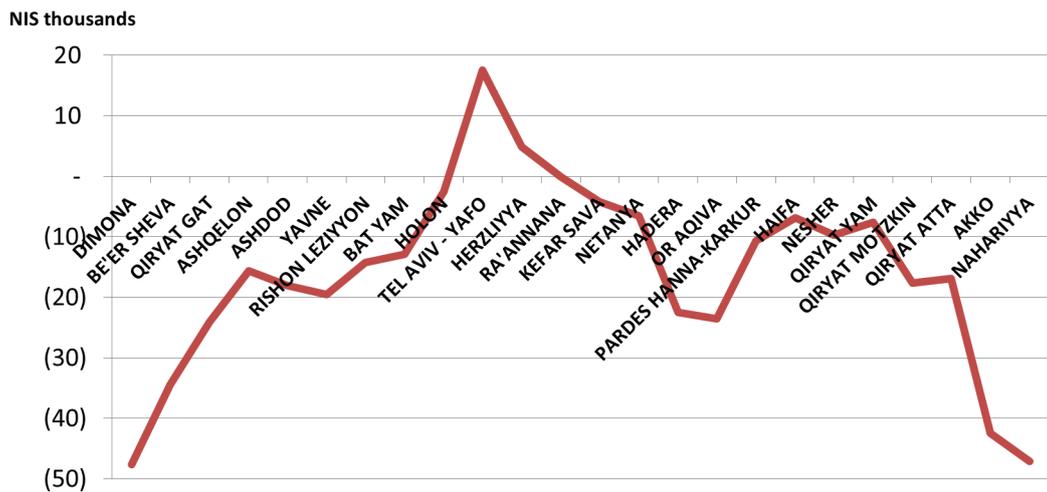
**Figure 11**

*Distribution of Change in Neighborhoods' Socioeconomic Level, by Distance from Tel Aviv*



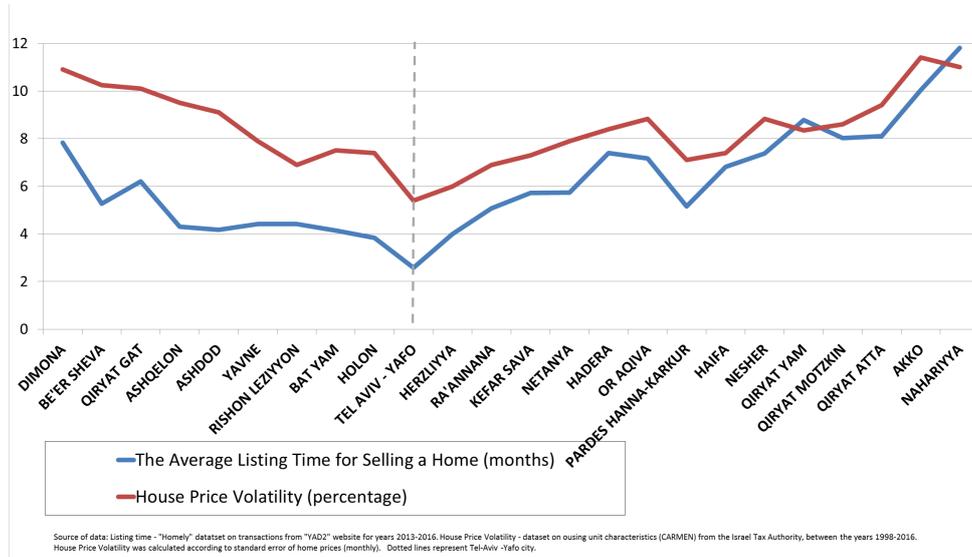
**Figure 12**

*Price Gap between the Last Asking Price and the First Asking Price along the Israel Coastline*

