THREE ESSAYS ON FINANCIAL INTERMEDIARIES REACTION TO CHANGING MARKET CONDITIONS

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ABSTRACT

This dissertation continues the tradition of identifying the effects of economic shocks to financial intermediaries. Its main contribution is to estimate the size of credit market disruptions in the form of government intervention, asset market crises, and competitive pressures, while using methods that are more novel and appropriate than those of previous work.

Chapter 1 examines the effect of the elimination of U.S. banking regulations, which are intended to expand the access of financial services within states and across state-lines, on entrepreneurship activity. It finds that there was increase in small business formation following the deregulation of interstate banking, but not intrastate banking. Results indicate allowing banks to lend and take deposits across state lines increases small business formation by up to 8%. There is a delayed impact following the passage of legislation indicating credit markets require time to adjust to the new regulatory environment. Heterogeneous effects exist across firm sizes in terms of economic impact magnitude and timing. The main contribution of the chapter is that examines the impact on entrepreneurship in separate periods after the initial passing and on subsets of small businesses.

Whereas Chapter 1 estimates the effect of a foreseen event, Chapter 2 focuses on the impact of unexpected housing crisis on financial intermediaries loan servicing decisions. As the housing market worsened mortgage lenders could not rely solely on

foreclosure processes to reduce losses on homes in default, rather many found the need to engage in modifying loan terms to allow borrowers to continue making mortgage payments. Modifications that increased the affordability of monthly payments were effective at halving the cumulative 36-month redefault rate for mortgages between 2008 and 2011. Findings indicate the improving economy and mortgage risk characteristics are not enough to explain the reduction in redefault. Instead, results find evidence of "learning –by-doing" i.e., servicers become better at targeting borrowers for modification and providing the appropriate payment relief over time. Voluntary government modification programs serve as guidelines for servicers to design and invest in their own modification processes. The impact of this learning by doing is evident before and after controlling for macroeconomic conditions, borrower characteristics, and loan terms. Previous studies do not effectively isolate the improvement in post-modification with an econometric model using a control group similar to this one. Furthermore, other studies consider only particular servicer subsets of mortgage modifications, such as private securitized, whereas the sample here considers all servicer types and payment reducing modifications. Ultimately, the results indicate mortgage modifications were an effective non-foreclosure alternative to keep homeowners in their homes and monthly payments flowing to mortgage servicers.

Chapter 3 examines the impact of changes in bank competition on bank capital in the United States. Allen et al. (2011) proposes excessive capital holdings, i.e., capital holdings above regulatory requirements, are attributable to market discipline arising from banks' asset side. Theory predicts competition incentivizes banks to hold higher levels of capital because this indicates a commitment to monitoring to encourage bank stability. I examine heterogeneous impacts of competition on capital over the business cycle and across bank size. Economic downturns usually bring significant changes to bank concentration, which can cause a different impact than during economic booms. Smaller banks can feel different competitive pressures than larger banks due to a focus on local lending activities. I have two main results. More intense competition is associated with higher bank capital ratios at all times (before, during, and after the financial crisis) for small, medium, and large banks. All banks see a larger impact during the crisis period compared to the pre- and post-crisis periods. The findings of this paper can have significant policy implications for the application of anti-trust regulation, since capital ratios are commonly used to restrain individual and systemic bank risk.

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I want to dedicate this work to my incredible loving parents. Thank you mom and dad for your unwavering faith in my infinite potential. You have given me the confidence that I can accomplish anything in life.

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CHAPTER 1. STARTING UP COMPETITION: BANK DEREGULATION AND SMALL BUSINESS

1.1. Introduction

Since the financial crisis of 2008-2009, there has been an increase in attention on the costs and benefits to an integrated and concentrated financial system. The ten largest banks hold nearly 77% of all banking assets (Banks run amok (2011)), which has helped these institutions earn the label “too-big-to-fail.” Furthermore, nearly all banks either directly (through their own subsidiaries) or indirectly (through borrowing or lending to other bank institutions) are involved in every financial market more than ever before (Loutskina and Strahan 2015). From the viewpoint of a bank borrower there is conflicting evidence whether increased bank concentration is beneficial.

Empirically it has been difficult to distinguish whether bank competition has an impact on (Berger et., al 2004) loan prices and credit availability. Theoretically, the vast majority of literature holds that competition increases credit availability and reduces borrowing costs (e.g. Carbo-Valverde et. al., 2009). However, there is a minority who believe that increased concentration can increase credit supply and decrease loan prices as a result of economies of scale (Peterson and Rajan 2005, McAllister and McManus 1993).

Between the 1970s and 1990s there was a rare opportunity to observe the effects of changes in bank concentration on idiosyncratic factors directly related to banks such as borrowers, as well as macroeconomic factors. State-level governments removed interstate and intrastate banking restrictions during this three-decade span.

Entrepreneurs rely heavily on funding via commercial banks. Black and Strahan (2002) demonstrate that nearly half of all small businesses rely on credit from a bank or Therefore, entrepreneurs are likely to be affected by changes to their local banking environment.

My analysis focuses on how interstate deregulation affects the formation of new small businesses. Specifically, I consider how allowing banks to cross state lines influences the number of firms with less than 50 employees within a state. The most precise way to study the effects of bank deregulation would be to analyze bank lending patterns at the individual bank level and borrower level. This would allow credit supply effects, and borrowing demand to be estimated. However, extensive data that tracks lending to small business at the state level is difficult to find.[1] This paper considers economic factors that would affect the demand and supply of small business borrowing at the state level given the current availability of data.

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[1] The National Survey of Small Business Finance has information on individual firms borrowing history, but data does not exist prior to 1993. Lending to small businesses (loans under $250,000) does exist in present day call reports, but not for the time period of analysis. Changes to small business lending at the national level is difficult to measure because the H.8 of The Federal Reserve, which tracks commercial bank trends, did not begin to publically track loan amount under $1,000,000 until 1997.

After controlling for state-level demographic and economic factors, I find that there is a positive and significant impact of allowing interstate banking on firm entrepreneurship. This effect remains significant and increases over time after deregulation likely because concentration and credit availability are slow to change. In addition to the direct effect of passing legislation permitting interstate banking, there is negative and significant interaction effect between deregulation and bank concentration in a state. Similar to the un-interacted effect of legislation, this effect persists in future periods, but remains negative. An interaction effect of this kind would indicate that states that pass legislation with high concentrations or those that increase concentration after passing legislation would be decreasing new business formation. I attribute these results to evidence that higher concentration bank markets have reduced lending and higher loan prices, which discourages entrepreneurs from pursuing new projects.

I estimate these effects across firm sizes. To my knowledge no other study has broken down entrepreneurship across this dimension. It is possible that some firms do not benefit from the increased competition because their population is excluded from the credit markets. I consider the age of the deregulation regulations in my estimations. No other study considers the effect of the deregulating each year after the legislation date to test for possible delays in legislative effects. These dynamic effects may vary by firm size wherein some firms are able to take advantage of changes in the credit supply more quickly.

The paper is organized in the following manner: Section 1.2 provides a review of relevant literature. Section 1.3 details the history of bank regulation. Section 1.4 outlines the empirical model. Section 1.5 covers the data utilized in the paper. Section 1.6 provides the analytical results. Section 1.7 reviews the robustness of the findings. Section 1.8 concludes.

1.2. Literature Review

The effect of interstate banking deregulation on areas of the economy outside the banking sector has been deeply explored issue.[2] Topics include employment in different industries, state GDP growth, and income inequality among others.

Many studies have used new corporations as a proxy for entrepreneurship. For instance, Black and Strahan (2002) find that allowing interstate banking increases entrepreneurship per capita. States with higher banking concentrations have fewer new corporations, supporting the conventional hypothesis that banks with market power limit the supply of credit to entrepreneurs seeking a loan. Furthermore, the rate of entrepreneurship per capita increases in states that remove intrastate banking restrictions (i.e., branching restrictions within a state). The interaction between banking concentration and intrastate branching deregulation has a positive effect on entrepreneurship per capita.

[2] See Black and Strahan (2002), Black and Strahan (2009), Jayaratne and Strahan(1996), and Beck et., al , (2010).

This indicates markets that are highly concentrated will have face more disruption from deregulation, therefore creating a larger impact on entrepreneurship than less concentrated markets. Bartik's (1989) empirical study also supports Black and Strahan's (2002) finding that fewer intrastate restrictions positively impacts new business formation.

Greater competition likely reduces the costs of providing credit on average. Fundamental market power economic theory predicts that increases in market openness and the number of competitors leads to a reduction in prices, to the benefit of borrowers. The evidence of Jayaratne and Strahan (1998) indicates declines in average loan prices of about 40 basis points following intrastate branching deregulation overall. Unfortunately, Jayaratne and Strahan (1998) do not isolate the impact of intrastate deregulation on small business loans. Jayaratne and Strahan (1998) find a non-statistically significant reduction in loan price following interstate deregulation, but limited controls in their regression leave doubt in the strength of this finding. There are likely important "soft factors" formed through relationships in allowing banks and other lenders to extend credit to potential borrowers. These relationships are not easily measurable, and this potential omitted factor bias is a limitation in any study of loan lending since banking relationships cannot be observed directly. If I were to assume small business loans followed the broader lending market trend of lower prices after deregulation found by Jayaratne and Strahan (1998), entrepreneurial activity ought to be enhanced by increased competition in banking as loan quantities increased.

Increased competition makes it easier for borrowers to switch lenders, which can reduce the incentive to commit to one lender and form a relationship. However, Boot and Thakor (2000) argue that competition increases the rewards for activities that allow lenders to distinguish themselves from other lenders, thus raising the incentive to invest in relationships.

Thus, I develop my first two hypotheses:

Hypothesis 1: There is a positive association between interstate banking and entrepreneurship within a state.

Hypothesis 2: There is a positive association between the interaction of interstate banking with banking concentration and entrepreneurship within a state.

Boustanifar (2014) examines the relationship between interstate and intrastate bank deregulation and state-level employment. The paper finds a significant positive impact from allowing intrastate branching and interstate banking on employment outcomes including wage growth and employment growth. Since these outcomes are measured as rates rather than levels, it does not account for a permanent increase associated with passing legislation to encourage bank competition. Figure 1-1 demonstrates the difference between a trend (3) and level change (2). Controlling for (3) exclusively would miss a permanent increase or decrease

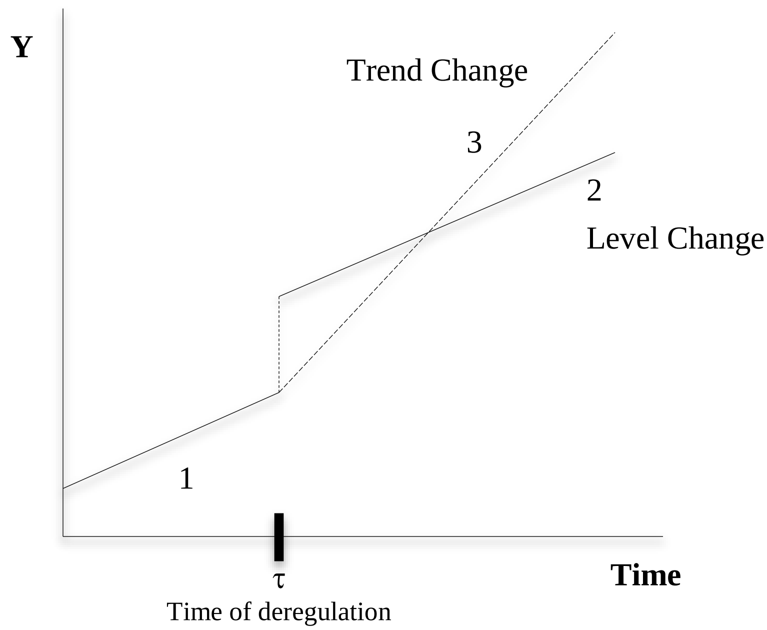

Figure 1-1. Level Change and Trend Change Comparison

(intercept change) in the dependent variable, Y, over time, and only account for the trend upwards or downwards (slope change) of the dependent variable over time. This distinction is noteworthy when Boustanifar (2014) tests whether new business formations may connect deregulation and improved employment outcomes. The rate of new business entry, rather than the levels of new business formation, is regressed upon the passing of banking deregulation legislation, The coefficient relating legislation to new business entry is insignificant, likely a result driven by a specification of rates , rather than levels, for the dependent variable. In my analysis I examine the impact of banking deregulation on both level and trend of small business formation.

In turn I develop my third hypothesis:

Hypothesis 3: There is a positive increase in the level entrepreneurship following interstate banking deregulation, but not in the rate of entrepreneurship.

1.3. Historical Background

Throughout the 1980s and early 1990s there was a slow progression to a banking system without restrictions on banking across state lines, known as an “open banking system”. The Douglas Amendment to the 1956 Bank Holding Company (BHC) Act caused a disjointed banking system across state lines, which allowed for the slow and staggered deregulation at the state-level of interstate banking. This amendment barred a BHC from acquiring banks outside the state where it was headquartered unless the target bank’s state permitted such acquisitions. Prior to the 1980s no state approved any proposed acquisitions, thus the amendment effectively blocked interstate banking. The first state to allow out-of-state acquisition was Maine in 1978 through a law that granted entry of BHCs into Maine if the state in which it was headquartered, allowed banks based in Maine the privilege of reciprocal entry. The first states to reciprocate were New York and Maine in 1982. Nearly all states except Hawaii passed laws allowing out-of-state banks entry. This state-by-state deregulation allows a researcher to isolate the impact of altering banking concentration on an economic variable within a state.

The fact that many banks had to wait to for dual state reciprocation made expansion across state lines difficult. The effects on economic activity within a state caused by legislation allowing entry of out-of-state banks might not be felt for years. To more precisely gauge the effect of banks entering a state, my analysis accounts for the actual year when another state reciprocated. For example, Alabama passed a reciprocal law allowing entry of out-of-state banks with several states including West Virginia in 1987. However, it was not until 1988 that West Virginia passed a similar law that completed the dual reciprocation needed for banks in either state to enter the other state. Numerous other state-pair examples exist. What makes this reciprocation matching process between states even more interesting is the fact that some states, such as Alaska and Arizona, passed laws that did not require any reciprocation from other states for out-of-state banks to enter. The full list of years when each state first reciprocated with at least one other state can be found in Table 1.1.

Table 1.1 Initial Year of Interstate Deregulation

State Abbreviation	Year Interstate Banking Permitted	State Abbreviation	Year Interstate Banking Permitted
AL	1987	OR	1986
AK	1982	PA	1986
AZ	1986	RI	1984
AR	1989	SC	1986
CA	1987	SD	1988
CO	1988	TN	1985
CT	1983	TX	1987
DE	1988	UT	1984
DC	1985	VT	1988
FL	1985	VA	1985
GA	1985	WA	1987
HI	1994	WV	1988
ID	1985	WI	1987
IL	1986	WY	1987
IN	1986		

IA	1991
KS	1992
KY	1985*
LA	1987
ME	1982
MD	1985
MA	1983
MI	1986
MN	1987*
MS	1988
MO	1986
MT	1993
NE	1990
NV	1985
NH	1987
NJ	1986
NM	1989
NY	1982
NC	1985
ND	1991
OH	1985
OK	1987

Notes: Year of deregulation of restrictions on geographical expansion, by state. * indicates the year differs from the year state passed legislation permitting out-of-state bank entry.

Other empirical studies do not account for the reciprocation requirement between states because they include an indicator for when the law was passed, but failed to account for when it was implemented. Black and Strahan (2002) and Boustanifar (2014) use a dummy indicating whether a state passed legislation permitting interstate banking. As a result, these prior studies may not accurately measure the effect of the law on economic variables within a state. I account for reciprocation by marking the legislation as active when at least one other state reciprocates. A few states have active years differing from the year of deregulation as seen in Table 1.1.

By the early 1990s there was recognition of the need for clear and unified rules guiding interstate banking at the national level. This desire became legislative reality after the passage of the Interstate Banking and Branching Efficiency Act of 1994 (IBBEA), which permitted BHCs to enter other states without permission and to operate branches across state lines. [3] Finally, in 1995 all states effectively allowed full entry from banks located anywhere in the United States.

1.4. Empirical Model

To investigate the impact of banking deregulation on entrepreneurship, I employ the following form:

$$\begin{aligned} &Entrepeneurship_{st} = \beta_0 + \beta_1 Deregulation_{state_{st}} + \\ &\beta_2 BankConcentration_{st} + \beta_3 Deregulation_{state_{st}} * \\ &BankConcentration_{st} + \beta_4 Deregulation_{intra_{st}} + \\ &\beta_5 Deregulation_{intra_{st}} * BankConcentration_{st} + X_{st}\beta + \delta_s + \gamma_t + \varepsilon_{st} \end{aligned} \quad (1)$$

where Entrepreneurship$_{st}$ equals the natural log of small business establishments

[3] In the case of merger and acquisition banks still required regulatory approval from their primary regulator such as the Federal Reserve, Office of the Comptroller of Currency (OCC), or Federal Deposit Insurance Corporation (FDIC). This approval only considered normal M&A guidelines without consideration for state regulatory approval.

with less than 50 employees per capita, in state s in quarter t.[4],[5] $Deregulation_{st}$ is a deregulation indicator which is equal to 1 for states without restrictions on entry via M&A and 0 otherwise. The interaction of $Deregulation_{st}$ and Bank $Concentration_{st}$ captures the dynamic relationship between changes in competition and deregulation. $Deregulation\_intra_{st}$ is a deregulation indicator, which is equal to 1 for states without restrictions on intrastate branching and 0 otherwise. The interaction of $Deregulation\_intra_{st}$ and Bank $Concentration_{st}$ captures the dynamic relationship between changes in competition and intrastate branching. In this specification, X_{st} contains time variant variables unique to states, which are demographic and economic factors. Demographic factors include proportion of state population in local MSAs, proportion of population that is female, proportion of the population with high school degree or some college, proportion of population with master's degree or more, population size, and population growth. Economic factors include annual GDP growth, real GDP in 2009 dollars, share of GDP in the following categories agriculture, mining, construction, manufacturing, transportation/ utilities, wholesale, retail, finance/insurance/ real estate, and services. The remaining variables δ_s and γ_t control for state and time fixed effects,

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[4] A conventional differences-in-differences approach does not work in my study because of the staggering of deregulation across many periods.

[5] I take the natural log of small business formations per capita as a proxy for entrepreneurship following Black and Strahan (2002) methodology. It uses natural log of new corporations per capita as a proxy for entrepreneurship.

respectively.[6] The coefficient of interest, β_1, captures the change in entrepreneurship following the policy change in the treatment state (states that deregulate) compared to the control group (states that did not deregulate).

Kroszner and Strahan (1999) find that the rise of technology promoting remote banking in all states creates pressure to deregulate interstate banking, and it is the differences of special interests' influence among states account for the staggered timing of deregulation. Additionally, Kroszner and Strahan (2011) and Boustanifar (2014) conclude that changes in economic conditions, including firm growth, lag deregulation. After considering this evidence, I conclude that the decision for a state to deregulate was exogenous to the amount entrepreneurship within that state.[7]

Equation (1) is estimated using fixed effects regression. I assume that the unobservable bank individual effects, δ_s, are correlated with the other independent variables. Such an assumption is reasonable because it is likely that individual bank effects, such as state culture, would be correlated with demographic and economic

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[6] Differences in tax rates and usury laws may impact incentives for entrepreneurial activity, but including the state effects should control for this variation (Hubbard and Gentry (1999) and Black and Strahan (2002)).

[7] A formal statistical check of this exogeniety is considered later in the form a granger causality test recommended by Angrist and Pischke (2009).

measures, such as education. A fixed effects estimation is implemented by using the within regression estimator to remove unobserved heterogeneity.

Correlation between a state's observations over time may be of concern. Not accounting for this possibility may provide biased estimates for the regression standard errors. I cluster standard errors by state because I assume observations across states are independent, but observations within the same state are correlated over time.

1.5. Data Sources

This paper estimates how bank competition within a state impacts the level of entrepreneurship, controlling for population and economic factors. The data is aggregated at the state level as a panel to take advantage of changes in interstate banking restrictions. The panel stretches from 1977 to 1994. Many of the data series relating to bank sector characteristics and demographics begin in 1977. The end of the panel was chosen to exclude observation years after the passage IBBEA in 1994 to avoid the possibility of misidentifying the effect of a permitting interstate banking at the state-level.

To measure the level of entrepreneurship within a state I use the number of business enterprises with less than 50 employees.[8] This measure is taken from state-level data from the Business Dynamic Statistics (BDS), which contains information collected

[8] Employment size is measured at the beginning of the year of observation

by the Census. It details establishment-level employment stocks and job flows, for continuing, entering, and exiting establishments, at annual frequency for the 1976 to the present period. [9,10] The BDS is a semi-aggregated publically available version of Census Bureau's Business Register (BR), which the primary source of information on the identity, location, employment, sales, payroll, and industry for 98% of all US businesses.

The main variable of interest is an indicator of a state's first forming a reciprocated relationship with another state to allow out-of-state bank entry. This type of exogenous change in competition is rare. Since deregulation at the state level was staggered across time, a panel model framework allow me to measure the dynamic impact of interstate banking on entrepreneurship, while controlling for time and state fixed effects. The reciprocity indicator is equal to one after a state effectively allows another state's banks to enter its borders (in most cases both states require dual reciprocation). The intrastate banking indicator is equal to one after a state permits banks to open additional branches within its borders. Dynamic indicators allow me to identify

[9] An "establishment" is a physical location. Establishment's information is updated continuously from tax records.

10 The firm-level data does contain detailed employment, location, industry classification, age, and entry/exit of the firm, but this data is highly confidential and difficult to access. Therefore, this analysis is limited to state-level data not separated by industry.

immediate and delayed effects of deregulating. Furthermore, there may be a change in magnitude every year since deregulation as the policy change takes full effect.

An indicator for the passage of interstate banking deregulation will capture the average long run effect of opening to another state, but, it fails to control for the level of competition within the state. A similar argument holds for intrastate branching deregulation. Passing interstate (intrastate) banking deregulation promotes competition between banks. Perhaps the effects of branching deregulation depend on the level of concentration in local banking markets. Markets with many banks and less competitive behavior may be less affected by lower barriers to entry following branching reform. To capture the interaction between banking competition and deregulation of interstate banking restrictions, the reciprocity indicator is interacted with the Herfindahl-Hirschmann Index (HHI) within the state. Concentration within a state is measured as the HHI of deposits. This measure is derived from FDIC's Summary of Deposits (SOD) is created from HHI concentrations at the Metropolitan Statistical Area (MSA) level, similar to Black and Strahan (2002) and Cetorelli and Strahan (2006).[11] First, I sum the squared share of deposits for each banking company in each MSA of a state. Next, each MSA concentration measure is averaged across all MSAs within the state, weighted by total deposits share in each MSA. Deposit concentration proxies how much market

[11] For more information on Call Reports, visit: http://www2.fdic.gov/Call_TFR_Rpts/inform.asp

power a bank has over the local loan market. To account for the effect of concentration in states without bank deregulation an un-interacted measure of asset HHI is will be used in the analysis.

Macroeconomic explanatory variables used in the investigation were collected from Bureau of Economic Analysis (BEA). These variables include annual population, GDP, and GDP share by industry at the state level.[12] States with larger populations are likely to have a larger demand for goods and services, which may mean more businesses found within the state. State GDP is another control that should be considered because states with higher production levels are more likely to have more firms. Certain industries may be characterized as having more small businesses, such as the service sector. I control for these possible industry differences by including measures of each major industry's GDP share. These variables are lagged one year to reduce reverse causality bias.

Demographic variables are constructed from the annual Current Population Survey (CPS) March supplement. Each variable is aggregated to the state level. Similar

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[12] GDP is reported in real 2009 US dollars

to the economic variables these variables are lagged one year to reduce reverse causality bias. Summary statistics for the data can be found in Table 1.2.

