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The Effect of Credit Constraints on Housing Choices: The Case of LTV limit[1]

Nitzan Tzur-Ilan[*]

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[*] Nitzan Tzur-Ilan, Bank of Israel, Research Department and Hebrew University, Nitzan.Tzur@boi.org.il ,Phone: 972-6552562

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חטיבת המחקר, בנק ישראל ת"ד 780 ירושלים 91007
Research Department, Bank of Israel. POB 780, 91007 Jerusalem, Israel

The Effect of Credit Constraints on Housing Choices: The Case of LTV limit

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Abstract

This paper examines the effects of a Loan-to-Value (LTV) limit on households' choices in the credit and housing markets. Using a large and novel micro database from Israel, including rich information on loans, borrowers and acquired assets, and using matching techniques, I find that the LTV limit had an effect on the mortgage contract terms (higher interest rates), but did not lead to credit rationing (no segment of the population is excluded from the market). The LTV limit induced borrowers to buy cheaper assets and to move farther from high demand areas to lower graded neighborhoods. The conclusion is that the LTV limit, the most common macroprudential policy tool, has an impact not only from a financial stability perspective, by reducing the leverage of households, but also affected their choices in the housing market.

השפעת מגבלות אשראי על בחירות הלווים בשוק הדיור: המקרה של המגבלה על שיעור המימון

ניצן צור-אילן

תקציר

עבודה זו בוחנת את השפעותיה של מגבלת שיעור המימון (LTV) על הבחירות שעושים משקי הבית בשוקי האשראי והדיור בישראל. בעזרת מסד נתונים פרטני ייחודי, המכיל מידע על לווי המשכנתאות, על מאפייני המשכנתאות ועל הדירות שנרכשו באמצעותן, ובעזרת טכניקות התאמה (Matching), נמצא כי מגבלת ה-LTV השפיעה על תנאי ההלוואה אך היא לא הובילה לקיצוב אשראי (אף מגזר באוכלוסייה לא הוחרג מן השוק). כמו כן נמצא כי למגבלת ה-LTV היתה השפעה על הבחירות של הלווים בשוק הדיור: משקי הבית שהושפעו מהמגבלה בחרו בנכסי דיור זולים יותר (במונחים ריאליים), רחוקים יותר מהמרכז, וממוקמים בשכונות בעלות דירוג סוציו-אקונומי נמוך יותר. המסקנה היא כי מגבלת ה-LTV, כלי המדיניות המקרו-יציבותי הנפוץ ביותר, משפיעה על הלווים לא רק מנקודת המבט של יציבות פיננסית, על ידי הפחתת המינוף של משקי בית, אלא משפיעה גם על הבחירות שלהם בשוק הדיור.

1. Introduction

The 2008 global financial crisis led many to recognize that not only is there a fundamental lack of understanding of the systemic risks in the financial system, the policies needed to prevent the realization of these risks are lacking. Macroprudential policy (MPP) is preventative; 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 (BIS, 2010).

MPPs related to the housing market are a major MPP tool used in several countries.[1] The most common MPP targeting the housing market is the imposition of a Loan-To-Value (LTV) limit on housing loans (Crowe *et al*., 2011a). An LTV limit is designed to protect the banking system from risks associated with excessively leveraged borrowers. There are considerable empirical evidences demonstrating that LTV limits reinforce the stability of banks by reducing potential risks from borrowers in case of sharp declines in housing prices (Ahuja and Nabar, 2011). However, LTV limits may have other 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 the LTV limit on consumer choices. I exploit a policy change that required

[1] Such as Denmark, Korea, the Netherlands and many more. For more details, see Appendix A.

banks in Israel (the only mortgage providers2) to increase capital provision for mortgages with an LTV of greater than 60 percent. (For an international comparison of LTV limits, see Appendix A.) 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 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, the banks are likely to increase the loan interest rate for the risky borrowers due to the LTV limit, which may cause the borrowers to lower their LTV ratio. As a result, those risky borrowers may face not only different loan terms but also different housing alternatives.

The difficulty in estimating the impact of LTV limits on the housing market is due to the fact that housing prices provide incentives for the imposition 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 as well as macroeconomic events and, often, a booming housing market.[3] Therefore, although MPP has attracted much attention among researchers and policymakers, MPP research still lacks a basic analytical framework.

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[2] Commercial banks in Israel are responsible for 96 percent of all mortgages to households, and 94 percent of total credit to households.
[3] See Chapter 2: The housing market in Israel.

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 unit purchased, and mortgage taken for 27,324 households spanning 18 months around the 2010 policy change (January 2010 to May 2011). Using this database, I identify causal relations between the LTV limit and consumer choices in the housing market.

This study contributes to the growing literature on MPP by proving a credible estimate of the impact of LTV limits, the most common MPP tool, on the housing market, which is an important and relevant policy question. Most of the existing studies use macroeconomic and cross-country data[4], and face problems of identification, control for country characteristics and difficulty in assessing the distributional effects. Using a household-level database, this study can help to uncover a causal link between the imposition of LTV limits and the demand for housing. To our knowledge, today there are only a few studies using microeconomic data to examine the effects of MPP tools on household choices in the credit and housing markets.[5] In addition, for our knowledge, the present study is the first to examine the impact of the LTV limits on housing unit characteristics other than price, such as asset size and location. In

[4] Crowe *et al* (2011b), Claessens *et al*. (2014), Lim *et al*. (2011).
[5] For example: Igan and Kang (2011), Han *et al.* (2015).

addition, LTV limits are occasionally criticized as preventing groups needing more access to credit markets from obtaining a loan. The household-level database used here enables us to evaluate the impact of LTV limits on different segments of the population, especially those with limited access to credit.

This paper provides an empirical assessment of the effect of the LTV policy on the subset of borrowers constrained by the policy, e.g. the average treatment effect on the treated. However, the treatment status is observed only ex-ante, before the policy. Ex-post, the borrower could have taken an LTV higher than 60 percent and paid a higher interest rate but could also 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 method 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" and "just above" the NIS 800,000 mortgage constraint. The two groups are equally affected by macroeconomic events, but only one group is affected by the LTV limit. I obtain similar results using these two approaches.

The first step is to examine whether the LTV limit is actually effective, that is, whether banks set aside more capital against risky loans, and change 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 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 is, on average, 0.23-0.35 percentage points higher than the interest rate paid by the borrower with identical characteristics just below the LTV limit. This increase in the interest rate may induce 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 in the literature usually arising from changes in credit constraints is whether the LTV limit affects the distribution of borrowers. That is, do I see the same types of borrowers before and after the imposition of the LTV limit or, for example, do banks avoid giving loans to certain households? The literature calls this 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. 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 credit rationing).

Therefore, the second main question of this paper will be: 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 has significant effects: After the imposition of the LTV limit, households bought significantly cheaper assets (8.1 percent) in real terms[6], 8.4 percent farther away from Tel Aviv (the business capital of Israel), and in lower graded neighborhoods (a 9.1 percent decline in the neighborhood's socioeconomic score). To understand the magnitudes of these changes, 55 percent of the Israeli population lives in the center of the country, within a radius of 40 kilometers from Tel Aviv. Within 6 months after the imposition of the LTV limit, affected borrowers moved 3.8 km farther from Tel Aviv, to a significantly lower-rated neighborhood, on average. I conclude that borrowers adjusted their housing choices in response to the LTV limitation.

