Unintended Consequences of Credit Constraints on Housing: The Case of LTV Limits

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Abstract

This paper examines the effects of a Loan-to-Value (LTV) limit on household choices in the credit and housing markets. Using a large and novel household-level database from Israel with detailed information on loans, borrower characteristics, and acquired housing units, and applying matching techniques, I find that the LTV limit had an effect on mortgage contract terms, but did not lead to credit rationing (no segment of the population was excluded from the market). I also find that the LTV limit induced borrowers to buy cheaper and lower-quality housing units and to move farther from high demand areas to lower-quality neighborhoods. I conclude that an LTV limit, the most common macroprudential policy (MPP) tool, affects not only financial stability by reducing the leverage of households, but also the decisions of borrowers in the housing market.

Keywords: Macroprudential policy, LTV, loan-to-value ratio, mortgage, real estate, household finance, financial stability

JEL classification: E43, E58, E61, G21, G28, R28, R51

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1. Introduction

The 2008 global financial crisis has led many economists to recognize that there was a fundamental lack of understanding of the systemic risks in the financial system, and that policies needed to prevent the realization of these risks were lacking. One such policy is macroprudential policy (MPP). Its goal is to strengthen the resilience of the financial system to shocks and to moderate the impact on real economic activity when financial risks are actually realized (Galati and Moessner, 2013).

MPPs related to the housing market are a major regulatory tool used in several countries. The most common MPP targeting the housing market is the imposition of a Loan-To-Value (LTV) limit on housing loans (Crowe et al., 2013). An LTV limit is designed to protect the banking system from risks associated with excessively leveraged borrowers. There is 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 (Nabar and Ahuja, 2011). However, LTV limits may have other, unintended effects on the economy and 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.

In this paper, I seek to empirically assess the impact of an LTV limit on consumer choices. I exploit a policy change introduced in October 2010 that required banks in Israel (the only mortgage providers[3]) to increase capital provision for mortgages with an LTV limit of greater than 60 percent.[4] This guideline did not apply to housing loans originally amounting to less than NIS 800,000 (≈US $200,000). I argue that imposing capital provisions for loans with an LTV limit of greater than 60 percent may shift several characteristics of the loan contract terms and therefore shift borrower behavior in the housing market. For example, banks are likely to increase the interest rate on loans to risky borrowers due to the LTV limit, which, in turn, may cause borrowers to lower their LTV ratio. As a result, risky borrowers may face not only different loan terms but also different housing alternatives.

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[3] Commercial banks in Israel are responsible for 96 percent of all mortgages to households, and 94 percent of total credit to households.

[4] For an international comparison of LTV limits, see Appendix A.

The main difficulty in estimating the impact of an LTV limit on the housing market is due to the fact that housing prices provide incentives for the imposition of the LTV limit, which, in turn, may affect housing prices, so that housing prices and policy measures are jointly determined. In addition, a key challenge in estimating the effect of an LTV limit on the housing and credit markets is that this policy tool is typically accompanied by a number of other prudential lending regulations, macroeconomic events, and, often, a booming housing market.[5] Therefore, although MPP has attracted much attention among researchers and policymakers, it still lacks a basic analytical framework.

I use a unique borrower-level dataset from the Bank of Israel with loan contract information and information on borrower characteristics. I merge this dataset with data from the Israel Tax Authority on the characteristics of the housing units purchased by those borrowers. The merged dataset contains information on borrower characteristics, housing units purchased, and mortgages taken by 27,324 households in the 18 months centered on the October 2010 policy change (January 2010 to May 2011). Using this database, I identify causal relations between the LTV limit and consumer choices in the credit and housing markets.

This study contributes to the growing literature on MPPs by providing a credible estimate of the impact of LTV limits, the most common MPP tool, on the housing market. 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, Claessens, 2015, Lim et al., 2011). Using a household-level database, this study uncovers a causal link between the imposition of an LTV limit and the demand for housing. 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 and Han et al., 2015). Moreover, to my knowledge, 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 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

[5] For more details, see Section 3.

limit on different segments of the population, especially those with limited access to credit.

This paper provides an empirical assessment of the effects of the LTV policy on the subset of borrowers constrained by the policy, i.e., the average treatment effect on the treated. However, the treatment status is observed only ex ante, before the policy went into effect. 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 use two different matching approaches that identify individuals affected by the policy in a not directly observable way. The first approach is based on a comparison between identical households before and after the introduction of the LTV limit. This approach suffers from the potential effects of time-varying macroeconomic events on the results. The second approach is based on a comparison between 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. I obtain similar results using these two approaches.

The first step is to examine whether the LTV limit was actually effective, that is, whether banks set aside more capital against risky loans, and changed interest rates for risky borrowers. This is done using the difference in the interest rate paid by two identical borrowers (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 (0.01–0.03 percentage points, not statistically significant). After the regulation, the interest rate paid by a borrower with an LTV ratio just above 60 percent was, on average, 0.23–0.35 percentage points significantly higher than the interest rate paid by the borrower with identical characteristics just below the LTV limit. 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.

An important question which usually arises in the literature as a result of changes in credit constraints is whether the LTV limit affects the distribution of borrowers. That is, does one see the same types of borrowers before and after the imposition of the LTV limit or, instead, do banks avoid giving loans to certain households? The literature calls the latter phenomenon “credit rationing,” 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 (Stiglitz and Weiss, 1981). I find that there was no significant change in the distribution of borrowers’ age and income after the imposition of the LTV limit (i.e., no support for the credit rationing hypothesis).

Therefore, the second main question of this paper is: how does the LTV limit affect the characteristics of the housing unit that borrowers decide to buy? Matching similar households (by income and age) before and after the LTV limit, I examine the differences in their choices in the housing market. The LTV limit had significant effects: after the imposition of the LTV limit, households bought assets that were 8.1 percent (significantly) cheaper in real terms,[6] 8.4 percent farther away from Tel Aviv (the business capital of Israel), and 9.1 percent lower in quality according to the neighborhood’s socioeconomic score. 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 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. To understand the magnitude of these changes, 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 farther away from Tel Aviv, to significantly lower-rated neighborhoods. I conclude that the LTV limit caused the borrowers to buy lower-quality assets.

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[6] Real home prices were inflated by the monthly change in the hedonic index of home prices in Israel.

A commonly raised concern regarding LTV limits is that they may inadvertently target young couples or first-time home buyers. This is because LTV limits impose direct financial constraints on households' ability to borrow and tend to be restrictive for those with little savings to use as a down payment, as they are at the beginning of their life cycle of earnings.[7] Using microeconomic data, this study explores which sub-segments of the Israeli population were most affected by the 2010 LTV limit: first-time home buyers versus investors (owners of more than one residential property), young versus older households (above the age of 40), and so forth. In line with Igan and Kang (2011), I find that older households and those who own more than one residential property were more affected by this LTV limit. A possible explanation of these results is that older buyers and investors are more flexible in their purchasing decisions than young borrowers or 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 work.

In summary, my main contribution to the literature is the estimation, using a rich data set, of borrower-level shifts in loan terms and borrower behavior in the housing market resulting from the imposition of an LTV limit. The estimated effects also have important policy implications. While LTV limits typically target commercial banks, my results show that they can have a considerable spillover effect on households. For example, an LTV limit may cause borrowers to move away from the center to a lower-quality neighborhood. Thus, understanding how market participants respond to the policy is crucial for developing an appropriate policy response framework. The lessons learned from this paper are important not only for Israel, but also for other countries around the world where LTV limits are used.

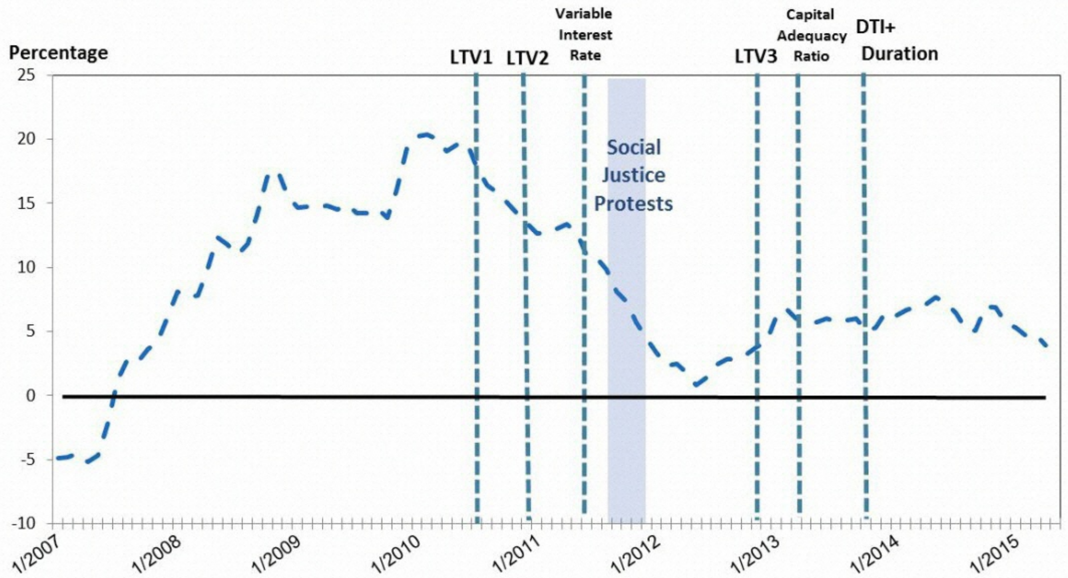
[7] In Blanchard et al., 2014, Stanley Fischer, who served as Governor of the Bank of Israel between 2005 and 2013, write that the LTV limit of October 2010 came under considerable pressure because "we were not allowing young couples to buy housing when we raised the capital ratio on high LTV loans" (page 94).