Table 1.2. Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Establishments Count 1-49 Employees	77,655.69	85,333.74	6,229.00	527,101.00
Establishments Count 1-4 Employees	43,175.43	48,156.31	3,485.00	284,024.00
Establishments Count 5-9 Employees	17,093.59	18,450.97	1,412.00	116,796.00
Establishments Count 10-19 Employees	10,270.04	11,160.27	802.00	73,458.00
Establishments Count 20-49 Employees	7,116.63	7,783.28	530.00	52,823.00
Establishment Entry Count 1-49 Employees	11,434.22	13,322.54	1,105.00	93,593.00
Establishment Entry Count 1-4 Employees	9,479.55	11,015.91	909.00	75,689.00
Establishment Entry Count 5-9 Employees	1,090.31	1,312.94	84.00	10,186.00
Establishment Entry Count 10-19 Employees	507.10	631.64	37.00	5,468.00
Establishment Entry Count 20-49 Employees	357.25	451.78	14.00	3,853.00
Establishment Entry Rate 1-49 Employees	39.26	8.96	24.60	92.60
Establishment Entry Rate 1-4 Employees	22.58	4.16	14.90	48.70
Establishment Entry Rate 4-9 Employees	6.52	1.90	3.50	18.20
Establishment Entry Rate 10-19 Employees	5.04	1.89	2.20	15.20
Establishment Entry Rate 20-49 Employees	5.12	2.38	1.40	15.60
MSA HHI	1,948.39	732.57	638.73	5,519.63
GDP in millions of 2009 dollars	150.65	182.50	10.27	1,177.89
GDP Drowth (%)	-3.15	-2.61	10.82	-11.50
Agriculture, Forestry, and Farming GDP Share (%)	2.96	2.96	0.01	19.19
Mining GDP Share (%)	4.10	8.09	0.00	49.66
Construction GDP Share (%)	4.60	1.52	0.89	29.81
Manufacturing GDP Share (%)	18.57	8.27	1.97	38.27
Transportation GDP Share (%)	9.28	1.97	4.05	24.52
Wholesale GDP Share (%)	6.35	1.49	1.28	9.90
Retail GDP Share (%)	9.13	1.37	3.04	11.93

Table 1.2 Continued

Variable	Mean	Std. Dev.	Min	Max
Finance, Insurance, and Real Estate GDP Share (%)	15.18	4.30	5.61	37.45
Services GDP Share (%)	15.22	4.68	5.15	35.10
Population Growth (%)	0.99	1.28	-3.77	8.63
Population	4,672,547.00	5,053,959.00	396,952.00	31,300,000.00

Notes: The panel spans 1977-1994. All data are annual.

1.6. Empirical Results

Initial regression findings are given in Table 1.3. In specification (1) the natural log of number of establishments per capita is regressed against two simple indicators for the year when intrastate or interstate legislation passed without controls. The deregulation coefficient is significant and positive, suggesting that deregulation is predictive of establishments per capita. Since the dependent variable is log transformed, β_1=0.0953 implies permitting interstate banking implies interstate deregulation raises establishments per capita by 10%, a large increase. This finding is consistent with Black and Strahan (2002) suggesting that increased competition from opening up banking markets leads to a significant increase in entrepreneurship. In their study permitting interstate banking increases incorporations per capita by 11.1%, a similar magnitude to β_1. The intrastate deregulation coefficient, β_4, is insignificant, indicating intrastate branching deregulation has no significant effect on the number of small establishments. This is consistent with Cetorelli and Strahan (2006), which finds a negative and insignificant impact of intrastate banking deregulation on entrepreneurship. If bank concentration is added to the regression, see specification (2), both deregulation coefficients are reduced somewhat, β_1=0.0769 and β_4=-0.0463. However, even after controlling for bank concentration the positive effect of allowing interstate banking remains statistically and economically significant.

Table 1.3 Regression Results

Dependent variable	ln(Establishments Count 1-49 Employees per Capita)			
	(1)	(2)	(3)	(4)
Interstate Deregulation	0.09531***	0.07692***	0.06978***	0.05660***
	(0.01883)	(0.01523)	(0.01114)	(0.01085)
Intrastate Deregulation	-0.05937	-0.04632	-0.03534	-0.02999
	(0.04581)	(0.04079)	(0.03329)	(0.02566)
Bank HHI	-	-0.00011	-0.00004	-0.00004
		(0.00009)	(0.00003)	(0.00003)
Interstate Deregulation*Bank HHI	-	-	-0.00003***	-0.00002***
			(0.00003)	(0.00002)
Intrastate Deregulation*Bank HHI	-	-	0.00001	0.00001
			(0.00001)	(0.00001)
Demographic controls	No	No	No	Yes
Economic controls	No	No	No	Yes
State fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
R-squared	0.6790	0.6953	0.7057	0.8154
N	918	918	918	918

Notes: Equation (1) is estimated with fixed effects regression over the 1977-1994 panel the within estimator to account for unobservable state effects (i.e., fixed effects). Time fixed effects are included in the specification, but omitted in the results to save space. The standard errors (shown in parentheses) are clustered at the state level to account for clustering. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

I estimate the full equation (1) in Table 1.3's specification (3) and (4) with the inclusion of HHI interacted with both deregulation indicators. We see that the interaction between asset HHI and interstate banking deregulation is significantly negative with and without additional demographic and economic controls.[13] This result suggests if a state remains more highly concentrated it will have fewer small businesses per capita than a state that remains less concentrated. According to the interacted interstate/HHI coefficient, β_2, a one standard deviation increase in HHI decreases entrepreneurship per capita by 2%. The magnitude of the coefficient on interstate deregulation in specification (3), β_1, indicates legalizing interstate banking leads to a 7% gain in establishments per capita. In the context of β_2's impact, β_3's impact on entrepreneurship is rather large. This is evidence of increased bank competition proving beneficial to small business borrowers forming new enterprises. After controlling for economic and demographic factors in the specification, see specification (4), the impact of bank concentration and interstate deregulation interacted remains economically significant; a 1.5% decrease in establishments per capita following a one standard deviation increase in bank concentration. The intrastate branching deregulation coefficient remains insignificant across all specifications, which indicates that intrastate branching insignificance is robust.

[13] Black and Strahan (2002) interact intrastate deregulation with bank concentration and find a positive and significant effect on entrepreneurship. This suggests lowering barriers within a state has a different effect than lowering barriers across states given the same level concentration of state.

1.7. Robustness

To correctly use my specification approach the parallel trends assumption must hold i.e., the before–after difference for the untreated states measuring what would have been the before–after difference for the treated states in the absence of interstate or intrastate deregulation. A test for this assumption involves whether the treatment effect holds after controlling for individual state time trends. I estimate a specification similar to Table 1.3 specification (3) with individual time trends. The sign and significance on all the deregulation variables remains the same providing evidence the parallel trends assumption holds for the specification.

To test for possible endogeniety caused by possible reverse causality between the number of establishments and the passing of interstate deregulation laws in a state I implement leading and lagging effects of the legislation into the specification. Angrist and Pischke (2009) describe how to perform a kind of Granger test on a difference-in-difference specification. If there was evidence that legislation causes significant changes prior to its adoption, the results of this analysis would be simple pre-trends incorrectly assigned to legislation. Conventionally, I would expect causes to occur prior to effects, but it is possible establishments could react to legislation they expect to pass in the near future. Table 4 outlines a modification of (1) with additional leads and lags controlling for year effects before and after bank deregulations.

Table 1.4 Regression Results with Dynamic Interstate Deregulation Effects

Dependent variable	ln(Establishments Count 1-49 Employees per Capita)
	(1)
Deregulation(t-3)	-0.02124
	(0.02055)
Deregulation(t-2)	-0.01813
	(0.01800)
Deregulation(t-1)	-0.01000
	(0.08899)
Deregulation(t)	-0.00500
	(0.00486)
Deregulation(t+1)	0.00297
	(0.00293)
Deregulation(t+2)	0.01982**
	(0.01003)
Deregulation(t+3)	0.03244***
	(0.00951)
Deregulation(t+4)	0.04216***
	(0.01593)
Deregulation(t+5)	0.05847***
	(0.01495)
Deregulation(t+6 forward)	0.06767***
	(0.01862)
Demographic controls	Yes
Economic controls	Yes
State fixed effects	Yes
Time fixed effects	Yes
R-squared	0.8596
N	918

Notes: I estimate Equation (1) with additional leads and lags with fixed effects regression over the 1977-1994 panel using the within estimator to account for unobservable state effects (i.e., fixed effects). Time fixed effects and controls from Equation (1) are included in the specification, but omitted in the results to save space. The standard errors (shown in parentheses) are clustered at the state level to account for clustering. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Each of the coefficients on the terms controlling for years prior to the legislation is insignificant, therefore the likelihood of the endogeniety of deregulation is minimal. Figure 1-2 clearly outlines the change in coefficient magnitude each year pre and post interstate bank deregulation.

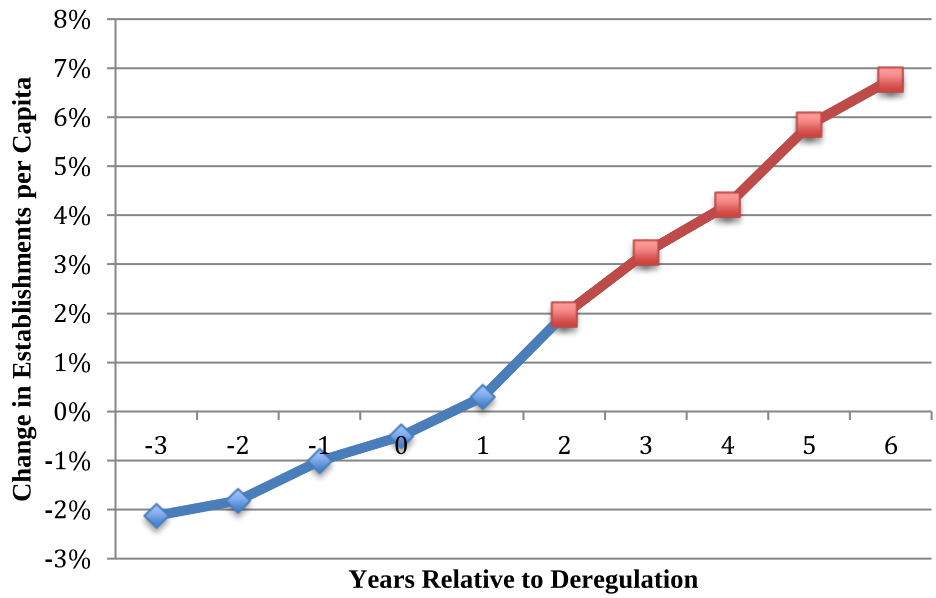

Figure 1-2. Dynamic Impact of Interstate Bank Deregulation

This figure shows the dynamic impact of interstate bank reform on establishment per capita growth. The figure plots the coefficients from Table 1.. I examine up to 3 years prior to deregulation and plot the yearly effect up to 6 or more years following deregulation. The blue indicates insignificant coefficients and red indicates significant coefficients.

The dynamic effects of passing legislation over time can be estimated by including terms controlling for years after deregulation legislation was passed in the regression. Indeed the results for this specification can be found in Table 1.. Each set of legislation year indicator coefficients is positive, but does not become significant until the first year after legislation has passed and beyond. Therefore, there is a delay in the effect of legislation of the creation of enterprises likely because it takes time for bank mergers and acquisitions, a common method for bank expansion, to be planned, finalized, and approved. Banks may be uncertain about the effects of legislation and do not change lending behavior until the effects on the small business credit market become clearer. Furthermore, the effect increases over time as the legislation ages. This increase in magnitude with legislative age is confirmed by standardizing the coefficients and observing an ordinal change in the coefficient sizes.[14] One year after interstate banking legislation passes there is 0.05% increase in entrepreneurship, however this effect increase significantly over time to 8.5% after the legislation has been in existence for at least six years.

[14] Standardized coefficients or beta coefficients are the estimates resulting from a regression analysis that has been transformed so that the variances of dependent and independent variables are each equal to 1. This transformation allows coefficients to be directly compared in terms of magnitude because the units are removed, the coefficients' units are each a standard deviation.

Table 1.5 Regression Results with Bank Concentration Effects

Dependent variable	ln(Establishments Count 1-49 Employees per Capita)	
	(1)	(2)
Deregulation(t)	-0.00201	0.00890*
	(0.00195)	(0.00455)
Deregulation(t+1)	0.00051*	0.01485**
	(0.00037)	(0.00059)
Deregulation(t+2)	0.01482**	0.02078***
	(0.00928)	(0.00462)
Deregulation(t+3)	0.0295***	0.03634***
	(0.00677)	(0.00991)
Deregulation(t+4)	0.0478***	0.04400***
	(0.00905)	(0.01063)
Deregulation(t+5)	0.0610***	0.05069***
	(0.01006)	(0.00762)
Deregulation(t+6 forward)	0.0815***	0.07603***
	(0.00264)	(0.00233)
Bank HHI	-0.00017	-0.00015
	(0.00016)	(0.00016)
Deregulation(t)*Bank HHI	-	0.00003***
		(0.00000)
Deregulation(t+1)*Bank HHI	-	0.00002***
		(0.00000)
Deregulation(t+2)*Bank HHI	-	-0.00002***
		(0.00000)
Deregulation(t+3)*Bank HHI	-	-0.00004***
		(0.00000)
Deregulation(t+4)*Bank HHI	-	-0.00005***
		(0.00000)
Deregulation(t+5)*Bank HHI	-	-0.00006***
		(0.00001)
Deregulation(t+6 forward)*Bank HHI	-	-0.00007***
		(0.00000)

Table 1.5 Continued

	(1)	(2)
Intra-state Deregulation	-0.04341	-0.03235
	(0.03899)	(0.00315)
Intra-state Deregulation*Bank HHI	-	0.00001
		(0.00001)
Demographic controls	Yes	Yes
Economic controls	Yes	Yes
State fixed effects	Yes	Yes
Time fixed effects	Yes	Yes
R-squared	0.8822	0.8901
N	918	919

Notes: I estimate Equation (1) with additional lags with fixed effects regression over the 1977-1994 panel using the within estimator to account for unobservable state effects (i.e., fixed effects). Time fixed effects and controls from Equation (1) are included in the specification, but omitted in the results to save space. The standard errors (shown in parentheses) are clustered at the state level to account for clustering. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

To ensure the fixed effects estimation technique is appropriate for the data well I implement a Hausman (1978) specification test. This statistical test indicates whether a random effects model or fixed effects model is more appropriate to represent the error variance of the regression estimation. Both will control for unobserved time-invariant heterogeneity, but one may provide more consistent coefficient estimates. After running the Hausman test, the t-statistic indicates that I can strongly reject at the 1% confidence level that random effects would provide a less biased estimate than a fixed effects model.

After reviewing regression results in Table 1., I conclude that Hypothesis 1, there is a positive association between interstate banking and entrepreneurship within a state, cannot be rejected. The sign of the legislation indicators are positive and statistically and economically significant. The increases in interstate deregulation coefficients magnitude over time indicates that the full effect of the legislation requires several years to mature. It likely takes several months or years for a bank in one state to create a plan to enter another state and act on the plan. Furthermore, once a bank enters into the new market space it will take time for the credit markets to reach new equilibriums with higher credit availability and lower loans prices. Additionally, incumbent banks within a state may be uncertain about the effects of the deregulation on small business credit markets, creating delays in the effects of legislation. On the demand side small businesses likely require several periods to react to the new credit markets and form new establishments. All of these factors taken together are possible sources of the increasing magnitude of coefficients observed in the model.

I reject Hypothesis 2, there is a positive association between the interaction of interstate banking with banking concentration and entrepreneurship within a state, since interaction term between interstate deregulation and HHI is negative and significant in specifications (3) and (4) in Table 1.3. This follows the logic that states with higher concentrations may harm local entrepreneurs by not reducing loan prices and increasing credit availability in comparison to less highly concentrated states. Over time the negative impact grows (standardized coefficients confirm this finding). My finding is inconsistent with Black and Strahan (2002), which finds a positive association. However, specification (2) in Table 1. provides alternative evidence; at the time of deregulation and one year later there is a positive association with higher HHI and entrepreneurship per capita, suggesting an increase of 1.5% in entrepreneurship the year after interstate banking is permitted. After interstate banking is permitted for two years the association between higher HHI and entrepreneurship per capita becomes negative. The change in sign after two years is conflicting evidence. This puzzling explanation can be reconciled if I assume some out-of-state banks were able to quickly take advantage of the newly opened credit markets in densely concentrated states. Entering a highly competitive state would be more disruptive than entering a state with less competition. Borrowers who had been previously unable to get any credit or at very high cost prior to the passing of the legislation in highly concentrated states could find ripe opportunities for newly available and potentially more affordable loans. If this theory is correct, the effect of a state remaining highly concentrated would begin to have negatively impact once these ready-to-go banks have completed their entrance into the market immediately following the deregulation. The negative impact is a 5% decrease in entrepreneurship per capita for a one standard deviation increase in HHI 5 years after permitting banking across state lines. Therefore, my results are consistent with Black and Strahan (2002) in the initial periods after interstate deregulation.

Since I find a positive relationship between deregulation of interstate banking and the level of entrepreneurship in a state, I test whether the interstate deregulation has a similar impact on the rate of entrepreneurship in a state. In Table 1. regress the entry rate of new enterprises on all of the variables in Equation (1). I find the bank

Table 1.6 Entry Rate Regression Results

Dependent variable	Establishments Entry Rate 1-49 Employees
	(1)
Inter-state Deregulation	0.25889
	(0.24167)
Intra-state Deregulation	-0.45959
	(0.42311)
Bank HHI	-0.00022
	(0.00021)
Inter-state Deregulation*Bank HHI	0.00019
	(0.00017)
Intra-state Deregulation*Bank HHI	-0.00002
	(0.00001)
Demographic controls	Yes
Economic controls	Yes

Table 1.6 Continued

State fixed effects	Yes
Time fixed effects	Yes
R-squared	0.7202
N	918

Notes: I estimate Equation (1) with additional lags with fixed effects regression over the 1977-1994 panel using the within estimator to account for unobservable state effects (i.e., fixed effects). Time fixed effects and controls from Equation (1) are included in the specification, but omitted in the results to save space. The standard errors (shown in parentheses) are clustered at the state level to account for clustering. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

deregulation coefficients and bank concentration terms are insignificant regardless of year after the passage of the interstate legislation.[15] I conclude that the rate of new small business formation cannot be significantly predicted by deregulation, whereas the level of new small business can be significantly predicted. Therefore, I reject Hypothesis 3, there is a positive increase in the level entrepreneurship following interstate banking deregulation, but not in the rate of entrepreneurship. The rejection of this hypothesis does not speak to any failure on the part of competition to encourage small business formation. Rather it simply indicates that competition altering the supply may have a constant effect over the long-term and alternative factors encourage growth rate changes.

The final segment of analysis is whether the above conclusions hold across small business size. This is an important consideration because some firm sizes may benefit

[15] This result is consistent with both Boustafinar (2014) and Strahan (2003) who find no impact of interstate deregulation on the rates of entrepreneurship.

from deregulation while others do not. For there to be a heterogeneous effect across firm sizes each firm size would be considered as in a different credit market, which may or may not be impacted by increased competition after deregulation. Table 6 provides strong evidence that there are separate effects by firm size from deregulation. Each size category supports the hypothesis conclusions made for the aggregate sample, however there is a delayed interstate deregulation impact for smaller firms (three years), whereas the effect occurs more immediately for larger firms. The economic impact on entrepreneurship per capita differs across firm size i.e., firms with 1-4 employees increase by 3% and firms with 20-49 employees increase by 8%. The effect of an increase in standard deviation in HHI also has a heterogeneous impact on entrepreneurship per capita; a 2.2% decrease for firms with 1-4 employees and a 6% decrease for firms with 20-49 employees.

Table 1.7 Regression Results with Bank Concentration Effects by Establishment Size

Dependent variable	ln(Establishments Count 1-4 Employees per Capita)	ln(Establishments Count 5-9 Employees per Capita)	ln(Establishments Count 10-19 Employees per Capita)	ln(Establishments Count 20-49 Employees per Capita)
	(1)	(2)	(3)	(4)
Deregulation(t)	-0.00201	-0.00101	0.01786***	0.01198***
	(0.00188)	(0.00112)	(0.00045)	(0.00945)
Deregulation(t+1)	0.00000	0.00000	0.02072***	0.02174***
	(0.00023)	(0.00011)	(0.00770)	(0.00633)
Deregulation(t+2)	0.00198	0.00899	0.03148***	0.03343***
	(0.00266)	(0.00923)	(0.00841)	(0.00899)
Deregulation(t+3)	0.01486**	0.01784*	0.03824***	0.04112***
	(0.00511)	(0.00124)	(0.00671)	(0.00816)
Deregulation(t+4)	0.02071***	0.02858**	0.04303***	0.06291***
	(0.00364)	(0.01509)	(0.00927)	(0.00957)
Deregulation(t+5)	0.02562***	0.03533***	0.05069***	0.06954***
	(0.00941)	(0.00943)	(0.01186)	(0.00772)
Deregulation(t+6 forward)	0.02951***	0.03927***	0.05820***	0.07694***
	(0.00021)	(0.00674)	(0.00804)	(0.00144)
Bank HHI	0.00020	0.00005	0.00003	0.00005
	(0.00195)	(0.00005)	(0.00004)	(0.00005)

Table 1.7 Continued

Deregulation(t)*Bank HHI	0.00253	0.00000	0.00002***	0.00003***
	(0.00195)	(0.00024)	(0.00000)	(0.00000)
Deregulation(t+1)*Bank HHI	0.00268	0.00000	0.00001**	0.00002***
	(0.00195)	(0.00019)	(0.00000)	(0.00000)
Deregulation(t+2)*Bank HHI	-0.00001	-0.00001	-0.00001***	-0.00004***
	(0.00002)	(0.00001)	(0.00000)	(0.00001)
Deregulation(t+3)*Bank HHI	-0.00001**	-0.00001**	-0.00001***	-0.00005***
	(0.00000)	(0.00000)	(0.00000)	(0.00001)
Deregulation(t+4)*Bank HHI	-0.00002***	-0.00002**	-0.00002***	-0.00005***
	(0.00000)	(0.00001)	(0.00000)	(0.00000)
Deregulation(t+5)*Bank HHI	-0.00002***	-0.00002***	-0.00004**	-0.00008***
	(0.00000)	(0.00000)	(0.00002)	(0.00000)
Deregulation(t+6 forward)*Bank HHI	-0.00003***	-0.00003***	-0.00005***	-0.00008***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Intra-state Deregulation	-0.01113	-0.01214	-0.01153	-0.01312
	(0.01389)	(0.00144)	(0.001501)	(0.01688)
Intra-state Deregulation*Bank HHI	0.00001	0.00002	0.00001	0.00004
	(0.00001)	(0.00002)	(0.00002)	(0.00004)
Demographic controls	Yes	Yes	Yes	Yes
Economic controls	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes

Notes: I estimate Equation (1) with additional lags using fixed effects regression over the 1977-1994 panel using the within estimator to account for unobservable state effects (i.e., fixed effects). Time fixed effects and controls from Equation (1) are included in the specification, but omitted in the results to save space. The standard errors (shown in parentheses) are clustered at the state level to account for clustering. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

The distinction of timing and magnitude across firm size becomes starker when each set of coefficients is plotted against one another in Figure 1-3. It becomes clear that

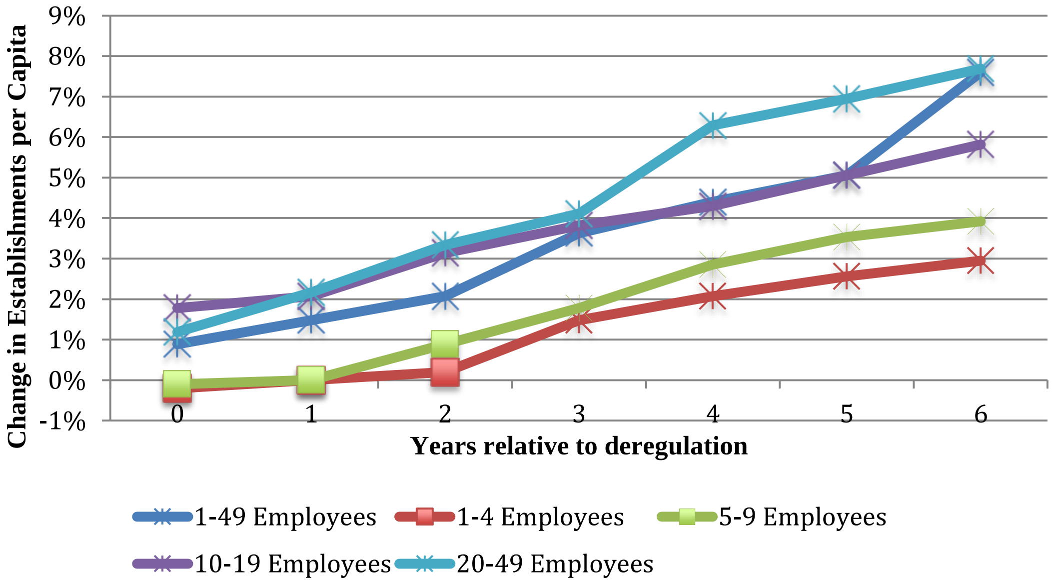

Figure 1-3. Dynamic Impact of Interstate Bank Deregulation by Enterprise Size

Figure 1-3 shows the dynamic impact of interstate bank reform on establishment per capita growth. The figure plots the coefficients from Table 1. specification (2) and Table 1.. I plot from the year of deregulation to up to 6 or more years following deregulation. The square markers indicate insignificant coefficients and the stars indicate significant coefficients.

smaller establishments receive smaller benefits with a delay following interstate banking deregulation than larger establishments. The competitive pressures introduced by deregulation likely differ across firm size due to a preference for less credit risk from new bank entrants. Smaller firms are likely newer and their prospects less certain than larger firms.