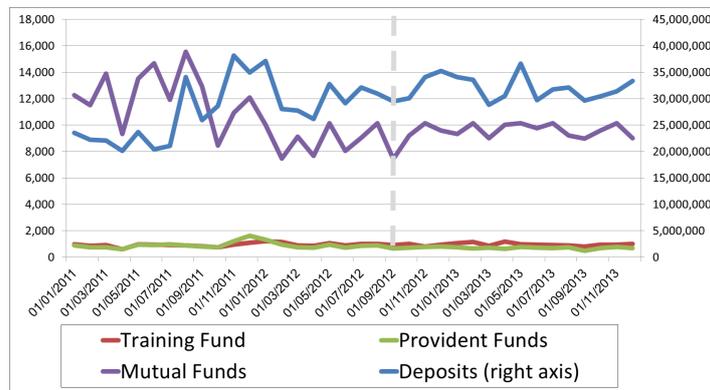


Source of data: "Homely" dataset on transaction from "YAD2" website, online listings of asking price on homes for sale, between the years 2013-2016. Dotted lines represents Tel-Aviv - Yafo city.

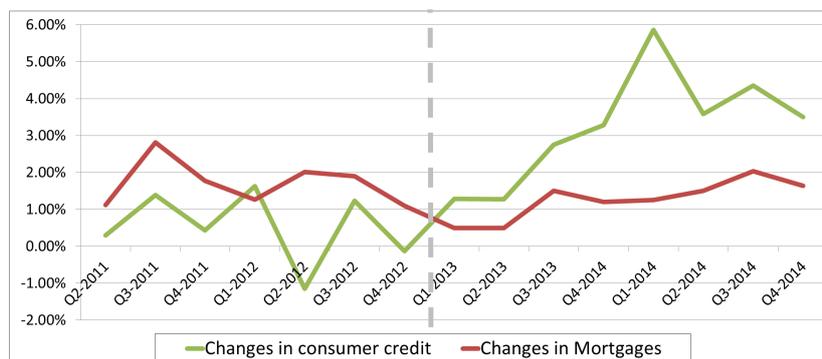
**Figure 13**  
*Risk Measure of Property Assets along the Israeli Coastline*



**Figure 14**  
*Withdrawals from Several Financial Resources, 2011–2013*



**Figure 15**  
Changes in Mortgages and Consumer Credit over Time



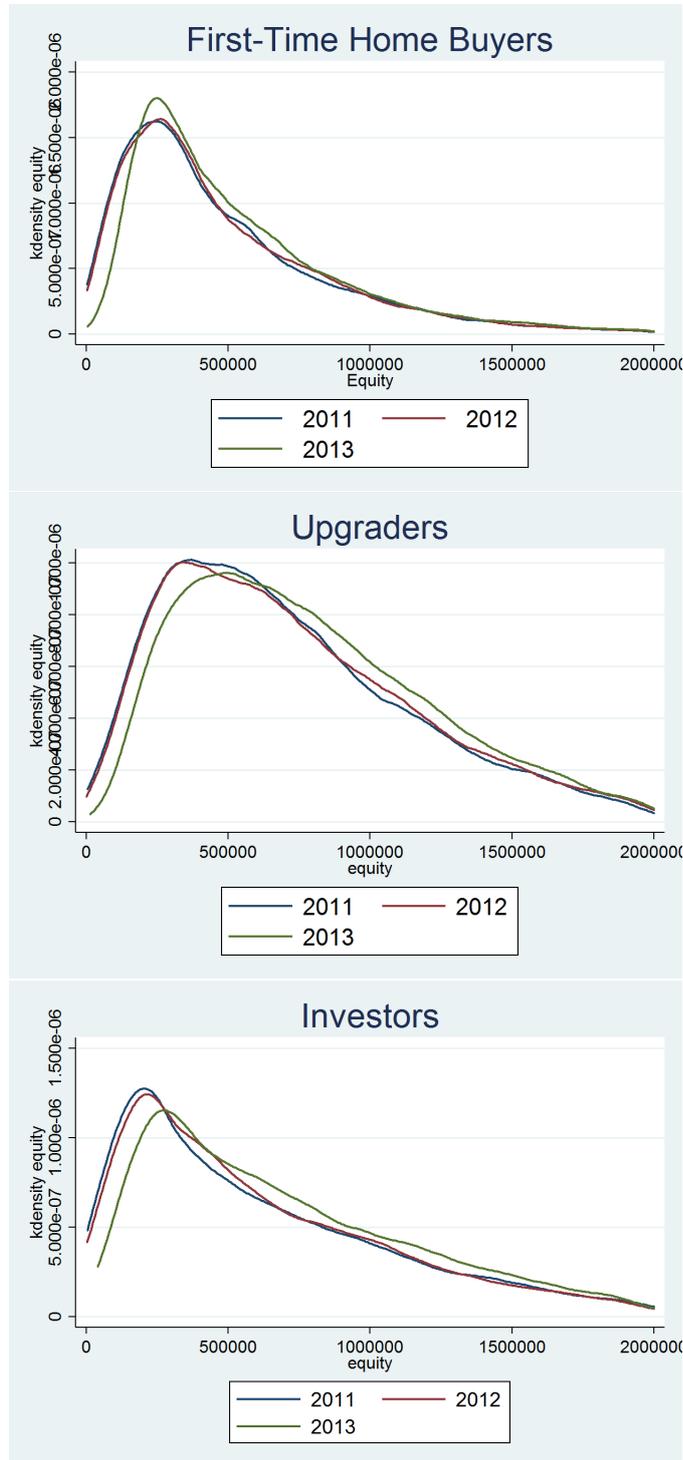
**Table 1**  
Summary Statistics - Soft LTV limit

