

Does Mortgage Deregulation Increase Foreclosures?

Evidence from Cleveland *

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Abstract

The financial crisis of 2007 has re-ignited a debate about the impact of regulating consumer mortgage markets. This paper examines 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 use a difference-in-difference research design that compares loan outcomes within and outside the city limits of Cleveland before and after the repeal of the law. Results indicate that deregulation caused a 49 percent increase in early foreclosures while it did not increase total loan volume, implying that bad loans as a share of overall loans increased substantially. Deregulation also increased the number of high-interest loans by 30 percent, increased loans made by subprime lenders by 40 percent. These results are robust to the window of time examined before and after deregulation, as well as to examining only areas on the boundary of the city limits. Moreover, placebo tests reveal there was no divergence in loan quality prior to the Cleveland repeal, and that there was no evidence of a similar divergence in Pittsburgh at the time of repeal in Cleveland. (JEL: D12; D18; G21; G28; K13; K35; R38)

Keywords: deregulation; financial regulation; consumer protection; home mortgage regulation; subprime lending; foreclosure

1 Introduction

The financial crisis of 2007 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 (Bernanke [2009]). Specifically, new mortgage products that correctly account for risk have the potential to increase the supply of loans to worthy low income households.

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

On November 20, 2006 the Ohio State Supreme Court overturned a city level 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 including prepayment penalties, increased interest following a default, mandatory arbitration, financing of credit insurance, lending without counseling, etc. Following the court order, mortgage lenders could freely charge interest rate beyond the 4.5 percentage point spread and include these additional terms as they saw fit. 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, using a unique mortgage data set that is matched to household foreclosures,

I find that deregulation has a profound impact on the composition of loans: loans with subprime interest rates increased by 30% and loans issued by subprime lenders increased by 40%. The most striking result, however, is that deregulation caused loan foreclosures to increase by 49%.¹ Moreover, the 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 loans with early foreclosures in Cleveland after and before deregulation and identifies the impact of the deregulation as the deviation from the counterfactual time trend.²

There are two major issues in making a causal interpretation of the impact of financial deregulation on loan composition and foreclosures. First, the deregulation should be exogenous and not picking up pre-existing lending patterns. This is plausibly the 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. Although after the Second District Court of Appeals ruled Dayton's mortgage regulation invalid in 2004 there might be some expectation of AFSA eventually winning the case, the timing of the ruling is 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.³

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

²Agarwal et al. [2010] use a similar method in their study of the impact of mandatory mortgage counseling on home mortgage lending in Cook County, Illinois.

³See section 2.1 for a detailed discussion.

The second problem with making a causal interpretation is whether the increased early foreclosures are driven by confounders that affect Cleveland and the suburbs differently before and after November 2006. I address this concern in several ways. To ensure the results are not driven by sample selection, I run robustness checks for loans not preempted by federal regulatory agencies, loans made within a smaller window of time and loans made within smaller areas on the city border; and I find a similar increase in early foreclosures. Furthermore, I run several falsification tests and find 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 studies have 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 on time-series variation before and after state laws became effective. In order to deal with omitted variable issue, they both limit their samples to counties that are located on 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 with/ getting 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 higher risk of default; therefore, this measure provides limited insight on quality effect of mortgage regulation. My paper fills in the gap by investigating the home foreclosures of loans made before and after an exogenous repeal of a local mortgage regulation.

Clauretje and Herzog [1990] and Pence [2006] show that the ease with which lenders can foreclose on their borrowers mitigates the loan losses and causes lenders to issue larger loans. My paper is the first study to my knowledge that identifies the effect of deregulation

on home foreclosures. Due to data limitation, only a few studies were able to address the quality effect of mortgage regulations. Using the 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] find that foreclosure rates of those loans between 2006 and 2008 are lower in states where the home mortgage regulations have border coverage and stricter restrictions. The proprietary loan data set provides detailed loan information including delinquency and foreclosure, but covers only around 50% of the home mortgage market. Alternatively, the Home Mortgage Disclosure Act (HMDA) data set provides extensive mortgage market coverage but has no information about loan performance. My unique data set matches the HMDA loan record to individual home foreclosure record. To directly count for the impact of deregulation, I include in the sample only the loans made six months before and after the deregulation and track their foreclosures within 30 months after loan origination.

There is great social and economic importance of measuring the impact of deregulation on foreclosures. In the short-run, foreclosures give rise to more crime in the neighborhood (Immergluck and Smith [2006b], Cui [2010]), lower the property values of nearby homes, and therefore harm the tax base for the local government (Immergluck and Smith [2005], Immergluck and Smith [2006a]; Schloemer et al. [2006]). In the long-run, they decrease residential investment and durable consumption (Mian et al. [2011]).