1.8. Conclusion

The directional impact of increased competition on business formation in the banking sector is full of mixed evidence. In my analysis I find empirical evidence in support of conventional market power theory, which finds increases in interstate competition provide incentives to promote additional small business activity. The evidence for a significant impact of intrastate branching deregulation is insignificant. My results holds both dynamically and across firm size even when market concentration is controlled for in the analysis. This paper supports the traditional economic viewpoint that competition leads to increased supply, in this case credit, which promotes additional equilibrium quantity to be consumed in the market

CHAPTER 2. SELF CURES AND LOAN MODIFICATIONS: A COMPARATIVE ANALYSIS OF REDEFAULT RISK CONSEQUENT TO THE MORTGAGE CRISIS

2.1. Introduction

As the housing market downturn that began in 2007 worsened through 2008 and 2009, mortgage lenders experienced an unprecedented surge of defaults. Lenders (or loan servicers acting on behalf of the lenders) can choose among various strategies for resolving a defaulted mortgage, with the objective of maximizing the private, present value of recovery amount net of recovery costs. The most common resolution strategy is a legal foreclosure process, whereby the lender takes possession of the property and puts it up for sale. Often, however, lenders pursue alternatives to foreclosure that may provide a shorter path to property liquidation including deed-in-lieu of foreclosure, short sale, or third-party sale.

Alternatively, the lender may modify the loan terms, such as reduce the interest rate, extend the maturity date of the loan, or reduce the principal balance, to facilitate return by the borrower to regularly scheduled payments. Mortgage modifications had been relatively uncommon prior to the mortgage crisis, but during the crisis their use increased substantially, spurred by foreclosure process delays and associated costs, and by government programs encouraging modification activity.

Our study examines redefault rates of loans selected for modification during 2008-2011, using similarly situated, self-curing borrowers as a control group. We find for the earlier (2008) cohorts wide gaps in redefault frequency between mortgages that receive loan modifications and their comparison groups. For example, the three-year cumulative redefault rate on modifications of privately securitized, fixed-rate mortgages was about double that of the comparison group of self-cures (76 percent versus 38 percent.) Over time, however, these gaps shrink substantially; for modifications and self-cures occurring in 2011, we observe essentially no difference in the three-year cumulative default rates in the case of conventional mortgages. We observe the same improvement in repayment performance for modified, conventional loans whether they are privately securitized; agency securitized; or bank portfolio loans. Improvement in repayment performance of FHA- and VA-insured loans is more modest; such that the gap relative to self-cures is reduced but not eliminated.

Thus, it appears that early on in the crisis, many servicers had limited experience with loan modification, consistent with observations set forth in Office of the Comptroller of the Currency (2011). Over the course of the downturn, as modification activity increased, servicers seem to have engaged in "learning-by-doing" that resulted in better performing loan modifications. That is, they became more adept at screening borrowers for modification eligibility; selecting modification terms best suited to a given situation; and offering other forms of support, such as financial counseling, to reduce redefault risk.

Moreover, we find over the course of the downturn, the survival curves of modified loans became less concave and more closely aligned with those of self-cures. Early on, most borrowers receiving a loan modification quickly redefaulted, which suggests that the modified terms provided a short-lived benefit and did not fundamentally affect the borrower's ability or willingness to repay the loan. Over time, the behavior of borrowers receiving modifications more closely resembled that of self-curing borrowers, suggesting that for many borrowers the loan modification terms facilitated their own efforts to get back on track.

The analysis compares the redefault behavior of borrowers who have received modifications to matched borrowers (with similar characteristics) who have self-cured, as a means of controlling for the influence of the changing economic environment and delinquent borrower population. The matching is based on specific key risk characteristics, including mortgage product category; geographic (state) location of the property; delinquency status (days past due); and date (year and quarter) of modification or cure, and also on propensity scores.[16] Both groups of borrowers (both modification and self-cured) experience initial delinquency, become current, and are at risk of redefault; what distinguishes them is that self-cures did not receive payment-reducing changes in loan terms to facilitate their remaining current. If default rates declined to a

[16] The propensity score is estimated for each loan in the sample, using models described in Section V.

similar degree for matched modified and self-cured loans, then the evolving economic conditions and delinquent borrower population were likely driving the change. We find, however, that modified loan redefault rates fell much more quickly than self-cure redefault rates, and attribute this phenomenon to the "learning-by-doing" process.

The paper is proceeds as follows. Section 2.2 discusses existing literature related to the motivation of modification as a foreclosure alternative and those related to the impact of modification on loan performance after changing loan terms. Section 2.3 provides some institutional background for the increase in modification and evolution of modification terms since the onset of the financial crisis in 2008. Section 2.4 describes the data sources and our approach to identifying modifications. Section 2.5 compares cumulative redefault rate between modifications and self-cures by year of modification or cure, after grouping loans within each sample using propensity score matching methods. Section 2.6 builds on this analysis using logistic regression models that predict redefault within 36 months of cure and control for economic trends, modification concessions, and loan and borrower risk characteristics. Concluding remarks and policy implications are presented in Section 2.7.

2.2. Literature Review

The literature on loan modification roughly divides into two strands. The first strand of research investigates factors on the borrower or servicer side that are important in determining which delinquent loans receive a modification, while the second focuses on the repayment performance outcomes. Our paper belongs to the latter strand. We quantify the likelihood that a borrower receiving a loan modification will redefault, relative to the redefault likelihood of a borrower who has "self-cured", and examine the change in comparative performance during 2008 (prior to the expansion of mortgage modification activity) through 2011 (after mortgage modification activity had peaked).

While a few previous studies have investigated the evolving performance of loan modifications, none has conducted extensive investigation as we do here. Moreover, no other studies have systematically compared the delinquency performance of modifications to that of self-cured loans – we are the first to use matched self-cures as a control group.

A lender or servicer's decision on whether (and if so, how) to modify a delinquent loan instead of pursuing foreclosure or other alternatives may depend, in part, on the factors that gave rise to the delinquency. The causes of mortgage delinquency and default can be segmented into *ability* versus *willingness* (or incentive) to pay (Haughwout, Okah, and Tracy 2009). Ability to pay relates to borrowers having adequate cash flow or liquidity to make a monthly mortgage payment. Incentive to pay can be described as the perceived financial payoff from maintaining ownership of the home; the borrower's current loan-to-value (CLTV) ratio is a key determinant of the overall financial payoff from maintaining ownership of the property and, hence, higher CLTV is associated with smaller incentive to pay. Past credit payment history (e.g. past FICO scores) may reflect ability to pay insofar as it reflects vulnerability to income or liquidity shocks; it may also in part reflect willingness to pay.

Das (2012) reflects on the role of these factors on the borrower side and develops a theoretical model of optimal loan modification strategy in a stochastic environment of home prices and interest-rates. According to this theory, a suitable loan modification scheme must be cognizant of both the borrower's ability to pay and willingness to pay. The model indicates that effective modifications require writing down of the principal balance.[17]

Meyer et al. (2014), also focusing on the borrower side, present evidence that borrowers behave strategically in response to news of availability of loan modifications for delinquent borrowers. This study analyzes borrower response to the program introduced by Countrywide Financial Corporation at the end of 2008 to settle a lawsuit filed by U.S. State Attorneys General, whereby interest rate modifications were offered to all subprime mortgages that were at least 60 days past due. The study finds a significant increase in borrower delinquency in response to the program, suggesting that borrowers were induced to become delinquent by the prospect of receiving a mortgage modification and payment reduction.

The modification literature also has examined potential differences between *investor* and *portfolio* loans with respect to modification decisions (reflecting differences in the behavior of servicers that act as agents for investors or for other lenders and those

[17] The model ignores any additional considerations on the servicer side, and does not explain why principal write downs are relatively uncommon.

who service loans for their own portfolios.) Piskorski, Seru, and Vig (2010) emphasize three main reasons securitized loans may be serviced differently from portfolio loans. First, servicer financial incentives may differ between the investor and portfolio contexts. Within the framework of Jensen and Meckling (1976), investors force servicers to fully internalize the costs and benefits of foreclosing or modifying a loan. Second, Private Service Agreements (PSAs) may legally bar servicers from utilizing certain modifications. Third, property rights are jointly-held by many bond-holders, and coordinating an agreement among the bondholders on a particular modification scheme is more difficult than agreeing to a simple foreclosure. Similarly, Piskorski, Seru, and Vig (2010) and Aggarwal et al. (2011) find evidence of higher incidence of loan modification for bank-held mortgages than for similar securitized mortgages.

Adelino, Gerardi, and Willen (2013) argue that the determination of whether to modify a loan is based primarily on a cost-benefit analysis, where the servicer's assessment is based on the likelihood of success on modifications relative to the cost of proceeding through the foreclosure process, rather than on the status of the mortgage as either bank-held or securitized. The latter cost is depends in part on the likelihood that the borrower will self-cure, which represents an opportunity cost of proceeding with foreclosure. Adelino, Gerardi, and Willen (2013) find some support for this view using mortgage servicing data associated with a mix of bank-held mortgages, and securitized mortgages (both by the Government Sponsored Enterprises (Fannie Mae and Freddie Mac), and private sector). In particular, they find increased reliance on modification as likelihood of borrowers self-curing from delinquency declined over the period 2006 through 2010.

Studies examining the ex-post repayment performance of loan modifications granted during the initial stages of the mortgage crisis in 2007 and 2008 find troublingly high rates of redefault, thus suggesting a limited role in loss mitigation. Haughwout, Okah, and Tracy (2010) analyze a sample of privately securitized, nonprime mortgages that were modified between December 2005 and March 2009, and find a "distressingly high" 56 percent average redefault rate within one year of modification. Quercia and Ding (2009), examine a sample of privately securitized mortgages (mostly nonprime) originated in 2005 and 2006 and modified in the second quarter of 2008. By the end of 2008, 45 percent of the modified loans had returned to delinquent status. Both studies find that redefault rates decline with the size of the payment reduction or principal reduction offered by a modification. Adelino, Gerardi, and Willen (2013) attribute the relative low frequency of modification of delinquent mortgages prior to 2009 in part to high probability of redefault.

Haughwout, Okah, and Tracy (2009) caution, however, that "while we can tell that borrowers (and mortgages) that receive a modification are different from those that do not, we do not model how a mortgage gets into our 'modified' sample." This possibility of selection effects precludes drawing strong inferences from the observed repayment performance of modified loans. Servicers may choose whether or how to modify a loan based on private information about the borrower quality at the time of modification, and observed redefault rates among the modified loans may reflect these ex-ante characteristics. Thus, a high redefault rate among modified loans does not necessarily imply that loans not granted modification would also have a high likelihood of redefault were they to receive a modification.

Subsequent studies, including U.S. Treasury (2012) and Schmeiser and Gross (2015), that look at the performance of loans modified later during the crisis period (through at least 2011) confirm the significant relationships to post-modification payment reduction and post-modification CLTV ratio. Moreover, these studies demonstrate that principal reduction has a comparatively large impact on likelihood of redefault due to the joint effects of payment reduction and reduced CLTV ratio.[18] Whereas interest rate reductions reduce the likelihood of redefault only by lowering monthly payments, a principal modification reduces the monthly payment as well as the CLTV ratio – thus, for an equivalent payment reduction, the principal modification has a greater mitigating impact on likelihood of redefault.[19] These studies also attribute some differences in performance by type of modification to a selection effect, such that type of modification is correlated with unobservable information about borrower quality.

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[18] Voicu et al. (2011), in addition to demonstrating that likelihood of redefault is inversely related to both the amount of payment reduction and amount of principal reduction, also finds that modifications associated with the Treasury Department's Home Affordable Modification Program (HAMP) were less likely to redefault compared to non-HAMP modifications.

[19] Schmeiser and Gross (2015) also find that term extension modifications that increase the amount of principal due are most likely to redefault.

Anderson, Kogler, and Kim (2012) conduct a multivariate analysis of 12-month redefault rates of privately securitized mortgages receiving modification in 2006 through March 2011. This study finds impacts of interest rate reduction and principal reduction on likelihood of redefault similar to those in the aforementioned studies. In addition, it finds significantly higher probability of redefault for subprime loans and for loans receiving modification at a later stage of delinquency. This study also finds significant reduction in redefault rates after 2008, and notes that this may reflect improving macroeconomic conditions or "improvement in the design and targeting of modifications that is not captured in payment reduction or modification type variables", but, unlike our study, does not attempt to disentangle these effects.

Scharlemann and Shore (2015) apply a regression discontinuity approach to identify and neutralize the selection effect associated with modification type in evaluating the impact of principal reduction on redefault. They examine modifications provided under the government sponsored Home Affordable Modification Program (HAMP) Principal Reduction Alternative (PRA). This program provides for principal reductions for borrowers with negative equity, up to the point of meeting a negative equity target (usually 115 percent CLTV ratio.) On the margin, consistent with a PRA selection effect, the PRA principal reduction modifications have a more beneficial impact on redefault rate than is inferred from extrapolation of the relationship of CLTV ratio to redefault in the HAMP population not eligible for PRA.

In the literature to date, the study most closely related to ours is Goodman et al. (2013), which documents and analyzes semi-annual declines in the redefault rate on modifications of privately securitized mortgages over 2008 through 2012.[20] The authors discuss various factors that may have driven this downward trend, such as granting modifications earlier in the delinquency process, with more generous payment relief.

To extend the literatures previous work, our paper provides an extensive analysis of the improvement in the repayment performance of loan modifications after 2008. Previous studies investigate repayment performance on privately securitized mortgages. We demonstrate improved payment performance also for agency-securitized and bank-held conventional mortgages, and for government-insured mortgages. In this paper, we evaluate the performance of borrowers receiving loan modification relative to a matched sample of self-cured borrowers, controlling for other factors that might be important in lowering the redefault rates of both groups (such as housing market recovery). Over the course of the downturn, we find improvement in the performance of modified loans relative to self-cures. Moreover, the steeper decline in redefault rate of modified loans relative to self-cures persists after controlling for the factors cited by Goodman et al. (2013).

[20] Richter (2010) cites expanded offering of concessionary modifications that offer significant payment reductions as the primary factor underlying a decline in redefault rates on modifications over the 2008 through 2010 period.

Our findings are also robust to restricting the sample to individuals in an advanced stage of mortgage delinquency or those exhibiting other indicators of financial stress. Thus, the observed improved repayment performance of loan modifications in this paper does not appear to be due to the selection effect described by Meyer et al. (2014) whereby borrowers who have capacity to pay, after observing others benefitting from modification, intentionally miss some payments in order to themselves become eligible and receive the benefit of a modification. Rather, the steeper decline in redefault rate of modified loans relative to self-cures, which persists after controlling for other factors, is consistent with our "learning by doing" hypothesis.

Finally, we note that the potential selection effect associated with borrowers being selected to receive a loan modification, that clouds interpretation of the results in some previous studies, is neutralized in our analysis, for two reasons. First, the matching of modifications and self-cures, including the use of propensity scores, narrows the scope for selection effects as it ensures that the comparison is between borrowers who are as similar as possible given the available information. Second, if there remains a selection effect associated with loan modification, then it appears to be changing over time in such a way as to lower the redefault rate of modified loans relative to self-cures, which is consistent with our "learning-by-doing" interpretation.

2.3. Institutional Background

At the onset of the crisis, most mortgage servicers were organized to foreclose quickly on delinquent borrowers rather than engage in loan modifications (Cordell et. al 2009). Kingsley, Smith, and Price (2009) found a mortgage foreclosure imposes costs on a number of parties: $7,200 on average to a homeowner, up to $50,000 in expenses for investors and servicers, $19,229 in costs to local governments, and $3016 drop in the price of adjacent homes. These costs encourage both public and private institutions to promote modifications, to foreclosure, to keep borrowers in their homes and avoid the foreclosure costs.

In 2008, the FDIC published the “Mod in a Box” guide that provided a template for payment-reducing modifications for distressed mortgage borrowers. The guide emphasized adjusting the interest rate, extending the term of the loan, or forgiving principal to reduce to monthly payments to sustainable levels (characterized as a 31 to 38 percent ratio of debt payment to income.) The guide was accompanied by an NPV calculator that servicers could use to compare the NPV of the loan with and without the modification, and thus facilitate the decision whether to offer a loan modification and with what terms.

The Federal Housing Finance Agency (FHFA) introduced a formal program later in 2008 to modify delinquent GSE loans. The program was characterized by a set of borrower eligibility requirements; a recommended "waterfall" of maturity date, interest rate and principal adjustments to achieve a target reduction in monthly payments; and an $800 incentive paid to the servicer for each implemented modification.

This early effort was succeeded in 2009 by the Home Affordable Modification Program (HAMP) with the objective of further encouraging standardized modifications. HAMP mirrored the FHFA program with a waterfall modification protocol and servicer financial incentives, but had an expanded budget and larger pool of eligible loans (Cordell et. al 2011). At the onset of the program, 85 percent of all delinquent mortgages were eligible for HAMP modification (U.S. Treasury Department, 2009). HAMP was expanded further in June 2010 to allow for more principal modifications under the Principal Reduction Alternative.

The development of these streamlined programs, along with the direct financial incentives offered to servicers, likely encouraged them to scale-up their modification programs and facilitated the learning-by-doing process. In the pursuit of efficiency, servicers had opportunities to "learn-by-doing"; that is, improve the selection of loans for modifications and decrease the probability of further delinquency, as modification practices became more refined from 2008 to 2011.

In addition to streamlining of the modification process and offering financial incentives to servicers, government agencies also made data related to mortgage modification and repayment performance more readily available to the public. For example, the OCC's quarterly Mortgage Metrics Report in mid-2008 vastly improved the public reporting of foreclosure and modification frequency and performance. The U.S. Treasury published the Making Home Affordable Program Performance Report to track the growth of HAMP modifications and their success post-modification. The availability of these data may have also played a role in the "learning by doing" process.

In 2013, the Consumer Financial Protection Bureau released new servicer guidelines that all large mortgage servicers must follow beginning in 2014, as Mortgage Servicing Rules under the Truth in Lending Act.[21] Publication these servicing guidelines suggests that the "learning by doing" process has materialized in the form of uniform servicing standards that emphasize mortgage modification as effective best practice loss mitigation tool.

[21] It primarily features rules regarding how loss mitigation efforts should be handled and when foreclosure processes can begin after a loan becomes delinquent, specifically it establishes longer timelines for non-foreclosure activities to occur.

2.4. Data Sources

In exploring redefault after modification or self-cure, we use a newly available database that merges loan-level mortgage servicing data from Black Knight Financial Services McDash (Black Night McDash) with borrower credit report data from Equifax. This database is named Equifax Credit Risk Insight Servicing™ McDash, hereafter referred to as Equifax CRISM.[22]

The mortgage servicing component in particular provides payment status (days-past-due); loan terms (interest rate maturity date); and current balance for each mortgage, updated monthly.[23] For mortgages that enter into delinquency and subsequent foreclosure, the start and end dates of the foreclosure process also are reported. This information is used to identify mortgages that become seriously delinquent and those that are receive a modification or that self-cure out of delinquency.

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[22] The algorithm used by Equifax CRISM to merge the two datasets, which is proprietary, utilizes information common to both component databases, including mortgage origination amount, mortgage origination date, zip code of the property (Black Night McDash), zip code of the borrower (Equifax), current balance on mortgage (at the end of each quarter), the borrower's payment history.

[23] The Black Knight McDash data are collected from the 10 largest U.S. mortgage servicers and account for approximately 75 percent of all mortgages in the U.S. as of year-end 2010 (Black Knight McDash estimate).

We initially specify the sample to consist of first-lien mortgages financing a primary (owner-occupied) residence that became at least 90 days past due or subject to foreclosure in 2008, 2009, 2010, or 2011. We then draw a 20 percent random sample from this overall population, yielding a sample size of about 2.6 million mortgages.

We then further narrow the sample to borrowers who went through a modification process or who self-cured from their mortgage delinquency during the period 2008 to 2011. We apply a process to identify mortgage modification and/or self-cured that is adapted from Adelino, Gerardi, and Willen (2013). The resulting sample contains 211,461 mortgages, of which 151,862 underwent modification and 59,599 were self-cured.[24]

As noted, our analysis compares the redefault behavior of borrowers who have received modifications to matched borrowers (with similar characteristics) who have self-cured. The matching process further reduces the sample size, due to our inability to identify suitable matches for all modifications or all self-cures, as described in the next section.

The data in CRISM indicate whether the mortgage is held in a bank portfolio, within an agency mortgage-backed security; or in a non-agency security. In addition, these data provide date of origination of the first mortgage, the original loan amount;

[24] A few loans with missing information on loan balance, maturity date, or interest rate are excluded from our sample.

original appraised value of the property, state and county location of the property; borrower FICO score at origination, and information on loan type (whether fixed rate; amortizing adjustable rate; or pay-option adjustable rate). The data also provide up-to-date borrower credit score (the Equifax Risk Score); total balance of the first plus any second-lien mortgage; and aggregated information from the borrower's credit record. We combine this detailed loan and borrower information with economic data, including the county-level repeat sales house price index (HPI) from CoreLogic and seasonally-adjusted unemployment rates by county from the U.S. Bureau of Labor Statistics. Thus, we control for a wide variety of factors associated with likelihood of being granted a loan modification and likelihood of redefault subsequent to modification or self-cure.

2.4.1. Identifying Mortgage Modification

A loan is classified as undergoing modification if we observe changes made to the principal balance, interest rate, or maturity date yielding a material reduction in the monthly payment. In addition, we require that in the course of modifying a loan the servicer returns the loan to non-delinquent status.[25]

[25] Modifications (as identified by changes in loan terms) that do not return a loan to non-delinquent status appear to be relatively uncommon.

Principal reduction modifications are identified by comparing the current principal balance to both the origination balance and previous month's balance. Similar to Adelino, Gerardi, and Willen (2013), we require that the current principal balance be at least 10 percent less than both the previous month's principal balance and original balance, but greater than 50 percent of the original balance[26]. An exception is made for mortgages reported in Equifax CRISM to have been assigned to a loss mitigation program, in which case any reduction in principal is equated with a principal reduction modification.[27]

Interest rate modifications are identified by comparing the reported interest rate between the current and previous month. In the case of a fixed-rate mortgage, a loan modification is indicated by an interest rate reduction of at least 50 basis points.[28] The same rule is applied to adjustable rate mortgages, provided the current month does not coincide with the original note's contractual reset date. If an adjustable rate mortgage reported in Equifax CRISM to have been assigned to a loss mitigation program, then a rate modification is indicated by a 1 percentage point reduction. Otherwise, rate

[26] This prevents loans that are simply receiving a monthly payment from identifying as modifications

[27] As explained in Adelino, Gerardi, and Willen (2013), the loss mitigation flags included in servicing data are not comprehensive, necessitating construction of modification indicators using reported changes in loan terms.

[28] Adelino, Gerardi, and Willen (2013) apply a more restrictive criterion (larger rate reduction) for identifying interest rate modifications, but do not require that a modification return a loan from serious delinquent to current status.

modification for an adjustable rate mortgage is indicated by an interest rate reduction that is no less than 1 percentage point and at least 50 basis points in excess of the decline in the index rate since the previous rate reset (or since origination, in the absence of a prior reset.)[29]

One other type of modification, involving combination rate and maturity adjustment, is identified by comparing reported interest rate and maturity date between the current and previous months. A modification is indicated if the maturity has been extended at least 12 months; the interest reduced by at least 12.5 basis points; and the required monthly payment has been reduced.[30]

Each modified mortgage is uniquely assigned to a modification category, such that priority is given to principal reduction and term extension. Thus, if the loan is indicated to be a principal modification, it would be assigned to that category regardless of whether an interest rate modification is also indicated. If rate and term modification but not principal reduction is indicated, the loan would be considered a rate and term modification.

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[29] The index rate, or market rate to which the mortgage note rate is tied, is represented by the 3-month Treasury bill rate for this calculation.

[30] In contrast, Adelino, Gerardi, and Willen (2013) identify term extensions that increase monthly payments as loan modifications.

2.4.2. Identifying Self-Cure

A first mortgage is indicated to be self-cured if it returns to current status without the material reduction in monthly payment indicative of loan modification. Note that this definition does not rule out some assistance by mortgage servicers, such as granting a term extension that returns the borrower to current status but increases the monthly payment, or forgiving some accrued interest or penalties

2.4.3. Composition of the (Pre-Match) Sample

Figure 2-1and Figure 2-2 show the composition of the pre-match sample with respect to counts and shares of modified versus self-cured mortgages, and by year of modification or cure. In 2008, the share of modifications among overall cures was not quite 40 percent, but by 2009 the share had doubled, with nearly 80 percent of all cures occurring via modification, due to rapid growth in number of modifications.