2. Background: The Housing Market 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. To date, nominal housing prices have risen by 128 percent since 2007, 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. Figure 1 shows the rate of change in housing prices in Israel and, depicted in vertical lines, the various MPP tools employed (see Appendix B for a detailed timeline). There was a slower rate of increase in housing prices around the time the restrictions were imposed, 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.

Figure 1: Rate of Change in Housing Prices in Israel[1]



Source: Israel Central Bureau of Statistics.

1 MPP tools represented by the vertical lines. For more details see Appendix B. The dotted line represents the monthly change in home prices (in annual terms).

3. The LTV Limit

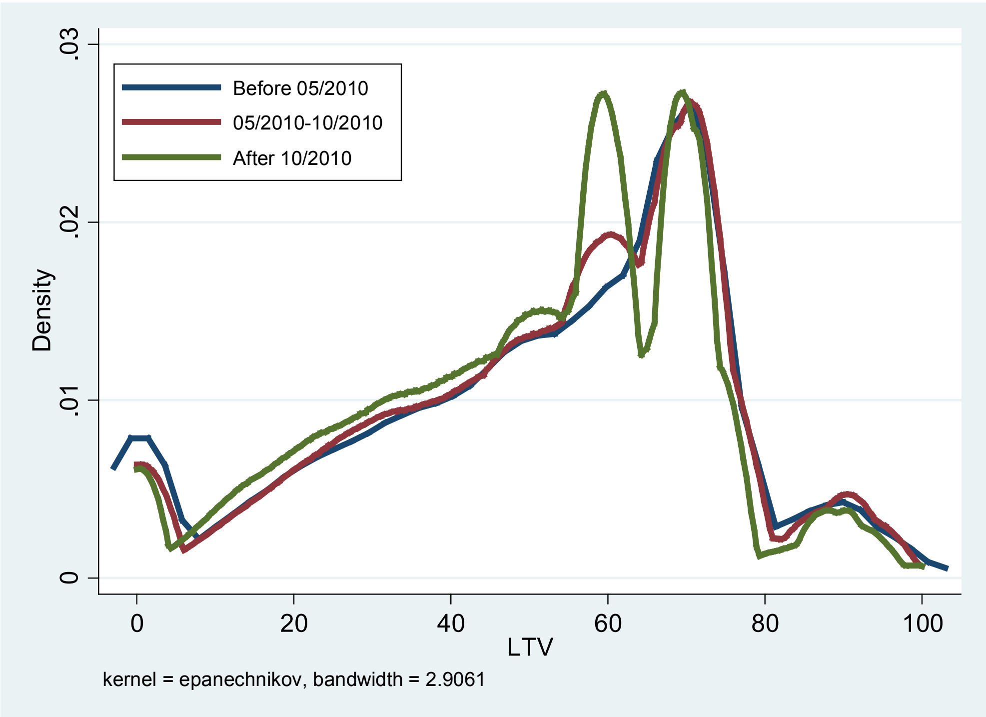
The first MPP issued by the Supervisor of Banks, in May of 2010, required banks to maintain an additional allowance of at least 0.75 percent of outstanding housing loans with an LTV of over 60 percent[8] on the date the loan was provided. This policy change was intended to make the loans more expensive for the banks, which were expected to roll the cost over to the borrowers and induce them to reduce loans with a higher LTV. In practice, however, it appears that this limit was not effective: the constraints were not binding, mainly because the actual allowance set by the commercial banks in Israel probably would have been higher in any case (see Figure 2).

In October 2010, the Supervisor of Banks issued a directive concerning capital provision for loans with a high LTV ratio. The banks were required to increase capital provisions for housing loans whose variable interest-rate portion was 25 percent or more from the existing 35–75 percent (depending on the loan characteristics) to 100 percent, with an LTV limit on the date of issue of more than 60 percent. The

8 Israeli households are not very indebted and LTV ratios on mortgages are relatively low: in 2010, the average LTV was 52 percent. For an international comparison of average LTV ratios, see Appendix A.

guidelines did not apply to housing loans originally amounting to less than NIS[9] 800,000. 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 greater than 60 percent faced higher interest rates, which incentivize them to lower leverage. As Figure 2 shows, in the aftermath of the second LTV limit issued by the Supervisor of Banks, banks began re-pricing loans with an LTV of greater than 60 percent. Therefore, this paper will focus only on the LTV limit issued in October of 2010. The advantage of focusing on the second LTV limit (the first effective one) is that it enables to examine the element of public surprise. Regarding the subsequent restrictions, such as LTV3[10], it will be possible to argue that the market foresaw them or learned how to bypass them.[11]

Figure 2: LTV Distribution by Date of MPP Implementation



[9] For the percentage of borrowers to whom the restriction applies, see Appendix I.

[10] For a detailed information regarding MPPs used in Israel, see Appendix B.
[11] According to Google Trends, the number of searches for the word "equity" in Israel increased by 50 percent between October 2010 and December 2012. This coincides with the period between the second and third LTV limits.

4. Literature Review

There is mixed evidence for the effect of credit constraints on the housing market. The literature mainly examines the effect of MPP on housing prices and can be divided into two main areas (Claessens, 2015). The first engages in aggregate analyses of a cross-section of countries, focusing on the procyclicality of the real estate and credit markets. The second involves country-specific studies using micro data that generally focus on specific risks or sectors.

Aggregate (or cross-country) studies generally find that direct MPPs, such as LTV limits, may create positive feedback between credit growth and asset price inflation. Lim et al. (2011) use a cross-section of 49 countries to show that LTV or PTI (Payment-to-Income) limits lower the correlation between the growth rate of credit and the growth rate of house prices. A number of studies find that LTV limits may slow the pace of home price increases, thereby lowering the chance of a bubble in the housing market. A 2011 IMF survey finds that LTV limits are effective in reducing price shocks and reducing feedback between asset prices and credit. Crowe et al. (2013) and the IMF (2013) examine the impact of various MPPs on housing prices during boom and bust periods and find that LTV limits have the best chance of curbing a real estate boom.

The main conclusion that emerges from cross-country studies is that LTV limits may inhibit the mechanism that creates a feedback loop between credit growth and housing price growth. However, the studies to date have several limitations. First, the time dimension in much of the international sample is limited due to data constraints. Second, studies of this type face challenges in accounting for specific country characteristics.[12] Third, most of the papers in this line of research rely on macroeconomic data or cross-country analyses, and are unable to assess distributional effects (Claessens et al., 2015). A final challenge facing aggregate studies is the task of empirically estimating the effect of LTV limits. Since LTV limits are usually implemented concurrently with other policies and macroeconomic events, it is difficult to attribute outcomes specifically to MPP tools.

[12] A prominent example is the quality of macroprudential supervision in countries where regulation is merely recommended and not binding.

The second area of research mentioned above consists of country-specific studies using microeconomic data and only one, or a few, MPPs. Studies of this kind estimate the causal effect of LTV limits by using microeconomic data to furnish information on the differences between households. Wong et al. (2011) use microeconomic data from Hong Kong and find that MPPs reduced cyclicality in the real estate market and caused a halt in the increase of housing prices. LTV limits can also affect prices by influencing expectations. Nabar and Ahuja (2011) try to estimate the effect of LTV and PTI limits on house prices expectations in Hong Kong. They find that the use of LTV limits causes a slowdown in the growth of residential property prices and in the quantity of transactions, albeit with a lag between the two. The lag is apparently due to the fact that the number of transactions is affected before property prices are affected, suggesting that LTV limits may affect property prices through expectations.

The country-specific study by Igan and Kang (2011) is the closest to the present study. Using microeconomic data from Korea (a survey of households' plans for housing tenure and expectations of housing prices), the authors find that expectations of future price increases and the probability of future purchases of residential property in Korea fell after the imposition of LTV limits. This phenomenon was more prevalent among older households and real-estate investors than among first-time home buyers. In other words, Igan and Kang's findings suggest that LTV limits lower expectations and incentives of investors, and therefore have a significant impact on housing market activity, both in terms of sales volumes and in slowing down the increase in housing prices. As such, LTV limits can help to put a brake on the dynamics that generate a housing market bubble. The present paper will also examine the effects of LTV limits on household choices in the credit and housing markets, but in contrast to the survey data used in Igan and Kang, it will look at the entire population of borrowers, and will examine actual purchasing decisions, rather than expectations about future purchases.