2 Evidence From Cleveland

2.1 The Federal and Local Home Mortgage Regulation

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 legislations. 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 laws. A mortgage that belongs to certain product category is considered to be a “high-cost

loan” and therefore regulated by the laws, if it carries fees and an Annual Percentage Rate (APR) that exceed certain thresholds (i.e. the trigger points). In the second part, certain disclosures requirements and lending restrictions are imposed on the covered loans, such as counseling requirements, prepayment penalty restrictions, balloon payment restrictions, and restrictions on mandatory arbitration. Typically, the state laws enhance both the coverage and the restrictions of HOEPA.

HOEPA actually only covers a small share of home mortgage loans because of high fees and interest rate thresholds. In addition, HOEPA also leaves the home-purchase loans unregulated. The home-purchase loans, however, are directly related to the home foreclosures. The Ohio state home mortgage regulation incorporates the coverage of HOEPA, and regulates only closed-end home equity loans. The Cleveland Home Mortgage Lending Ordinances of 2002⁴. however, extended the coverage to all home loans including home-purchase loans. Moreover, Cleveland Home Mortgage Lending Ordinances implement lower interest and fee thresholds which result in wider coverage. For instance, for the first-lien loans, the Ohio law regulates only “those having an interest rate that exceeds by more than 8 percentage points the yield on Treasury securities”. The Cleveland Ordinances extended the coverage of the law to “those having an interest rate between 4.5 and 8 percentage points above the yield on certain Treasury securities”.

In addition to the restrictions implemented by Ohio state law, the Cleveland Ordinances prohibits prepayment penalties, loan flipping, balloon payments, negative amortization, an increased interest rate on default, advance payments, mandatory arbitration, financing of credit insurance, lending without counseling, lending without due regard to prepayment, and payments to home improvement contractors under certain circumstances. These mortgage contract terms directly and indirectly affect the borrowers’ payment burden and therefore their default and foreclosure probability.

When Cleveland Ordinances were in effect, mortgage loans secured by home properties

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

within the city limits of Cleveland were required to comply with the Ordinance, regardless of the location of the lenders' offices. Therefore the lenders could not circumvent the regulation by redirecting their borrowers to their suburban offices. This regulatory design makes it possible to identify the regulated loans simply by the location of the properties that secured the loans.

The Cleveland Home Mortgage Lending Ordinance passed on April 23, 2002, including extended coverage and extended restrictions, were challenged shortly after enactment by the American Financial Services Association (AFSA). The Cleveland Ordinances 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 declared the Cleveland Ordinances to be in conflict with Ohio law. The Cleveland Ordinances were reinstated on December 2, 2004, when the Eighth District Court of Appeals ruled the Cleveland Ordinances valid. On November 20, 2006, the Cleveland Ordinances were ruled unconstitutional by the Supreme Court of Ohio because it implemented stricter restrictions than the Ohio law.⁵ This paper interprets this repeal as a deregulation, and samples 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.

Before the Eighth District Court of Appeals ruled Cleveland Ordinances valid in 2004, there were precedents where courts overturned local home mortgage regulation. In particular, the Second District Court of Appeal of Ohio ruled Dayton Home Mortgage Ordinance invalid in June, 2004. If in anticipation of AFSA to win the case, the lenders and borrowers ignored the law before it was actually repealed, the DID estimate will bias towards zero. Any non-zero effects can be interpreted as the lower bound of the true effect.

⁵Appendix A provides a review of the lawsuit.

2.2 The Matched Mortgage and Foreclosure Data

The loan and foreclosure data come from the Loan Origination and Foreclosure Matched Data of Cuyahoga County, available through NEO CANDO (Northeast Ohio Community and Neighborhood Data for Organizing⁶). Matching the Home Mortgage Disclosure Act (HMDA) data with the Cuyahoga foreclosure filing records, 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. [2008] for a detailed explanation of the statistical matching procedure.

My sample of loans includes home-purchase loans secured by owner-occupied one- to four-family housing properties in Cuyahoga county. The matched data set tracks the foreclosure filings up through December 2009; therefore, the foreclosure variable is censored. In this paper, a loan is defined as “early foreclosed” if the lender/ servicer files a foreclosure complaint within 30 months after loan origination. Besides the data availability, another important reason to define early foreclosures with a 30-month moving window is that typical Adjustable Rate Mortgages (ARMs) reset the interest rates in 3 years or 5 years. By using a narrower window to define early foreclosures, I can exclude the possibility that my foreclosure results are driven by ARM interest rate reset.

I aggregate individual loan data from Cuyahoga County to tract-month level. The constructed longitudinal data set consists of 5,700 monthly observations of 475 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, 12.1 out of 100 matched loans in Cuyahoga ended up in early foreclosures.

Table 1 provides the summary statistics of the treatment group and control group. The

⁶NEO 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.

outcome variables include the number of loans, the amount of loans, and the number of early foreclosed loans. Each outcome is measured at the tract level in each month between June 2006 and May 2007. Cleveland and the suburbs have similar number of loans with early foreclosures in each period, however, both groups experience decrease in foreclosed loans after deregulation. Suburban tracts have more overall loans than city tracts both before and after the repeal, and there are more loans made before the repeal than after the repeal in both the suburbs and the city. The total loan amount also shows similar pattern. In other periods, Cleveland has higher foreclosure rates than its suburbs.