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 (square meters)	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
	socio-economic level of Neighborhoods	11.9	3.61	10.4	3.5	-1.5***	0.0

\*\*\* p<0.01, \*\* p<0.05, \*p<0.1

Sources: Data on mortgages from the Bank of Israel, Data on purchase transactions from the Israel Tax Authority. Note: Real house prices was inflated by the overall (monthly) change in house prices. Socio-economic level of Neighborhoods - index of neighborhoods consisting of 16 different variables, including demographic, education, etc. Neighborhoods are classified into one of twenty clusters, 1 being the lowest socioeconomic status. The sample covers the period Jan 2010- May 2011.

**Figure 16**  
*Changes in Equity Distribution before and after the LTV Limit*



**Table 2**  
*Summary Statistics - Strict LTV limit*

First-Time Home Buyers	% Observation = 46				
	Mean	St. 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

**Table 3**  
*Sample Statistics before the Matching Process*

Average, per household	Before Matching			After Matching		
	Treated	Untreated	P-Value	Treated	Untreated	P-Value
Toal 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

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

**Table 4**  
*Changes in the Interest Rate for Matched Borrowers above and below the LTV Limit*

	61% VS 59%				61-65% VS 55-59%			
	Average Rate (1)	Average Rate (2)	Spread (3)	Spread (4)	Average Rate (5)	Average Rate (6)	Spread (7)	Spread (8)
ATT	.358*** (.078)	.251*** (.081)	0.213* (.110)	0.258** (.129)	.312*** (.065)	.297*** (.063)	0.251*** (.086)	0.259*** (.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 5**  
*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	<b>-67,789***</b> (36,135)	<b>0.41</b> (2.7)	<b>-0.03</b> (0.05)	<b>4.3***</b> (1.7)	<b>-2.2***</b> (0.8)
ATT (%)	<b>-4.7%</b>	<b>-1.1%</b>	<b>-0.9%</b>	<b>14.7%</b>	<b>-12.3%</b>

Note: \*\*\*, \*\*, \* indicate significance at the 1, 5, and 10 percent levels, respectively.  
Treated borrowers are defined as those that borrowed from 900,000 to 1,000,000 NIS.  
The untreated borrowers are those that borrowed 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.  
Tel-Aviv: the the business center of Israel.

**Table 6**  
*Difference-in-Differences Matching Estimation of The Effect of LTV Limit on Credit and Housing Choices*

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

This table reports the average treatment effect on the treated (ATT) for each variable and for each 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 difference-in-differences 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. \*\*\*, \*\*, \* indicate significance at the 1, 5, and 10 percent levels, respectively.

