

# THREE ESSAYS ON HOUSING AND CREDIT

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The late-2000s financial crisis has brought great attention to housing market and home mortgage market. I study housing and credit issues using evidence from the United States and China. In Chapter 1, I examine how a court-mandated repeal of a home mortgage regulation in Cleveland affected home foreclosures. To distinguish the effect of deregulation from confounding factors, I apply a difference-in-difference research design using suburbs as the control group for Cleveland. Results indicate that deregulation caused a 49 percent increase in early foreclosures while it did not increase total loan originations. This result implies that bad loans as a share of overall loans increased substantially. In Chapter 2, I evaluate the impact of housing provident funds (HPFs), saving programs that collect deposits from employees and their employers and provide mortgage loans with discounted rates. I exploit a natural experiment in China where the amount of home loans available to employees depends on previous deposits to the fund. Results indicate that after HPF loans became available in 1998, households with two members enrolled in the HPF program enjoyed homeownership 18 percent greater than those with only one member enrolled. Furthermore, each additional year of HPF deposits increased homeownership by 4 percent. The results suggest that HPF loans allow higher housing consumption for eligible contributors. In Chapter 3, my coauthor and I document that home mortgage loans made by out-of-state banks without local branches are more likely to have high interest rates and are sold more to the secondary market than other types of mortgages. Drawing evidence from Home Mortgage Disclosure Act Data from 2005 to 2008, we find that the demand for nonlocal mortgages is larger in the neighborhood where local banks' denial rate is higher. And banks make more nonlocal mortgages to out-of-state borrowers if they have better access to the secondary market. The results suggest that securitization may provide bad incentives for risk-taking.

**Keywords:** deregulation; foreclosure; credit access; housing consumption; nonlocal lending; securitization.

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## PREFACE

I am grateful for the invaluable advice, guidance and support of my committee.

## 1.0 DOES MORTGAGE DEREGULATION INCREASE FORECLOSURES? EVIDENCE FROM CLEVELAND

### 1.1 INTRODUCTION

The late-2000s financial crisis has re-ignited a debate about the impact of regulating consumer mortgage markets. Several scholars and prominent policymakers argue that the deregulation of mortgage lending markets has caused the large number of loan defaults and foreclosures. Elizabeth Warren, for example, argues that non-transparent terms within mortgage contracts have lured misinformed borrowers to take on loans that were inappropriate for their financial circumstances (Warren [2007]). However, Ben Bernanke argues that lighter regulation of mortgage markets can spur financial innovations that broadly benefit low income households. Specifically, new mortgage products that correctly account for risk have the potential to increase the supply of loans to creditworthy low-income households (Bernanke [2009]).

Does deregulation of mortgage markets enable lenders to take advantage of less informed borrowers? If so, we would observe that deregulation would cause bad loans to increase as a share of overall loans. Or, does deregulation enable more creditworthy borrowers to obtain good loans? If so, we would observe that the share of bad loans does not increase following deregulation. This paper uses a court-ordered repeal of home mortgage regulations in Cleveland, Ohio, to answer those questions.

On November 20, 2006, the Ohio State Supreme Court overturned a city law designed to protect borrowers against so-called “predatory” lending practices. Specifically, when the spread between the mortgage interest rate and T-bill rate exceeded 4.5 percentage points, lenders could not add additional terms, such as prepayment penalties, increased interest following a default, mandatory arbitration, or financing of credit insurance, lending without counseling. Following the high court order, mortgage lenders could freely charge interest rates beyond the 4.5 percentage point spread

and include additional terms arbitrarily. As I shall argue in more detail in the paper, the court ruling provides a unique natural experiment for identifying the impact of deregulation on mortgage markets. Specifically, drawing upon a unique mortgage data set that is matched to household foreclosures, I have found that deregulation had a profound impact on the composition of loans following the Ohio court’s order for deregulation. Loans with subprime interest rates increased by 30 percent and loans issued by subprime lenders increased by 40 percent. However, the most striking result, is that deregulation caused loan foreclosures to increase by 49 percent.<sup>1</sup> Moreover, the court-ordered deregulation had no significant impact on overall loan originations, implying that bad loans as a share of overall loans increased substantially.

The court-mandated repeal makes Cleveland an ideal subject for a natural experiment in deregulation. Tracts within Cleveland constitute a treatment group and tracts within neighboring suburban municipalities serve as a control group. I use the control group to construct the counterfactual time trend of credit demand and credit quality for Cleveland in the absence of deregulation. The difference-in-difference (DID) estimation compares the number of originated loans and the number of foreclosed loans in Cleveland before and after deregulation and identifies the impact of the deregulation as the deviation from the counterfactual time trend.<sup>2</sup>

There are two major issues in making a causal interpretation of the impact of mortgage deregulation on loan composition and foreclosures. First, the deregulation should be exogenous. It should not pick up pre-existing lending patterns. This is the plausible case for several reasons. The Ohio Supreme Court’s ruling in 2006 closed the case between American Financial Services Association (AFSA) and the City of Cleveland. Because AFSA sued the city of Cleveland immediately after the city passed its mortgage regulation in 2002, the court order in 2006 is not endogenous to lending patterns in Cleveland between 2002 and 2006. However, because the Second District Court of Appeals had ruled the Ohio city of Dayton’s mortgage regulation invalid in 2004, there might have been some expectation of AFSA eventually winning the case. Nonetheless, the timing of the ruling remained plausibly uncertain. To the extent the lenders and borrowers could ignore the law before deregulation, my estimates for increased foreclosures can be interpreted as the lower bound of the true effect.<sup>3</sup>

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<sup>1</sup>An early loan foreclosure is defined as foreclosure complaint filed by the lender within 30 months of the origination.

<sup>2</sup>Agarwal et al. [2010] use a similar method when they study the effects of mandatory mortgage counseling on home mortgage lending in Cook County, Illinois.

<sup>3</sup>See section 1.2.1 for a detailed discussion.

The second problem for a causal interpretation is whether increased early foreclosures were driven by confounders that affected Cleveland and its suburbs in different ways before and after November 2006. I address this concern in several ways. To ensure the results are not driven by sample selection, I ran robustness checks for loans not preempted by federal regulatory agencies, loans made within a smaller window of time, and loans made within smaller areas along the city border. I found a similar increase in early foreclosures among each of these three subsets of loan activity. Furthermore, I ran several falsification tests. I found no evidence of treatment effects in 12 months and 6 months before the true treatment; no treatment effects in fake city areas or in the city of Pittsburgh; and no treatment effect when I use local social economic conditions and housing prices as outcome variables. These pieces of evidence suggest that the increased early foreclosures are not driven by seasonal pattern, social and economic shocks, or the financial crisis.

Several other studies have set the stage for this paper. [Bostic et al. \[2008\]](#) exploit state level cross-sectional variations in home mortgage regulations, and [Ho and Pennington-Cross \[2006\]](#) add time-series variation before and after state laws became effective. In order to deal with omitted-variable issues, they both limit their samples to counties that are located along state borders. Using mortgage data from the Home Mortgage Disclosure Act (HMDA), they find mixed evidence on whether mortgage regulations impede credit flow in the subprime sector. They measure subprime credit supply by an individual borrowers' probability of applying for and receiving a loan from a subprime lender defined by the Department of Housing and Urban Development (HUD). According to HUD's classification, a loan made by a so-called "subprime lender" is not necessarily subject to a higher risk of default. Therefore, this measure provides limited insight into the quality effect of mortgage regulation. My paper fills this gap by investigating foreclosures of home loans made before and after an exogenous repeal of local mortgage regulation.

[Clauret and Herzog \[1990\]](#) and [Pence \[2006\]](#) show that the ease with which lenders can foreclose on borrowers mitigates loan losses and causes lenders to issue larger loans. To my knowledge, my study is the first that identifies the effect of deregulation on home foreclosures. Due to data limitation, only a few studies were able to address the qualitative effects of mortgage regulations. Using proprietary loan information of a stock of home mortgages made between 2000 and 2006 in zip code areas on state borders, [Ding et al. \[2011\]](#) found that rates of foreclosure on home loans between 2006 and 2008 are lower in states where home mortgage regulations have broader coverage and stricter restrictions. The proprietary loan data set provides detailed loan information, including delinquencies and foreclosures. However, it covers only around 50 percent of the home mortgage

market. Moreover, it over-represents the subprime mortgage sector. As an alternative, the Home Mortgage Disclosure Act (HMDA) data set provides extensive mortgage market coverage. However, it offers no information about loan performance. My unique data set matches the HMDA loan records to individual home foreclosure records. To account for the direct impact of deregulation, I include in the sample only those loans made six months before and after the deregulation ruling and track foreclosure filings for 30 months after loan origination.

My paper also provides a new perspective to track home mortgage performance. In most reports of regional foreclosures ([Schiller and Hirsh \[2008\]](#), [Nelson \[2008\]](#), [Richter et al. \[2011\]](#)), changes in reported numbers of new foreclosures usually reflect changes in the macroeconomic environment at the time of foreclosure. Factors such as the housing market's collapse, a sluggish economy, and extended high unemployment will contribute to an increase in contemporaneous foreclosures. By tracking the origination period of foreclosed loans, I can compare loan performance in different legal environments and investigate the impact of deregulation.

There is great social and economic importance in quantifying the effects of deregulation on foreclosures. In the short-run, foreclosures cause more crimes in the neighborhood ([Immergluck and Smith \[2006b\]](#), [Cui \[2010\]](#)), lower the property values of nearby homes, and reduce the tax base of local governments ([Immergluck and Smith \[2005\]](#), [Immergluck and Smith \[2006a\]](#), [Schloemer et al. \[2006\]](#), [Schuetz et al. \[2008\]](#), [Lin et al. \[2009\]](#)). In the long-run, foreclosures hinder residential investment and durable consumption ([Mian et al. \[2011\]](#)).

## 1.2 EVIDENCE FROM CLEVELAND

### 1.2.1 The Federal and Local Home Mortgage Regulation

Although my empirical evidence is drawn from Cleveland, Ohio, the mortgage regulation in Cleveland is not a unique case. Rather it fits into the broader framework of home mortgage regulation across the nation. In 1994, the U.S. Congress enacted the first comprehensive home mortgage lending statute, the Home Ownership and Equity Protection Act (HOEPA). Afterwards, many states adopted similar legislation. Both federal and state home mortgage regulations are defined in two parts ([Ho and Pennington-Cross \[2006\]](#)). The first stage defines the coverage of the law. A mortgage belonging to certain product categories is considered to be a "high-cost loan". Therefore,

it is regulated by law if it carries fees and an Annual Percentage Rate (APR) that exceed certain thresholds (so-called “trigger points”). In a second section, certain requirements and restrictions, such as mandatory counseling and disclosure and limits on prepayment penalties and balloon payments, are imposed on covered loans. Typically, state laws enhance both coverage and restrictions mandated under HOEPA.

HOEPA covers only a small share of home mortgage loans, namely, those having high fees and high interest-rates. Moreover, HOEPA leaves home-purchase loans unregulated. However, home-purchase loans are directly related to home foreclosures. Ohio home mortgage regulation incorporates the coverage of HOEPA and regulates only closed-end home equity loans. However, the Cleveland Home Mortgage Lending Ordinance of 2002<sup>4</sup> extended HOEPA coverage to all home loans including home-purchase loans. Moreover, the Cleveland Home Mortgage Lending Ordinance implemented lower interest and fee thresholds and resulted in wider coverage. For instance, among first-lien loans, Ohio law regulates only “those having an interest rate that exceeds by more than 8 percentage points over the yield on Treasury securities.” The Cleveland ordinance extended coverage to “those having an interest rate between 4.5 and 8 percentage points above the yield on certain Treasury securities.”

In addition to restrictions under Ohio state law, the Cleveland Ordinance prohibited prepayment penalties, loan flipping, balloon payments, negative amortization, an increased interest rate on default, advance payments, financing of credit insurance, lending without counseling, lending without due regard to prepayment, and, under certain circumstances, payments to home improvement contractors. These mortgage contract terms directly and indirectly affect borrowers’ payment burdens and therefore probabilities for default and foreclosure.

While the Cleveland ordinance remained in effect, mortgage loans secured by home properties within the city limits of Cleveland were governed by the ordinance, regardless of the location of lenders’ offices. As a result, lenders could not circumvent regulation by redirecting their borrowers to suburban offices. This regulatory design made it possible to identify regulated loans by the locations of properties that secured loans.

The Cleveland Home Mortgage Lending Ordinance was passed on April 23, 2002, and was immediately challenged by the American Financial Services Association (AFSA). The Cleveland ordinance first took effect on July 26, 2002, when AFSA’s preliminary injunction was denied, and temporarily ended on September 22, 2003, when Cuyahoga County Court of Common Pleas de-

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<sup>4</sup>Ordinance 737-02, coded as Section 659.02, passed on April 23, 2002; meant to be effective on April 25, 2002.

clared the Cleveland Ordinance to be in conflict with Ohio state law. The Cleveland Ordinance was reinstated on December 2, 2004, when the Eighth District Court of Appeals ruled the Cleveland Ordinance valid. On November 20, 2006, the Supreme Court of Ohio ruled the Cleveland Ordinance unconstitutional on the grounds that it implemented greater restrictions than Ohio law.<sup>5</sup> Interpreting this court ruling as a deregulation, this paper investigates the quality and quantity of home loans made in Cuyahoga County between June 2006 and May 2007. Within the sampling period, the Cleveland home mortgage regulation was effective in the first 6 months and invalid for the next 6 months.<sup>6</sup>

Before the Eighth District Court of Appeals ruled in 2004 that the Cleveland Ordinance was valid, there were precedents in which judges overturned local home mortgage regulation. In particular, the Second District Court of Appeal of Ohio ruled in June, 2004, that the Dayton Home Mortgage Ordinance was invalid. Prior to actual repeal of the Cleveland ordinance, it is possible that borrowers and lenders anticipated that the law would be overturned eventually. If lenders and borrowers could ignore the law before it was actually repealed because of weak law enforcement in anticipation of a repeal, the DID estimate will be biased towards zero. Any non-zero effects can be interpreted as the lower bound of the true effect.

### 1.2.2 The Matched Mortgage and Foreclosure Data

The loan and foreclosure data used in this paper come from the Loan Origination and Foreclosure Matched Data of Cuyahoga County, available through NEO CANDO (Northeast Ohio Community and Neighborhood Data for Organizing<sup>7</sup>). Matching the Home Mortgage Disclosure Act (HMDA) data with the Cuyahoga foreclosure filings, the data set records individual home-purchase loans in Cuyahoga County made from 2005 – 2008 and tracks their foreclosure filings up through December 2009. Each record has information about the loan, the borrower, the parcel location of the home property, mortgage registration date, foreclosure filing date, and court auction date. See [Coulton et al. \[2008b\]](#) for a detailed explanation of the statistical matching procedure.

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<sup>5</sup>Appendix [A.1](#) provides a review of the lawsuit.

<sup>6</sup>After the Cleveland Home Mortgage Ordinance was overturned, home purchase loans were no longer regulated anywhere in Cuyahoga County, home equity loans in both Cleveland and the suburbs were governed by Ohio state law.

<sup>7</sup>NEO CANDO is a free and publicly accessible social and economic data system of the Center on Urban Poverty and Community Development, a research institute housed at Case Western Reserve University's Mandel School of Applied Social Sciences.



My sample of loans includes home-purchase loans secured by owner-occupied one- to four-family housing properties in Cuyahoga County. Because foreclosure filings are censored in December 2009, a loan is defined as “early foreclosed” if the lender/ servicer files a foreclosure complaint within 30 months after loan origination.<sup>8</sup> Besides the data availability, another important reason to define early foreclosures with a 30-month moving window is that a typical Adjustable Rate Mortgage (ARM) has an interest rate reset in 3 years or 5 years. By using a narrower window to define early foreclosures, I avoid the complication of treatment effects driven by ARM interest rate resets.

I aggregate individual loan data from Cuyahoga County to tract-month level. The constructed longitudinal data set consists of 5,496 monthly observations of 458 tracts in Cuyahoga County between June 2006 and May 2007, i.e. six months before and after the repeal of the law. During this period, on average 12.1 out of 100 matched loans in the sample ended up in early foreclosures.

Table 1.1 provides the summary statistics of the treatment group and control group. The outcome variables to be investigated include the number of early foreclosed loans, the number of originated loans, and the amount of originated loans. The average foreclosure rates are also provided as a reference. Each variable is measured at the tract level in each month between June 2006 and May 2007. On average there are more loans originated in the suburb tracts than in the city tracts. However, foreclosure rates are generally higher in the city. As a result, the average number of foreclosed loans in a tract is similar for the city and for the suburbs. Compared to the period before the repeal, both the city and the suburbs experience a decline in loan origination in the period after the court nullified the ordinance.

### 1.2.3 Empirical Strategy

Before the Ohio Supreme Court repealed the home mortgage regulation of Cleveland on November 20, 2006, mortgages secured by home properties in Cleveland were governed by municipal law while those secured by properties in the suburbs were governed by state law. After November 20, 2006, the mortgages in the suburbs remained in the same regulatory regime, while those in Cleveland were deregulated. Using the nearby municipalities as a control for credit demand and credit quality

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<sup>8</sup>Foreclosure filing as an outcome is a measure for loans in extreme cases. A foreclosure filing initiates the judicial foreclosure process mandated by Ohio law, but may not result in an actual foreclosure if the borrower successfully pays off the amounts due in the grace period or a mortgage modification is reached. In some scenarios, borrowers may have short sales or file for chapter 13 bankruptcy to avoid foreclosure. According to [Cutts and Merrill \[2008\]](#), the expected time from foreclosure filings to the completion of foreclosure sales for Ohio foreclosures is 372 days, the actual average time is 480 days.