Using the approach of Igan and Kang (2011), the IMF (2014) analyzes housing prices in Israel using macro-economic dataset. It finds that LTV and PTI limits succeeded in slightly lowering the number of transactions, but there is no evidence that these measures had any effect on the growth rate of housing prices. In addition, the study finds that six months after LTV limits went into effect, they were already more effective than other MPP tools in leading to declines in the share of real-estate investors and in the number of new mortgages.

Yet even case studies concentrating on one particular country face some obstacles. For example, finding the causal effect of LTV limits on housing and credit markets is a difficult task, mainly because of the feedback loop between credit and housing prices. As a result, the issue of endogeneity remains a major problem in most of these studies. In particular, the endogeneity between MPP tools and real and financial developments[13]creates a downward bias in the estimates of the effects of policy measures, which can lead to the erroneous conclusion that MPP tools are not effective. In addition, some steps may be deliberately designed not to have an immediate impact, so that their influence will be felt only after some time. Finally, if MPPs are expected, their effects may be felt prior to the date of actual imposition because of expectations. Therefore, there is a need to employ more sophisticated methods using microeconomic data to solve the identification problems. Overall, the empirical evidence for the effectiveness of MPPs on the housing market is still preliminary.

This contribution of this study to the MPP literature is threefold. First, it uses unique microeconomic data to identify the effects of LTV limits on households' choices in the credit and housing markets. There is at present little evidence of the indirect effect of MPPs on consumer behavior in the credit and housing markets. Second, it explores the impact of LTV limits on parameters of the housing asset other than price, such as the size and location of the housing unit. Third, it examines the impact of LTV limits on different subpopulations, such as young adults versus older adults and first-time home buyers or home buyers seeking to upgrade their housing versus investors.

[13] For example, policy makers may limit an LTV in response to an increase in home prices.

5. The Database

The study uses microeconomic data on housing loans from all seven commercial banks in Israel between the years 2010 and 2011.[14] To our knowledge, it is the only study containing microeconomic data on mortgages that includes data from all banks in Israel. It thus includes data on the following variables for each loan: date of issue, LTV, value of the acquired property, interest rate, maturity, and others.

5.1 Data Construction

This study focuses on the period from January 1, 2010 to May 1, 2011. The goal was to focus on a limited time period centered on the October 2010 imposition of the LTV limit. One reason for this time frame is that, according to a 2014 survey of the IMF, MPPs may take up to six months to prove effective. In addition, it is better to test a time frame that is relatively free of external shocks that could influence the results, such as the outbreak of the social protests in July 2011 and the introduction of additional variable interest rate limits in April 2011. Therefore, observations subsequent to May 1, 2011 were eliminated from the dataset, leaving approximately 90,000 observations (for more information, see Appendix C).

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, housing units are identified by the combination of block, parcel, and sub-parcel numbers, enabling the specific identification of housing units that share the same street address. The physical characteristics of each housing unit such as floor area and number of rooms are recorded, allowing for the identification of the characteristics of housing units. In order to clarify details about housing units acquired with a mortgage, the study's microeconomic dataset on mortgages includes block, parcel, and sub-parcel numbers. Using the above fields, it is possible to merge these two datasets.

It is important to note that the recording of blocks and parcels in the mortgage dataset is distorted. In 36 percent of the records, this information is omitted, and in another 26% only partial information is provided. Therefore, the matching was carried

[14] This data covers about 95 percent of all mortgage loans in Israel.

out through the other remaining fields in the two datasets: price, date, and city of the purchased asset. However, these fields too are distorted in the mortgage database. In 50 percent of the entries, the price is missing; in 57 percent, the date is missing; and in 14 percent, the city is missing. Appendix D shows the process of how these two files were merged under these constraints.

The matching process, described in detail in Section 6, is not parametric, and so it is highly sensitive to extreme observations. Therefore, I limited the average age of borrowers to 20 to 80 years, and omitted the 1 percent of values at the extreme upper and lower ends of the distribution of total income per household, home price, and loan size variables.

In summary, the original mortgage dataset was merged with 27,324 observations from the CARMAN dataset, amounting to approximately one-third of the observations in the mortgage dataset from January 2010 to May 2011. There are 11,224 observations after October 2010 (when the LTV limit was imposed) in the "treatment group," and 16,100 observations before that date in the "control group." 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 E), a Kolmogorov–Smirnov test of equality distributions showed no significant difference between the treatment and control groups before and after the merger.

5.2. Summary Statistics

The dataset was divided into two periods: before and after the impostion of the LTV limit in October 2010. Table 1 shows descriptive statistics of mortgage contracts, borrower characteristics, and home purchase transactions before and after the impostion of the 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[15]. Table 1 shows a decline in the quality of neighborhoods after the LTV limit. Hence households moved farther away from the business center to lower-quality neighborhoods. The econometric challenge in this paper will be to attribute these changes in housing preferences to the LTV limit.

Table 1: Summary Statistics

Summary Statistics

		Before the LTV Limit (N=16,100)		After the LTV Limit (n=11,224)		Difference	
Dataset	Variable	Mean	S.D.	Mean	S.D.	Coef	S.E.
Mortgage Contracts	Loan amount (NIS thousands)	554	346	565	348	11**	4.3
	Average 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
Borrower Characteristics	Total income (NIS 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 Purchase Transactions	Nominal home prices (NIS thousands)	1,078	601	1,106	614	28***	7.4
	Real home prices (NIS thousands)	1,026	572	968	537	-58***	6.8
	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
	Distance from Tel Aviv (km)	45.2	45.7	47.8	45.8	2.6***	0.5
	Quality 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 (Carmen Database) are from the Israel Tax Authority.
Note: Real home prices was inflated by the monthly change in the Index of Home Prices.

6. Estimation Method

The imposition of an LTV limit is not an exogenous decision. Countries showing signs of a rapid rise in housing prices are more likely to impose restrictions. In addition, increases in the growth rate of housing prices and home loans tend to move together in the same direction and to influence each other in a two-way feedback loop. In other words, rising housing prices necessitate restrictions, which in turn affect

[15] For more details on how the CBS calculates this index, see: http://www.cbs.gov.il/publications13/1530/pdf/intro02_e.pdf

house prices. Thus, the estimation suffers from the problem of endogeneity, and therefore does not necessarily indicate causality. It is therefore necessary to look at the decisions of households at the individual level to more accurately examine the effects of LTV limits on preferences in the housing and credit markets.

This paper focuses on the policy's effect on the subset of borrowers constrained by the LTV limit. However, the treatment status is observed only before the policy because after the policy shock the borrower could have taken LTV>60 percent and paid a higher interest rate, or the borrower could have chosen LTV<60 percent and bought a different asset. Hence, this paper uses two ways of identifying affected borrowers.

6.1 Matching Method: Cross-Period 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 first to find and compare households with similar characteristics before and after the imposition of the LTV limit and then to compare their choices in the housing and credit markets.

Let Yi 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., $T_i = 1$. In the case of households that borrowed before October 2010, $T_i = 0$. The purpose is to measure the impact of the average of LTV limits on housing and mortgage choices. This impact is denoted by Y_1. This value is the expected difference between the choices of households under the limits and the choices made without imposing the limit. For example,

$$\mathrm{Y}_1 = \mathrm{E}(Y_{i1}|T_i = 1) - \mathrm{E}(Y_{i0}|T_i = 1)$$

Our dataset shows only the choices of the household after the limits were imposed $(Y_{i1}|T_i = 1)$ or the choices of the household before the limits were imposed $(Y_{i0}|T_i = 0)$ but the value $(Y_{i0}|T_i = 1)$ is not available (counterfactual). If, for example, only the wealthiest households buy homes after the imposition of restrictions, the comparison between the choices of households before and after the imposition of the restrictions is problematic.

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.

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: $\mathrm{ATT}=\mathrm{E}(\mathrm{Y}_1-\mathrm{Y}_0|\mathrm{T}=1,\mathrm{X})$. 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; Heckman et al., 1998).[16,17] 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

[16] 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.

[17] 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 (2011) 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.

implements a bias-correction component to the outcome variables. (For more details, see Abadie and Imbens, 2011.)

In our case, the treatment group is defined as all the households that took mortgages after the announcement of the LTV limits, and the control group is the households in the preceding period. The matching was done according to the overall level of the household's income and the average age per household. The outcome variables are: real home price (in NIS), nominal 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 quality of the neighborhood (scale of 1 to 20).

The matching process finds a match for each observation in the treatment group (11,224 observations) with the nearest observation in the control group. Since matching was applied with replacement, 7,903 observations from the control group were matched with the treatment 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 E examines the differences between the borrowers' characteristics in the control group that were matched to the treatment group (7,903 borrowers) compared to the borrowers in the control group (16,100). 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 F 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 there results.

Table 2 shows the results of a test designed to examine similarity between the treated and control groups. In general, those who took mortgages before the imposition of the LTV limit were statistically significantly different in age and income (according to a means test) from all those who took a mortgage after that point. The matching process led to the conclusion that the two groups did not differ significantly from one another by age or income.

Table 2: Sample Statistics before and after the Matching Process

	Before Matching			After Matching		
	Treated	Control	P-Value	Treated	Control	P-Value
Monthly Income (NIS)	14,764	14,168	0.00	14,764	14,759	0.51
Age	41.47	41.68	0.04	41.47	41.47	0.49

Note: "Treated" -households borrowing after the announcement of the LTV limit (26.10.2010 - 1.5.2011).