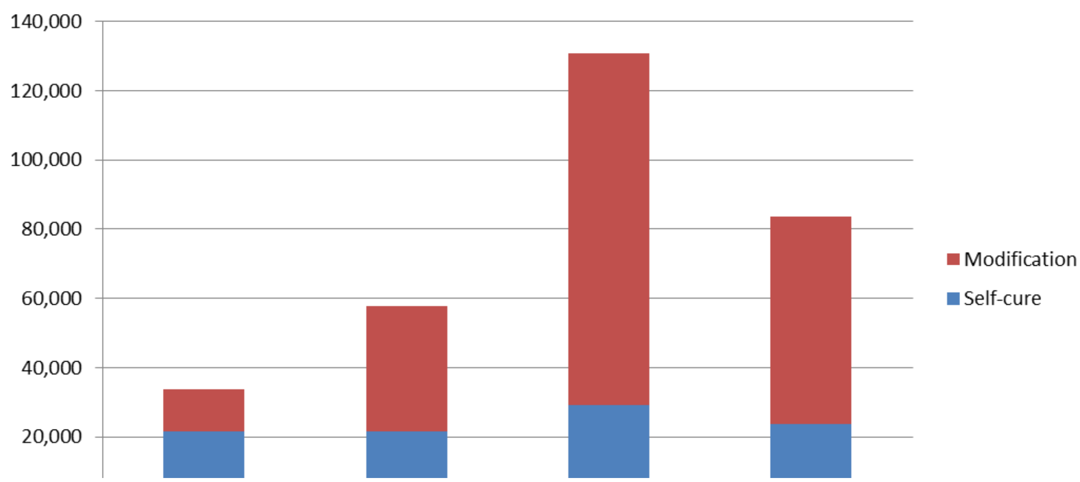

Figure 2-1 Cure Type by Vintage

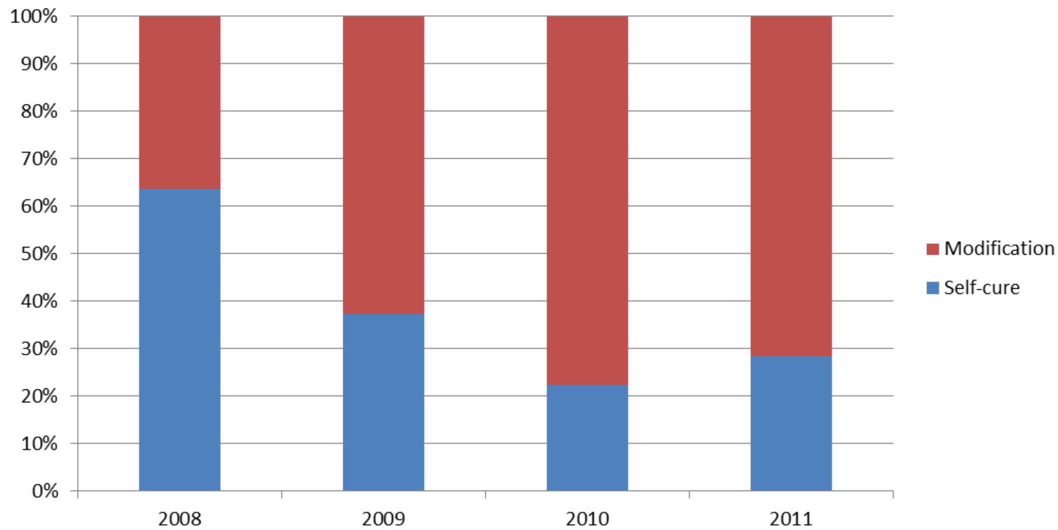

Figure 2-2 Shares of Modification and Self-cure

Figure 2-3 through Figure 2-8 disaggregate the modification trends by the various types of mortgage modification –principal reduction; interest rate reduction, and rate and term adjustment. Interest rate modification clearly dominates all the other categories in every year. Also notable is the rising share of principal write-down modifications between 2008 and 2011. The rollout of the HAMP program in 2009, which encourages principal reductions, is the most likely explanation of the rise in principal write-down modifications in 2010 and 2011.

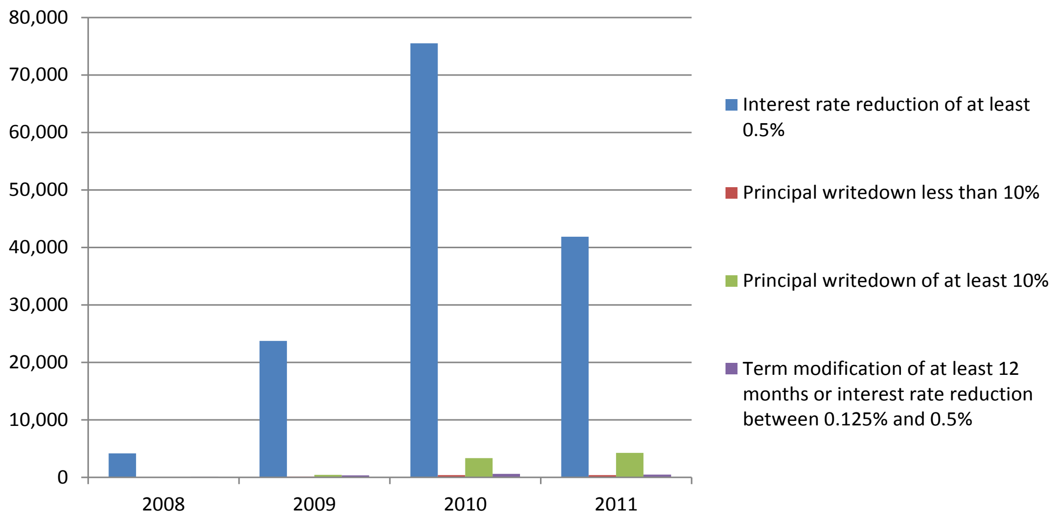

Figure 2-3 FRM Modification Type Frequency by Vintage

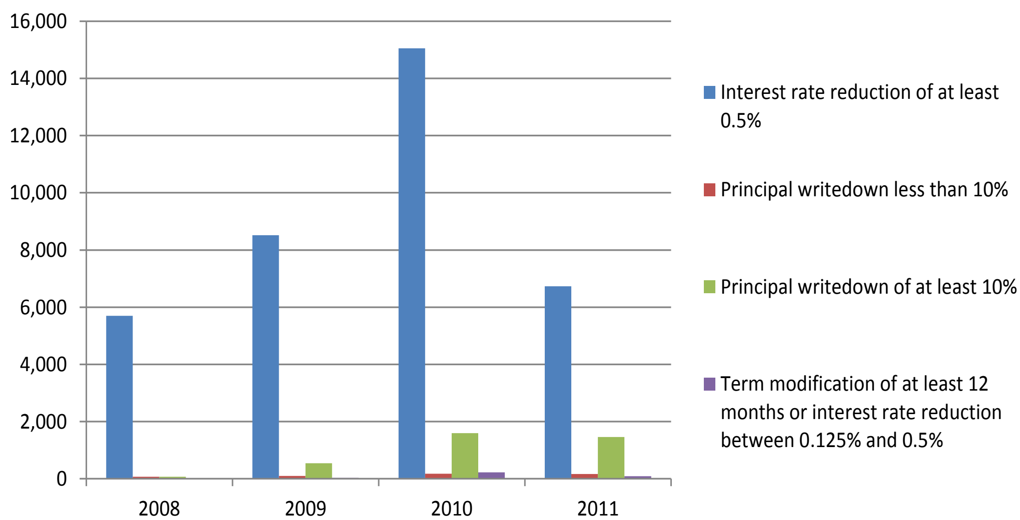

Figure 2-4 Amortizing ARM Modification Type Frequency by Vintage

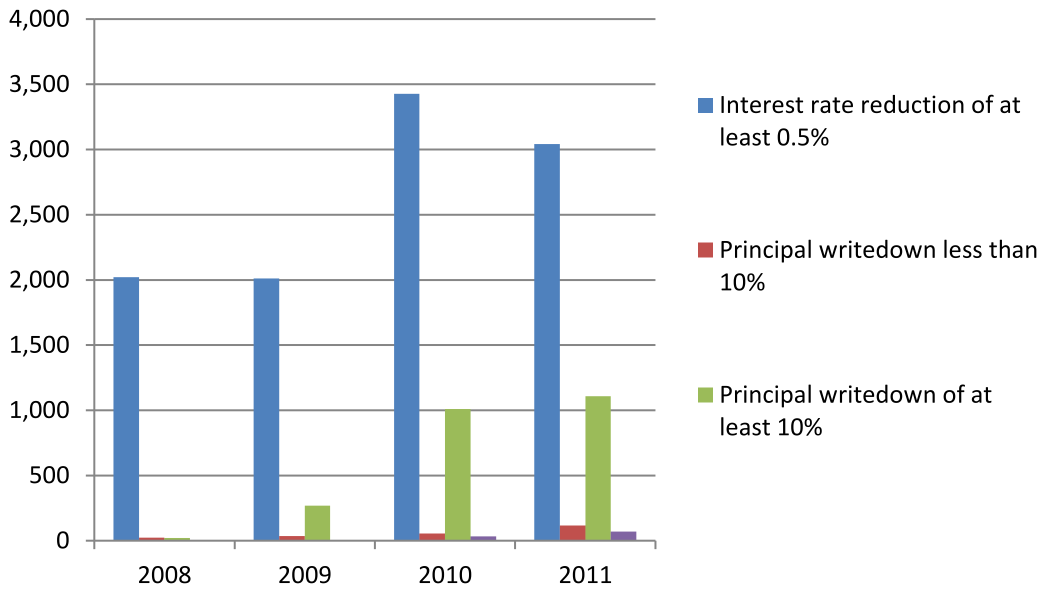

Figure 2-5 Option ARM Frequency by Vintage

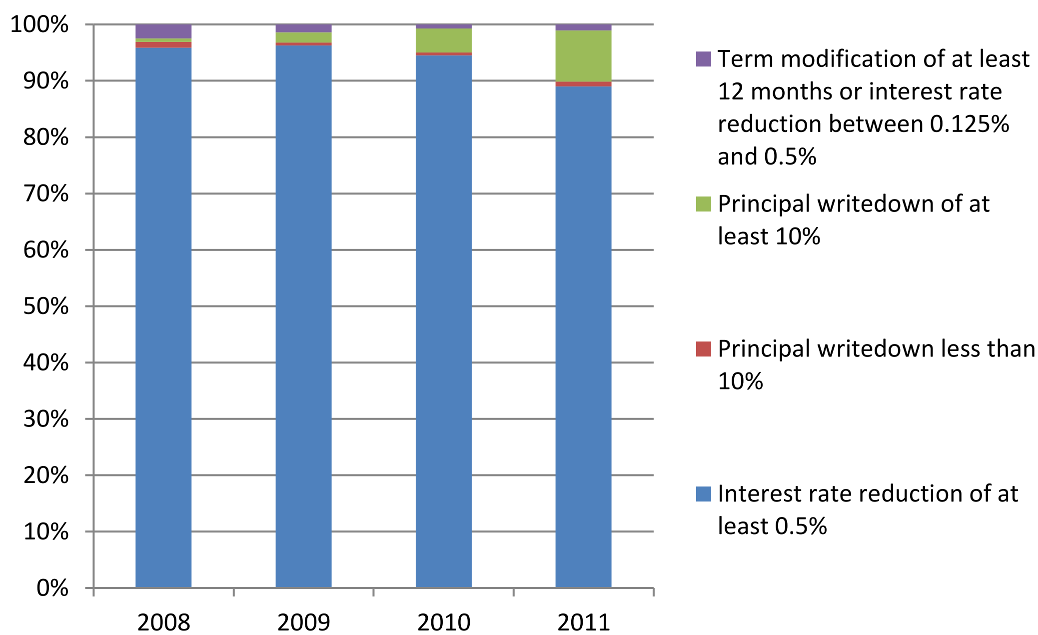

Figure 2-6 FRM Modification Type Frequency by Vintage

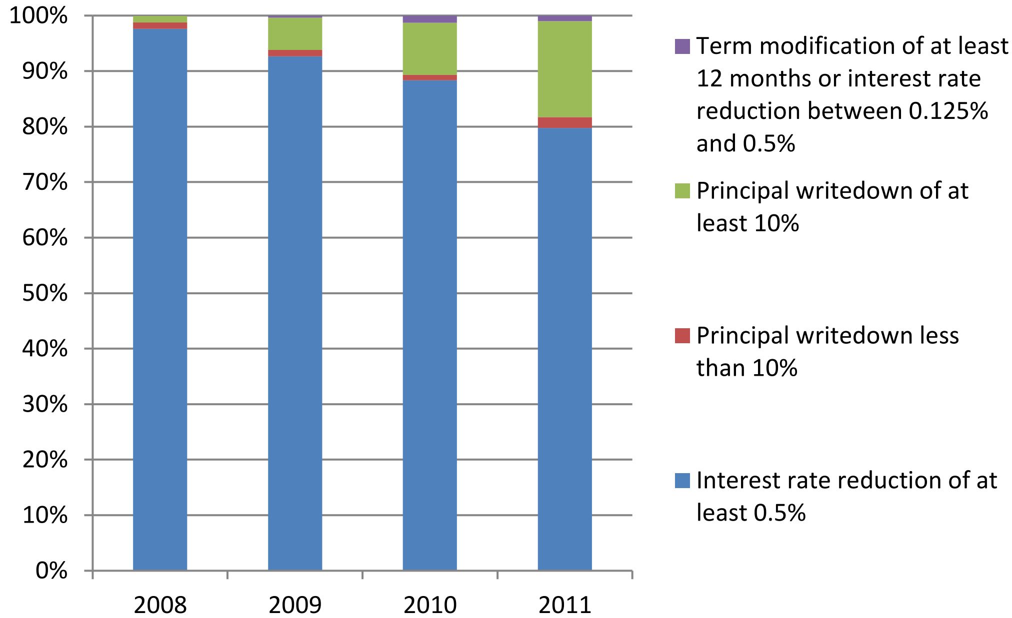

Figure 2-7 Amortizing ARM Modification Shares by Vintage

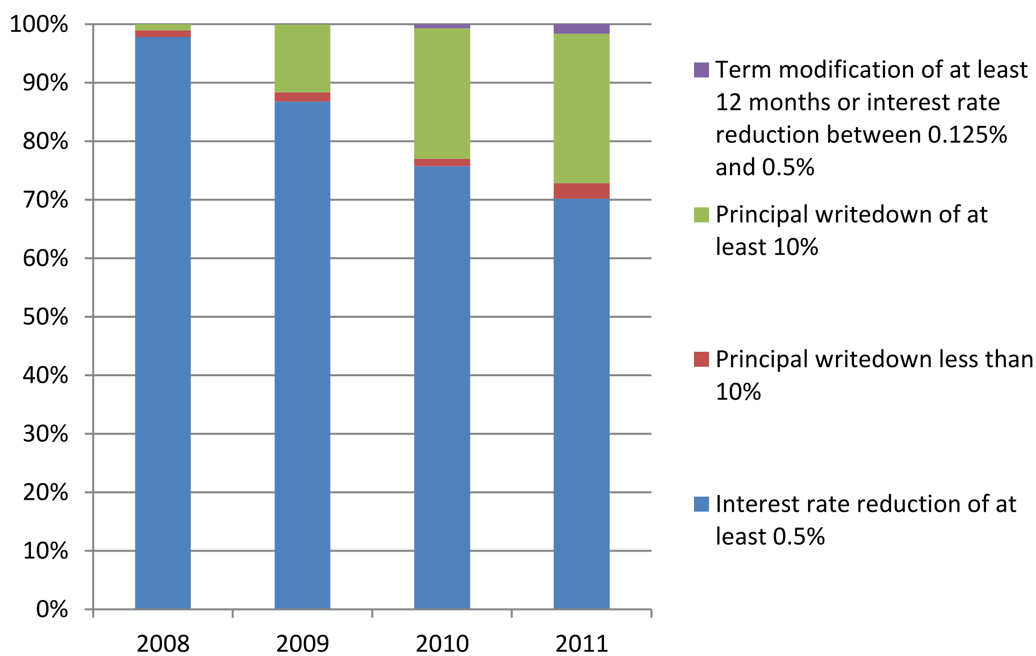

Figure 2-8 Option ARM Modification Shares by Vintage

2.5. Empirical Methods and Results

2.5.1. The Cumulative Comparisons with Propensity Score Matching

The analysis compares the redefault behavior of borrowers who receive modifications to matched borrowers who self-cure, as a means of controlling for the influence on redefault of the changing economic environment and delinquent borrower population. Both groups experience initial delinquency, become current, and are at risk of redefault; what distinguishes them is that self-cures did not receive payment-reducing changes in loan terms to facilitate their remaining current. The matching process ensures that the comparison is between borrowers who are as similar as possible given the available information, thus narrowing the scope for selection effects.

The matching process is based on propensity scoring overlaid with matching on selected categorical variables (for product category; state location of the property; delinquency status prior to cure; and year and quarter of cure.) Propensity score matching, introduced by Rosenbaum and Rubin (1983), is used to account for observable heterogeneity across "treated" and "non-treated" entities using conditional probability of treatment given observable characteristics.[31] In our study the "treated" are modified loans

[31] Other common matching algorithms include: nearest-neighbor matching, kernel matching, local linear matching, Mahalanobis metric matching, Mahalanobis metric matching including the propensity score, and difference in differences methods. We use propensity score matching because it gives us the greatest specification flexibility to match within the subgroups of delinquency, investor, state, and date of cure. Furthermore, it is easier to control the specification of characteristics (e.g. credit score, ltv, etc.) that determine the matching distance.

and the “non-treated” are self-cured loans, such that selection for modification is implemented by mortgage servicers on behalf of investors.[32]

The propensity score matching process involves three steps. First, observable characteristics are regressed on a treatment indicator using a probit or logit specification, and the predicted values are recorded. Next, each treated observation is matched with a similar non-treated observation based on a calculated propensity score and matching criterion. Last, a regression model or other analytical model can be administered to the matched observations to compare the outcomes of the treated versus non-treated. Separate regressions are run for three investor/guarantor categories: loans insured by the Federal Housing Authority or Veterans Administration (FHA/VA); loans securitized and guaranteed by Fannie Mae and Freddie Mac (GSE); and loans held in bank portfolios or privately securitized.

Initially, we run a logistic regression model to predict propensity $p(x_i)$ for borrower i of being selected for modification, $M_i = 1$:

$$p(x_i) = pr(M_i = 1 | X_i = x_i) = \frac{1}{1 + e^{-X_i \beta}} \tag{1}$$

[32] Propensity score matching is commonly used to reduce selection bias in treatment evaluation studies. For a more complete discussion of the origination and motivation for propensity score matching see Guo, Barth, and Gibbons (2006).

where X_i is a vector of covariates.[33] The latter include the ranges for dynamic LTV defined above, along with ranges of delinquency status (days past due) as of the month prior to cure. In addition, the covariates include log of the mortgage outstanding balance; spread of the interest rate spread (relative to the 10-year Treasury); and the borrower's Risk Score and bankruptcy status; and the indicator for more than one active, first mortgage account in the borrower's credit file, each measured as of three months prior to the date of cure. Other covariates include the indicators for subprime mortgage, jumbo mortgage, retail origination channel, and product type. In addition, the regression model includes state (location of the property) fixed effects, year-of-origination dummy, and year-quarter of cure indicators.[34] For the pooled bank portfolio and private securities regression, we use a portfolio dummy to differentiate the two subpopulations

[33] We assume the error terms of modification selection follows a logistic distribution,.

[34] We tested specifications with year-over-year changes in unemployment rates. Since this variable proved insignificant at both the state and county level, we excluded it from the final specification.

2.5.2. Propensity Score Results

The logit regression results are shown in Table 1. For all three product types, the coefficients of the CLTV buckets are significantly positive for all (except for the highest, CLTV greater than 120 category), and monotonically increasing with higher CLTV buckets, indicating that loans with higher CLTV are more likely to be chosen for modification. The only exception is for FHA mortgages with CLTV greater than 120, where the probability of getting modification declines.[35]

[35] This might reflect servicing rules specific to FHA loans.

Table 2.1 Logistic Regression Model Predicting Propensity Scores

Dependent variable	FHA		GSE		Private Securitized/Portfolio	
Modification flag						
Independent variable	Coef.	Std Err.	Coef.	Std Err.	Coef.	Std Err.
CLTV 80-90 flag (3 months prior to cure)	0.1362***	(0.0455)	0.3808***	(0.0243)	0.1624***	(0.0214)
CLTV 90-100 flag (3 months prior to cure)	0.3189***	(0.0423)	0.5645***	(0.0278)	0.3268***	(0.0278)
CLTV 100-110 flag (3 months prior to cure)	0.4016***	(0.0718)	0.6275***	(0.0887)	0.3898***	(0.0629)
CLTV 110-120 flag (3 months prior to cure)	0.7279***	(0.2746)	1.4312***	(0.4249)	0.6668***	(0.1880)
CLTV >120 flag (3 months prior to cure)	-0.5308***	(0.2371)	1.5123***	(0.3275)	0.7546***	(0.1069)
12 month % change in HPI	0.3745	(0.3324)	-0.5144**	(0.2042)	-0.0932	(0.1724)
Subprime flag	0.2154***	(0.0284)	0.0961***	(0.0268)	0.0605***	(0.0219)
Retail flag	-0.1842***	(0.0299)	-0.1627***	(0.0188)	-0.1822***	(0.0208)
Bankruptcy status (3 months prior to cure)	-1.3129***	(0.0815)	-0.9618***	(0.0598)	-0.7970***	(0.0565)
Multiple first mortgage flag	-0.3760***	(0.0596)	-0.256***	(0.0239)	-0.1246***	(0.0234)
Variable ARM flag			0.2467	(0.1620)	-0.0542	(0.0428)
One year ARM flag			0.4070*	(0.2408)	1.3274***	(0.1365)
Two/Three year Arm flag			0.4272***	(0.1001)	0.6140***	(0.0339)
Amortizing ARM flag			-0.5229***	(0.0337)	-0.2037***	(0.0293)
Option ARM flag			-0.0495	(0.0873)	0.2696***	(0.0427)
Portfolio flag					0.1737***	(0.0219)
Log balance amount (3 months prior to cure)	0.7776***	(0.0346)	0.8128***	(0.0206)	0.6705***	(0.0237)
Jumbo flag					14.0935***	(0.9819)
Log balance amount (3 months prior to cure)*Jumbo flag					-1.1008***	(0.0749)
10-year treasury interest rate spread (3 months prior to cure)	0.6905***	(0.0251)	0.2118***	(0.0129)	0.2827***	(0.0065)
Risk score (3 months prior to cure)	-0.0015***	(0.0002)	0.0005***	(0.0001)	0.0004***	(0.0001)
90 to 120 dpd flag (month prior to cure)	-1.718***	(0.1556)	-1.7351***	(0.0858)	-1.8981***	(0.0644)
120 to 180 dpd flag (month prior to cure)	-0.5305***	(0.1555)	-1.0659***	(0.0847)	-1.1620***	(0.0636)

Table 2.1 Continued

	FHA		GSE		Private Securitized/Portfolio	
Independent variable	Coef.	Std Err.	Coef.	Std Err.	Coef.	Std Err.
180 to 270 dpd flag (month prior to cure)	-0.2223	(0.1559)	-0.6671***	(0.0849)	-0.8409***	(0.0639)
270 to 360 dpd flag (month prior to cure)	-0.1272	(0.1579)	-0.4850***	(0.0865)	-0.6542***	(0.0660)
360 to 540 dpd flag (month prior to cure)	-0.0281	(0.1593)	-0.3502***	(0.0868)	-0.4736***	(0.0662)
540 to 720 dpd flag (month prior to cure)	0.0498	(0.1783)	-0.2995***	(0.0947)	-0.2860***	(0.0745)
N	48,075		84,662		78,724	
Pseudo R-squared	0.4111		0.3123		0.2597	

Notes: The dependent variable is a loan receiving a loan modification. Models include state fixed effects, year of origination controls, and year-quarter of cure controls. Additional controls include quarter of cure indicators. Results are robust to the inclusion of state fixed effects. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

The coefficients of the delinquency buckets indicate that likelihood of modification tends to increase as the borrower falls behind in payments. Subprime loans exhibit higher likelihood of modification, as do loans originated through non-retail channels such as through loan brokers, a characteristic typical of subprime loans. Likelihood of loan modification increases with the loan balance.

Borrowers experiencing more severe, overall financial difficulties, as indicated by bankruptcy filing or lower Risk Score three months prior to cure, have a reduced likelihood of receiving a loan modification, except in the case of FHA loans. For FHA loans, which are government –insured, unlike the GSE and privately securitized loans, borrowers with lower Risk Scores are more likely to receive a modification. Borrowers with more than one open mortgage account in their credit file (where the additional mortgages would be associated with non-primary residences) are less likely to receive a modification (on the mortgage associated with their primary residence.) Interest rate spread three months prior to cure is positively related to probability of receiving a modification, which may be due to wider range of interest rate reductions potentially available for rate modifications.

2.5.3. Matching Modifications to Self-cures

Borrowers are first matched on the basis of geographic location, date of modification or cure, delinquency bucket, and investor/guarantor category. In this step, we create segments of the modified and self-cured loans based on the state in which the property is located; the year and quarter of cure (through modification or self-cure); the same delinquency status in the month prior to being cured, and the same investor/guarantor bucket. In the next step, we match modified loans to self-cured loans within the same segment based on the estimated propensity score (predicted value for probability of modification from the logit regression), as described below.