A commonly raised concern regarding LTV limits is that they may inadvertently target young couples or first-time home buyers, because LTV limits both impose direct financial constraints on households' ability to borrow and tend to be restrictive for those with little savings to use as a downpayment, as they are at the beginning of their life cycle of earnings.[7] Using microeconomic data, this study

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

[7] Stanley Fischer, who served as the Governor of the Bank of Israel between May 2005 and June 2013, wrote in the book

explores which sub-segments of the population were most affected by the LTV limits: first-time home buyers vs. investors (owning more than one residential property); young vs. older households (above the age of 40); etc. In line with Igan and Kang (2011), I find that older households and those who own more than one residential property were affected more by this LTV limit. A possible explanation for these results is that older buyers and investors are more flexible, so that they can move more easily than young borrowers or first-time home buyers, who usually have different limitations 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 of borrower-level shifts in loan terms and borrower behavior in the housing market resulting from the imposition of an LTV limit, using a rich data set. The estimated effects also have important policy implications. While LTV limits typically target commercial banks, my results show that they can have a not inconsiderable spillover effect on households. For example, an LTV limit may cause borrowers to move farther from the center, to a lower graded 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

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"Macroprudential Policy – What Have We Learned" 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).

important not only for Israel, but also for other countries around the world where LTV limits are used.

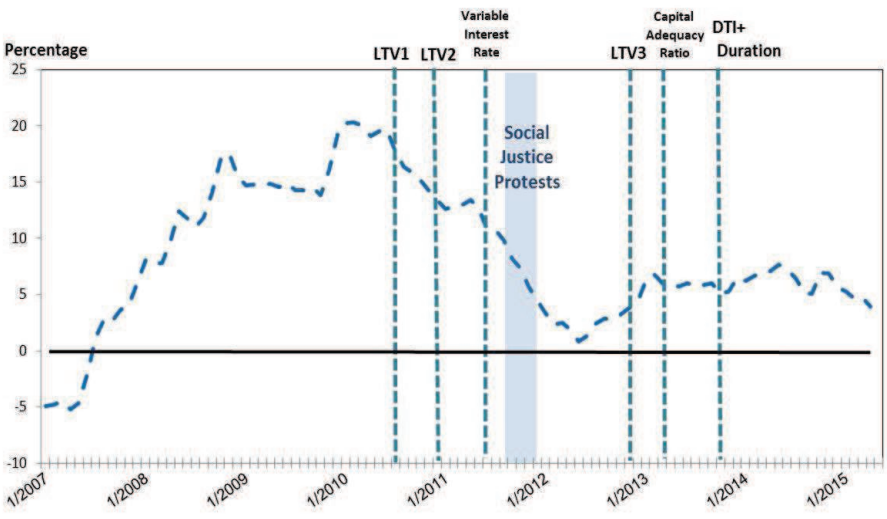
2. **Background: The Housing Market in Israel**

Israel's financial system has not been 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, have contributed to a trend of rising asset prices in Israel, especially housing prices. Since 2008, home prices in Israel have increased each year, with total cumulative increase of 97 percent (68 percent in real terms). 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.*, 2011a).

In view of these trends, between 2010 and 2014, the Israeli Supervisor of Banks 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 try to rein in the pace of home price increases. Figure 1 shows the rate of change in housing prices in Israel and, in vertical lines, the various MPP tools (see Appendix B for a detailed timeline). There is slower rate of increase in housing prices around the time the restrictions were

imposed, but the challenge is to isolate the impact of MPPs on the housing market from other macroeconomic events that occurred around the same time.

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



Source: Israel Central Bureau of Statistics.

[1] MPP tools are shown on the vertical lines. For more details see appendix B. The dotted line represents the monthly change in home prices (in annual terms).

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 over the cost to the borrowers and induce them to

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

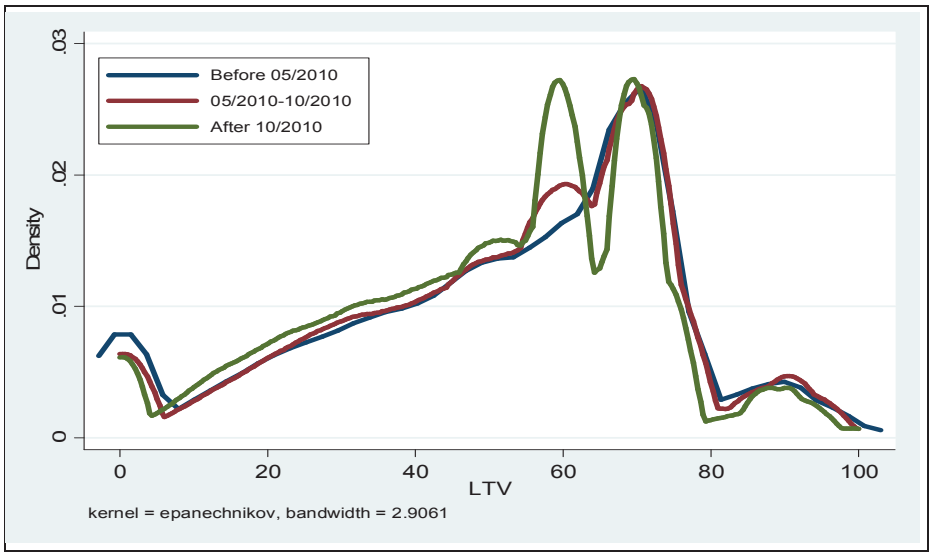
reduce loans with a higher LTV. In practice, as shown in Figure 2, it appears that this limitation was not effective, i.e. the constraints were not binding, especially because the actual allowance implied by the commercial banks in Israel probably would have been higher in any case.

In October 2010, the Supervisor of Banks issued a new Directive concerning capital provision for loans with high LTV ratio. The banks were required to increase capital provisions for housing loans to 100 percent rather than the existing 35–75 percent (depending on the loan characteristics), with an LTV on the date of issue of more than 60 percent, and where the variable interest rate portion of the loan was 25 percent or more. The guidelines did not apply to housing loans originally amounting to less than NIS 800,000.[9] Since the limits would force the banks to tie up more capital against these loans, borrowers wanting to take a loan with a an LTV greater than 60 percent faced higher interest rates, which made them choose loans with an LTV of less than 60 percent. As Figure 2 shows, following the second limitation issued by the Supervisor of Banks, banks began re-pricing loans with an LTV higher than 60 percent. Therefore, this paper will focus only on the limitation issued in October 2010. The advantage of focusing on the first (effective) restriction is that it will be possible to examine the element of public surprise. Regarding the

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

later restrictions, it will be possible to argue that the market foresaw them or learned how to bypass them.[10]

Figure 2: The LTV Distribution by Date of MPPs Implementation



3. Literature Review

The MPPs literature mainly focuses on the effect of those policies on bank stability. These studies find that LTV limits lower bank losses in downturns (Krznar and Morsink 2014, Lim *et al*., 2011). However, the transmission mechanisms of credit constraints from the borrower's perspective are not well explored in the literature. Previous studies

[10] According to Google Trends, the number of searches for the word "equity" in Israel increased by 50 percent from the time of the October 2010 imposition of the LTV limits until the end of 2012, following the last imposition of an LTV limit.

mainly focus on the effect of MPPs on the delinquency ratio at the borrower level (Elul *et al*., 2010).

There is mixed evidence on the effect of credit constraints on the housing market. The literature mainly examines the effect of MPP on home prices and can be divided into two main areas (Claessens, 2014). 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 studies generally find that direct MPPs, such as an LTV limit, may create a positive feedback between credit growth and asset price inflation. Lim *et al*. (2011), using a cross-section of 49 countries, show that LTV or PTI (Payment to Income) limitations 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: IMF (2011) finds that LTV limits are effective in reducing price shocks and reducing the feedback between asset prices and credit. Crowe *et al*. (2011a) and IMF (2013) examine the impact of various MPPs on home prices during boom and bust periods and find that the LTV limits have the best chance of curbing a real estate boom.