Table 1.1: Summary statistics: Cuyahoga County, June 2006- May 2007

VARIABLES	Before Repeal				After Repeal			
	Suburban		Cleveland		Suburban		Cleveland	
	mean	sd	mean	sd	mean	sd	mean	sd
Foreclosed loans	0.27	(0.59)	0.22	(0.49)	0.11	(0.37)	0.14	(0.39)
Total loan count	2.58	(2.23)	0.96	(1.34)	1.55	(1.61)	0.59	(0.96)
Total amount(\$1,000)	397.79	(443.92)	89.36	(134.20)	247.29	(324.07)	54.02	(96.35)
Foreclosure rates(%)	9.26	(22.40)	20.96	(36.23)	6.71	(21.20)	22.03	(38.25)
Number of tracts	276	276	182	182	276	276	182	182

Notes: Summary statistics of count/amount observed in a census tract in a given month.

in Cleveland prior in the absence of the city’s home mortgage regulation, a DID estimation can compare the outcomes of home mortgages in Cleveland before and after repeal of the law.

The DID estimation’s causal inference hinges on the exogeneity of the city law’s repeal. Given that AFSA challenged the law immediately after its enactment in 2002, repeal of the law in 2006 is not endogenous to composition of loans or lending and borrowing behaviors between 2002 and 2006. Although there might have been some expectation of AFSA winning the case, timing of the court ruling is exogenous. In my analysis, I investigate only loans made between May 2006 and June 2007, i.e., six months before until six months after the court ruling. If lenders and borrowers anticipated that AFSA would win the case and ignored the law before it was actually repealed, my estimation results can be interpreted as the lower bound of the true effect.

Respectively, from top to bottom in each panel, Figure 1.1 presents Cleveland and suburban time trends for the (log) number loans with early foreclosures, the (log) number of loans, and the (log) total loan amount. Even before repeal, gaps appear between Cleveland time trends and suburban time trends in each panel because of social and economic characteristics that differ between the two groups. The DID method allows for differences between the treatment and control groups, as long as those differences persist in the absence of the treatment. In all three panels,

Cleveland and suburban municipalities share common time trends prior to repeal, which suggests that the suburban municipalities provide a reasonable control group for Cleveland. The DID method assumes Cleveland and suburban municipalities would continue to follow the same time trend after November 2007 if deregulation had not occurred. In this way, the method measures the impact of deregulation as the deviation between Cleveland and suburban time trends for outcomes following the repeal of the law.

Figure 1.1 shows an obvious seasonal pattern: time trends for all outcomes in both groups decline toward the end of 2006. The declines could be attributed to reduced house-hunting during the Cleveland winter.<sup>9</sup> Although both originations and amounts of home mortgages rise in early 2007, the number of foreclosed loans remains low for both city and suburbs, which suggests that foreclosed loans were generally less frequent after November 2006. As reported in Table 1.1, average foreclosure rates are lower in this period. It is likely that lenders were systematically more conservative in extending credit risk prior to the global financial crisis. Evidence in Section 1.3.5 suggests that both Cleveland and its suburbs saw sharp increases in foreclosures on vantage mortgages in the 6 months prior to November 2006, i.e., those originated before my sampling period. In managing the increase in failing loans in Cuyahoga County, lenders may have wanted to tighten lending standards.

The pattern of time trends at the end of 2006 calls for a control group. Otherwise, one may misinterpret the general decrease in foreclosures as an impact of deregulation. As the control group in the DID design, the suburbs control for factors that have common impact on city and suburban tracts. However, if the seasonal pattern, financial crisis, or unobserved factors have differential impact on the two groups, one may misinterpret the impact of those factors as treatment effects of deregulation. I address this concern in several ways. In Section 1.3.1, I provide robustness cases for loans made in a narrower window of time and loans made on the boundary of the city limits of Cleveland. I find comparable treatment effects. In Section 1.3.4, I test the seasonal pattern and financial crisis as alternative explanations; in Section 1.3.5, I statistically test the housing bubble and bad economy as alternative explanations. I find no statistically significant evidence to support those alternatives.

Satisfying the assumption of common time trend in the absence of treatment, the DID method identifies the impact of deregulation as the deviation between the Cleveland time trend and the suburban time trend in outcome variables following the court's ruling against home mortgage

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<sup>9</sup>The top left panel of Figure 1.2 shows the time trends for foreclosed loans between June 2005 and May 2006, and the same seasonal pattern appears at the end of 2005.

Figure 1.1: Time Trend: May 2006–June 2007



Notes: The sample includes home purchase loans for 1- to 4-family housing units secured by owner-occupied housing properties in Cuyahoga County.

regulation. Even without rigorous statistical testing, one can visually spot whether a deviation occurs between the time trends of the two groups. After repeal, the gap in early foreclosure (Top Panel) between two groups disappears, while loan count (Middle Panel) and loan amount (Bottom Panel) in the two groups track each other closely. Those patterns suggest that there was a relative increase in foreclosed loans for Cleveland after deregulation. However, there is no change in overall originations and total loan amounts.

Given the evidence in the time trends, I propose a panel Poisson model to identify the treatment effect. The Poisson specification assumes the treatment group and the control group would have had proportional transitory changes in the count outcomes if deregulation had not happened. This specification is preferred to an OLS specification where the dependent variable is the count. In other words, the latter specification assumes a level of transitory change in both groups in the absence of the treatment. This assumption is inappropriate because the two groups start with very different levels of counts. Consequently, the transitory shock is dramatic. The specification is also preferred to an OLS specification where the foreclosure rate is the outcome variable. Due to the low frequency of the originations and especially early foreclosures at the tract-month level, the normal distribution assumption of OLS will be violated because of inflated zeros. The Poisson estimation accounts for the cluster of zeros.

The estimation equation is the following:

$$E(y_{it}|X) = \exp(\theta_i + \eta_t + \alpha_1\delta^c + \alpha_2\gamma^R + \alpha_3(\delta^c \times \gamma^R)) \quad (1.1)$$

$y_{it}$  is the loan count for census tract  $i$  in month  $t$ . In the foreclosure specification,  $y_{it}$  is the count of loans that were made in census tract  $i$  in month  $t$  and were foreclosed within 30 months after origination. In the loan origination specification,  $y_{it}$  is the count of loans that were made in census tract  $i$  in month  $t$ .  $\theta_i$  is the tract fixed effects that control for all time-invariant characteristics of the tracts. Given the short sampling period (12 month), the population, housing stocks, demographics of residents, and so forth are likely to remain stable and therefore be perceived as time-invariant.  $\eta_t$  is the month fixed effects that control for any common shocks to all of Cuyahoga County in a month.  $\delta^c$  is a dummy for tracts in Cleveland.  $\gamma^R$  is a dummy for the periods after the repeal. In a panel Poisson specification,<sup>10</sup>  $\alpha_3$  is the log odds ratio that measures the treatment effect and is the coefficient of interest.

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<sup>10</sup>A user written Stata module, XTPQML, is used to estimate Fixed-effects Poisson (Quasi-ML) regression with robust standard errors.

$$\alpha_3 = \log \frac{E(y_{Cleveland,after}|X)}{E(y_{Cleveland,before}|X)} / \frac{E(y_{Subs,after}|X)}{E(y_{Subs,before}|X)} \quad (1.2)$$

A positive  $\alpha_3$  suggests that the odds ratio is greater than 1 and the event is more likely in Cleveland after deregulation.

The causal interpretation of the DID estimates relies on similarities between control and treatment groups as well as the comparability of observations before and after treatment. I provide two robustness tests on sub-samples with enhanced similarity. The first sub-sample includes only the tracts on each side of the city border (Pence [2006]). The second sub-sample narrows the observation window to three months before and after repeal. Furthermore, to check the robustness of the sample, I restrict it to loans with foreclosures earlier than 24 months and loans not preempted from local home-mortgage regulation by federal agencies .

## 1.3 EMPIRICAL RESULTS

### 1.3.1 The Effect of Deregulation on Home Foreclosures

In this section, I estimate Equation 1.1 and test whether deregulation causes an increase in bad loans (measured in early foreclosures). The results are reported in Table 1.2. All specifications include month and tract fixed-effects, and standard errors are clustered at the tract level.

Column (1) is the baseline case. It presents the number of early foreclosures, which are defined as foreclosure complaints filed in court by the lender or servicer within 30 months after the loan origination. The sample includes tract-level observations six months before and after the repeal of regulation. The coefficient of interest is the interaction between Cleveland and repeal. This is the treatment effect of deregulation. The point estimate is positive and precisely estimated. This suggests the repeal caused more bad loans. Specifically, the point estimate of 0.4 translates into an odds ratio of 1.49. This implies a 49 percent increase in the number of loans with early foreclosures in Cleveland after the repeal. The estimate is significant at 1 percent level.

According to the federal preemption enforced by the Office of the Comptroller of the Currency (OCC) in 2004<sup>11</sup>, some lending restrictions implemented by the Cleveland Ordinance did not apply

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<sup>11</sup>See Appendix A.2 for a short review.

Table 1.2: Effect of Deregulation on Foreclosures

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Baseline	Non-preempted	b=3 months	Border	24-month
Cleveland×Repeal	0.40*** (0.14)	0.47*** (0.16)	0.36** (0.18)	0.44* (0.24)	0.49*** (0.16)
Observations	4,200	2,681	1,560	1,332	3,804
Number of tracts	350	313	260	111	317
Tract FE	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES
Control	NO	NO	NO	NO	NO

Notes: The dependent variable is the number of loans that are made in a tract-month combination and end up with early foreclosures. Column 1, Column 2, and Column 5 include all tracts in Cuyahoga county between June 2006 and May 2007. Column 1 counts all loans; Column 2 counts only loans made by lenders that do not enjoy federal preemption of local mortgage regulations. Column 3 includes all tracts in Cuyahoga County between September 2006 and February 2007. Column 4 includes tracts on Cleveland city border between June 2006 and May 2007. Column 1–4 defines early foreclosures as foreclosure complaints filed by lenders within 30 months after origination; Column 5 defines early foreclosures as foreclosure complaints filed by lenders within 24 months after origination. *Cleveland* is a dummy for census tracts in Cleveland. *Repeal* is a dummy for the period after November 30, 2006. Estimates from a panel Poisson estimation are reported. Robust standard errors in parentheses are clustered at the census tract level. \*\*\*  $p \leq 0.01$ , \*\*  $p \leq 0.05$ , \*  $p \leq 0.1$

to national banks and their subsidiaries. The impact of deregulation would be smaller for national banks than for state banks. To deal with this compositional effect, I exclude loans made by national banks in Column (2). The estimate of impact is similar to the baseline case.

Since the deregulation in November 2006 more or less coincided with onset of the global financial crisis<sup>12</sup>, the baseline estimate may capture the differential impact of the crisis between Cleveland and its suburbs. In order to address this concern, in column (3) I eliminate the the periods of four to six months before and after deregulation. The estimates in column (3) are similar to the baseline results, which suggests that differential shocks to the city and its suburban municipalities do not drive the treatment effect.

Another concern is whether the suburbs serve as a legitimate control group. As shown in the top panel of Figure 1.1, the foreclosure time series of Cleveland and its suburbs followed the same trend before deregulation. This suggests that the suburbs provide a legitimate control. To enhance similarity between the treatment and control groups, I investigated a sub-sample of loans that are secured by housing properties in tracts along the city border. Column (4) reports the estimate of the impact of deregulation on secured loans in areas with the most similar economic and social conditions. The estimate size is comparable to the baseline case.

In Columns (1)-(4), early foreclosure is defined by a 30-month moving window. One may wonder whether the increased early foreclosures are driven by timing effects. Column (5) in Table 1.2 defines early foreclosures by a 24-month window and still shows a positive estimate of similar size.

The results in this section suggest that deregulation stimulated an increase in loan foreclosures. One interpretation is that deregulation encouraged borrowers to obtain more costly loans. After repeal, lenders freely charged interest rate spreads exceeding 4.5 percentage points and imposed other non-interest terms in the contract. For example, balloon payments, increased interest rates following defaults, and prepayment penalties were no longer prohibited on loans with interest-rate spreads greater than 4.5 percentage points. Those loans terms, when implemented by lenders, increased borrowers' payment burdens and thereby increased the probability of defaults and foreclosures. In particular, [Quercia et al. \[2007\]](#) show that refinanced loans with prepayment penalties or balloon payments are more likely to experience foreclosure than loans without those features.

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<sup>12</sup> <http://timeline.stlouisfed.org/pdf/CrisisTimeline.pdf>



### 1.3.2 The Effect of Deregulation on Loan Originations and Amounts

The estimation results from the panel Poisson model provide evidence of increased foreclosures in absolute counts but not as a share of total loan originations. If originated loans in Cleveland increased at a higher rate than foreclosures, then the share of bad loans may actually have decreased following deregulation. To determine whether foreclosures increased as a share of overall loans, I further investigated the impact of deregulation on loan originations.

The dependent variable in Panel A of Table 1.3 is the monthly count of originated loans in each tract of Cuyahoga County before and after deregulation. All specifications include the tract fixed effects and month fixed effects. The estimate of the coefficient  $\alpha_3$  in Equation 1.1 and the tract-clustered standard errors are reported. The Column (1) reports the baseline case, where the sample includes tract observations in all of Cuyahoga County six months before and after repeal. The precise estimate is very close to zero in magnitude. It indicates that deregulation did not have a significant impact on loan origination. Combined with increased foreclosures identified in the previous section, this estimate provides evidence that foreclosures as a share of overall loans increased following deregulation in Cleveland.

Because of federal preemption, local home mortgage regulation affects national banks less frequently than state banks and non-depository lenders. The sample in Column (2) excludes the loans made by national banks. Estimated treatment effects are near zero and similar in size to those in the baseline case. This result suggests that the distribution of loan originations under state and national charters remained the same after repeal. In turn, that stable distribution implies that increased early foreclosures were not the result of borrowers seeking mortgages under the charter that allowed mortgages to carry heavier risks.

To enhance the similarity of the observations between the control and treatment groups before and after treatment, I provide two more robustness checks. Column (3) restricts the sample to loans made within three months before or after repeal. Column (4) restricts the sample to loans made in tracts along the city border. These estimates also show no significant change in the loan count in Cleveland after the repeal.

In Panel B, Table 1.3, I estimate the impact of deregulation on total loan amounts. Across four specifications, estimates differ insignificantly from zero, which suggests total loan amounts did not change as a result of mortgage deregulation in Cleveland. The unchanged loan count and

Table 1.3: Effect of Deregulation on Loan Origination and Amount

	(1)	(2)	(3)	(4)
VARIABLES	Baseline	Non-preempted	b=3 months	Border
<i>Panel A: Loan Count</i>				
Cleveland×Repeal	0.02	0.03	0.01	-0.04
	(0.05)	(0.06)	(0.08)	(0.09)
Observations	5,496	3,517	2,622	1,560
Number of tracts	458	436	437	130
<i>Panel B: Loan Amount</i>				
Cleveland×Repeal	-0.03	-0.01	-0.01	-0.07
	(0.06)	(0.06)	(0.09)	(0.13)
Observations	5,496	3,517	2,622	1,560
Number of tracts	458	436	437	130
Tract FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES
Control	NO	NO	NO	NO

Notes: The dependent variable in Panel A is the number of loans made in a tract-month combination. The dependent variable in Panel B is the total loan amount in a tract-month combination. Column 1 and Column 2 include all tracts in Cuyahoga County between June 2006 and May 2007. Column 1 counts all loans; Column 2 only counts loans made by lenders that do not enjoy federal preemption of local mortgage regulations. Column 3 includes all tracts in Cuyahoga county between September 2006 and February 2007. Column 4 includes tracts on Cleveland city border between June 2006 and May 2007. *Cleveland* is a dummy for census tracts in Cleveland. *Repeal* is a dummy for the period after November 30, 2006. Estimates from a panel Poisson estimation are reported. Robust standard errors in parentheses are clustered at the census tract level. \*\*\*  $p \leq 0.01$ , \*\*  $p \leq 0.05$ , \*  $p \leq 0.1$

unchanged total loan amount implies that the average loan amounts after the repeal in Cleveland remained unchanged relative to average loan amounts in suburban municipalities.

### 1.3.3 Differential Effects by Lender Types and Loan Types

Since the Cleveland Home Mortgage Lending Ordinance regulated only “high-cost” loans and certain “predatory” lending behaviors, one may expect the deregulation to affect subprime borrowers and lenders more than prime borrowers and lenders. I ran DID estimations by loan types and lender types to differentiate the impact. A loan is defined as “subprime” if the spread between its annual percentage rate (APR) and the applicable Treasury yield is equal or greater than three percentage points. A lender is defined as “subprime” if it was listed in the 2005 subprime lender list by the Department of Housing and Urban Development (HUD). Those definitions follow the convention in the literature ([Ho and Pennington-Cross \[2006\]](#); [Bostic et al. \[2008\]](#)).

Table 1.4 reports the DID results by loan types and lender types. The outcome variable in Panel A is the number of foreclosed loans. The estimate for loans with subprime interest rates (Column 2) is significant and similar in size to the one in the baseline case (Column 1). However, the estimate for loans with prime interest rates is less precise (Column 3). A caveat here is that some “prime” loans were also covered by the Cleveland Ordinance if fees exceeded the regulatory threshold. Results in Column (4) and Column (5) imply that loans made after repeal by both subprime and prime lenders experienced an increase in early foreclosures of similar magnitude. This is precisely because both subprime and prime lenders made loans that were regulated by the Cleveland Ordinance.