6.2 Difference-in-Differences Matching: Within Periods

One might wonder if other time-varying effects, such as an increase in housing prices or macroeconomic events, could have affected the results. To tackle this concern, a difference-in-differences matching estimator approach is used, incorporating observable household characteristics and accounting for unobservable time-varying macroeconomic effects. To minimize concerns about selection, households are matched based on observable characteristics, age, and income, as explained in the matching method section above. This matching is meant to ensure that the comparison is made between otherwise similar households, with the one salient difference between the two groups being the sensitivity to the LTV limit. While the impact of macroeconomic variables applies to both groups, only one group is affected by the LTV limit.

As mentioned above, according to the October 2010 LTV limit, the banks were required to increase capital provisions for mortgages exceeding NIS 800,000 and with an LTV of greater than 60 percent. This study thus examine two groups: those that borrowed NIS 600,000 to 700,000, just below the threshold (the untreated group), and those that borrowed just above the threshold, from NIS 900,000 to 1,000,000 (the treatment group). The reason for choosing those levels of loan amounts is that there is a low probability of overlap between the two groups, but at the same time those groups are not very different in observable characteristics (for matching purposes).

The difference-in-differences estimation will examine the differences in the averages of the outcome variables between the treated and the untreated groups, between before and after the limit. Then, the matching process, described extensively in the estimation method, will be compared between the treatment group and untreated group, and from the untreated population, only the observations with the best matching by observable characteristics in the treatment group will be selected, and will be called the control group. The interpretation of the outcome variables will be based on the post-treatment outcome gaps between the two groups (treated versus control groups). So the ATT now is calculated as: ATT= $(\text{after-before})_{\text{treated}}$ - $(\text{after-before})_{\text{control}}$.

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).

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.

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.

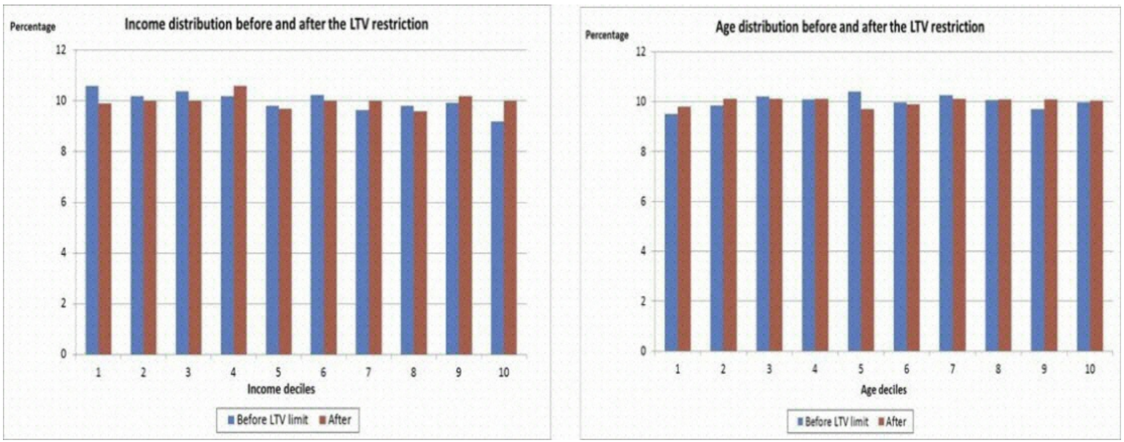
7. Results

7.1 Test for Credit Rationing

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.

I examine the distribution of borrower age and income before and after the imposition of the LTV limit (Figure 3). A Kolmogorov–Smirnov test shows that there was no significant change in the distribution of borrower age and income. Thus, according to this test, there is no sign of credit rationing.

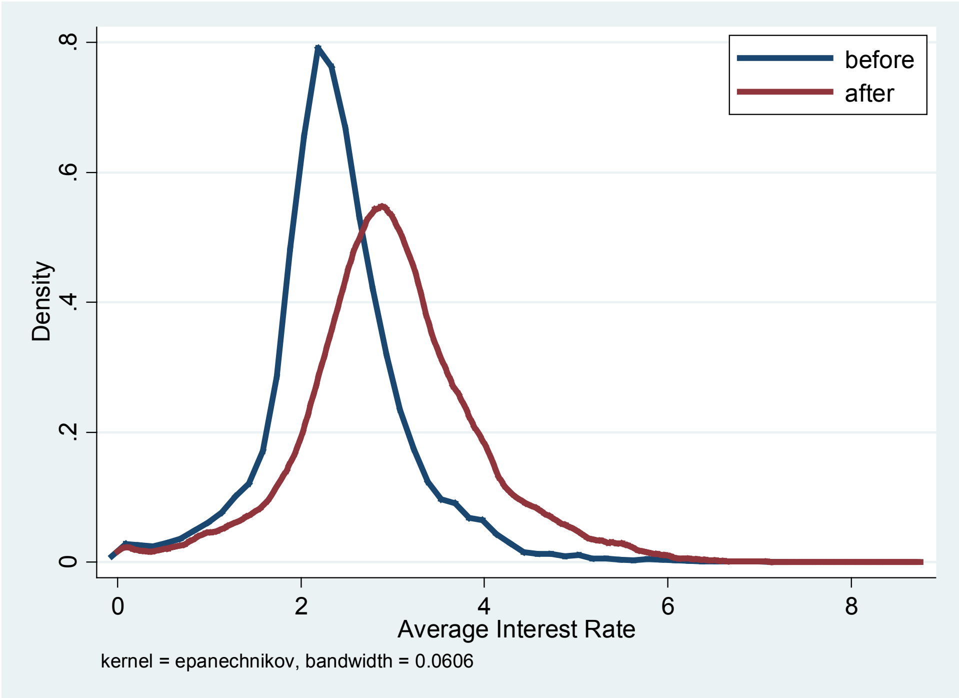
Figure 3: Change in Distribution of Borrower Characteristics



7.2 The Effectiveness of the LTV Limit

The 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.

Figure 4: Change in the Average Interest Rate for Borrowers with LTV>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 identical 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[18] 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).

[18] 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.

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

Note: Heteroskedasticity-consistent standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.
Spread is the interest rate over the prime. ATT is the Abadie-Imbens bias corrected average treated effect matching estimator.
Treatment: Those who borrow above the 60 percent LTV threshold. Borrowers were matched, after the LTV limit, by income, age, bank and duration of loan.

7.3 The Effect of the LTV Limit on Housing Characteristics (Cross-Period Matching)

The LTV limit, which increases the interest rate for loans with LTV higher than 60 percent, induced risky borrowers to reduce their leverage, as seen in Figure 2. I therefore examine the changes in their housing choices after the introduction of the LTV limit. Using the matching process, I can match households with similar age and income before and after the LTV limit, and examine the differences in their choices in the credit and housing markets.

Table 5 presents the ATT parameter of the matching process. The LTV limit had significant effects on the borrowers: after the imposition of the LTV limit, the same household (in terms of age and income) bought assets that were 8 percent (significantly) cheaper in real terms, 8.4 percent farther away from the center of Tel Aviv (around 4 kilometers[19]), and 9 percent lower in quality according to the neighborhood's socioeconomic level).[20] Those borrowers also reduced the size of their housing unit by 1 percent.

[19] 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 Appendix G.
[20] Appendix H presents the LTV ratio of different districts in Israel. The LTV ratio is quite similar in different districts.

Table 5: Effect of LTV Limit on Housing Market: Matching Procedure

Dep. Variable:	Nominal Home Prices (NIS)	Real Home Prices (NIS)	Size (sq.m.)	Rooms	Distance from Tel Aviv (KM)	Neighborhood Ranking
ATT	**1,397**	**-83,401*****	**-1.52**	**-0.04*****	**3.8*****	**-1.8*****
	(8,947)	(8,193)	(1.1)	(0.01)	(0.7)	(0.4)
ATT (%)	**0.1%**	**-8.1%**	**-1.6%**	**-1.0%**	**8.4%**	**-9.1%**

Note: Heteroskedasticity-consistent standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively. Number of observations: 11,224. ATT is the Abadie-Imbens bias corrected average treated effect matching estimator. Treated is defined as households who borrow after the LTV limit (October 2010).