The method of nearest-neighbor, caliper matching is used to identify borrowers with sufficiently close propensity scores. This method involves selecting pairs of modified and self-cured loans, within the same characteristic segment, with absolute difference in propensity scores less than a specified caliper value (α). We allow multiple self-cure loans to be matched to a unique modified loan in the same segment. In line with traditional, one-to-many matching procedures we assign a weight of 1 to the modified loans (treated) and weight each of the matched self-cure (control) loans by the reciprocal of the number self-cure loans matched to a specific modified loan.

In the event of a modified loan cannot find a match within the caliper the observation is dropped from the analysis. In order to minimize loss of observations of modified loans due to lack of suitable matches, we allow for replacement in the matching process (a particular self-cured loan may be paired with multiple modified loans in the same segment), up to a limit of 25 uses of the same self-cured loan.

On the one hand, choosing a larger caliper interval increases the matching rate, but may increase the likelihood of producing inexact matches. On the other hand, reducing the caliper increases matching accuracy at the cost of reducing the matched sample size. Following Ding et al. (2011) we select a caliper of 0.1 for our analysis to balance the selection of a larger sample with matching accuracy.

2.5.4. Redefault Rate Comparisons

The matched sample allows for a controlled comparison of redefault frequency of self-cured versus modified loans. We can also examine trends in the comparative redefault rates over time, by grouping modifications and self-cures into cohorts based on date of the initial cure initial cure from delinquency.

In Figure 2-9 to 2-12, we plot the monthly cumulative redefault rates of modifications and matched self-cures over a period of 36 months after the loans resets to current, by investor/guarantor category, distinguishing between the earliest cure cohort in

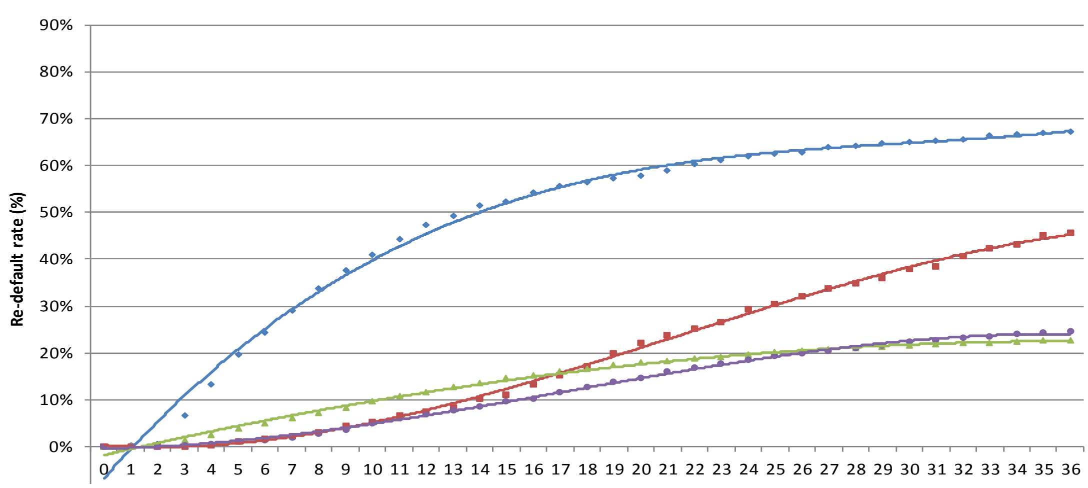

Figure 2-9 Agency Cumulative Redefault 2008 vs. 2011

our sample (2008) and the latest (2011). For ease of visualization, we fit the data points to cubic polynomials, which generally provide a close approximation to the empirical redefault curves.

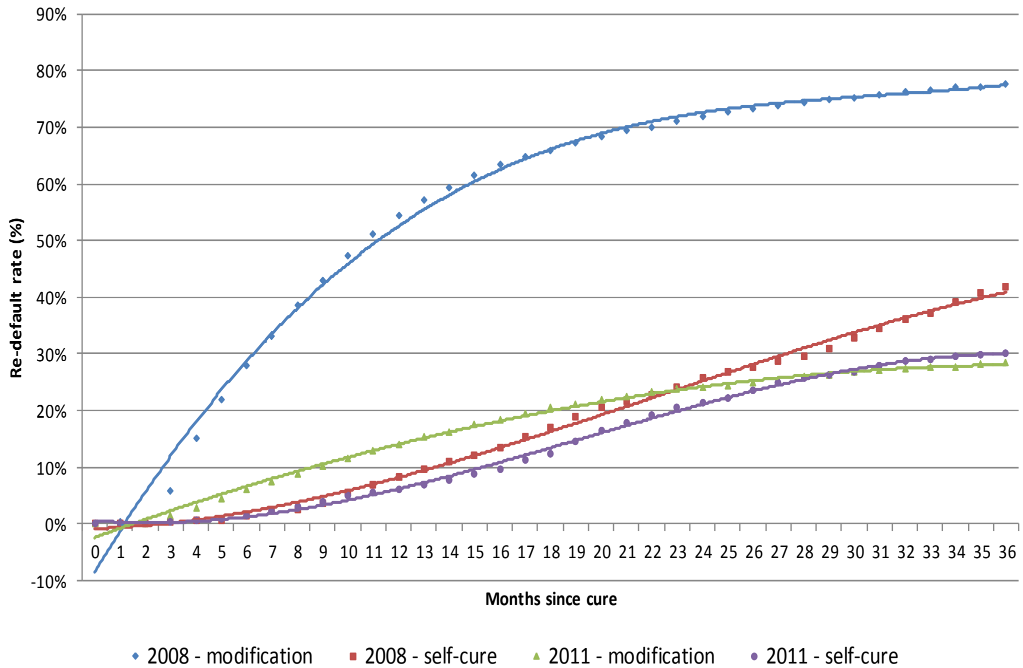

Figure 2-10 Private Securitized Cumulative Redefault 2008 vs. 2011

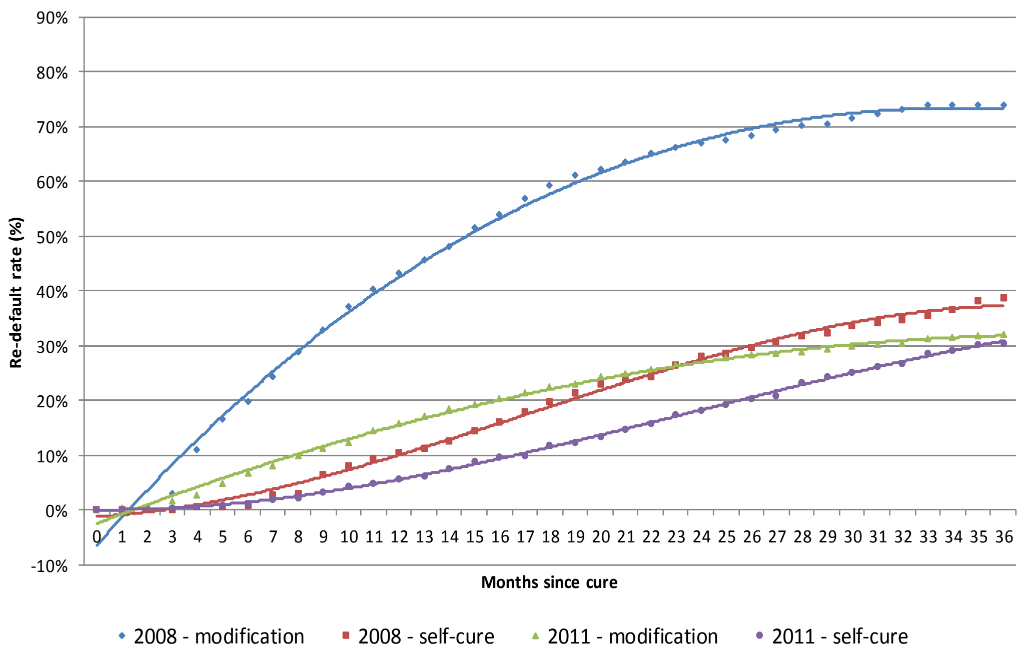

Figure 2-11 Portfolio Cumulative Redefault 2008 vs. 2011

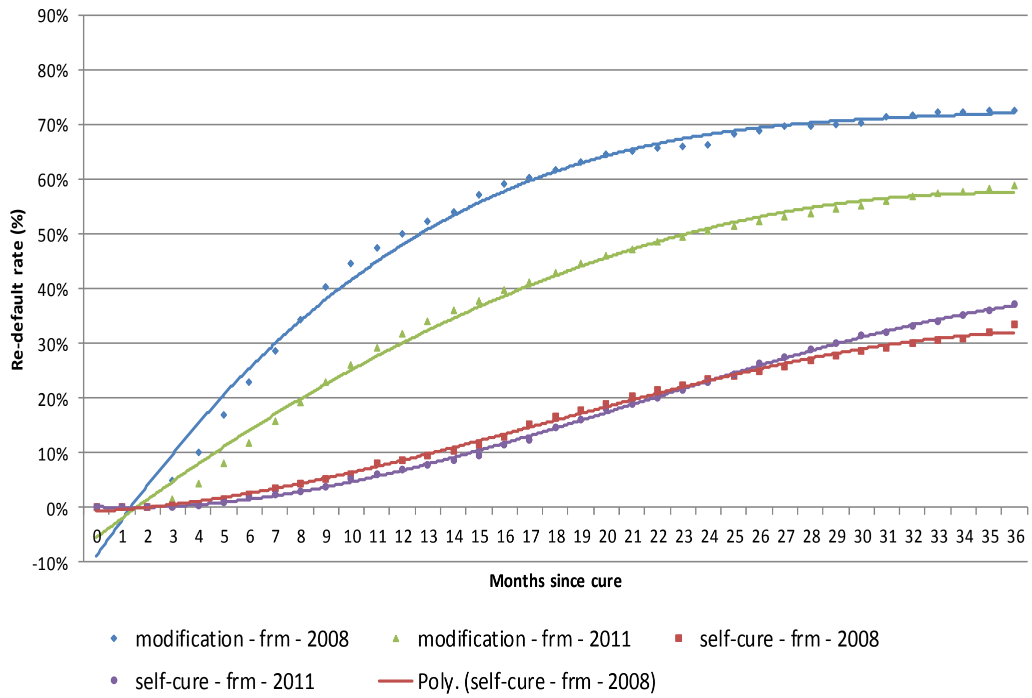

Figure 2-12 FHA Cumulative Redefault 2008 vs. 2011

Figure 2-13 to 2-15 presents the same information disaggregated by product type—FRM, Option ARM, and ARM, specifically for the private securitized category. Congruent redefault patterns are observed across all three product types. The private securitized category is selected here for illustrative purposes only; the Agency and portfolio categories also exhibit congruent patterns across product types.

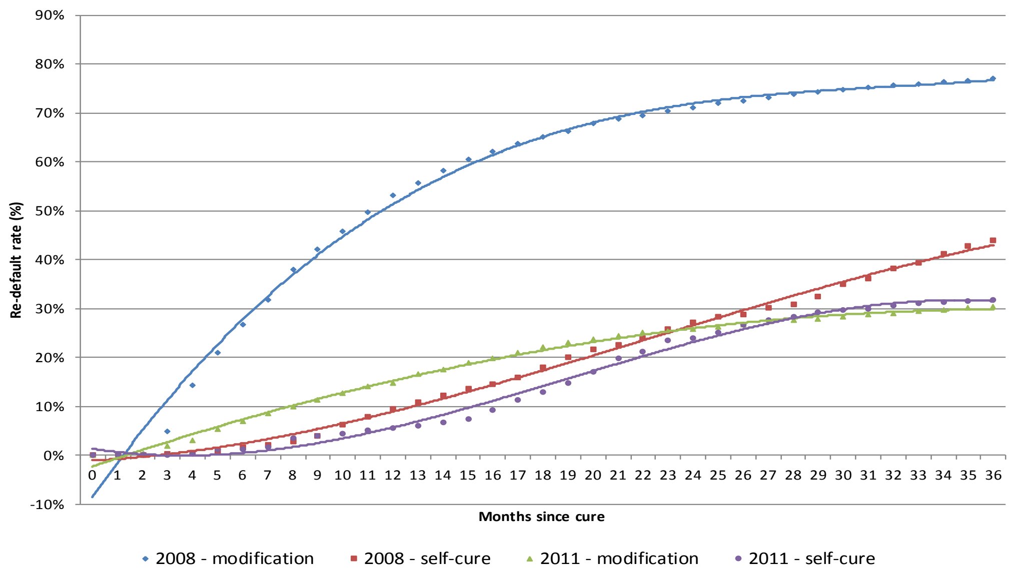

Figure 2-13 Private Securitized Amortizing ARM Cumulative Redefault 2008 vs. 2011

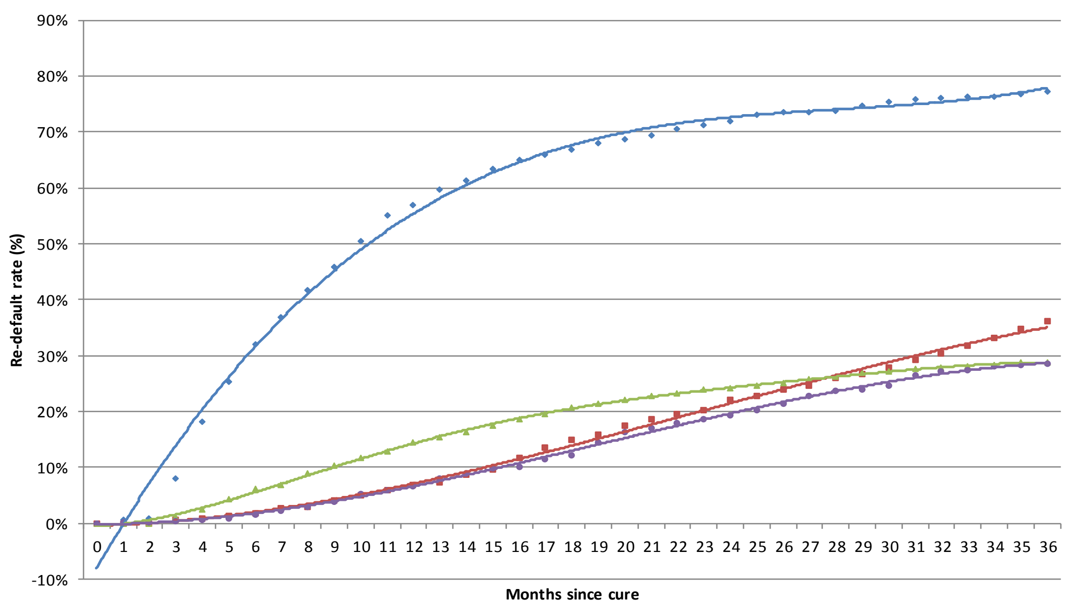

Figure 2-14 Private Securitized FRM Cumulative Redefault 2008 vs. 2011

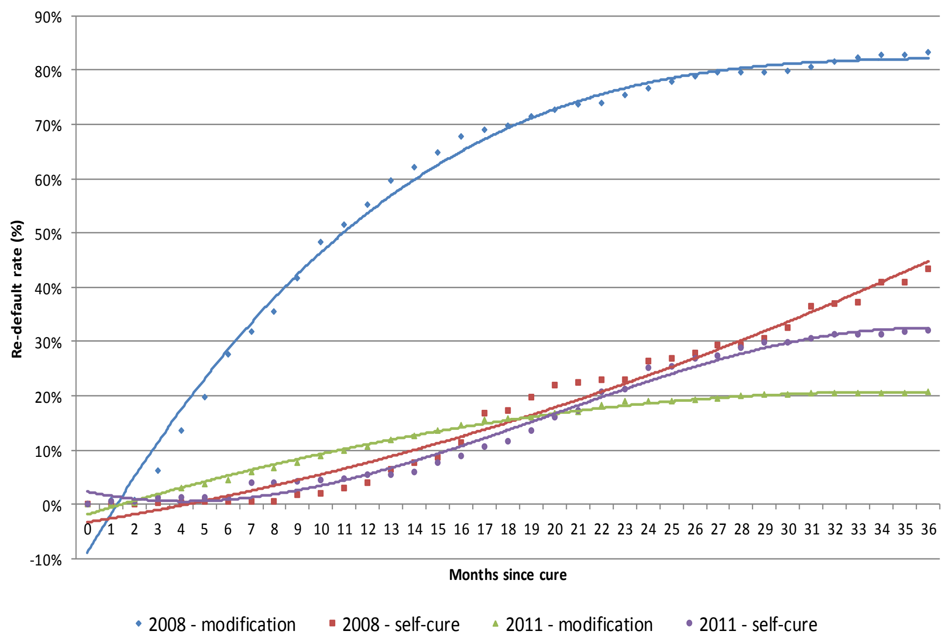

Figure 2-10 Private Securitized Option ARM Cumulative Redefault 2008 vs. 2011

Figure 2-16 to Figure 2-18 presents monthly cumulative redefault rates of modifications and matched self-cures through 36 months after the initial cure, by year of cure, again singling out for illustrative purposes the private securitized category. For each cohort year we observe a notable decline in the redefault rate of modified loans with the largest decline occurring from 2009 to 2010.

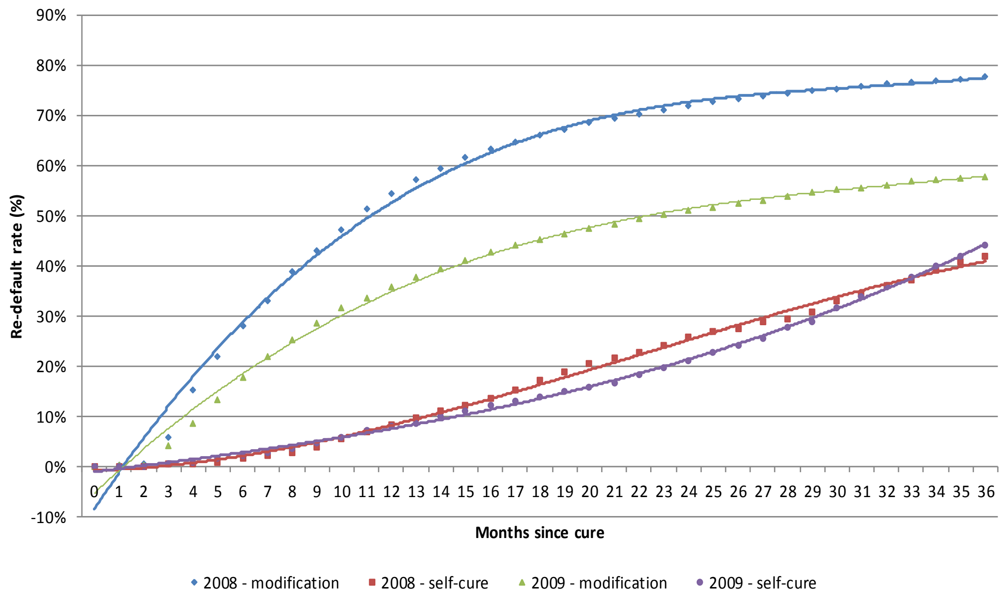

Figure 2-11 Private Securitized Cumulative Redefault 2008 vs. 2009

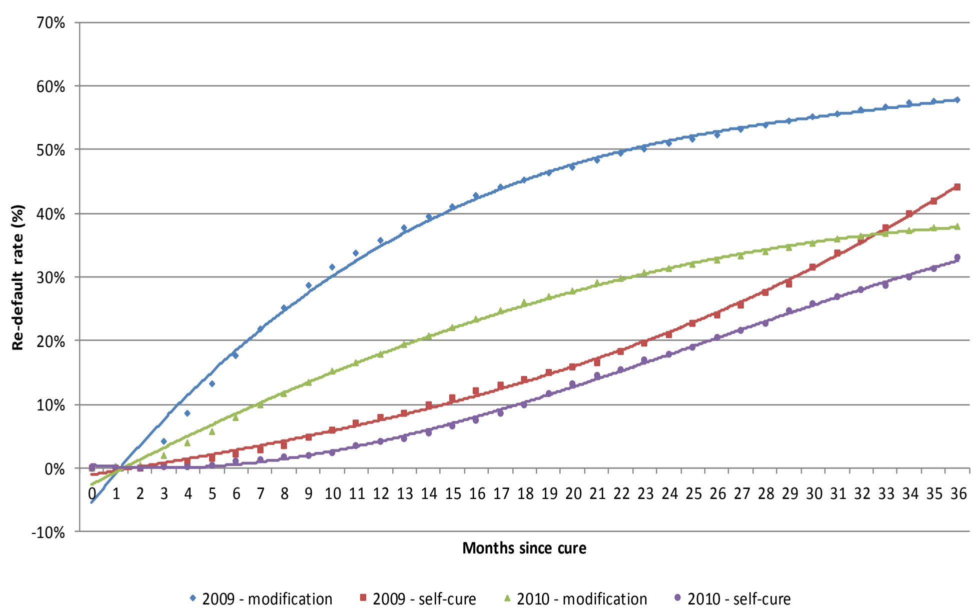

Figure 2-12 Private Securitized Cumulative Redefault 2009 vs 2010

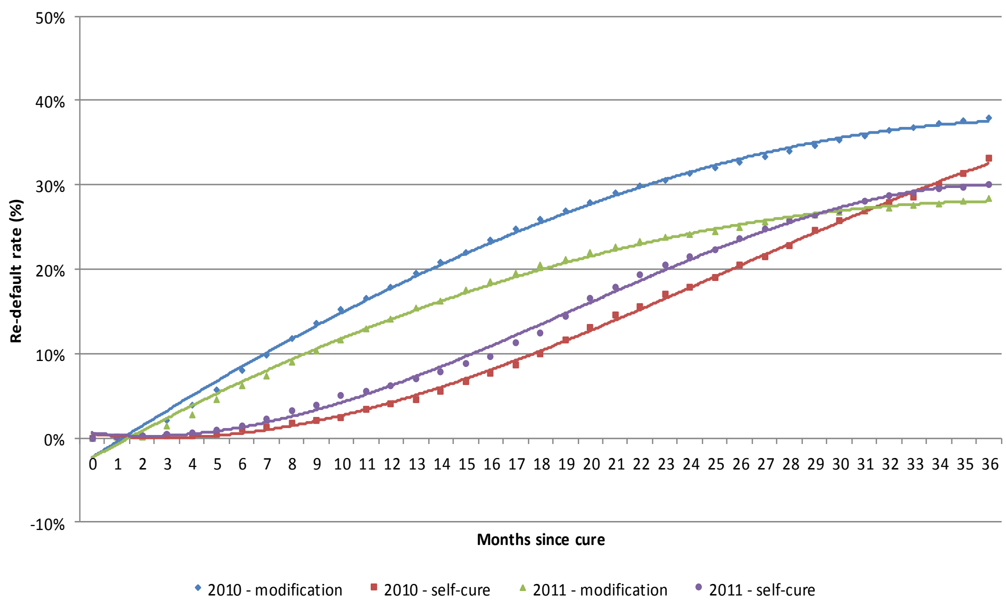

Figure 2-13 Private Securitized Cumulative Redefault 2010 vs. 2011

From Figure 2-9 to Figure 2-18 we observe that in 2008, the cumulative redefault frequency of modified loans far exceeds that of self-cured loans in the same segment of characteristics. Between 2008 and 2011, except in the FHA/VA category, the decline in 36-month cumulative redefault rate is much more dramatic for modified loans compared to self-cured loans. By 2011, except for FHA/VA, we observe the same cumulative redefault frequency for both modified loans and self-cured loans – the convergence of their cumulative redefault frequencies.

This is an important piece of evidence that supports our "learning-by-doing" process and has not been explored in the literature.[36] Early on in the crisis, many servicers had limited experience designing modification programs. As modification activity increased over the course of the downturn, servicers engaged in "learning-by-doing" that improved loan modification outcomes. Servicers became more adept at screening borrowers for modification eligibility. Based on the repayment performance associated with previous modification decisions, they updated the criteria or net present-value calculations applied to assess whether a modification is preferable to inaction or foreclosure, so as to lead to better outcomes.

Learning-by-doing could also take the form of better tailoring modification terms to ensure a borrower can reasonably maintain future monthly payments. Again, servicers could be updating their intuition or models for what modification terms are likely to be successful for a given borrower based on their accumulated experience. Another potential form of learning-by-doing is developing tools to prevent default –post-modification support or intervention such as financial counseling. Overall, servicers became better at modifications by avoiding past mistakes.[37]

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[36] Since the loans are matched based upon location of property; date of cure; and delinquency status prior to cure, as well as investor/guarantor type and propensity score, the convergence of redefault rates cannot be attributed to the modified and self-cured loans becoming more alike with respect to observable factors such as local economic conditions.