Panel B reports loan origination results by loan type and lender type. Although the total loan count did not change after deregulation, the number of loans made with subprime interest rates and the number of loans made by subprime lenders increased significantly in Cleveland relative to the suburban municipalities after the repeal. A coefficient of 0.27 translates into an odds ratio of 1.31 and implies a 30 percent increase in the subprime loans. A coefficient of 0.34 translates into an odds ratio of 1.40 and implies a 40 percent increase in the loans made by subprime lenders. Meanwhile, the number of loans with prime interest rates and the number of loans made by prime lenders did not increase after repeal. These results suggest that although deregulation did not affect overall loan-origination levels, it changed the composition of loans.

Table 1.4: Differential Effects By Lender Type and Loan Type

	(1)	(2)	(3)	(4)	(5)
	By APR			By Lender	
	All	Subprime	Prime	Subprime	Prime
<i>Panel A: Count of Loans with Early Foreclosures</i>					
Cleveland×Repeal	0.40*** (0.14)	0.50*** (0.17)	0.39 (0.27)	0.44* (0.23)	0.32* (0.18)
Observations	4,200	3,528	1,722	2,664	3,444
Number of tracts	350	294	186	222	287
<i>Panel B: Origination Count</i>					
Cleveland×Repeal	0.02 (0.05)	0.27*** (0.09)	0.02 (0.07)	0.34** (0.16)	0.06 (0.06)
Observations	5,496	4,980	5,148	4,128	5,412
Number of tracts	458	415	429	344	451
Tract FE	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES
Control	NO	NO	NO	NO	NO

Continued on Next Page...

Table 1.4 – Continued

	By APR			By Lender	
	All	Subprime	Prime	Subprime	Prime
<i>Panel C: Loan Amount</i>					
Cleveland×Repeal	-0.03 (0.06)	0.23** (0.10)	-0.01 (0.08)	0.34* (0.18)	0.00 (0.07)
Observations	5,496	4,980	5,148	4,128	5,412
Number of tracts	458	415	429	344	451
Tract FE	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES
Control	NO	NO	NO	NO	NO

Notes: All coefficients are estimated from a panel Poisson model where the dependent variable is the count/amount of loans in a tract-month combination. Loans with early foreclosures are defined by a 30-month moving window. Column 1 includes all loans; Column 2 includes loans with subprime interest rates, Column 3 includes loans with prime interest rates; Column 4 includes loans made by subprime lenders defined by the Department of Housing and Urban Development (HUD), Column 5 includes loans made by prime lenders. *Cleveland* is a dummy for census tracts in Cleveland. *Repeal* is a dummy for the period after November 30, 2006. Robust standard errors in parentheses are clustered at the census tract level. \*\*\*  $p \leq 0.01$ , \*\*  $p \leq 0.05$ , \*  $p \leq 0.1$

Similar patterns were found for loan amounts in Panel C. The total loan amount for both loans with subprime rates and loans by subprime lenders increased after deregulation. The increase in loan amount is similar in magnitude to the increase in loan count. This suggests that subprime loans made after deregulation were of comparable size to those made before deregulation.

### 1.3.4 Falsification Tests: Timing and Locality

In this section, I test whether my result for increased early foreclosures in Cleveland after deregulation is driven by seasonal patterns in the credit market, shocks to the city, or shocks from the financial crisis.

Figure 1.1 shows a salient seasonal pattern in loan origination and foreclosures. First, I ran falsification tests for artificial “repeals” 12 months and 6 months before actual repeal. If my findings are driven entirely by seasonal patterns, one would see a “treatment” effect in the same month every year or half-year, even without repeal.

Graphs in the top panel of Figure 1.2 present Cleveland and suburban time trends six months before and after the false treatments in November 2005 and May 2006. Although there is a salient seasonal pattern for both treatment and control groups, there is no visual evidence of deviation between the time trends of the two groups. Column (1) and Column (2) in Table 1.5 present estimation results for false treatments in November 2005 and May 2006. There are no statistically significant treatment effects in those falsifications. This result suggests that the treatment effect in the baseline case is not driven by seasonal pattern in the home credit market.

Moreover, as mentioned in Section 1.2.3, the coincidence of repeal and the global financial crisis may lead one to misinterpret the impact of the financial crisis as the impact of deregulation. To address this concern, I ran two more falsifications. I first restricted the sample to suburban tracts only and then designated the “inner suburban ring” as the treatment group and the “outer suburbs” as the control group. If economic shocks to Cleveland in November 2006 explain the increase in foreclosed loans, one would expect the same shock to produce a “treatment effect” for suburban tracts adjacent to the city. In addition, if there were a change in home purchasing or payment preferences among borrowers in Cleveland after November 2006, then such preferences were likely to be contagious across the city border. One would expect to find a treatment effect for inner suburban tracts if a change in preference explains the increase in early foreclosures.

Figure 1.2: Falsifications of Early Foreclosures



Notes: The upper panels are treatments at false timing, the lower right panel is the treatment on the suburban tracts on the city border, the lower right panel is the treatment on Pittsburgh.

Table 1.5: Falsification Test: Foreclosures

	(1)	(2)	(3)	(4)	(5)
	Nov. 2005	May 2006	Inner Subs	Pittsburgh	Pittsburgh
	6 month	6 month	6 month	6 month	3 month
City×Repeal	-0.13 (0.11)	-0.17 (0.12)	0.02 (0.20)	0.32 (0.24)	-0.02 (0.39)
Observations	4,524	4,320	2,544	2,412	756
Number of tracts	377	360	212	201	126
Tract FE	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES
Control	NO	NO	NO	NO	NO

Notes: The dependent variable is the number of loans made in a tract-month combination and ending in foreclosures within 30 months after origination. Column 1 includes all tracts in Cuyahoga County between June 2005 and May 2006. Column 2 includes all tracts in Cuyahoga County between December 2005 and November 2006. Column 3 includes the suburban tracts in Cuyahoga County between June 2006 and May 2007. Column 4 includes all tracts in Allegheny County between June 2006 and May 2007. *City* is a dummy for census tracts in Cleveland (Column 1 and Column 2), the tracts in “inner suburbs” (Column 3), and tracts in Pittsburgh (Column 4 and Column 5). *Repeal* is a dummy for the period after November 30, 2005 (Column 1), or May 31, 2006 (Column 2), or November 30, 2006 (Column 3–5). Estimates from a panel Poisson estimate are reported. Robust standard errors in parentheses are clustered at the census tract level. \*\*\*  $p \leq 0.01$ , \*\*  $p \leq 0.05$ , \*  $p \leq 0.1$



On the bottom panel of Figure 1.2, the left graph shows time trends for the inner suburban ring and the outer suburbs. There is no visual evidence of the gap closing between the two time trends. Column (3) represents the result for a false treatment of the inner suburban ring. The estimation result finds no statistically significant treatment effect for inner suburban tracts. This result suggests that differential shocks do not explain increased foreclosures in Cleveland after deregulation.

I also ran a DID estimation for observations in Pittsburgh and its suburbs during June 2006 to May 2007.<sup>13</sup> In comparison with the suburbs, if the global financial crisis systematically caused differential impacts on Cleveland during the treatment period, one would expect to find more foreclosures in other cities by comparison to their suburbs in the same period.

On the lower panel of Figure 1.2, the right figure plots time trends of early foreclosures for Pittsburgh and its suburbs. Although the time series for Pittsburgh and its suburbs are noisier than those for Cleveland and its suburbs, both series share similar trends before the treatment. The gap between the Pittsburgh trend and the suburban trend does not change after the Cleveland law was repealed. This result suggests no divergence of loan quality in Pittsburgh after November 2006.

Using a 6-month and 3-month observation window respectively in Table 1.5, Column (4) and Column (5) present the falsifications for “fake” deregulation in Allegheny County in November 2006. The early foreclosure is defined by a 30-month moving window. Neither estimate is statistically significant. This suggests that the differential shock of the financial crisis was unlikely to have caused increased foreclosures in Cleveland.

### 1.3.5 Treatment Effect: Deregulation vs. Economic Shocks

In Section 1.3.1, using the suburban municipalities as a control group, I find more bad loans in Cleveland after deregulation than before deregulation. A causal interpretation of the treatment effect assumes that, if deregulation never happened, there would have been no divergence in loan quality developed between Cleveland and its suburbs. This section tests whether confounders such

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<sup>13</sup>The Allegheny home foreclosure records up through November 2009 are matched with the 2005–2008 HMDA loan records, using a similar procedure as by Coulton et al. [2008b]. Thirteen out of 100 matched loans in the Cuyahoga sample were foreclosed by the end of December 2009, 6 out of 100 matched loans in Allegheny county were foreclosed by the end of November 2009. During June 2006 and May 2007, 12.1 out of 100 matched loans in Cuyahoga ended up with early foreclosures, 3 out of 100 matched loans in Allegheny ended up with early foreclosures.

as the housing market bubble and bad economy would have caused more foreclosures (Haughwout et al. [2008]).

As documented in the literature (Jackson and Kasperman [1980]; Deng et al. [2000]; Foote et al. [2008]; Quercia and Stegman [1992]), borrowers default on home mortgages when a mortgage balance exceeds a house's value (the negative-equity theory) or income shocks such as unemployment and divorce occur (the trigger event theory). Accordingly, even in the absence of deregulation, we should expect more loans to be foreclosed during the treatment period when 1) the treatment-period loans reflect higher housing prices at origination (a housing bubble) or 2) borrowers are vulnerable to income shocks (a bad economy).

First, I tested whether individual housing prices were higher between December 2006 and May 2007 than between June 2006 and November 2006. I ran an OLS estimation with a DID specification where the dependent variable is an individual housing sales price in my sample period. The interaction term between Cleveland and the deregulation measures the expected change in the difference between sale prices for Cleveland housing and those for suburban housing after the repeal. Given housing characteristics remain constant.<sup>14</sup> If loans made after November 20, 2006, in Cleveland start with higher housing prices at origination than prices of homes sold before that date, one would expect a positive estimate.

In Table 1.6, Column (1) and Column (2) report the coefficients and tract-clustered standard errors. Column (2) also controls for housing characteristics such as overall condition, number of units built on a parcel, total usable square footage, year of construction, building style, external wall type, and number of bedrooms and bathrooms. Both specifications control for tract fixed effects and month fixed effects. The resulting insignificant negative coefficients provide strong evidence that the increased number of loans with early foreclosures in Cleveland after the repeal was unlikely to be the result of stress from higher housing prices.

Next, I tested whether the local economy between December 2006 and May 2007 was worse than between June 2006 and November 2006. Because monthly unemployment rates and household incomes are unavailable, I use the number of contemporaneous foreclosures in a given census tract as a measure for the social and economic stress in the neighborhood. More newly reported foreclosures

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<sup>14</sup>The sales price and property characteristics at parcel level are from Cuyahoga Auditor Parcel Characteristics File, available through NEO CANDO.

Table 1.6: Social and Economic Conditions Before and After the Repeal

	(1)	(2)	(3)
	Sales Price	Sales Price	Foreclosed Homes
Cleveland×Repeal	-7,537.35 (11,087.85)	-6,678.26 (11,032.27)	-0.07 (0.13)
Observations	28,006	28,006	4,032
Tract FE	YES	YES	YES
Month FE	YES	YES	YES
Control	NO	YES	NO

Notes: Column 1 and Column 2 report the coefficients estimated from an OLS model where the dependent variable is the sales price of each home sales transaction during June 2006 and May 2007. Column 2 controls for the overall housing condition, number of units built on the parcel, total usable square footage, year built, building style, external wall type, and number of bedrooms and bathrooms. Column 3 reports the coefficients estimated from a panel Poisson model where the dependent variables is the number of foreclosed home during June 2006 and May 2007 in each tract. The sample only includes the homes with home-purchase loans made between January 1, 2005 and May 31, 2006. *Cleveland* is a dummy for Cleveland. *Repeal* is a dummy for the period after November 30, 2006. Robust standard errors in parentheses are clustered at the census tract level. \*\*\*  $p \leq 0.01$ , \*\*  $p \leq 0.05$ , \*  $p \leq 0.1$

in a tract in a given month imply a worse economic situation for the tract. I ran a panel Poisson estimation to test the treatment effect.<sup>15</sup>

Figure 1.3 plots the trends of foreclosed homes in Cleveland and its suburbs before and after the repeal. Starting from May 2006, there is an uptick in the contemporaneous foreclosures for both the city and the suburbs. This trend suggests a worsening economy in all of Cuyahoga County. Although the number of foreclosed homes is noisy, the gap between the two trends appears to widen after the repeal. This widening gap suggests that there were relatively fewer foreclosed homes in Cleveland in the 6 months after deregulation than in the 6 months before deregulation. The suburbs provide the control group. Column (3) of Table 1.6 reports the estimate of the treatment effect and the tract-clustered standard error. The treatment effect is negative and insignificant. This result suggests that loans in Cleveland after the repeal were unlikely to be made in an environment with more home foreclosures occurring than before repeal.

## 1.4 DISCUSSION

Given that the Cleveland home mortgage regulation had been contested in a lawsuit from its enactment, the findings in this paper are very compelling. To the extent that lenders and borrowers could anticipate deregulation and thus ignored the Cleveland Ordinance before its actual repeal, the increase in early foreclosures would have been greater if the Cleveland home mortgage regulation had not been challenged.<sup>16</sup>

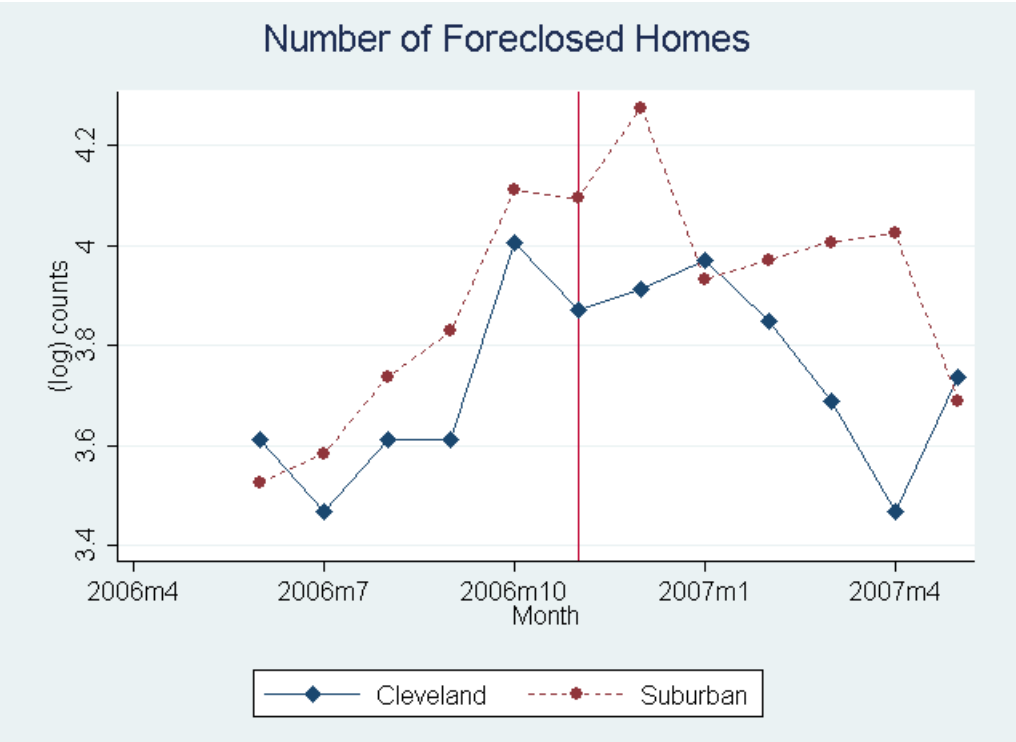
Although repeal of the Cleveland Home Mortgage Ordinance can be perceived as exogenous, the enactment of the Ordinance was likely to be endogenous to pre-existing lending patterns in Cleveland. One would expect the periods of deregulation and regulation to reflect different magnitudes of treatment effects for loan quality. There is anecdotal evidence that immediately following

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<sup>15</sup>The dependent variable is the number of foreclosed homes in tract  $i$  in month  $t$ . The sample includes only loans made before repeal (and hence were not affected by deregulation) but were foreclosed during June 2006 and May 2007 (within 6 months before or after the Ohio Supreme Court's repeal of mortgage regulation in Cleveland. The foreclosure information is from the Loan Origination and Foreclosure Matched Data of Cuyahoga County, 2005-2008. Therefore only foreclosures of homes with loans made between January 1, 2005, and May 31, 2006, are included.

<sup>16</sup>In the sampling period, there was another major institutional change in the home mortgage market in Cuyahoga. On January 1, 2007, Ohio home mortgage lending bill SB 185 took effect. This bill amended most laws regulating consumer real estate lending in Ohio.(Appendix A.3 provides a review of the amendments.) My DID results will be questionable if SB 185 curbs early foreclosures more effectively in the suburban municipalities than in the city. Because Cleveland possessed higher subprime rates and higher early foreclosure rates before SB 185 took effect, it is likely that mortgage lending in Cleveland is more sensitive to the new enforcement of consumer protection measures, and city foreclosures should decrease more than in the suburbs as a result of SB 185 enforcement.

Figure 1.3: Social and Economic Shocks Measured by Foreclosed Homes



Notes: Each data point is the (log) number of foreclosed homes in the specified month in the treatment/ control group.

passage of the Cleveland Ordinance in 2002, many lenders exited the Cleveland market and caused a sharp decline in loan originations. If the law deterred and/or evicted "predatory" lenders, one would expect the magnitude of deregulation to be a lower bound for the magnitude of regulation because only the surviving lenders are observed in my sample. Otherwise, if the law drove honest lenders from the market because compliance costs made it unprofitable to originate honest loans, one would expect the magnitude of deregulation to be an upper bound for regulation.