**Table 7***The Effect of LTV Limit on Credit and Housing Choices using DID Matching (Percentage Change)*

	<b>First-Time Home Buyer</b>	<b>Upgraders</b>	<b>Investors</b>
	<b>70-75 VS 75-80</b>	<b>65-70 VS 70-75</b>	<b>45-50 VS 50-55</b>
Real 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

## APPENDIX A - MPPs USED IN ISRAEL, IN CHRONOLOGICAL ORDER

MPPs	Date	Type of MPP
Soft LTV limit	Oct. 2010	Loans with an LTV ratio above 60 percent, 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 800,000 NIS
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	Nov. 2012	LTV will be limited as follows: 75% for a single housing unit; 50% for investors; 70% for improvers;
PTI + Duration	Aug. 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	Sep. 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

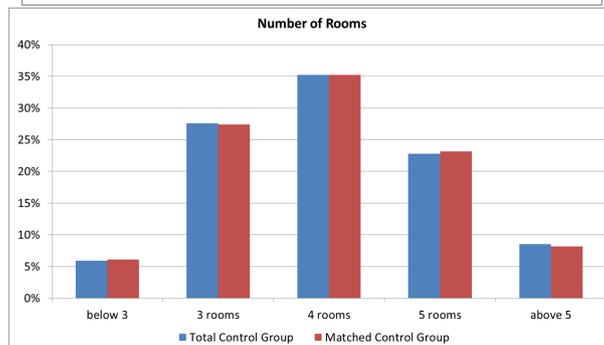
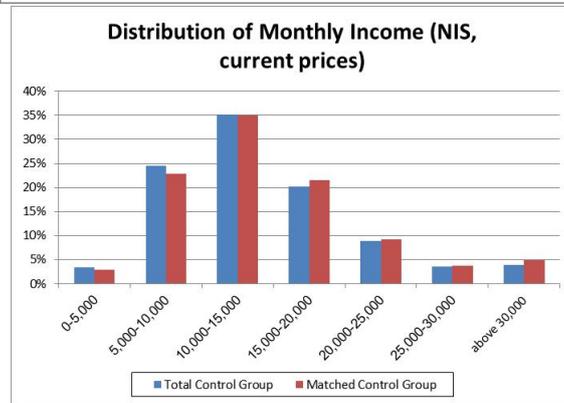
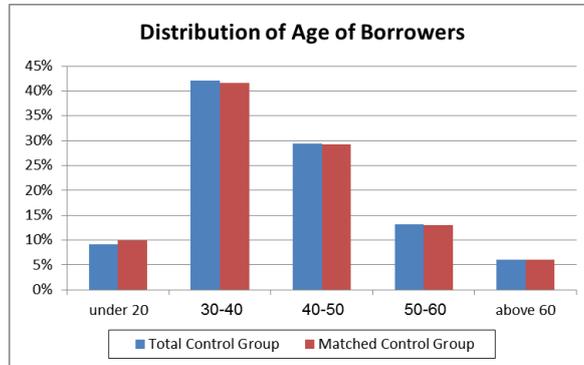
## APPENDIX B - MERGING THE MORTGAGES DATABASE TO THE REAL ESTATE DATABASE (CARMAN)

The mortgages file is merged with the CARMAN file through the following fields common to the two files: date of the transaction, transaction price, city of property, block and parcel numbers. As mentioned in the text, the recording of the block and parcel numbers in the mortgages file is distorted, with 36 percent of the records blank, and others showing only partial information. As a result, the block and parcel number field was 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 the purchased assets. Such a match is found in approximately 65,000 records (step 1). In cases where there was more than one match in the mortgages file, the block-parcel field was 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 was a blank date field in the mortgages file with one match to city and price, there were 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 were 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 were 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 were obtained (step 6). Finally, cases in which there was a city but not a price match were 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 were obtained (step 7). When a match is made by locality and date, with a gap of up to NIS one thousand, 400 paired observations were obtained (step 8). In cases where there is a price adjustment, and a match in the date field and in the block-parcel field, but there is no information on the city, 40 observations were obtained (step 9). In cases where there was no unique detection and the block-parcel field provides a unique identification, 23 paired observations were obtained (step 10).