[37] While learning-by-doing has played an important role in the decline in redefault frequency of modified loans, there may also be other reasons that affect the redefault rate of modified loans. Goodman et al. (2013) point to the granting modifications to loans earlier in the delinquency process (which itself may

We also observe that the cumulative redefault curves of modified loans are concave; frequency of redefault declines with time since cure. However, the 2008 modified loan cohort exhibits substantially more concavity than the 2011 cohort. It appears that among borrowers receiving modifications, redefault is concentrated in the earlier months after cure. Early relapse into default might reflect servicer misjudgment in selecting borrowers for modification or in designing the modified loan terms. The concavity of redefault curve is consistent with servicer "learning-by-doing" that averts such misjudgments. In contrast, the cumulative redefault curves of self-cured loans are linear (or slightly convex). The likelihood and timing of redefault within the first few years after self-cure appear to be fairly uniformly distributed across self-cured borrowers, which may be driven at least partially by random incidence of the income or liquidity shocks that trigger the redefault.

2.5.5. Empirical Redefault Model

Building upon our visual analysis of redefault patterns over-time, we create a redefault model to predict default within 36 months after the modification/self-cure date. This allows us to observe the effect of modification or self-cure vintage (year), a proxy for "learning-by-doing," on the likelihood of redefault following a cure.

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reflect learning-by-doing) with more generous payment relief. We explore these alternative explanations in the next section.

With the matched sample of modified loans paired with self-cures, weighting each observation as described earlier, we use the following simple logistic model to identify modification and self-cure vintage effects on redefault post-cure:

$$redefault\ flag_i = \alpha + \beta_0 modified_{2008_i} + \cdots \beta_3 modified_{2011_i} + \tag{2}$$

$$\beta_4 selfcure_{2009_i} + \cdots \beta_6 selfcure_{2011_i} + \beta X_i$$

where:

- $redefault\ flag_i$ is a binary indicator for 90+days-past-due delinquency for loan *i* in month *t*. We focus on a loan reaching 90+ days-past-due within 36 months of the cure date.

- $modified_{t_i}$ is the vintage dummy variable indicating modification in year *t* for loan *i*.

- $selfcure_{t_i}$ is the vintage dummy variable indicating self-cure in year *t* for loan i. We omit the 2008 self-cure vintage indicator, and use 2008 self-cure as the baseline for comparing all $selfcure_{t_i}$ and $modified_{t_i}$ indicators.

- X_i includes the set of key characteristics unique to loan *i*.

We estimate separate logistic regressions for each product type (FRM, amortizing ARM, and option ARM) within each investor/guarantor category (FHA, GSE, Portfolio, and Private securitized) for a total of nine regression equations.[38]

We control for local economic conditions changes during the period following the cure date, since these factors could be important in affecting a borrower's redefault risk. We include in the analysis change in the county unemployment rate and percentage change in HPI during the 18 months following the cure date, each splined at 0. We also include a set of indicators for ranges of CLTV, measured as of three months prior to the cure date, as borrowers have a stronger incentive to remain current on a mortgage the larger their equity stake in the property. We also control for borrower risk characteristics by including natural log of loan balance, Risk Score, and bankruptcy status, measured as of three months prior to the cure date.

Additional control factors included in the analysis are indicators for delinquency status one month prior to cure date; a principal modification flag, a subprime flag; and amount of interest rate reduction from modification splined at 2% and 4%. Lastly, state (property location) fixed effects are included to capture unobserved state heterogeneity across states, such as state's policies related to servicing.

[38] FHA rarely guarantees amortizing or pay-option ARM loans, and less than 1 percent of GSE loans are pay-option ARM. Therefore, we consider these to be outlier categories and exclude them from the analysis.

2.5.6. Redefault Model Results

The coefficients β_0 to β_6 indicate how likelihood of redefault for modified and self-cured loans changed over the course of the downturn, and whether the degree of convergence over time. Coefficients that are decreasing in absolute value for later vintages indicate decreasing likelihood of predicted redefault within 36 months. Note that these coefficients indicate the likelihood of redefault of each modification or self-cure vintage relative to the baseline category consisting of self-cures in 2008.

The logit regression results for predicting 36-month cumulative redefault probability are shown in Table 2.2, Table 2.3, and Table 2.4 for FRM, amortizing ARM, and option ARM, respectively. The regressions are run separately for each investor/guarantor category--FHA, GSE, privately securitized, and portfolio loans. Corresponding redefault odds ratios for each modification and self-cure vintage (relative to the baseline) are presented in Tables 2.5, 2.6, and 2.7, respectively.

Table 2.2 . Impact of FRM Loan Modification Vintage on Mortgage Performance (90+ Days Late)

Dependent variable								
Redefault flag	FHA		GSE		Private Securitized		Portfolio	
Independent variable	Coef.	Std. Error	Coef.	Std. Error	Coef.	Std. Error	Coef.	Std. Error
modified 2008	1.8693***	(0.1792)	0.3422***	(0.1186)	0.2079	(0.1379)	-0.1457	(0.2482)
modified 2009	1.8740***	(0.1575)	-0.6104***	(0.0921)	-0.3176***	(0.1224)	-0.4263**	(0.2100)
modified 2010	1.5017***	(0.1578)	-1.3287***	(0.0912)	-1.1965***	(0.1260)	-1.0453***	(0.2124)
modified 2011	1.3924***	(0.1597)	-1.6734***	(0.0942)	-1.5131***	(0.1331)	-1.2962***	(0.2213)
self-cure 2009	0.1103	(0.1544)	-0.4691***	(0.0915)	-0.0223	(0.1189)	-0.1672	(0.2043)
self-cure 2010	0.1235	(0.1556)	-1.2261***	(0.0898)	-1.0862***	(0.1216)	-1.0240***	(0.2066)
self-cure 2011	0.3633**	(0.1578)	-1.5441***	(0.0926)	-1.4782***	(0.1285)	-1.0085***	(0.2154)
(+) change in unemployment rate 1-18 months after cure	0.0052	(0.0257)	0.0961***	(0.0150)	0.1159***	(0.0248)	0.1752***	(0.0460)
(-) change in unemployment rate 1-18 months after cure	-0.0091	(0.0134)	0.0236**	(0.0093)	0.1026***	(0.0185)	0.0842***	(0.0290)
(+) % change in HPI 1-18 months after cure	-0.4550	(0.2849)	-1.2816***	(0.1921)	-2.3843***	(0.3639)	-2.1510***	(0.5944)
(-) % change in HPI 1-18 months after cure	-1.6782***	(0.2971)	0.2285	(0.2210)	0.4763	(0.4249)	-0.8993	(0.7005)
CLTV 80-90 flag (3 months prior to cure)	0.0940***	(0.0337)	0.1259***	(0.0191)	0.2392***	(0.0335)	0.3991***	(0.0562)
CLTV 90-100 flag (3 months prior to cure)	0.2991***	(0.0313)	0.2755***	(0.0205)	0.4984***	(0.0503)	0.5322***	(0.0566)
CLTV 100-110 flag (3 months prior to cure)	0.5282***	(0.0562)	0.6068***	(0.0757)	0.6348***	(0.1418)	0.8735***	(0.1104)
CLTV >110 flag (3 months prior to cure)	1.0383***	(0.2014)	0.9935***	(0.3097)	0.6702***	(0.2916)	-0.0111	(0.5184)

Table 2.2 Continued

	FHA		GSE		Private Securitized		Portfolio	
Independent variable	Coef.	Std. Error	Coef.	Std. Error	Independent variable	Coef.	Std. Error	Coef.
log balance amount (3 months prior to cure)	0.2942***	(0.0227)	-0.0087	(0.0167)	0.0205	(0.0295)	-0.0465	(0.0453)
90 to 120 dpd flag (month prior to cure)	-0.5585***	(0.1298)	-1.0405***	(0.0696)	-0.9261***	(0.0819)	-0.9359***	(0.1677)
120 to 180 dpd flag (month prior to cure)	-0.5675***	(0.1285)	-0.9508***	(0.0670)	-0.8354***	(0.0756)	-0.7717***	(0.1604)
180 to 270 dpd flag (month prior to cure)	-0.5158***	(0.1284)	-0.8729***	(0.0668)	-0.7930***	(0.0749)	-0.5108***	(0.1598)
270 to 360 dpd flag (month prior to cure)	-0.4463***	(0.1294)	-0.7216***	(0.0676)	-0.7402***	(0.0786)	-0.5499***	(0.1662)
360 to 540 dpd flag (month prior to cure)	-0.2512*	(0.1297)	-0.6081***	(0.0673)	-0.5605***	(0.0756)	-0.4630***	(0.1625)
540 to 720 dpd flag (month prior to cure)	-0.3916***	(0.1415)	-0.3873***	(0.0722)	-0.3523***	(0.0876)	-0.7879***	(0.1956)
Risk score (3 months prior to cure)	-0.0031***	(0.0001)	-0.0032***	(0.0000)	-0.0034***	(0.0001)	-0.0040***	(0.0002)
Subprime flag	0.1294***	(0.0193)	0.2415***	(0.0202)	0.1567***	(0.0317)	0.2556***	(0.0487)
Bankruptcy status (3 months prior to cure)	-0.0196	(0.0890)	0.1339*	(0.0686)	0.3163**	(0.1446)	0.7264***	(0.1989)
Principal modfication flag	-0.1505*	(0.0909)	-0.6994***	(0.0479)	-0.6572***	(0.0804)	-0.4867***	(0.1396)
Interest rate modification 0-2% spline	-0.0304	(0.0250)	0.2525***	(0.0222)	0.4235***	(0.0444)	0.3937***	(0.0678)
Interest rate modification ≥ 2 and $< 4\%$ spline	-0.2907***	(0.0587)	-0.1380***	(0.0194)	0.0926**	(0.0400)	0.0772	(0.0643)
Interest rate modification $\geq 4\%$ spline	-0.3041	(0.2140)	-0.4594***	(0.0274)	-0.2642***	(0.0292)	-0.2223***	(0.0374)
N	104,136		257,203		61,194		17,962	
Pseudo R-squared	0.1329		0.0911		0.1629		0.1469	

Notes: The dependent variable is a FRM loan re-defaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Self-cure loans can are limited to matching up to 25 unique modified loans. Self-cure loans are weighted base upon number of matches to a particular modified loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.3 Impact of Amortizing ARM Loan Modification Vintage on Mortgage Performance (90+ Days Late)

Dependent variable						
Redefault flag	GSE		Private Securitized		Portfolio	
Independent variable	Coef.	Std. Error	Coef.	Std. Error	Coef.	Std. Error
modified 2008	0.8350**	(0.4165)	0.1701*	(0.0868)	0.7615***	(0.2503)
modified 2009	-0.9988***	(0.3223)	-0.5329***	(0.0909)	-0.0468	(0.2558)
modified 2010	-1.6690***	(0.3205)	-1.0912***	(0.0951)	-0.5813**	(0.2588)
modified 2011	-1.7525***	(0.3491)	-1.2877***	(0.1106)	-0.4565	(0.3190)
self-cure 2009	-0.3580	(0.3178)	-0.4176***	(0.0843)	-0.1831	(0.2420)
self-cure 2010	-1.4739***	(0.3131)	-1.1512***	(0.0891)	-0.2954	(0.2455)
self-cure 2011	-1.9276***	(0.3410)	-1.2342***	(0.1042)	-0.3916	(0.3096)
(+) change in unemployment rate 1-18 months after cure	0.1420**	(0.0678)	0.0696***	(0.0193)	0.1155**	(0.0557)
(-) change in unemployment rate 1-18 months after cure	0.0238	(0.0390)	0.1033***	(0.0202)	0.0145	(0.0496)
(+) % change in HPI 1-18 months after cure	-1.5813*	(0.8919)	-1.9540***	(0.4118)	-3.6520***	(1.3397)
(-) % change in HPI 1-18 months after cure	-0.593	(0.9727)	-1.6033***	(0.3965)	-0.9033	(1.0742)
CLTV 80-90 flag (3 months prior to cure)	0.1742**	(0.0774)	0.1328***	(0.0327)	0.3479***	(0.0901)
CLTV 90-100 flag (3 months prior to cure)	0.3019**	(0.1179)	0.2407***	(0.0473)	0.7372***	(0.1269)
CLTV 100-110 flag (3 months prior to cure)	1.5338***	(0.3973)	0.3655***	(0.1092)	1.0163**	(0.4070)
CLTV >110 flag (3 months prior to cure)	0.3840	(1.2828)	0.5527***	(0.1439)	0.2219	(0.3797)
log balance amount (3 months prior to cure)	0.2317**	(0.1049)	0.1615***	(0.0333)	-0.1620*	(0.0885)
90 to 120 dpd flag (month prior to cure)	-1.0141***	(0.2826)	-0.3767***	(0.0825)	-1.3551***	(0.3452)
120 to 180 dpd flag (month prior to cure)	-0.8511***	(0.2695)	-0.4012***	(0.0748)	-1.2617***	(0.3339)
180 to 270 dpd flag (month prior to cure)	-0.9188***	(0.2700)	-0.2905***	(0.0731)	-1.3389***	(0.3327)

Table 2.3 Continued

	GSE		Private Securitized		Portfolio	
Independent variable	Coef.	Std. Error	Coef.	Std. Error	Coef.	Std. Error
270 to 360 dpd flag (month prior to cure)	-0.6629**	(0.2718)	-0.0822	(0.0759)	-0.8754***	(0.3390)
360 to 540 dpd flag (month prior to cure)	-0.3788	(0.2690)	-0.3267***	(0.0731)	-0.9988***	(0.3372)
540 to 720 dpd flag (month prior to cure)	-0.3842	(0.2958)	-0.1618*	(0.0830)	-0.9995***	(0.3663)
Risk score (3 months prior to cure)	-0.00328**	(0.0003)	-0.0037***	(0.0001)	-0.0031***	(0.0004)
Subprime flag	0.3157**	(0.1400)	0.0973***	(0.0332)	0.3936***	(0.1073)
Bankruptcy status (3 months prior to cure)	-0.2925	(0.3787)	0.3556***	(0.1303)	0.5513	(0.3545)
Principal modfication flag	-1.1619***	(0.1552)	-0.8803***	(0.0813)	-0.5628***	(0.2168)
Interest rate modification 0-2% spline	0.3254***	(0.1117)	0.2003***	(0.0482)	0.3112**	(0.1373)
Interest rate modification ≥ 2 and < 4% spline	-0.2128**	(0.0833)	0.0615	(0.0420)	0.1662	(0.1099)
Interest rate modification ≥4% spline	-0.5253***	(0.1131)	-0.1680***	(0.0233)	-0.1761***	(0.0628)
N	13,474		54,041		5,872	
Pseudo R-squared	0.1472		0.1603		0.1838	

Notes: The dependent variable is a Amortizing ARM loan redefaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Self-cure loans are limited to matching up to 25 unique modified loans. Self-cure loans are weighted base upon number of matches to a particular modified loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.4 Impact of Option ARM Loan Modification Vintage on Mortgage Performance (90+ Days Late)

Dependent variable				
Redefault flag	Private Securitized		Portfolio	
Independent variable	Coef.	Std. Error	Coef.	Std. Error
modified 2008	1.8430***	(0.2744)	0.3079	(0.4078)
modified 2009	1.0255***	(0.2516)	-0.4501	(0.3795)
modified 2010	0.1152	(0.2648)	-0.9125**	(0.3741)
modified 2011	0.0262	(0.2952)	-1.5304***	(0.4058)
self-cure 2009	1.4161***	(0.2372)	0.1777	(0.3619)
self-cure 2010	-0.0840	(0.2466)	-0.3026	(0.3502)
self-cure 2011	0.2122	(0.2750)	-1.9490***	(0.3813)
(+) change in unemployment rate 1-18 months after cure	0.2853***	(0.0567)	0.1974*	(0.1028)
(-) change in unemployment rate 1-18 months after cure	-0.0366	(0.0520)	-0.1096	(0.0916)
(+) % change in HPI 1-18 months after cure	-3.2789***	(0.6716)	-0.1574	(1.1520)
(-) % change in HPI 1-18 months after cure	-3.4266***	(1.1105)	0.7129	(1.8213)
CLTV 80-90 flag (3 months prior to cure)	0.0039	(0.0718)	0.2951**	(0.1161)
CLTV 90-100 flag (3 months prior to cure)	0.4038***	(0.1358)	0.4801**	(0.1895)
CLTV >100 flag (3 months prior to cure)			0.3149	(1.0229)
log balance amount (3 months prior to cure)	0.1696*	(0.0876)	0.1565	(0.1104)
90 to 120 dpd flag (month prior to cure)	-0.4508**	(0.1959)	0.2723	(0.4301)
120 to 180 dpd flag (month prior to cure)	-0.7357***	(0.1628)	0.3490	(0.3998)
180 to 270 dpd flag (month prior to cure)	-0.5519***	(0.1585)	0.2609	(0.3990)

Table 2.4 Continued

	Private Securitized		Portfolio	
Independent variable	Coef.	Std. Error	Coef.	Std. Error
270 to 360 dpd flag (month prior to cure)	-0.4606***	(0.1595)	0.8554**	(0.4208)
360 to 540 dpd flag (month prior to cure)	-0.4338***	(0.1513)	0.6380	(0.3937)
540 to 720 dpd flag (month prior to cure)	-0.5748***	(0.1766)	0.2936	(0.4155)
Risk score (3 months prior to cure)	-0.0046***	(0.0004)	-0.0026***	(0.0006)
Subprime flag	0.2817**	(0.1240)	0.3899*	(0.2082)
Bankruptcy status (3 months prior to cure)	-0.0771	(0.3230)	0.1911	(0.4441)
Principal modfication flag	-1.1652***	(0.1515)	-0.8516***	(0.2307)
Interest rate modification 0-2% spline	-0.0894	(0.1008)	-0.1552	(0.1818)
Interest rate modification ≥ 2 and < 4% spline	-0.1373	(0.1090)	-0.2690	(0.1794)
Interest rate modification ≥4% spline	-0.324***	(0.0900)	-0.1749	(0.1422)
N	11,361		2,478	
Pseudo R-squared	0.2312		0.2295	

Notes: The dependent variable is a option ARM loan redefaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Self-cure loans are limited to matching up to 25 unique modified loans. Self-cure loans are weighted base upon number of matches to a particular modified loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. †There are no observations with CLTV values greater than 120. Results are robust to alternative CLTV bucket combinations.

Table 2.5 FRM Redefault Odds Ratios

Vintage redefault odds ratio (Base case is self-cure 2008)	FHA		GSE		Private Securitized		Portfolio	
	modified	self-cure	modified	self-cure	modified	self-cure	modified	self-cure
2008	6.484	1.000	1.408	1.000	1.231	1.000	0.864	1.000
2009	6.514	1.117	0.543	0.626	0.728	0.978	0.653	0.846
2010	4.489	1.131	0.265	0.293	0.302	0.337	0.352	0.359
2011	4.025	1.438	0.188	0.213	0.220	0.228	0.274	0.365

Table 2.6 Amortizing ARM Redefault Odds Ratios

Vintage redefault odds ratio (Base case is self-cure 2008)	FHA		GSE		Private Securitized	
	modified	self-cure	modified	self-cure	modified	self-cure
2008	2.305	1.000	1.185	1.000	2.142	1.000
2009	0.368	0.699	0.587	0.659	0.954	0.833
2010	0.188	0.229	0.336	0.316	0.559	0.744
2011	0.173	0.145	0.276	0.291	0.633	0.676

Table 2.7 Option ARM Redefault Odds Ratios

Vintage redefault odds ratio (Base case is self-cure 2008)	FHA		GSE	
	modified	self-cure	modified	self-cure
2008	6.316	1.000	1.361	1.000
2009	2.789	4.121	0.638	1.195
2010	1.122	0.919	0.401	0.739
2011	1.027	1.236	0.216	0.142

The “learning-by-doing” hypothesis is clearly reflected in the estimated coefficients of the modification and cure vintage variables, and in the corresponding odds ratio results shown in Tables 2.5 to 2.7. These odds ratios indicate the likelihood that a self-cure or modification vintage will redefault, relative to the odds ratio of the 2008 self-cure vintage (the baseline case.) For example, in Table 3B for GSE loans, the odds ratio of the 2008 self-cure vintage (against itself) is 1.0, and the odds ratio of the modified 2008 vintage is 1.408, indicating that the GSE fixed-rate mortgages modified in 2008 are 1.408 times more likely to redefault within 367 months of cure (via modification) compared to loans that self-cured in 2008. Overall, we observe significantly higher likelihood of redefault for loans modified in 2008 compared to 2008 self-cures. Consistent with our observations from Figures 2-9 to 2-18 and with the learning-by-doing hypothesis, the gaps narrow over time. Differences in redefault probability between loans modified and self-cured in 2009 is substantially smaller than in 2008, and no significant gaps remain after 2009, with the exception of FHA loans.

The control factors are also mostly significant and have directional effects consistent with economic intuition. For example, for changes in unemployment rate indicators, coefficients are positive for both unemployment increase and unemployment decrease indicator coefficients, indicating that probability of redefault increases with a rise in the unemployment rate and decreases with a decline in the unemployment rate. Likelihood of redefault responds asymmetrically to the change in unemployment rate – the default rate is more responsive to a rising unemployment rate increase compared to improving employment conditions. Loans with higher CLTV are associated with greater likelihood of redefault. When home prices rise (as indicated by an increase in the local HPI), the probability of redefault declines. Specifying unemployment and HPI as directional changes rather than actual changes simplifies the estimations interpretation. As expected, borrowers with higher risk score (as of 3 months before cure) are less likely to redefault and subprime borrowers are more likely to redefault. Consistent with previous studies, we find that principal reduction modification is associated with smaller redefault risk than other types of modification. We do not find a consistent relationship between the size of an interest rate reduction and likelihood of redefault.

2.6. Robustness

We perform a variety of robustness tests with additional results. First, we limit the sample to only borrowers that became delinquent on at least one consumer finance, credit card, or other consumer loan, within plus or minus six months of becoming 90 days past due on the first lien mortgage.[39] This restriction reduces the sample size by 25 percent.[40] Again, this is to ensure that the borrower's becoming delinquent on the mortgage prior to

[39] These other consumer loans are any loans that do not fall under the following categories: auto, first mortgage, second lien, or student loans.

[40] The credit account delinquency condition removes borrowers across all matching criteria categories, which will cause the weighting scheme used in the analysis to be incorrect. To remedy the incorrect weights we re-estimate the propensity score model with the reduced sample, re-match the data, and estimate the redefault model on the reduced sample. Both the initial delinquent account restriction and the matching process reduce the sample size.

receiving a loan modification or to self-curing was due to financial stress and not to strategic default behavior. The results are presented in Tables 2.8,2.9, and 2.10 for FRM, amortizing ARM, and option ARM, respectively. Overall, the results confirm the "learning-by-doing" pattern discussed earlier.