The significant impact of the Ohio Supreme Court's deregulation of the Cleveland municipal mortgage market hinges on the restrictions and coverage of the Cleveland Home Mortgage Ordinance. The ordinance imposed sharp restrictions on home-purchase loans, which are not regulated by Ohio law yet constitute the majority of loans that end up in foreclosures. If state or local home mortgage regulation does not cover home-purchase loans, one may find smaller or no treatment effects of deregulation on bad loans. By January 21, 2009, 16 of 50 U.S. states had home mortgage regulations that cover home purchase loans. However, eight city home mortgage ordinances implementing stricter restrictions than state law have been preempted or repealed.<sup>17</sup>

The findings in this paper suggest that the return to a deregulated mortgage market in Cleveland ushered in a substantial increase in lenders' issuances of low-quality loans which ended up in early foreclosures. There are at least two reasons why mortgage regulation can help foster healthier loans. Given the complexity of a mortgage contract, in the absence of regulation, an ordinary borrower can easily be lured into a "predatory" contract without awareness. Regulation mitigates the predatory risk arising from information asymmetry by simply banning a comprehensive list of "predatory" contract terms for high-interest loans. Moreover, regulation effectively protects the most vulnerable borrowers; those borrowers who end up with high-interest loans are most likely to be individuals with limited financial literacy.

A thorough social welfare analysis of deregulation, which is beyond the scope of this paper, would take into account both borrowers' and lenders' surpluses and require more informative data about both parties in the transaction. There may be a redistribution of loans on the borrowers' side when some borrowers with no access to home credit under regulation become able to obtain loans after the repeal of mortgage regulation. Those borrowers may or may not be those whose loans are foreclosed later. While lenders made the same number of loans yet foreclosed on more of them, they were still able to make higher profits. With the originate-to-distribute business model,

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<sup>17</sup>Butera & Andrews, Detailed Status Summary Chart of State and Local Predatory Lending Legislation (2007)

lenders earn revenue primarily from originations as well as the interest rates and fees charged; most losses from loan foreclosures are transmitted to the secondary mortgage market.

## 1.5 CONCLUSION

This paper studies the impact of mortgage deregulations on credit flow and loan quality using a natural experiment in Cleveland, OH, where local home mortgage regulation was exogenously repealed by the State of Ohio Supreme Court. Using suburban municipalities as the control group for Cleveland, the DID strategy identifies the impact of deregulation as the deviation between Cleveland and suburban time trends of outcome variables following repeal of Cleveland's municipal regulation of mortgages.

The DID results provide evidence that deregulation did affect the total home credit flow in Cleveland. However, it stimulated the origination of more loans made with subprime interest rates as well as more loans made by subprime lenders. More strikingly, 49 percent more loans ended up with early foreclosures. After deregulation, moreover, both subprime and prime lenders made more loans that ended up in early foreclosures.

These results suggest that the Cleveland home mortgage regulation, without reducing credit supply, implemented restrictions under which originated loans would survive longer without foreclosure. Robustness checks and falsification tests provide evidence that increased early foreclosures are not driven by the timing of foreclosures or seasonal patterns of foreclosure. Moreover, the social and economic environment in which the loans are made and shocks from the worldwide financial crisis were not the primary reasons for increased foreclosures after deregulation. Finally, as an additional point, if the Cleveland Ordinance had not been challenged in court, the repeal would have had greater impact.

The findings in this paper have significant implications for consumer financial protection. They provide the first empirical evidence that 1) regulation of home purchase loans with stricter restrictions than HOEPA would generate loans that survive longer and 2) that home mortgage regulations triggered by high prices and high fees do have greater regulatory impact on the subprime sector than on the prime sector. My paper also suggests that deregulation of consumer financial protection laws gives rise to more bad loans that financially stress borrowers and does not necessarily increase the credit flows.

## 2.0 CREDIT ACCESS AND HOUSING CONSUMPTION: THE CASE OF HOUSING PROVIDENT FUND

### 2.1 INTRODUCTION

Literature has suggested that credit access is an important determinant of homeownership.<sup>1</sup> In the past 20 years, the U.S. government has encouraged credit expansion through affordable loans (Quercia et al. [2003]), government-sponsored enterprises (GSEs) (Bostic and Gabriel [2006]), community-reinvestment act (CRA) lending (Gabriel and Rosenthal [2008]), and non-conventional loans such as VA (Veterans Affairs) loans (Rosenthal et al. [1991]) and FHA (Federal Housing Administration) loans (Duca and Rosenthal [1991]). These efforts have promoted homeownership in the United States effectively, especially among low-income and minority groups (Bostic and Surette [2001]); however, they also caused deterioration in the lending standard, resulting in a large number of mortgage defaults and home foreclosures (Mian and Sufi [2009b], Dell’Ariccia et al. [2008]).

Countries throughout Asia, including China, Thailand, India, and Singapore use housing provident fund (HPF) programs to encourage homeownership. HPFs are of interest in comparative public finance because they are quite different from the programs used in the United States. Functioning as long-term housing savings funds, HPF programs collect deposits, which are a certain percentage of the employee’s salary and matched by the employer, during the employment period. These programs are similar to 401(k) or IRA retirement programs in the United States (Buttimer Jr et al. [2004]); however, the funds can only be withdrawn for home purchases, home improvement, and home construction. In addition, HPF programs allow the depositor to apply for housing loans that can be as large as 10-15 times the contributor’s cumulative deposit in the fund. Compared to commercial home mortgages, HPF loans generally have lower interest rates and lower down pay-

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<sup>1</sup>See Rosenthal [2002] for a review



ment requirements. Because HPF deposits and HPF loans are employment-based, HPF loans have low denial and default rates. Thus, a HPF program can be an alternative for national governments to promote homeownership in a safer and more sustainable way.

Clearly identifying the benefit of HPF loans on the homeownership is challenging because of the biases resulting from sorting and selection. In countries where deposits to a HPF program are voluntary, studying enrollment in the HPF may pick up unobservable differences between participants and non-participants that would also affect homeownership. For example, employees working in more profitable companies may be more likely to enroll in the HPF because their employers will contribute to their housing benefits. Meanwhile, these employees are also likely to earn more income that can be applied toward homeownership regardless of HPF enrollment. In countries where depositing to a HPF is mandatory for certain employment sectors, employees may take HPF loan benefits into account when making career choices: individuals with a higher demand for homeownership may be more likely to work for sectors covered by the HPF program.

To address these concerns, I studied the HPF program in China where the program gradually covered more employment sectors over the years and where the amount of the HPF loan depends on previous deposits. The evolution of China's HPF program provides substantial variation in the HPF loan benefit across households and across time. In this paper, I defined the treatment group as households with both the member head and the spouse working in sectors eventually covered by the HPF program, i.e., double enrollment. I defined the control group as households with either the husband or wife working in a HPF-covered sector, i.e., single enrollment. The treatment group has greater HPF loan benefit in that double enrollment allows them to make larger deposits into the fund and to apply for a larger loan amount. Treatment status is categorized by the household's employment status last observed before HPF loans became available in 1998. As household members may change their sectors afterwards, this design estimates only the intention-to-treat.

Using a longitudinal data set with full tenure and the employment history of households surveyed both before and after the establishment of the HPF program in China, I examined the effect of HPF loan benefit on two outcomes: homeownership and renter status.<sup>2</sup> These two outcomes provide well-defined measures for studying housing consumption. If the HPF loan benefit does promote housing consumption, I expect to find more transition from renter to homeowner for households with double enrollment. Using the difference-in-difference empirical design, I found that the HPF loan benefit for double enrollment increased homeownership by 18 percent. When I controlled for HPF enrollment

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<sup>2</sup>In my data set, households may own or rent housing or receive their housing without charge.

history, one additional year of enrollment in the HPF program increased homeownership by 4 percent for both groups, and double enrollment in HPF increased homeownership by 15 percent. These findings suggest that the cumulative HPF deposit, as the potential source for the down payment and the basis of the loan limit, strongly affect the household's decision to own a home. The results are robust to the inclusion of household characteristics and the national time trend of homeownership. The effect on homeownership did not change when I restricted the analysis to households with a higher demand for housing. This suggests that eligible households are likely to take advantage of the HPF loan benefit to fund their housing consumption.

The sample period from 1989 to 2006 witnessed substantial economic and social changes in China. In particular, the annual income and living standard for urban households improved significantly, and the demand for housing grew rapidly. To test whether the differential in homeownership across HPF enrollment correlates with other social and economic factors that can also contribute to more housing consumption for households with double enrollment, I ran several falsification tests. Specifically, I examined whether there was a differential change across HPF enrollment in ownership of other household assets such as motorcycles, automobiles, washing machines, and cameras. I found no evidence of households with double enrollment holding more of these household assets after HPF loans became available.

The HPF program of 1994 in China was part of a broader housing reform. The reform also initiated the privatization of public housing, housing subsidies, and a dual housing system with affordable housing for low-income households and commercial housing for moderate- to high-income households (Duda et al. [2005] and Deng et al. [2011]). One may wonder to what extent that increased housing consumption is attributable to the difference in housing endowment granted by the privatization of public housing, a phenomenon during 1994-1998 especially for the state sector (Wang and Murie [2000], Fu et al. [2000], Wang [2011], Wang [forthcoming], Iyer et al. [2009], and Iyer et al. [2011]). To answer this question, I included partial ownership resulting from the 1994-1998 housing privatization as an outcome variable and compared the impact of HPF loan benefit to the impact of the entire housing reform. The estimation results suggest that households with double enrollment also benefit more from housing privatization; however, they still enjoyed higher homeownership after HPF loans became available in 1998.

This study contributes to the literature of credit constraint and homeownership (Rosenthal [2002], Bostic et al. [2005], Duca and Rosenthal [1994], Haurin et al. [1996], Zorn [1989]). Linne-man and Wachter [1989] found that both wealth and income constraints reduced homeownership

propensities with a stronger impact for wealth constraints. [Barakova et al. \[2003\]](#) measured the relative importance of credit-, income-, and wealth-based constraints and showed that financing constraints continue to have an important impact on potential homebuyers. My paper suggests that a HPF program can mitigate both the credit- and wealth-based constraints in that it provides low-interest home loans and withdrawals from previous deposits as the source of the down payment. My paper is also in line with the findings that alternative mortgage products with flexible underwriting guidelines ([Quercia et al. \[2003\]](#)) or a government guarantee against default ([Rosenthal et al. \[1991\]](#)) can mitigate the effect of mortgage constraints and promote homeownership for the targeted groups.

To my knowledge, my paper is the first to identify the impact of HPFs. Evidence from various sources and different methods of calculation give mixed findings on the extent of HPF loan use. [Li \[2010\]](#) found, in household surveys in Guangzhou in 2005 and Shanghai in 2006-07, that commercial bank loans are the major source of finance. According to the author, even for Shanghai, the first city in China to establish an HPF program in 1991, HPF withdrawals only contribute to 2.1% of the purchase expense and HPF loans contribute another 3.5%. [Burell \[2006\]](#) estimates that 4-6% of HPF depositors obtain HPF loans, whereas [Deng et al. \[2011\]](#) reports that the ratio of loan beneficiaries to depositors is 25% nationwide, 67% for Shanghai, and 17% for Beijing. Despite the disagreement in the literature on HPF use, my paper finds that HPF loan benefit stimulated the transition from renters to homeowners. This paper also sheds light on the broader literature of Chinese housing reform. Historically, the privatization of housing had various implications for different social and economic groups ([Huang and Clark \[2002\]](#), [Wang and Murie \[2000\]](#), [Wang \[2001\]](#)). Housing in China not only has a direct impact on property rights ([Pudney and Wang \[1995\]](#)) and liquidity constraint ([Wang \[2011\]](#)), but also has an indirect effect on social status ([Wei et al. \[2012\]](#)), entrepreneurial choice ([Wang \[forthcoming\]](#), [Iyer et al. \[2009\]](#)), and labor supply in the private sector ([Iyer et al. \[2011\]](#) and [Fu et al. \[2000\]](#)).

## **2.2 THE CHINESE HOUSING PROVIDENT FUND AND HOUSING REFORM**

The HPF in China, first initiated in 1994 by the state counsel and overseen by the Ministry of Construction and the Ministry of Finance, is a nationwide mandatory savings program for housing based on employment status. The HPF program mandates that employees working in the sectors

covered by the program save a certain share of their monthly salary to the fund, with employers matching the deposit. The employee's accumulated deposits can be withdrawn for a home down payment, home purchase, home improvement, or self-construction at any point during employment or for other uses at the time of retirement. Besides allowing withdrawals from the employee's own deposits, the HPF also provides home mortgage loans to eligible HPF depositors. HPF depositors become eligible to apply for HPF loans after 12 months of consecutive deposits to the fund. By the end of 2008, 41% of the cumulative HPF deposit was used for HPF withdrawals, and 34% of the cumulative HPF deposit was used for HPF loans (Ministry of Housing and Urban-Rural Development of China, 2006-2008).

The HPF program can help promote housing consumption through both withdrawals of deposits for home down payments and HPF home loans. Given the high demand for homeownership in China, housing price is high relative to the typical Chinese household income (Wei et al. [2012]). Most households require financing for home purchases. Burrell [2006] estimated the expense to own a typical housing unit for a household with three members in 2002. For a condominium with 70-80 square meters of living space, the average sales price was 3000 yuan per square meter. This results in a housing value to household income ratio of 13.4 for typical urban households with two manufacturing employees. Given the high cost-to-income ratio, the down payment can be a barrier to homeownership (Engelhardt [1994], Ortalo-Magne and Rady [2006], Guiso and Jappelli [2002]). If a household saves 30% of its annual household income for a 20% down payment, it will take 9 years to accumulate the down payment. Making a withdrawal from the mandatory HPF deposits can help the employee pay the down payment. Although making a withdrawal from the HPF may be postponed until after retirement, it is not worthwhile to wait because the interest rate received on HPF deposits is low. For instance, in 2012, the HPF deposit interest rate was 2.8%, close to the short-term interest rate of 2.85% for a 3-month deposit, but much lower than 5.3% for the 5-year deposit interest rate (Source: People's Bank of China).

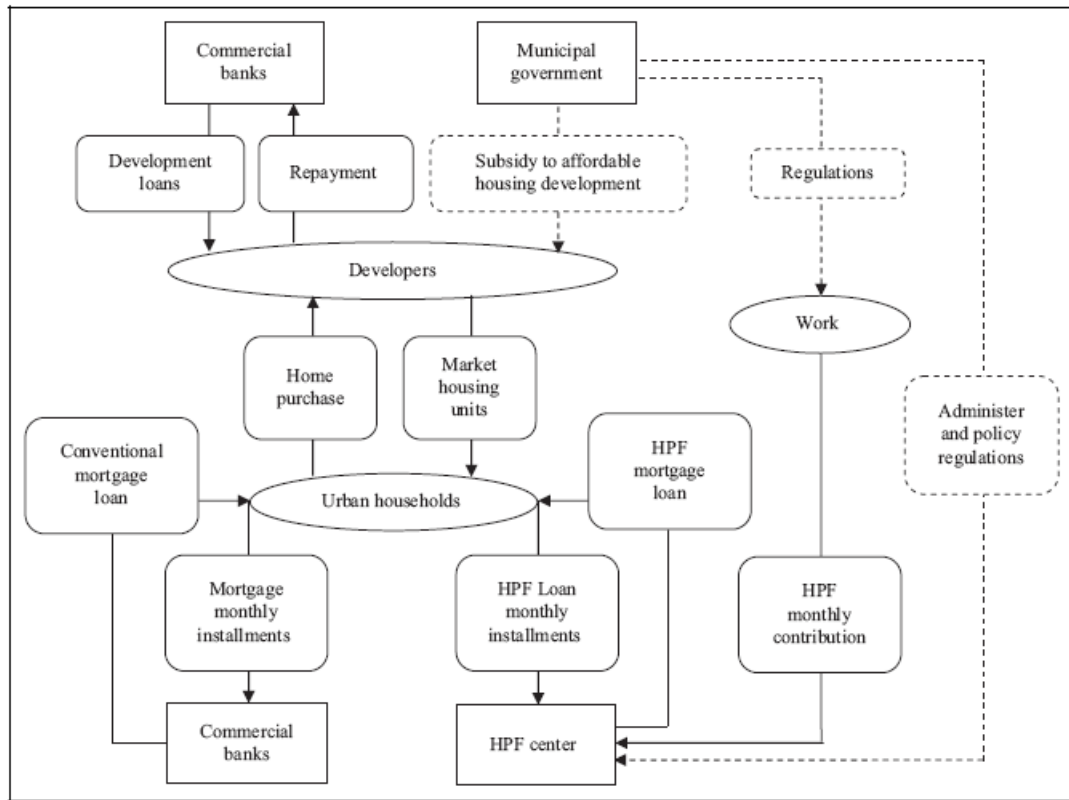
After saving enough money for a down payment, households can finance the remainder of the home's purchase price with HPF loans, commercial loans, or a combination of the two, i.e., portfolio loans.<sup>3</sup> A HPF loan is the top mortgage option for eligible households (Yeung and Howes [2006]). Like other business mortgages and commercial home mortgages in China, the interest rate of HPF loans is uniformly set by People's Bank of China, the central bank, and does not depend on the

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<sup>3</sup>See Figure 2.1 for a snapshot of the Chinese Housing Finance System, Source: Huang and Clark [2002].

creditworthiness of the borrower or the local social and economic conditions. <sup>4</sup> Table 2.1 reports the historical interest rates for HPF loans and commercial loans. It is obvious that HPF loans have lower rates than commercial ones. In addition, HPF loans only require a 20% down payment, while commercial loans typically require at least 30%. As a result, eligible employees will first borrow from the HPF, only applying for a commercial loan to cover any short fall resulting from the required loan amount being greater than the allowable HPF loan amount. The allowable HPF loan amount depends on the employee's previous HPF deposits, the value of property, the household income, and the HPF loan limit set by local HPF Management Committees. <sup>5</sup>

Figure 2.1: Housing Financing in China



Source: [Deng et al. \[2011\]](#) [Deng and Fei \[2008\]](#)

<sup>4</sup>Since 2005, commercial banks have been allowed some flexibility in setting interest rates in the range of 90%-100% of the baseline level set by the central bank ([Deng et al. \[2011\]](#)), but the variation is relatively small.