To understand the magnitude of these changes, Appendix G 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, 3.8 km (8.4 percent) farther away from Tel Aviv, to significantly lower-quality neighborhoods.[21]

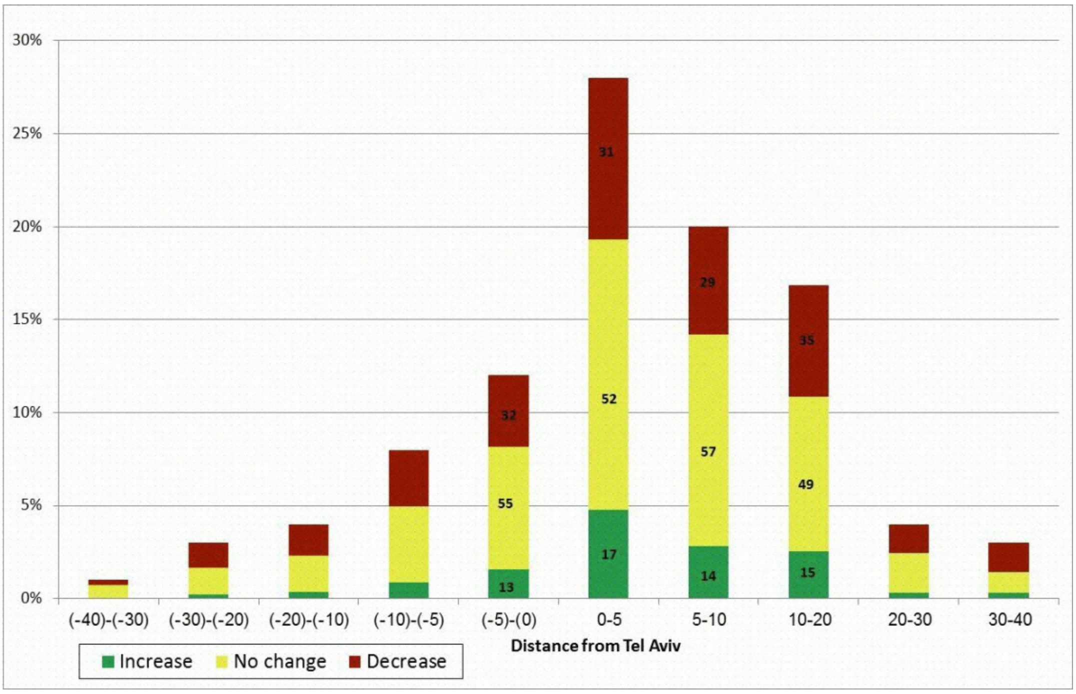
This paper assumes that the housing supply is inflexible, at least in the short term. It also examines only the choices in the housing market of the affected borrowers, which is a subgroup of the buyers with a mortgage (which in turn is a subgroup of home buyers). Hence, questions such as, who bought 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 quality of their neighborhood. There are, of course, very high-quality suburbs of Tel Aviv. Figure 5 shows the distribution of changes in neighborhoods quality by distance from Tel Aviv. The changes are divided into three groups: upgrade (green), downgrade (red), and no change in quality of

[21] In fact, at the same time as the imposition of the LTV limit, there was an increase in demand for housing units in the periphery of Israel. As shown in Appendix J, after the LTV limit, housing prices increased more in the periphery than in the center of Israel.

neighborhood (yellow). First, although the average move was of 3.8 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-quality neighborhoods than to higher-quality areas (a statistically significant difference).

Figure 5: Distribution of Change in Neighborhoods' Socioeconomic Level, by Distance from Tel Aviv



Next, I examine the differential effect of the LTV limit on sub-segments of the population, particularly young and low-income borrowers.[22] This test sheds light on the question of whether LTV limits make it difficult for households in need of credit to purchase property.

To this end, the sample was divided into two groups according to the average age of borrowers: young[23]—up to the age of 40—versus older adults. The matching process was carried out for each group individually. The results are shown in Table 6. Both groups were affected by the LTV limit, yet older adults were more affected than

[22] Appendix I presents the percentage of borrowers to whom the LTV limit applies, by type of borrower, before the LTV limit (October 2010).

[23] 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.

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.

Table 6: Effect of LTV Limit on Housing Market: Matching Procedure by Age Group

Average			Observations	Nominal Home Prices (NIS)	Real Home Prices (NIS)	Size (sq.m.)	Rooms	Distance from Tel Aviv (km)	Quality of Neighborhoods
average age<=40	Before Matching	Untreated	8,207	974,198	926,691	92.16	3.86	44.15	9.50
		Treated	5,848	1,015,132	888,101	91.99	3.84	46.21	8.40
		Difference		40,933*** (8,481)	-38,590*** (7,794)	-0.16 (1.33)	-0.01 (0.01)	2.06*** (0.74)	-1.1*** (0.35)
	After Matching (ATT)	Control	3,916	23,119**	-55,229***	0.73	-0.01	3.0***	-1.3***
		Treated	5,848	[10,186]	[9,314]	[1.82]	[0.02]	[0.98]	[0.41]
		Change (%)		2.3%**	-5.8%***	1%	0%	7%***	-13%***
average age>40	Before Matching	Untreated	7,892	1,186,375	1,129,430	102.85	4.12	46.31	10.70
		Treated	5,376	1,203,951	1,053,544	102.19	4.11	49.61	8.90
		Difference		17,576 (12,169)	-75,887*** (11,202)	-0.66 (0.88)	-0.01 (0.02)	3.3*** (0.85)	1.8*** (0.41)
	After Matching (ATT)	Control	3,768	-11,458	-103,060***	-2.8**	-0.06***	4.9***	-2.0***
		Treated	5,376	[14,896]	[13,588]	[1.12]	[0.02]	[1.1]	[0.57]
		Change (%)		-1%	-9%***	-3%**	-1%***	11%***	-18%***

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

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.

The results are shown in Table 7. 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.[24]

Table 7: Effect of LTV Limit on Housing Market: Matching Procedure by Buyer Type

Average			Observations	Nominal Home Prices (NIS)	Real Home Prices (NIS)	Size (sq.m.)	Rooms	Distance from Tel Aviv (km)	Quality of Neighborhoods
First-time home buyer	Before Matching	Untreated	6,636	874,824	918,439	88.01	3.72	43.99	9.45
		Treated	4,272	840,928	960,587	86.63	3.72	45.46	9.01
		Difference		-33,896*** (8,284)	42,148*** (8,998)	-1.38 (0.9)	0.00 (0.01)	1.47* (0.85)	-0.44* (0.27)
	After Matching (ATT)	Control	3,081	13,337	-60,179***	-2.28*	-0.04*	1.85***	-0.6**
		Treated	4,272	[10,928]	[9,984]	[1.23]	[0.02]	[1.1]	[0.3]
		Change (%)		1%	-8%***	-3%*	-1%*	4%***	-6%**
Upgraders	Before Matching	Untreated	6,492	1,260,878	1,199,697	111.12	4.38	44.10	9.99
		Treated	4,656	1,293,354	1,131,047	110.96	4.38	47.40	9.11
		Difference		32,476*** (11,824)	-68,650*** (10,881)	-0.16 (0.9)	0.00 (0.02)	3.3*** (0.85)	-0.88*** (0.29)
	After Matching (ATT)	Control	1,712	5,344	-93,021***	-1.43*	-0.02	3.9***	-1.1***
		Treated	2,236	[13,311]	[12,165]	[1.1]	[0.02]	[1.1]	[0.3]
		Change (%)		0%	-8%***	-1%*	0%	9%***	-11%***
Investors	Before Matching	Untreated	2,856	1,040,101	989,110	88.23	3.70	50.34	9.59
		Treated	2,236	998,116	873,748	87.88	3.61	53.42	8.41
		Difference		-41,985** (20,921)	-115,362**** (19,259)	0.35 (0.9)	0.09*** (0.03)	3.08** (1.5)	-1.18*** (0.34)
	After Matching (ATT)	Control	3,158	-49,656**	-122,680***	-0.13***	-0.08*	5.57***	-1.5***
		Treated	4,656	[25,014]	[22,940]	[0.04]	[0.04]	[1.9]	[0.41]
		Change (%)		-5%**	-12%***	0%***	-2%*	9%***	-15%***

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

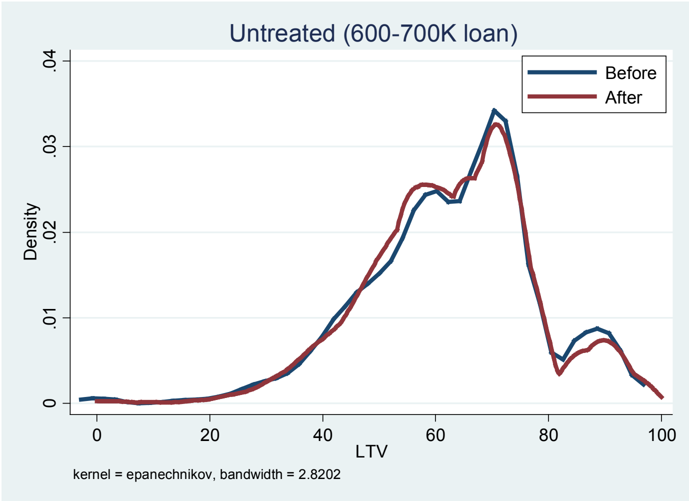
7.4 Difference-in-Differences Matching: Within-Period Matching

It is possible that borrowers choose different assets not because of the LTV limit but because of other time-varying macroeconomic events. I will use the fact that the LTV limit applies only to those who borrowed more than NIS 800,000 and examine two groups: the treatment group (those who borrowed between NIS 900,000 and NIS 1,000,000) and a control group (those who borrowed NIS 600,000–700,000). The same macroeconomic conditions apply to both groups, but the LTV limit applies only to the treated group.

Figure 6 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.