The main conclusion that emerges from cross-country studies is that the 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 is limited in much of the international sample because of data constraints. Furthermore, studies of this type face challenges of accounting for specific country characteristics.[11] Second, most of the papers in this line of research rely on macroeconomic data or cross-country analyses, and are unable to assess the distributional effects (Claessens *et al.,* 2014). Finally, it has also been challenging to empirically estimate the effect of LTV limits because MPPs are usually implemented concurrently with other policies and macroeconomic events, making it difficult to attribute outcomes specifically to MPP tools.

The second area of research consists of country-specific studies using microeconomic data and using only one, or a few, MPPs. Using microeconomic data to furnish information on the differences between households, it is possible to estimate the causal effect of the LTV limit. Wong *et al*. (2011) use microeconomic data from Hong Kong and find that the MPPs reduced the cyclicality in the real estate market and caused a halt in the increase of home prices. LTV limitations can also affect prices by influencing expectations. Ahuja and Nabar (2011) try to estimate the effect of the LTV and PTI limitations on expectations in Hong Kong. They find that the use of LTV causes a slowdown in the growth of residential property prices and in the quantity of transactions, albeit with a delay between the two. The pattern is apparently that the number of transactions is

[11] Including the quality of macroprudential supervision, for example in countries where regulation is merely a recommendation and not binding.

affected before property prices are affected, suggesting that the LTV limitation may affect property prices through expectations.

The study by Igan and Kang (2011) is the closest to the present study, using microeconomic data (survey on households' plan for housing tenure and expectations of home prices) from Korea. 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 "investors" than among first-time home buyers. In other words, those findings suggest that the imposition of LTV limits lowers the expectations and incentives for investors, and therefore have a significant impact on activity in the housing market, both in terms of sales volumes and in slowing down the increase in housing prices. This can help to brake 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 will look at the entire population of borrowers (in contrast to the survey data used in Igan and Kang (2011)), and will examine actual purchasing decisions, rather than expectations about the future.

Using the approach of Igan and Kang (2011), the IMF (2014) analyzes home prices in Israel: LTV and PTI limits succeeded in slightly lowering the number of transactions, but there was no evidence that these measures had any effect on the growth rate of home prices. In addition, the study finds that six months after the imposition of the limitations, these measures had led to a decline in

the share of "investors" in the market and to a decline in the number of new mortgages, with LTVs more effective than other MPP tools.

Even case studies concentrating on one particular country face some obstacles. For example, finding the causal effect of the LTV limits on the 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[12] creates a downward bias in the estimates of the effects of policy measures, which can lead to the erroneous conclusion that those measures were 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 these MPPs were expected, their effects may have occurred 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 on the effectiveness of MPPs on the housing market is still preliminary.

This study could contribute to MPP literature by using unique microeconomic data to identify effects of the LTV limits on households' choices in the credit and housing markets. There is little evidence of the indirect effect of MPPs on consumer behavior in the credit and housing markets. This study is one of the first to explore

[12] For example, if policy makers have limited the LTV in response to an increase in home prices.

the impact of the LTV limits on parameters of the housing asset other than price, such as size and location of the housing unit, an area that has not yet been studied in the literature. In addition, this study will examine the impact of the LTV limits on different sub-populations, such as young adults versus older adults, first-time home buyers or those seeking to upgrade their housing versus investors.

4. The Database

The study uses microeconomic data on housing loans from seven commercial banks in Israel between the years 2010 and 2011.[13] The following variables are constructed for each loan: date of issue; the LTV; the value of the acquired property; interest rate; maturity; and others. To our knowledge, to date, this is the only source containing microeconomic data on mortgages that includes data from all banks in Israel.

4.1. Data Construction

This study focuses on the period from January 2010, the beginning of the database, to May 1, 2011. The goal was to focus on a limited time period surrounding the October 2010 imposition of the LTV limit. The literature (IMF, 2014) reports that MPPs have also proven effective around six months after the imposition of restrictions. 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 social protests that occurred in the summer of 2011 and the

[13] Covering around 95 percent of all mortgage loans in Israel.

additional variable interest rate limits that were imposed on April 27, 2011. Therefore, observations following May 1, 2011 were eliminated, 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. Physical characteristics of each housing unit such as the floor area and the number of rooms are recorded, allowing for the identification of characteristics of housing units. In order to clarify details about a housing unit acquired with a mortgage, the study's microeconomic database on mortgages includes block, parcel and sub-parcel numbers. Using these fields, it is possible to merge these two datasets.

However, the recording of blocks and parcels in the mortgage database is distorted. In 36 percent of the records, this information is omitted, and in others, only partial information is provided. Therefore, the matching was carried out through the other remaining fields in the two databases: price, date and city of the asset financed. However, these fields are also distorted in the mortgage database. In 50 percent of the entries, the price field is missing; in 14 percent, the city field of the asset is missing; and in 57 percent, the date of

purchase field is missing. Appendix D shows the process of how these two files were merged, given these constraints.

Because the matching process, described in detail further on, is not parametric, it is highly sensitive to extreme observations. Therefore, after the average age of borrowers was limited to 20 to 80 years, the 1 percent of values at the extreme upper and lower ends of the distribution of the total income per household, housing price and loan size variables, have been omitted.

In conclusion, the original mortgage database was merged with 27,324 observations from the CARMAN database, amounting to approximately one-third of the observations from the mortgage database, from early 2010 through May 1, 2011. There are 11,224 observations after October 2010, when the LTV limits were imposed, in the "treatment group", and 16,100 observations before the imposition of the restrictions in the "control group". Following a test that was run to determine whether the observations in the mortgage database that were matched to the CARMAN database are similar in character to the entirety of the mortgage observations, and whether these matched observations are similar in character to all the observations in the CARMAN database (Appendix E), a Kolmogorov-Smirnov test of equality distributions shows no significant difference between the groups before and after the merger.

4.2. Summary Statistics

The dataset was divided into two periods: before and after the impostion of the LTV limit (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 shown in these sample statistics. There is an increase in the average interest rate after the LTV limit, and a decrease in the average LTV ratio as a result of the limitation. In general, the age of the borrowers declined between the two periods, whereas income increased slightly.

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 constructs a socioeconomic index of neighborhoods, consisting of 16 different variables, including demography, education, employment, income, and the standard of living. These 16 variables are 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 1 shows that the quality of neighborhoods declined after the LTV limit, so households moved farther from the center to lower quality neighborhoods. But the econometric challenge in this paper will be to attribute those changes in housing preferences to the LTV limitation.

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 thousand)	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 thousand)	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 house prices (NIS thousand)	1,078	601	1,106	614	28***	7.4
	Real house prices (NIS thousand)	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 house prices was inflated by the monthly change in the Index of Home prices.

5. 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 rate of growth in housing prices and housing loans tend to move together in the same direction and to influence each other in a two-way feedback loop. In other words, the change in housing prices contributed to those same restrictions, which in turn affect housing prices. As a result, 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 the LTV limits on preferences in the housing and credit markets.

The LTV limit required banks to set aside more capital against risky loans. Hence, the LTV limit changed the terms of the loan contract. This paper focuses on the policy's effect on the subset of borrowers constrained by it. But 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.

5.1. Matching Method (cross-period matching)

Since there is no information in the dataset on the decisions of the household before and after the LTV limit, but only at one point in time following the restriction, the challenge is to find and compare households with similar characteristics before and after the imposition of the LTV limit and compare their choices in the housing and credit markets.

Let *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 having received 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 the 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 limitation. 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 bias in estimating the impact of the 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 the closest observation in terms of observable characteristics in the control group. First, I match the household that received treatment with a similar household that did not receive treatment based on observed characteristics, and then compare the results for the paired households by estimating the average effect of the treatment group to obtain the average treatment affect (ATT) for those in the treatment group.