<sup>5</sup>While established as a national program, the operation of the HPF is decentralized to local HPF Management Committees in each province that set the local deposit ratios and loan limits. As a result, there exists substantial variations in the implementation of the HPF program, which can be further explored in future research.

Table 2.1: Interest Rates Set by the Central Bank

	HPF rates		commercial rates				
	<5 yrs.	>5 yrs.	<6 mos.	6-12 mos.	1-3 yrs.	3-5 yrs.	>5 yrs.
1991.04.21			8.10	8.64	9.00	9.54	9.72
1993.05.15			8.82	9.36	10.80	12.06	12.24
1993.07.11			9.00	10.98	12.24	13.86	14.04
1995.01.01			9.00	10.98	12.96	14.58	14.76
1995.07.01			10.08	12.06	13.50	15.12	15.30
1996.05.01			9.72	10.98	13.14	14.94	15.12
1996.08.23			9.18	10.08	10.98	11.70	12.42
1997.01.00	7.65	8.10					
1997.10.23			7.65	8.64	9.36	9.90	10.53
1998.03.25			7.02	7.92	9.00	9.72	10.35
1998.07.01			6.57	6.93	7.11	7.65	8.01
1998.12.07			6.12	6.39	6.66	7.20	7.56
1999.06.10			5.58	5.85	5.94	6.03	6.21
1999.09.21	4.14	4.59					
2002.02.21	3.60	4.05	5.04	5.31	5.49	5.58	5.76
2004.10.29	3.78	4.23	5.22	5.58	5.76	5.85	6.12
2005.03.17	3.96	4.41					
2006.04.28	4.10	4.59	5.40	5.85	6.03	6.12	6.39
2006.08.19			5.58	6.12	6.30	6.48	6.84
2007.03.18	4.32	4.77	5.67	6.39	6.57	6.75	7.11
2007.05.19	4.41	4.86	5.85	6.57	6.75	6.93	7.20
2007.07.21	4.50	4.95	6.03	6.84	7.02	7.20	7.38
2007.08.22	4.59	5.04	6.21	7.02	7.20	7.38	7.56
2007.09.15	4.77	5.22	6.48	7.29	7.47	7.65	7.83
2007.12.21			6.57	7.47	7.56	7.74	7.83

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Table 2.1 – Continued

	HPF rates		commercial rates				
	<5 yrs.	>5 yrs.	<6 mos.	6-12 mos.	1-3 yrs.	3-5 yrs.	>5 yrs.
2008.09.16	4.59	5.13	6.21	7.20	7.29	7.56	7.74
2008.10.08	4.32	4.86					
2008.10.09			6.12	6.93	7.02	7.29	7.47
2008.10.22	4.05	4.59					
2008.10.30			6.03	6.66	6.75	7.02	7.20
2008.11.27	3.51	4.05	5.04	5.58	5.67	5.94	6.12
2008.12.23	3.33	3.87	4.86	5.31	5.40	5.76	5.94
2010.10.20			5.10	5.56	5.60	5.96	6.14
2011.02.09			5.60	6.06	6.10	6.45	6.60
2011.04.05			5.85	6.31	6.40	6.65	6.80
2011.07.07			6.10	6.56	6.65	6.90	7.05

Notes: “Zhongguo Renmin Yinhang Guanyu Shangtiao Jinrong Jigou Renminbi Cundaikuan Jizhun Lilv de Tongzhi (A Notification about Adjusting the Renminbi Interest Rates for Financial Institutes)”, in listed years. Source: People’ Bank of China.

Before 1998, only the four largest state-owned commercial banks, including the Industrial and Commercial Bank of China (ICBC), the Agricultural Bank of China (ABC), the Bank of China (BoC), and China Construction Bank (CCB), could make HPF loans. These loans were exclusively available to work units for housing construction. After 1998, HPF loans were reoriented toward employees for individual loans to finance housing purchases (Burell [2006]). In this paper, the HPF loan benefit is calculated for eligible households only after 1998. In addition, I also define the length of enrollment in the HPF as the years since the earliest enrollment. This can affect home purchases in two ways. First, the longer the enrollment, the more deposits that were made, and the more funds available for withdrawal for the down payment. Employees are typically allowed to withdraw up to 90 % of accumulated deposits. Second, the longer enrollment and greater amount of deposits

accumulated permits a larger HPF loan amount. Most local HPF Management Committees allow HPF loans of as much as 10 times the amount of accumulated deposits.

The Chinese central government has allowed more types of enterprises into the HPF program through waves of policy changes between 1994 and 2002.<sup>6</sup> The state sector, including party organizations, administration, and public institutions, and the state-owned industrial enterprises were the first to be covered by the HPF program since 1994. The collective sector, including enterprises owned by the township, county, city, or province, and the new private business sector have been included in the HPF program since 1999. The past decade has witnessed substantial growth in HPF deposits and wider use of HPF loans. By the end of 2008, 77.5 million employees had participated in the the HPF program (Ministry of Housing and Urban-Rural Development of China, 2006-2008). Given that borrowers of HPF loans must have stable employment and previous deposits into the fund, the credit risk is relatively low. The nationwide delinquency rate of HPF loans was 0.07% in 2006, 0.06% in 2007, and 0.04% in 2007 (Ministry of Housing and Urban-Rural Development of China, 2006-2008).

The establishment of the HPF program in China was part of the large-scale housing reform officially started in 1994 with earlier small-scale experiments in late 1980s. Public housing used to be part of the welfare package offered by work units, especially those in the state sector, allowing employees to live in public housing for low rent or even free of charge. The official transition from welfare distribution to housing benefits started in 1994 with mandatory rent increases and the privatization of public housing. When an employee purchased housing from the work units at a subsidized price, the household was entitled only to partial ownership, i.e., the use rights to use and inherit, but not the rights to sell (Wang and Murie [2000]). A dual housing system was also established to provide affordable housing for low-income households and commercial housing to moderate- to high-income households. Welfare distribution of housing was officially prohibited in 1998. At this time instead, the work units could provide a lump sum housing subsidy at the time of housing purchase. Housing reform granted the employees in the state sector some housing endowment (Wang [forthcoming], Wang [forthcoming]), but had limited impact on traditional homeowners and those out of the state sector (Wang and Murie [2000]).

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<sup>6</sup>See Table 2.2 for the progressive coverage of the HPF program by types of enterprises.



Table 2.2: The Progressive Implementation of Chinese HPF

	1994	1998	1999	2002
Types of enterprises	SC(1994)No.43	SC(1998)No.23	SC(1999)No.262	SC(2002)No.350
State sector	Y	Y	Y	Y
Collective sector	N	N	Y*	Y*
Private sector	N	N	Y**	Y**
Self-employed	N	N	N	N
Minimum contribution Ratio	5%	5%	5%	5%
Prevailing contribution ratio			8%	10-12%
fund use	housing construction	home purchase	home purchase	home purchase
Lending institutes	only four	all commercial banks	all commercial banks	all commercial banks
Housing reform	sales of public housing	banning welfare housing	private housing	private housing
	rent reform	housing subsidies		
	affordable housing	affordable housing		

Notes: Y–Applicable, Y\*–Applicable, but depends on enterprises’ performance, Y\*\*–Applicable, higher ratio for some, N–Not applicable. SC(1994)No.43: “Guowuyuan guanyu shenhua chengzhen zuhfang zhidu gaige de jueding(A decision from the State Council on deepening the urban housing reform)”, effective on July 18, 1994; SC(1998)No.23: “Guowuyuan guanyu jingyibu shenhua chengzhen zhufang zhidu gaige jiakuai zhufang jianshe de tongzhi(A notification from the State Council on further deepening the reform of urban housing system and accelerating housing construction )”, effective on July 3, 1998; SC(1999)No.262: “Zhufang gongjijing guanli tiaoli (A regulation on the management of housing provident fund )”, effective on April 3, 1999; SC(2002)No.350: “Guowuyuan Guanyu Jinyibu Jiaqiang Zhufang Gongjijing Guanli de Tongzhi (Notice on Strengthening the Administration of the Housing Provident Fund)”. Source: State Council, People’s Republic of China.

## 2.3 DATA AND EMPIRICAL DESIGN

Household survey data from the China Health and Nutrition Survey (CHNS) is used in this study. CHNS followed a multistage, random cluster process to draw sample households from the nine provinces: <sup>7</sup> Liaoning <sup>8</sup>, Jiangsu, Shandong, Henan, Hubei, Hunan, Guangxi, Guizhou, and Heilongjiang<sup>9</sup>. Each observation in my sample is an urban household surveyed in least two out of the seven waves of CHNS in 1989, 1991, 1993, 1997, 2001, 2004, 2006, with at least one survey before and one survey after 1998. The baseline sample consists of surveyed urban households whose head and spouse were between 18-43 years old in 1989. <sup>10</sup>

I defined treatment status by the employment status in the last period observed before 1998. The treatment group consists of households with both the head and the spouse working in the sectors eventually covered by the HPF program, i.e., in the state, private, and the collective sectors. <sup>11</sup> The control group consists of households with only one person in the couple working in a HPF-covered sector. To make the treatment group and the control group more comparable, I also removed any households where either person in the couple was self-employed because their income source is quite different from the rest of the sample. Including those households does not affect the results. <sup>12</sup>

The longitudinal household data set includes variables such as household income, household assets, household composition, household members' dates of birth, employment type, tenure type, value of home measured by private rent value, etc. Table 2.3 reports the summary statistics. There were 1,503 household observations in my sample, with 491 observations in the treatment group and 316 observations in the control group prior to 1998, and 419 observations in the treatment group and 277 observations in the control group after 1998 when HPF loans became available to employees. The treatment and control groups are similar in observable characteristics. Moreover, the DID estimates suggest that the observable characteristics of the two groups does not diverge over time. In addition, I also examined the consumption of motorcycles, automobiles, washing machines, and cameras to test whether household income contributes to more durable consumption.

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<sup>7</sup>See Figure 2.2 for a map of provinces in which the survey has been conducted.

<sup>8</sup>Liaoning was missing in 1997 and is therefore not included in my sample.

<sup>9</sup>Heilongjiang first joined in 1997 and is therefore not included in my sample.

<sup>10</sup>By the end of my sample, the cohort is between 35-60 years old. Most men in China retire at 60 years old, while most women retire at 55 years old.

<sup>11</sup>I define the collective sectors as township enterprise or county, city, or provincially-owned enterprise, the self-employed as the owner or manager of entities with employees.

<sup>12</sup>The estimation results for the sample including those households are available upon request.

Figure 2.2: Map of CHNS Regions



Source: China Health and Nutrition Survey  
(<http://www.cpc.unc.edu/projects/china>).

Table 2.3: Summary of Statistics

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Before 1998				After 1998				DID	se
	control		treat		control		treat			
	mean	sd	mean	sd	mean	sd	mean	sd		
<i>Housing Consumption</i>										
ownership(full)	0.60	0.49	0.31	0.46	0.74	0.44	0.66	0.47	0.20***	0.05
ownership (full and partial)	0.65	0.48	0.40	0.49	0.85	0.36	0.80	0.40	0.20***	0.05
renter	0.32	0.47	0.58	0.49	0.13	0.34	0.19	0.39	-0.20***	0.05
<i>Demographics</i>										
household size	3.63	0.80	3.35	0.67	3.35	0.95	3.04	0.76	-0.03	0.08
household income(1000 yuan)	17.19	11.61	15.94	7.87	34.93	31.82	33.29	28.91	-0.39	2.29
head's age	38.99	8.16	37.70	7.22	47.95	8.20	47.42	7.65	0.76	0.82
spouse's age	37.16	7.63	35.94	6.39	46.10	7.72	45.68	7.32	0.81	0.76

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Table 2.3 – Continued

VARIABLES	Before 1998				After 1998				DID	se
	control		treat		control		treat			
	mean	sd	mean	sd	mean	sd	mean	sd		
<i>Household Assets</i>										
motor	0.08	0.27	0.10	0.30	0.28	0.45	0.24	0.43	-0.06	0.04
car	0.01	0.10	0.00	0.06	0.05	0.21	0.03	0.17	-0.01	0.02
washer	0.79	0.40	0.89	0.32	0.88	0.32	0.93	0.26	-0.05	0.03
camera	0.19	0.40	0.32	0.47	0.35	0.48	0.45	0.50	-0.03	0.05
# obs.	316		491		277		419		1,503	

Notes: The sample consists of urban households whose head and spouse were between 18-43 years old in 1989 and were surveyed in least two out of the seven waves of CHNS in 1989, 1991, 1993, 1997, 2001, 2004, 2006, with at least one survey before 1998. The treatment group included households with both head and spouse working in the sectors covered by the HPF. The control group included households with only one of the couple working in HPF-covered sectors.

I use the following OLS probability regression with a DID specification to estimate the treatment effect of HPF loan benefit.

$$Outcome_{it} = \alpha_0 DoubleAccess_{it} + X_{it}\beta + \theta_i + \mu_t + \Phi(t) + \epsilon_{it} \quad (2.1)$$

$Outcome_{it}$  is the outcome variable for the household  $i$  in year  $t$ . The two outcome variables of interest are the homeownership and renter rate.  $DoubleAccess_{it}$  is the time-varying HPF loan benefit for household  $i$ ; it equals one only for households in the treatment group after 1998.<sup>13</sup>  $X_{it}$  is a vector of control variables.  $\theta_i$ ,  $\mu_t$  are household fixed effects and year fixed effects, respectively. The household fixed effects control for any time invariant factors that affect home purchase decisions; year fixed effects control for common macroeconomic shocks in each survey year.  $\Phi(t)$  is a quadratic function of the number of years since 1989, which controls for the common trend for economic development and the growing demand for housing in China since the 'open door' policy of 1979.

To account for the impact of previous deposits into the HPF by employees, I also run the following specification:

$$Outcome_{it} = \alpha_1 DoubleAccess_{it} + \alpha_2 Length_{it} + X_{it}\beta + \theta_i + \mu_t + \Phi(t) + \epsilon_{it} \quad (2.2)$$

$Length_{it}$  is the number of years since the earliest inclusion of household  $i$  in the HPF program; it equals zero for all household observations before 1994. For the households whose HPF enrollment results from state employment,  $Length_{it} = current\ year - 1994$ ; for the households whose HPF enrollment results from employment in collective and private sectors,  $Length_{it} = current\ year - 1999$ . The parameters of interest are  $\alpha_0$  in Equation 2.1,  $\alpha_1$  and  $\alpha_2$  in Equation 2.2.  $\alpha_0$  measures the overall effect of HPF loan benefit for households with double enrollment.  $\alpha_1$  measures the effect of HPF loan benefit, in addition to the effect of an additional year of deposit to the HPF program, measured by  $\alpha_2$ . As discussed in Section 2.2, the length of enrollment affects the amount of accumulated deposits and therefore the amount available for withdrawal for the down payment on the housing purchase and the maximum size of any HPF loans.

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<sup>13</sup>Although the HPF program started in 1994, it provided home mortgage loans to employees only after 1998; before 1998, it provided loans to work units for housing construction.

## 2.4 HPF ELIGIBILITY AND HOUSING CONSUMPTION

In this section, I examine whether the HPF loan benefit has differential impact on housing consumption across the HPF enrollment since 1998. In the top panel of Figure 2.3, the solid lines plot the time trends of full homeownership for the treatment group and the control group in the sample period. The two vertical lines mark the time of housing reform in 1994 and the time when HPF loans became available in 1998. Homeownership for the control group was slightly above 60 percent before 1998 and increased to 70-80 percent after 1998. However, homeownership for the treatment group was only between 20 percent and 40 percent before 1998. After HPF loans became available in 1998, homeownership of the treatment group accelerated to nearly 80 percent in 2006. These time trends provide visual evidence that HPF loan benefit has differential impact on homeownership.

I first estimated the treatment effect of access to HPF loans on housing consumption, and then I controlled for the years since first enrollment in the HPF program. As explained previously, the length of enrollment affects the amount of deposits accumulated in the HPF and the maximum loan amount available to the employee. Table 2.4 reports the estimation results. The outcome variables are homeownership in Panel A and renter rate in Panel B. Columns 1-2 are the baseline cases with individual household fixed effects and time fixed effects. Columns 3-4 control for household demographics such as the head's and the spouse's age, household size, and annual household income. Columns 5-6 further control for the national time trend of housing consumption since 1989.

The estimates in Column 1 show that the HPF loan benefit increases homeownership by 19 percent more for households with double enrollment than those with single enrollment, and reduces the their probability of renting by 18 percent. The fixed effect estimates are similar to the difference-in-difference estimates without FEs in Column 9 of Table 2.3. The estimates for the DID specifications with FEs are robust. Adding household demographics in Column 3 and both household demographics and the time trend in Column 5 does not change the estimates.

When I controlled for the length of enrollment in Table 4 (Column 2, Column 4, and Column 6), one additional year of HPF deposit increased homeownership by 4-5 percent for both the treatment and the control group. In addition, households with double enrollment enjoyed an additional 15 percent higher homeownership. Renter rate decreased by 7 percent for each additional year of HPF deposit, and the households with double enrollment were 12-13 percent less likely to rent homes. Once again the DID specifications with FE are robust to additional control variables and the national time trend.