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

# APPENDIX C - DISTRIBUTION OF THE KEY VARIABLES IN THE CONTROL GROUP VERSUS THE CONTROL GROUP MATCH TO THE TREATMENT GROUP

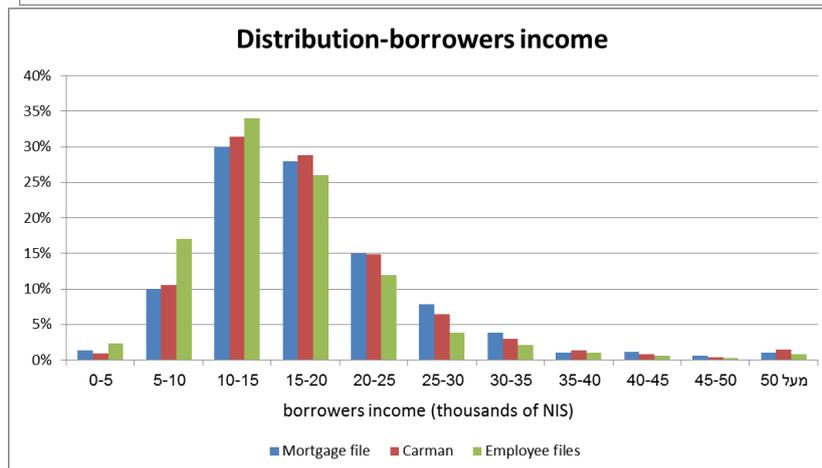
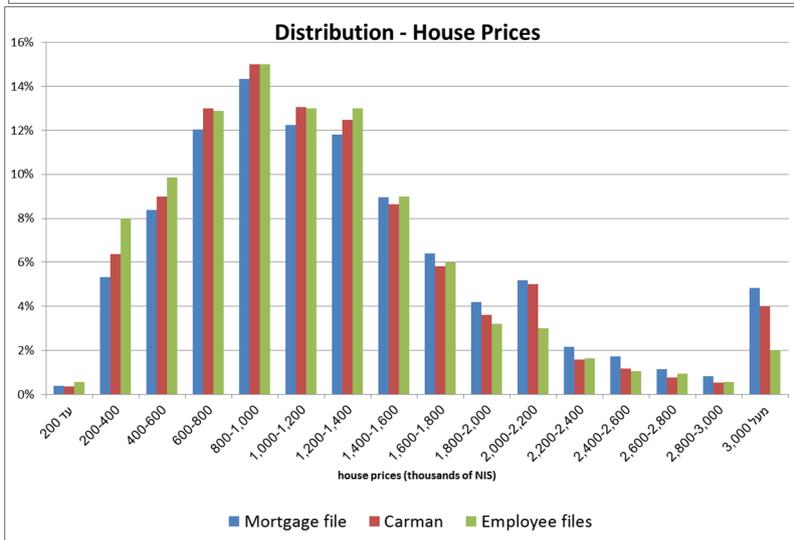
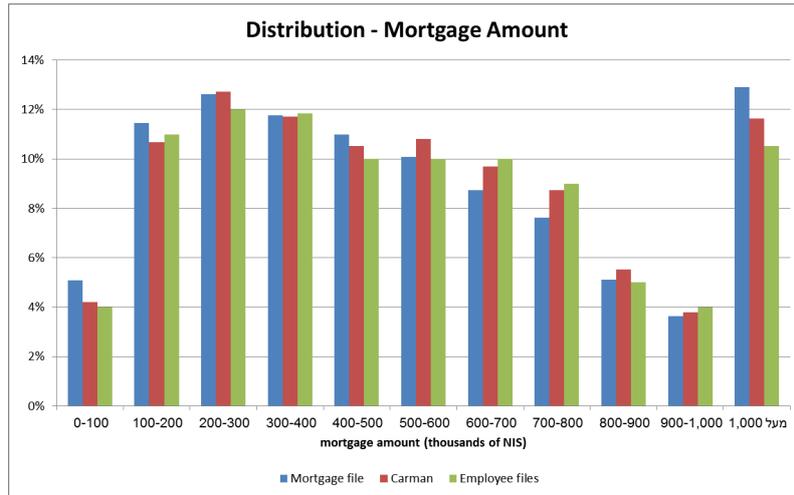