The matching pairs of households before and after the imposition of LTV limits was done on the basis of their observed characteristics. X denotes age and income at the initial stage. When the observable

characteristics of the household not in the treatment group were close to those in the treatment group, to whom the limits applied, applying a certain distance measure, the households not in the treatment group received a higher weight in the construction of these pairings. I will focus the ATT parameter on the treatment group for an individual with characteristics X: ATT=E($Y_1 - Y_0|T = 1, X$). Y_1 and Y_0 are the outcome variables for households that received treatment and those that did not receive treatment, respectively (Abadie and Imbens, 2006; Abadie *et al.*, 2004; Heckman *et al*., 1998)[14,15].

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 these characteristics. The estimate of Abadie-Imbens minimizes the distance (Mahalanobis distance) of the observed characteristics vector between the control

[14] 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.

[15] 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 (2002) 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.

group and the treatment group. This estimate finds exact pairings on categorical variables, but the pairings according to the continuous variables will not be exact, although they will be very close. This study recognizes this issue and implements a bias-correction component to the outcome variables (For more details, see Abadie and Imbens, 2002).

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 period before. The matching was done according to the observable variables of the overall level of the household's income and the average age per household. The outcome variables are: real home price (in shekels), nominal home price (in shekels), 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 1-20).

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 F examines at 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 I expect to see a larger representation of young people. Because income increased between the two periods, we expect to see a representation of borrowers with higher incomes. Appendix E 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 this result.

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).

5.2. Difference-in-Differences Matching (Within Periods)

One might wonder if other time-varying affects, such as an increase in home 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. 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 LTV limitation. 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 limits, the banks were required to increase capital provisions for mortgages exceeding NIS 800,000 and with an LTV higher than 60 percent. This study then examined 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 of 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 will examine the differences in the average of the outcome variables between the treated and the untreated groups, before and after the limitation. 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 outcomes 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, those observations that were matched to the treatment group by observable characteristics, includes 1,498 observations (895 observations before 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 present 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.

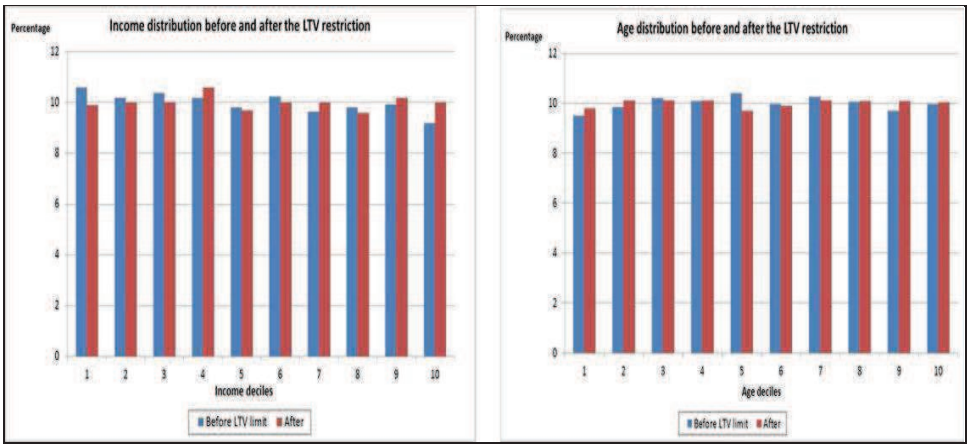
6. Results

6.1. 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's 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, there is no sign of credit rationing.

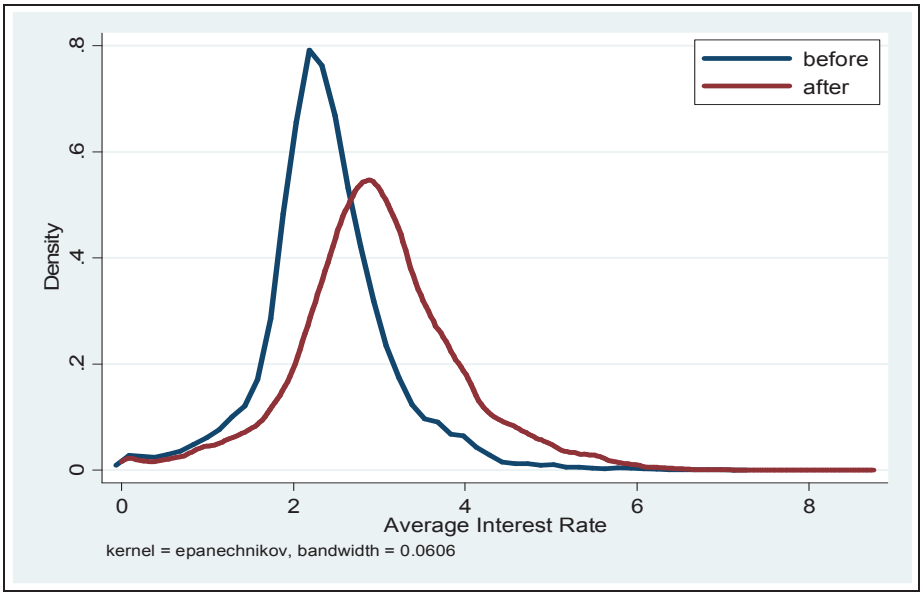
Figure 3: Change in the Distribution of Borrower Characteristics



6.2. The effectiveness of the LTV limitation

The LTV limit required banks to set aside more capital against risky loans (e.g. loans with an LTV ratio above 60 percent). I examine whether the limitation was binding and the reaction of the banks to this policy. Figure 4 shows that the banks increased the average interest rate charged to risky borrowers.

Figure 4: <u>The Change in the Average Interest Rate for Borrowers with LTV>60 percent</u>



Because Figure 4 present interest rate charged in 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 the loan) just above and below the 60 percent LTV limit (61 percent vs. 59 percent in the first test and 61–65 percent vs. 55–59 percent in the second test). Because the prime interest rate changes between the periods, which could bias our 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 (0.01-0.03 percentage points difference in their interest

rate). After the LTV limit, the interest rate paid by a borrower with an LTV just above 60 percent is 0.21-0.36 percentage points higher than the interest rate charged to an identical borrower just below the LTV limit (Table 4).

Table 4

Changes in the Interest Rate for the Matched Borrowers Above and Below the Limitation, after the LTV limit

	61% VS 59%				61-65% VS 55-59%			
	Average Rate (1)	Average Rate (2)	Spread (3)	Spread (4)	Average Rate (5)	Average Rate (6)	Spread (7)	Spread (8)
ATT	.358***	.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 - 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 the loan.

6.3. The Effect of the LTV Limit on Housing Characteristics

The LTV limit, which increases the interest rate for loans with LTV higher than 60 perent, induced risky borrowers to reduce their leverage, as seen in Figure 2. I therefore examine the changes in their housing choices after the introducing of the LTV limit. Using the matching process, we 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 from 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 significantly (8 percent) cheaper assets (in real terms), 8.4 percent farther away from the center of Tel Aviv (around 4 kilometers[16]), in lower quality neighborhoods (a 9 percent decline in the neighborhood's socioeconomic level).[17] Those borrowers also reduce the size of their housing unit by 1 percent.

Table 5

The 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)	Quality of Neighborhoods
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- housholds who borrow after the LTV limit (October 2010).

To understand the magnitudes of these changes, Appendix G shows the map of population dispersion in Israel. 55 percent of Israeli population lives in the center of the country, which is up to 40

[16] The variable distance from Tel-Aviv-Yafo was censored at to 40 KM, to focus only on the most populated areas in Israel, as showed in Appendix G.