Figure 2.3: Time Trends of Homeownership and Household Assets

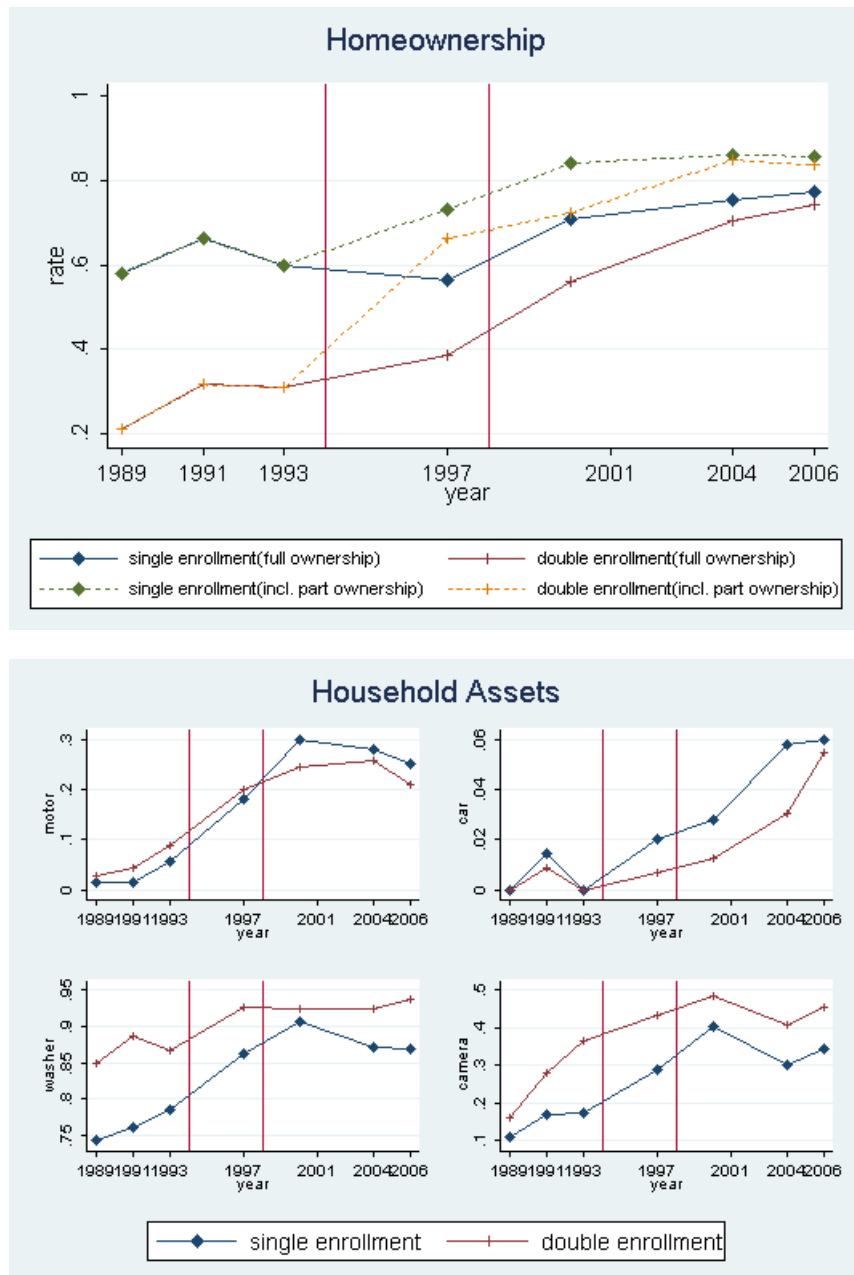




Table 2.4: HPF Eligibility and Housing Consumption

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: homeownership</i>						
Double Access	0.19*** (0.05)	0.15*** (0.05)	0.18*** (0.05)	0.15*** (0.04)	0.18*** (0.05)	0.15*** (0.04)
length		0.05*** (0.01)		0.04*** (0.01)		0.04*** (0.01)
Observations	1,503	1,503	1,479	1,479	1,479	1,479
Number of hhid	284	284	281	281	281	281
<i>Panel B: Renter Rate</i>						
Double Access	-0.18*** (0.05)	-0.13** (0.05)	-0.17*** (0.05)	-0.12** (0.05)	-0.17*** (0.05)	-0.12** (0.05)
length		-0.07*** (0.01)		-0.07*** (0.01)		-0.07*** (0.01)
Observations	1,503	1,503	1,479	1,479	1,479	1,479
Number of hhid	284	284	281	281	281	281
Household FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Control	NO	NO	YES	YES	YES	YES
Time trend	NO	NO	NO	NO	YES	YES

Notes: *DoubleAccess* measures the effect of additional HPF loan benefit to households with double enrollment; it equals one for household observations in the treatment group after 1998, zero otherwise. *Length* is the number of years since the earliest enrollment in the HPF program. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1, standard errors are clustered at the household level.

## 2.5 HPF ELIGIBILITY AND HOUSEHOLD ASSETS

The DID identification strategy assumes that the progressive coverage of HPF is not endogenous to factors that can otherwise affect the households' home purchase decisions, such as household income differential, and different household composition across HPF enrollment, and employment sector. To provide evidence against the endogeneity hypothesis, I ran falsifications to examine whether the HPF loan benefit since 1998 for households with double enrollment affected the households' assets other than home. If there is a treatment effect, it may be possible that the effects I found in the previous section only picked up the structural break in household income across sectors or other unobservable differences that would increase homeownership for the treatment group more than the control group.

The bottom panel of Figure 2.3 plots the time trends of asset ownership for the treatment and control groups. Overall, the households in both groups owned more household assets over time as the economy improved rapidly in China. However, the two groups differed in their preference for particular household assets. The treatment group, i.e., those that have both head and spouse working in the state, private, and collective sectors (double enrollment), was less likely to own automobiles, but more likely to own washing machines and cameras. The two groups showed a similar preference toward motorcycles, which is the common transportation mode in most Chinese cities. Over time, asset ownership for each group followed each other closely, suggesting no evidence of divergence in the asset ownership in the period after 1998.

To examine statistically the change in the asset ownership for the two groups, I first estimated the treatment effect of access to HPF loans on household assets, and then I controlled for the years since first inclusion in the HPF program. Table 2.5 reports the estimation results. The outcome variables are the ownership of motorcycles (Column 1-2), automobiles (Column 3-4), washing machines (Column 5-6), and cameras (Column 7-8). All specifications include individual household fixed effects, time fixed effects, household demographics, and the national time trend. Although some of the estimates are not precisely estimated, overall they suggest that after HPF loans became available in 1998, households with double enrollment are less likely to own other household assets than households with single enrollment.

Table 2.5: HPF Eligibility and Household Assets

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	motorcycle		automobile		washing machine		camera	
Double Access	-0.09**	-0.07*	-0.01	-0.01	-0.02	-0.01	-0.04	-0.03
	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.05)	(0.05)	(0.05)
length		-0.02*		-0.01**		-0.01		-0.01
		(0.01)		(0.01)		(0.01)		(0.02)
Observations	1,464	1,464	1,464	1,464	1,479	1,479	1,478	1,478
Number of hhid	281	281	281	281	281	281	281	281
Household FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control	YES	YES	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES	YES	YES

Notes: *DoubleAccess* measures the effect of additional HPF loan benefit to households with double enrollment; it equals one for household observations in the treatment group after 1998, zero otherwise. *Length* is the number of years since the earliest enrollment in the HPF program. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ , standard errors are clustered at the household level.

## 2.6 DIFFERENTIAL EFFECTS BY HOUSING DEMAND

Unlike the Chinese Household Income Project Survey (Chen [2012]), CHNS does not include survey questions about the households' amount of HPF deposits or their actual use of HPF loans. The HPF loan benefit is proxied by the employment types of household members observed in CHNS. Thus, this paper can only estimate the impact of HPF loan benefit rather than the actual use of HPF loans. To confine attention to households that are more likely to use HPF loans, I further restricted the DID analysis to the subgroups of households with higher demand for housing. These are households with more than three members, those with more than one child, those with a son as the first child, and those with a child of marriage age during the treatment period.

Table 2.6 reports the results. Column 1 shows that for the 555 households with more than three household members, the HPF loan benefit for households with double enrollment increased homeownership by 20 percent and decreased the renter rate by 21 percent. After controlling for HPF enrollment history in Column 2, the difference in homeownership was 17 percent, and each additional year of HPF enrollment increased homeownership by 3 percent for all HPF-eligible households. For households with more than one child (Column 3-4), HPF loan benefit increased homeownership by 25 percent and decreased the renter rate by 34 percent. For households whose first child is a son (Column 5-6), the HPF loan benefit increased homeownership by 15 percent with a 14 percent decrease in the renter rate. For households with a child of marriage age in the treatment period (Column 7-8), HPF loan benefit increased homeownership by 22 percent and decreased the renter rate by 16 percent. An additional year of HPF enrollment increased homeownership by 6 percent, or reduced the renter rate by 8 percent. These results suggest that the treatment effects are likely to be the result of accessed to HPF loans.

Table 2.6: Differential Effects by Housing Demand

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	household size>3		children>1		first son		marriage children	
<i>Panel A: Homeownership</i>								
Double Access	0.20**	0.17**	0.25***	0.22***	0.15**	0.11*	0.22**	0.17*
	(0.08)	(0.07)	(0.08)	(0.08)	(0.06)	(0.06)	(0.10)	(0.09)
length		0.03*		0.04**		0.04***		0.06***
		(0.02)		(0.02)		(0.02)		(0.02)
Observations	555	555	482	482	707	707	388	388
Number of hhid	98	98	86	86	136	136	70	70
Household FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control	YES	YES	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES	YES	YES

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Table 2.6 – Continued

VARIABLES	household size>3		children>1		first son		marriage children	
<i>Panel B: Renter Rate</i>								
Double Access	-0.21**	0.17**	-0.34***	-0.28***	-0.14*	-0.07	-0.16*	-0.11
	(0.08)	(0.07)	(0.08)	(0.08)	(0.07)	(0.07)	(0.10)	(0.09)
length		0.03*		-0.08***		-0.08***		-0.08***
		(0.02)		(0.02)		(0.02)		(0.02)
Observations	555	555	482	482	707	707	388	388
Number of hhid	98	98	86	86	136	136	70	70
Household FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control	YES	YES	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES	YES	YES

Notes: *DoubleAccess* equals one for household observations in the treatment group after 1998, zero otherwise. *Length* is the years since the earliest inclusion in the HPF program. Column 1-2 is the subsample of households with more than 3 household members. Column 3-4 is those with more than one child. Column 5-6 is those with a son as the first child. Column 7-8 is those with children aged above 20 in 1998. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1, standard errors are clustered at the household level.

## 2.7 HPF VS. HOUSING REFORM

In the previous sections, I examined the effect of HPF loan benefit on full homeownership of new purchases on the market. In this section, I also include partial ownership shared with the work unit to differentiate the effect of the HPF program from the effect of housing privatization. As explained previously, the introduction of the HPF program in China coincided with housing privatization that unbundled welfare housing from state sector employment. This housing reform allowed households that used to live in state-owned housing to privatize and purchase their home property. Wang [2011] suggests that the housing reform allowed households that used to live in free public housing to consume more housing; however, Iyer et al. [2009] show that in a context where tenancy is secure and credit channels do not operate, property rights do not have obvious benefits in terms of income, consumption, or the quality of housing.

Even though housing privatization in 1994 may have caused divergence in wealth between state employees and non-state employees, the housing reform can be interpreted as a one-time wealth shock. The effect of HPF loan benefit, however, took off later and gradually. The dotted lines in top panel of Figure 2.3 plot the extensive ownership including both full homeownership and the partial homeownership; the solid line plots full homeownership only. After housing privatization in 1994, partial homeownership increased immediately by a substantial amount while full homeownership changed very little. This suggests that housing privatization gave a one-shot boost to partial homeownership, which may have caused upward bias of the treatment effect if partial homeownership is included in the estimation.

To distinguish the impact of access to HPF loan from housing reform, I first reviewed the differential in extensive homeownership across HPF enrollment before and after HPF loans became available in 1998, and then examined the differential before and after housing privatization in 1994. Table 2.7 reports the estimation results. When both full and partial homeownership is accounted for, the HPF loan benefit starting in 1998 increased homeownership by 16-18 percent, which is similar to the impact on the full homeownership only. On the other hand, the entire housing reform of 1994, including both the HPF program and the privatization of public housing, increased homeownership by 17-23 percent. The DID specifications with fixed effects are robust to the inclusion of household demographics and national time trend for both outcomes. The estimation results suggest that housing reform did promote homeownership and added to the treatment effect of access to HPF loans.

Table 2.7: HPF vs. Housing Reform

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Homeownership Including Partial Ownership						
Double Access	0.18***	0.12**	0.16***	0.11**	0.16***	0.11**
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
length		0.08***		0.07***		0.07***
		(0.01)		(0.01)		(0.01)
Observations	1,503	1,503	1,479	1,479	1,479	1,479
Number of hhid	284	284	281	281	281	281
Household FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Control	NO	NO	YES	YES	YES	YES
Time trend	NO	NO	NO	NO	YES	YES

Continued on Next Page...



Table 2.7 – Continued

	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: Homeownership Including Partial Ownership						
Reform	0.25***	0.18***	0.23***	0.17***	0.23***	0.17***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
length		0.07***		0.07***		0.07***
		(0.01)		(0.01)		(0.01)
Observations	1,503	1,503	1,479	1,479	1,479	1,479
Number of hhid	284	284	281	281	281	281
Household FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Control	NO	NO	YES	YES	YES	YES
Time trend	NO	NO	NO	NO	YES	YES

Notes: The dependent variable in this table is homeownership including both full ownership from new purchases in the housing market and partial ownership shared with the work unit as a result of the housing reform between 1994 and 1998. In panel A, *DoubleAccess* is a dummy that equals one for the treatment group after 1998. It measures the effect of HPF loan benefit for double enrollment. In panel B, *Reform* is a dummy that equals one for households in the treatment group after 1994. *Reform* measures the effect of the change in homeownership as a result of both housing reform and the availability of HPF loans. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ , standard errors are clustered at the household level.

## 2.8 CONCLUSION

In this paper I examined whether HPF programs helped promote housing consumption. To overcome the biases due to sorting and selection, I studied the natural experiment in China where employees in different sectors were gradually included in the mandatory HPF.

Results indicate that for households with double enrollment in the program having access to HPF loans increased homeownership by 18 percent. When the length of HPF enrollment was accounted for, double enrollment in HPF increased homeownership by 15 percent, with one additional year of HPF deposits adding 4 percent of homeownership to households with both single enrollment and double enrollment. Similarly, double enrollment reduces the renter rate by 17 percent. The estimates for the difference-in-difference specifications with fixed effects are robust to the inclusion of household demographics and the national time trend. Treatment effects are similar for households with higher demand for housing, i.e., the ones more likely to actually use HPF loans. This suggests that the difference in housing consumption across the treatment and the control groups is likely to stem from the HPF loans borrowed by eligible households.

Finally, I evaluated the impact of the entire housing reform from 1994-1998 by including partial homeownership resulting from housing privatization in the state sector starting in 1994. I found evidence that this housing privatization boosted homeownership immediately after the reform in 1994. The entire housing reform induced a greater positive impact on housing consumption, likely through housing privatization.

The findings in this paper suggest that HPF programs can be an effective way for the government to promote housing consumption. As an alternative to credit expansion of commercial loans resulting in loose lending standards, HPF loans are employment-based and depend on the borrowers' previous deposits, and therefore have lower credit risk.

## 3.0 NONLOCAL MORTGAGE LENDING AND THE SECONDARY MARKET INVOLVEMENT

### 3.1 INTRODUCTION

Remote lending has become an important channel in the application and origination of home mortgages. Out-of-state banks benefit more from remote lending than their in-state counterparts because branching outside the charter state is costly due to restrictions on interstate banking. This paper aims to investigate the impact of remote lending and interstate banking on the mortgage market, using mortgage data from the periods before and after the subprime crisis.

We classify mortgages by whether the lending bank is chartered in the state of the borrower and whether the bank has branch offices in the Metropolitan Statistical Area of the borrower. This generates four types of mortgages: in-state local, in-state nonlocal, out-of-state local, and out-of-state nonlocal mortgages. Using a sample of mortgages from state-chartered banks, collected under the Home Mortgage Disclosure Act from 2005 to 2008, we find two salient features of out-of-state nonlocal mortgages. First, they are more likely to be subprime loans. We define subprime or high-priced loans as those with annual percentage rate equal or greater than 3 percentage points for the first lien loans comparing to the applicable treasury yield. In 2005, 67 percent of out-of-state nonlocal mortgages are high-priced, however, for other mortgages, only 12 to 19 percent. Second, out-of-state nonlocal mortgages are more likely to be sold to the secondary market. 80 percent of those loans are sold to the secondary market, but only 43 to 48 percent for others.

Understanding the substantially high subprime rate and sales rate of out-of-state nonlocal mortgages is very important. As [Coulton et al. \[2008a\]](#) show, loans made by nonlocal banks get foreclosed earlier than those made by the local banks in Cuyahoga County in Ohio. Also, subprime mortgages end up with foreclosure almost more than six times as often as prime mortgages [Gerardi et al. \[2007\]](#). As the secondary market grows, it may take up too many loans of low quality [Gabriel](#)

and Rosenthal [2007], presumably because banks can sell those loans, especially subprime loans, to other investors through the secondary market.

We first investigate borrowers' characteristics of different mortgages and find that borrowers with less credit worthiness, such as without co-applicant and higher loan-to-income ratio, are more likely to apply for and obtain out-of-state nonlocal mortgages. Using logit model, we show that out-of-state nonlocal mortgages are more likely to be subprime and to be sold on the secondary market than others, even after controlling for the observed characteristics of borrowers and mortgages.

Then, we construct panel data sets of home mortgages at the community and bank level and provide empirical evidences from both the demand side and the supply side to explain the high subprime and sales rates of out-of-state nonlocal mortgages.

On the demand side, we show that the demand for nonlocal mortgages, measured by the share of nonlocal applications, is greater in the neighborhoods where local banks have higher lending standards, measured by the denial rate in the previous year<sup>1</sup>. Our explanation is that local banks select better qualified borrowers by taking advantage of their information about local communities and their easier access to borrowers' banking accounts (Mester et al. [2007]) and financial background, so less creditworthy borrowers tend to end up with nonlocal mortgages.

On the supply side, there are evidences that the secondary market provides the incentive for banks to originate the risky out-of-state nonlocal mortgages. Using panel data at bank level, we show that banks that are more active on the secondary market originate a higher proportion of out-of-state nonlocal mortgages. We also find that banks with smaller assets and larger amount of total applications, given everything else equal, have higher shares of out-of-state nonlocal mortgages. This suggests that, for smaller banks, the secondary market might play even larger role in their origination of out-of-state nonlocal mortgages.

### 3.2 LITERATURE REVIEW

Our paper shows that home mortgages made by lenders without branch offices in the local market are of lower credit quality. This finding adds to the studies of lending channels that have been well studied in the context of small business lending. It is unclear in this stream of literature whether

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<sup>1</sup>Using the same measure, Dell'Ariccia et al. [2009] investigate the relation between lending standards and credit boom of mortgages.

distant lending results in bad credit. [DeYoung et al. \[2008\]](#) show that increase in the distance between small business borrowers and their lenders during the 1990s was disproportionately large for borrowers located in low-income and minority neighborhoods. However, [Petersen and Rajan \[2002\]](#) argue that the distant borrowers are not necessarily the ones with high quality credits, due to the improvement in lenders' productivity and the increasing competition in the credit market. Our paper provides evidence that borrowers of nonlocal loan are those who cannot get loans in the local market and they have relatively worse observable characteristics than the ones who borrow from local market.

Our paper enriches the studies on home mortgage lending from the perspective of lending channel. Although the bank-borrower relationship may not play a key role in home mortgage lending, the presence of branch offices in the local markets facilitates the collection of information about the local housing market and socioeconomic conditions. [Ergungor \(2010\)](#) shows that lenders are more willing to lend to a low- to moderate-income neighborhood if they have a bank branch in the neighborhood. [Loutskina and Strahan \[2009\]](#) show that banks with more geographically concentrated mortgage lending performed better during the crisis because of greater use of private information.

Our paper extends the lending channel studies to the home mortgage market by accounting for the impact of interstate banking. Distinguishing in-state banks from out-of-state banks can differentiate the costs to comply with host state laws and banks' commitment to local prosperity. Moreover, we also consider the impact of interstate banking and the incentive effect of the secondary market on nonlocal mortgage lending. This paper thus contributes to the growing literature on securitization and the secondary market, especially the incentives of banks to expand lending to risky borrowers. [Loutskina and Strahan \[2009\]](#) show that securitization in secondary market reduces the importance of the financial conditions of lenders for credit supply. Our paper also confirms this conclusion by showing that the smaller banks are more involved in the secondary market. Using the HMDA data between 1992 and 2004, [Gabriel and Rosenthal \[2007\]](#) show that the secondary market purchase helps to expand the credit supply in terms of higher origination rates, and the effect is even greater for subprime lenders. [Mian and Sufi \[2009a\]](#) argue that the sharp increase in the fraction of loans sold on the secondary market expands the home mortgage credit supply to the neighborhoods of low credit quality in terms of low denial rates. Our study on the out-of-state nonlocal mortgages compliments these previous findings related to the secondary market.

Our paper provides evidence from a panel of banks that operates between 2005 and 2008 to show that banks that have closer connection to the secondary market in the previous year will make more risky loans in areas outside their charter states where they do not have local branch offices. The result is consistent with the findings in the literature. [Keys et al. \[2010\]](#) and [Dell’Ariccia et al. \[2009\]](#) point out that securitization adversely affects the banks’ incentives to screen the loans, and therefore banks lower their lending standard when making home mortgage loans. [Purnanandam \[2011\]](#) shows that banks that are more active in secondary market in the pre-crisis period have more defaulted loans in the post-crisis period, especially for capital-constraint banks. [Rosen \[2011\]](#) finds that the securitization market facilitates the wholesale lending channel and increases the share of nonlocal loans, which tend to be riskier on average than loans made by local banks.

### 3.3 DATA

The major data resource is the Home Mortgage Disclosure Act (HMDA) data from 2005 to 2008, which accounts for approximately 80% of all home mortgages nationwide ([Bercovec and Zorn, 1996](#); [Avery and Brevoort, 2007](#)). Since our focus is the impact of interstate banking and lending channels on the home mortgage market, we use a sample of home mortgage applications and loans made by state chartered commercial banks. We then use the Institution Record and the MSA Office Information to identify the charter states of banks and the locations of their branches.

Our sample includes conventional home purchase loans for 1-family to 4-family housing units secured by first lien. Each observation is a loan application that may lead to origination if borrowers and banks reach agreements. The interest rate spreads are reported if the difference between the annual percentage rate and the applicable treasury yield is equal or greater than 3-percentage point. Only complete applications initiated by a nature person are included—loans initiated by a corporation or other entity that is not a nature person are excluded. The applications withdrawn by the applicants and the loans purchased by banks are also excluded.

Table 3.1: Application and Origination by Loan Type

	In-state banks			Out-of-state banks			All banks
	Local	Nonlocal	Sub-total	Local	Nonlocal	Sub-total	Total
Panel A: 2005-2008							
Number of applications	890,282	360,444	1,250,726	223,642	486,035	709,677	1,960,403
Share of total application	0.45	0.18	0.64	0.11	0.25	0.36	1
Share of applications become loans	0.86	0.82	0.85	0.78	0.71	0.73	0.81
Panel B: 2005							
Number of applications	266,145	105,018	371,163	63,609	169,261	232,870	604,033
Share of total application	0.44	0.17	0.61	0.11	0.28	0.39	1
Share of applications become loans	0.87	0.83	0.86	0.79	0.75	0.76	0.82
Panel C: 2006							
Number of applications	240,414	96,689	337,103	58,399	168,705	227,104	564,207
Share of total application	0.43	0.17	0.6	0.1	0.3	0.4	1
Share of applications become loans	0.85	0.82	0.84	0.78	0.69	0.71	0.79
Panel D: 2007							
Number of applications	210,167	87,183	297,350	60,795	93,614	154,409	451,759
Share of total application	0.47	0.19	0.66	0.13	0.21	0.34	1
Share of applications become loans	0.86	0.82	0.85	0.77	0.7	0.73	0.81
Panel E: 2008							
Number of applications	173,556	71,554	245,110	40,839	54,455	95,294	340,404
Share of total application	0.51	0.21	0.72	0.12	0.16	0.28	1
Share of applications become loans	0.85	0.81	0.84	0.8	0.64	0.71	0.8

### 3.3.1 Total Application and Origination Rate

Table 3.1 shows the distribution of loan applications and originations among different banks through different lending channels. The total number of applications with in-state banks is around twice of that with out-of-state banks. This indicates that there are more in-state banks in the market or the applicants prefer in-state banks to out-of-state banks when they apply for home mortgages. Among the applications with in-state banks, more than two-thirds are local applications; while for the application with out-of-state banks, only one-third are local applications. These contrasting facts reflect the less availability of branches owned by out-of-state banks than those owned by in-state banks because of the regulation barrier faced by out-of-state banks.

The majority of local applications go to in-state banks, but most of nonlocal applications go to out-of-state banks. Such distribution might be attributed to the fact that the applicants who apply with local branches prefer in-state banks to out-of-state banks, while the applicants who choose nonlocal banks are indifferent between in-state banks and out-of-state banks. Moreover, the out-of-state banks might be more specialized in conducting nonlocal loans because they are restricted in establishing branches in other states.

The origination rate, that is, the share of applications that become loans, is the highest for in-state local applications and the lowest for out-of-state nonlocal applications. This may imply that the applicants of out-of-state nonlocal loans have poorer qualities or are less creditworthy.

Out-of-state nonlocal applications are more responsive to time-varying shocks. The share of out-of-state nonlocal applications in total applications gets smaller over time, from around 30 percent in 2005 and 2006 to 16 percent in 2008; but the share of out-of-state local applications is stable and the share of in-state applications increases. The origination rate of out-of-state nonlocal applications decreased from more than 70 percent in earlier years to 64 percent in 2008, while the origination rates of other types of applications remain almost the same over years.

### 3.3.2 Features of Loans and Borrowers

Table 3.2 shows the most salient features of different types of mortgages. Compared to others, the out-of-state nonlocal mortgages have a much larger proportion of subprime loans and are sold much more of their mortgages to the secondary market. For instance, 67 percent of the out-of-state nonlocal mortgages are subprime and 80 percent of their mortgages are sold on the secondary



market in 2005. These shares are around 50 and 30 percentages higher than those of other types of mortgages.

The subprime rate of out-of-state nonlocal mortgages drops substantially over the years, while this rate remains relatively stable for the other mortgages, which narrows the gaps among different mortgages. The sales rates drop for all types of loans other than the out-of-state local loans, indicating that secondary market activities are affected dramatically by the subprime crisis. However, the sales rate of the out-of-state nonlocal mortgages is still much higher than that of other types in 2008.

The borrowers of out-of-state non-local mortgages have lower loan-to-income ratio and are less likely having co-applicant; they are more likely from minority groups, such as non-white, female, applicants from minority tracts and low-income tracts (Table 3.3). Given banks' charter (in-state or out-of-state), the average income of the borrowers who file up non-local application is lower than that of borrows who apply through local branches (Table 3.4).

### 3.3.3 Banks' Characteristics

Given the salient features of out-of-state nonlocal mortgages relative to others, it is interesting to investigate the characteristics of out-of-state nonlocal mortgage lenders. First of all, we find a particular bank, Fremont Investment & Loan <sup>2</sup>, originated a large amount of out-of-state nonlocal mortgages in 2005 and 2006; about 90 percent of their mortgages are subprime and are sold to the secondary market. We recheck the features documented in Section 3.2 without this major lender and find that the sales rate and the subprime rate of out-of-state nonlocal loans become smaller, but are still significantly higher than other types. The higher sales rate and subprime rate of out-of-state nonlocal mortgages are not driven entirely by this particular bank. In the empirical analysis, we control for the impact of this particular lender.

The degree of market concentration is higher for the out-of-state nonlocal mortgages. In 2005 and 2006, less than 10 banks account for 80 percent of the total out-of-state nonlocal mortgages. The market concentration decreases after the subprime crisis. In 2008, the top 70 banks originate 80 percent of the out-of-state nonlocal mortgages.

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<sup>2</sup>FREMONT INVESTMENT & LOAN originated 67.77% (48.83%) of the out-of-state nonlocal loans in 2005 (2006) and was closed in August 2008. According to the Federal Deposit Insurance Corporation (FDIC) press release, "On March 8, 2007, the FDIC issued a cease and desist order against Fremont Investment & Loan, Brea, California (Bank), and its parent corporations, Fremont General Corporation and Fremont General Credit Corporation."

Table 3.2: Characteristics of Originated Loans by Type

	In-state banks		Out-of-state banks	
	Local	Non-local	local	Non-local
Panel A: 2005				
Share of sold loans	0.48	0.47	0.43	0.8
Share of subprime loans	0.12	0.19	0.12	0.67
Interest rate spread	4.71	4.53	4.48	5.69
Share with a co-applicant	0.55	0.59	0.51	0.36
Loan-to-income ratio	2.07	1.88	2.14	1.87
Loan amount (thousands)	168.98	143.24	199.36	152.78
Panel B: 2006				
Share of sold loans	0.48	0.47	0.4	0.76
Share of subprime loans	0.14	0.22	0.17	0.58
Interest rate spread	4.77	4.56	4.59	6.11
Share with a co-applicant	0.54	0.57	0.47	0.38
Loan-to-income ratio	2.01	1.84	2.02	1.88
Loan amount (thousands)	168.02	143.6	191.68	165.7
Panel C: 2007				
Share of sold loans	0.48	0.47	0.46	0.68
Share of subprime loans	0.1	0.18	0.12	0.24
Interest rate spread	4.28	4.22	4.51	4.69
Share with a co-applicant	0.54	0.58	0.49	0.49
Loan-to-income ratio	2.08	1.89	2.13	2.14
Loan amount (thousands)	171.96	141.96	192.84	180.04
Panel D: 2008				
Share of sold loans	0.39	0.36	0.51	0.63
Share of subprime loans	0.13	0.21	0.1	0.14
Interest rate spread	4.54	4.54	4.19	4.11
Share with a co-applicant	0.54	0.59	0.5	0.55
Loan-to-income ratio	2.06	1.82	2.17	2.21
Loan amount (thousands)	184.4	146.45	207.95	217.31

Table 3.3: Borrower and Neighborhood Characteristics by Loan Type

	In-state banks		Out-of-state banks	
	Local	Non-local	Local	Non-local
Panel A: 2005				
Share of white borrowers	0.91	0.94	0.91	0.71
Share of male borrowers	0.76	0.79	0.76	0.67
Share of minority tract	0.31	0.3	0.34	0.54
Share of low-income tract	0.15	0.1	0.15	0.23
Panel B: 2006				
Share of white borrowers	0.91	0.94	0.9	0.75
Share of male borrowers	0.76	0.79	0.74	0.68
Share of minority tract	0.31	0.29	0.35	0.51
Share of low-income tract	0.16	0.1	0.16	0.22
Panel C: 2007				
Share of white borrowers	0.92	0.95	0.9	0.89
Share of male borrowers	0.76	0.79	0.74	0.74
Share of minority tract	0.28	0.26	0.33	0.35
Share of low-income tract	0.15	0.09	0.15	0.14
Panel D: 2008				
Share of white borrowers	0.92	0.95	0.91	0.92
Share of male borrowers	0.77	0.8	0.75	0.77
Share of minority tract	0.27	0.26	0.32	0.29
Share of low-income tract	0.15	0.09	0.14	0.11

Table 3.4: Borrowers' Income by Loan Type

	In-state banks		Out-of-state banks	
	Local	Non-local	Local	Non-local
Panel A: 2005				
Annual income (thousands)	106.75	98.49	122.96	100.2
Income to MSA median income ratio	1.79	1.96	2.14	1.7
Share of low-income borrowers	0.22	0.22	0.22	0.23
Tract to MSA median income ratio	110	107.15	114.39	102.29
Panel B: 2006				
Annual income (thousands)	110.83	102.5	121.91	111.02
Income to MSA median income ratio	1.83	2.03	2.07	1.87
Share of low-income borrowers	0.22	0.22	0.21	0.2
Tract to MSA median income ratio	110.31	106.8	113.02	103.94
Panel C: 2007				
Annual income (thousands)	113.6	103.63	123.18	115.26
Income to MSA median income ratio	1.9	2.11	2.11	2.09
Share of low-income borrowers	0.24	0.22	0.23	0.21
Tract to MSA median income ratio	110.73	106.89	113.6	110.42
Panel D: 2008				
Annual income (thousands)	129.48	111.43	137.69	141.88
Income to MSA median income ratio	2.07	2.19	2.27	2.51
Share of low-income borrowers	0.27	0.22	0.24	0.2
Tract to MSA median income ratio	111.9	107.28	117.09	115.24

The biggest out-of-state nonlocal lenders are not necessarily big banks; those banks with small assets are more specialized in nonlocal lending and sell extremely high proportions of their subprime mortgages to the secondary market. After the subprime crisis, a smaller proportion of their mortgages they sell are subprime loans.

### 3.4 SUBPRIME LOANS AND SALES ON THE SECONDARY MARKET

In this section, we estimate the impact of different factors, especially loan types, on the propensity of a mortgage being subprime and being sold on the secondary market. To study the determinants of subprime loans, we estimate the following transformed logit model:

$$Pf(H_i = 1) = \alpha_1 ON_i + \alpha_2 IN_i + \alpha_3 OL_i + \beta_1 Fremont_i + \beta_2 Border_i + X_i \gamma \quad (3.1)$$

where  $Pf(H_i = 1)$  the transformed probability function and  $H$  is a discrete variable that equals one if loan  $i$  is subprime and zero otherwise.  $ON$ ,  $IN$ , and  $OL$  are the dummies for the out-of-state nonlocal, in-state nonlocal, and out-of-state local loans. To compare the impact of the loan dummies on the subprime status, we use the in-state local loans as the base case.  $Fremont$  is the dummy for loans made by the biggest lender of out-of-state nonlocal mortgages;  $Border$  is the dummy for loans made to borrowers in MSAs that cross the state borders.  $X$  is a vector of controls that include property occupancy, presence of co-applicant, loan amount, gender and race of borrowers, year and state dummies; it also includes the median income, minority share, and population of the neighborhood.  $\alpha_1$ ,  $\alpha_2$ ,  $\alpha_3$ ,  $\beta_1$ ,  $\beta_2$ , and  $\gamma$  are the parameters.

The main finding is that nonlocal loans are more likely to be subprime relative to in-state local loans, as shown in the column 1 and column 2 of Table 3.5, where we report the marginal effects, that is, the expected marginal impact of each independent variable on the probability of becoming subprime loan. The marginal effects of nonlocal loans from both out-of-state and in-state banks are positive and significant, and such effects are robust with the inclusion of control variables.

We then investigate the determinants of the incidence of sale on the secondary market by estimating the following transformed logit model:

$$Pf(S_i = 1) = \alpha_1 ON_i + \alpha_2 IN_i + \alpha_3 OL_i + \alpha_4 ON_i H_i + \alpha_5 IN_i H_i + \alpha_6 OL_i H_i + \beta_1 Fremont_i + \beta_2 Border_i + X_i \gamma \quad (3.2)$$

Table 3.5: The Determinates of Subprime Status and the Sales Status

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Subprime status			Sales status	
Out-of-state nonlocal	0.120**	0.118**	0.226***	0.230***	0.201**
	(0.043)	(0.0413)	(0.0654)	(0.065)	(0.0689)
In-state nonlocal	0.100***	0.105***	0.00215	0.0127	0.0229*
	(0.00745)	(0.00814)	(0.0114)	(0.011)	(0.0109)
Out-of-state local	0.00986	0.00878	0.00706	-0.0135	-0.0314
	(0.0463)	(0.0435)	(0.0402)	(0.0412)	(0.0406)
Fremont	0.799***	0.788***	0.473***	0.478***	0.389***
	(0.0241)	(0.0271)	(0.0402)	(0.0419)	(0.0427)
Border MSA	0.0194	0.00855	0.0537**	0.0589***	0.0585***
	(0.0122)	(0.011)	(0.0179)	(0.017)	(0.0169)
Subprime			-0.320***	-0.358***	-0.397***
			(0.0321)	(0.0342)	(0.0246)
Fremont*subprime					0.163**
					(0.0554)
Out-of-state nonlocal*subprime					0.153*
					(0.0632)
In-state nonlocal*subprime					-0.0699**
					(0.025)
Out-of-state local*subprime					0.156
					(0.0822)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes	Yes
<i>N</i>	1,567,367	1,507,404	1,567,367	1,507,404	1,507,404

Robust standard errors in parentheses

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

$Pf(S_i = 1)$  is the transformed probability function and  $S_i$  is a discrete variable that is equal to one if the loan is sold on the secondary market and zero otherwise. Again, the impact of loan dummies is estimated relative to in-state local loans. Other explanatory variables are the same as those in the logit model studying subprime loans. We also include the interaction items between loan types and subprime status, because the subprime loans of different types might have different propensities to be sold on the secondary market.

In all the specifications, the marginal effects of the out-of-state nonlocal loans are positive and significant, indicating that those loans are more likely to be sold on the secondary market. These marginal effects of different factors on the sales status from different logit specifications are shown in column 3 to column 5 of Table 3.5.

The subprime loans are less likely to be sold to the secondary market than the prime loans, however, the subprime loans from out-of-state nonlocal banks are more likely to be sold on the secondary market, compared to those originated by other groups, as seen from the positive marginal effects of the interaction item.

### **3.5 HIGH SUBPRIME AND SALES RATES OF OUT-OF-STATE NONLOCAL MORTGAGES**

In this section, we attempt to explain why out-of-state nonlocal mortgages have relatively high subprime and sales rates. First, we use multinomial logit model to study how different characteristics of borrowers can explain the choice of different types of mortgages. We find that applicants who are female, nonwhite, from minority neighborhood, or without co-applicants are more likely to apply for and get out-of-state nonlocal mortgages. These suggest that borrowers of out-of-state nonlocal mortgages might be less credit worthy. The multinomial logit model might suffer from the unobserved factors affecting the origination of individual mortgages. Hence, we use fixed effects models to provide further evidence using community panel data.

#### **3.5.1 Higher Local Lending Standard, Higher Nonlocal Credit Demand**

Given that local banks are able to screen the relatively better qualified borrowers by taking advantage of their knowledge of local communities and by the easier access to the financial information

of local borrowers, less credit-worthy borrowers might end up with nonlocal mortgages more likely. To test this conjecture, we investigate the relation between the lending standard of local banks and the demand for nonlocal mortgages using neighborhood level data. We expect that neighborhoods with high local lending standards have a higher share of nonlocal applications.

A bank's branching decision is likely to be endogenous to the characteristics of local communities, so is the scale and share of nonlocal lending in a local mortgage market. To deal with this potential endogeneity problem, we construct a panel data set at the Census tract level by collapsing the HMDA mortgage applications during 2005-2008. We test our hypothesis by estimating the following fixed effects model,

$$Share_{nt} = T_n + \beta Denial_{nt-1} + \beta_t Y_t + \varepsilon_{nt} \quad (3.3)$$

$Share_{nt}$  is the share of nonlocal applications in total applications in tract  $n$  in year  $t$ . We use shares to represent the demand for nonlocal mortgages because they are less sensitive to the sizes of census tracts than other measures, such as the total amounts of nonlocal mortgages.  $Denial_{nt-1}$  is the denial rate of the applications with local banks and is used as a proxy for the lending standard implemented by local banks. We use denial rate in previous year because the lagged variable is less likely to be correlated with factors that affect nonlocal mortgage shares at this period. Also, borrowers typically make their decisions of loan application based on the conditions that local and nonlocal banks propose; borrowers need time to learn about those conditions.

The tract fixed effects,  $T_n$ , control for the unobservable tract characteristics that are potentially correlated with the share of nonlocal lending, especially the average credit quality of borrowers in a tract. The aggregation at the tract level aims to overcome the problem that the HMDA data lacks of credit information for individuals. At the tract level, unless there is migration of large scale, the credit pool will remain similar over the short period covered by our sample. The year fixed effects,  $Y_t$ , control for changes in the macro economy and the housing policies that affect all tracts in a given year.

The estimated impact of the lending standard on nonlocal demands for mortgages is positive and significant in all specifications, as shown in Table 3.6. This supports the hypothesis that nonlocal banks get less creditworthy loan applicants. The OLS estimates overvalue the impact of lending standard on nonlocal demand, comparing to the Fixed Effects estimates. The reason is that OLS does not control for the unobserved quality that negatively correlates to dependent and independent variables.



Table 3.6: The Local Lending Standard and the Nonlocal Credit Demand

VARIABLES	(1)	(2)	(3)	(4)
	Nonlocal share		Out-of-state nonlocal share	
	OLS	FE	OLS	FE
Lagged local denial rate	0.092*** (0.005)	0.015*** (0.006)	0.089*** (0.004)	0.017*** (0.005)
Observations	106,239	106,239	106,239	106,239
R-squared	0.028	0.024	0.03	0.029
Tract FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes

Robust standard errors in parentheses

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

The estimated impact of local lending standard on the demand for out-of-state nonlocal mortgages is slight higher than that on the demand for nonlocal mortgages. This is consistent with the data analysis. Among the nonlocal mortgages, we observe that out-of-state nonlocal loans are more likely to be high-priced and sold to the secondary market than their in-state counterparts. This might reflect the impact of even longer distance between borrowers and lenders of the out-of-state mortgages; it may also due to the obligation of out-of-state banks to promote lending to low-income neighborhoods and low-income groups (Schwartz [1998]).<sup>3</sup>

### 3.5.2 Higher Sales in Secondary Market, Higher Out-of-State Nonlocal Lending

As demonstrated in previous sections, nonlocal borrowers are less creditworthy than other borrowers (see also Rosen [2011]). Except the compensation from higher interest rate, the lenders of nonlocal mortgages, especially those out-of-state lenders, might take advantage of the secondary market to sell mortgages of low quality but still get revenues from origination and other transaction fees.

If the secondary market does provide incentives for banks to make nonlocal mortgages, we expect that banks that are more active on the secondary market will originate more out-of-state nonlocal mortgages, given everything else equal. To investigate this, we estimate different versions of the following fixed effects model,

$$Share_{jt} = B_j + Sales_{jt-1}\beta + X_{jt}\gamma + \beta_t Y_t + \varepsilon_{jt} \quad (3.4)$$

$Share_{jt}$  is the share of out-of-state nonlocal loans for a given bank  $j$  in year  $t$ . The explanatory variable in interest is,  $Sales_{jt-1}$ , the sales rate of the bank in the previous year. This measure is a proxy for the bank's operation skills on the secondary market such as loan packing and marketing, as well as its closer connection with the purchasers on the secondary market. The bank fixed effects,  $B_j$ , control for the unobservable bank characteristics that are correlated with the out-of-state nonlocal lending and the participation in the secondary market. The year fixed effect,  $Y_t$ , controls for shocks in a given year that affect all banks.  $X_{jt}$  is a vector of control variables, such as total assets, total application counts (including applications for government backed loans and refinance loans), shares of loans sold to different types of purchasers on the secondary market, and the share of subprime loans.

The HMDA loan origination data are aggregated to the bank level to construct a panel data set in order to control for the fixed effects of banks. We restrict the sample to banks that originate

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<sup>3</sup>Distinguishing the different explanations is left for future research.

more than 10 loans, have a proportion of out-of-state nonlocal loans greater than 10 percent, and sell at least one loan to the secondary market. In total, there are 407 state banks in the sample. The biggest out-of-state nonlocal lender, Fremont Investment & Loan, is no longer a concern in this analysis because it only accounts for one observation among more than 3000 thousand banks.

We find that banks that successfully sell a larger proportion of their mortgages in the previous year—more active or capable on the secondary market—originate a higher proportion of out-of-state nonlocal mortgages. In Table 3.7, column 1 and 2 show the results from the model without any control variables; column 3 and 4 show the results with controls. The OLS and FE estimators in all specifications are positive and significant. According to the FE estimates in column 4, the share of out-of-state nonlocal mortgages of a bank increases 0.19 percent if its sales rate in the previous year increases by one percent.

Interestingly, the size of a bank’s total assets has negative impact on its share of out-of-state nonlocal mortgages, while the total number of applications has positive impact on the out-of-state nonlocal share. These patterns are robust across different specifications. This implies that banks with smaller assets or those receiving more applications originate more out-of-state nonlocal mortgages. A bank without large assets is less likely to have branches and holds large amounts of mortgages, so it has to make more nonlocal mortgages and sell them to the secondary market. Banks receiving more applications are more likely to get that expansion from areas without branches, that is, involve more in the out-of-state nonlocal mortgage market. Such findings are similar to those in Purnanandam (2010) that also uses the HMDA data.

The share of out-of-state nonlocal loans might be determined simultaneously with the share of sold loans since the characteristics of the mortgages originated by a lender also affect its decision of secondary market participation. To deal with this problem, we use a two stage least square model (2SLS) and report the results in Table 3.7. In the 2SLS regression, the lagged share of sold loans is instrumented by a dummy for subsidiaries of Banking Holding Companies, the lagged asset and origination count, and the lagged share of conforming loans originated by the lender. These instrumental variables affect the secondary market involvement of banks, but are less likely correlated with the share of out-of-state nonlocal mortgages of banks.

Comparing to the estimates from OLS and fixed effects (FE) models, the 2SLS estimates show larger impact on the nonlocal share from the secondary market involvement, which strengthens our previous findings. However, the significance level of the 2SLS estimates is lower than those from OLS and FE models, and the estimates from OLS and FE and 2SLS have overlapped confidence

Table 3.7: The Sale Ability and the Out-of-state Nonlocal Lending Share

VARIABLES	(1) OLS	(2) FE	(3) 2SLS	(4) OLS	(5) FE	(6) 2SLS
Lagged sales rate	0.096** (0.039)	0.186*** (0.063)	0.972** (0.394)	0.104*** (0.038)	0.188*** (0.065)	0.395** (0.193)
Assets(\$billion)				-0.001** (0.001)	-0.001*** (0.000)	
Application count (1000)				0.002*** (0.001)	0.001*** (0.000)	
Lagged GSE purchase				0.013 (0.025)	0.058 (0.053)	0.086* (0.052)
Lagged private securitization				0.034 (0.063)	-0.023 (0.026)	0.113 (0.095)
Lagged affiliation purchase				0.187** (0.092)	0.034 (0.027)	0.292** (0.119)
Lagged subprime ratio				0.191*** (0.059)	-0.027 (0.078)	0.306*** (0.085)
Observations	407	407	407	407	407	407
R-squared	0.034	0.072		0.197	0.117	
Bank FE	NO	YES	NO	NO	YES	NO
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Control	NO	NO	NO	YES	YES	YES

Robust standard errors in parentheses

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

intervals. Such findings suggest that the simultaneity problem biases downward the effect of the secondary market involvement on the share of out-of-state nonlocal mortgages.

### 3.6 CONCLUSIONS

This paper documents and investigates the high subprime rate and the high sales rate of out-of-state nonlocal mortgages. We show that nonlocal mortgage demand is greater in the neighborhoods where the borrowers get rejected by local banks more often. This indicates that less-creditworthy and riskier applicants tend to end up with nonlocal loans. Additionally, we explore the incentives to make nonlocal loans to low-quality applicants in other states by investigating banks' involvement in the secondary market. We find that banks sell more loans to the secondary market in the past will originate a higher proportion of out-of-state nonlocal loans. The risky lending made by out-of-state nonlocal banks might be one contributing factor of the Subprime Crisis. Our findings suggest that government regulatory agent should be careful about the rules on interstate banking and brokers.

## APPENDIX

### REGULATORY ENVIRONMENT IN CLEVELAND

#### A.1 CLEVELAND PREDATORY ORDINANCE: A TIMELINE

- February 22, 2002, the Ohio General Assembly enacted Sub.H.B. No. 386, 149 Ohio Laws, Part IV, 6938, incorporating much of the substance the federal Home Ownership and Equity Protection Act of 1994 into Ohio law.
- The city of Cleveland promulgated Cleveland Codified Ordinance 737-02 on April 23, 2002, meant to be effective on April 25, 2002. (Ordinance 659.02 ; Ordinance 45-03, January 15, 2003).
- AFSA filed immediately to enjoin Cleveland and Dayton from enforcing the Ordinance.
- July 26, 2002, Judge Peggy Foley Jones denied AFSA's request for a preliminary injunction. As a result, Cleveland Home Mortgage Lending Ordinance took effect on July 26, 2002 for all loans closed on or after that date.
- September 22, 2003, the Cuyahoga County Court of Common Pleas declared most of Cleveland's Home Mortgage Ordinance to be in direct conflict with Ohio law and, therefore, unenforceable.
- The Eighth District Court of Appeals, ruled Cleveland's Ordinance valid (*American Financial Services Association v. City of Cleveland*, Case No. 83676 (December 2, 2004), 159 Ohio App.3d 489, 2004-Ohio-6416). This conflicted with the Second District Court of Appeals' decision to rule that Dayton's ordinance was invalid (*City of Dayton v. Ohio*, 2004 WL 1367067 (June 18, 2004), 157 Ohio App.3d 736, 2004-Ohio-3141).
- November 20, 2006, Ohio Supreme Court ruled Cleveland Ordinance unconstitutional (*American Financial Services Association v. City of Cleveland*, 112 Ohio St.3d 170, 2006-Ohio-6043).

- During the time of the law suit, if a separate suit was brought against a lender for violation of Cleveland’s ordinance, it would have been thrown out before the Ohio Supreme Court ruled in 2006.
- February 2004, Fitch Ratings addressed the Cleveland home mortgage regulation and decided to continue to rate RMBS that include mortgage loans originated in the City of Cleveland. (“Assuming that any Ordinance is effective or that all of the Ordinances are effective, they prohibit the City of Cleveland from entering into a contract for goods or services with a financial institution which is deemed a predatory lender as defined by the Ordinances and violations of the Ordinances are classified as misdemeanors, unless an assignee is found to knowingly assist a predatory lender, such penalties cannot be assessed against an assignee.”).

## A.2 FEDERAL PREEMPTION

Because of the dual banking system and the inter-state banking trend, state anti-home mortgage regulations apply to various home mortgage creditors to different extent based upon creditors’ charters under state or federal regulations. Federal creditors enjoy preemption authorized by federal regulatory agencies, which also provide parity for state creditors regarding certain mortgage transactions. Out-of-state banks, generally regulated by its home state, are treated as national banks in the host state.

The Comptroller of the Currency (OCC), National Credit Union Administration (NCUA), and Director of the Office of Thrift Supervision (OTS) adopted regulations authorizing federally chartered thrifts to engage in alternative mortgage financing.<sup>1</sup> In order to eliminate the discriminatory impact that those regulations have upon other housing creditors, the Alternative Mortgage Transaction Parity Act (AMTPA, effective October 15, 1982) provided them parity with federally chartered creditors by authorizing all housing creditors to make, purchase, and enforce alternative mortgage transactions so long as transactions are in conformity with regulations issued by Federal agencies. By law, OCC promulgated regulations for providing alternative mortgage parity to state-chartered

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<sup>1</sup> 12 U.S.C. 3802(1): an “alternative mortgage transaction” is a loan or credit sale secured by an interest in residential real property –(A) in which the interest rate or finance charge may be adjusted or renegotiated; (B) involving a fixed-rate, but which implicitly permits rate adjustments by having the debt mature at the end of an interval shorter than the term of the amortization schedule; or (C) involving any similar type of rate, method of determining return, term, repayment, or other variation not common to traditional fixed-rate, fixed-term transactions, including without limitation, transactions that involve the sharing of equity or appreciation; described and defined by applicable regulation.

banks, the NCUA for state-chartered credit unions, and the OTS for both state-chartered thrifts and mortgage companies(non-depository institutions approved by HUD).

### **A.3 OHIO PREDATORY LENDING BILL OF 2007, SB 185**

The Ohio anti-predatory lending bill of 2007, SB 185, effective on January 1, 2007, substantially revised most laws regulating consumer real estate lending in Ohio. Those changes include:

- Creating a mechanism for criminal background checks for real estate lending professionals (state statutes).
- Reducing the amount of permitted prepayment penalties (Second Mortgage Loan Act, Interest Act).
- Prohibiting certain lending practices and requiring certain disclosures (Mortgage Broker Registration Act).
- Creating new duties and liabilities for non-depository mortgage lenders (Consumer Sales Practices Act).
- Adding new consumer protections (High-cost Home Loan Act).
- Adding consumer protections regarding title insurance agents and appraisers (state statutes).



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