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

Figure 6: LTV Distribution before and after Imposition of the LTV Limit: Treatment and Control Groups



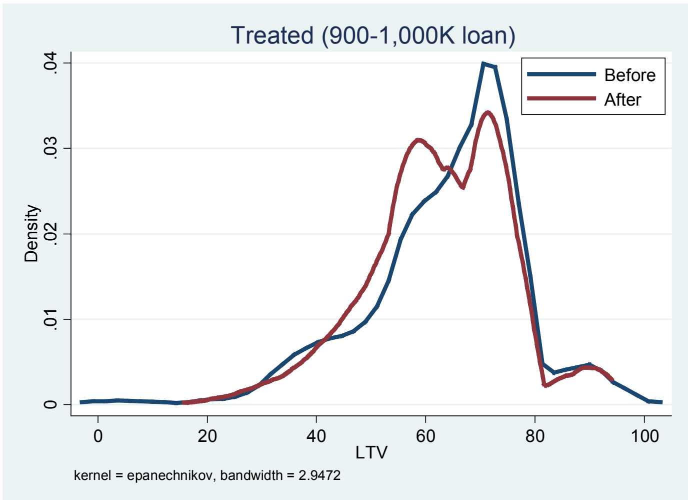


Table 9 compares the average real price of the housing units between the treatment group and the untreated group before and after the imposition of the LTV limit. The difference-in-differences estimation shows that there was a significant decline of NIS 43,000 in real house prices after the imposition of the LTV limit. The ATT parameter (Abadie-Imbens), which takes into account differences in observable characteristics (including distance between their characteristics and not only average values), obtains an even larger, significant gap of approximately NIS 68,000 between the treatment and control groups.

Table 9: Effect of LTV Limit on Real Home Prices (NIS): Difference-in-Differences Matching Estimations

	Before	After	Difference
Treated	1,456,884*** (15,896)	1,382,296*** (15,520)	**-74,728***** (22,242)
Untreated	1,210,884*** (15,296)	1,179,178*** (20,044)	**-31,706** (24,855)
Difference in Mean	246,000*** (22,592)	203,118*** (24,577)	-43,022* (23,714)
DID Matching (by observable characteristics)			**-67,789*** (36,135)

Note: Heteroskedasticity-consistent standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively. treated borrowers are defined as those that borrowed 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 3,462 untreated borrowers. Control borrowers are a subset of the untreated borrowers selected as the closest match to the treated borrowers based on a set of borrower characteristics: Age and income. There are 1,498 borrowers in the control group. ATT is the Abadie-Imbens bias corrected average treated effect matching estimator.

Table 10 shows the change in distance from Tel Aviv (km) of purchased properties among the three groups (treated, untreated and control) before and after the imposition of the LTV limit. The results indicate a clear and significant distancing of 3.9 km from Tel Aviv in the treated group compared with the untreated group (very close to the result in the cross-period matching method). Using the matching estimator, I obtain an even larger difference for the treated group—4.3 km farther from the center after the LTV limit.

Table 10: Effect of LTV Limit on Distance from Tel Aviv: Difference-in-Differences Matching Estimations

	Before	After	Difference
Treated	28.3*** (1.15)	31.5*** (1.54)	**3.2**** (1.9)
Untreated	41.2*** (1.63)	40.5*** (1.41)	**-0.7** (1.2)
Difference in Mean	-12.9*** (1.6)	-9*** (2.1)	3.9*** (1.5)
DID Matching (by observable characteristics)			**4.3***** (1.7)

Note: Heteroskedasticity-consistent standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively. treated borrowers are defined as those that borrowed 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 3,462 untreated borrowers. Control borrowers are a subset of the untreated borrowers selected as the closest match to the treated borrowers based on a set of borrower characteristics: Age and income. There are 1,498 borrowers in the control group. ATT is the Abadie-Imbens bias corrected average treated effect matching estimator.

Table 11 shows the change in the quality of neighborhoods, in a socioeconomic index rating neighborhoods on a scale of 1 to 20, for the three groups (treated, untreated, and control) before and after the imposition of the LTV limit. The difference-in-differences result indicates a decline in the level of the quality of the neighborhoods after the LTV limit in the treated group versus the untreated group. Using the matching estimator, I obtain a larger decline of 2.2 points (a 17 percent decline) in the quality of neighborhoods. Hence, the treatment group moved to significantly lower-quality neighborhoods.

Table 11: Effect of the LTV Limit on the Quality of Neighborhoods: Difference-in-Differences Matching Estimation

	Before	After	Difference
Treated	12.7*** (1.1)	10.3*** (1.8)	**-2.4***** (0.7)
Untreated	10.6*** (1.51)	10.1*** (1.5)	**-0.5** (0.5)
Difference in Mean	2.1** (0.9)	0.2 (0.8)	-1.9** (0.8)
DID Matching (by observable characteristics)			**-2.2***** (0.8)

Note: Heteroskedasticity-consistent standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively. treated borrowers are defined as those that borrowed 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 3,462 untreated borrowers. Control borrowers are a subset of the untreated borrowers selected as the closest match to the treated borrowers based on a set of borrower characteristics: Age and income. There are 1,498 borrowers in the control group. ATT is the Abadie-Imbens bias corrected average treated effect matching estimator.

8. Extensions and Robustness Tests

8.1 Difference-in-Differences Using Control Variables: Hedonic Approach

Instead of using a simple difference-in-differences method that examines the change in the average value of the outcome variables, this section focuses on the price component of the housing unit and uses a more advanced difference-in-differences method that adds control variables, using hedonic regression to compare home price dynamics before and after the imposition of the LTV limit between treated and untreated groups (as before). In particular, I estimate the following hedonic equation:

$$\ln(p_{ilt}) = \alpha + \beta' x_i + \delta_l + \theta_t + \gamma Treatment_l * \theta_t + \varepsilon_{ilt}$$

Where p is the price of property i in locality statistical area l sold on date t, and x is a vector of property characteristics: number of rooms, log area (square meters), and log age (of the housing unit). δ is a locality statistical area fixed effect, θ is a year fixed effect, and ϵ_{ilt} is a well-behaved error term clustered at the locality statistical area level.

Table 12 shows the results. In the full sample, the price decreases by around 4 percent after the LTV limit, for the treated borrowers in comparison with untreated ones. If I divide the sample into 2 subgroups, by age and by buyer type, I can see that the borrowers above the age of 40 were more affected by the LTV limit, and the "investors" were more affected than first-time home buyers (same results as before).

Table 12: Effect of the LTV Limit on Real Home Prices: Hedonic Approach

	Dependent variable: log price				
	All sample	Age<40	Age≥40	FOB	Investors
	(1)	(2)	(3)	(4)	(5)
After x Treatment	**-0.038*** **	**-0.031*** **	**-0.046*** **	**-0.025*** **	**-0.061** **
	(0.01)	(0.01)	(0.013)	(0.015)	(0.03)
Rooms	0.15***	0.1***	0.12***	0.09***	0.08***
	(0.006)	(0.007)	(0.009)	(0.008)	(0.009)
Log (area)	0.653***	0.62***	0.60***	0.61***	0.68***
	(0.01)	(0.013)	(0.017)	(0.02)	(0.023)
Log (age)	-0.028***	-0.03***	-0.035***	-0.035***	-0.065***
	(0.006)	(0.006)	(0.009)	(0.008)	(0.018)
Quality of neighborhoods	Yes	Yes	Yes	Yes	Yes
Year Fes	Yes	Yes	Yes	Yes	Yes
Observations	3,545	1,918	1,627	1438	493
R-squared	0.81	0.8	0.8	0.79	0.77

Note: *** p<0.01, ** p<0.05, *p<0.1, Standard errors are reported in parentheses. FOB - first home buyers

8.2 Adding Explanatory Variables to the Matching Method

It is possible that age and income are not the only explanatory variables that can explain changes in housing preferences. Other variables that can influence households' decisions when purchasing a residential property include the size of the household, i.e., the number of family members, or the previous place of residence, each of which can serve as an indicator of residential preferences and socioeconomic level.

Data from the Israel Tax Authority contains an anonymous random sample of 10 percent of all employees and their spouses. This file contains information on wages and main demographic characteristics. Two variables from this file are used: residential district two years before the acquisition transaction and number of children. Linking this employee file with the mortgage data resulted in 1,563 identifiable records, representing approximately 6 percent of the number of observations included in the dataset. Appendix E examines whether the observations matched to the employee file are similar in their characteristics to the overall database, and there appear to be no significant differences between groups according to the Kolmogorov–Smirnov test.

Table 13 presents the results of the estimations for this subsample. The matched sample is much more limited than the original one, but it allows for a closer match of the control group to the treatment group. The results in Table 13 indicate that there is no significant change in the results from those presented earlier.

Table 13: Effect of LTV Limit on Housing and Credit Markets (Adding Explanatory Variables, Matching Procedure)

ATT	Observations	Nominal Home Prices (NIS)	Deflated HHPI (NIS)	Size (sq.m.)	Rooms	Distance from Tel Aviv (km)	Quality of Neighborhoods
Control	569	8,530	-79,476***	-1.3	-0.03	3.36*	-1.9**
Treated	718	[33,788]	[31,296]	[2.68]	[0.06]	[1.9]	[0.8]

Note: Heteroskedasticity-consistent standard errors are in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively. ATT is the Abadie-Imbens bias corrected average treated effect matching estimator. Treated- housholds who borrow after the LTV limit (October 2010).