[17] Appendix H reports on the LTV ratio of different districts in Israel. The LTV ratio is quite similar around different districts.

kilometers from Tel-Aviv. Within 6 months after the imposition of the LTV limit, affected borrowers moved, on average, 3.8 km (8.4 percent) farther from Tel Aviv, to a significantly lower quality neighborhood[18,19].

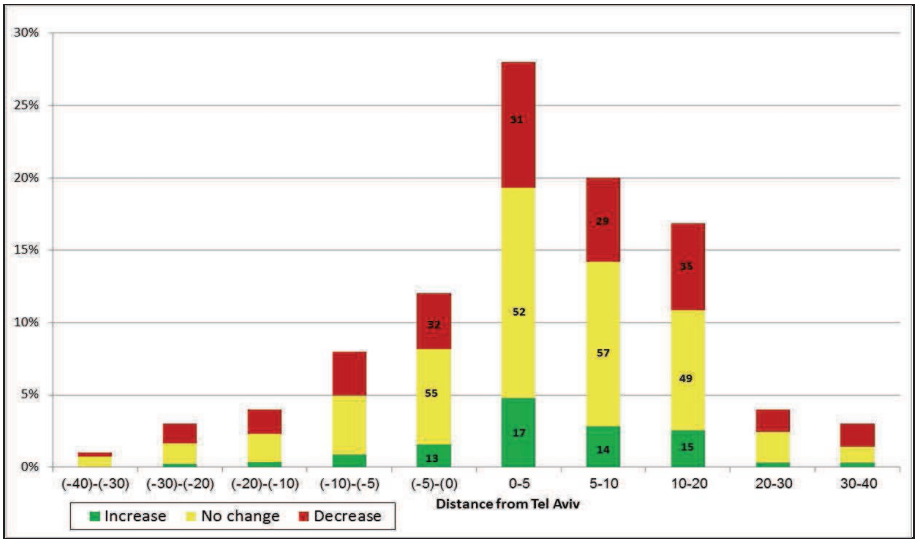
An interesting question that arises from the results of the matching method is what kind of neighborhood the borrowers moved to? More specifically, one might wonder that some borrowers moved farther from Tel Aviv but actually improved their welfare by increasing the quality of their neighborhood. There are of course very high quality neighborhoods outside of Tel Aviv. Figure 5 shows the distribution of changes in socioeconomic level (quality of neighborhoods), by distance from Tel Aviv. The changes in socioeconomic level are divided into 3 groups: upgrade (improvement) (green), downgrade (red) and no change in the quality of neighborhood (yellow). First, although the average distance from

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

[19] This paper assumes that the housing supply is inflexible, at least in the short term. It also only examines the choices in the housing market of the affected borrowers, which is a sub-group of the buyers with a mortgage (which in turn is a sub-group of home buyers). Hence, there remain questions, such as, who bought the remaining dwellings in the center of Israel (that the treatment group cannot afford due to the limitation), what happened to the prices of those dwellings, and whether the LTV limitation had an effect on the supply of housing (for example, did the developers started to build more homes in the periphery, or smaller homes), are not within the scope of this paper.

Tel Aviv was 3.8 km, 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 sub-group 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.[20] This test sheds light on the question of whether LTV

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

limits make it difficult for households in need of credit to purchase property.

With this is mind, the sample was divided into two groups according to the average age of borrowers: young[21]—up to the age of 40—vs 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 were younger adults. 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 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 about 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

The Effect of the LTV Limit on the Housing Market (Matching Procedure), by Age Group

[21] The median age of mortgage borrowers is 41.5. The percentage of young borrowers before the imposition of the LTV limit was 49 perecnt and after the limits were imposed it was 51 percent.

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, those seeking to upgrade their housing situation and investors (who own 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 the LTV limit. The price of their housing purchases declined sharply and they purchased assets farther away from the center. Apparently investors are more flexible in their responses to limitations because they are not purchasing a primary residence and they are only weighing investment considerations.

There is no evidence to suggest that the LTV limit discriminates against weaker population segments.[22]

Table 7

The Effect of the LTV Limit on the 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***	42,148***	-1.38	0.00	1.47*	-0.44*
				(8,284)	(8,998)	(0.9)	(0.01)	(0.85)	(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***	-68,650***	-0.16	0.00	3.3***	-0.88***
				(11,824)	(10,881)	(0.9)	(0.02)	(0.85)	(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**	-115,362****	0.35 (0.9)	0.09***	3.08**	-1.18***
				(20,921)	(19,259)		(0.03)	(1.5)	(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.

6.4. Difference-in-Differences Matching (Within-Period Matching)

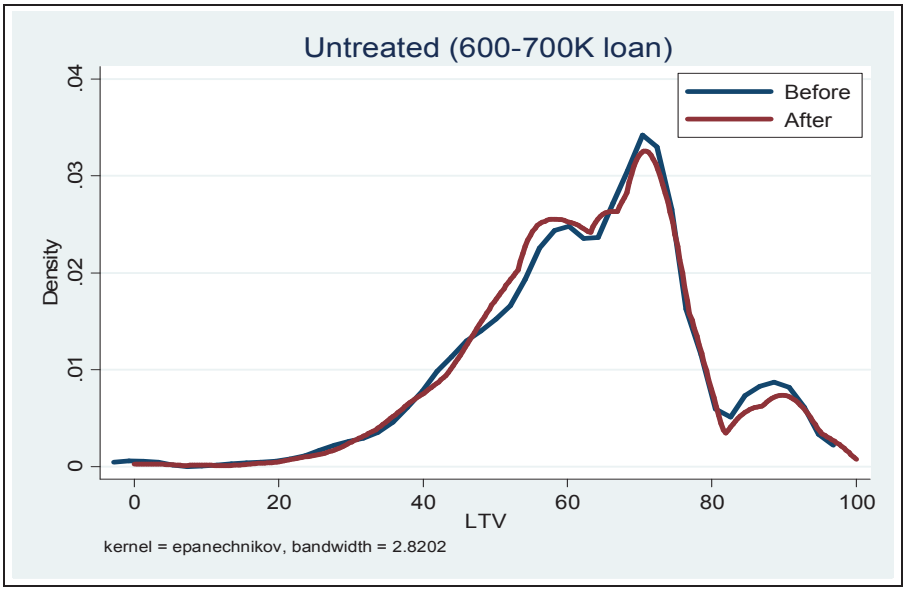
It is possible that borrowers choose different assets not because of the LTV limit but rather due to other time-varying macroeconomic

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

events. I will use the fact that the LTV limit applied only to those who borrow more than NIS 800,000 and examine two groups: the treatment group (those who borrow between NIS 900,000 and NIS 1,000,000) and a control group (those who borrow 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 the LTV to significantly lower values according to the Kolmogorov-Smirnov test, while there was no change in distribution of the LTV in the untreated group.