Table 2.8 Impact of FRM Loan Modification Vintage on Mortgage Performance (90+ Days Late) without strategic behavior

Dependent variable				
Redefault flag	FHA	GSE	Private Securitized	Portfolio
Independent variable	Coef	Coef	Coef	Coef
modified 2008	1.8856***	0.4447***	0.2164	0.1392
	(0.1943)	(0.1342)	(0.1609)	(0.3219)
modified 2009	1.9545***	-0.4770***	-0.2893**	-0.3693
	(0.1687)	(0.1032)	(0.1428)	(0.2563)
modified 2010	1.5366***	-1.1648***	-1.2303***	-0.9160***
	(0.1690)	(0.1024)	(0.1475)	(0.2591)
modified 2011	1.4358***	-1.5214***	-1.6253***	-1.3107***
	(0.1710)	(0.1057)	(0.1566)	(0.2675)
self-cure 2009	0.1446	-0.3862***	0.0123	0.0629
	(0.1652)	(0.1025)	(0.1393)	(0.2510)
self-cure 2010	0.1734	-1.1243***	-1.0841***	-0.8616***
	(0.1666)	(0.1009)	(0.1428)	(0.2526)
self-cure 2011	0.4130**	-1.4256***	-1.5453***	-0.8163***
	(0.1690)	(0.1040)	(0.1511)	(0.2608)
N	88,795	186,170	45,141	12,548
Pseudo R-squared	0.0735	0.0893	0.1642	0.148

Notes: The dependent variable is a FRM loan redefaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Additional controls include a constant and quarter of cure indicators. The sample includes loans with borrowers that became at least 60+ DPD on a non-mortgage, non-auto, or non-student loan within +/- 6 months of becoming 90+DPD delinquent on a first mortgage. Self-cure loans can are limited to matching up to 25 unique modified loans. Self-cure loans are weighted base upon number of matches to a particular modified loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.9 Impact of Amortizing ARM Loan Modification Vintage on Mortgage Performance (90+ Days Late) without strategic behavior

Dependent variable			
Redefault flag	GSE	Private Securitized	Portfolio
Independent variable	Coef.	Coef.	Coef.
modified 2008	0.96261*	0.96261*	0.40183
	(0.53894)	(0.53894)	(0.35550)
modified 2009	-1.07224***	-1.07224***	-0.21477
	(0.39402)	(0.39402)	(0.33867)
modified 2010	-1.81198***	-1.81198***	-0.60780*
	(0.39286)	(0.39286)	(0.34291)
modified 2011	-1.98901***	-1.98901***	-0.18236
	(0.42942)	(0.42942)	(0.41371)
self-cure 2009	-0.50395	-0.50395	-0.19279
	(0.38917)	(0.38917)	(0.31901)
self-cure 2010	-1.76921***	-1.76921***	-0.35104
	(0.38401)	(0.38401)	(0.32782)
self-cure 2011	-2.22117***	-2.22117***	-0.52237
	(0.41966)	(0.41966)	(0.41047)
N	9,061	35,253	3,670
Pseudo R-squared	0.1657	0.1536	0.1892

Notes: The dependent variable is a Amortizing ARM loan re-defaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. The sample includes loans with borrowers that became at least 60+ DPD on a non-mortgage, non-auto, or non-student loan within +/- 6 months of becoming 90+DPD delinquent on a first mortgage. Self-cure loans are limited to matching up to 25 unique modified loans. Self-cure loans are weighted base upon number of matches to a particular modified loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.10 Impact of Option ARM Loan Modification Vintage on Mortgage Performance (90+ Days Late) without strategic behavior

Dependent variable			
Redefault flag	GSE	Private Securitized	Portfolio
Independent variable	Coef.	Coef.	Coef.
modified 2008	0.96261*	0.96261*	0.40183
	(0.53894)	(0.53894)	(0.35550)
modified 2009	-1.07224***	-1.07224***	-0.21477
	(0.39402)	(0.39402)	(0.33867)
modified 2010	-1.81198***	-1.81198***	-0.60780*
	(0.39286)	(0.39286)	(0.34291)
modified 2011	-1.98901***	-1.98901***	-0.18236
	(0.42942)	(0.42942)	(0.41371)
self-cure 2009	-0.50395	-0.50395	-0.19279
	(0.38917)	(0.38917)	(0.31901)
self-cure 2010	-1.76921***	-1.76921***	-0.35104
	(0.38401)	(0.38401)	(0.32782)
self-cure 2011	-2.22117***	-2.22117***	-0.52237
	(0.41966)	(0.41966)	(0.41047)
N	9,061	35,253	3,670
Pseudo R-squared	0.1657	0.1536	0.1892

Notes: The dependent variable is a Amortizing ARM loan re-defaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Self-cure loans are limited to matching up to 25 unique modified loans. Self-cure loans are weighted base upon number of matches to a particular modified loan. The sample includes loans with borrowers that became at least 60+ DPD on a non-mortgage, non-auto, or non-student loan within +/- 6 months of becoming 90+DPD delinquent on a first mortgage. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Second, we restrict the sample to borrowers who became at least 120 days past due prior to self-curing or receiving a loan modification. This restriction reduces the sample size by 15 percent.[41] The reason for this restriction is to limit the presence of borrowers who may have defaulted strategically in order to become eligible for a loan modification, such as in Mayer et. al (2014) and Jagtiani and Lang (2011). If the frequency of such strategic behavior increased over the course of the downturn, then it could explain the declining redefault rates on modifications. We believe the likelihood of such strategic behavior is small overall, and is greatest when borrowers perceive modifications to be obtainable quickly. That is, likelihood of such strategic behavior declines the longer the wait for a modification, because of the accumulating stress and the increasing risk of losing the home to foreclosure. By restricting the sample to more severely delinquent borrowers, we mitigate the likelihood of strategic behavior.[42]

The results with this more narrowly defined sample are presented in Tables 2.11, 2.12, and 2.13 for FRM, amortizing ARM, and option ARM, respectively. Again, the results are consistent with our previous findings.

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[41] Since loans are matched based on delinquency status, the weighting scheme used in the analysis will be unaffected.

[42] The Countrywide program studied by Mayer et. al likely encouraged strategic default, as it became widely publicized that borrowers were eligible for modification once they became 60 days or more past due.

Table 2.11 FRM Impact of FRM Loan Modification Vintage on Mortgage Performance (90+ Days Late) with 120+ DPD delinquency robustness.

Dependent variable				
Redefault flag	FHA	GSE	Private Securitized	Portfolio
Independent variable	Coef.	Coef.	Coef.	Coef.
modified 2008	0.5671***	0.3049**	0.3403**	-0.58660*
	(0.2046)	(0.1407)	(0.1688)	-0.3488
modified 2009	0.9012***	-0.6267***	-0.2768*	-0.7714***
	(0.1783)	(0.1071)	(0.1450)	-0.2978
modified 2010	0.5411***	-1.3811***	-1.1630***	-1.2709***
	(0.1787)	(0.1062)	(0.1487)	-0.2989
modified 2011	0.4053**	-1.7403***	-1.5275***	-1.5603***
	(0.1806)	(0.1089)	(0.1559)	-0.307
self-cure 2009	0.2863	-0.5648***	0.0004	-0.3440
	(0.1747)	(0.1066)	(0.1421)	-0.2955
self-cure 2010	0.3253*	-1.3229***	-1.0979***	-1.2438***
	(0.1763)	(0.1048)	(0.1447)	-0.2941
self-cure 2011	0.1287	-1.6243***	-1.5606***	-1.2517***
	(0.1785)	(0.1074)	(0.1513)	-0.3022
N	82,106	221,700	50,460	14,286
Pseudo R-squared	0.0862	0.0881	0.1670	0.1450

Notes: The dependent variable is a FRM loan redefaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Additonal controls include a constant and quarter of cure indicators. Self-cure loans can are limited to matching up to 25 unique modified loans. Self-cure loans are weighted base upon number of matches to a particular modified loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.12 Impact of Amortizing ARM Loan Modification Vintage on Mortgage Performance (90+ Days Late) with 120+ DPD delinquency robustness.

Dependent variable			
Redefault flag	GSE	Private Securitized	Portfolio
Independent variable	Coef.	Coef.	Coef.
modified 2008	0.8625*	0.2143**	0.4510
	(0.4911)	(0.0988)	(0.3216)
modified 2009	-0.7888**	-0.5501***	-0.0191
	(0.3789)	(0.1016)	(0.3074)
modified 2010	-1.4460***	-1.1145***	-0.4655
	(0.3753)	(0.1054)	(0.3018)
modified 2011	-1.5308***	-1.3217***	-0.2048
	(0.4032)	(0.1208)	(0.3647)
self-cure 2009	-0.0888	-0.4491***	-0.1564
	(0.3753)	(0.0949)	(0.2926)
self-cure 2010	-1.3751***	-1.2026***	-0.1536
	(0.3688)	(0.0993)	(0.2887)
self-cure 2011	-1.7007***	-1.2971***	-0.27724
	(0.3957)	(0.1144)	(0.3567)
N	12,436	47,126	5,019
Pseudo R-squared	0.1399	0.1657	0.1852

Notes: The dependent variable is a Amortizing ARM loan re-defaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Self-cure loans are limited to matching up to 25 unique modified loans. Self-cure loans are weighted base upon number of matches to a particular modified loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.13 Impact of Option ARM Loan Modification Vintage on Mortgage Performance (90+ Days Late) with 120+ DPD delinquency robustness.

Dependent variable		
Redefault flag	Private Securitized	Portfolio
Independent variable	Coef.	Coef.
modified 2008	2.00094***	0.31875
	(0.30918)	(0.46728)
modified 2009	1.17769***	-0.48431
	(0.27230)	(0.43028)
modified 2010	0.26289	-0.79165*
	(0.28385)	(0.41755)
modified 2011	0.12247	-1.37115***
	(0.31526)	(0.45013)
self-cure 2009	1.56441***	0.31561
	(0.25933)	(0.41702)
self-cure 2010	0.02107	-0.14441
	(0.26581)	(0.39618)
self-cure 2011	0.29861	-1.87164***
	(0.29496)	(0.42600)
N	10,387	2,245
Pseudo R-squared	0.213	0.2391

Notes: The dependent variable is a option ARM loan re-defaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Self-cure loans are limited to matching up to 25 unique modified loans. Self-cure loans are weighted base upon number of matches to a particular modified loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

As a final robustness check, we perform a different match-pair process on the modified and self-cured loans. The baseline model has weighting scheme that assigns a weight of 1 on each modified loan. Weights assigned to self-cure loans are equal to the inverse of the number of self-cure loans matched to the same modified loan. We now reverse the matching and weighting process by matching each self-cured loan to multiple modified loans, so that self-cured loans are assigned a weight of 1, whereas each modified loan is given a weight equal to the inverse of the number modified loans matched to the same self-cure.

The results using the new weighting scheme are presented in Tables 2.14, 2.15, and 2.16 for FRM, amortizing ARM, and option ARM, respectively. The results confirm our earlier findings.[43]

[43] Lin et al., (2013) point out that large samples can cause a researcher to claim support for results with no practical significance when relying on p-values. We address this concern by estimating the results on several random subsamples of the data, and do not find any significant changes in results.

Table 2.14 Impact of FRM Loan Modification Vintage on Mortgage Performance (90+ Days Late) Self-Cure Treatment

Dependent variable				
Redefault flag	FHA	GSE	Private Securitized	Portfolio
Independent variable	Coef.	Coef.	Coef.	Coef.
modified 2008	1.78641***	0.57938***	0.26775**	-0.11576
	(0.09981)	(0.07575)	-0.1258	(0.22964)
		-		
modified 2009	1.97731***	0.29453***	-0.17444	-0.25085
	(0.12123)	(0.08097)	-0.13623	(0.21599)
		-	-	-
modified 2010	1.48351***	1.13492***	1.15456***	1.05319***
	(0.12167)	(0.08486)	-0.14693	(0.22342)
		-	-	-
modified 2011	1.34147***	1.41300***	1.29653***	1.22697***
	(0.12290)	(0.09007)	-0.156	(0.23412)
		-		
self-cure 2009	0.05043	0.20661***	0.14052	-0.17082
	(0.11047)	(0.07961)	-0.12891	(0.20309)
		-	-	-
self-cure 2010	0.18736	0.96362***	0.97167***	0.92741***
	(0.11228)	(0.08013)	-0.13792	(0.21246)
		-	-	-
self-cure 2011	0.37020***	1.22113***	1.28151***	0.93335***
	(0.11463)	(0.08516)	-0.14643	(0.22332)
N	127,764	477,753	82,446	16,225
Pseudo R-squared	0.0569	0.1214	0.1755	0.1391

Notes: The dependent variable is a FRM loan re-defaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Modified loans can are limited to matching up to 25 unique self-cure loans. Modified loans are weighted base upon number of matches to a particular self-cure loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.15 Impact of Amortizing ARM Loan Modification Vintage on Mortgage Performance (90+ Days Late) Self-Cure Treatment

Dependent variable			
Redefault flag	GSE	Private Securitized	Portfolio
Independent variable	Coef.	Coef.	Coef.
modified 2008	0.47274	0.47274	0.53553
	(0.40211)	(0.40211)	(0.34089)
modified 2009	-0.36324	-0.36324	-0.2559
	(0.37333)	(0.37333)	(0.37629)
modified 2010	-1.23634***	-1.23634***	-1.15570***
	(0.38537)	(0.38537)	(0.39173)
modified 2011	-1.38013***	-1.38013***	-0.87200*
	(0.43572)	(0.43572)	(0.47563)
self-cure 2009	-0.15064	-0.15064	-0.46429
	(0.36366)	(0.36366)	(0.35557)
self-cure 2010	-1.11604***	-1.11604***	-0.53546
	(0.36916)	(0.36916)	(0.36650)
self-cure 2011	-1.66479***	-1.66479***	-0.81652*
	(0.42030)	(0.42030)	(0.45763)
N	16,997	62,338	5,145
Pseudo R-squared	0.165	0.16	0.1734

Notes: The dependent variable is a Amortizing ARM loan re-defaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Modified loans are limited to matching up to 25 unique self-cure loans. Modified loans are weighted base upon number of matches to a particular self-cure loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.16 Impact of Option ARM Loan Modification Vintage on Mortgage Performance (90+ Days Late) Self-Cure Treatment

Dependent variable		
Redefault flag	Private Securitized	Portfolio
Independent variable	Coef.	Coef.
modified 2008	0.3850	0.5080
	(0.4028)	(0.6211)
modified 2009	-0.6445	-0.1233
	(0.3971)	(0.6412)
modified 2010	-1.7421***	-0.0879
	(0.4303)	(0.6870)
modified 2011	-1.5216***	-0.9818
	(0.4781)	(0.7545)
self-cure 2009	-0.1928	0.2914
	(0.3600)	(0.5910)
self-cure 2010	-1.3163***	0.2289
	(0.3923)	(0.6527)
self-cure 2011	-1.3609***	-0.4847
	(0.4403)	(0.7093)
N	12,969	1,968
Pseudo R-squared	0.2413	0.2513

Notes: The dependent variable is a option ARM loan re-defaulting (i.e., delinquency becomes 90+DPD) within 36 months of curing. The models include state fixed effects. Modified loans are limited to matching up to 25 unique self-cure loans. Modified loans are weighted base upon number of matches to a particular self-cure loan. The ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

These results support the view that learning by-doing process has been an important factor influencing the significant decline in redefault rates of modified loans in recent years. Moreover, we believe that the learning-by-doing interpretation prevails over alternative explanations suggested in other studies, which include the use of multiple modifications, increased use of principal modifications, and servicers performing modification earlier in the delinquency process.

In response to the findings in Goodman et. al (2011) whereby declining redefault rates during 2008-2011 reflect an increasing number of loans experiencing multiple modifications, we find that the frequency of re-modification was extremely low (less than 3.5% of the sample each year.)[44] Therefore, the incidence of re-modification is unlikely to have played a significant role in the observed decline in redefault rate after modification.

[44] We define a re-modification as a loan receives a 20% change in payment in month while remaining non-delinquent. Even when the amount of payment change is decreased to less than 20% the proportion of loans that identify as re-modified does not change significantly.

Further, Goodman et al. (2012) attribute the improving performance of loan modifications to an increased share of principal modifications, which perform better than other types of modifications. We specifically control for principal modification incidence using a principal modification dummy. We find that principal modification is associated with lower redefault risk, consistent with Goodman et. al (2012), but we still find significant effects of modification vintage even after controlling for principal modification.

Finally, Ashworth, Landy, and Yang (2012) and Goodman, Yang, Landy, and Ashworth (2013) argue that the decline in redefault rate after reflects modifications occurring at earlier stages of delinquency in the later vintages. However, our analysis controls for the time period between becoming delinquent and when the modification occurs, so that the learning-by-doing effects are not attributable to accelerated modification.

2.7. Conclusion

A loan modification is intended to improve a borrower's financial circumstances in a manner that makes future default on mortgages less likely. However, for some borrowers, mortgage modification only delays the inevitable redefault. Historically, redefault rates of modified loans have been quite high, leading to a perception that loan modifications are not cost effective. However, over the course of the recent downturn, performance of modified loans improved dramatically. There has been a significant decline in redefault rates for those that cured after a modification.

In this paper, we examine why the mortgage redefault rate has declined for those that cured through mortgage modification (we focus on modifications that reduce monthly mortgage payment only). Our analysis controls for observable factors that are likely to influence a loan's likelihood of redefault following modification, and additionally utilizes a matched sample of self-cured loans as a control group.

The results suggest that the decline in redefault rate after modification has been driven at least partially by the changing behavior on the lender/servicer's side. That is, servicers have become more experienced through a large number of mortgage modification that took place during the financial crisis, and they changed the way they select which mortgages to modify (they have changed the criteria for modification selection). We believe that this "learning-by-doing" process, wherein servicers learned from past mistakes and successes, best explains this phenomenon.

Our findings imply that public policies associated with housing finance or home ownership support could achieve the goals more effectively when there are more opportunities for lenders and mortgage servicers to learn from not only their own past mistakes and successes but also the overall experience from the mortgage industry. Increased transparency and information dissemination around loss mitigation strategy could potentially enhance the overall mortgage performance and home ownership.

Our analysis also suggests an avenue for further research around this issue. It would be valuable from the public policy standpoint whether mortgage modifications were "worth it" from the lenders' and borrowers' perspectives as well as from the perspectives of the overall economy. A more in-depth cost-benefit analysis and welfare analysis could potentially provide additional insights on several policy questions, such as whether home ownership should continue to be subsidized, whether mortgage modification is the most effective loss mitigation tool, who should bear the cost of such modification, and what else can be done to lower mortgage redefault rate even further.

CHAPTER 3. AN EMPIRICAL INVESTIGATION BETWEEN BANK COMPETITION AND BANK CAPITAL

3.1. Introduction

Modern banking involves leveraging assets to equity, which creates an incentive for excessive risk-taking behavior. A substantial difference exists in financial leverage between banks and non-financial companies. Namely, banks' median book leverage is 92.7% and median market leverage is 88.8%, while median book and market leverage of non-financial companies in Frank and Goyal (2009) is 24% and 23%, respectively. Not only do banks operate under higher leverage ratios, they are highly interconnected into the financial system and the transactions of the economy. This implies the failure of a bank can have much larger economic welfare impacts than the failure of a non-financial firm.

To reduce bank risk behavior, policymakers impose regulatory requirements on the minimum level of capital that banks must hold. In the U.S., regulators such as the Federal Deposit Insurance Corporation (FDIC), Federal Reserve, and Office of the Comptroller of Currency (OCC) follow the Basel Accords set by the Basel Committee.[45]

[45] The Basel Committee is an international organization that includes representatives from 28 countries. Their mission is to increase financial and monetary stability, discuss financial issues with central banks and financial community, and act as a bank to central banks and international organizations. See http://www.bis.org/index.htm for more information.

The three pillars of Basel aim to ensure sufficient capital regulation, bank supervision, and market discipline to reach the goal of more stable financial markets. Capital regulation is the main focus for regulators in reporting and policy. The minimum capital requirement in the U.S. is 4% for Tier 1 capital and 8% for total capital.[46] The FDIC, Federal Reserve, and OCC enforce the Basel accords.

Banks' push-back against capital regulation requirements consists of arguments that maintaining high levels of capital impedes their ability to compete because equity is more costly than debt.[47] If this were true, profit-maximizing banks would operate at levels of capitalization that do not exceed regulatory minimum requirements. However, observed average capital ratios tended to significantly exceed minimum requirements from 2001 to 2013, see Figure 3-1. Flannery and Rangan (2008) report U.S. banks held capital up to 75% above regulatory requirements. Barth et al. (2006) find similar excess capital holdings for banks in other countries. Ashcraft (2001) concludes that there is a lack of evidence for significant impacts from regulation on capital holdings. The lack of compelling evidence for any effect of regulation on bank capital suggests determinants other than regulation explain these findings.

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[46] The U.S. regulatory minimums are 4% (as a percentage of total risk weighted assets) for Tier 1 capital and 8% (as a percentage of total risk weighted assets) for the sum of Tier 1 and Tier 2 capital, or total capital. Tier 1 capital includes common stock, disclosed reserves (retained earnings), and non-redeemable non-cumulative preferred stock. Tier 2 capital is supplementary bank capital such as revaluation reserves, undisclosed reserves, hybrid instruments and subordinated term debt.

[47] See Modigliani and Miller (1958).

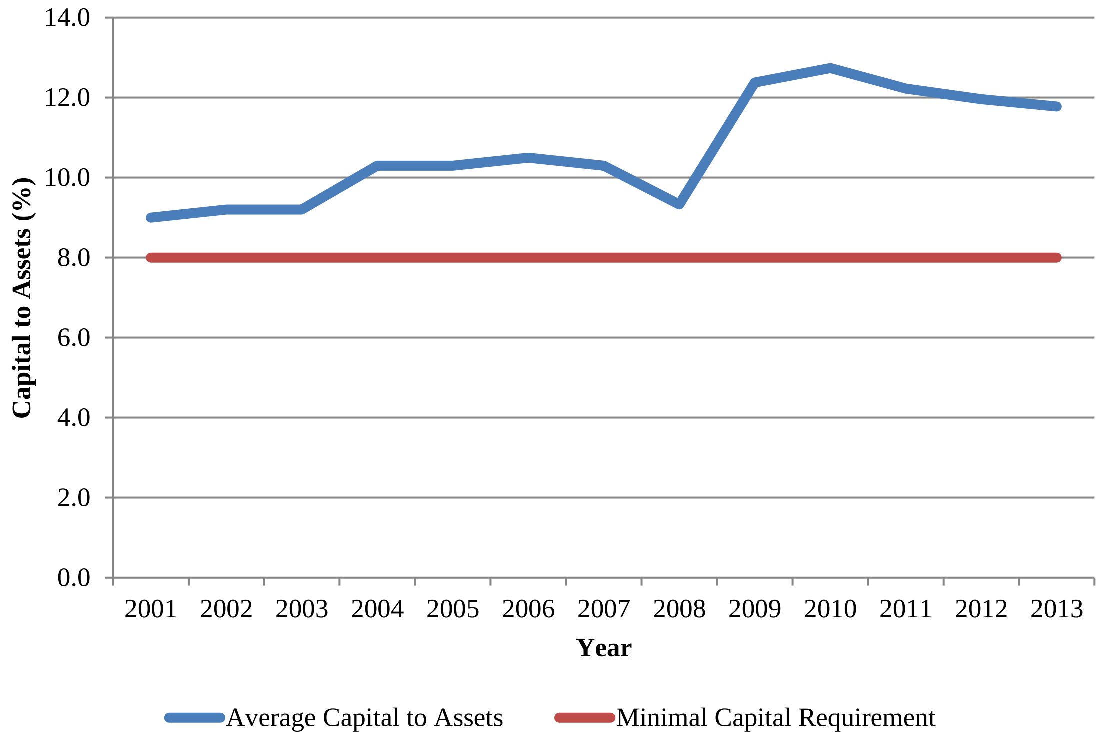

Figure 3-1 Bank Capital to Assets

Allen et al. (2011) offer an alternative explanation for observed excess capital holdings. They propose that excessive capital holdings above capital requirements derive from market discipline on banks’ asset side. According to the model, competition motivates banks to hold higher levels of capital as a commitment signal to monitoring, which attracts creditworthy borrowers. Additionally, capitalization levels impact the type of lending behavior.

In this paper, I use a large sample of 9,792 banks across the U.S. to offer unique contributions to the bank capital literature. First, I am the first to conduct an empirical test of the theory put forward by Allen et al. (2011) that competition drives capital ratios for U.S banks. I examine if there is robust evidence before, during, and after the most recent financial crisis of a positive relationship between competition and higher capital. Second, I use measures of competition that control for local market concentrations and competitive behavioral conduct. Third, I test the heterogeneous impact of competitive conduct between small, medium, and large banks. The sample of small banks contains institutions with less than or equal to $1 billion (bn) in total assets. Banks with total assets greater than $1 billion, but less than $3 billion are categorized as medium-sized banks. Large banks are those with at least $3 billion worth of assets. Finally, I compare my results with Schaeck and Cihak (2012), a cross-country study of the competition capital relationship in the pre-crisis period.

This research is crucial for policy making post-financial crisis. If competition incentivizes (or disincentivizes) a capital safety net, knowledge of the directional impact can inform debate about how to improve regulation and policy responses since the recent crisis. Thus, if the positive relationship put forth in Allen et al. (2011) holds, focusing regulation on policy encouraging competition in the banking sector would be advisable.

The results of all these analyses are as follows. More intense competition is associated with higher bank capital ratios before, during, and after the financial crisis for small medium, and large banks. The smaller banks also hold more capital than medium and large banks. Competition changes have a larger impact on capital ratios during the financial crisis. The results are robust whether competition is defined in terms of conduct (H-statistic) or concentration (HHI).

The remainder of this paper is organized as follows. Section 3.2 outlines the relevant literature. Section 3.3 describes the data and the variables and provides summary statistics. Section 3.4 explains the identification strategy and empirical considerations. Section 3.5 discusses the main results based on bank size and time period relative to the financial crisis. Section 3.6 includes the robustness tests. Section 3.7 discusses the economic interpretation of the results. Section 3.8 concludes the paper.

3.2. Data Sources

The bank-specific data consist of quarterly call reports from 2001:Q1 through 2013:Q4 (52 quarters) filed by FDIC-insured U.S. commercial banks.[48] The panel, which is unbalanced due to mergers, acquisitions, entry, and exit of banks, is collected from the Wharton Research Data Services (WRDS). The panel initially contains 11,403 individual banks. All monetary values are converted to real terms using CPI data.[49] All variables

[48] The panel begins in 2001 due to limited availability of certain call report data relating to Tier and Tier 2 capital prior to that year.

[49] All variables are in 2013:Q1 dollars for convenience. Note with the exception of the size variable, all variables are derived as a ratio, therefore the specific base year chosen for real terms is irrelevant.

from the income statement are annualized to avoid bias based on quarter.[50] To reduce outlier bias, all observations with continuous variables that lie above the 99th percentile or below the 1st percentile are removed from the sample.[51] I exclude any observation missing relevant variables.[52] The banking concentration data is an end of Q2 measure. I measure all of the bank variables at the end of Q2 each year to be consistent with this banking measure. After applying the above filters, a total of 9,792 banks remain in the panel providing 72,326 annual observations.