## APPENDIX D - DISTRIBUTION OF KEY VARIABLES IN THE MORTGAGES AND CARMAN FILES: SEPARATE VERSUS MERGED SAMPLE

After merging the mortgages file with the CARMAN file, which contains information on the characteristics of the property, the question of whether the observations of the merged file indeed reflect the observations in the CARMAN file must be considered. One of the advantages of combining the mortgages file with the CARMAN file is the potential for identifying 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 between the reasons for the purchase in the CARMAN file versus in the mortgages file 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 mortgages, which does not necessarily represent the entire population of home buyers.

<b>Cause of Purchase</b>	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 percent of the employees in Israel. A Kolmogorov–Smirnov test of equality in the distribution of mortgage amounts, house prices, and borrower income showed no significant differences among the three resources.



## APPENDIX E -SUMMARY STATISTICS - STRICT LTV LIMIT, LEVERAGED BORROWERS

**Table 8**

<b>First-Time Home Buyers</b>		% Observation = 12.4			
	Mean	St. 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 (square meters)	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 the Neighborhood	10.0	2.6	8.0	10.0	12.0

<b>Upgraders</b>		% 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 (square meters)	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

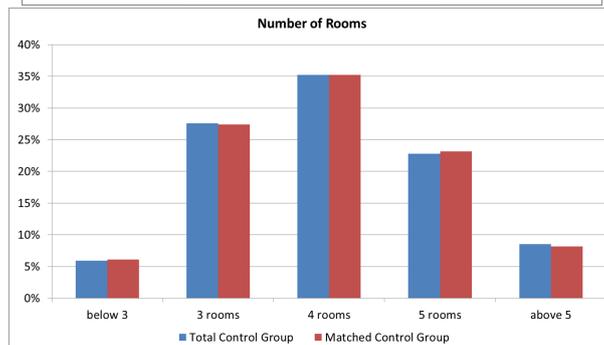
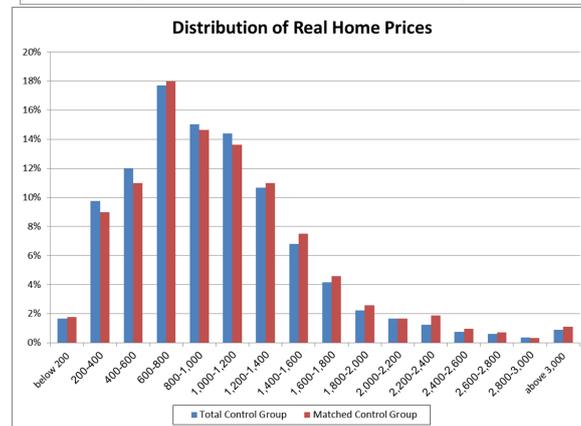
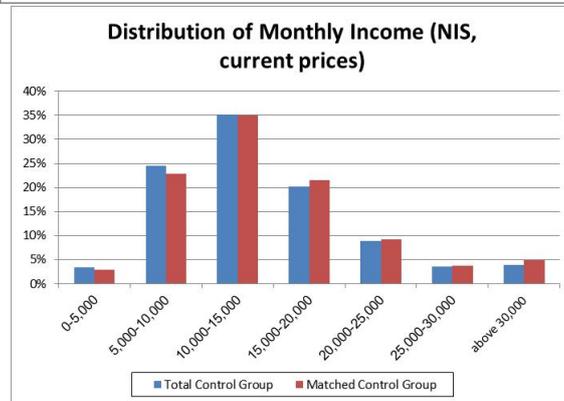
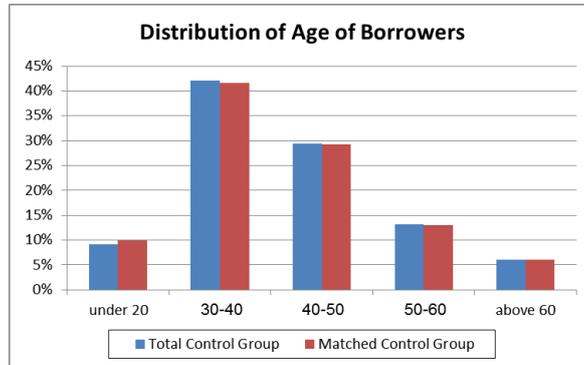
  

<b>Investors</b>		% Observation = 60.1			
	Mean	St. dev.	25%	50%	75%
House Price (NIS)	1,012,185	753,054	480,000	825,000	1,300,000
Borrower Monthly Income (NIS)	16,765	11,789	10,100	13,962	19,500
Borrower age	42.9	10.0	35.5	41.6	49.0
LTV (%)	65.0	8.8	59.7	64.0	70.0
Average Interest Rate (%)	2.98	0.82	2.49	2.89	3.40
Loan Duration (years)	20.1	7.2	15.0	20.0	25.0
Area (square meters)	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 the 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 affected borrowers within the group: First-Time Home Buyers - those with LTV above 75%, for Upgraders - those with LTV above 70%, Investors - those with LTV above 50%. The sample covers the period Jan 2012- Oct. 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 - DISTRIBUTION OF THE KEY VARIABLES IN THE CONTROL GROUP VERSUS THE CONTROL GROUP MATCH TO THE TREATMENT GROUP



## APPENDIX G - EFFECT OF LTV LIMIT ON HOUSING MARKET, BY BUYERS TYPE

the sample was divided into two groups according to the average age of borrowers: young<sup>28</sup>—up to the age of 40—versus older adults. The matching process was 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, by about 5 km (11 percent), as opposed to a move of 3 km (7 percent) by younger purchasers. These results may be attributable to the possibility that older adults are more flexible in their purchasing decisions and can either delay purchasing decisions 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 close to their parents or their work.

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 was carried out again for each of the three 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 away 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.<sup>29</sup>

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<sup>28</sup>The median age of mortgage borrowers is 41.5. The percentage of young borrowers was 49 before the imposition of the LTV limit percent and 51 percent after.

<sup>29</sup>Igan and Kang (2011) obtain similar results, namely, that older households and investors are more influenced by policy interventions.

**Table 9**  
*Effect of LTV Limit on Housing Market: Matching Procedure by Age Groups*

		Real Home Prices (NIS)	Size (sq.m.)	Rooms	Distance from Tel Aviv (KM)	Socioeconomic level of the Neighborhood
Age<=40	ATT	-55,229*** [9,314]	0.73 [1.82]	-0.01 [0.02]	3.0*** [0.98]	-1.3*** [0.41]
	ATT (%)	-5.8%***	1%	0%	7%***	13%***
Age>40	ATT	-103,060*** [13,588]	-2.8** [1.12]	-0.06*** [0.02]	4.9*** [1.1]	-2.0*** [0.57]
	ATT (%)	-9%***	-3%**	-1%***	11%***	18%***

**Table 10**  
*Effect of LTV Limit on Housing Market: Matching Procedure by Buyer Type*

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

## APPENDIX H - EFFECT OF LTV LIMIT ON HOUSING MARKET, BY BUYERS TYPE

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

$$\eta = \frac{\Delta Y}{\Delta LTV} \times \frac{\overline{LTV}}{\overline{Y}} \quad (6)$$

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

**Table 11**  
*Elasticity of Demand in the Credit and Housing Markets*

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

This table shows the elasticity of demand in the credit and housing markets for each borrower type. The elasticity of demand was 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.