Figure 6: LTV Distribution Before and After the Imposition of the LTV limit: Treatment and Control Groups



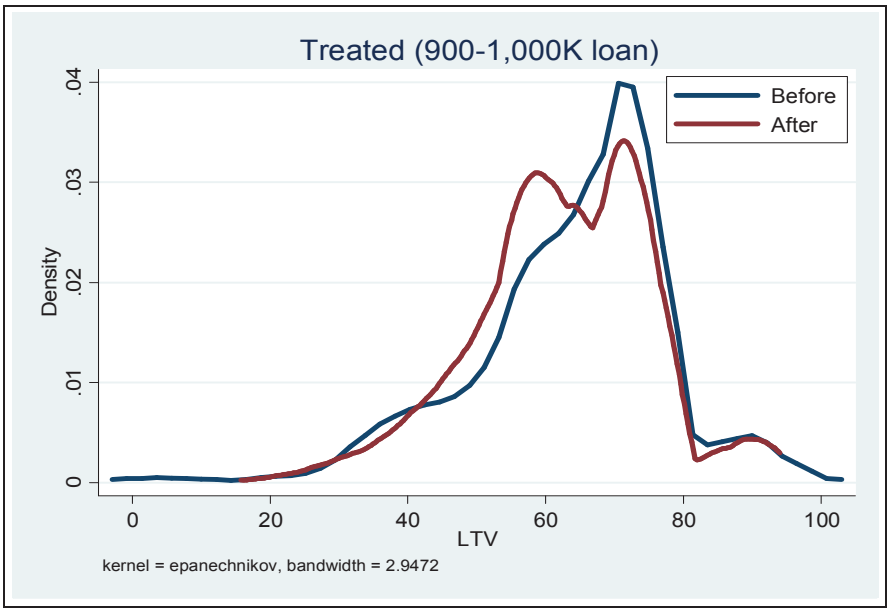


Table 9 compares the average real price of the housing units between the treatment group and the untreated group before and after the imposition of the LTV limit. The difference-in-differences estimation shows that there was a significant decline of NIS 43,000 in real home prices after the imposition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

Table 9

The Effect of the 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

The Effect of the LTV Limit on Distance from Tel Aviv

(Difference-in-Differences Matching Estimations)

	Before	After	Difference
Treated	28.3***	31.5***	**3.2****
	(1.15)	(1.54)	(1.9)
Untreated	41.2***	40.5***	**-0.7**
	(1.63)	(1.41)	(1.2)
Difference in Mean	-12.9***	-9***	3.9***
	(1.6)	(2.1)	(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, on a scale of 1-20 on an index of socioeconomic characterization of neighborhoods, among 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 quality of neighborhoods after the LTV limitation 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. So the treatment group moves to a significantly lower quality neighborhood.

Table 11

The Effect of the LTV Limit on the Quality of Neighborhoods (Difference-in-Differences Matching Estimation[1])

	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.

7. Extensions and Robustness Tests

7.1. 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, 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 divided the sample into 2 sub-groups, by age and by buyer type, I can see that the borrowers above the age of 40 were affected more by the LTV limit, and the "investors" were affected more than first-time home buyers (same results as before).

Table 12

The 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

7.2. 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 could influence households' decisions when purchasing a residential property include household size, or the previous place of residence, which can serve as an indicator of residential preferences and socioeconomic level

Data from the Israel Tax Authority contain 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 the 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 data set. 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 sub-sample. 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

The 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).

8. Concluding Remarks

Since the 2008 financial crisis, macroprudential policy has attracted substantial attention, and the literature on this issue is growing rapidly. However, despite the importance of the wide use of MPP 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 cheaper houses, farther from the center, in a neighborhood with a lower socioeconomic rating.

The main purpose of 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—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 (percent)

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, by 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 than 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% shall 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—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 the 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 will be 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 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 of a blank date field in the mortgages file with one match with 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 which 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 one thousand NIS, 400 paired observations were obtained (step 8). In cases where there is a price adjustment, and a match in date 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

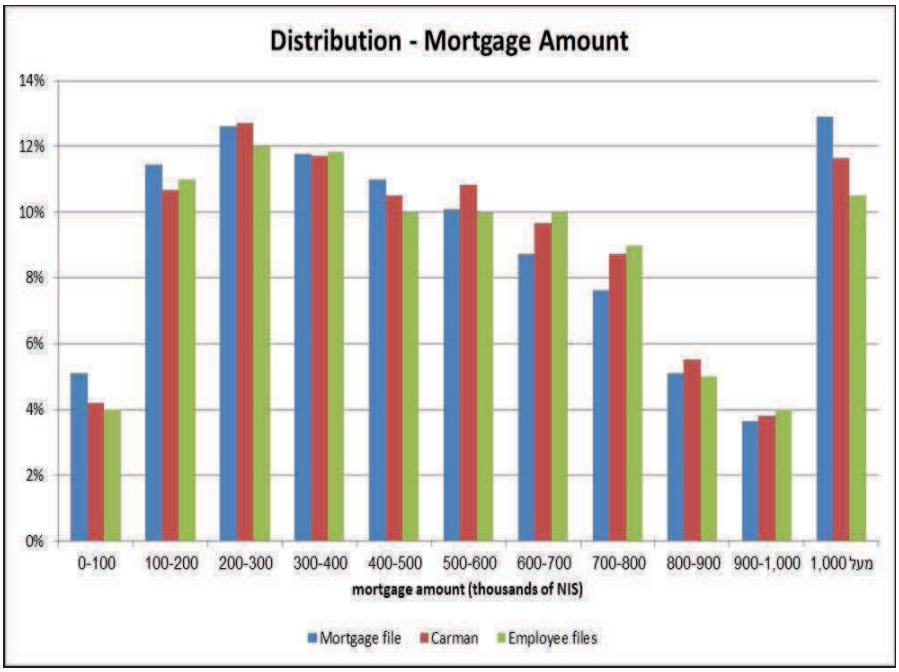
Distributions of Key Variables in the Mortgages and CARMAN Files Separately versus the Merged Sample

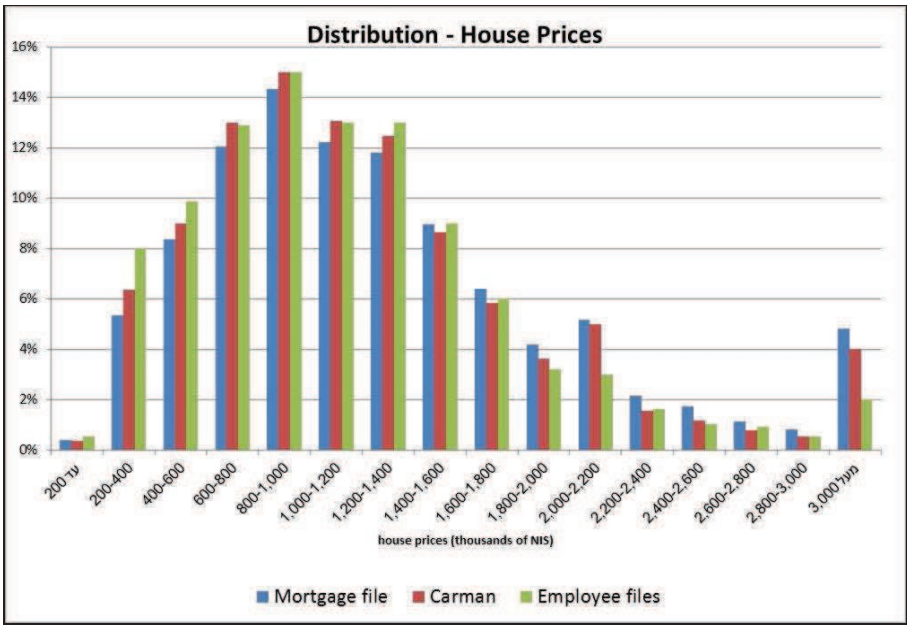
After merging the mortgages file with the CARMAN file, which contains information on the characteristics of the property, the question of whether those observations of the merged file indeed reflect the observations in the CARMAN file must be considered. One of the advantages of combining the mortgages 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 isparticularly 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 employees file between early 2010 and May 2011. The differences between the two samples can also be attributable 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 homebuyers.