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 regulated by city law while those secured by properties in the suburban were regulated by state law. After November 20, 2006, the mortgages in the suburbs remained in the same regulatory regime, while the ones in Cleveland experienced a deregulation. Using the nearby municipalities to control for the credit demand and credit quality in Cleveland in the absence of the repeal, comparing the outcomes of home mortgages in Cleveland before and after the repeal of the law, a DID estimation identifies the impact of the deregulation.

A causal inference of the DID estimation hinges on the exogeneity of the repeal of the city law. Given that the law was challenged by AFSA right after the enactment in 2002, the repeal of the law in 2006 is not endogenous to the composition of loans or the pre-existing lending and borrowing behaviors between 2002 and 2006. Although there might be some expectation of AFSA winning the course, the timing of the court ruling is exogenous. In my analysis, I investigate only the loans made between May 2006 and June 2007, i.e. six months before and six months after the court ruling. If in anticipation of AFSA to win the case, the lenders and borrowers ignored the law before it was actually repealed, my estimation results can be interpret as the lower bound of the true effect.

Figure 1 presents the Cleveland and suburban time trends for the (log) number loans with early foreclosures, the (log) number of loans, and the (log) total loan amount, respectively in each panel. Even before the repeal, there are gaps between Cleveland time trends and the suburban time trends in each panel because of different social and economics characteristics of these two groups. The DID method allows differences between the treatment group and the control group, as long as the differences persist in the absence of the treatment. In all three panels, Cleveland and suburban municipalities share common time trends before the repeal, suggesting that the suburban municipalities provide a reasonable control group for Cleveland. The DID method assumes Cleveland and the suburban municipalities continue to follow the same time trend after November 2007 had the deregulation not happened, and measures the impact of deregulation as the deviation between Cleveland and the suburban time trends in the outcomes following the repeal of the law.

Figure 1 shows an obvious seasonal pattern: time trends for all outcomes in both groups go down towards the end of year, and go up starting from December 2006. Moreover, all outcomes for both groups drop after January 2007, when the financial crisis turmoil began. As the control group in the DID design, the suburban municipalities controls for any factors that have common impact on the city tracts and the suburban tracts. However, if the seasonal pattern, the financial crisis, or other unobserved factors have differential impact on the two groups, one may misinterpret the impact of those factors as the treatment effects of the deregulation. I address this concern in several ways. In Section 3.1, I provide robustness cases for loans made in narrower window of time and loans made on the boundary of the city limits of Cleveland, and I find comparable treatment effects. In Section 3.4, I test the seasonal pattern and financial crisis as alternative explanations; in Section 3.5, I statistically test housing bubble and bad economy as alternative explanations; I find no evident in support of 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 deregulation. Without rigorous statistical test, one can visually spot whether there is a deviation 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. It suggests that the loans with early foreclosures in Cleveland increases, but the origination volume and the total loan amount in Cleveland does not change after deregulation, using the suburbs as a control for the counterfactual time trend of Cleveland.

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 the deregulation did not happen. This specification is preferred to a OLS specification where the dependent variable is the count, because the latter assumes some level of transitory change in both groups in the absence of the treatment, which is inappropriate given that the two groups start with very different levels of counts and that the transitory shock is dramatic. The estimation equation is the following:

$$E(y_{it}|X) = \exp(\theta_i + \eta_t + \alpha_1\delta^c + \alpha_2\gamma^R + \alpha_3(\delta^c \times \gamma^R)) \quad (1)$$

y_{it} is the loan count⁷ 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 with 30 months after origination. In the loan volume specification, y_{it} is the count of loans that were made in census tract i in month t . θ_i is the tract fixed effects, and η_t is the month fixed effects. δ^c is a dummy for tracts in Cleveland. γ^R is a dummy for the periods after the repeal. In a panel Poisson specification,⁸ α_3 is the log odds ratio that measures the

⁷I do not use the foreclosure rate as an outcome because origination and especially early foreclosures are low frequency events when I aggregate to tract-month level. As a result, the normal distribution assumption of OLS will be violated because of inflated zeros. The Poisson estimation accounts for the cluster of zeros.

⁸A user written Stata module, XTPQML, is used to estimate Fixed-effects Poisson (Quasi-ML) regression with robust standard errors.

treatment effect and is the coefficient of interest.

$$\alpha_3 = \log \frac{E(y_{Cleveland,after}|X)}{E(y_{Cleveland,before}|X)} / \frac{E(y_{Subs,after}|X)}{E(y_{Subs,before}|X)} \quad (2)$$

A positive α_3 suggests that 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 the similarity between control and treatment groups, and the comparability of the observations before and after the 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]), and the second sub-sample narrows the observation window to three months before and after the repeal. Furthermore, as robustness checks, I restrict my sample to loans not preempted from the local home mortgage regulations by the federal regulatory agencies and loans with early foreclosures within 24 months.