9. 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-quality housing, farther away from the center, and into neighborhoods with lower socioeconomic ratings.

The main purpose of an MPP is to stabilize the banking system. A stable financial system is a public good from which the entire population derives utility. However, this paper finds that only a subset of the population bears the cost of this public good: the borrowers (home buyers), especially risky (high LTV) borrowers. Is this the optimal way to fund bank stability? Understanding the market participants' response to LTV limits is crucial for the development of appropriate policy tools in the future.

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Appendixes

Appendix A

Examples of LTV Limits as Macroprudential Tools in Different Countries: Percentage

Country	LTV limit	Average LTV	Year
Austria	80	85	2010
Belgium	75	60	2010
Denmark	80	73	2012
Finland	90	87	2012
France	80	75	2011
Germany	80	70	2012
Italy	80	59	2012
Korea	50	51	2009
Netherlands	100	101	2012
Sweden	85	67	2012
Israel	60	52	2010

Source: Shim et al. (2013), Crowe et al. (2011), IMF (2011), European Central Bank (2015), Housing Finance Network.

Appendix B

MPPs Used in Israel, in Chronological Order

MPP	Date of Press Release	Macroprudential Tool
LTV1	May 24, 2010	A provision at a minimum rate of 0.75% for housing loans with an LTV higher than 60%
LTV2	October 25, 2010	Those loans with an LTV greater then 60% with a variable interest rate on at least 25% of the loan and weighted at 35—75% of weighted capital must provide a 100% allocation (this does not apply to housing loans less then NIS 800,000)
Variable Interest Rate	April 27, 2011	The portion of the housing loan at a variable interest rate (variable within up to 5 years) will be limited to one-third of the total loan
LTV3	November 1, 2012	LTV will be limited as follows: 75% for a first housing unit; 50% for investors; 70% for improvers
Capital Adequacy Ratio	February 19, 2013	For the calculation of capital adequacy ratios, housing loans where the LTV ratio is up to 45% will be weighted at 35% (unchanged from previous weighting). Housing loans with an LTV ratio of between 45% and 60% will be weighted at 50% and housing loans with an LTV ratio of 60—75% will be weighted at 75%
PTI + Duration	August 21, 2013	The PTI ratio was limited to 50% of income. Housing loans where the monthly repayment is over 40% will be weighted at 100 percent for the purpose of calculating the capital adequacy ratio The portion of the loan at variable rate interest was limited to two-thirds of the loan for all loan periods The loan period was limited to 30 years

Appendix C

Data Construction of Housing Loans: Omitted Observations

Mortgages have been omitted if the monthly PTI exceeded 100 percent or was equal to 0 percent (1.5 percent of total observations), or if the purpose of the loan was not for the purchase of a dwelling (26 percent of total observations). In addition, mortgages have been omitted if there were over two borrowers (1.5 percent of total observations), because this study focuses on loans taken by households. The gross database includes information on the total income and the age of the borrowers. If two parties took one loan, their average age was calculated. Also omitted were loans to borrowers aged under 20 or over 80 (0.2 percent of total observations). The total remaining is 90,217 observations from January 2010 to May 2011.

Appendix D

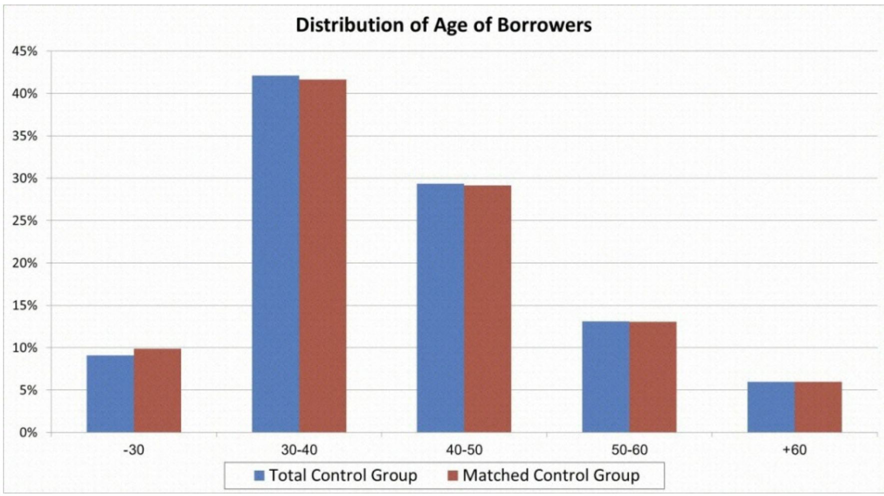
Merging the Mortgages Database to the Real Estate Database (CARMAN)

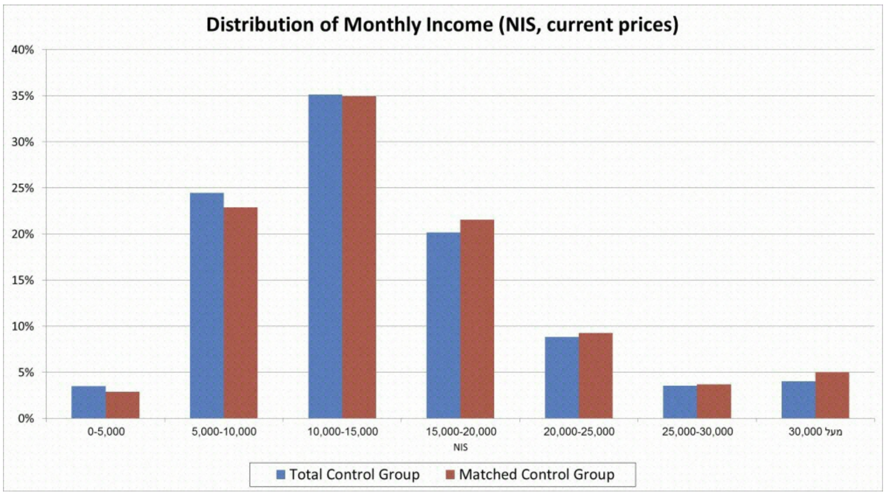
The mortgages file is merged with the CARMAN file through the following fields common to the two files: date of the transaction, transaction price, city of property, block and parcel numbers. As mentioned in the text, the recording of the block and parcel numbers in the mortgages file is distorted, with 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).

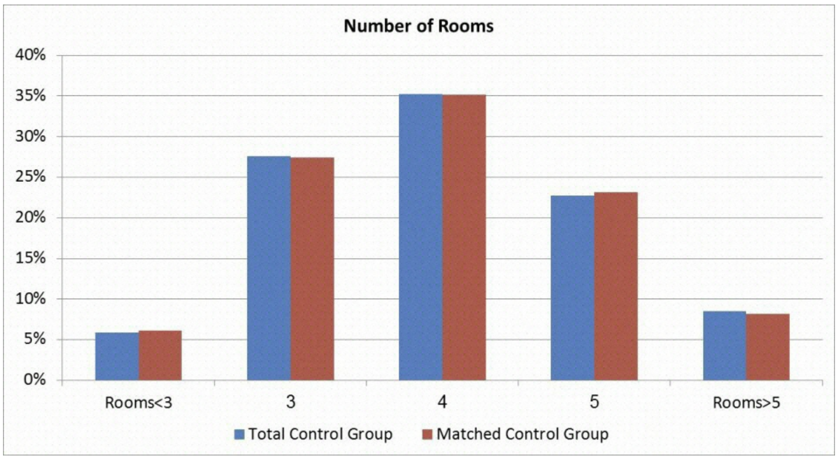
Steps	Exact City	Exact Price	Exact Date	Single Match	,Block Parcel and Subparcel	Range	Number of Identified Observations	Comments
1	+	+	+	+	-	-	65,000	
2	+	+	+	-	+	-	2,000	
3	+	+	-	+	-	-	500	date missing
4	+	+	-	-	+	-	4,700	date missing
5	+	+	-	-	+	+/- 20 days	2,290	
6	+	+	-	+	-	+/- 5 days	160	
7	+	-	+	-	+	+/- 100K NIS in house prices	14,668	
8	+	-	-	-	-	+/- 1,000 NIS in house prices	400	
9	-	+	+	+	-	-	40	
10	-	+	+	-	+	-	23	