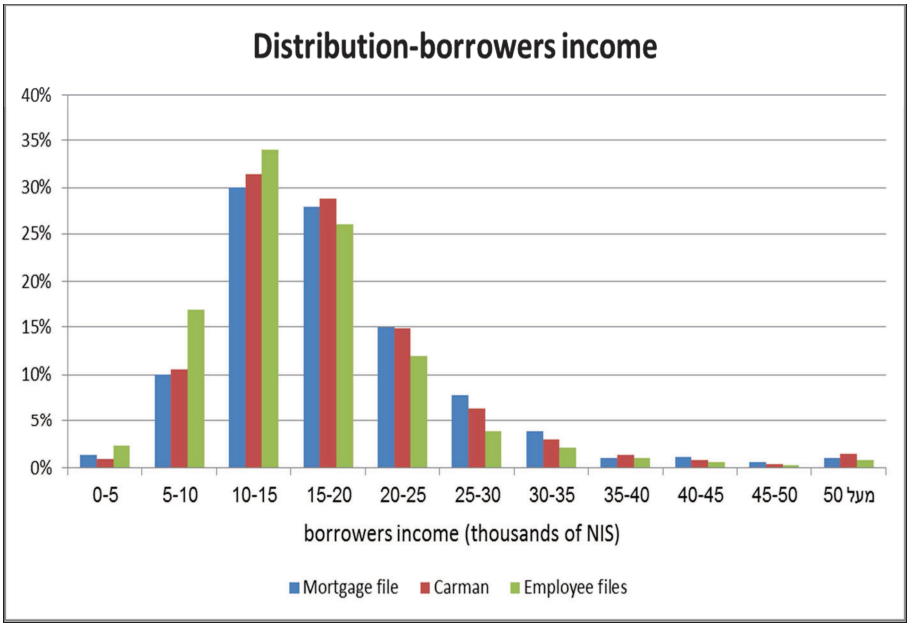
Cause of Purchase	Carman	Mortgages file	employee files
First Home Buyers	34%	42%	43%
Upgraders	37%	40%	42%
Investors	29%	18%	15%

CARMAN is linked to the employees 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, home prices and borrowers income showed no significant differences among the three resources.

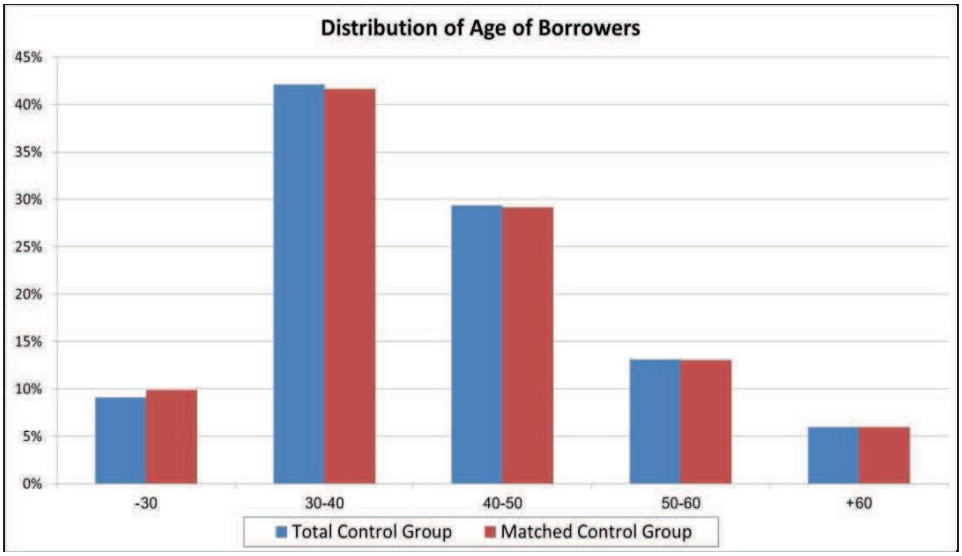


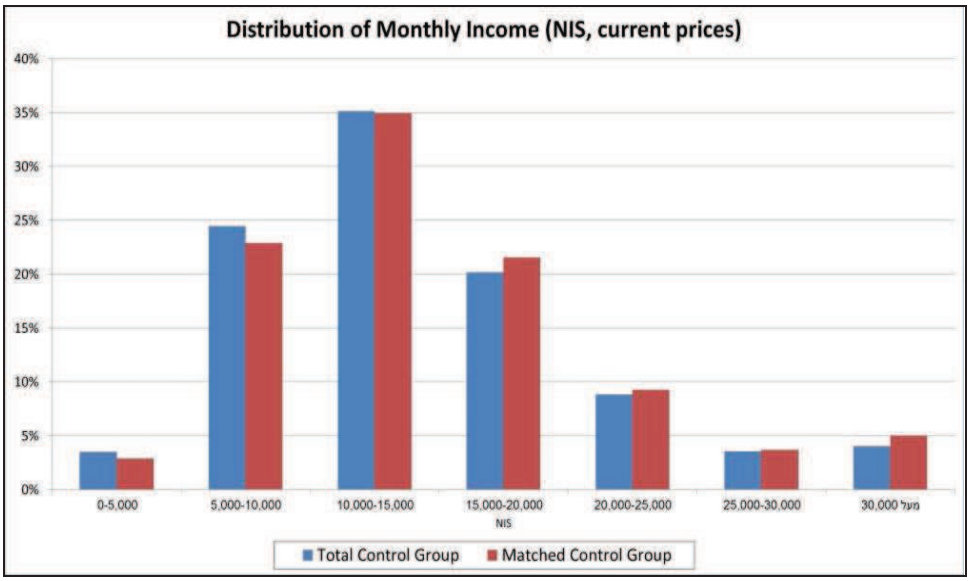

Distribution - House Prices
16%
14%
12%
10%
8%
6%
4%
2%
0%
עד 200
200-400
400-600
600-800
800-1,000
1,000-1,200
1,200-1,400
1,400-1,600
1,600-1,800
1,800-2,000
2,000-2,200
2,200-2,400
2,400-2,600
2,600-2,800
2,800-3,000
מעל 3,000
house prices (thousands of NIS)
Mortgage file
Carman
Employee files

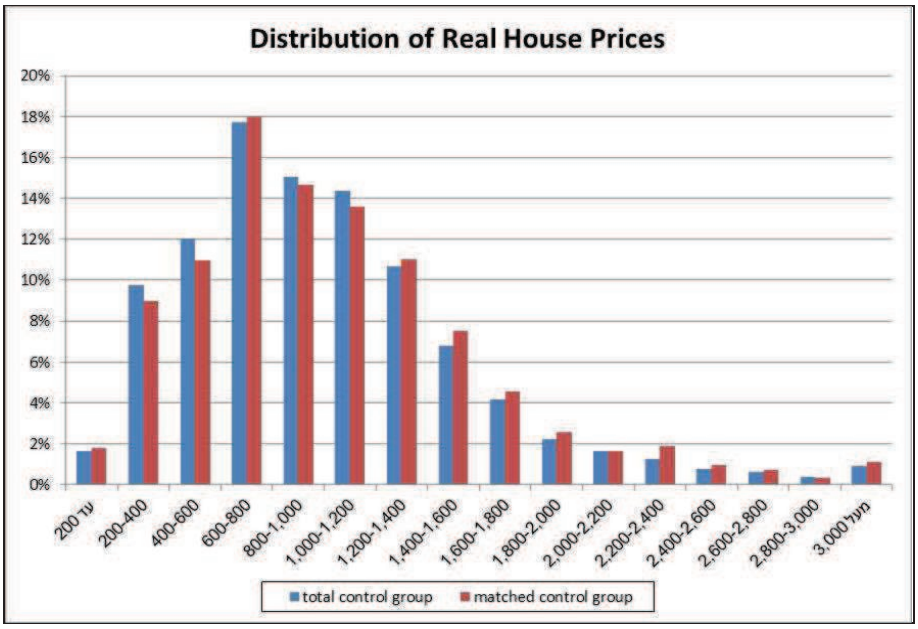

Distribution-borrowers income
40%
35%
30%
25%
20%
15%
10%
5%
0%
0-5
5-10
10-15
15-20
20-25
25-30
30-35
35-40
40-45
45-50
מעל 50
borrowers income (thousands of NIS)
Mortgage file
Carman
Employee files