3 Empirical Results

3.1 The Effect of Deregulation on Home Foreclosures

In this section, I estimate Equation (1) and test whether deregulation causes bad loans (measured as early foreclosures) to increase. The results are reported in Table 2. All specifications include month and tract fixed-effects: and, standard errors are clustered at the tract level.

Column (1) is the baseline case. Here early foreclosures are defined as foreclosure complaints filed in court by the lender within 30 months after the loan origination. The sample includes tract-level observations six months before and after the repeal. 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 and implies a 49% increase in the number of loans with early foreclosures in Cleveland after the repeal, and the magnitude is significant at 1% level.

According to the federal preemption enforced by Office of the Comptroller of the Currency (OCC) in 2004⁹, some of the lending restrictions implemented by Cleveland Ordinances did not apply to national banks and their subsidiaries. The impact of deregulation would be smaller for the national banks than for the state banks. To deal with this compositional effect, I exclude the loans made by national banks in Column (2). The estimates are similar to the one in the baseline case.

Since the deregulation in November 2006 more or less coincides with the onset of the financial crisis¹⁰, the baseline estimate may be really capturing the differential impact of the financial crisis in Cleveland versus its suburbs. In order to address this concern, in column (3) I eliminate the the period of four to six months after and before the deregulation. The estimates in column (3) are similar to the baseline results, which suggests that the treatment effect is not driven by differential shocks to the city and the suburban municipalities.

Another concern is whether the suburbs serve as a legitimate control group. As shown in the top panel of Figure 1, the foreclosure time series of Cleveland and the suburbs followed the same trend before deregulation, suggesting the legitimacy of the suburbs as the control. To enhance the similarity between the treatment group and the control group, I investigate a sub-sample of loans that are secured by housing properties in tracts on the city border. Column (4) reports the estimate of the impact of deregulation on those loans in areas with most similar economic and social conditions. The size of estimate is comparable to the baseline case.

Early foreclosure is defined by a 30-month moving window in Column (1)-(4). One may wonder whether the increased early foreclosures are driven by timing effect. Column (5) in Table 2 defines early foreclosures by a 24-month window, and still shows positive estimate of similar size.

⁹See Appendix B for a short review.

¹⁰<http://timeline.stlouisfed.org/pdf/CrisisTimeline.pdf>

One possible interpretation of my findings is that deregulation caused borrowers to take on more costly loans. After the repeal, lenders can freely charge interest rates beyond 4.5 percentage points spread and impose other non-interest terms in the contract. For example, balloon payments, increased interest rates after default, and prepayment penalties are no longer prohibited for loans with interest rates beyond 4.5 percentage points spread. Those loans terms, when implemented by the lenders in the loan contracts, will increase the borrowers' payment burden and therefore increase the default and foreclosure probability. Quercia et al. [2007] shows that refinanced loans with prepayment penalties and those with balloon payments are more likely to experience a foreclosure than loans without those characteristics.

3.2 The Effect of Deregulation on Loan Volume and Amount

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

The dependent variable in Panel A of Table 3 is the loan count 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 α_3 in Equation (1) and the tract-clustered standard errors are reported. The Column (1) reports the baseline case, where the sample includes the tract observations in the entire Cuyahoga County six months before and after the repeal. The estimates are very close to zero in magnitude and are precisely estimated. The estimate indicates that deregulation does not have a significant impact on loan origination. Combined with increased foreclosures discovered in the previous section, it suggests that the foreclosures as a share of the overall loans increased following deregulation in Cleveland.

Compared to state banks and non-depository lenders, the national banks are less affected by local home mortgage regulation due to the federal preemption. The sample in Column (2) excludes the loans made by national banks. The estimated treatment effects are close to zero and similar in size to the ones in the baseline case, suggesting that the distribution of loan origination across the state charter and national charter remains the same after the repeal. It implies that the increased early foreclosure is not the result of borrowers sorting into the charter that makes worse loans.

To enhance the similarity of the observations in the control group and the treatment group, as well as before and after the treatment, I provide two more robustness checks. Column (3) restricts the sample to loans made within three months before and after the repeal; Column (4) restricts sample to only loans made in the tracts on the city border. The estimates show no significant change in the loan count in Cleveland after the repeal.

In Penal B of Table 3, I estimate the impact of deregulation on the total loan amount. Across four specifications, the estimates are insignificantly different from zero, suggesting the total loan amount does not change as a result of deregulation in Cleveland. The unchanged loan count and unchanged total loan amount implies that the average loan amount for loans in Cleveland remained unchanged relative to that for the ones in the suburban municipalities after the repeal.

3.3 Differential Effects by Lender Types and Loan Types

Since the Cleveland home mortgage regulation regulated only the “high-cost” loans and certain “predatory” lending behaviors, one may expect the deregulation to affect the subprime borrowers and the lenders more than the prime borrowers and lenders. I run 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 point. 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 4 reports the DID results by loan types and lender types. Panel A reports foreclosure results by loan type and lender type. The estimate for loans with subprime interest rates is significant and similar in size to the one in the baseline case, but the estimate for the loans with prime interest rates is estimated less precisely. The results imply that both loans made by subprime lenders and the loans made by prime lenders experience more early foreclosures after repeal, and the magnitudes are similar.

Panel B reports loan volume 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% increase in the subprime loans. A coefficient of 0.34 translates into an odds ratio of 1.40 and implies a 40% 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 do not increase after repeal. These results suggest that although deregulation does not affect the overall loan volume, it changes the composition of loans.

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

3.4 Falsification Tests: Timing and Locality

In this section, I test whether my result of increased early foreclosures in Cleveland after deregulation is driven by seasonal pattern in credit market, shocks to the city, or shocks

from the financial crisis.

As shown in Figure 1, there is a salient seasonal pattern in loan origination and foreclosures. I run the DID estimation for artificial “repeals” 12 months and 6 months before the actual repeal. If my findings are driven entirely by the seasonal pattern, one should expect to see a “treatment” effect in the same month every year/ every half year even in the absence of the repeal.

The graphs on the top panel of Figure 2 present the Cleveland and suburban time trends six months before and after the false treatments in November 2005 and May 2006, respectively. There is no visual evidence of deviation between the time trends of the treatment and control groups. Column (1) and Column (2) in Table 5 present the estimation results for false treatments in November 2005 and May 2006, respectively. Falsifications for fake treatments show insignificant estimates of the opposite sign, suggesting the treatment effect in the baseline case is not driven by seasonal pattern in the home credit market.

Moreover, as mentioned in Section 2.3, the coincidence of repeal and the financial crisis may mislead one to interpret the impact of the financial crisis as the impact of deregulation. To further address this concern, I run two more falsifications. I first restrict the sample to suburban tracts only, and assign the “inner suburbs”¹¹ as the treatment group and the “outer suburbs” as the control group. If there is any economic shocks to Cleveland in November 2006 that can cause more foreclosures, one should expect to find the shock producing a “treatment effect” for the suburban tracts in proximity to the city. In addition, if there is any change in home purchasing preference or payment preference among the borrowers in Cleveland after November 2006, then the preferences are likely to be contagious across geographies regardless of the city border. One should expect to find a treatment effect for the inner suburban tracts if the change in preference explains the increase in early foreclosures.

The left graph on the bottom panel of Figure 2 show the time trends for the inner suburban tracts and the outer suburban tracts. There is no evidence of closing gap between

¹¹census tracts on the Cleveland city border

the two time trends. Column (3) represents the result for false treatment for the inner suburban tracts. The estimate is close to zero and insignificant, suggesting no treatment effects for the inner suburban tracts, using the outer suburban tracts as the control group.

I also run a DID estimation for the observations in Pittsburgh and its suburbs during June 2006 to May 2007.¹² If differential impact of the financial crisis on the city and the suburbs caused more foreclosures in Cleveland after November 2006, then it would also cause more foreclosures in other cities, relative to their suburbs in the same period. One should expect to see a treatment effect in Pittsburgh if the financial crisis explains the increased early foreclosures in Cleveland.

The right figure on the lower panel of Figure 2 plots the time trend of early foreclosures for Pittsburgh and its suburbs. Although the time series for Pittsburgh and its suburbs are noisier than the ones for Cleveland and its suburbs, they 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, suggesting no divergence of loan quality in Pittsburgh after November 2006.

Column (4) and Column (5) in Table 5 presents the falsification for loans made in Allegheny County in the same sampling period. The early foreclosure is defined by a 30-month moving window. The estimate in Column (4) is positive, yet insignificant. Narrowing the window of time to 3 month before and after the repeal, however, results in a slightly negative estimate, and again insignificant. The DID result for false “repeal” in Pittsburgh suggests that the shock of the financial crisis did not cause the treatment effect found earlier.

¹²The 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. [2008]. Thirteen out of 100 matched loans in the Cuyahoga sample were ever foreclosed by the end of December 2009, 6 out of 100 matched loans in Allegheny county were ever foreclosed by the end of November 2009. During June 2006 and May 2007, 12.1 out of 100 matched loans in Cuyahoga end up with early foreclosures, 3 out of 100 matched loans in Allegheny end up with early foreclosures.

3.5 Treatment Effect: Deregulation v.s. Economic Shocks

In Section 3.1, I find more bad loans in Cleveland after deregulation than before deregulation, using the suburban municipalities as the control group. A causal interpretation of the treatment effect assumes that if the deregulation never happened, then there would be no divergence in loan quality between Cleveland and its suburbs. This section tests whether confounders such as 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 if, the mortgage balance exceeds the house value (the negative-equity theory), or income shocks such as unemployment and divorce occur (the trigger event theory). Accordingly, we should expect more loans made in the treatment period to get foreclosed even in the absence of deregulation if, compared to the ones made in the control period, those loans start with higher housing prices at origination (housing bubble); or borrowers of those loans are more vulnerable to income shocks (bad economy).

First, I test whether the individual housing price is higher in the period between December 2006 and May 2007 than in the period between June 2006 and November 2006. I run a 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 the sales prices for Cleveland housing and that for suburban housing after the repeal, given housing characteristics are held constant.¹³ If loans made after November 20, 2006 in Cleveland start with higher housing price at origination than that of the ones made before, one would expect the estimate to be positive.

Column (1) and Column (2) of Table 6 reports the coefficients and tract-clustered standard errors. Column (2) includes control housing characteristics such as the overall

¹³The sales price and property characteristics at parcel level are from Cuyahoga Auditor Parcel Characteristics File and Cuyahoga Auditor Parcel Characteristics File, available through NEO CANDO.

condition, number of units built on the parcel, total usable square footage, year built, building style, external wall type, number of bedrooms and bathrooms. Both specifications control for tract fixed effects and month fixed effects. The insignificant negative coefficients provide strong evidence that the increased number of loans with early foreclosures in Cleveland after the repeal cannot be attributed to the stress from higher housing prices.

Next, I test whether the local economy in the period between December 2006 and May 2007 is worse than in the period between June 2006 and November 2006. Because monthly unemployment rate and household income are unavailable, I use the number of foreclosed homes in a given census tract as a measure for the social and economic stress in the neighborhood and run a panel poisson estimation to test the treatment effect.¹⁴

Figure 3 plots the trends of foreclosed homes in Cleveland and its suburbs before and after the repeal. Although the number of foreclosed homes is noisy, the gap between the two trends seems to widen after the repeal, suggesting that there were relatively fewer foreclosed homes in Cleveland in the 6 months after deregulation than in the 6 months before deregulation, using the suburbs as the control group. Column (3) of Table 6 reports the estimate of the treatment effect and the tract-clustered standard error. The treatment effect is negative and insignificant, suggesting that the loans in Cleveland after the repeal were not made in an environment with more home foreclosures than before the repeal.

4 Discussion

Given that the Cleveland home mortgage regulation has been under lawsuit since its enactment, the findings in this paper are very compelling. To the extent the lenders and borrowers could anticipate the deregulation and therefore ignore the Home Mortgage Ordinances before actual repeal, the increased early foreclosures found in this paper suggest

¹⁴The dependent variable is the number of foreclosed homes in tract i in month t . The sample only includes the loans that were made before the repeal (hence were not affected by deregulation), but were foreclosed during June 2006 and May 2007, i.e. within 6 months before and after the repeal. The foreclosure information is from the Loan Origination and Foreclosure Matched Data of Cuyahoga County, 2005-2008, therefore only the foreclosures of homes with loans made between January 1, 2005 to May 31, 2006 are included.

that the impact would have been greater had the Cleveland home mortgage regulation not been challenged.

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 amends most of the laws regulating consumer real estate lending in Ohio.¹⁵ My DID results will be questionable if SB 185 curbs early foreclosures more effectively in the suburban municipalities than in the city. Given that Cleveland possessed higher subprime rates and higher early foreclosure rates before SB 185 took effect, it is more likely that mortgage lending in Cleveland is more sensitive to the new enforcement of consumer protection measure, and the foreclosures should decrease more than in the suburbs as a result of the enforcement of SB 185.

Another caveat for the foreclosure results is that the “foreclosure” in this paper is defined by foreclosure filings, which initiate the judicial foreclosure process mandated by Ohio law. Foreclosure filings as an outcome is a measure for loans in extreme cases. 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. A foreclosure filing may not result in an actual foreclosure if the borrower successfully pays off the amounts due in the grace period. In some scenarios, borrowers may want to have short sales or file for chapter 13 bankruptcy to avoid foreclosure.

This paper is inconclusive about the mechanism that leads to more foreclosures. Results in Column (1) and Column (2) of Panel A in Table 3 show that there is no redistribution of loans across city border or across federal preemption eligibility after deregulation, which suggests that the increased foreclosures cannot be attributed to borrowers of bad credit sorting into the city or sorting into preempted lenders when the lending restrictions were lifted. However, there is more than one post-deregulation scenario that can explain the increased foreclosures. For example, lenders might lower the lending standard to include borrowers who are more vulnerable to macroeconomic shocks later; lenders might offer less

¹⁵Appendix C provides a review of the amendments.

favorable loan terms that stress the borrowers later; finally, lenders might be reluctant to modify the loans that are made after deregulation once they are defaulted.¹⁶

In my further investigation (Xu [2011]), I find that removal of price-triggered lending restrictions does not affect the composition of borrowers; however, it increases the likelihood of getting an interest rate above the trigger point and the likelihood of having an early foreclosure, given observable characteristics of borrowers and loans. Following the removal of the lending restrictions, subprime lenders are more likely to approve loans, while borrowers are more selective with the loan offers. It suggests that borrowers are aware of the higher exposure to lending terms that used to be prohibited by law.

5 Conclusion

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

The DID results provide evidence that deregulation does not affect the total home credit flow in Cleveland; however, there are more loans made with subprime interest rates or made by subprime lenders. More strikingly, 49% more loans end up with early foreclosures. After deregulation, both subprime and prime lenders make more loans that ended up in early foreclosures.

These results suggest that the Cleveland home mortgage regulation, without reducing the credit supply, implemented restrictions under which the originated loans would survive longer. The robustness checks and falsification tests provide evidence that the increased

¹⁶However, it is unlikely any systematic differences in the loan remedy procedure would generate the treatment effect found in my sample because as suggested by Richter et al. [2011], fewer than 0.1% of active loans were modified before December 2009.

early foreclosures are not driven by the definition of early foreclosure or the seasonal pattern of foreclosure. Moreover, the social and economic environment in which the loans are made and the shock from the financial crisis are not the reasons for the increased foreclosures after deregulation. Finally, if the Cleveland Ordinances were not challenged in court, the repeal would have had greater impact.

The significant impact of the deregulation in Cleveland hinges on the intensive restrictions and the wide coverage of the Cleveland Home Mortgage Ordinances. Those ordinances imposed intensive restrictions on home-purchase loans, which are never regulated by the state law but are the majority of the loans that ended up in foreclosure. If a 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 out of the 50 states have home mortgage regulations that cover home purchase loans. However, 9 city home mortgage regulations that implement stricter restrictions than the state home mortgage regulations have been preempted by state law.¹⁷

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

¹⁷<http://www.veritasaudits.com/blog/laws-and-regulations/sfpl/>

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A 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 Ordinances 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 these Ordinances.
- July 26, 2002, Judge Peggy Foley Jones denied AFSA's request for a preliminary injunction. As a result, Cleveland Home Mortgage Lending Ordinances 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 Ordinances to be in direct conflict with Ohio law and, therefore, unenforceable.
- The Eighth District Court of Appeals, ruled Cleveland's Ordinances 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 Ordinances unconstitutional (American Financial Services Association v. City of Cleveland, 112 Ohio St.3d 170, 2006-Ohio-6043).
- During the time of law suit, if a lawsuit was brought against a lender for violation of Cleveland's ordinance, it would have been thrown out by the time 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.”).

B Federal Preemption

Due to the dual banking system and the inter-state banking trend, the state anti-home mortgage regulations apply to various home mortgage creditors to different extent according to their identities. The federal creditors enjoy the preemption authorized by their federal regulatory agencies, which also provide the parity for state creditors regarding some specific mortgage transactions. Out-of-state banks, generally regulated by the home state, and are treated as national banks in the host state.

The Comptroller of the Currency (OCC), the National Credit Union Administration (NCUA), and the Director of the Office of Thrift Supervision (OTS) adopted regulations authorizing federally chartered thrifts to engage in alternative mortgage financing.¹⁸ In order to eliminate the discriminatory impact that those regulations have upon nonfederally chartered 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 the transactions are in conformity with the regulations issued by the Federal agencies. Under the law, the OCC promulgated regulations for providing alternative mortgage parity to state-chartered banks, the NCUA for state-chartered credit unions, and the OTS for both state-chartered thrifts and mortgage companies(nondepository institutions approved by HUD).

C 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 of the law regulating consumer real estate lending in Ohio. Those changes include:

¹⁸12 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.

- Creating 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).

Table 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.

Table 2: Impact of Deregulation on Foreclosures

VARIABLES	(1)	(2)	(3)	(4)	(5)
	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 only counts loans made by lenders that do not enjoy the 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. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$

Table 3: Impact of Deregulation on Loan Count and Amount

VARIABLES	(1) Baseline	(2) Non-preempted	(3) b=3 months	(4) Border
<i>Panel A: Loan Count</i>				
Cleveland×Repeal	0.02 (0.05)	0.03 (0.06)	0.01 (0.08)	-0.04 (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 (0.06)	-0.01 (0.06)	-0.01 (0.09)	-0.07 (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 the 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. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$

Table 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: Loan 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
<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. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$

Table 5: Falsification Test: Foreclosures

	(1)	(2)	(3)	(4)	(5)
	Nov. 2005 6 month	May 2006 6 month	Inner Subs 6 month	Pittsburgh 6 month	Pittsburgh 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 that are made in a tract-month combination and end up with early 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. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$

Table 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

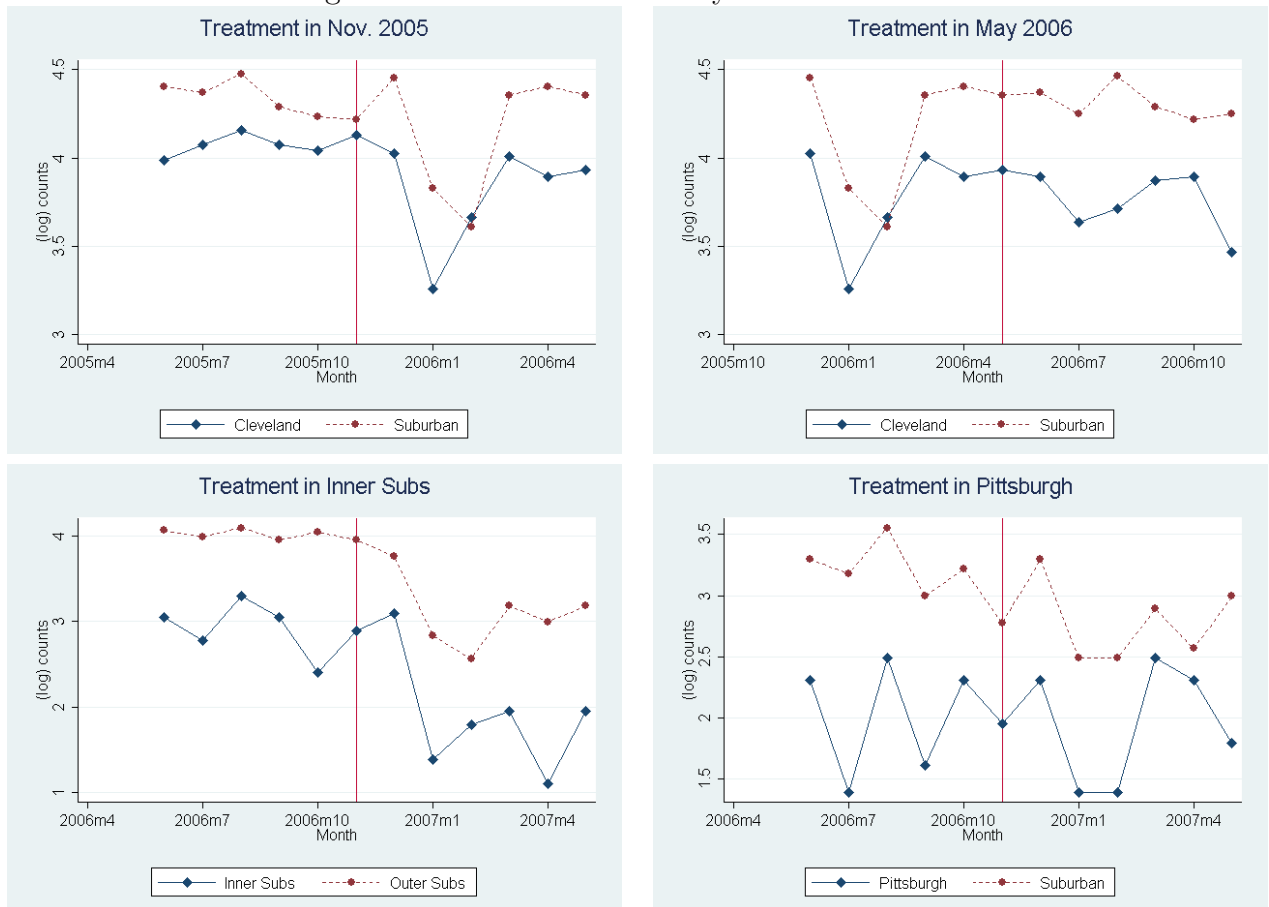
Notes: Column 1 and Column 2 report the coefficients estimated from a OLS model where the dependent variable is the sales price of each home sales transaction during June 2006 and May 2007. Column 2 controls for the overall housing condition, number of units built on the parcel, total usable square footage, year built, building style, external wall type, 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. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$

Figure 1: Time Trend: May 2006–June 2007



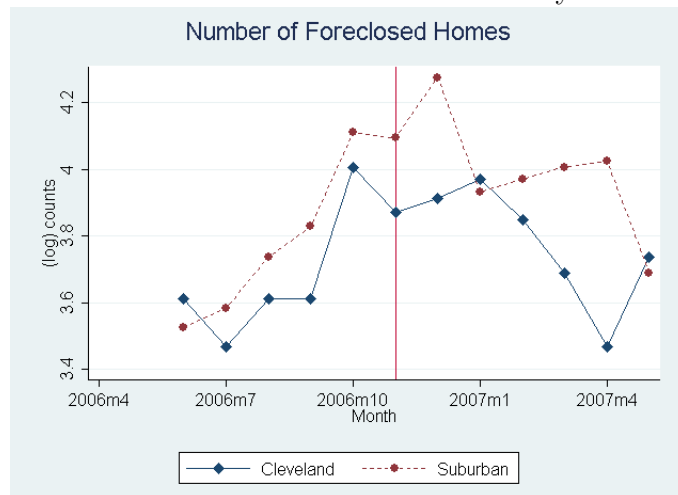
Notes: The sample includes home purchase loans for 1- to 4-family housing units secured by owner-occupied housing properties in Cuyahoga county.

Figure 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.

Figure 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.