The Impact of Housing Credit on Personal Bankruptcy

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Abstract

We use a linked housing transaction dataset and a personal bankruptcy dataset to study the impact of housing credit on personal bankruptcy in Singapore. Using a difference-in-differences (DD) approach, we find that an increase in housing credit increases the monthly instalment by 460-800 Singapore dollar, and increases the likelihood of personal bankruptcy by 0.15-0.22 percentage points for house buyers who have more exposure to the housing credit increase. To investigate the mechanisms, we show that the observed effect is unlikely to be driven by composition and selection of irresponsible buyers. The effect is mainly due to the increasing debt burden.

JEL Classification: D14, R30, E51

Keywords: Personal Bankruptcy, Housing Market, Housing Credit Policy, Loan-to-Value Ratio, Household Finance, Real Estate

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I. Introduction

What is the impact of housing credit on house buyers? Standard economic theory suggests that housing credit can facilitate consumption-smoothing for house buyers. However, recent research in the US shows that increasing the flow of credit might be counterproductive, and rise of household debt is considered one of factors contributing to the Great Recession (Mian and Sufi 2014). The expansion of mortgage credit leads to excessive household debt and mortgage defaults (Mian and Sufi 2009), causing individuals to spend less (Mian, Rao, and Sufi 2013).

Personal bankruptcy is an extremely disruptive household finance decision with severe welfare consequences for individuals. It is important to understand what causes personal bankruptcy. Previous empirical studies suggest that it is caused by strategic behavior (Fay, Hurst, and White, 2002), negative shocks (Domowitz and Sartain 1999; Himmelstein et al. 2009), access to high-interest credit (Ellis 1998; Skiba and Tobacman 2009). It is not clear whether access to relatively low-interest credit, such as housing credit, is related to personal bankruptcy.

In this paper, we link the previously mentioned literature by studying the impact of housing credit on personal bankruptcy in Singapore. Singapore is a good setting to study personal bankruptcy since bankrupts restructure their debt payments before discharge, rather than continue their lives free of their existing debts. This makes personal bankruptcy an extreme event with negative welfare consequences. Moreover, the relationship between housing credit and personal bankruptcy is difficult to identify with existing research due to both lack of data and lack of research design.

In this paper, we use precise identifiers to merge the dataset with over 150,000 housing transactions from 1995 to 2012 with the personal bankruptcy dataset in Singapore. Our identification strategy is a difference-in-differences (DD) approach. We explore two sources of variations. The first is the time variation arising from housing credit policy change. On July 19, 2005, the Monetary Authority of Singapore (MAS) in Singapore increased the housing Loan-To-Value (LTV) limit from 80 to 90 percent. The second source of variation is cross sectional and arises from different housing prices and dimensions. The exposure of house buyers to the LTV limit change was determined both by whether they bought houses before or after policy change and by housing prices and dimensions. Our identification strategy relies on the interaction of the two sources of variation, and only the interaction can be interpreted as plausibly exogenous.

We find a strong first stage. The increase in LTV limit in 2005 increases the actual LTV of those who bought relatively expensive houses by 4.5%-5% larger. The policy change in 2005 increases the monthly instalment of those who bought relatively expensive

houses by 460-800 dollar larger, about 18% to 30% from the mean. More importantly, we find that comparing with purchasing a relatively cheap house, purchasing a relatively *expensive* house after the increase in LTV limit increases the likelihood of becoming bankrupt by 0.15-0.20 percentage points. Purchasing a relatively *large* house after the increase in LTV limit increases the likelihood of bankruptcy by 0.11-0.18 percentage points. The results are robust with various control variables and flexible interaction terms. We then conduct placebo test to check the validity of the DD strategy. We find that there are no differential trends in personal bankruptcy before the LTV limit increase in 2005, and the DD strategy is valid.

We further investigate the two possible channels of the observed results. The first is through composition effect. There are several possible composition effects after the policy change. For example, an increase in LTV limit encourages existing private house buyers to buy more expensive houses than they could afford before (selection into our treatment group). Households might also switch from buying subsidized public houses or renting houses to buying private houses (selection into our sample). The second is the debt burden effect: after the increase of LTV limit, house buyers can borrow more given the same house value but pay a larger monthly payment, increasing vulnerability to bankruptcy.

To test the composition effects, we exploit another housing policy change in 2002 as a control experiment. On Sept 1, 2002, while maintaining the LTV at 80 percent, the MAS reduced minimum cash payment from 20 to 10 percent of housing value. Savings in the Central Provident Fund (CPF) can be used for the remaining 10 percent of housing value.[1] Since the LTV limit did not change in 2002, house buyers cannot borrow more given the same house value. Because minimum cash payment was reduced from 20 to 10 percent of housing value, house buyers can buy expensive houses they could not have afforded before. And households might also switch from buying subsidized public houses or renting houses to buying private houses. If composition effect is the primary contributing factor, the 2002 policy change should have similar effect to the 2005 policy. We use a similar DD strategy to estimate the impact of the 2002 housing policy change on personal bankruptcy. We find that the coefficients of the interaction terms are close to zero and insignificant. These results suggest that the composition effect is unlikely to be the primary channel.

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[1] The Central Provident Fund (CPF) is a comprehensive compulsory social security savings plan in Singapore. The overall scope and benefits of the CPF consist of retirement, healthcare, home ownership, family protection, and asset enhancement (also see, Agarwal, Pan and Qian 2014). Working Singaporeans and permanent residents, and their employers have to make monthly contributions to the CPF by law (Central Provident Fund Act).

To test the debt burden effect, we use precise identifiers to merge our housing and personal bankruptcy datasets with a lawsuit dataset of more than 532,000 lawsuits in Singapore. We use whether the house buyer is a defendant in a credit-related lawsuit after buying a house as an indicator of debt burden. We have two strategies to test the debt burden effect. First, we use the similar DD strategy to estimate the impact of the LTV limit increase in 2005 on being a defendant in a credit-related lawsuit. We find that, comparing with purchasing a relatively cheap house, purchasing a relatively expensive house increases the likelihood of becoming a defendant in a credit-related lawsuit by 0.15-0.20 percentage points. Compared with purchasing a relatively small house, purchasing a relatively large house increases the likelihood of becoming a defendant in a credit-related lawsuit by 0.20-0.22 percentage points. Thus, the results are consistent with the debt burden effect. Second, we estimate the heterogeneous effect of the LTV limit increase on personal bankruptcy based on whether house buyers are defendants in credit-related lawsuits. The debt burden effect predicts that the impact of the LTV limit increase in 2005 is larger for those who are defendants in credit-related lawsuits compared to those who are not. Using a specification with triple interactions, we find that the impact of the LTV limit increase in 2005 is 8.4-11 percentage points greater for those who are defendants in credit-related lawsuits compared to those who are not. These results from the above two strategies suggest that the debt burden effect is the primary explanation for the observed pattern. Our findings are consistent with Keys et al (2014)'s findings that a sizable decline in mortgage payments induces a significant drop in mortgage defaults using the housing loan data in the US.

An important concern is the selection for irresponsible house buyers. In our sample, there might be irresponsible house buyers who will become bankrupt no matter what house they are buying. Since the house buyer can choose their housing prices and dimensions, it is possible that an irresponsible home buyer is more likely to buy a relatively more expensive or larger house after the LTV limit increase in 2005. In this case, finding a positive treatment effect would be a spurious outcome of an omitted variable about one type of house buyers. We have two strategies to test this issue. First, we test the selection on observables. We use the similar DD strategy to estimate the impact of the LTV limit increase on five observables that might correlate with this type of house buyers: whether they file bankruptcy before buying houses, whether they are defendants in credit related lawsuits before buying houses, gender, ethnicity and age cohort. We find that there is no evidence that those who buy relatively more expensive houses after the LTV limit increase in 2005 are selected to be more irresponsible. Second, we use the housing buying pattern before the LTV limit increase in 2005 to estimate the simulated housing prices and dimensions. We use the similar DD strategy to estimate the impact of the LTV limit increase in 2005 on personal bankruptcy but use simulated housing prices (dimensions) instead of actual prices (dimensions). If selection is the main issue, we should observe that the estimated results using simulated prices and dimensions are close

to zero and different from those results with actual prices and dimensions. We find that the coefficients of interaction terms are positive and significant. The magnitudes are similar to the estimation that uses actual price. Therefore, both strategies suggest that selection by irresponsible buyers is unlikely to be an important issue.

Another concern is that since personal bankruptcy is generally not an immediate effect of a policy, there might be other changes in policy or the Singaporean economy that impact our results. It is possible that an increase in the bankruptcy rate is due to the increase in interest rates of housing loans rather than the LTV limit increase in 2005. This hypothesis predicts that given the LTV limit, when interest rate increases/decreases, personal bankruptcy of those who buy relatively expensive houses increase/decrease. We restrict our sample to the period with a fixed LTV limit but fluctuate the interest rate. We find no evidence that an increase in bankruptcy rate is due to the increase in interest rates.

It is important to understand whether the results are driven by speculators or owner - occupied house buyers. We use two proxies to identify speculators: whether house buyers sell the house in a sub-sale and whether house buyers buy multiple houses from 1996 to 2012.[2]We restrict our sample to speculators and use the similar DD strategy to estimate the impact of the LTV limit increase in 2005 on personal bankruptcy. We find that the coefficients are close to zero and insignificant. These results suggest that the main results are not driven by speculators but by owner-occupied house buyers.

Note that our research design cannot analyze the overall welfare effect of housing credit. We identify the negative effects of housing credit but the benefit of housing credit is difficult to estimate, which is beyond the scope of the study.

We also investigate the relationship between personal bankruptcy and house-selling behavior. Using a simple OLS regression, we find that that bankrupt home owners are about 9.0 percentage points more likely to sell their houses. Conditional on selling houses, the return on house sales for bankrupt home owners is 10.7 percentage points less than those who are not bankrupts. Why is the return of house sale for bankrupt home owners? One possibility is that the bankruptcy law requires the house to vest in the Official Assignee and be sold after bankruptcy. Another possibility is that home owners know their financial burden and bankruptcy prospects, and they might sell houses even before bankruptcy to smooth consumption and mitigate negative shocks. We find that for those who sell houses and become bankrupt, 57.4 percent of them sell their houses before becoming bankrupt. Those who sell their houses before becoming bankrupt have a 12.3 percent price discount compared to those who do not file for bankruptcy. Those who sell

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[2]A "sub-sale" is the sale of a unit before the government issues the temporary occupation permit by someone who bought the unit from the developer. If house buyers sell the house with a sub-sale to earn profit from housing price increase, they are likely to be speculators.

their houses after becoming bankrupt have and 8.7 percent price discount compared to those who do not become bankrupt. These results suggests that the low return from forced sale are not only due to the bankruptcy law but also the second possibility: home owners know their financial burden and bankruptcy prospects; therefore they sell houses even before bankruptcy to smooth consumption and mitigate negative shocks.

This paper contributes to several strands of economics, law and banking literature. First, our results contribute to literature on the impact of credit. Standard economic theory suggests that housing credit can facilitate consumption-smoothing for house buyers. Empirical research generally supports the predictions of the positive impact of credit. For example, increased access to credit can lead consumers to improve their welfare. Another positive impact is to insure against income fluctuation, as increased access to credit can help insure consumption in the short run (Islam and Maitra 2010) and help smooth inter-seasonal consumption changes (Menon 2003).

Recent empirical research also finds the negative impact of credit in the setting of payday loans. Access to payday loan increases personal bankruptcy rates (Skiba and Tobacman 2009), increases households' difficulty paying mortgage, rent and utility bills (Melzer 2011), reduces overall job performance and workforce retention (Carrell and Zinman 2013), and increases financially-motivated crimes (Cuffe 2013). We study the impact of credit on the housing market. Housing credit is different from payday loans because of its large loan size, relatively low interest rate, and utilization by larger proportion of population with relatively good credit history. We find the negative impact of housing credit on personal bankruptcy in the setting of Singapore. Mian and Sufi (2009) also study the negative impact of housing credit. Using ZIP code-level data in the US, they find that sharp relative growth in mortgage credit is followed by a sharp relative increase in defaults in ZIP codes with a high share of subprime mortgages. They show evidence that the growth in mortgage credit is likely due to the supply of credit. Keys et al (2010) show that the ease of securitization reduces subprime lenders' incentives to screen subprime mortgage borrowers, and thus increases the defaults. Our paper is different in the following two aspects: first, we study the supply shock of housing credit in private housing in Singapore, where home buyers are relatively rich and have relatively good credit history; second, the outcome we study is personal bankruptcy rather than mortgage defaults, which has different welfare consequences for individuals since bankrupts in Singapore restructure their debts payment before discharge, rather than continue their lives free of their existing debts.

Second, our results contribute to the literature on the reasons for personal bankruptcy. There are three explanations for personal bankruptcy in the literature. The first is strategic motive, which predicts that households are more likely to file bankruptcy when their financial benefit from filing is higher than cost (Fay et al. 2002). Agarwal, Liu and

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Mielnicki (2003) show that increase in the benefit of homestead and property exemptions encourages bankruptcy. Gross and Souleles (2002) show that the rise of bankruptcy is likely to be due to the decline in cost of filing bankruptcy. Livshits et al (2010) use a heterogeneous agent life-cycle model, and find that a decline in the cost of bankruptcy together with a decline in the cost of lending are primary factors driving the rise of personal bankruptcy in the US. Gross et al. (2014) show that tax rebates can cause a temporary increase in bankruptcies files due to liquidity constraints and bankruptcy filing costs. The second is negative shocks, which includes those who intend to repay their debts but have to file for bankruptcy due to negative medical or income shocks (Domowitz and Sartain 1999; Himmelstein et al. 2009). The third reason is access to high-interest credit such as credit cards (Ellis 1998) and payday loans (Skiba and Tobacman 2009). Additional lending reduces the chances that prior debt is repaid and hence further increases indebtedness resulting in bankruptcy. This is also in line with how receiving cash transfers that are sufficiently large to pay off unsecured debt can allow individuals to postpone bankruptcy. (Bizer and DeMarzo 1992, Hankins et al. 2010). Our study adds more evidence related to the third explanation. We show that increased access to housing credit might lead to more personal bankruptcy, even with relatively low interest rates. Moreover, we show that the main channel of the increase in personal bankruptcy is the increase in debt burden for home owners, which is consistent with the findings in Keys et al (2014) that a sizable decline in mortgage payments induces a significant drop in mortgage defaults using the housing loan data in the US.

Finally, our house-selling results are related to the illiquidity in the housing market and its adjustment. Campbell et al. (2011) show that forced sale due to death or bankruptcy results in a price discount. For example, they find large foreclosure discounts about 27 percent of house prices on average. Our results are consistent with theirs and add more evidence supporting the price discount associated with forced sale. Moreover, we show that home owners sell houses even before bankruptcy, suggesting that they use housing to adjust for large negative shocks. This result is consistent with the results and predictions in Chetty and Szeidl (2007) about commitment consumption.

The remainder of the paper is organized as follows. The next section describes data and institutional background in Singapore. Section III presents the empirical strategy and results. Section IV discusses the possible channels. We discuss the relationship between personal bankruptcies and selling behavior in Section V. Section VI concludes.

II. Data and Institutional Background

II.A. Institutional Background

Singapore is an island nation with a land area of about 716 square kilometers (projected to increase to 766 square kilometers by the year 2030). As of 2013, the population of the country was 5.47 million, including 3.34 million citizens and 527,000 permanent residents. The resident population is ethnically diverse, comprising 74 percent Chinese, 13 percent Malays, nine percent Indians, and three percent other.[3]

Singapore's home ownership rate of over 90 percent is among the world's highest. The residential market comprises two segments: public housing and private housing. About 82 percent of housing units were built by the government's Housing and Development Board, mostly for direct sale to eligible citizens at subsidized prices. These public housing units are primarily owner-occupied. The government closely regulates the sale and resale of public housing units and tightly controls information on sale prices.

Private housing refers to condominiums, private apartments, landed property and executive condominiums (EC).[4] About 18% of residential units are in the private housing market. The private housing market is laissez-faire, except that foreigners are not allowed to buy low-rise residential units such as single-family homes. Private housing may be bought for investment – to rent or re-sell. In this paper, we focus on private housing.

The loan-to-value (LTV) ratio is the ratio of a loan to the value of an asset purchased. In the housing market, LTV ratio is used by banks to represent the ratio of the mortgage amount as a percentage of the total appraised value of real property. For example, if one borrow $800,000 to purchase a $1 million property, the LTV ratio is 80 percent. In general, the higher the LTV ratio, the riskier the loan is for a lender.

[Insert Figure I]

Figure I shows the key LTV limit changes for private residential properties in Singapore after 1996. On May 15, 1996, the Monetary Authority of Singapore (MAS) introduced the 80 percent Loan-To-Value limit for bank-originated loans to prevent the housing market from overheating and to ensure that there are sound lending practices across property market cycles. Due to the increased cost of living in Singapore, the government introduced measures to keep housing affordable for citizens. A Housing Loan Notice was issued on July 22, effective September 1, 2002, and replacing the previous Housing Loan Directive. The Notice allows the use of CPF savings beyond the LTV limit for up to a further ten percent of the value of the property, leaving the minimum cash requirement at

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[3]These and the following statistics of Singapore's population and housing market are drawn from the *Population trends 2014*, Department of Statistics, Singapore.

[4]Condominium buildings are high-rise buildings with exclusive access to amenities such as parking, sports facilities, and children's playgrounds. Apartment buildings are high-rise buildings without such exclusive amenities. Landed properties are single-family homes, and have unique street addresses and 6-digit postal codes.

ten percent.[5] Over the next three years, the housing market had sufficient time to adjust, and MAS decided to increase the LTV limit to 90 percent on July 19, 2005. The minimum cash payment was also decreased to five percent; this limit remains in place to deter over-borrowing and minimize potential losses by banks arising from borrower default. As such, a purchaser can pay the remaining 5 percent using their CPF savings.[6] In 2010, the MAS introduced several measures to stabilize the heating housing market. On Feb 20, 2010, the LTV limit was lowered to 80 percent while the minimum cash payment remained at five percent.[7] On August 30, 2010, The LTV limit was lowered from 80 to 70 percent for borrowers who have one or more outstanding housing loans.[8] Since our dataset includes housing transactions from 1995 to 2012 and personal bankruptcy generally takes a long time after loans are initiated, we focus our sample between May 15, 1996 and Feb 20, 2010. Since most LTV limit changes have announcement dates and effective dates that are very close together, the policy is generally a surprise to home buyers and sorting around the policy date is negligible.

Personal Bankruptcy refers to the status of any individual debtor who has been adjudged bankrupt by a bankruptcy order. Like many developed economies such as the US, Singapore has strict laws governing bankruptcy, which are encompassed in the Bankruptcy Act of 1995. This Act not only stipulates the procedures of personal bankruptcy but also provides alternative ways in which insolvent individuals can reach some form of compromise with their creditors and avoid bankruptcy. It was further amended in 1999 to encourage technopreneurial activity and in 2009 to provide for the Debt Repayment Scheme (DRS).

[Insert Figure II]

Figure II describes the procedures of personal bankruptcy under the Bankruptcy Act in Singapore. When a debtor becomes insolvent, the debtor will firstly be allowed to propose to their creditors a voluntary arrangement. This is known as the interim order and is effective for an initial period of 42 days subjected to the approval by the majority of creditors and the extensions of the court. This is a negotiated debt settlement where the debtor discloses his assets and liabilities and proposed how he intends to settle the debts owed. Once this debt arrangement is settled, it will be deemed as complete and both

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[5]MAS Website, http://www.mas.gov.sg/News-and-Publications/Media-Releases/2002/MAS-Issues-Revised-Housing-Loan-Rules--30-August-2002.aspx

[6]MAS Website,   http://www.mas.gov.sg/News-and-Publications/Media-Releases/2005/MAS-Issues-Revised-Housing-Loan-Rules.aspx

[7]MAS Website, http://www.mas.gov.sg/News-and-Publications/Media-Releases/2010/Measures-To-Ensure-a-Stable-and-Sustainable-Property-Market.aspx

[8]MAS Website, http://www.mas.gov.sg/News-and-Publications/Media-Releases/2010/Measures-to-Maintain-a-Stable-and-Sustainable-Property-Market.aspx

parties benefit. In the event that the voluntary arrangement fails, either the debtor or the creditor can file the bankruptcy petition. Upon filing of a bankruptcy application in the High Court by either the debtor or the creditor, the debtor will be pronounced bankrupt in approximately four to six weeks. In a creditor's petition, a creditor will have to issue a statutory demand of payment from a debtor within 21 days. If the debtor does not meet this demand, the creditor will then file a bankruptcy application in the High Court, and a hearing date will be given. In the case of a debtor's petition, a statement of affairs must be filed together with the bankruptcy petition and is granted if the court is satisfied that the debtor is unable to pay his or her debts.

After the hearing, another measure to help the debtor is through the DRS, which was put in place in 2009. The DRS was modeled after Chapter 13 of the US Bankruptcy Code and is targeted at individual debtors with debts not exceeding $100,000. This allows the debtor to propose a repayment plan setting out the terms for the repayment of his debt, payable within five years.

In the event that an individual is declared bankrupt, he or she will have to submit a statement of assets. At this point, all of the assets will vest in the Official Assignee for the benefit of the creditors. However the Official Assignee will allow the debtor to keep possessions of assets that are deemed necessary and these includes: (1) furniture, (2) personal effects, (3) limited tools of trade, (4) any private properties held by debtor on trust for any other person, (5) HDB flats (where at least one of the owners is a Singapore Citizen), (6) monies in their CPF account, (7) life insurance policies (expressed to be for their spouse or children benefit), (8) life insurance nominations, (9) any other properties that are excluded under any other written law as well as (10) compensations awarded for legal actions in repeat of their personal injuries or wrongful act against them.[9] Creditors who hold security over a debtor's asset, have a right to sell that assets, and any proceeds of sale are to be remitted for the benefit of the creditors. The Official Assignee will then sell off the remaining assets; the dividends received will be paid to the creditors who have provided proof of debt. At this point, the debtor is allowed to make an offer of compensation or arrangement to pay his creditors, subjected to their approval.

There are two ways a debtor can get out of bankruptcy: Annulment of the Bankruptcy Order and Discharge from Bankruptcy by the High Court or by the Official Assignee. In the first case, the Bankruptcy Order made against the debtor will be annulled either when the debtor repays his debt in full or makes a settlement offer which is accepted by those creditors who hold at least 75 percent of the total debt owed. Annulment of the Bankruptcy Order puts the debtor in the same position he or she would be in if no

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[9]IPTO website, https://www.mlaw.gov.sg/content/ipto/en/bankruptcy-and-debt-repayment-scheme/bankruptcy/information-for-bankrupts/information-for-bankrupts1.html

Bankruptcy Order had been made. In the second case, the debtor may apply to the High Court for an Order of Discharge. The High Court will then hear from both the Official Assignee and the creditors involved before deciding whether to discharge the debtor from bankruptcy. The Official Assignee may also, based on the debtor's conduct, discharge the debtor, provided that at least three years have lapsed since the start of bankruptcy and the proven debt is less than $500,000. This is subjected to consideration of factors such as the cause and period of bankruptcy, the debtor's assets and payments to the bankruptcy estate for the benefit of the creditors, the debtor's conduct, and the level of co-operation given to the Official Assignee in the administration of the bankruptcy affairs.

II.B. Data and Summary Statistics

We use three main data sources. The first is a proprietary dataset of legal filings of more than 150,000 private housing transactions from 1995 to 2012. The records include the address of the residential unit including the name of the building, floor and unit number, attributes of the unit, and details of the transaction. The unit attributes are the legal tenure (freehold or leasehold), type of development (apartment, condominium, or landed), and housing dimension in square meters.[10] The transaction details are the date, names and personal identifiers of buyers and sellers, type of sale (new sale, sub-sale, or resale), and the price. A "new sale" is the sale of the unit by the real estate developer, which may occur before or after the government issues the temporary occupation permit that allows the unit to be lived in. A "sub-sale" is the sale of a unit before the government issues the temporary occupation permit by someone who bought the unit from the developer. A "resale" is the sale of the unit after the issuance of the temporary occupation permit, by a party who is not the developer. Since we do not observe the actual LTV ratio for each housing transaction, we use an Intent-to-Treat (ITT) estimation.

Our second source is a bankruptcy dataset that contains more than 75,000 personal bankruptcy cases processed by the Supreme Court of Singapore from 1985 to 2012. For each bankruptcy case, detailed information on debtor, total bankruptcy amount and dates of different bankruptcy processes, (such as Statutory Demand, petition and hearing) is captured. Bankruptcy events (credit card, car loan or home mortgage) are not reported.

The third source is a proprietary dataset of court records that contains more than 532,000 lawsuits from 1994 to 2012. For each case, the record includes the filing date and the names and personal identifiers of the plaintiffs and defendants. The record also includes the nature of lawsuits such as credit reason. We link the data to residential property

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[10]Freehold is an unlimited tenure of ownership, while leasehold is a limited tenure of ownership, typically 99 years, after which ownership reverts to the government (also see Giglio, Maggiori, and Strobel, 2015).

transactions through the buyer's personal identifier. We use whether the house buyer is a defendant in a credit related lawsuit as an indicator of debt burden.

Our datasets have several advantages. First, they are representative of private housing transactions, personal bankruptcy, and lawsuits in Singapore. Second, all the datasets contain unique identifiers; as a result, we can merge the datasets Third, all the datasets include information about date of respective events. This is helpful because we can construct timelines by merging these datasets. Finally, the datasets includes various individual and housing characteristics that allow us to control for these variables in the analysis.

One limitation of the above datasets is that there is no mortgage information so that we do not know the actual LTV ratio or the monthly instalment for each housing transaction. We explore another proprietary dataset of mortgage loans originated between 1992 and 2012 from a large representative bank in Singapore. The mortgage dataset includes housing price (but not housing dimensions), the LTV ratio, monthly instalment, origination date, interest rate, loan terms, and homebuyers' characteristics. Note that the mortgage dataset does not include personal identifiers so that we cannot link it to previous three dataset. Therefore, we mainly use this dataset to check the first stage of the policy impact in Singapore.

Since we study the impact of LTV limit change in Figure I, we focus our sample between May 15, 1996 and February 20, 2010. Table I reports the summary statistics.

[Insert Table I]

The dataset includes a total of 144574 unique buyers between May 15, 1996 and February 20, 2010. Of these buyers, 102960 made a single property purchase and 41614 made multiple property purchases during this period. We report the summary statistics for single property buyers and multiple property buyers separately. For single house buyers, we report two sub-samples based on whether the housing price is above the 50th percentile of housing prices in the same year within the same region. We also conduct a t-test for differences in means. For multiple property buyers, we report the combined statistics of all buyers.

About half of housing buyers were male, and approximately 93 percent were ethnic Chinese. Housing transactions below the 50th percentile of prices involved more Condominiums (67 percent) and Private Apartments (26 percent) but less freehold property (31 percent) than transactions above the 50th percentile of prices (57, 12, and 57 percent respectively). More than half of housing transactions are New Sales, and the average age of the buyer at purchase was around 40 years old. Properties sold above the 50th percentile of housing prices had a mean price of about 1.2 million SGD and a mean

size of 188 square meters.[11] Properties sold below the 50th percentile of housing prices had a mean price of 0.6 million SGD, and had a mean size of 109 square meters. 0.89 percentage point of single property buyers below 50th percentile of housing prices file for bankruptcy during the study period (including 0.81 percentage points who file a single bankruptcy and 0.08 percentage points of who file multiple bankruptcies). 0.90 percentage point of single property buyers above 50th percentile of housing prices file for bankruptcy during the study period (including 0.77 percentage points of filing a single bankruptcy and 0.13 percentage points of filling multiple bankruptcies). For buyers of multiple properties, 0.65 percentage points file for a single bankruptcy and 0.09 percentage points file for multiple bankruptcies. We focus on single house buyers in the following analysis, and check the robustness in Appendix Table A3.

III. Empirical Strategy

III.A. The First Stage

Our identification strategy is a difference-in-differences (DD) approach. We explore two sources of variations. The first is the time variation arising from housing credit policy change. On July 19, 2005, the Monetary Authority of Singapore (MAS) in Singapore increased the housing Loan-To-Value (LTV) limit from 80 to 90 percent. The second source of variation is cross-sectional and arises from different housing prices and dimensions. The exposure of house buyers to the LTV limit change was determined both by whether they buy houses before or after policy change and by housing prices and dimensions. Our identification strategy relies on the interaction of the two sources of variation, and only the interaction can be interpreted as plausibly exogenous.[12]

Our housing transaction dataset does not include loan characteristics of the houses, such as the LTV ratio or the monthly instalment. To study the first stage, we explore our representative mortgage level dataset including the LTV ratio and the monthly instalment between 1996 and 2010. Since the exposure of house buyers to the LTV limit change was determined both by whether they bought houses before or after policy change and by housing prices and dimensions, we can define our treatment and controls groups based on

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[11]USD-SGD exchange rate ranged from 1.3479 – 1.8515 during this period.

[12]Our DD strategy is slightly different from a textbook DD strategy since the exposure (housing prices and dimensions) to the LTV limit change is determined by individuals. Some empirical papers use this type of DD strategy when the treatment after policy is potentially determined by individuals, and then check the potential biases against a textbook DD strategy (Duflo 2000,Melzer 2011,Kleven et al,2014). For example, individuals can potentially migrate to treated areas after policy changes, so estimated effects might, in fact, be migration effects. Following this literature, we first use the DD strategy to estimate the treatment effects then check the potential biases.

housing prices and dimensions. The control group comprises of those who purchase relatively cheap (or small) houses. In contrast, the treatment group comprises of those who purchase relatively more expensive (larger) houses.

[Insert Figure III]

Figure III presents the impact of the LTV limit increase on actual LTV. For those houses above median prices in the same year within the same region in Singapore, we observe the bunching around 80% but not 90% before the LTV limit increase in 2005 (Panel A). However, we observe more bunching around 80% and 90% after the LTV limit increase in 2005 (Panel B). For those houses below median prices in the same year within the same region in Singapore, we observe similar patterns (Panel C and D). These figures suggest that the LTV limit increase did change borrowers' behaviors and make more home buyers borrow at the higher limit.

To analyze the first stage, we study the impact of the LTV limit increase on the actual LTV ratio and the monthly instalment by carrying out a differences-in-differences analysis for the control and treatment groups before and after the LTV limit increase. Our estimation equation is:

$$Y_i = \delta T_i + \beta T_i \cdot I_t^{post} + \sum_c \tau_c I_c + \sum_t \gamma_t I_t + \sum_r \varphi_r I_r + \varepsilon_{it} \qquad (1)$$

where i indexes an individual and r indexes the region in which an individual resides. T_i is an indicator for the treatment and takes on a value of 1 if the residential property that the individual has purchased is above the 50th percentile of housing prices in thesame year within the same region or if the residential property is above the 50th percentile of housing dimensions in the same year within the same region. I_t^{post} is an indicator variable that takes on a value of 1 if the house is purchased after the LTV limit increase in 2005. The equation also includes social economic variables such as the purchaser's gender, ethnicity, age when the house is purchased, and cohort, year and region-fixed effects, $\sum_c I_C$, $\sum_t I_t$ and $\sum_r I_r$.

The outcome of interest, denoted as Y_i, is the LTV ratio or the monthly instalment. The coefficient of interest in equation (1) is β, which is the estimated impact of the LTV limit increase on the LTV ratio or the monthly instalment. A positive coefficient for $T_i \cdot I_t^{post}$, for example, indicates that for an individual who has purchased a property priced above the 50th percentile, the increase in LTV limit increases the LTV ratio and the monthly instalment.

[Insert Table II]

Table II presents the first stage results: the impact of 2002 and 2005 policy changes on the LTV ratio and the monthly instalment. While the LTV of both those who bought relatively expensive houses and cheap houses increased after the 2005 policy, in column 1 and 2, we show that the increase in LTV of those who bought relatively expensive houses is 4.5%-5% larger. In column 3 and 4, we find that the increase in monthly instalment is 460-800 dollar larger for those who bought relatively expensive houses, about 18% to 30% from the mean. This implies a larger increase of debt burden after the LTV limit increase[13]. However, column 5 to 8 show that the 2002 policy had no effects on the LTV ratio and the monthly instalment. These results suggest that the 2005 policy did have a strong first stage impact on the LTV ratio and the monthly instalment, while the 2002 policy did not. We use the 2002 policy as a control experiment in analysis later.

III.B. Baseline Estimates on Personal Bankruptcy

The basic idea behind the identification strategy can be illustrated using simple two-by-two tables and raw bankruptcy data. Table II shows the proportion of personal bankruptcy by housing prices and housing dimensions. We use two proxies to measure the exposure of house buyers to the LTV limit increase. The first proxy is whether the housing price is above the 50th percentile of housing prices in the same year within the same region. The second proxy is whether the housing price is above the 50th percentile of housing dimensions in the same year within the same region.

[Insert Table III]

In Panel A, we compare the change in bankruptcy rates after the LTV limit increase in 2005 between the house buyers above the 50th percentile of housing prices and those below it. We also conduct a similar analysis between the house buyers above and below the 50th percentile of housing dimensions. In both cases, the DD estimates are about 0.12 percentage points. These differences-in-differences are not significantly different from 0. This simple estimator suggests that compared with purchasing a relatively cheap house, purchasing a relatively expensive house after the increase in LTV limit increases the likelihood of becoming bankrupt by 0.12 percentage points. In Panel B, we also conducted the control DD experiment on the policy change on Sept 1, 2002 that allowed for the use of CPF savings to finance up to ten percent of the price of the property but maintained the same LTV limit. We find that the DD estimates are much smaller with

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[13]Table A.2 presents a robustness check for Table II using quartiles of the housing price as treatment cutoffs. The results are also consistent with Table II, the first stage impacts on the LTV ratio and the monthly instalment are increasing as the percentile of housing price is increasing (Column 5 and 6).

relatively large standard errors and are not statistically significant. We will use a regression framework to analyze the basic idea in the reminder of Section III.

The main empirical strategy involves using a differences-in-differences (DD) technique. We study the impact of the LTV limit increase on bankruptcy rates by carrying out a differences-in-differences analysis for the control and treatment groups before and after the LTV limit increase. We use the specification similar to equation (1):

$$Y_i = \delta T_i + \beta T_i \cdot I_t^{post} + \sum_t \alpha_t I_t \cdot X_i + \sum_c \tau_c I_c + \sum_t \gamma_t I_t + \sum_r \varphi_r I_r + \varepsilon_{it} \qquad (2)$$

Since the housing transaction dataset has more information about housing dimensions and housing types, in equation (2), we add X_i represent control variables including total price and dimensions of a house, whether the sale type is resale, whether the house is a private apartment or condominiums, and whether the tenure is freehold. I_t is an indicator variable for the year-fixed effects. Therefore, $I_t \cdot X_i$ is characteristics specific to a residential property interacted with year fixed effects. The outcome of interest, denoted as Y_i, is an indicator variable for whether an individual will be declared bankrupt after purchasing a house. The coefficient of interest in equation (1) is β, which is the estimated impact of the LTV limit increase on personal bankruptcy rate. A positive coefficient for $T_i \cdot I_t^{post}$, for example, indicates that for an individual who has purchased a property priced above the 50th percentile, the increase in LTV limit increases the likelihood of personal bankruptcy after they buy the house. Since we do not observe the actual LTV ratio for each housing transactions, our estimation is an Intent-to-Treat (ITT) estimation.

[Insert Table IV]

Table IV reports the estimation results of equation (1). The sample is restricted to housing transaction between May 15, 1996 and February 20, 2010. During this period, some house buyers who buy multiple houses or file multiple bankruptcies. We further restrict our sample to those who buy only one house and file at most one bankruptcy during this period. We will analyze those who buy multiple houses in Table XIV. Columns 1-4 report estimates from an OLS regression and columns 5-8 report marginal effects from a logistic regression. The interest variables are the interaction terms as their coefficients reflect the impact of the LTV limit increase in 2005. In all odd columns (i.e., columns 1, 3, 5 and 7), we include year, region and cohort-fixed effects but not interaction terms between year-fixed effects and control variables. In all even columns (i.e., columns 2, 4, 6 and 8), we include both year, region and cohort-fixed effects as well as interaction terms between

year-fixed effects and control variables. Standard errors are clustered at 82 postal sectors.[14]

In column 1, we find that compared with purchasing a relatively cheap house, purchasing a relatively expensive house after the increase in LTV limit increases the likelihood of becoming bankrupt by 0.15 percentage points. The coefficient is significant at the 5% level. When we include interaction terms between year-fixed effects and control variables in column 2, the coefficient increases to 0.20 percentage points and is significant at the 1 percent% level. Since the average bankruptcy rate after purchasing a house is 0.41 percentage points, our point estimates suggest that the magnitude of impact is about a 37 to 49 percent increase in bankruptcy. In column 3, we find that compared with purchasing a relatively small house, purchasing a relatively large house after the increase in LTV limit increases the likelihood of becoming bankrupt by 0.11 percentage points. When we include interaction terms between year-fixed effects and control variables in column 4, the coefficient increases to 0.18 percentage points and is significant at the 5% level. Our point estimates suggest that the magnitude of impact is about a 27 percent to 44 percent increase in bankruptcy.

The above results prove robust when we use a logistic regression in columns 5-8. In column 5, the marginal effect is 0.19 percentage points and it is significant at the 10% level. When we include interaction terms between year-fixed effects and control variables in column 6, the coefficient is 0.29 percentage points and is significant at the 1% level. In columns 7 and 8, we find that compared with purchasing a relatively small house, purchasing a relatively large house after the increase in LTV limit increases the likelihood of becoming bankrupt by 0.25-0.27 percentage points. Both estimated coefficients are significant at the 1% level. The magnitudes of these estimates in the logistic regression are similar to or larger than those in the OLS regression. In the reminder of paper, we only report results from OLS regressions.

We test the robustness of Table IV in Appendix Tables A.1 to A.3. In Table IV, we use the median housing prices/dimensions in the same year within the same region as treatment cutoffs. Table A.1 presents a robustness check for Table IV using three alternative treatment cutoffs: median housing prices/dimensions in the same year, median housing prices/dimensions in the same sale type in the same year, and median housing prices/dimensions in the same property type in the same year. We find that the results are consistent with Table IV when using alternative treatment cutoffs. Table A.2 presents a robustness check for Table IV using quartiles of the treatment index as treatment cutoffs.

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[14]In Singapore, the postal code consists of 6 digits: the first two numbers represent the postal sector whereas the remaining four numbers define the delivery point within the sector. In other words, each building has a unique postal code.

The results are also consistent with Table IV, although the relationship between housing price/dimensions and personal bankruptcy is not linear. In Table IV, we focus on single house buyers. Table A.3 presents robustness check for Table IV using both single house buyers and multiple house buyers. For multiple house buyers, we use the first house purchase as the date of house transaction. The results are also consistent with Table IV.

In sum, our estimation in Table IV show a consistent and robust result that the increase in LTV limit increases the personal bankruptcy rate for those who have more exposure to the policy change if they buy houses after 2005.

We then analyze the heterogeneous effects of the LTV limit increase on bankruptcy based on sale type, housing type, and tenure type. To make the treatment group and control group comparable, we define the treatment indicator T_i so that it is takes on a value of 1 if the residential property that the individual has purchased is above the 50th percentile of housing prices/dimensions in the same year within the same sale type, housing type or tenure type.

[Insert Table V]

The results are reported in Table V. Columns 1-4 report estimates of houses purchased by sale type. Columns 5-8 report estimates of houses purchased by housing type. Columns 9-12 report estimates of houses purchased depending on whether it's a freehold property. We find that most of the coefficients of the interaction terms are still positive, but some are not significant since the sample size is reduced when we analyze different types of houses. The coefficients of interaction terms for a resale property are positive and significant at the 1% level. The magnitude of the coefficients for resale property is larger than those for new sale. The coefficients of the interaction terms for a condominium or private apartment are generally larger in magnitude than those for landed property or others. The coefficients of the interaction terms for a freehold property are larger in magnitude than those for a property that is not freehold. Therefore, the impact of the LTV limit increase is stronger for resale housing, condominium and private apartment, and freehold houses.

III.C. Placebo Test

The identification of DD strategy replies on the common trend assumption. In our setting, we need to show there are no differential trends in personal bankruptcy before the LTV limit increase in 2005. With the rich dataset of housing transactions from 1996, we can test in a systematic manner by creating several placebo policy changes in each year from 1998 to 2005. For each placebo policy year, we restrict our sample to three years before and three years after leaving the sample size similar before and after. In this case, the

sample used in each test comprises data for sequential six-year periods. We also add the real policy date, July 19, 2005. We interact the placebo policy change with the treatment and conduct the placebo DD estimation for each placebo policy year. As we move the placebo policy year from 1998 to 2005, the coefficients of the interaction term should be zero when the placebo policy year is far before 2005, but become positive and significant when the placebo policy year is close to the actual year of the LTV limit increase (specifically, 2005). In this case, we can conclude that there are no differential trends in personal bankruptcy before the LTV limit increase in 2005, and our observed effects are due to the LTV limit increase.

[Insert Table VI]

The estimates of the interaction terms are reported in Tables VI. We find that most of the coefficients are insignificant and small in magnitude before 2005. However, in 2005, the estimated coefficients are significant at the 1% level and their magnitudes increase sharply relative to those before 2005. The magnitudes are similar to the estimation from Table IV. Therefore, the results from the placebo tests suggest our DD strategy is valid: there are no differential trends in personal bankruptcy before the LTV limit increase in 2005, and our observed effects are due to the LTV limit increase.

IV. Possible Explanations

Section III shows a consistent and robust pattern that the increase in LTV limit increases personal bankruptcy rates for those who have more exposure to the policy change if they buy houses after 2005.   In this Section, we further investigate factors which potentially contribute to the observed results. There are two possible sources. The first is the composition effect. There are several possible composition effects after the policy change. For example, an increase in LTV limit encourages existing private house buyers to buy more expensive houses than they could afford before (selection into our treatment group). Households might also switch from buying subsidized public houses or renting houses to buying private houses (selection into our sample). The second is the debt burden effect: after the increase of LTV limit, house buyers can borrow more given the same house value but pay a larger monthly payment. This increases the burden of monthly payments and makes the buyer more vulnerable to bankruptcy.

IV.A. Composition Effect

The first possible explanation is the composition effect. As it is described in Figure I, in 2005, the MAS increased the LTV limit from 80 to 90 percent and reduced the minimum cash payment from ten to five percent. The policy increased access to housing credit and

encouraged house buyers to buy more expensive house than they could afford before. For example, suppose a house buyer has $100,000 cash; she can only buy a house with value no more than $1 million since the minimum cash payment is 10 percent before the LTV limit increase. However, after the LTV limit increase, with the same $100,000 cash she can buy a house with value up to $2 million. It is possible the observed effects are due to that house buyer choice: buyers choose to buy more expensive houses, borrowing too much from banks and making themselves vulnerable to bankruptcy.

We cannot test the composition effect directly because we only observe the houses they buy and do not observe the counterfactual: which houses would they buy without the LTV limit increase. Instead, we test the composition effect indirectly using the 2002 housing policy change as a control experiment.

On Sept 1, 2002, while maintaining the LTV at 80%, the MAS reduced minimum cash payment from 20 to ten percent of housing value. After this change, savings in the Central Provident Fund (CPF) could be used for the remaining ten percent of housing value. Since LTV limit did not change in 2002, house buyers could borrow more given the same house value. Because the minimum cash payment was reduced from 20 to ten percent of housing value, house buyers could buy more expensive houses than they could afford before. And households might also switch from buying subsidized public houses or renting houses to buying private houses. If composition effect is the factor contributing to our effects, we should observe the 2002 policy change having similar effect as 2005 policy.

We use the similar DD strategy to estimate the impact of 2002 housing policy change on personal bankruptcy. The regression is similar to Equation (1) with two exceptions. First, I_t^{post} is an indicator variable that takes on a value of 1 if the house is purchased after the minimum cash payment decrease in 2002. Second, we restrict our sample before July 19, 2005, so that our results will not be contaminated by other policy changes.

[Insert Table VII]

Table VII presents the impact of the minimum cash payment decrease on personal bankruptcy. We restrict our sample from May 15, 1996 to July 19, 2005 in Columns 1-4. To check for robustness, in columns 5-8 we also restrict our sample to three years before and three years after the 2002 policy change, leaving the sample size similar before and after 2002. We find that the coefficients of interaction terms are close to zero and insignificant. These results suggest that composition effect is unlikely to be the main source of the observed pattern.

IV.B. Debt Burden Effect

Another explanation of our results is the debt burden effect. Since the increase of the LTV limit in 2005, house buyers can borrow more given the same house value but pay a larger monthly payment. This increases the burden of monthly payments and makes house buyers vulnerable to bankruptcy. That the debt burden effect is different from the composition effect because even there is no change in housing choices, the debt burden might increase when they borrow more from banks.

To test the debt burden effect, we use identifiers to merge our housing and personal bankruptcy datasets to a dataset with more than 532,000 bankruptcy lawsuits in Singapore. For each case, the record includes the filing date and the names and personal identifiers of the plaintiffs and defendants. The record also includes the nature of lawsuits such as credit reason. Thus we know who are involved in the credited related lawsuits and whether the case is before or after they buy the house. We use whether the house buyer is a defendant in a credit related lawsuit after they buy a house as an indicator for debt burden. We have two strategies to test the debt burden effect.

First, we use a similar DD strategy to estimate the impact of the LTV limit increase in 2005 on being a defendant in a credit-related lawsuit after buying a house. Those who have a large debt burden are more likely to involve defaults in credit cards or overdraft. Thus they are more likely to become a defendant due to credit. We run a regression similar to Equation (1) except that the dependent variable is whether the house buyer is a defendant in a credit-related lawsuit after they buy a house.

[Insert Table VIII]

We present the results in Panel A, Table VIII. Column 1 and 2 show that, compared with purchasing a relatively cheap house, purchasing a relatively expensive house increases the likelihood of becoming a defendant in a credit-related lawsuit by 0.15-0.20 percentage points. Column 3 and 4 show that, compared with purchasing a relatively small house, purchasing a relatively large house increases the likelihood of becoming a defendant in a credit-related lawsuit by 0.20-0.22 percentage points. Since the average rate of being a defendant in a credit related lawsuits after purchasing a house is 0.86 percentage points, our estimate suggests that the magnitude of impact is about a 17 to 26 percent increase. Thus, the results are consistent with the debt burden effect: the increase in LTV limit increases the likelihood of being defendants in credit-related lawsuits for those who have more exposure to the policy change if they buy houses after 2005. Our results are consistent with Keys et al (2014)'s findings that a sizable decline in mortgage payments induces a significant drop in mortgage defaults using the US housing loan data.

To strengthen our identification of the debt burden effect, we explore the rich lawsuits dataset with a falsification test. The lawsuits dataset includes information about both

defendants and plaintiffs. The debt burden effect predicts that the increase in LTV limit increases the likelihood of being defendants in the credit related lawsuits but will not change the likelihood of being plaintiffs in the credit related lawsuits. We conduct a falsification test with the same specifications as the previous test, except that that the dependent variable is substituted with whether the house buyer is a plaintiff in a credit related lawsuit after they buy a house. The results of falsification test are reported in Panel B, Table VIII. We find that the coefficients of interaction terms are positive and insignificant. The magnitudes are much smaller than those in Panel A. These results are consistent with the debt burden effect and strengthens our identification.

The identification produced by the DD strategy relies on the common-trend assumption. In our setting, we need to show there are no differential trends in credit related lawsuits before the LTV limit increase in 2005. We test this a systematically by creating several placebo policy changes in each year from 1998 to 2005.   The estimates of the interaction terms are reported in Appendix Table A.4. We find that all the coefficients are insignificant and small in magnitude. Therefore, the results from the placebo tests suggest our DD strategy is valid: there are no differential trends in credit-related lawsuits before the LTV limit increase in 2005, and our observed effects are due to the LTV limit increase.

To test the debt burden effect, our second strategy is to estimate the heterogeneous effects of the LTV limit increase on personal bankruptcy as represented by whether house buyers are defendants in credit-related lawsuits. If the increase in the LTV limit increases the debt burden, those buyers who have more exposure to the policy change will become more vulnerable to personal bankruptcy. We use the lawsuits data to split our sample into two subsamples: those who those who are defendants in credit-related lawsuits after buying houses, and those who are not. The debt burden effect predicts that the impact of the LTV limit increase on personal bankruptcy is larger for those who are defendants in credit-related lawsuits after buying houses compared to those who are not. We use a specification with triple interactions to test this hypothesis in Equation (2).

$$
\begin{aligned}
Y_i &= \delta_1 T_i + \delta_2 C_i + \delta_3 T_i \cdot C_i + \delta_4 C_i \cdot I_t^{post} + \beta_1 T_i \cdot I_t^{post} + \beta_2 T_i \cdot C_i \cdot I_t^{post} \\
&+ \sum_t \alpha_t I_t \cdot X_i + \sum_c \tau_c I_c + \sum_t \gamma_t I_t + \sum_r \varphi_r I_r + \varepsilon_{it}
\end{aligned}
\tag{2}
$$

where C_i is an indicator variable that equals one if individual i is a defendant in a credit related lawsuit; other variables have the same meanings as in Equation (1). We mainly add the triple interaction term among three variables: whether house buyers have more exposure to the policy change, whether house buyers are defendants in credit related lawsuits, and whether they buy houses after the LTV limit increase. In this specification. β_1 measures the impact of LTV ratio increase on personal bankruptcy for those who are

not defendants in a credit-related lawsuit. Our main coefficient of interest is the coefficient of triple interaction term- β_2 , which measures the different impact of LTV ratio increase on personal bankruptcy between those who are not defendants in a credit-related lawsuit and who are not. The debt burden effect predicts that β_2 is positive.

[Insert Table IX]

We presents the results from Equation (2) in Panel A, Table IX. We use the sample from May 15, 1996 to February 20, 2010. In columns 1 and 2, we use housing price to measure exposure to the policy change. We find that the impact of the LTV limit increase in 2005 is 11 percent greater for those who are defendants in credit-related lawsuits than for those who are not. The coefficients are significant at the 5% level. In columns 3 and 4, we use housing dimensions to measure exposure to the policy change. We find that the impact of the LTV limit increase in 2005 is 8.4 percentage points larger for those who are defendants in credit- related lawsuits than for those who are not. The coefficients are significant at the 10% level. Therefore, the results from Panel A are consistent with the debt burden effect.

We also conduct a falsification test similar to Panel B, Table VIII, on these results We use the same specification as Equation (2) except that C_i is an indicator variable that equals one if individual i is a plaintiff in a credit-related lawsuit after they buy a house. The results of falsification test are reported in Panel B, Table IX. We find that the coefficients of triple interaction terms are not robust and insignificant. The magnitudes are much smaller than those in Panel A. These results are consistent with the debt burden effect and strengthens our identifications.

In sum, we find that the increase in LTV limit increases the likelihood of being defendants in a credit-related lawsuit for those who have more exposure to the policy change if they buy houses after 2005. Moreover, the impact of the LTV limit increase on personal bankruptcy is larger for those who are defendants in credit-related lawsuits after buying houses than for those who are not. The results from the above two strategies suggest that the debt burden effect is the primary source of the observed pattern.

IV.C. Selection for Irresponsible Buyers

An important concern is selection for irresponsible house buyers. In our sample, there might be irresponsible house buyers who will become bankrupt no matter what house they are buying. Since the house buyer can choose their housing prices and dimensions, it is possible that irresponsible house buyers are more likely to buy an expensive or large house after the LTV limit increase in 2005. In this case, finding a positive treatment effect would be a spurious outcome of an omitted variable about type of house buyers. We have

two strategies to test this issue: the first is to test selection on observables, and the second is to use observables to estimate the simulated exposure to the LTV limit increase.

First, we estimate the selection on observables. We use a similar DD strategy to estimate the impact of the LTV limit increase on five observables that might correlate with the type of house buyers: whether they file bankruptcy before buying houses, whether they are defendants in credit-related lawsuits before buying houses, gender, ethnicity and age cohort. We run five separate regressions similar to Equation (1) except that the dependent variables are the above-mentioned five variables. The basic idea can be explained using the first variable as an example. Coefficients of interaction terms that are positive and significant suggest that those who file bankruptcy before buying houses are more likely to buy a relatively expensive or large house after the LTV limit increase. In contrast, if the coefficients are negative and significant, it suggests that those who file bankruptcy before buying houses are more likely to buy a relatively cheap or small house after the LTV limit increase. If the coefficients are not significantly different from zero, it suggests that the tendency to buy expensive or large houses are similar before and after the LTV limit increase for those who file bankruptcy before buying   houses. Since whether they file bankruptcy before buying houses is an indicator for irresponsible buyers, the zero coefficients suggest there is no evidence of selection for irresponsible buyers.

[Insert Table X]

Table X presents the results of the five regressions with each dependent variable in a different panel. We find that most coefficients of interaction terms are close to zero and insignificant. There is only one positive and significant result in Panel D, suggesting that ethnic Chinese descent are more likely to buy expensive or large house after the LTV limit increase. However, the result is not robust across different specifications. Therefore, the results in Table X suggest that there is no evidence that those who buy relatively more expensive houses after the LTV limit increase in 2005 are selected to be more irresponsible.

Second, we use the housing buying pattern before the LTV limit increase in 2005 to estimate the simulated housing prices and dimensions that house buyers would buy. The estimation equation is as follows:

$$exposure_i = \delta X_i + \sum_c \tau_c I_c + \sum_t \gamma_t I_t + \sum_r \varphi_r I_r + \varepsilon_{it} \qquad (3)$$

where the dependent variable is the exposure to the LTV limit increase, which is either housing price or housing dimension. Other variables have the same meanings as in Equation (1). We restrict our sample from May 15, 1996 to July 19, 2005. We estimate all coefficients in Equation (3) and use the coefficients to predict the simulated exposure to

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the LTV limit increase, $exposure_i$, in the sample from July 19, 2005 to February 20, 2010. The simulated exposure is thus based entirely on the observables and the house buying pattern before the LTV limit increase. Under the assumption that the housing buying pattern does not change before and after the LTV limit increase, house buyers should buy the houses at the simulated housing prices or simulated housing dimensions after the LTV limit increase.

We use a similar DD strategy to estimate the impact of the LTV limit increase in 2005 on personal bankruptcy but use simulated housing prices (dimensions) instead of actual prices (dimensions). If selection for irresponsible buyers is the main issue, we should observe that the estimated results with simulated prices (dimensions) are close to zero and different from those results with actual prices (dimensions). We run a regression similar to Equation (1) except that the definition of the treatment and control groups change. T_i takes on a value of 1 if the simulated housing price is above the 50th percentile of simulated housing prices or dimensions in the same year within the same region.

[Insert Table XI]

We presents the results in Table XI. Columns 1 to 4 report estimates from OLS regressions and columns 5 to 8 report marginal effects from a logistic regression. Column 1 to 4 show that, compared with purchasing a (simulated) cheap house, purchasing a (simulated) expensive house after the increase in LTV limit increases the likelihood of becoming bankrupt by 0.17-0.25 percent. Compared with purchasing a (simulated) small house, purchasing a (simulated) large house after the increase in LTV limit increases the likelihood of becoming bankrupt by 0.12-0.18 percentage points. Columns 5 to 8 also show positive and significant results, and the magnitudes are similar to Columns 1 to 4.

In sum, the results in Table XI show a consistent and robust pattern that the coefficients of interaction terms with the simulated housing prices (dimensions) are positive and significant. The magnitudes are similar to the estimation with actual housing prices (dimensions) in Table IV. The results are not consistent with the predictions from selection for irresponsible buyers. Therefore, both strategies suggest that selection on irresponsible buyers is unlikely to be an important issue.

IV.D. Interest Rate

Another concern is that since personal bankruptcy is generally not an immediate effect of a policy, there might be other changes in policy or Singapore economy that have contributed to bankruptcy rates. For example, it is possible that increase in bankruptcy rate is due to the increase in interest rates of housing loan rather than the LTV limit

increase in 2005. This hypothesis predicts that given an LTV limit, when interest rate increases/decreases, personal bankruptcy of those who buy expensive house increases/decreases after the interest rate change.

The Singapore Interbank Offered Rates (SIBOR) are the benchmark for property loans in Singapore. They are determined by the demand and supply of funds existing in the Singapore interbank market. SIBOR are shared across multiple banks and hence are more open and transparent than other rates. The actual interest rate of a mortgage depends on home loan packages. Banks often quote the one or three-month SIBOR rates plus a spread value to be the actual interest rate.

[Insert Figure IV]

Figure IV describes the 3month SIBOR from 1995 to 2013.[15] The SIBOR decreased from 1998 to 2001, remain constant at low rates from 2001 to 2004, increased from 2004 to 2007 and then decreased after 2007. To separate the effect of interest rate from the LTV limit change, we need to find the period with fixed LTV limit and the cutoff years with large differences in interest rate levels before and after. Based on Figure IV, we find two sample periods: (1) from 1998 to 2004 and (2) from 2001 to July 19, 2005. The LTV limit remained 80 percent in both sample periods. In sample period (1), the SIBOR generally decreased until cutoff year 2001. In sample period (2), the SIBOR rate was less than one percent before 2004 and after July 1, 2004, it increased to one to two percent.

We use the results from Table VI to test the effect of interest rate in period (1). If housing loan interest rates affect personal bankruptcy, we should observe the coefficients of interaction terms to be negative and significant for cutoff years 1999 to 2001. However, the results are mostly not significant from zero and the sign of coefficients are not robust.

To study the sample period (2), we run a regression similar to Equation (1) except that the sample is restricted to period (2), and I_t^{post} is an indicator variable that takes on a value of 1 if the house is purchased after July 1, 2004. If housing loan interest rates affect personal bankruptcy, we should observe the coefficients of interaction terms to be positive and significant.

[Insert Table XII]

Table XII present the results for sample period (2). In columns 1 to 4, the dependent variable is an indicator variable for whether an individual will be declared bankrupt after

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[15]MAS Website, https://secure.mas.gov.sg/dir/domesticinterestrates.aspx

purchasing a house. We find that the coefficients of interaction terms are all insignificant and small in magnitude. In fact, they are mostly negative.

In sum, based on the study in the two sample periods, we find no evidence that an increase in bankruptcy rate is due to an increase in interest rates.

Why don't we see the effects of interest rate rise if the story of bankruptcy is about debt burden effects? We run the similar regression using the LTV ratio and the monthly instalment as dependents variables. The results are presented in Column 5 and 6. Although the interest rate is increasing, but we do not find evidence that the LTV ratio or the monthly instalment are increasing more for those who bought relatively expensive houses. The evidence in Table XII shows that when the increase of interest rate does not increase the monthly instalment, personal bankruptcy does not change. Recall Table II and IV show that when the increase of LTV limit increases the monthly instalment, and thus debt burden, personal bankruptcy increases. Therefore, these results further support our story that debt burden is the main channels of personal bankruptcy.

IV.E Discussion

Another alternative explanation is that the observed effects are not due to the policy change in LTV but other shocks, such as global recession after 2008. For example, it is possible that higher-income households bought expensive houses and they suffered more from asset price declines in the global recession or suffered greater increase in unemployment risk. In our design, we compare those who buy relatively expensive houses and relatively cheap houses, before and after the policy change. However, global recession should affects higher-income household no matter whether they bought houses before or after the policy change in 2005. If only global recession plays a role, we should observe no effects in differences-in-difference estimation. Thus, the effects are unlikely to be due to other shocks as global recession.

It is important to understand whether the results are driven by speculators or owner - occupied house buyers. If the results are driven by speculators, the policy implication is to fight against speculation to reduce personal bankruptcy. However, if the results are driven by owner-occupied house buyers, policy makers need to reconsider the welfare implication of housing credit policies. To test whether the results are driven by speculators, we use two proxies: whether house buyers sell the house in a sub-sale, i.e., the sale of a unit before the government issues the temporary occupation permit by someone who bought the unit from the developer, and whether house buyers buy multiple houses from 1996 to 2012. If house buyers sell the house with a sub-sale to earn profit from housing price increase, they are likely to be speculators. We restrict our sample to

speculators and use the similar DD strategy to estimate the impact of the LTV limit increase in 2005 on personal bankruptcy.

[Insert Table XIII]

Table XIII presents the results for the speculator sample. Columns 1 to 4 show the results for those who sell a house with sub-sale. The coefficients of the interaction term are insignificant and the signs are not robust. Columns 5 to 8 show the results for those who buy multiple houses. The results are also insignificant and the signs are not robust. Moreover, the magnitudes are much smaller than the Table IV sample of single house buyers. The results in Table XIII, together with the results in Table IV, suggest that the main results are not driven by speculators but owner -occupied house buyers.

V. Personal Bankruptcy and Selling behavior

We focus on the impact of the LTV limit increase on personal bankruptcies in previous sections. In this section, we investigate the relationship between personal bankruptcy and house-selling behaviour. We use OLS regressions to investigate the correlation between personal bankruptcy and houses sale. The estimation equation is as follows:

$$Y_i = \beta \cdot bankruptcy + \delta X_i + \sum_t \alpha_t I_t \ X_i + \sum_c \tau_c I_c + \sum_t \gamma_t I_t + \sum_r \varphi_r I_r + \varepsilon_{it} \qquad (4)$$

where the dependent variable is whether individual sells a house or the return from the housing sale. The return is defined as the selling price minus buying price, divided by the buying price. *bankruptcy* indicates whether the individual declares bankruptcy. Other variables have the same meanings as in Equation (1).

[Insert Table XIV]

Table XIV presents the estimation results from Equation (4). Since we are considering house-selling behavior, we include multiple house buyers in the sample. Panel A reports the correlation between personal bankruptcy and selling behavior, and Panel B reports the correlation between being a defendant in a credit-related lawsuit and selling behavior. In Column 1, the dependent variable is whether an individual sell a house. In Column 2 and 3, the dependent variable is the natural log of housing price. In Column 4 and 5, the dependent variable is the return from the house sale. In columns 2 and 4, we restrict the sample to those who sell houses. In Column 3 and 5, we further exclude the sample who sell houses three years before or after the personal bankruptcy. In Panel A Column 1, we

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find that home owners are about 9.0 percentage points more likely to sell their houses if they become bankrupt. The coefficients are significant at the 1% level.

Why are home owners more likely to sell houses when they are involved in bankruptcy? One possibility is that the bankruptcy law requires the house to vest in the Official Assignee and be sold after bankruptcy. Another possibility is that home owners know their financial burden and bankruptcy prospects; therefore, they sell houses even before bankruptcy to smooth consumption and mitigate negative shocks.

We find that for those who sell houses and become bankrupt, 57.4 percent sell their houses before becoming bankrupt. In Panel A column 2, we show that those who sell their houses before becoming bankrupt have a 12.3 percent price discount compared to those who do not file for bankruptcy. Those who sell their houses after becoming bankrupt have and 8.7 percent price discount compared to those who do not become bankrupt. The coefficients are significant at the 1% level. The results suggest that people sell houses before bankruptcy to smooth consumption and mitigate negative shocks. Houses that were sold long before bankruptcy might have been sold for reasons totally unrelated to the bankruptcy. Hence, we further investigate the return of house sale near the date of bankruptcy. When we exclude the sample who sell houses three years before or after the personal bankruptcy, price discount increases to 15.7 and 16.0 percent (column 3), respectively. The results are similar when we use return from the housing sale as dependent variables in columns 4 and 5.

In Panel B, we study the correlation between being a defendant in a credit-related lawsuit and selling behavior. We find similar results as in Panel A. Home owners are about 5.6 percentage points more likely to sell houses if they are defendants in credit-related lawsuits. Those who sell their houses before the credit-related lawsuits have an 9.3 to 12.1 percent price discount as compared to those who are not involved in the lawsuits. The results suggest that people sell houses before lawsuits to smooth consumption and mitigate negative shocks.

Our findings are generally consistent with the theory of price discount from forced sale (Campbell et al. 2011) and add to literature about the illiquidity in the housing market. However, our results are slightly different. Campbell et al. (2011) find that price discounting before bankruptcy is close to zero and price discounting is negative and larger after bankruptcy. In this paper, we find that some home owners sell houses even before bankruptcy with a very low return. These results are consistent with the second possibilities of low return from forced sale: home owners knows their financial burden and bankruptcy prospect, they sell houses even before bankruptcy to smooth consumption and mitigate negative shocks. These results are also consistent with the results and predictions in Chetty and Szeidl (2007) about commitment consumption.

VI. Conclusions

In this paper we study the impact of housing credit on personal bankruptcy. The question is difficult to answer due to both lack of data and research design. We use precise identifiers to merge a dataset with over 150,000 housing transactions from 1995 to 2012 and a personal bankruptcy dataset in Singapore. Our identification strategy is a differences-in-differences (DD) approach. We show that the increase in LTV limit in 2005 increases the actual LTV of those who bought relatively expensive houses by 4.5%-5% larger. The policy change in 2005 increases the monthly instalment of those who bought relatively expensive houses by 460-800 dollar larger, about 18% to 30% from the mean. More importantly, we find that, compared with purchasing a relatively cheap house, purchasing a relatively expensive house after the increase in LTV limit increases the likelihood of becoming bankrupt by 0.15-0.20 percentage points. Compared with purchasing a relatively small house, purchasing a relatively large house after the increase in LTV limit increases the likelihood of becoming bankrupt by 0.11-0.18 percentage points. The results are robust with various control variables and flexible interaction terms. Our placebo test show that our research design is valid, we also find that there are no differential trends in personal bankruptcy before the LTV limit increase in 2005.

Next, we identify potential factors that contributing to housing credit and personal bankruptcy. We show that the debt burden effect is the main reason for the increased bankruptcy levels. Specifically, we find that, compared with purchasing a relatively cheap house, purchasing a relatively expensive house increases the likelihood of becoming a defendant in a credit related lawsuit by 0.15-0.20 percentage points. Thus, the results are consistent with the debt burden effect.

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Table I. Summary Statistics

	Single Property Buyers			Multiple Property Buyers
	Price below 50th Pct	**Price above 50th Pct**	**p-value (means)**	**All**
Male	0.49	0.49	0.31	0.53
Chinese	0.93	0.93	0.00	0.95
Condominiums	0.67	0.57	0.00	0.57
Private Apartments	0.26	0.12	0.00	0.22
Freehold	0.31	0.57	0.00	0.48
New Sale	0.51	0.56	0.00	0.52
Buyers' Age at First Purchase (In Years)	40.20	42.28	0.00	41.11
Mean Price of Property	606792	1219679	0.00	1009134
Mean Size of Property Purchased (In Sq. Metres)	109.25	188.13	0.00	158.93
Single Bankruptcies	0.0081	0.0077	0.55	0.0065
Multiple Bankruptcies	0.0008	0.0013	0.02	0.0009
Obs	56044	46916		41614

Note: The results presented in this table are obtained using data from May 15, 1996 to February 20, 2010. The summary statistics for both 102,960 single property buyers and 41,614 multiple property buyers are reported. For single house buyers, two sub-samples based on whether the housing price is above the 50th percentile of housing prices in the same year within the same region are reported. For multiple property buyers, the combined statistics of all buyers are reported.

Table II. The Impact of the policy changes on the LTV Ratio and the Monthly instalment

Specification:	OLS Regression							
Policy Date	July 19, 2005				September 1st, 2002			
Sample:	All Sample				Year 1996 - 2005			
Dep. Var.:	LTV		Monthly Instalment		LTV		Monthly Instalment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Indicator for houses over 50th percentile of housing price × Indicator after policy	5.079***	4.4933***	799.381***	458.4969***	3.013	2.2380	-0.534	134.1389
	(1.259)	(1.051)	(163.705)	(136.927)	(2.384)	(2.176)	(179.908)	(148.970)
Obs.	3686	3686	3686	3686	1520	1520	1520	1520
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Control variables for loan characteristics	N	Y	N	Y	N	Y	N	Y

Note: Columns 1 to 4 present the impact of 2005 policy change using data from May 15, 1996 to February 20, 2010. Columns 5 to 8 present the impact of 2002 policy change using data from May 15, 1996 to July 19, 2005. The dependent variable in column 1, 2, 5 and 6 is loan-to-value ratio. The dependent variable in column 3, 4, 7 and 8 is monthly instalment. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All columns include year, region and cohort fixed effects.

Table III. Proportion of Personal Bankruptcy by Housing Prices and Housing Dimensions

	Proportion of Personal Bankruptcy					
	Housing Prices			Housing Dimensions		
	Above 50th Pct	Below 50th Pct	Difference	Above 50th Pct	Below 50th Pct	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest						
Before 19 July 2005	0.0059	0.0066	0.0007	0.0062	0.0064	0.0001
			(0.0006)			(0.0006)
After 19 July 2005	0.0023	0.0019	-0.0005	0.0026	0.0016	-0.0011
			(0.0005)			(0.0005)
Difference	0.0036	0.0048	0.0012	0.0036	0.0048	0.0012
	(0.0006)	(0.0006)	(0.0009)	(0.0006)	(0.0006)	(0.0009)
Panel B: Control Experiment						
Before 1 Sept 2002	0.0072	0.0083	0.0011	0.0078	0.0077	-0.0001
			(0.0009)			(0.0009)
After 1 Sept 2002	0.0043	0.0048	0.0004	0.0044	0.0047	0.0003
			(0.0008)			(0.0008)
Difference	0.0028	0.0035	0.0007	0.0034	0.0030	-0.0004
	(0.0009)	(0.0009)	(0.0013)	(0.0009)	(0.0009)	(0.0013)

Note: The proportion of personal bankruptcy by housing prices and housing dimensions are shown in this table. Two proxies are used to measure the exposure of house buyers to the LTV limit increase: whether the housing price is above the 50th percentile of housing prices in the same year within the same region and. it is above the 50th percentile of housing dimension in the same year within the same region. In Panel A, the change in bankruptcy rates after the LTV limit increase in 2005 was compared between the house buyers whose prices are above the 50th percentile of housing prices and of housing dimensions, and those whose price are below. In Panel B, the control DD experiment on the policy change in Sept 1, 2002 was conducted.

Table IV. The Impact of the LTV Limit Increase on Personal Bankruptcy

Dep. Var.:	Personal Bankruptcies							
Sample:	All Samples							
Specification:	OLS Regression				Logistic Regression			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Bankruptcy after purchase of house								
Indicator for houses over 50th percentile of housing prices	-.0010	-.0015			-.00080	-.0010		
	(.00064)	(.00070)**			(.00051)	(.00058)*		
Indicator for houses over 50th percentile of housing prices × Indicator after July 19, 2005	.0015	.0020			.0019	.0029		
	(.00071)**	(.00075)***			(.0011)*	(.0010)***		
Indicator for houses over 50th percentile of housing dimensions			.00013	-.00087			.00013	-.00050
			(.00071)	(.00063)			(.00054)	(.00047)
Indicator for houses over 50th percentile of housing dimensions × Indicator after July 19, 2005			.0011	.0018			.0025	.0027
			(.00070)	(.00076)**			(.00088)***	(.00087)***
Obs.	102853	102853	102853	102853	102853	102853	102853	102853
Log likelihood					-2898.7254	-2838.0698	-2897.1048	-2838.4685
Panel B: Bankruptcy within 5 years after purchase of house								
Indicator for houses over 50th percentile of housing prices	-.00068	-.0011			-.00057	-.00086		
	(.00052)	(.00056)**			(.00045)	(.00051)*		
Indicator for houses over 50th percentile of housing prices × Indicator after July 19, 2005	.0011	.0015			.0012	.0019		
	(.00056)*	(.00058)***			(.00069)*	(.00066)***		
Indicator for houses over 50th percentile of housing dimensions			.000037	-.00076			.000043	-.00053
			(.00055)	(.00052)			(.00045)	(.00043)
Indicator for houses over 50th percentile of housing dimensions × Indicator after July 19, 2005			.00076	.0011			.0012	.0014
			(.00066)	(.00069)			(.00069)*	(.00069)**
Obs.	102853	102853	102853	102853	102853	102853	102853	102853
Log likelihood					-1802.943	-1742.2135	-1802.7836	-1743.7273
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y	N	Y	N	Y

Note: The results presented in this table are obtained using data from May 15, 1996 to February 20, 2010. In Panel A, the dependent variable is whether an individual will be declared bankrupt after purchasing a house. In Panel B, the dependent variable is whether an individual will be declared bankrupt within five years after purchasing a house. Columns 1 to 4 present the results from performing an ordinary least-squares regression. Columns 5 to 8 present the results from performing a logistic regression. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the size of a house is above the 50th percentile of housing dimensions in the same year within the same region in Singapore. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All columns include year, region and cohort fixed effects. The results in the odd columns (columns 1, 3, 5 and 7) are obtained without including controls. The results in the even columns (columns 2, 4, 6 and 8) are obtained after including controls.

Table V. Heterogeneous Effects of the LTV Limit Increase

Dep. Var.:	Personal Bankruptcies											
Specification:	OLS Regression											
Sample:	New Sale		Resale		Condominium or private apartment		Landed property or others		Freehold property		Not freehold	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10	(11)	(12)
Indicator for houses over 50th percentile of housing prices ×	.0017		.0052		.0012		.0034		.0027		.0011	
Indicator after July 19, 2005	(.0013)		(.0020)**		(.00067)*		(.0026)		(.0013)**		(.0013)	
Indicator for houses over 50th percentile of housing dimensions ×		.00071		.0041		.00025		-.0020		.0015		-.00010
Indicator after July 19, 2005		(.0013)		(.0015)***		(.00078)		(.0027)		(.0014)		(.0014)
Obs.	54583	54583	47676	47676	84277	84277	18576	18576	44227	44227	58415	58415
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: The results in this table are obtained using data from May 15, 1996 to February 20, 2010. The dependent variable is whether an individual will be declared bankrupt after purchasing a house. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the size of a house is above the 50th percentile of housing dimensions in the same year within the same region in Singapore. All results are obtained from performing an ordinary least-squares regression. All columns include year, region and cohort-fixed effects as well as controls. Columns 1 and 2 present the results for residential property that is a new sale. Columns 3 and 4 present the results for residential property that is a resale. Columns 5 and 6 present the results for residential property that is a condominium or private apartment. Columns 7 and 8 present the results for residential property that is a landed property or others. Columns 9 and 10 present the results for residential property that is a freehold property, while columns 9 and 10 present the results for residential property which is not. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level.

Table VI. Placebo Test

Dep. Var.:	Personal Bankruptcies								
Sample:	All Samples								
Specification:	OLS Regression								
Placebo cutoff time	1998	1999	2000	2001	2002	2003	2004	2005	July 19, 2005
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Treatment Index is Housing Price									
Indicator for houses over 50th percentile	-.00066	.000074	-.0019	-.0013	.00099	-.00079	.00012	.0018	.0019
of Treatment Index × placebo cutoff year	(.0035)	(.0021)	(.0023)	(.0027)	(.0021)	(.0013)	(.0014)	(.00096)*	(.0011)***
Panel B: Treatment Index is Housing Dimensions									
Indicator for houses over 50th percentile	-.0051	-.0022	-.0061	-.0027	-.0014	-.0019	-.00075	.0025	.0027
of Treatment Index × placebo cutoff year	(.0040)	(.0021)	(.0023)***	(.0027)	(.0019)	(.0013)	(.0015)	(.00099)**	(.0011)**
Obs.	13379	23785	38660	47220	55748	61452	63304	59019	59019
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: The samples for each column are based on sequential six-year samples. For instance, given that the cutoff time of 2002, the samples used will be the following two three-year samples: 1999-2002 and 2002-2005. The dependent variable is whether an individual will be declared bankrupt after purchasing a house. All results presented above are obtained from performing an ordinary least-squares regression. The results in Panel A are obtained with the housing prices as the treatment index. The results in Panel B are obtained with the housing dimensions as the treatment index. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the price of a house is above the 50th percentile of housing dimensions in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing prices* is interacted with *Placebo cutoff time* in the column header. *Placebo cutoff time* represents *Placebo(year)* for columns 1 to 8. Therefore, the first column of panel A, for instance, presents the estimate for the coefficient of *Indicator for houses over 50th percentile of housing prices x Placebo1998. Placebo(year)* is the cutoff year from which common trends are compared across. For example, for placebo2001, the cutoff year is 2001 and the data before 2001 is compared to the data after 2001. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All results are obtained from performing an ordinary least-squares regression. All columns include year, region and cohort fixed effects as well as controls.

Table VII. Composition Effect

Dep. Var.:	**Personal Bankruptcies**							
Specification:	**OLS Regression**							
Sample:	**Year 1996 - 2005**				**Year 2000 - 2005**			
Treatment Index	**Housing Prices**		**Housing Dimensions**		**Housing Prices**		**Housing Dimensions**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Indicator for houses over 50th percentile of treatment index ×	.00050	-.000013	-.00055	-.0016	.00034	.000024	-.00031	-.0010
Indicator after September 1, 2002	(.0011)	(.0013)	(.0010)	(.0011)	(.0012)	(.0013)	(.0011)	(.0012)
Obs.	63352	63352	63352	63352	57003	57003	57003	57003
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y	N	Y	N	Y

Note: The dependent variable is whether an individual will be declared bankrupt after purchasing a house. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the size of a house is above the 50th percentile of housing dimensions in the same year within the same region in Singapore. The sample year varies across the table. Columns 1 to 4 uses data from 1996 to 2005 while columns 5 to 8 uses data from 2000 to 2005. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All results are obtained from performing an ordinary least-squares regression. All columns include year, region and cohort -fixed effects as well as controls.

Table VIII. Debt Burden Effect

Dep. Var.:	Credit-Related Lawsuits			
Sample:	All Samples			
Treatment Index	Housing Prices		Housing Dimensions	
	(1)	(2)	(3)	(4)
Panel A: If a person is a defendant in a credit-related lawsuit				
Indicator of housing over 50th percentile of treatment index × Indicator after July 19, 2005	.0020	.0015	.0020	.0022
	(.0011)*	(.0012)	(.0011)*	(.0011)**
Obs.	134315	134315	134315	134315
Panel B: If a person is a plaintiff in a credit-related lawsuit				
Indicator of housing over 50th percentile of treatment index × Indicator after July 19, 2005	.00014	.000087	.00012	.00011
	(.00018)	(.00022)	(.00021)	(.00024)
Obs.	136417	136417	136417	136417
Year, region and cohort fixed effects	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y

Note: The result in this table is obtained using data from May 15, 1996 to February 20, 2010. In Panel A, the dependent variable is whether an individual is a defendant in a credit-related lawsuit after purchasing a house. In Panel B, the dependent variable is whether an individual is a plaintiff in a credit-related lawsuit after purchasing a house. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the size of a house is above the 50th percentile of housing dimensions in the same year within the same region in Singapore. *Indicator after July 19, 2005* takes 1 if the sample date is after July 19, 2005. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All results are obtained from performing an ordinary least-squares regression. All columns include year, region and cohort-fixed effects as well as controls.

Table IX. Heterogeneous Effect of the LTV Limit Increase Based on Credit-Related Lawsuits

Dep. Var.:	Personal Bankruptcies			
Specification:	OLS Regression			
Sample:	All Samples			
Treatment Index	Housing Prices		Housing Dimensions	
	(1)	(2)	(3)	(4)
Panel A: If a person is a defendant in a credit-related lawsuit				
Indicator for houses over 50th percentile of treatment index × Indicator after July 19, 2005	-.00087	-.000056	.00011	.00075
	(.00048)*	(.00054)	(.00046)	(.00058)
Indicator for credit-related lawsuits ×	.11	.11	.084	.084
Indicator for houses over 50th percentile of treatment index × Indicator after July 19, 2005	(.053)**	(.053)**	(.049)*	(.049)*
Obs.	134315	134315	134315	134315
Panel B: If a person is a plaintiff in a credit-related lawsuit				
Indicator for houses over 50th percentile of treatment index × Indicator after July 19, 2005	.000044	.0000045	.00006	.000059
	(.000080)	(.00010)	(.000081)	(.000097)
Indicator for credit-related lawsuits ×	-.000051	.00023	-.000010	.00020
Indicator for houses over 50th percentile of treatment index × Indicator after July 19, 2005	(.000080)	(.00023)	(.000071)	(.00021)
Obs.	136417	136417	136417	136417
Year, region and cohort fixed effects	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y

Note: The result in this table is obtained using data from May 15, 1996 to February 20, 2010. The dependent variable is whether an individual will be declared bankrupt after purchasing a house. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the size of a house is above the 50th percentile of housing dimensions in the same year within the same region in Singapore. *Indicator after July 19, 2005* takes 1 if the sample date is after July 19, 2005. *Indicator for lawsuits with credit reasons* takes 1 if an individual is involved in a credit-related lawsuit. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All results are obtained from performing an ordinary least squares regression. All columns include year, region and cohort-fixed effects as well as controls.

Table X. Selection for Irresponsible Buyers

Sample:	All Samples			
Specification:	OLS Regression			
Treatment Index	Housing Prices		Housing Dimensions	
	(1)	(2)	(3)	(4)
Panel A : Dependent Variable is bankruptcy before buying house				
Indicator for houses over 50th percentile of Treatment Index ×	.00037	.00041	.00055	.00049
Indicator after July 19, 2005	(.00084)	(.00090)	(.00081)	(.00086)
Obs.	102853	102853	102853	102853
Panel B : Dependent Variable is a defendant involved in a credit-related lawsuit				
Indicator for houses over 50th percentile of Treatment Index ×	.00029	.0012	.00010	.00058
Indicator after July 19, 2005	(.00090)	(.00092)	(.00077)	(.00084)
Obs.	151956	151956	151956	151956
Panel C : Dependent Variable is male				
Indicator for houses over 50th percentile of Treatment Index ×	-.0033	-.000053	.0034	.0067
Indicator after July 19, 2005	(.0057)	(.0044)	(.0059)	(.0051)
Obs.	102853	102853	102853	102853
Panel D : Dependent Variable is whether is Chinese				
Indicator for houses over 50th percentile of Treatment Index ×	.0070	.0016	.011	.0062
Indicator after July 19, 2005	(.0045)	(.0044)	(.0055)**	(.0049)
Obs.	102853	102853	102853	102853
Panel E : Dependent Variable is cohort				
Indicator for houses over 50th percentile of Treatment Index ×	-.014	-.022	-.037	-.043
Indicator after July 19, 2005	(.033)	(.031)	(.028)	(.029)
Obs.	101629	101629	101629	101629
Year, region and cohort fixed effects	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y

Note: The result in this table is obtained using data from May 15, 1996 to February 20, 2010. There are five dependent variables in this table: whether an individual will be declared bankrupt after purchasing a house, if an individual is a defendant involved in a credit -related lawsuit, if an individual is male, if an individual is of Chinese descent, and cohort, which refers to when an individual is born. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the size of a house is above the 50th percentile of housing dimensions in the same year within the same region in Singapore. *Indicator after July 19, 2005* takes 1 if the sample date is after July 19, 2005. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All results are obtained from performing an ordinary least-squares regression. All columns include year, region and cohort-fixed effects as well as controls.

Table XI. The Impact of the LTV Limit Increase: Simulated Housing Prices and Dimensions

Dep. Var.:	Personal Bankruptcies							
Sample:	All Samples							
Specification:	OLS Regression				Logistic Regression			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Indicator for houses over 50th percentile of housing prices × Indicator after July 19, 2005	.0017 (.00070)**	.0025 (.00094)***			.0021 (.0011)*	.0028 (.0013)**		
Indicator for houses over 50th percentile of housing dimensions × Indicator after July 19, 2005			.0012 (.00087)	.0018 (.0011)*			.0021 (.0011)*	.0017 (.0013)
Obs.	102853	102853	102853	102853	102853	102853	102853	102853
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y	N	Y	N	Y
Log likelihood					-2898.4129	-2843.1249	-2898.5542	-2844.9376

Note: The result in this table is obtained using data from May 15, 1996 to February 20 2010. The dependent variable is whether an individual will be declared bankrupt after purchasing a house. Columns 1 to 4 present the results from performing an ordinary least-squares regression. Columns 5 to 8 present the results from performing a logistic regression. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of simulated housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the size of a house is above the 50th percentile of simulated housing dimensions in the same year within the same region in Singapore. *Indicator after July 19, 2005* takes 1 if the sample date is after July 19, 2005. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All columns include year, region and cohort-fixed effects.

Table XII. Impact of Interest Rates on Personal Bankruptcy, the LTV Ratio and the Monthly Instalment

Sample:	Bankruptcy Dataset				LTV Dataset	
Specification:	OLS Regression					
Dep. Var.:	Bankruptcy after purchase of property				LTV Ratio	Instalments
	(1)	(2)	(3)	(4)	(5)	(6)
Indicator for houses over 50th percentile of housing prices × Indicator after July 1, 2004	-.00040	.000051			2.523	-97.202
	(.0012)	(.0013)			(3.178)	(187.655)
Indicator for houses over 50th percentile of housing dimensions × Indicator after July 1, 2004			-.00012	-.000078		
			(.0013)	(.0015)		
Obs.	43061	43061	43061	43061	652	652
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y	N	N

Note: The result in this table is obtained using data from May 15, 1996 to February 20, 2010. The dependent variable varies in this table as from column 1 to 4, the dependent variables is whether an individual will be declared bankrupt after purchasing a house, and for column 5 to 6, the dependent variables are the LTV ratio and the monthly instalment. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the size of a house is above the 50th percentile of housing dimensions in the same year within the same region in Singapore. *Indicator after July 1, 2004* takes 1 if the sample date is after July 1, 2004. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All results are obtained from performing an ordinary least-squares regression. All columns include year, region and cohort-fixed effects as well as controls.

Table XIII. Speculators

| Dep. Var.:	**Personal Bankruptcies**							
Specification:	**OLS Regression**							
Sample:	**Those who sell houses with subsale**				**Those who buy multiple houses**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Indicator for houses over 50th percentile of housing prices × Indicator after July 19, 2005	-.010 (.014)	.0077 (.016)			-.00031 (.0012)	.00001 (.0013)		
Indicator for houses over 50th percentile of housing dimensions × Indicator after July 19, 2005			-.019 (.019)	.015 (.020)			.00061 (.0011)	.00025 (.0012)
Obs.	1257	1257	1257	1257	41614	41614	41614	41614
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y	N	Y	N	Y

Note: The results in this table are obtained using two different data samples from May 15, 1996 to February 20, 2010. For column 1 to 4, the sample is restricted to those who sell their house in a sub-sale, which refers to the purchase and sale of a property before completion based on the assumption of a price increase that will allow for a quick profit. For columns 5 to 8, the sample is restricted to those who buy multiple houses. The dependent variable is whether an individual will be declared bankrupt after purchasing a house. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the size of a house is above the 50th percentile of housing dimensions in the same year within the same region in Singapore. *Indicator after July 19, 2005* takes 1 if the sample date is after July 19, 2005. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All results are obtained from performing an ordinary least-squares regression. All columns include year, region and cohort-fixed effects as well as controls.

Table XIV. Correlation between Personal Bankruptcy and Selling Behavior

| Specification:	OLS Regression				
Dep. Var.:	Whether individual sell house	Log(housing price)		Returns from Sale	
Sample:	All Samples	Those who sell houses	Those who sell house (close to bankruptcy/lawsuit)	Those who sell houses	Those who sell house (close to bankruptcy/lawsuit)
	(1)	(2)	(3)	(4)	(5)
Panel A: the correlation between personal bankruptcy and selling behavior					
Indicator for personal bankruptcy	0.090 (.012)***				
Indicator for selling houses before personal bankruptcy		-.123 (.025)***	-.157 (.028)***	-.115 (.026)***	-.160 (.024)***
Indicator for selling houses after personal bankruptcy		-.087 (.026)***	-.160 (.040)***	-.083 (.031)***	-.144 (.043)***
Obs.	138917	10634	10556	10634	10556
Panel B: the correlation between being a defendant in a credit-related lawsuit and selling behavior					
Indicator for credit-related lawsuits	0.056 (.007)***				
Indicator for selling houses before the credit-related lawsuits		-.093 (.026)***	-.121 (.033)***	-.108 (.025)***	-.132 (.032)***
Indicator for selling houses after the credit-related lawsuits		-.087 (.034)**	-.122 (.052)**	-.099 (.034)***	-.110 (.047)**
Obs.	143573	9001	8764	9001	8764
Year, region and cohort fixed effects	Y	Y	Y	Y	Y

Note: The result in this table is obtained using data from May 15 1996 to February 20, 2010. Panel A reports the correlation between personal bankruptcy and selling behavior, and Panel B reports the correlation between being a defendant in a credit-related lawsuit and selling behavior. In Column 1, the dependent variable is whether individual sells a house. In columns 2 and 3, the dependent variable is the natural log of housing price. In columns 4 and 5, the dependent variable is the return from the house sale. In columns 2 and 4, we restrict the sample to those who sell houses. In columns 3 and 5, we further exclude the sample who sell houses three years before or after the personal bankruptcy. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All results are obtained from performing an ordinary least squares regression. All columns include year, region and cohort fixed effects as well as controls.

Figure I. Key LTV Limit Changes for Private Residential Properties
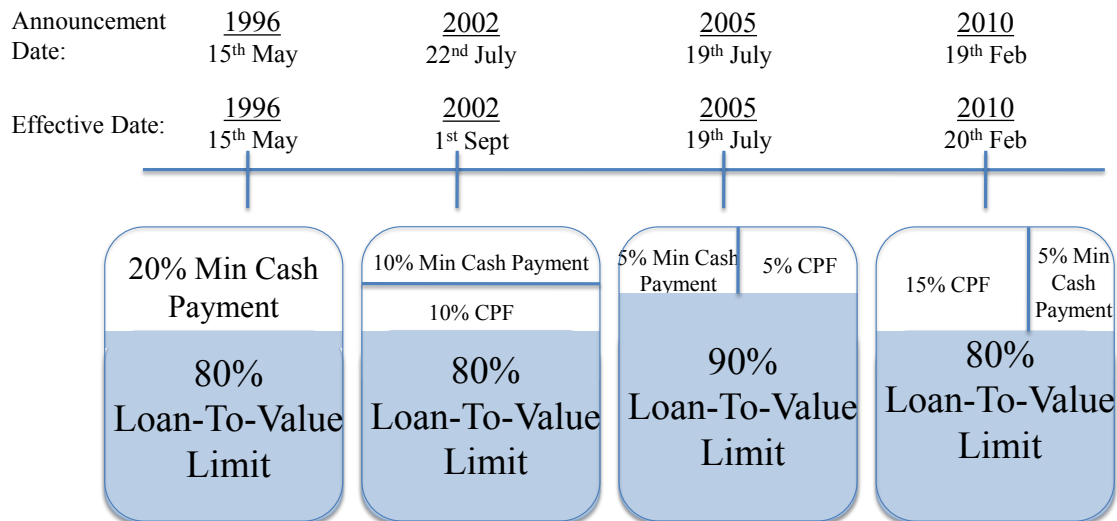
Note: Figure I shows the key loan-to-value (LTV) limit changes for private residential properties in Singapore after 1996. The LTV limit is the ratio of a loan to the value of an asset purchased. In housing market, LTV limit is used by banks to represent the ratio of the mortgage amount as a percentage of the total appraised value of real property

Figure II. Personal Bankruptcy Procedures
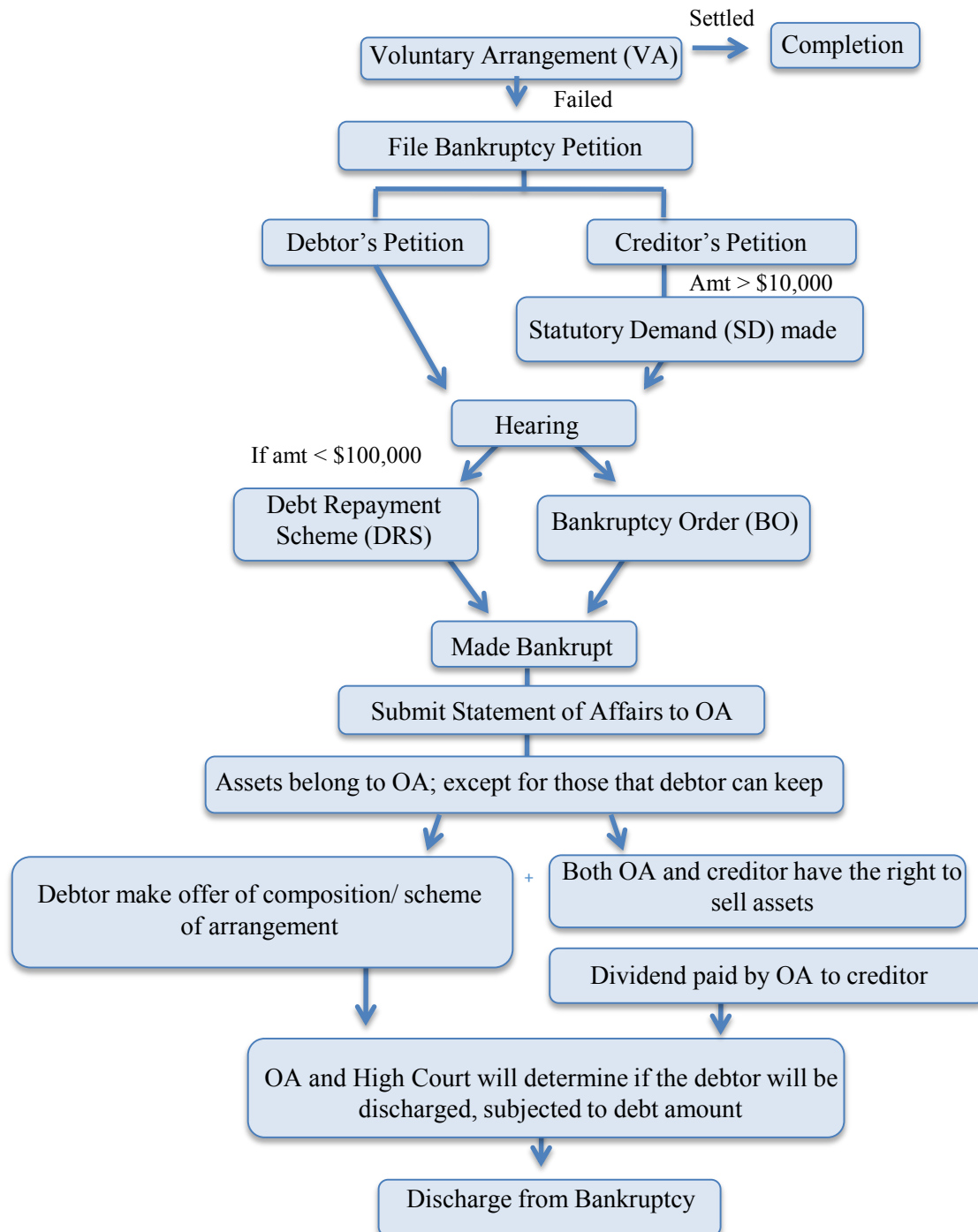
Note: Figure II describes the procedures of personal bankruptcy under the Bankruptcy Act in Singapore. Personal Bankruptcy refers to any individual debtor who has been judged bankrupt by a bankruptcy order. Only the High Court or the Official Assignee (OA) is able to discharge the debtor from Bankruptcy subjected to several conditions.

Figure III: The Impact of LTV Limit Increase on Actual Loan-to-Value Ratio
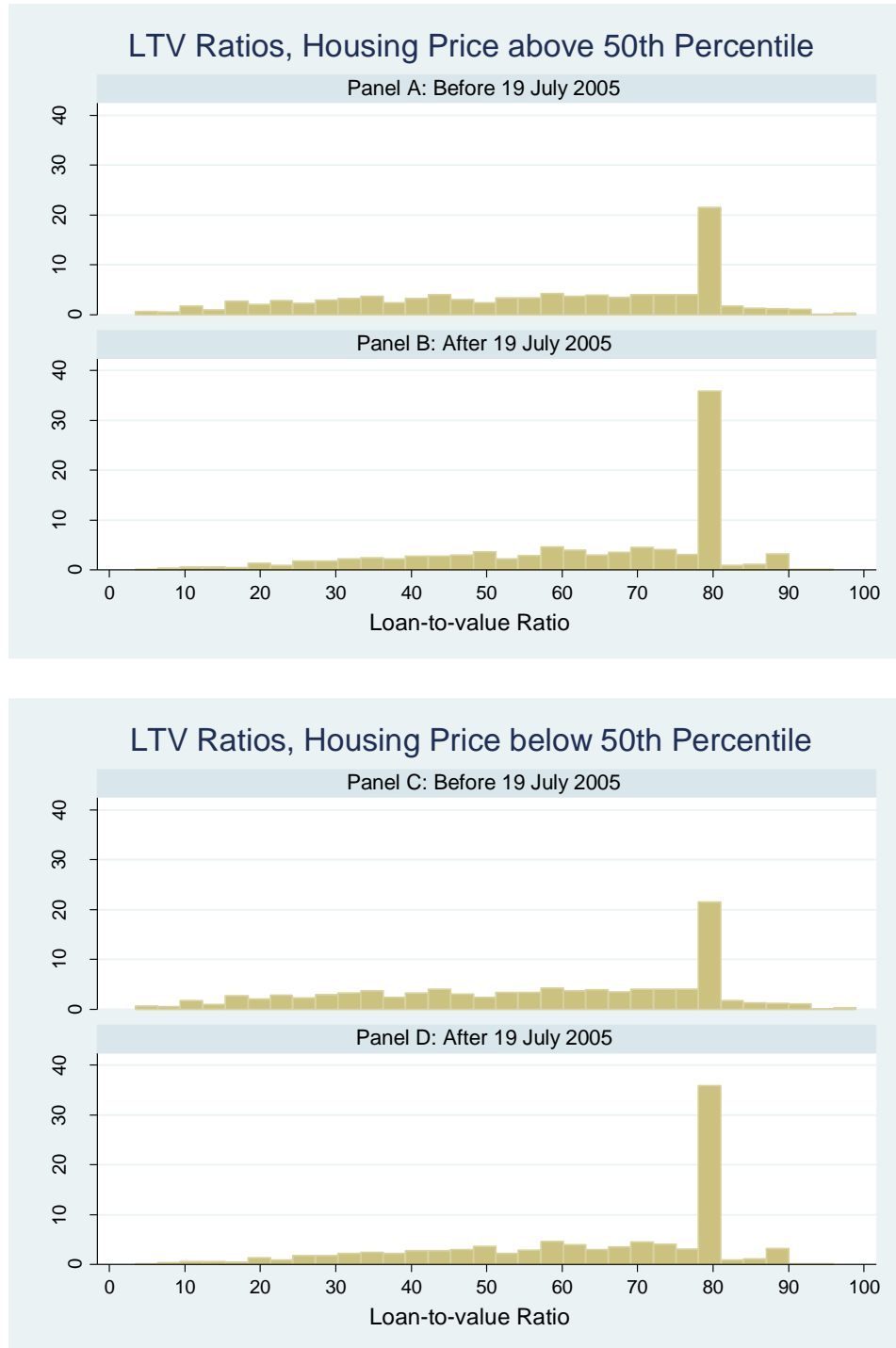
Note: Figure III shows the distribution of actual LTV for relatively expensive houses and cheap houses before and after the LTV limit increase in 2005. Panel A and B shows the sample of houses above median housing price in the same year within the same region in Singapore. Panel C and D shows the sample of houses below median housing price in the same year within the same region in Singapore.

Figure IV. Three-Month Singapore Interbank Offered Rates (SIBOR)
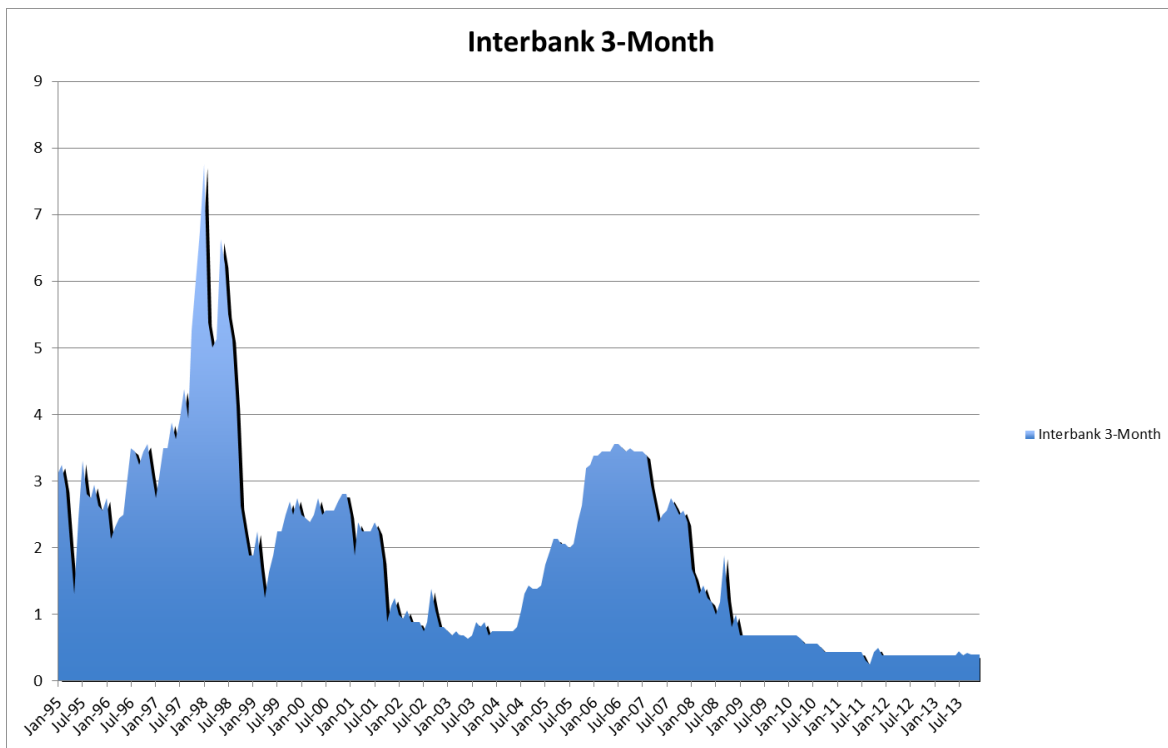
Note: The sample period for SIBOR rates is from January 1995 to July 2013. Three-month SIBOR rates are the benchmark for property loans in Singapore and they are shared across multiple banks.

Table A.1 The Impact of the LTV Limit Increase on Personal Bankruptcy:

Alternative Treatment Cutoffs

Dep. Var.:	**Personal bankruptcies**							
Sample:	**Year 1996 - 2010**							
Specification:	**OLS Regression**				**Logistic Regression**			
Treatment Index	**Housing Prices**		**Housing Dimensions**		**Housing Prices**		**Housing Dimensions**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Use Median in each year as treatment cutoff								
Indicator for houses over 50th percentile of treatment index × Indicator after July 19, 2005	.0014	.0019	.00057	.0011	.0017	.0030	.0020	.0021
	(.00074)*	(.00082)**	(.00076)	(.00083)	(.0011)	(.0011)***	(.00091)**	(.00091)**
Panel B: Use Median in each year's sales type as treatment cutoff								
Indicator for houses over 50th percentile of treatment index × Indicator after July 19, 2005	.0015	.0014	-.00030	-.00046	-.00090	.0020	.00064	.00060
	(.00062)**	(.00066)**	(.00069)	(.00074)	(.0010)	(.0011)*	(.00084)	(.00095)
Panel C: Use Median in each year's property type as treatment cutoff								
Indicator for houses over 50th percentile of treatment index × Indicator after July 19, 2005	.0021	.0024	.0014	.0015	.0025	.0036	.0022	.0026
	(.00078)***	(.00085)***	(.00073)*	(.00082)*	(.0012)**	(.0012)***	(.00093)**	(.00095)***
Obs.	102853	102853	102853	102853	102853	102853	102853	102853
Log likelihood								
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y	N	Y	N	Y

Note: Table A.1 presents robustness check for Table IV using alternative treatment cutoffs. The dependent variable is whether an individual will be declared bankrupt after purchasing a house. In Panel A, *Indicator for houses over 50th percentile of treatment index* takes 1 if the treatment index of a house is above the 50th percentile of treatment index in the same year in Singapore. In Panel B, *Indicator for houses over 50th percentile of treatment index* takes 1 if the treatment index of a house is above the 50th percentile of treatment index in the same sale type in the same year in Singapore. In Panel C, *Indicator for houses over 50th percentile of treatment index* takes 1 if the treatment index of a house is above the 50th percentile of treatment in the same property type in the same year in Singapore. Columns 1 to 4 present the results from performing an ordinary least-squares regression. Columns 5 to 8 present the results from performing a logistic regression. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All columns include year, region and cohort-fixed effects. The results in the odd columns (columns 1, 3, 5 and 7) are obtained without including controls. The results in the even columns (columns 2, 4, 6 and 8) are obtained after including controls.

Table A.2 The Impact of the LTV Limit Increase on Personal Bankruptcy, the LTV Ratio and the Monthly Instalment: Quartile as Treatment Cutoffs

Sample:	Bankruptcy Dataset				Mortgage Dataset	
Specification:	OLS Regression					
Dep. Var.:	Personal Bankruptcies				LTV	Instalments
	(1)	(2)	(3)	(4)	(5)	(6)
Indicator for houses between 25th and 50th percentile of housing prices × Indicator after July 19, 2005	.00031	-.000019			0.916	105.003
	(.00077)	(.00080)			(1.716)	(112.478)
Indicator for houses between 50th and 75th percentile of housing prices × Indicator after July 19, 2005	.0019	.0018			3.137**	390.234***
	(.00091)**	(.00091)**			(1.331)	(133.444)
Indicator for houses over 75th percentile of housing prices × Indicator after July 19, 2005	.0014	.0023			6.691***	676.405**
	(.0010)	(.0011)**			(1.728)	(257.837)
Indicator for houses between 25th and 50th percentile of housing dimension × Indicator after July 19, 2005			-.000063	-.00035		
			(.0011)	(.0011)		
Indicator for houses between 50th and 75th percentile of housing dimension × Indicator after July 19, 2005			.0023	.0022		
			(.00083)***	(.00086)**		
Indicator for houses over 75th percentile of housing dimension × Indicator after July 19, 2005			-.00033	.00042		
			(.0011)	(.0014)		
Obs.	102853	102853	102853	102853	3686	3686
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y	N	N

Note: Table A.2 presents robustness check for Table II and IV using quartiles of treatment index as treatment cutoffs. In columns 1 to 4, the dependent variable is whether an individual will be declared bankrupt after purchasing a house. In columns 5 and 6, the dependent variables are the LTV ratio and the monthly instalment. *Indicator for houses between 25th and 50th percentile of housing prices* takes 1 if the prices of a house is between the 25th and the 50th percentile of housing prices in the same year in the same region. *Indicator for houses between 50th and 75th percentile of housing prices* takes 1 if the prices of a house is between the 50th and the 75th percentile of housing prices in the same year in the same region. *Indicator for houses over 75th percentile of housing prices* takes 1 if the prices of a house is over the 75th percentile of housing prices in the same year in the same region. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All columns include year, region and cohort-fixed effects. The results in the odd columns (columns 1, 3, 5 and 7) are obtained without including controls. The results in the even columns (columns 2, 4, 6 and 8) are obtained after including controls.

Table A.3 Impact of the LTV Limit Increase on Personal Bankruptcy:

Multiple House Buyers

Dep. Var.:	Personal Bankruptcies							
Sample:	Year 1996 - 2010							
Specification:	OLS Regression				Logistic Regression			
Treatment Index	Housing Prices		Housing Dimensions		Housing Prices		Housing Dimensions	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All cases including multiple houses and bankruptcies								
Indicator for houses over 50th percentile of treatment index ×	.00096	.0013	.0010	.0013	.0013	.0021	.0023	.0022
Indicator after July 19, 2005	(.00061)	(.00059)**	(.00059)*	(.00063)**	(.00093)	(.00095)**	(.00076)***	(.00076)***
Obs.	144573	144573	144573	144573	144573	144573	144573	144573
Panel B: Quartile of all cases including multiple houses and bankruptcies								
Indicator for houses between 25th percentile to 50th	.00064	.00026	-.00026	-.00069	-.0013	-.00075	-.0016	-.0012
percentile of treatment index × Indicator after July 19, 2005	(.00077)	(.00080)	(.00082)	(.00082)	(.0014)	(.0014)	(.0014)	(.0014)
Indicator for houses between 50th percentile to 75th	.00071	.00054	.0019	.0017	-.00021	.00098	.0027	.0030
percentile of treatment index × Indicator after July 19, 2005	(.00070)	(.00067)	(.00067)***	(.00070)**	(.0010)	(.00098)	(.0014)***	(.0010)***
Indicator for houses above 75th	.00098	.0017	.000059	.00036	.0010	.0019	.0015	.00070
percentile of treatment index × Indicator after July 19, 2005	(.00084)	(.00086)*	(.00091)	(.00096)	(.0013)	(.0015)	(.0013)	(.0013)
Obs.	144573	144573	144573	144573	144573	144573	144573	144573
Log likelihood					-3936.8467	-3844.1935	-3936.636	-3842.7572
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	N	Y	N	Y	N	Y	N	Y

Note: Table A.3 presents robustness check for Table IV using both single house buyers and multiple house buyers. In Panel A, we use median of treatment index in the same region in the same year as treatment cutoffs. In Panel B, we use quartiles of treatment index in the same region in the same year as treatment cutoffs. Columns 1 to 4 present the results from performing an ordinary least-squares regression. Columns 5 to 8 present the results from performing a logistic regression. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All columns include year, region and cohort-fixed effects. The results in the odd columns (columns 1, 3, 5 and 7) are obtained without including controls. The results in the even columns (columns 2, 4, 6 and 8) are obtained after including controls.

Table A.4 Debt Burden Effect: Placebo Test

Sample:	All Samples							
Specification:	OLS Regression							
Placebo cutoff year	1998	1999	2000	2001	2002	2003	2004	2005
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: If a person is a defendant in a credit-related lawsuit, Treatment Index is Housing Price								
Indicator for houses over 50th percentile of Treatment Index × placebo cutoff year	-.0028	-.0033	-.0039	-.00033	.00012	.00056	.00054	.0019
	(.0047)	(.0033)	(.0027)	(.0022)	(.0025)	(.0017)	(.0022)	(.0023)
Obs.	85407	85407	85407	85407	85407	85407	85407	85407
Panel B: If a person is a defendant in a credit-related lawsuit, Treatment Index is Housing Dimensions								
Indicator for houses over 50th percentile of Treatment Index × placebo cutoff year	-.0033	-.0030	-.0030	-.00095	.0015	.00016	.0021	.00092
	(.0047)	(.0023)	(.0028)	(.0020)	(.0021)	(.0013)	(.0021)	(.0015)
Obs.	85407	85407	85407	85407	85407	85407	85407	85407
Year, region and cohort fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Control × Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y

Note: The samples includes housing transactions May 15, 1996 to July 19, 2005. The dependent variable is whether an individual will be declared bankrupt after purchasing a house. All results presented above are obtained from performing an ordinary least-squares regression. The results in Panel A are obtained with the housing prices as the treatment index. The results in Panel B are obtained with the housing dimensions as the treatment index. *Indicator for houses over 50th percentile of housing prices* takes 1 if the price of a house is above the 50th percentile of housing prices in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing dimensions* takes 1 if the price of a house is above the 50th percentile of housing dimensions in the same year within the same region in Singapore. *Indicator for houses over 50th percentile of housing prices* is interacted with *Placebo cutoff time* in the column header. *Placebo cutoff time* represents *Placebo(year)* for columns 1 to 8. Therefore, the first column of panel A, for instance, presents the estimate for the coefficient of *Indicator for houses over 50th percentile of housing prices x Placebo1998. Placebo (year)* is the cutoff year across which common trends are compared. For example, for placebo2001, the cutoff year is 2001 and the data before 2001 is compared to the data after 2001. Standard errors are clustered by 82 postal sectors. Robust clustered errors are reported in parentheses. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. All results are obtained from performing an ordinary least-squares regression. All columns include year, region and cohort-fixed effects as well as controls.