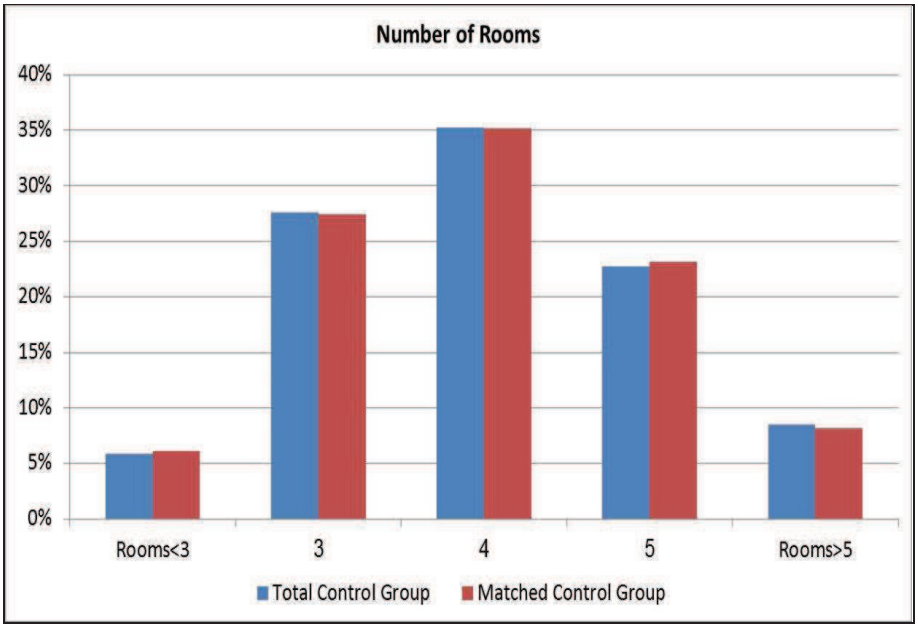
Appendix F

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



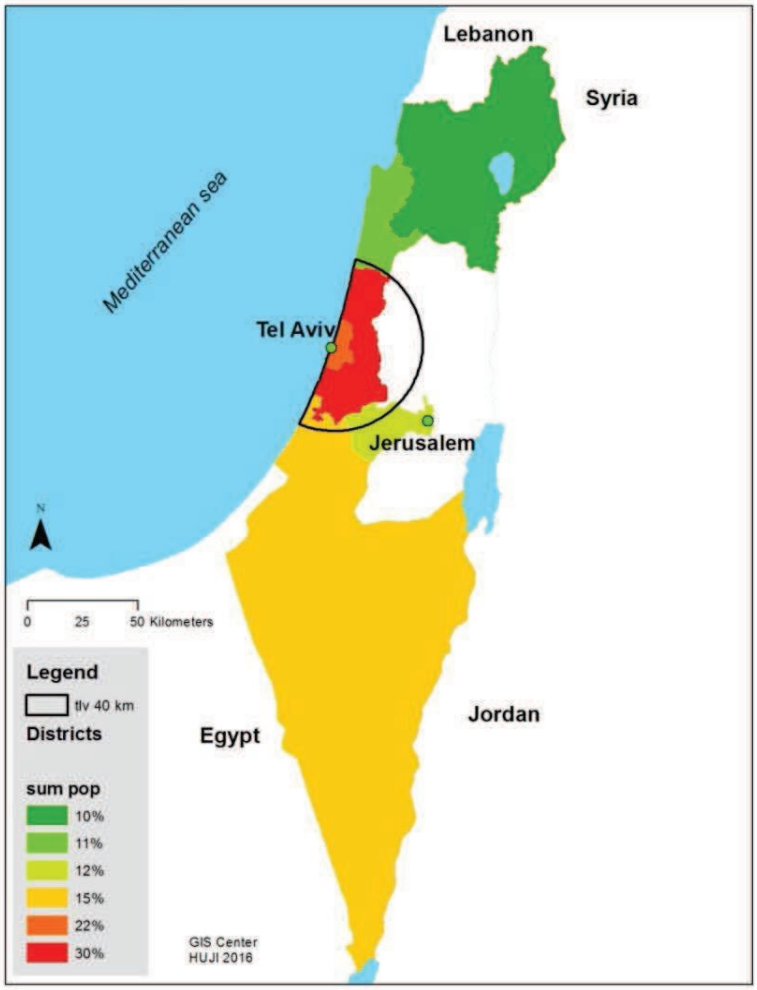



Distribution of Real House Prices
20%
18%
16%
14%
12%
10%
8%
6%
4%
2%
0%
עד 200
200-400
400-600
600-800
800-1,000
1,000-1,200
1,200-1,400
1,400-1,600
1,600-1,800
1,800-2,000
2,000-2,200
2,200-2,400
2,400-2,600
2,600-2,800
2,800-3,000
מעל 3,000
total control group
matched control group


Number of Rooms
40%
35%
30%
25%
20%
15%
10%
5%
0%
Rooms<3
3
4
5
Rooms>5
Total Control Group
Matched Control Group

Appendix G

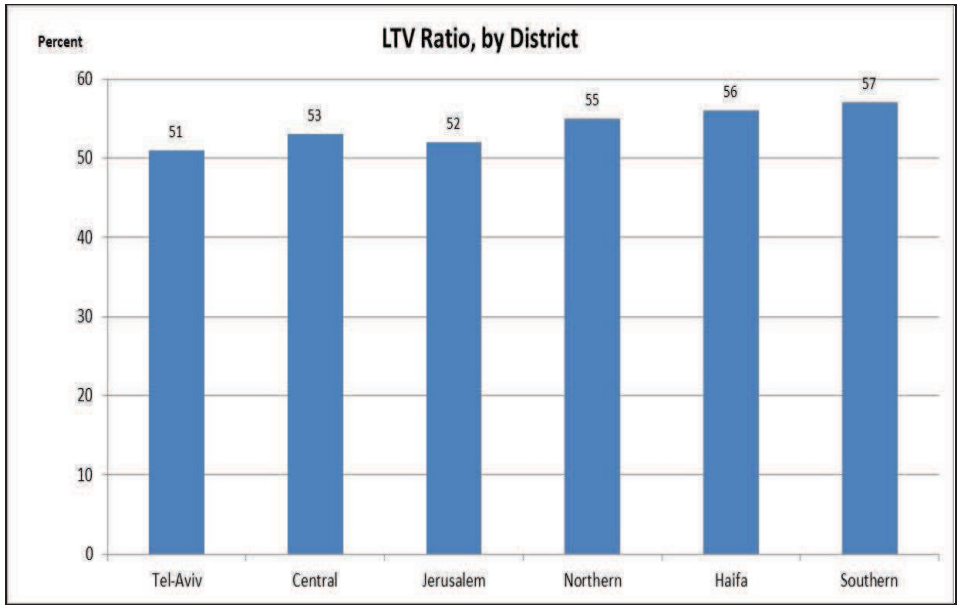
Map of Israeli population[23] dispersion:



Appendix H

[23] Population that is relevant for the sample, from the Israeli Central Bureau of Statistics.

Changes between the periphery and the center of Israel



Appendix I

The 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: