Bank of Israel **Research Department**

LTV Limits and Borrower Risk

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Discussion Paper 2018.12
December 2018

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I would like to thank Yishay Yafeh, Nathan Sussman, Efraim Benmelech, David Genesove, Natalya Presman and Robert Kelly for their helpful suggestions. I would also like to thank the participants at the Bank of Israel Research Department Seminar, the participants at the Conference on Systemic Risk Analytics, the Joint European Central Bank and Central Bank of Ireland research workshop for their helpful comments and discussions. The views expressed herein are those of the author and do not necessarily reflect the views of the Bank of Israel and the Hebrew University.

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המגבלה על שיעור המימון וסיכון הלווים

ניצן צור אילן

תקציר

המאמר מתחקה אחר השפעותיה של מגבלת ה-LTV (Loan To Value – יחס ההלוואה לערך הדירה) שהוטלה בישראל בשנת 2012. למגבלה נקבעו שלוש מדרגות לפי סוג הלווה – רוכש דירה לראשונה, משפר דיור ומשקיע. המאמר מתמודד עם האתגר הנובע מכך שאין יודעים מהו שיעור המימון שהיה הלווה נוטל אלמלא המגבלה. נמצא שצעד מקרו-יציבותי זה השיג את אחת ממטרותיו המרכזיות – הפחתת מינוף הלווים. בשוק הדיור נמצא כי הלווים רכשו נכסים רחוקים יותר ממרכז הארץ ובשכונות שדירוגן הסוציו-אקונומי נמוך יותר ; המשקיעים הושפעו יותר משאר סוגי הלווים ; והתגובה למגבלה האמורה הייתה חזקה מהתגובה למגבלת ה-LTV שהוטלה בשנת 2010. בשוק האשראי הובילה המגבלה להעלאת הריבית ולהארכת התקופה לפירעון, והסברים אפשריים לכך נידונים במאמר.

LTV Limits and Borrower Risk

Nitzan Tzur-Ilan

(Preliminary Draft, Please Do Not Quote)

Abstract

This paper explores the effects of the hard loan-to-value (LTV) limit implemented in Israel in 2012, which had three different cutoffs according to the borrower type: first-time home buyer, upgrader, or investor. The paper tries to overcome identification challenges where the treatment status is not observed. I find that this macroprudential policy measure succeeded in achieving its main goal, which was to reduce borrowers' leverage. I also find that constrained borrowers bought assets farther from the center of Israel, in neighborhoods with lower socioeconomic rankings; and a much stronger response than the impact of the 2010 soft LTV limit. Investors were found to be the borrower type most affected by the LTV limit. In terms of the credit market, the effect of the LTV limit on mortgage terms is counterintuitive: the limit increased the interest rate and the term to maturity. Plausible explanations for those results are discussed.

Keywords: LTV, macroprudential policy, mortgages, housing market, systemic risk

JEL classification: E58, E61, G18, G21, R28

1. Introduction

Macroprudential policies (MPPs) are designed, among other reasons, to mitigate borrower risk associated with a housing boom. Given the major role of the housing market in credit cycles, as well as the link between housing finance and the global financial crisis (Duca et al., 2010, 2011), MPPs have recently been given greater emphasis by regulatory authorities. These policies aim to create a buffer in the event of a housing boom and to build up bank and borrower resilience to financial shocks in order to ensure that shocks from the housing sector do not threaten financial stability.[1]

MPPs targeting the mortgage market, particularly by imposing loan-to-value (LTV) limits on housing loans, have become extremely common (Claessens, 2015, IMF, 2013). LTV limits are 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 et al. (2015) for theoretical arguments and Demyanyk and Hemert (2011) for empirical evidence. However, LTV limits have important implications at the borrower level that are not well explored in the literature, including their impact on the mortgage and housing choices of constrained borrowers.

The key difficulty in measuring the effect of credit constraints on the price of housing is to establish the direction of causality between credit conditions and housing prices. Easier and cheaper access to credit can reduce financing constraints and thereby increase the demand for housing. On the other hand, credit conditions can be a response measure to expectations of a stronger housing demand (Favara and Imbs, 2015).

While a large body of literature exists on the link between credit flows and housing prices,[2] only a few recent papers measure the impact of specific MPP measures on the housing market, mainly by using cross-country or single-country macroeconomic data (Kuttner and Shim, 2016; Vandenbussche et al., 2015; Cerutti et al., 2015). These papers explore a tentative link between macroprudential measures and housing prices, but offer more conclusive findings on the impact of these measures on credit growth. However, a limitation of most of these papers is that they use data at the aggregate level and rely on the use of average indicators in their identification strategy in cross-country or single-country models. In particular, they

[1] "Managing House Price Booms: The Role of Macroprudential Policies," IMF Conference on Housing Markets, December 2014.
[2] Related literature includes Duca et al. (2011), Kelly et al. (2018), and Han et al. (2017).

miss the complexity of the impact of LTV limits on borrower behavior in the credit and housing markets, which can be tested only with adequate intra-country borrower-specific variation. A notable exception is Igan and Kang (2011) who use survey data to show that the introduction of LTV and PTI (payment-to-income ratio) limits likely lowered home price expectations and delayed home purchases in Korea. De Araujo et al. (2016) paper is another exception, which explores the effects of a LTV limit implemented in Brazil, using credit registry data. They find that borrowers constrained by regulation borrowed at higher interest rates, shortened maturities, reduced loan amounts, and lower LTV ratios than unregulated borrowers, and that these borrowers also purchased more affordable homes and were less likely to be in arrears 12 months after taking the loan. However, their paper differs from this one in terms of the nature of the regulation, the focus on the side effects of the LTV limit on borrower risk, the identification strategy and the outcome variables.

Israel has a great deal of experience in MPP, and in this paper I exploit a regulatory experiment arising from a 2012 policy change that limits the LTV ratio according to the type of buyer. Under this new policy, a banking corporation cannot approve a housing loan with an LTV of more than 75% for borrowers 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"). Before the 2012 limit, there were no strict limits on the LTV ratio. There was only the 2010 MPP that required higher capital requirement for loans with an LTV ratio above 60% - which will be called "soft" LTV limit. The reason for the term "soft" LTV limit is, as shown in Tzur-Ilan (2017), that borrowers could have borrowed above the 60% limit and pay a higher interest rate on their loans. In contrast to the "soft" LTV limit of 2010 that it replaced, the "hard" LTV limit of 2012 does not allow borrowing above the specified limit for each borrower type. In this paper I examine the effect of the hard LTV limit on households' choices in the credit and housing markets. In particular, I examine whether the hard LTV limit has had any side effects on borrower risk during a housing boom in Israel.

In this paper I address four main issues, which are different from those dealt with in Tzur-Ilan (2017). First, I examine a hard LTV limit, which is more popular MPP tool than the soft LTV limit (Crowe et al., 2013). Second, I try to overcome the identification challenge of the 2012 LTV limit where the treatment status is not observed: due to the "hard" LTV limit, it is only possible to observe borrowers below

the LTV cutoff; the counterfactual leverage choices without the limit are not known. Third, I examine the elasticity of the demand for credit and housing for different borrower types, using the 2012 limit structure that assigns different cutoffs to first-time home buyers, upgraders, and investors (in contrast to the 2010 limit structure, which did not distinguish between different borrower types). This is of interest for two main reasons. First, LTV limits are criticized for making it difficult for households in need of credit to purchase property and hence for discriminating against weaker segments of the population. To test this, I examine whether the borrowers most affected by the policy were the first-time home buyers. Second, in the literature it has been argued that real-estate investors are the group with the largest effect on housing prices during a housing boom (Kuttner and Shim, 2016). Indeed, a key motivation for tightening LTV ratios in many countries is to stop real-estate investor activities in the housing market (Han et al., 2017). I therefore examine the elasticity of the response to different LTV constraints by different categories of borrowers.[3] The fourth issue I examine is whether the hard LTV limit induced borrowers to take out unsecured credit instead of mortgages, thereby increasing the borrower's risk of default. Whereas the soft LTV limit can be compared to a tax, as the borrower must pay a higher interest rate above a certain threshold, the hard LTV limit can be seen as a more rigid constraint which can only be circumvented if borrowers resort to alternative financing sources such as unsecured credit. Table 1 summarizes the differences between the regulatory environment and empirical analysis of the 2010 soft LTV limit discussed in Tzur-Ilan (2017) and the 2012 hard LTV limit discussed here:

[3] To the best of my knowledge, Igan and Kand (2011) is the only paper that examines the effect of a hard LTV limit on investors' behavior.

Table 1 - Differences between the Soft LTV Limit and the Hard LTV Limit:

	Soft LTV limit	**Hard LTV limit**
Time of implementation	October 2010	October 2012
The new policy	A banking corporation is required to increase capital provisions for housing loans (depending on the loan characteristics) to 100 percent, with an LTV limit on new loans of more than 60 percent.	A banking corporation cannot approve a housing loan with an LTV of more than75% for borrowers acquiring a first home (henceforth "first-time home buyers").
Exceptions	The guidelines do not apply to housing loans originally amounting to less than NIS 800,000.	70% for borrowers needing to sell their existing home within 18 months ("upgraders"), and 50% for borrowers buying a home for investment purposes ("investors").
Intended consequences	Since the LTV limit would force the banks to tie up more capital against these loans, borrowers wanting to take a loan with an LTV of more than 60 percent face higher interest rates.	Does not allow borrowing above the specified limit for any borrower type.
Frequency of use around the world	Not very common, mainly been used in developing economies.	The most popular MPP tool (Crowe et al., 2013).
Empirical identification challenge	Borrower choice is observable; a borrower could still borrow the same amount of money but paying higher interest rates. The NIS 800K exception can be used to divide borrowers into treatment and control groups.	The treatment status is not observed after the LTV limit is imposed (counterfactual choice of leverage unknown).
Effect on different borrower types	No distinction between different borrower types.	Different cutoffs for first-time home buyers, upgraders, and investors
Leakages	Borrowers can borrow from banks at a higher cost so the probability of leakages is low.	The hard limit might cause constrained borrowers to take out unsecured credit instead of mortgages.

This study uses the same loan-level data (merged with the housing characteristics dataset) as been use in Tzur-Ilan (2017), but for different time period[4]. The merged dataset contains information on borrower characteristics, housing units purchased, and mortgages taken out by 34,021 households in the 21 months framing the October 2012 policy change (i.e., between January 2012 and September 2013, when another MPP tool was implemented).

[4] Tzur-Ilan (2017) was centered on the years 2010–2011.

The main identification challenge in this paper is that the treatment status can be observed only before the policy change, whereas after the policy change the constrained borrowers can no longer be distinguished by means of their LTV ratios. To overcome this challenge, I adopt two methods for predicting the leverage choices of borrowers. Both methods use the borrower's age and income to predict the LTV ratio that the borrower would have chosen in the absence of an LTV limit (age and income were the variables with the highest predicted power). Although other variables such as number of family members and previous place of residence are indicators of residential preferences and socioeconomic status and hence might explain LTV choices, they do not increase the predicted power when added to the regression, and hence I chose to restrict the variables to age and income.

The first method predicts the LTV ratio using a linear regression (an OLS approach), where the LTV ratio is, as mentioned, a function of age and income. The fitted value from this regression is used to predict the post-regulation LTV distribution. Treated borrowers are those borrowers whose predicted LTV is above the limit. This method is based on the assumption that the LTV limit did not affect borrower income and age distribution, nor cause borrowers to leave the mortgage market (see Section 5.1). The second method for predicting the leverage choices of treated borrowers is a matching method that allows the prediction model for the LTV ratio to be nonlinear and assumes nonparametric prediction. To identify the treated borrowers, households after the LTV limit are matched to the closest households before the policy change based on observed characteristics (age and income), and the difference between their choices in the credit and housing markets is examined. Both methods use the difference-in-differences method to control for unobservable time-varying macroeconomic effects, by setting a control group that is below the cutoff both before and after the implementation of the hard LTV limit (i.e., the control group is unaffected by the LTV limit).

The results show that the hard LTV limit did not cause borrowers to leave the credit and housing markets, and the same borrowers (in terms of age and income) were in the market before and after the policy change. Tzur-Ilan (2017) yields qualitatively similar results for the "soft" LTV limit. However, these results for the hard LTV limit are unexpected because the hard LTV limit left borrowers—especially investors—with much less credit to buy a property. It can be assumed, at least for

investors, that one objective of this policy measure was to deter some real estate investors from the credit and real estate markets, but apparently it didn't succeed.

In terms of the housing characteristics, affected borrowers bought cheaper assets, smaller, farther from the business center, and in lower-graded neighborhoods. Compared to the results in Tzur-Ilan (2017), the 2012 hard LTV limit had more positive results in terms of MPP goals. It had a much greater impact on borrowers' choices in the credit and housing markets than the 2010 soft LTV limit, probably because the hard limit did not give them a choice to stay highly leveraged. This is especially true for investors, who moved farther from the center (24%), to lower-quality neighborhoods (18%), and into smaller houses (14%), compared to 15%, 9%, and -2%, respectively, in response to the 2010 soft LTV limit (as shown in Tzur-Ilan (2017)). 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. Also, as a result of the 2012 limit borrowers mostly changed location (away from the center), whereas in response to the 2010 limit they mostly changed the quality of their neighborhood. Also, in the "hard" LTV limit case, the borrowers changed the size of their dwelling, whereas in response to the "soft" LTV limit there was not any change in the size of the dwelling

However, imposing a maximum LTV limit on new contracts yielded counterintuitive results in the credit market. Although a maximum 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 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.

Regarding the last explanation, MPPs typically target large financial intermediaries (e.g., banks) by enforcing the LTV ratio (Claessens, 2015). This focus on financial intermediaries, however, 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. To the best of my knowledge, Braggion, Manconi, and Zhu (2017) 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.[5,6] Although I do not have loan-level data on consumer credit, I use macro-data and other indicators from loan-level data on mortgages to examine whether the shift in the demand for consumer credit was partly due to the LTV limit. Using aggregate data, I find that after the imposition of the LTV limit, there was a shift in consumer credit relative to mortgages (which cannot be explained by other macroeconomic events). Also, using loan-level data, an increase was found, after the change in policy, in the amount of equity that the borrowers reported to the bank when they came to take out a mortgage. I examine whether this equity came from financial resources, but as no changes were seen in financial resources around the time of the imposition of the limit, I suspect that the money came from consumer credit. As mentioned, 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.

The paper is organized as follows. Section 2 discusses 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 whether the hard LTV limit affects borrower risk and Section 7 concludes.

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

[6] Cizel et al. (2016) 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.

2. Background

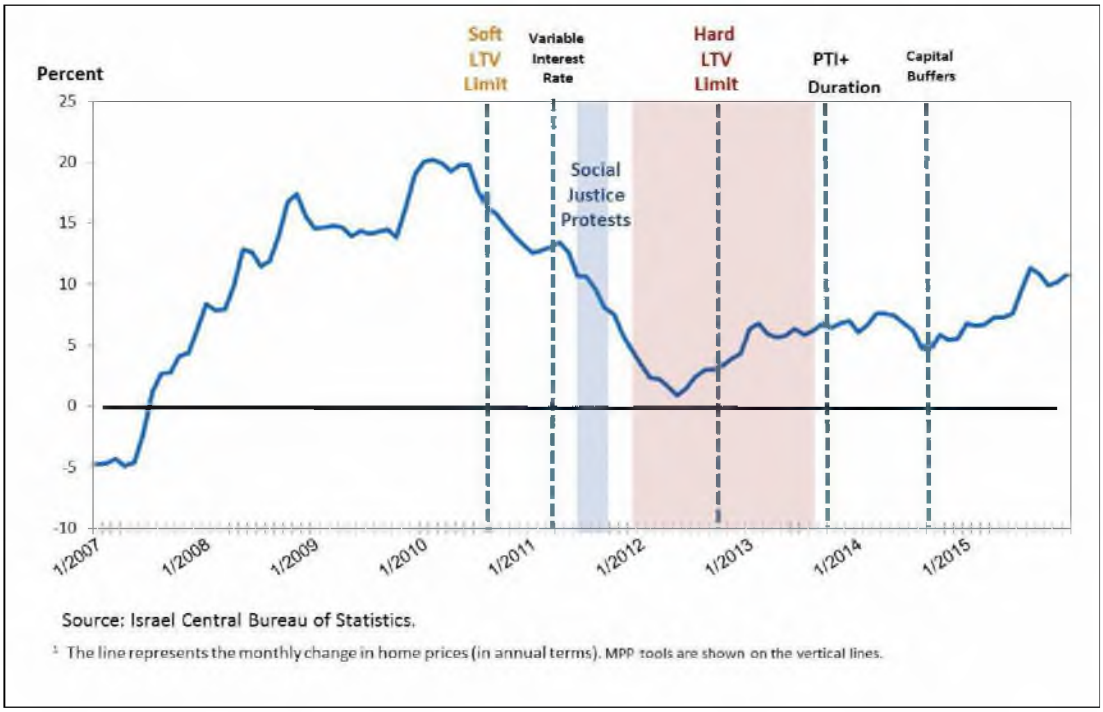
2.1. The Housing Market and Housing Finance in Israel

The financial crisis of 2008 caused interest rates to fall in most advanced economies. This was true in Israel as well, where declines in interest rates led, among other things, to a shift of capital to housing investments. The increase in investor demand for houses, combined with bureaucratically constrained rates of construction, drove housing prices upward.[7] As a result of this process, Israel experienced a rapid and continuous rise in housing prices. Real housing prices in Israel increased by approximately 70 percent from 2008 to the beginning of 2016. 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 rest.[8][9]

At the same time, the volume of housing loans increased by an average of approximately 6 percent each year, raising concerns among policymakers. As a result, between 2010 and 2014, the Banking Supervision Department of the Bank of Israel adopted a number of macroprudential policies intended to prevent households from overleveraging, maintain financial stability, and 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 try to rein in the pace of home price increases. Figure 1 shows the rate of change in housing prices in Israel; the vertical lines indicate the dates the various macroprudential policies were implemented.[10]

[7] Other factors contribute to the increase in housing prices, such as demographic, increase in households' income and so on.
[8] IMF, 2014.
[9] However, it is important to note that Israel, in contrast to those other countries, suffered from a decline in housing prices in the previous decade.
[10] For a summary of the various MPP tools, see Tzur-Ilan (2017).

Figure 1: Rate of Change in Housing Prices in Israel



Source: Israel Central Bureau of Statistics.

[1] The line represents the monthly change in home prices (in annual terms). MPP tools are shown on the vertical lines.

2.2 The Israeli LTV Limit of 2012

The MPP tool that this paper focuses on was implemented in October 2012, when the Banking Supervision Department of the Bank of Israel issued a new directive limiting the LTV ratio in housing loans. This directive was applied to loans approved from November 1, 2012.[11] It established that a banking corporation could not approve mortgage with an LTV of more than 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.[12] 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 hard LTV limits. Moreover, prior regulation had

[11] Overall, Israeli households are not very indebted and so the LTV ratios on mortgages are relatively low. In 2012, the average LTV ratio was 56 percent. For an international comparison of average LTV ratios, see Tzur-Ilan (2017).

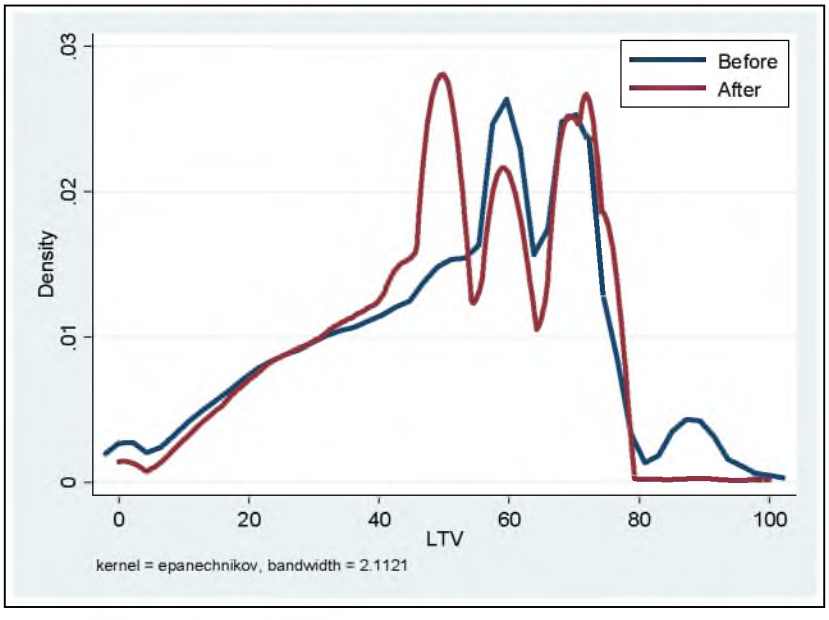
[12] An investment home is defined as a second home in accordance with reports to the Israel Tax Authority, as well as any dwelling acquired by a nonresident.

strongly favored regulatory capital measures using risk weights (e.g., as a function of LTV; for details see Tzur-Ilan (2017).

This paper focuses on the period between January 2012 and September 2013 (the 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.[13]

Figure 2 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 2 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.

Figure 2: LTV Distribution before and after the 2012 LTV Limit



[13] The red area in Figure 1 also includes a MPP applied in February 2013 that changed the capital adequacy ratios for different caps on the LTV ratio. This policy change will not affect the results of this study because it will be included in the control group, as discussed in Section 4.1.

3. Data

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 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 the period from January 1, 2012 to September 1, 2013. The goal is to focus on a limited time period framing the October 2012 imposition of the LTV limit. One reason for this extended time frame is that the literature[14] reports that MPPs prove effective up to six months after they are imposed. In addition, it is better to test a time frame that is relatively free of external shocks that might influence the results, such as the additional MPPs that were imposed after September 2013 (as shown in Figure 1). Accordingly, observations following September 1, 2013 were eliminated, leaving approximately 104,000 observations.

This dataset is merged with another dataset on housing unit characteristics (CARMEN) from the Israel Tax Authority containing information on all home sales transactions and their characteristics. I was able to merge one-third of all the observations from the loan-level dataset to the CARMEN dataset.[15] The data and its construction are explained in detail in Tzur-Ilan (2017), which uses the same dataset but focuses on a different time period—between January 2010 and May 2011—and examines a MPP tool that was implemented in October 2010.

Because the matching process, described in detail below, is not parametric, it is highly sensitive to extreme observations. Therefore, after the average age of borrowers was limited to 20–80 years, the 1 percent of value at each of the extreme

[14] See, e.g., IMF (2013).

[15] As explained in detail in Tzur-Ilan (2017), the merger procedure did not cause any bias (i.e., the observations in the merged dataset, mortgages and housing unit level data are similar in character to those in the complete dataset of all mortgage observations).

upper and lower ends of the distribution of the total income per household, housing price, and loan size variables was omitted.

3.2 Summary Statistics

The dataset includes 34,021 observations between January 1, 2012 and September 1, 2013 (when other MPP tool was implemented). There are 17,260 observations before the LTV limit was imposed in October 2012 and 16,761 observations after it was imposed. The transactions were divided into three borrower types: first-time home buyers, upgraders, and investors.

Table 2 presents summary statistics on mortgage contracts, borrower characteristics, and home purchase transactions before the imposition of the 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.

The Israeli Central Bureau of Statistics constructs a socioeconomic index of neighborhoods, consisting of 16 different variables, including demography, education, employment, income, and standard of living. The 16 variables are combined 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 2 shows the quality of neighborhoods for all three groups and, as expected, the upgraders bought assets in higher-quality neighborhoods.

Although many previous studies have examined investor behavior in the housing market in Israel (e.g. Ben-Naim, 2009, 2010), this is the first paper that uses loan-level data to examine investor behavior in the credit market in Israel. Using these data, this paper finds that approximately 60 percent of investors use a loan to finance a housing purchase. 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 consist 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.

Table 3 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. (As can be seen in Table 2, the average LTV for investors was 52.5 percent, above the limit cutoff.) 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-quality neighborhoods, compared to the average borrower.

Table 2 Summary Statistics

The table reports summary statistics for the sample, which is distributed into three groups according to type of buyer: first-time home buyers, upgraders (who seek to upgrade their housing situation), and investors (who own more than one residential property). Each panel reports details about the loan, the house bought using the loan, and borrower characteristics, for the period Jan 2012-Oct 2012 (i.e., before the LTV limit). Number of observations: 34,021 borrowers.

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
Area (square meters)	87.4	32.7	65.0	84.0	103.0
Rooms	3.7	0.9	3.0	4.0	4.0
Distance from Tel Aviv-Jaffa (km)	38.4	29.5	12.7	28.8	52.9
Quality of Neighborhood	10.6	3.5	9.0	11.0	13.0

Upgraders % Observation = 39

	Mean	St. dev.	25%	50%	75%
House Price (NIS)	1,330,092	684,012	920,000	1,260,000	1,660,000
Borrower Monthly Income (NIS)	15,913	7,776	11,000	14,420	19,173
Borrower age	42.9	9.6	36.2	41.2	48.5
LTV (%)	51.0	19.0	37.0	54.1	66.0
Average Interest Rate (%)	2.90	0.76	2.45	2.82	3.27
Loan Duration (years)	21.2	7.3	15.7	22.2	26.1
Area (square meters)	110.9	53.8	82.0	104.0	129.0
Rooms	4.4	1.1	4.0	4.0	5.0
Distance from Tel Aviv-Jaffa (km)	39.9	30.2	12.7	29.9	59.8
Quality of Neighborhood	11.3	3.8	9.0	12.0	14.0

Investors % Observation = 14

	Mean	St. dev.	25%	50%	75%
House Price (NIS)	1,031,760	712,695	590,000	995,000	1,500,000
Borrower Monthly Income (NIS)	19,928	11,674	12,300	17,500	24,000
Borrower age	44.7	10.9	36.5	43.1	52.0
LTV (%)	52.5	18.8	40.0	58.0	68.0
Average Interest Rate (%)	2.96	0.85	2.46	2.89	3.42
Loan Duration (years)	18.5	7.7	12.9	20.0	25.0
Area (square meters)	87.2	56.4	55.0	75.0	106.0
Rooms	3.6	1.2	3.0	3.0	4.0
Distance from Tel Aviv-Jaffa (km)	42.7	33.0	9.9	40.7	69.0
Quality of Neighborhood	10.6	3.7	8.0	11.0	13.0

Table 3 Summary Statistics for the Leveraged Borrowers

The table reports summary statistics for the sample, which is distributed into three groups, according to type of buyer. Each panel reports detailed information for each subgroup of affacted borrowers within the group: First-Time Home Buyers - those with LTV above 75%, 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 observaions represents the percentage of affacted borrowers within the group. Number of observations: 34,021 borrowers.

First-Time Home Buyers % 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
Quality of Neighborhood	10.0	2.6	8.0	10.0	12.0

Upgraders % Observation = 12.3

	Mean	St. dev.	25%	50%	75%
House Price (NIS)	1,119,732	526,471	790,000	1,020,000	1,350,000
Borrower Monthly Income (NIS)	15,200	8,274	10,625	14,500	20,133
Borrower age	40.1	7.4	34.7	39.2	44.3
LTV (%)	77.7	6.8	74.0	75.0	83.0
Average Interest Rate (%)	3.26	0.64	2.86	3.10	3.43
Loan Duration (years)	26.0	5.4	24.5	26.7	30.0
Area (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
Quality of Neighborhood	11.1	3.2	9.0	11.0	13.0

Investors % Observation = 60.1

	Mean	St. dev.	25%	50%	75%
House Price (NIS)	1,012,185	753,054	480,000	825,000	1,300,000
Borrower Monthly Income (NIS)	16,765	11,789	10,100	13,962	19,500
Borrower age	42.9	10.0	35.5	41.6	49.0
LTV (%)	65.0	8.8	59.7	64.0	70.0
Average Interest Rate (%)	2.98	0.82	2.49	2.89	3.40
Loan Duration (years)	20.1	7.2	15.0	20.0	25.0
Area (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
Quality of Neighborhood	10.3	3.6	8.0	10.0	13.0

3.3 Changes in the LTV Distribution

MPP usually applies for the distribution margin, i.e., the extreme values of the distribution. But, as Figure 3 shows, the LTV cutoff for investors was not in the distribution margin but rather at a below average value, which may have had a strong impact on this group's activity in the credit and housing markets.

Figure 3: LTV Distribution by Buyer Type, before the LTV Limit

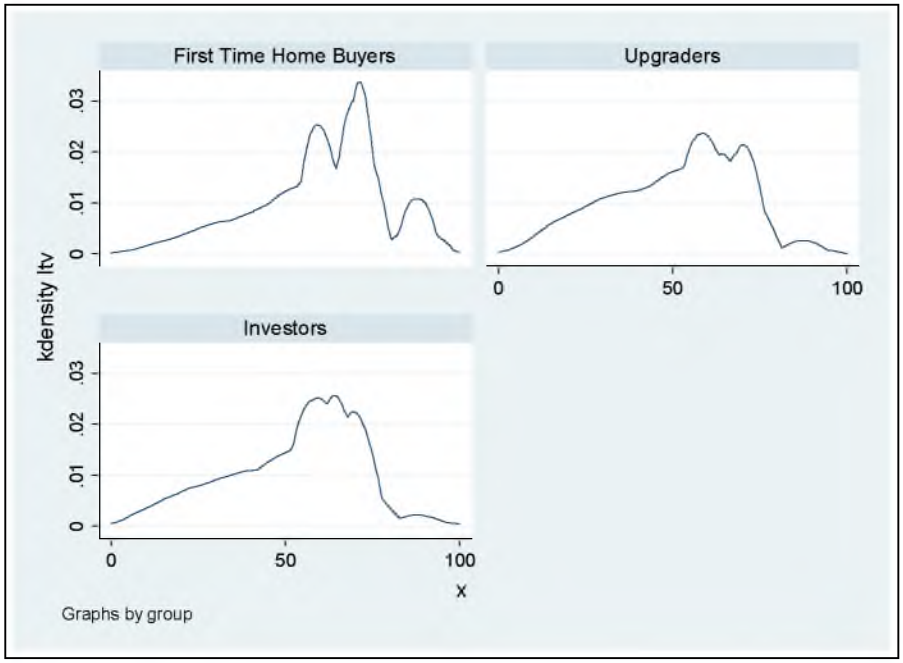
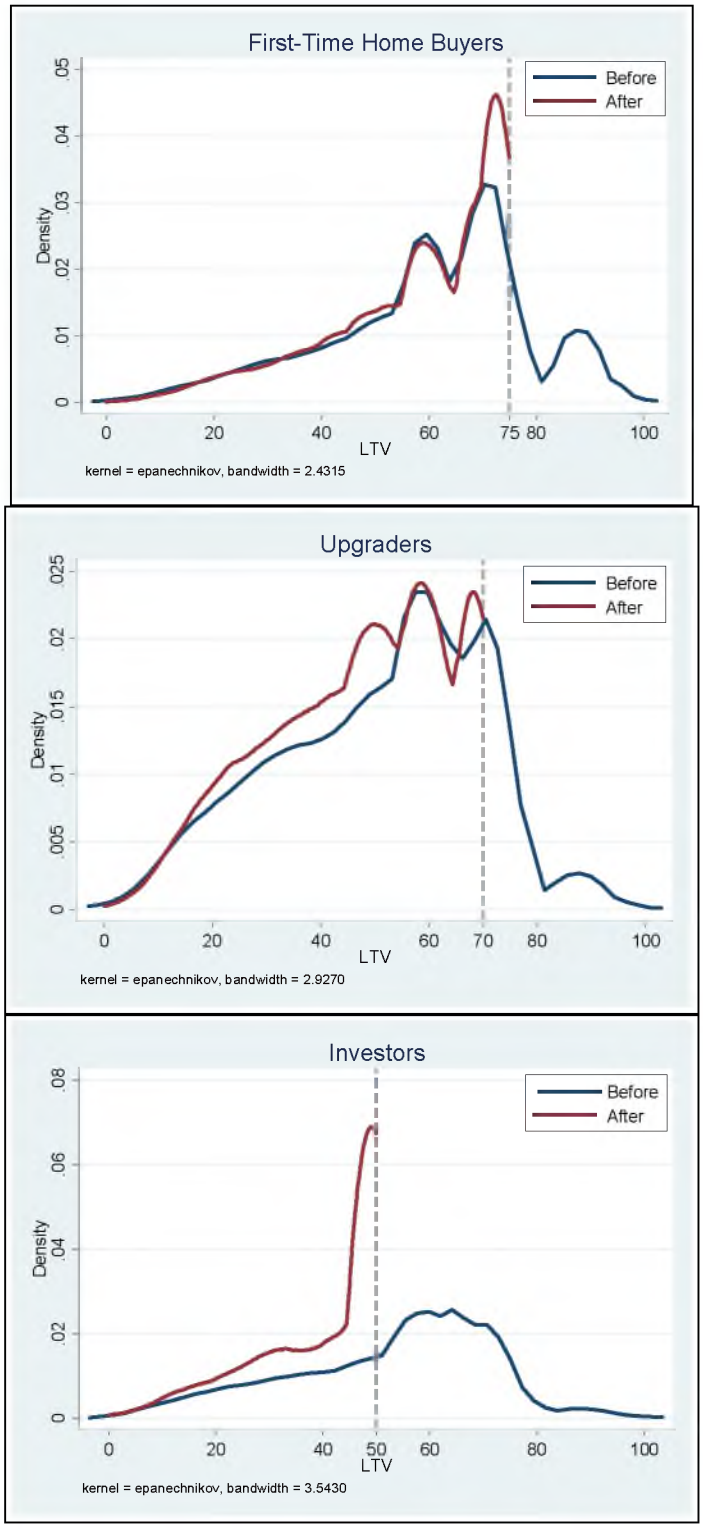


Figure 4 shows the change in the LTV distribution before and after the LTV limit for each borrower type. After the LTV limit there was high density around the value of the cutoffs, for each borrower type. This phenomenon has already been described in the literature, by Ofir and Mugerman (2017), that the limit was erroneously interpreted by the public as the value recommended by the regulator instead of what it actually is—the maximum value. Figure 4 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 LTV distribution after the limit, according to the Kolmogorov–Smirnov test.[16]

[16] Kolmogorov–Smirnov test also been examined for the change in the distribution up to the limit and has been found that there were a significant change in the LTV distribution after the limit, for each buyer type.

Figure 4: LTV Distribution by Buyer Type, before and after the LTV Limit



4. Identification Approach

To evaluate the impact of MPPs, I will try to estimate the link between credit availability and the credit and housing choices of affected borrowers. I will focus on the MPP'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 distinguish constrained borrowers based on the LTV ratio: 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."[17] Accordingly, and this is my key contribution to the literature, this paper uses a prediction of the borrower's leverage choices.

Two methods for identifying the affected borrowers, i.e., borrowers who would have chosen higher a LTV ratio in the absence of the LTV limit, are shown. Both methods use the borrower's age and income to predict the LTV ratio that the borrower would have chosen without the limit. One method is linear and the other non-parametric.

4.1 Predicted LTV Ratio Using OLS Approach

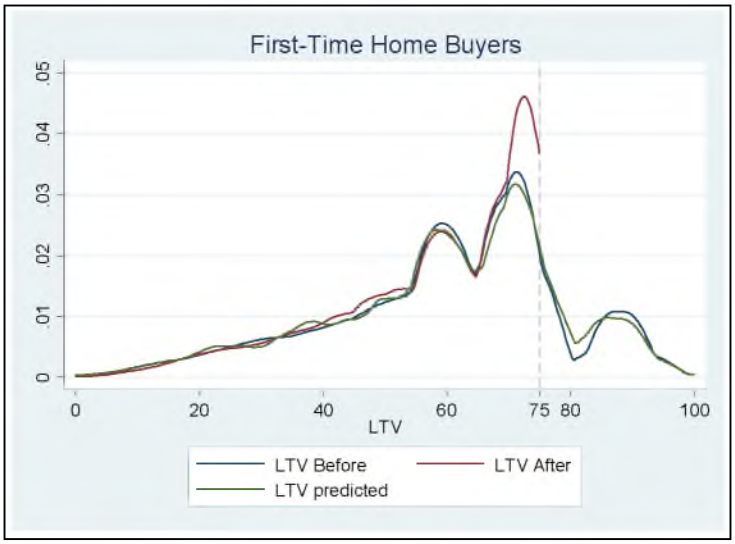
The LTV distribution for the period after the intervention can be predicted using the borrowers' monthly income (in log terms) and age (including age squared) from the period before the intervention. By a linear regression (or OLS approach), the LTV distribution can be predicted for each borrower type. Other borrower characteristics[18] have been used to predict the LTV distribution, but income and age have been found to have greater explanatory power[19]. Also, to strength the prediction's credibility, I divide the sample before the policy intervention into five random subsamples. For

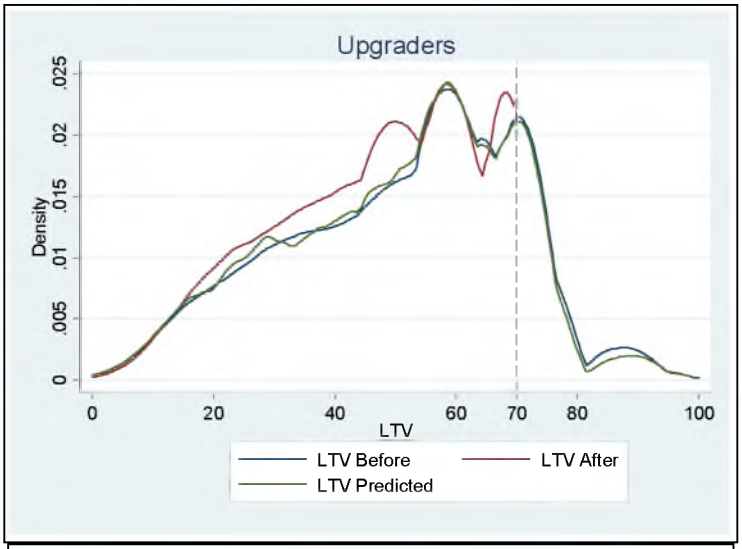
[17] See also Manski and Pepper (2013).
[18] Other borrower characteristics include number of family members and previous place of residence (as indicators of socioeconomic status and residential preferences, respectively).
[19] See De Araujo et al. (2016) use only income as a predictor of the LTV ratio.

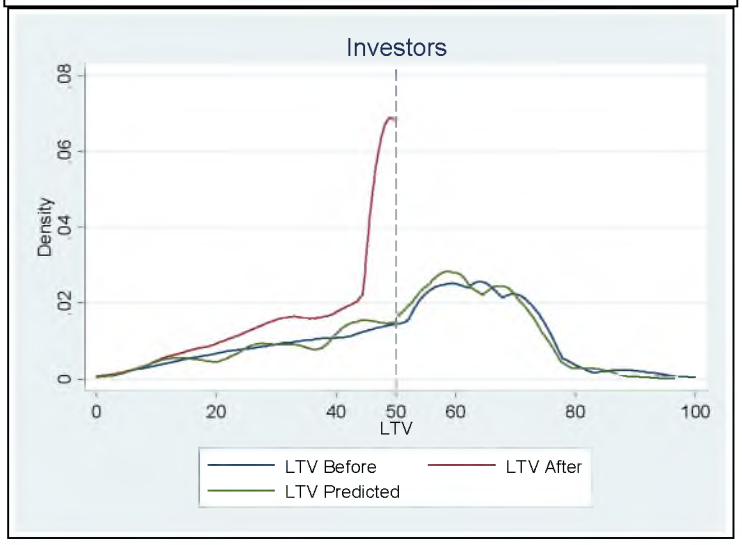
each subsample, I predict the LTV ratio (using the age and income of the borrower) and calculate the root mean square error (out of sample) between the predicted and the actual LTV ratio. The results are between 3.9 and 5.3 root mean square errors on average. To further strengthen the prediction's credibility, the same test is replicated precisely around a placebo period two years prior to the intervention. Again, there is no significant change in the LTV distribution between the predicted LTV and the actual LTV distribution, according to the Kolmogorov–Smirnov test, and the results are between 3.98 and 4.7 root mean square errors. This prediction is based on the assumption that the borrowers' income and age distributions remain unchanged after the regulation and borrowers do not leave the credit and housing markets due to the new regulation.

Figure 5 shows the LTV distribution before and after the limit for each borrower type, and the predicted LTV distribution for the period after the intervention. Overall, for each buyer type, there is no significant change between the predicted LTV distribution and the actual distribution before the limit, according to the Kolmogorov–Smirnov test. This could be expected due to the fact that there was no significant change in the distribution of borrower age and income after the imposition of the restriction (as shown in Section 5.1).

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







Now it is possible to assume what LTV ratio each borrower would have chosen in the absence of the limit. Therefore, one can calculate the change in the LTV ratio for each borrower due to the limit and can also calculate what kind of choices (in the credit and housing markets) the leveraged borrower would have made in the absence of the limit. For example, for an investor who borrowed with a 45% LTV ratio after the intervention, but according to the prediction would have chosen an 80% LTV in the absence of the intervention, we obtain the average interest rate and home price that 80% LTV borrowers usually choose. We can compare those choices to the actual choices made (with the 45% LTV ratio) after the intervention and can calculate the elasticity of demand for each borrower type due to the limit, and examine which borrowers were most affected by the policy. The outcome variables in the credit market are the mortgage interest rate, its term to maturity, and the default rate. In the housing market, the outcome variables are the change in real housing prices, the size of the dwelling, distance from Tel Aviv (the business capital of Israel), and the change in the socioeconomic level of the neighborhood.

In the next step, 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[20] 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). [21,22]

The DID framework can be described as follows. A fraction of a population receives a treatment between two time periods, $\in \{0,1\}$, i.e., "before" and "after" the LTV limit, respectively. Between these two periods, some fraction of the population

[20] See Abadie (2005).
[21] 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.
[22] For more details, see Section 5.

is exposed to the treatment. Let $Y(i,t)$ be the interest rate outcome for individual i at time t. We denote $D(i,t) = 1$ if individual i has been exposed to the treatment prior to period t, and $D(i,t) = 0$ otherwise. We call those individuals for whom $D(i,1) = 1$ "treated," and those for whom $D(i,1) = 0$ "controls" (or "untreated"). Since individuals are exposed to treatment only after the first period, $D(i,0) = 0$ for all i. The conventional DID estimator is usually derived using a linear parametric model. Suppose that the outcome variable is generated by components of a variance process:

$$Y\ (i,t) = \delta(t) + \ \alpha \times D(i,t) + \eta(i) + \ \upsilon(i,t),$$

Where $\delta(t)$ is a time-specific component, α is the impact of the treatment, $\eta(i)$ is an individual-specific component, and $\upsilon(i,t)$ is an individual transitory shock that has a mean of zero at each period, $t = 0, 1$, and is possibly correlated in time. Each borrower has two potential outcomes: $Y_t(1)$ if exposed to treatment and $Y_t(0)$ if not exposed. The outcomes in the empirical application will refer to the characteristics of the mortgage and the characteristics of the housing unit bought with the mortgage.

4.2 Predicted LTV Ratio Using Non-Parametric Matching

Regarding the first method, the OLS approach was used to predict the LTV ratio that the borrowers would have chosen in the absence of the LTV limit. While the OLS approach is perfectly valid, there are a few concerns about its implementation, the major one being that by using OLS, we make strong assumptions about normality or the linear relationship between the covariates of interest. By contrast, equivalent non-parametric statistical methods make no assumptions about the population distribution from which the data are taken. In addition, the OLS approach allows for extreme outliers in the estimation, which can bias the interest rate estimates substantially.

Therefore, the matching method[23] was used as a second estimation strategy, as it is less parametric and more closely related to the notion of a randomized experiment. By this method, only households that are (slightly) below the cutoff[24] 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

[23] The matching method is used rather than propensity score matching when the database is large and there are a small number of observable variables, as is the case in this study.

[24] 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).

income). 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,[25] 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 and maturity) and to the characteristics of the housing unit bought with the mortgage (price and location). To obtain the outcomes, I use the Abadie and Imbens (2011) estimator,[26] which makes it possible to match households based on their income and age; since income and age are continuous variables, they will probably not yield an exact match (though it should be close). To overcome this difficulty, I apply a "bias-correction" component to the interest rate estimates, by focusing on the average treatment effect on the treated (ATT)[27] for an individual with characteristics X (age and income): $ATT=E(Y1-Y0|T=1,X)$. The outcome variables for households that receive treatment and those that do not are $Y1$ and $Y0$, respectively.[28]

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 (described in Section 4.1). The interpretation of the outcome variables is based on the post-treatment outcome gaps between the treatment and control groups. Thus, the ATT is calculated as ATT = (after/before) treatment group - (after/before) control, and it is calculated separately for each outcome variable and for each borrower type.

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[25] The assumption here is that they are from the same distribution before and after the LTV limit, in terms of their characteristics (this is shown in Section 5.1).

[26] The calculations were made using STATA software employing the Nnmatch (nearest-neighbor 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.

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

[28] See Abadie and Imbens (2011); Abadie et al. (2004); Heckman et al. (1997).

5. Results

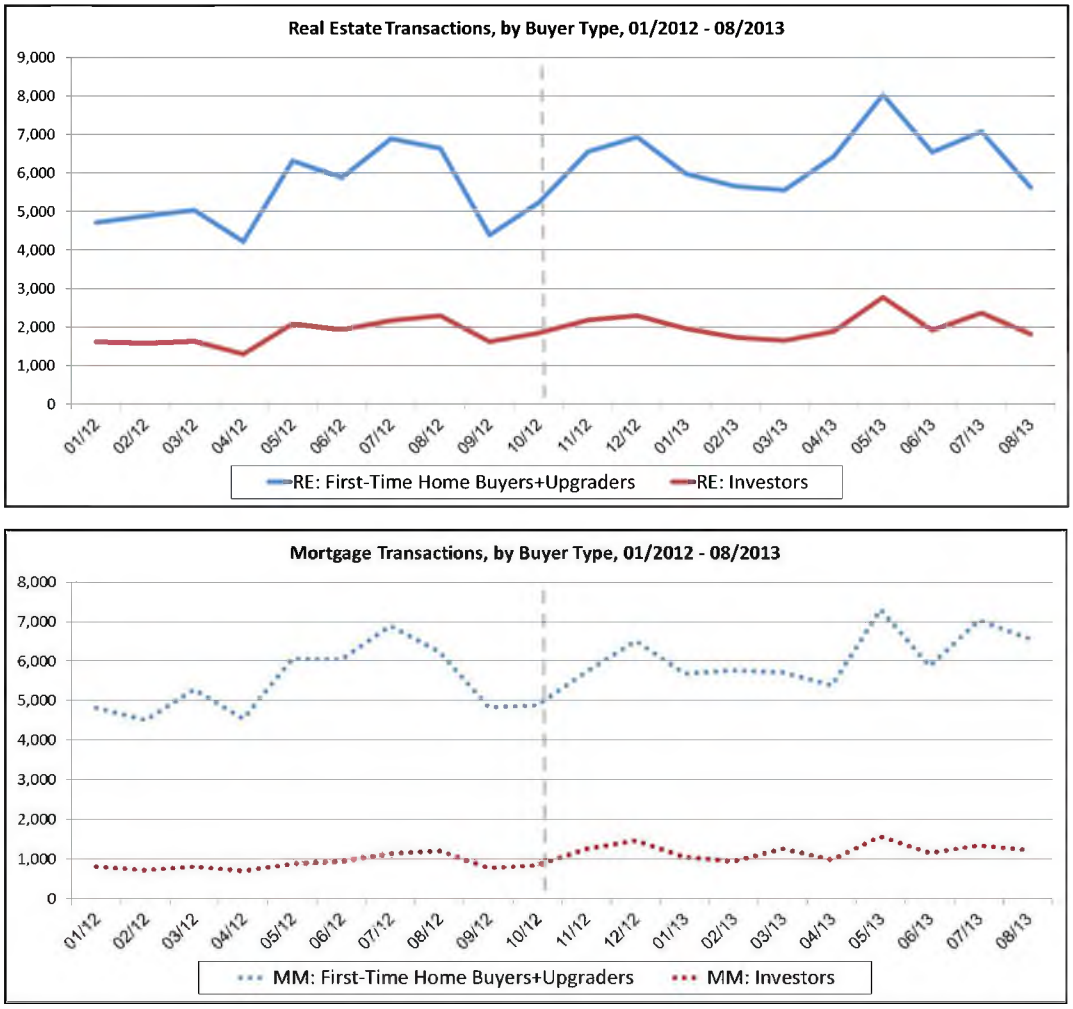
The LTV limit can affect borrowers in three ways. First, borrowers may decide to leave the credit and housing markets because they do not have enough equity to buy a home. In this case, a drop in transactions in the credit and housing markets is expected, and a change in borrower characteristics, if, for example, only high-income households could afford to buy homes due to the limit and there were only older borrowers with higher income. Second, borrowers who do have the same amount of equity after the limit may decide to buy cheaper and more affordable homes. Third, borrowers may still choose to buy the same homes they wanted before the limit, but to take out unsecured or unregulated credit, such as consumer credit.[29] This section examines the first two above-mentioned effects of the LTV limit on borrowers. The third effect is discussed in Section 6.

5.1 Changes in the Distribution of Borrower Characteristics

Using aggregate data from the Bank of Israel, we examine whether households left the credit and housing markets due to the new policy. Figure 6 presents changes in the activity in those two markets, before and after the policy intervention, for each borrower type. As can be seen, apparently households did not leave those markets following the restrictions.

[29] Braggion, Manconi, and Zhu (2017) find that policy intervention in the real estate mortgage market caused an increase in the demand for peer-to-peer lending in China.

Figure 6: Activity in the Credit and Housing Markets, by Borrower Type



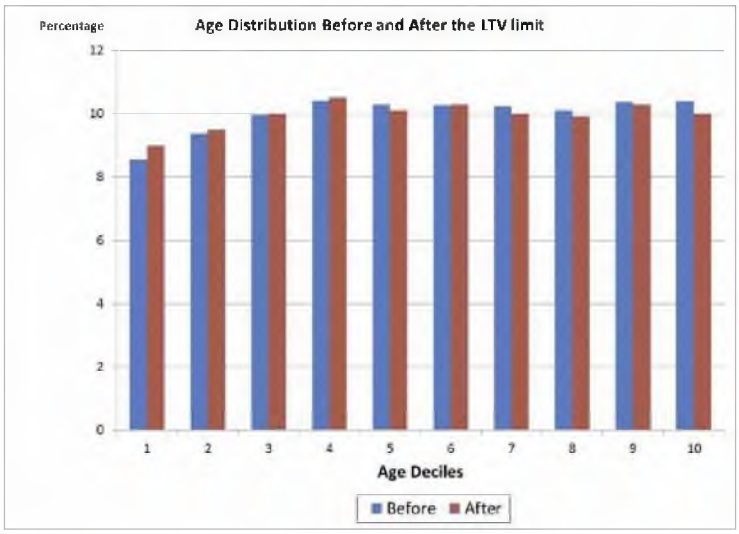
An important question that usually arises in the literature on changes in credit constraints is whether the LTV limit affects the distribution of borrower characteristics. That is, are the same types of borrowers present before and after the imposition of the restriction, or does the LTV limit push out certain types of borrowers, e.g., borrowers with limited access to the credit market?[30] Figure 7 examines the distribution of borrower age and income before and after the imposition of the LTV limit. A Kolmogorov–Smirnov test shows that there was no significant change in the distribution of borrower age and income. In other words, the population of borrowers is the same in terms of age and income, before and after the restriction.

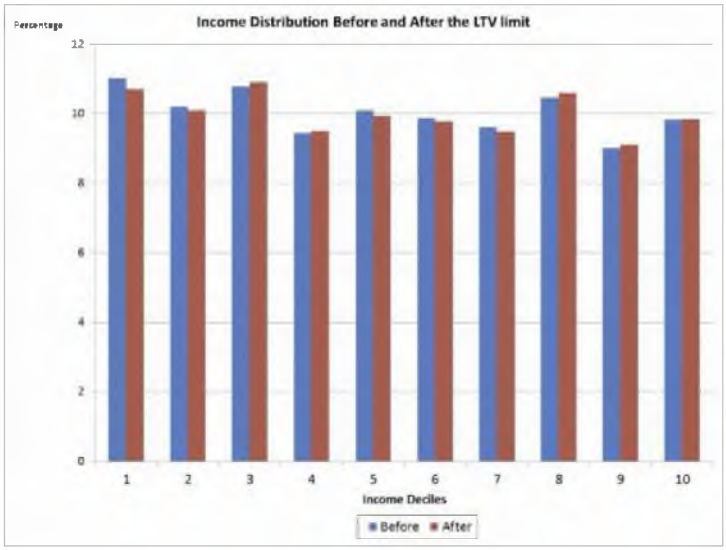
Tzur-Ilan (2017) yielded similar results, but the soft LTV limit did allow borrowers to take out loans with a high LTV ratio and pay a higher interest rate. Therefore, the percentage of those who might leave the housing market due to the

[30] See, e.g., Stiglitz and Weiss (1981).

soft- limit was expected to be lower than in the hard-limit case. Also, it might be that due to the hard LTV limit, only wealthier borrowers will continue to borrow because low-income borrowers will not have enough equity to take out a mortgage. However, the results show that the same kind of borrowers (in terms of age and income) stay in the mortgage market and continue to buy houses, but with lower mortgage, so it might be that they are buying cheaper homes or finding other ways to increase their equity.

Figure 7: Change in Distribution of Borrower Characteristics





5.2 Effect of the LTV Limit on the Credit and Housing Markets

5.2.1 Difference-in-Differences Using the OLS Predicted LTV Distribution

To control for unobservable or other macroeconomic events that might affect the elasticity 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 (control) group as a symbol of other macroeconomic events that might have occurred at the same time. Recall that we do not know what the borrowers' leverage choices would have been 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. Figure 5 shows the three distributions: LTV before (blue), LTV after (red), and LTV predicted (green), for each borrower type. The basic assumption in DID estimation is that the treatment and control groups will both be very close to the cutoff. We therefore examine groups that are very close to the cutoff (above and below) while ruling out transitions between the groups. Because the root mean square error in the prediction is a maximum of 5 percentage points (as mentioned in Section 4), we set the treatment group to be 5 points higher than the cutoff (before and after the policy intervention) and the control group to be 5 points lower than the cutoff (before and after the policy intervention). This is referred to as "the local treatment effect".

Table 4 presents the results of the (local) effect of the LTV limit on the credit and housing choices of affected borrowers. Even when the macroeconomic changes that occurred at the same time as the policy intervention (for example, the sharp rise in housing prices) are taken into account, it can be seen that the LTV limit did change borrower choices in the credit and housing markets. The results on the housing market in Table 4 show that there was a significant decline in real housing prices[31] after the LTV limit for each borrower type (but more so in the treatment group, compared to the control group). In each group there was also a significant change in housing characteristics: borrowers in the treatment group bought smaller homes, farther from the center, and in lower-quality neighborhoods, compared to borrowers in the control group. In the credit market there are counterintuitive results: 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, for each buyer type there is an increase in the interest rate and in the term to maturity and will be discussed in Section 6.2.

[31] Real housing prices are deflated by the monthly change in the Index of Housing Prices.

Table 5 shows the change in each variable of Table 4 as a percentage. For each variable, the borrower type most affected by the new policy is represented in red. As can be seen, investors were most affected by the policy change, and changed their choices in the housing market: they bought houses that were 21 percent less expensive, 16 percent smaller, 24 percent farther from the center, and in neighborhoods that were 21 percent lower in quality. The other two groups were also affected by the policy intervention and changed their choices in the housing market, but less so compared to the investors.

Investors changed their choices significantly more than other two groups, probably because they are more resilient to changes and care more about maximizing their yield and less about the quality of the neighborhood. Also, one possible reason for the higher increased in the interest rate for investors is that the new policy led them to buy riskier assets, and in particular assets farther from the center (will be discussed in Section 6.1).

Table 4: Difference-in-Differences Estimation of Effect of LTV Limit on Credit and Housing Choices

The table reports the change in credit and housing characteristics, between the treatment and control groups, above and below the cutoff respectively, before and after the LTV limit. Number of observations: First-Time Home Buyers: There are 2,915 borrowers in the control group and 2,859 in the treatment group. Upgraders: 1,526 borrowers in the control group and 1,590 in the treatment group. Investors: 450 borrowers in the control group and 510 borrowers in the treatment group. Note: Real home prices were inflated by the monthly change in the Index of Home Prices. Standard errors take into account prediction stage estimation uncertainty and ***; **; * indicate significance at the 1, 5, and 10 percent levels, respectively.

	First-Time Home Buyers	Upgraders	Investors
	65-70 VS 80-85	60-65 VS 75-80	40-45 VS 55-60
Real home prices (NIS)	**-80,728*** **	**-65,751*** **	**-154,261*** **
	(13,246)	(17,924)	(10,211)
Size (square meters)	**-10.2*** **	**-6.4*** **	**-16.1*** **
	(2.5)	(1.7)	(2.7)
Distance from Tel Aviv (km)	**6.7*** **	**3.8*** **	**14.6*** **
	(1.7)	(1.2)	(2.7)
Neighborhood quality	**-1.0** **	**-0.3**	**-1.9*** **
	(0.4)	(0.3)	**(0.4)**
Interest Rate (p.p.)	**0.35*** **	**0.19**	**0.51*** **
	(0.13)	(0.17)	(0.22)
Maturity (years)	**1.2*** **	**0.6***	**0.9** **
	(0.3)	(0.4)	(0.4)
Default (p.p.)	**-0.3*** **	**-0.16** **	**-0.1**
	(0.11)	(0.8)	(0.1)
N	5,774	3,116	960

Table 5: Difference-in-Differences Estimation of Effect of LTV Limit on Credit and Housing Choices (Percentage Change)

Percentage	First-Time Home Buyers	Upgraders	Investors
	65-70 VS 80-85	60-65 VS 75-80	40-45 VS 55-60
Real home prices	**-0.10*** **	**-0.07*** **	**-0.21*** **
Size	**-0.12*** **	**-0.06*** **	**-0.16*** **
Distance from Tel Aviv	**0.13*** **	**0.07*** **	**0.24*** **
Neighborhood quality	**-0.10** **	**-0.03**	**-0.21*** **
Interest Rate	**0.35*** **	**0.19**	**0.51*** **
Maturity	**0.05*** **	**0.02***	**0.05** **
Default	**-0.3*** **	**-0.16** **	**-0.1**

5.2.2 Difference-in-Differences Using Non-Parametric Matching

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 and 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. Overall, the results are similar to those of the first estimation strategy. In terms of housing characteristics, affected borrowers bought lower-quality assets: cheaper, smaller, farther from the center, and in lower-quality neighborhoods. As Table 7 shows, investors changed their choices more than the other two groups (the same result was obtained by the first estimation strategy). Again, we obtain counterintuitive results on the effect of the imposition of a hard LTV limit on new contracts in the credit market. For all three borrower types, the affected borrowers paid a higher interest rate and increased their terms to maturity (despite the decrease in the loan amount).

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

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 estimat which borrowers would have borrowed above the limit before the policy, conditional on borrower income and age. Treated borrowers would have had an LTV near to and above the cutoff, if they had been allowed to borrow before the policy intervention. 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.

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

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

	First-Time Home Buyer	Upgraders	Investors
	70-75 VS 75-80	65-70 VS 70-75	45-50 VS 50-55
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**

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 quality of the neighborhoods, while the size of the housing unit changed the least. It can be seen that the 2012 hard 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-quality neighborhoods (18%), and into smaller houses (14%), compared with 15%, 9%, and -2%, respectively, in response to the 2010 soft LTV limit (see Tzur-Ilan (2017)). This is probably because the 2012 hard 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 quality of the neighborhood. The final difference between the results in this paper and those of Tzur-Ilan (2017) is that here the borrowers changed the size of their housing unit, whereas after the 2010 limit there was no such change.

5.2.3 The Elasticity Response of the LTV Limit on Each Borrower Type

The structure of the 2012 limit can help examine the elasticity impact of the hard LTV limit on each borrower type, because different buyer types are limited by different LTV cutoffs. This is a matter of interest for two main reasons. First, the hard LTV limit is criticized for making it difficult for households in need of credit to purchase houses and hence discriminating against weaker segments of the population. In this section, I calculate the elasticity response due to the 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 hard LTV limit is calculated differently for each borrower type, according to $\eta = \frac{\Delta Y}{\Delta LTV} * \frac{\overline{LTV}}{\bar{Y}}$.

Table 8 shows the elasticity of the change in the choices of borrowers in the credit and housing markets, for each borrower type. In terms of the credit and housing markets, 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. In terms of the interest rate, investors are the most affected and have the

highest elasticity. But in terms of other variables in the credit market, such as maturity and default, first-time home buyers exhibited the highest elasticity.

Table 8: Elasticity of Demand in the Credit and Housing Markets

This table shows the elasticity of demand in the credit and housing markets for each borrower type. In the first stage the predicted LTV distribution was calculated using an OLS approach and the borrower's income and age as a prediction of the LTV ratio (Figure 5 shows the results of the prediction). It was then know what LTV ratio each borrower would have chosen in the absence of the limitation. In the second stage 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, in order to calculate the elasticity of demand.

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

6. Additional Perspectives on the Effectiveness of the Hard LTV Limit: Does It Lower Borrower Risk?

6.1 Are Housing Assets Farther from the Center Riskier?

One of the main findings so far are that the hard 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 asking price and the last price of each ad (between the years 2013 and 2016). Figure 8 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 9 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 8, 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 hard 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.

Figure 8: Price Gap between the First Asking Price and the Last Asking Price along the Israel Coastline

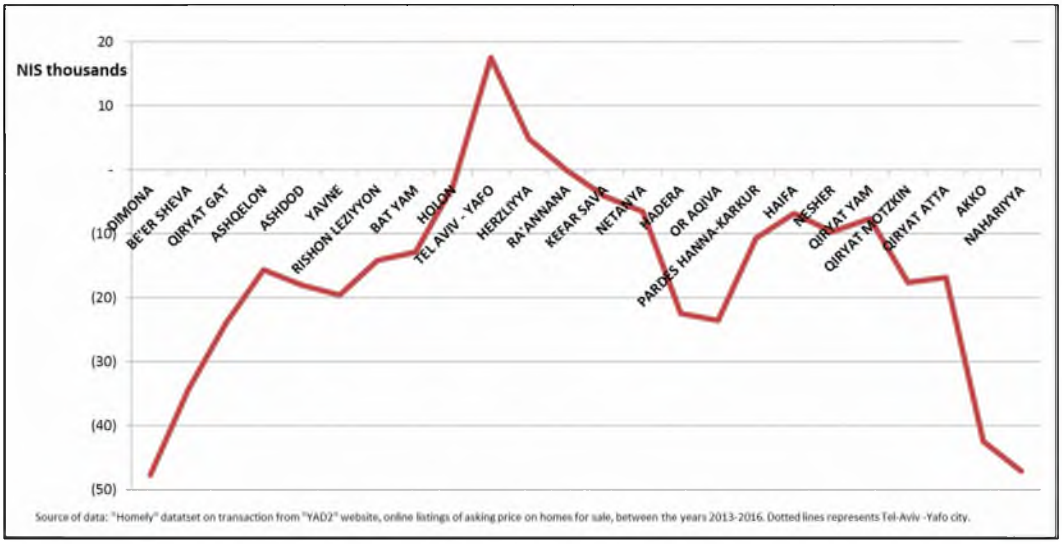
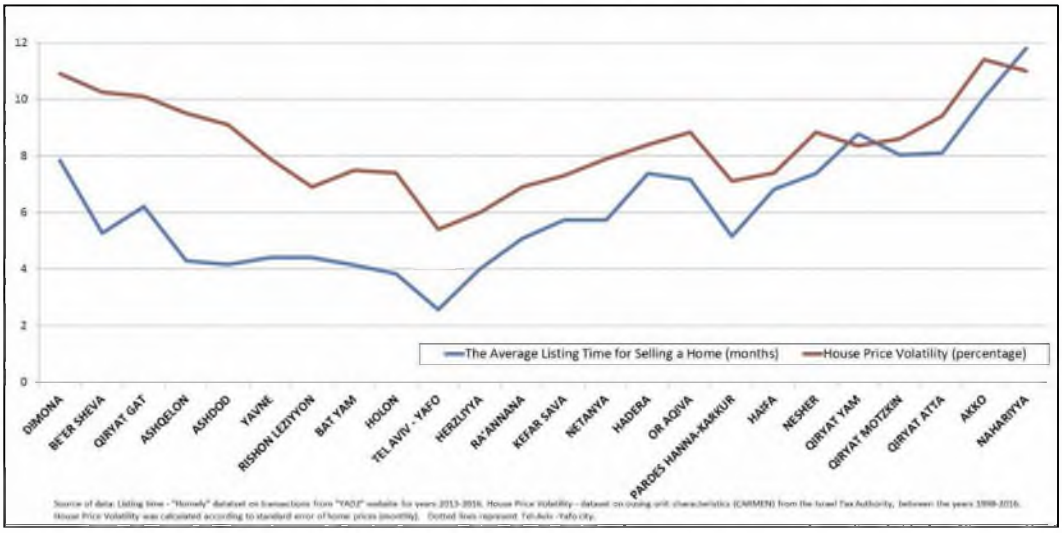


Figure 9: Risk Measure of Property Assets along the Israeli Coastline



6.2 Shifts in Demand for Consumer Credit

Another important and counterintuitive result from this paper is that the hard LTV limit increased the interest rate and the term to maturity on those loans. One potential consequences of the hard LTV limit is that because of its structure, might cause borrowers to take out consumer credit. Any increase in deposit requirements for borrowers carries the risk that borrowers will resort to unsecured borrowing to meet the new requirements. In some instances the hard LTV limit may transfer borrowing to riskier short-term sources of finance, thereby reducing the effectiveness of the

measures[32]. These consumer loans are riskier than mortgages, both for the banks and for the households. This section examines the common side effect of the hard LTV limit to see whether such a tool, which is supposed to lower borrower risk, actually leads borrowers to take out riskier credit.

Following the imposition of a hard 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 10 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[33] (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 Section 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,

[32] 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.

[33] The assumption here is that the demand for mortgages stays the same after the policy intervention, and therefore this figure is an underestimate because housing prices increased in the years following the intervention.

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 11 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 hard LTV limit on the rate of change in mortgages is examined (controlling for interest rate, housing transactions, real estate taxes, and rent prices). Second, the impact of the hard 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 hard 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 12 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. 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.

Figure 10: Withdrawals from Several Financial Resources, 2011–2013

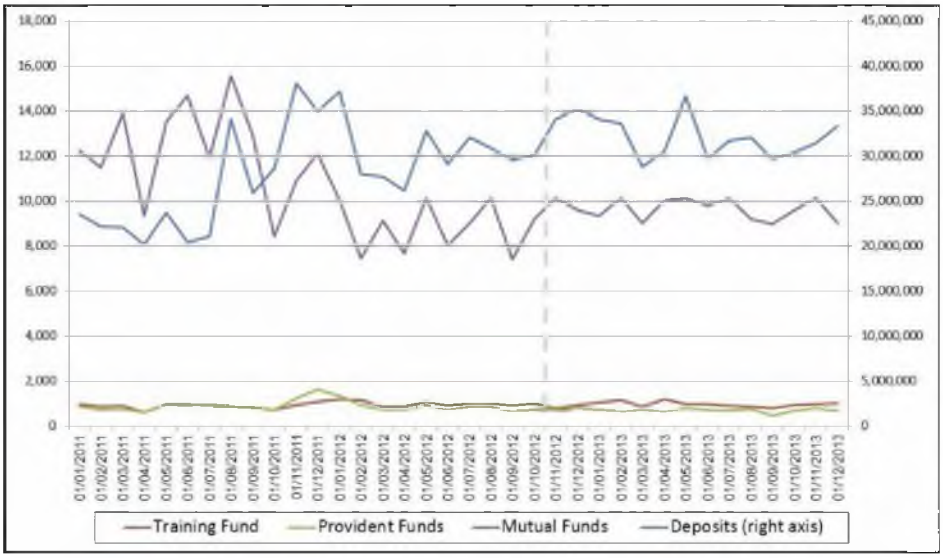
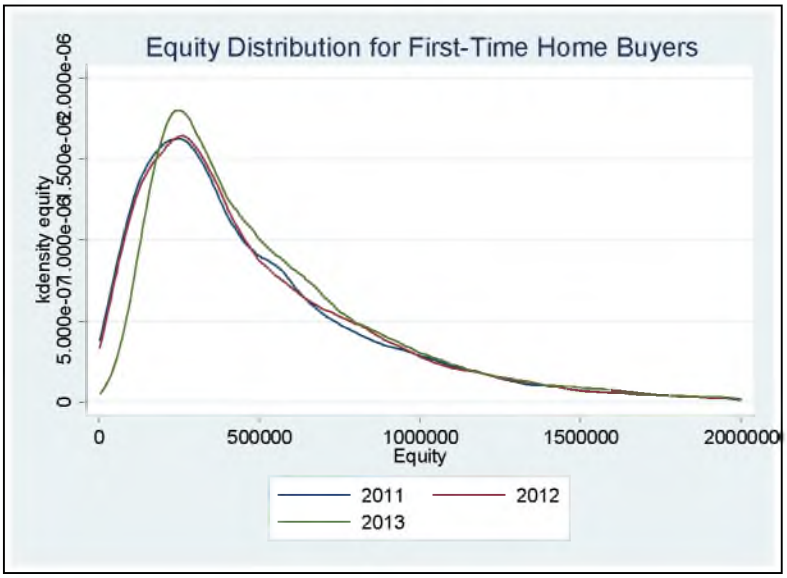
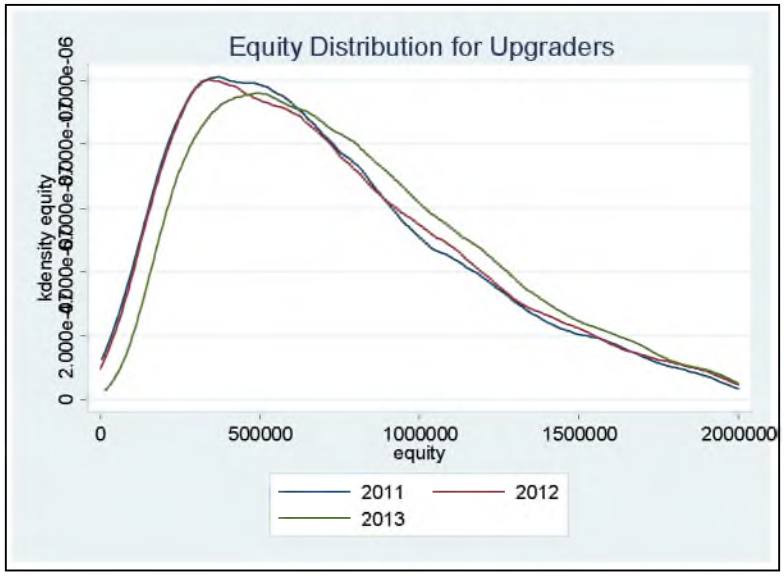


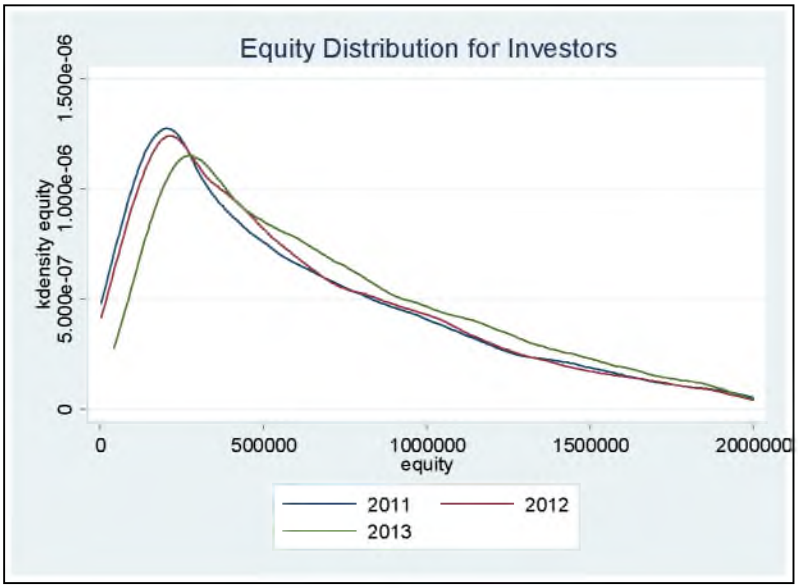
Figure 11: Changes in Mortgages and Consumer Credit over Time

Billion NIS	2011	2012	2013	2014
Mortgages (yearly change, flows)	21.2	21.5	21.7	18.9
Consumer Credit (yearly change, flows)	4.4	1.8	7.6	7.7

Figure 12: Changes in Equity Distribution before and after the LTV Limit







7. Conclusion

This paper exploits the effect of a policy change that limits the LTV ratio (i.e., a hard LTV limit) according to the type of buyer (first-time home buyers, upgraders, and investors). The hard LTV limit, implemented in Israel in 2012, was the first time that this MPP tool was used in Israel; prior regulation had focused on LTV-based capital provisions (i.e. soft LTV limit). An important finding of the present paper is that the two policies, soft and hard LTV limits, are in fact very similar and generate similar effects on household choices in the credit and housing markets.

The results of this paper show that the hard LTV limit did not induce borrowers to leave the real-estate or the mortgage markets. Similar results have been found for the soft LTV limit. The paper also finds that the LTV limit contributed to achieve the goals of the MPP policy; some borrowers lowered their leverage due to the limit and purchased cheaper assets, in lower-quality neighborhoods often located farther from the center. This is especially true for investors, who seem to have responded to the LTV limit more than the other two groups. In general, the magnitudes of the effects documented in this study are larger than in Tzur-Ilan (2017), probably because of the higher costs associated with circumventing a hard LTV limit in comparison with a soft one. Under the hard 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). Additionally, consumer credit is typically of shorter duration, thereby increasing the borrower's monthly debt payment. Probably due to the increase in the monthly debt payment on the non-mortgage credit, some borrowers were forced to increase their mortgage duration, resulting in an increase in the mortgage interest rate.

Another way to summarize the effects of the hard 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. Whereas the former effect is present also in the case of the soft LTV limit discussed in Tzur-Ilan (2017), the hard 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.

At a different level, the paper leaves many unanswered questions, mainly in relation to the counterfactual: How stable would the financial system have been had the LTV limit not been imposed? Is an LTV limit equivalent in terms of its effects to a system where each bank sets its own LTV limits? How would the banks price risk? And how do MPP tools affect the supply of credit and the level of competition among financial institutions? These are interesting questions I hope to pursue in future research.

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