Appendix E

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







Appendix F

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

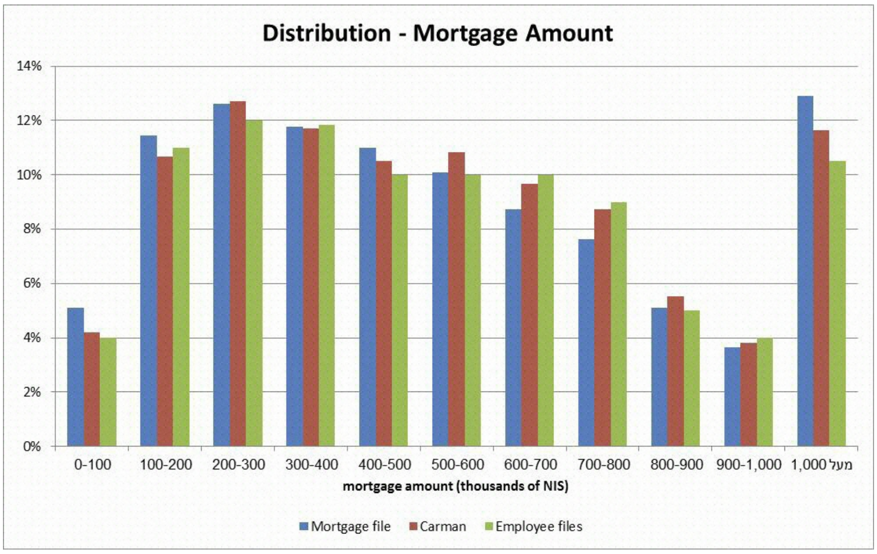
After merging the mortgages file with the CARMAN file, which contains information on the characteristics of the property, the question of whether the observations of the merged file indeed reflect the observations in the CARMAN file must be considered. One of the advantages of combining the mortgages 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

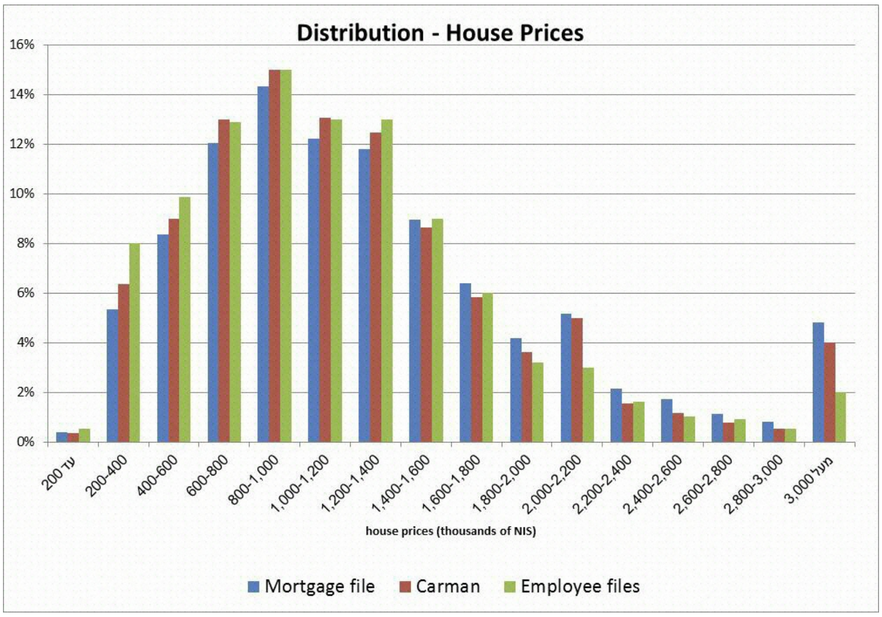
CARMAN file versus in the mortgages file and in the employee file between early 2010 and May 2011. The differences between the two samples can also be attributed to the fact that the mortgages file contains data only about those who have taken mortgages, which does not necessarily represent the entire population of home buyers.

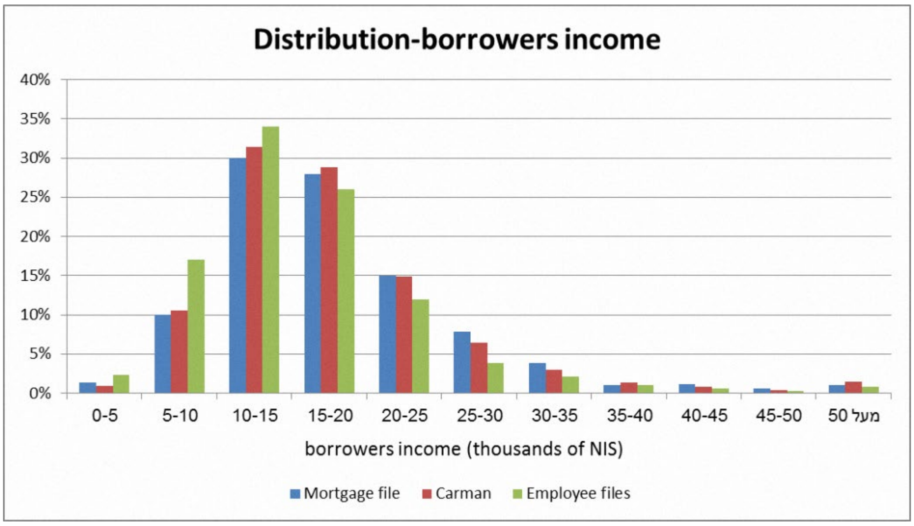
Cause of Purchase	Carman	Mortgages file	employee files
First Home Buyers	34%	42%	43%
Upgraders	37%	40%	42%
Investors	29%	18%	15%

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.



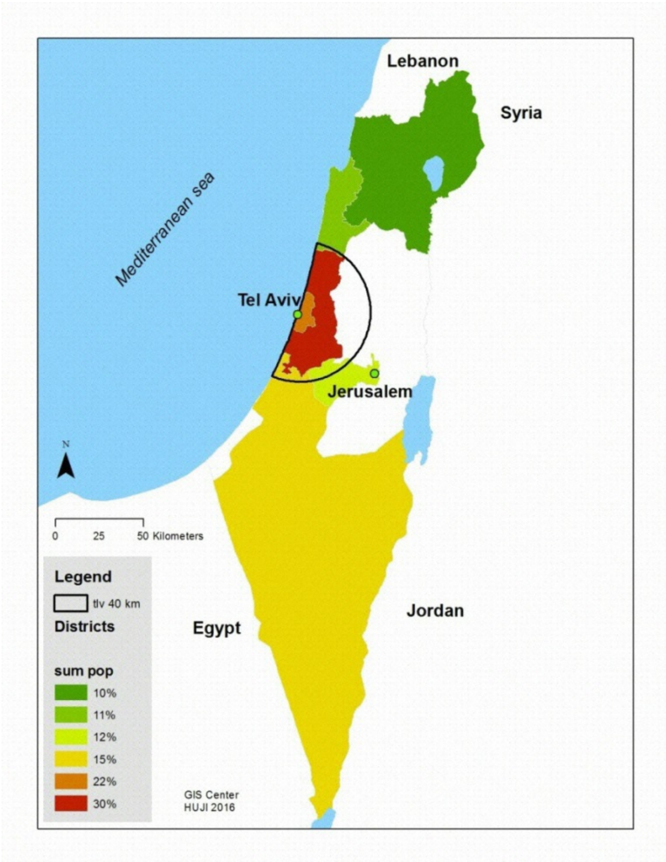




Appendix G

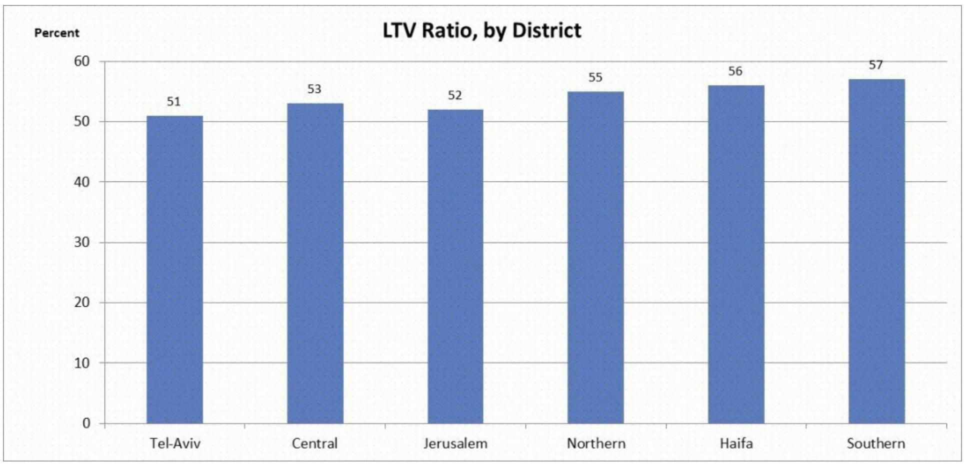
Israeli Population Distribution Map[25]

[25] Population relevant to the sample, from the Israeli Central Bureau of Statistics.



Appendix H

Changes between the Periphery and the Center of Israel



Appendix I

Percentage of Borrowers to whom the LTV limit applies, by Type of Borrower, before the LTV limit (October 2010)

Before the LTV limit	All Sample	First-time home buyer	Upgraders	Investors	average age<=40	average age>40
LTV>60%	52%	62%	45%	53%	60%	44%
Loan Amount>800K NIS	30%	23%	35%	32%	27%	33%
Variable interest rate portion>25%	97.8%	97.6%	98.0%	98.4%	97.5%	98.0%
Sum of the 3 conditions	19%	18%	20%	19%	18%	20%

Appendix J

Changes in Housing Prices (Hedonic Index), by Distance from Tel Aviv, before and after the LTV Limit