As stated earlier, the sample of small banks contains institutions with less than or equal to $1 billion in total assets. Banks with total assets greater than $1 billion, but less than $3 billion are categorized as medium-sized banks. Large banks are those with at least $3 billion in assets. Berger and Bouman (2013) and Kashyup et. al. (2002) provide convincing evidence that bank lending and borrowing behavior differs substantially enough across bank size to warrant the need to control for heterogeneous size impacts of

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[50] Income variables are annualized because income is measured cumulatively over the year in the income section of a call report. For example, net income in 2002:Q3 is the sum of all net income earned from Jan. 1st, 2002 to Sept. 30th, 2002. Thus, if income is non-negative every quarter, the net income reported in the call report for Q3 is always greater than in Q1 or Q2. Annualization removes this problem.

[51] Those observations above the 99th percentile or below the 1st percentile had ratios measured in hundreds, thousands, negative hundreds, or negative thousands. Due to the point-in-time nature of a call report, extreme values will exist in the data. These banks were most likely either in the process of a (rare) disastrous failure, or major balance sheet alterations at the time of submitting a call report. These outlying values do not accurately reflect the results of a bank's typical operational decisions, and therefore are excluded.

[52] Banks eliminated this way had variables missing for the entire panel. An alternative to dropping the bank is to include a dummy variable for a missing value. I do not use the dummy method because it is time invariant. Therefore, the fixed effects regression estimator cannot estimate the dummy coefficients.

all individual bank measures. For instance, bank liquidity can have hetergeneous size effects on capital because of different appetites for liquid assets (Berger and Bouman, 2009).

The first measure of bank competition is bank concentration. I take the Metropolitan Statistical Area (MSA) weighted average of a bank's deposits share in each market similar to Cetorelli and Strahan (2006). This is done in three steps. First, a bank's MSA level HHI is calculated wherever it has a branch presence. For instance, if a bank owns 5 branches within an MSA, this bank's share would equal the sum of all its deposits in those 5 branches, divided by the total deposits held by all bank branches within that market. This market share squared is the bank's deposit HHI within that MSA.[53] In an MSA where a single bank owned all of the branches, the HHI would equal 1. In a MSA with an infinite number of banking firms the HHI would approach 0.

Second, I weight these individual MSA across a bank's geographies by the share of total bank deposits held in that MSA. For example, if a bank has a presence in two MSAs, but one MSA has 75% of the deposits, then the smaller deposit MSA receives a weight of ¼ and the larger deposit MSA receives a weight of ¾.

Finally, the deposited weighted average of these MSA HHI becomes the total HHI calculation for a bank.

[53] If a branch is not located in a MSA, then market shares are calculated at the county level.

Bank deposit information is collected from the FDIC Summary of Deposits (SOD) report.[54] This measure is derived from Bank Call Reports.[55] Deposit concentration proxies how much market power a bank has over the local loan market. This is a common measure of local banking concentration in the U.S. HHI is calculated on an annual basis. [56]

I apply an alternative measure of competition, Panzar and Rosse's (1987) H-Statistic, for robustness. An H-Statistic captures revenue elasticities to changes in factor prices and is therefore a measure of competitive conduct in a market. The H-statistic is a measure internal to a bank whereas the HHI bank concentration gauges activity external to a bank. The H-Statistic is based on a general market model that determines equilibrium output by maximizing profits at both the firm and industry levels. To obtain the H-Statistic I begin where profits are maximized, where marginal revenue equals marginal cost:

$$R_i(x_i,n,z_i) - C_i(x_i,w_i,t_i) = 0 \quad (1)$$

Where:

R_i denotes marginal revenues and C_i refers to marginal costs of bank i.

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[54] The Summary of Deposits reports branch deposits as of June 30th in each annual report.

[55] For more information on Call Reports, visit: http://www2.fdic.gov/Call_TFR_Rpts/inform.asp

[56] See Collender and Shaffer (2003), Akhigbe and McNulty (2005), and Cetorelli and Strahan (2006).

x_i is output

n is the number of banks

w_i is the vector of m input prices for bank i

z_i and t_i are exogenous inputs that shift revenue and cost functions

H measures how movements in factor input prices, (δw_{ki}), translate into equilibrium revenues, (δR_i^*), earned by bank i. In other words, H is a measure of the sum of the elasticities of reduced-form revenues with respect to factor prices and is equal to:

$$H = \sum_{k=1}^{m} \frac{\partial R_i^*}{\partial w_{k_i}} \frac{w_{k_i}}{R_i} \quad (1a)$$

H proxies competition to the extent input price shifts cause changes in revenue. If I assume cost functions are homogeneous of degree 1 in input prices, increases in factor prices trigger a proportional increase in revenue under perfect competition. Revenue changes by the same percentage as costs (H=1), since perfectly competitive firms adjust prices to pass on the cost increase. Under monopolistic competition, revenues increase less, and increases in factor input prices are not reflected in revenue. Vesala (1995) shows that the H-Statistic is an increasing function of demand elasticity, suggesting that as H increases, less market power is exercised. The magnitude of H can be interpreted as follows:

1) $H \leq 0$ indicates monopoly equilibrium, perfectly colluding oligopoly or conjectural variation short-run oligopoly

2) $0 < H < 1$ indicates monopolistic competition

3) H=1 indicates perfect competition.

To obtain estimate values for H, I follow Claessens and Laeven (2004) and Schaeck, K. and Cihak, M. (2012). I estimate the following equation to derive the H-statistic:

$$\ln(R_{it}) = \alpha + \beta_1 \ln(W_{1it}) + \beta_2 \ln(W_{2it}) + \beta_3 \ln(W_{3it}) + \beta_4 \ln(Y_{4it}) + \beta_5 \ln(Y_{5it}) + \beta_6 \ln(Y_{6it}) + \varepsilon$$

(2)

where R denotes revenue to total assets (proxy for output price). W_1 captures interest expenses to total debt (proxy for price of funds), W_2 denotes personnel expenses to total assets (proxy for price of labor), and W_3 is the ratio of other operating expenses to total assets (proxy for price of fixed assets). To control for risk and size, Y_1 captures the ratio of deposits to total debt, Y_2 captures loans to total assets, Y_3 is equity to total assets, and Y_4 captures total assets. H is calculated as the sum of the coefficients $\beta_1 + \beta_2 + \beta_3$. Each H-statistic is calculated annually for each bank size category.

Summary calculations of the H-statistic and HHI, the main variables of interest, are found in Table 3.1 to 3.3 by bank size. A cross section of number of MSA branch location count by bank size is located in Table 3.4 The vast majority of banks have branches in less than 5 MSAs.

Table 3.1 Summary Statistics Small Banks

Assets ≤$1 Bn

Variable	Mean	Std. Dev.	Min	Max
Tier 1 Capital/Assets	0.21	0.08	0.00	0.55
MSA HHI	56.00	120.92	0.00	5746.37
H-Statistic	0.65	0.13	0.46	0.88
Allowance for Loan Losses/Total Loans	0.02	0.01	0.00	1.00
Pre-Tax Profit/Assets	0.01	0.10	-7.68	20.62
Liquidity	0.15	0.15	-0.92	1.00
Interest Rate Spread	1.99	1.20	-0.02	3.50
Size	16.34	1.04	9.08	18.42
log(Revenue/Assets)	-2.78	0.40	-8.15	3.69
log(Interest Expense/Liabilities)	-4.04	0.71	-11.00	2.19
log(Employee Expenses/Assets)	-4.12	0.44	-8.78	2.43
log(Operating Expenses/Assets)	-5.54	0.57	-11.84	0.77
log(Deposits/Liabilities)	-0.09	0.25	-9.12	0.00
log(Total Loans/Assets)	-0.48	0.43	-12.04	0.16
log(Equity/ Assets)	-2.23	0.37	-9.67	0.00
Real GDP growth	0.66	0.45	-0.13	1.89
Merger	0.02	0.15	0.00	1.00
Failed	0.00	0.05	0.00	1.00
Number of Banks	9,001			
Number of Observations	65,533			

Table 3.2 Summary Statistics Medium Banks

$1 Bn<Assets≤$ 3 Bn

Variable	Mean	Std. Dev.	Min	Max
Tier 1 Capital/Assets	0.16	0.06	0.00	0.55
MSA HHI	156.51	447.60	0.00	6361.81
H-Statistic	0.46	0.07	0.33	0.60
Allowance for Loan Losses/Total Loans	0.02	0.02	0.00	1.00
Pre-Tax Profit/Assets	0.01	0.02	-0.18	0.72
Liquidity	0.05	0.13	-0.64	0.98
Interest Rate Spread	2.05	1.15	-0.02	3.50
Size	18.88	0.31	18.42	19.52
log(Revenue/Assets)	-2.79	0.37	-5.85	0.34
log(Interest Expense/Liabilities)	-4.22	0.82	-9.29	-1.55
log(Employee Expenses/Assets)	-4.32	0.53	-11.71	-1.55
log(Operating Expenses/Assets)	-5.69	0.64	-13.34	-3.03
log(Deposits/Liabilities)	-0.23	0.67	-10.22	0.00
log(Total Loans/Assets)	-0.45	0.43	-13.19	0.06
log(Equity/ Assets)	-2.32	0.30	-4.66	-0.04
Real GDP growth	0.60	0.40	-0.13	1.89
Merger	0.12	0.33	0.00	1.00
Failed	0.00	0.06	0.00	1.00
Number of Banks	467			
Number of Observations	3,910			

Table 3.3 Summary Statistics Large Banks

$3 Bn< Assets

Variable	Mean	Std. Dev.	Min	Max
Tier 1 Capital/Assets	0.13	0.10	-0.03	0.35
MSA HHI	289.38	799.11	0.00	8036.56
H-Statistic	0.36	0.13	0.16	0.57
Allowance for Loan Losses/Total Loans	0.02	0.01	0.00	0.13
Pre-Tax Profit/Assets	0.01	0.02	-0.37	0.18
Liquidity	0.04	0.15	-0.52	0.95
Interest Rate Spread	2.02	1.17	-0.02	3.50
Size	20.87	1.20	19.52	26.00
log(Revenue/Assets)	-2.75	0.43	-4.79	-0.59
log(Interest Expense/Liabilities)	-4.25	0.88	-8.10	-2.67
log(Employee Expenses/Assets)	-4.60	0.94	-11.28	-2.34
log(Operating Expenses/Assets)	-5.95	1.11	-15.79	-2.94
log(Deposits/Liabilities)	-0.47	1.26	-11.21	0.00
log(Total Loans/Assets)	-0.54	0.53	-6.62	0.02
log(Equity/ Assets)	-2.32	0.30	-3.89	-0.33
Real GDP growth	0.62	0.41	-0.13	1.89
Merger	0.24	0.43	0.00	1.00
Failed	0.00	0.06	0.00	1.00
Number of Banks	324			
Number of Observations	2,793			

Table 3.4 Frequency of Banks by number of MSA Locations in 2007

# MSA Locations	$3 Bn< Assets	$1 Bn<Assets≤$ 3 Bn	Assets ≤$1 Bn
0	20	17	1,829
1	73	149	6,025
2	26	90	838
3	22	76	208
4	18	45	55
5	18	32	21
6-10	67	50	21
11-15	28	3	2
16-20	13	4	1
21-30	16	-	1
31-50	8	-	-
51-100	9	1	-
101-150	5	-	-
151-200	-	-	-
201+	1	-	-

Notes: If a Bank does not exists in 2007, then the first year a bank appears in the panel is included in the frequency calculation instead.

Bank-specific controls are included to account for observable correlations with capital ratio, which mitigates the impact of omitted variable bias. To control for credit risk, the ratio allowance for loan losses to total loans is included. As banks take additional credit risks they are expected to hold additional capital to cover these risks (Altunbas et al., 2007). Flannery and Rangan (2008) and Berger and Bouwman (2013) find a positive relation between profitability and capital. Therefore, I include the ratio of profit to total assets in each estimation. Merger activity may impact capital choice; a bank likely will make adjustments to optimize capital based on the framework of the new merged organization rather than the premerger entity. Therefore, I include an indicator for merger activity during the year of observation. Failing banks could be highly leveraged due to plummeting asset values and likely have inadequate capital. I account for these outlying cases by including an indicator designating the year a bank fails.[57] Beyond segmenting the sample by banks size, I control for bank size in each regression with the natural logarithm of total assets. Smaller banks may use more soft-information lending technology than larger banks (Berger et al., 2005). The last bank-specific factor I include is the liquidity ratio, calculated as cash and balances due plus securities plus federal funds sold and repurchase agreements plus trading account assets minus pledged

[57] I define a bank failure as an outcome where a bank undergoes liquidation rather than becoming acquired or merging with another bank.

securities, all divided by total liabilities. The liquidity ratio estimates how well liquid assets cover any assumed liabilities. I expect banks that are more liquid accumulate less capital due to reduced risk.

Macro-level variables – the spread between short-term interest rates and long-term interest rates, and GDP output growth – are gathered from the Federal Reserve of St. Louis using the Federal Reserve Economic Data (FRED) repository. [58,59] I aggregate these to the annual level. These are included to account for bank profit decisions correlated with the economic environment.[60] The spread between short and long term interest rates is a proxy for interest rate risk that banks face arising from the maturity mismatch of short-run liabilities and long-run assets. Laeven and Majnoni (2003) explain that capital ratios are pro-cyclical if banks accumulate capital during expansionary portions of the business cycle. Jokipii and Milne (2008) empirically confirm the existence of this relationship in European banks. The inclusion of GDP growth accommodates the existence of this relationship.

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[58] I use 3-month and 10-year Treasury bill rates to represent short term and long-term interest rates respectively.

[59] See http://research.stlouisfed.org/fred2/.

[60] Inflation remains an important determinant in banks' capital decisions. All values are adjusted using 2013:Q1 CPI to mitigate this factor.

The summary statistics for small, medium, and large banks are reported in Table 3.1 to 3.3. Together these variables control for observable macroeconomic environment and individual bank risk factors.

3.3. Literature Review

The Allen et al. (2011) model examines the determinants of capital formation under different market concentration conditions and provides the main motivation of this paper. Their one-period model shows that market discipline can arise from the asset side of a bank balance sheet, in contrast to previous literature which focuses on bank liabilities (Calomiris and Kahn, 1991, Diamond and Dybvig, 1983, and Mehran and Thakor, 2011). The Allen et al. model demonstrates that the choice of monitoring effort gives rise to a moral hazard problem because monitoring existing borrowers is costly and because banks have limited liability. Banks are required to hold equity capital to provide an incentive for sufficient levels of monitoring. Banks putting capital "skin in the game" internalizes the cost of defaulting, which reduces the problem arising from limited liabilities due to deposit funding. Beyond putting capital at stake, an incentive to monitor arises from lending. If a project funded by the bank succeeds, an increase in the (marginal) loan rate gives the bank a higher payoff, providing the bank an additional incentive to engage in monitoring. There are different costs associated with each of these strategies. Holding capital incurs direct private costs, whereas increases in loan rates negatively impacts borrowers. Therefore, borrowers favor banks with high levels of capital because they signal a willingness to monitor without imposing direct costs upon the borrower. Assuming that a bank's capital is observable, a bank with higher levels of capital attracts good credit risks. Thus, in a loan market with higher levels of competition, theory predicts there will be higher levels of capital to attract the good borrowers. These results suggest hypothesis H1:

H1: Competition disciplines banks to operate at higher capital ratios.

In the model of Allen et al. (2011), loans do not require an identical monitoring effort. Monitoring impacts relationship loans, but not transactional lending. Banks can choose to lend based on hard or soft information.[61] The main distinction between hard and soft information is the ease with which the information can be quantified, which is more a continuum classification than a binary categorization. Credit scores are more easily quantified than a bank lender's written opinion. Thus, credit scores are a form of hard information whereas the opinion is a type of soft information. Berger et al. (2005) provide evidence of a difference in lending information technology between small and large banks. Smaller banks lend more to a base of borrowers that have more soft information than hard information. This lending requires intensive monitoring using appropriate technology. On the other hand, large banks utilize more hard information technology. These factors affect the lending and borrowing behavior of banks, according

[61] For a more detailed discussion on the distinction between hard and soft information see Peterson (2004).

to size. Specifically, large banks are willing to lend at greater distances, hold more impersonal relationships with borrowers, are more ineffective at relieving credit constraints, and have shorter and less exclusive relationships than smaller banks. Since capital plays a major role in the types of lending banks engage in, the Allen et al. (2011) model implies a link between bank size and capital ratios. Berger and Bouwman (2013) indicate size could be a source of economic strength for a bank, just like capital, but has diminishing marginal value. For example, larger banks have access to financial markets unavailable to more resource-constrained smaller banks. Hence, capital offers the greatest benefit to small banks, without size advantages. This rational formulates hypothesis H2:

H2: Smaller banks operate with higher capital ratios than larger banks.

Schaeck and Cihak (2012) test the validity of H1 and H2 in a panel of OECD countries in the pre-crisis period. Their results indicate H1 and H2 both hold. I use these findings as a benchmark for my own results in the pre-crisis period for U.S. banks.

I extend Schaeck and Cihak (2012) by testing for changes in the relationship between bank capital and competition over a business cycle. During a downturn, bank customers are more likely to be sensitive to banks survivability and seek those with higher capital ratios as a signal of low risk (Allen et al., 2011). Indeed, Berger and Bouwman (2013) find evidence that higher capital ratios enhance bank survival probability of small, medium, and large banks during bank crises. Competitive pressures may intensify in poor economic times. Hence, better-capitalized banks, in theory, can easily take customers away from lesser-capitalized peers. Therefore, I expect competition to have a larger impact on bank capital during the financial crisis of 2007 through 2009, leading to hypothesis H3:

H3: Competition has a greater impact on capital ratios during the financial crisis.

3.4. Empirical Specification

I estimate the impact of competition on capital holdings with the following specification:

$$capital_{it} = \alpha + \beta_1 competition_{it-1} + \beta_2 medium_i + \beta_3 large_i + \beta_4 medium_i * competition_{it-1} + \beta_5 large_i * competition_{it-1} + X\beta + \delta_i + \gamma_t + \varepsilon_{it} \quad (3)$$

where:

capital is the capital ratio for bank i at time t.

competition is the competition measure for bank i at time t.

medium is an indicator for banks with assets ≥ \$1bn and < \$3bn

large is an indicator for banks with assets ≥ \$3bn

$X\beta$ is a set of macroeconomic and bank specific time variant controls

δ is a vector of time fixed effects for time t

γ is a vector of fixed effects for bank i

I estimate equation (3) using fixed effects regression. I assume that the unobservable bank individual effects, δ_i, are correlated with the other covariates. Such an assumption is reasonable because it is likely that individual time-invariant bank effects, such as bank culture, would be correlated with bank measures, such as size. The fixed effects estimation is implemented by using the within estimator to remove unobserved time-invariant heterogeneity.

Correlation of a bank's observations over time may be of concern. Not accounting for this possibility may provide biased estimates for the regression standard errors. To avoid such bias, the errors are clustered by bank.

An important component of this analysis is accounting for the potential endogeneity of competition. Endogeneity can arise when competition depends on capital ratios. For example, acquirer banks may choose to merge based on the high level of capital of a potential target, which would impact the market power of the consolidated bank. Such a scenario suggests a positive link between capital and the reduction in competition. Alternatively, a bank with low capital ratios may have lower charter values, which may encourage risk-taking competition (Keeley, 1990). Such behavior indicates a negative relationship between capital ratios and competition. To reduce the influence of endogeneity, the competition variables are lagged by one year. Additionally, the other control variables are lagged by one period year to reduce any further causality bias driven by the covariates.

3.5. Empirical Results

The estimates for equation (3) are in Table 3.5. I interact size indicators with relevant bank characteristics to allow for differential impacts of each variable.62 Model (1) indicates there is a negative, statistically significant relationship between a bank's HHI and capital ratio. Indeed, as a given bank becomes more concentrated in its deposit markets, it is predicted to lower its capital ratios after controlling for observable macroeconomic and individual bank covariates. I can analyze if this impact differs across bank sizes by examining the significance of the interaction coefficient "<=$3 bn and >$1 bn indicator *lag(MSA HHI)" and ">$3 bn *lag(MSA HHI)". Both interaction coefficients prove positive significant confirming medium and large banks' capital ratios are impacted differently by lagged MSA HHI.

[62] Any insignificant bank size interacted term is not included in the final estimation in Table 2.

Table 3.5 Regression Results

Dependent Variable			
Tier 1 capital ratio	All Banks	Single MSA banks with ≤$1bn	5+ MSA with >$3 bn
Variables	(1)	(2)	(3)
lag(MSA HHI)	-0.028002***	-0.096885***	-0.003265**
	(0.008914)	(0.001128)	(0.001151)
$1 bn< and ≤$3 bn indicator *lag(MSA HHI)	0.020018***	-	-
	(0.002486)		
>$3 bn indicator *lag(MSA HHI)	0.025518***	-	-
	(0.005210)		
lag(Allowance for loan losses)	0.960483***	0.71738***	0.624368**
	(0.076050)	(0.038295)	(0.017862)
lag(Pre-tax profit/assets)	0.317965***	0.258221**	0.204998**
	(0.0888123)	(0.015095)	(0.102848)
lag(Liquidity ratio)	0.216167***	0.144655***	0.032373***
	(0.070130)	(0.022771)	(0.013094)
$1 bn< and ≤$3 bn indicator *lag(Liquidity ratio)	-0.156865***	-	-
	(0.057459)		
>$3 bn indicator*lag(Liquidity ratio)	-0.243094***	-	-
	(t0.098293)		

Table 3.5 Continued

Variables	All Banks	Single MSA banks with ≤$1bn	5+ MSA with >$3 bn
	(1)	(2)	(3)
lag(Interest rate spread)	-0.0020493**	-0.007610***	-0.0061859*
	(0.0012864)	(0.002138)	(0.002514)
lag(merger)	0.006161**	0.0043215***	0.011390**
	(0.001189)	(0.001259)	(0.003616)
$1 bn< and ≤$3 bn indicator *lag(merger)	0.001800**	-	-
	(0.000483)		
>$3 bn indicator *lag(merger)	0.0058606**	-	-
	(0.001257)		
lag (Size)	-0.054482***	-0.056163***	-0.041162**
	(0.017818)	(0.006759)	(0.009064)
$1 bn< and ≤$3 bn indicator *lag(size)	0.024625***	-	-
	(0.011140)		
>$3 bn indicator*lag(size)	0.034846**	-	-
	(0.003122)		
Failed	-0.094308***	-0.088642***	-0.040278***
	(0.009475)	(0.006457)	(0.009165)
$1 bn< and ≤$3 bn indicator *lag(Failed)	0.029399**	-	-
	(0.015589)		
>$3 bn indicator*lag(Failed)	0.058782**	-	-
	(0.021429)		

Table 3.5 Continued

Variables	All Banks	Single MSA banks with ≤$1bn	5+ MSA with >$3 bn
	(1)	(2)	(3)
lag(Real GDP)	0.005183***	0.004493***	0.004894**
	(0.000458)	(0.001518)	(0.001110)
R^2	0.2526	0.1735	0.3763
N	63,896	44,512	1,012

Notes: Results from equation (3). Equation (3) is estimated using fixed effects over the 2001-2013 panel with the within estimator to account for unobservable state effects (i.e., fixed effects).Observations are end of Q2 measures. Time fixed effects are included in the specification, but omitted in the results to save space. The standard errors (shown in parentheses) are clustered at the bank level to account for clustering. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

A Wald test, see Table 3.6, confirms the heterogeneous impact of lagged HHI differs between small medium and small banks. The positive sign of the size HHI interactions indicates that the impact of lagged HHI on bank capital ratio is reduced for medium and even more reduced for large banks in comparison to small banks. To quantify the economic significance of these results, I consider the impact of an increase in a small bank's HHI on its capital ratio. A 1% increase in small bank mean HHI, 0.56 units, causes a -1.57% points decrease in a bank's Tier 1 capital ratio. Considering the mean Tier 1 capital ratio is 21% for small banks, this is no small change. Using the marginal effects of lagged HHI I estimate that a 1% increase in medium bank mean HHI, 1.56 units, causes a -1.25% points decrease in a bank's Tier 1 capital ratio. A similar calculation finds a 1% increase in large bank mean HHI, 2.89 units, causes a -0.72% point reduction in a bank's Tier 1 capital ratio. I conclude all bank capital decisions are sensitive to alterations in banking concentration regardless of size and economically significant. However, this impact diminishes as bank size increases.

3.6. Robustness

I test the sensitivity of my results with changes in economic variable measurement and model specification. First, I test whether a fixed or random effects estimation is more appropriate, a standard procedure when using panel data. I utilize a Hausman (1978) specification test. Both FE and RE control for unobserved time-invariant heterogeneity, but one may provide biased coefficient estimates in my models. I find the estimated

Hausman test statistic indicates that I can reject the hypothesis that random effects are unbiased at the 1% confidence level. Therefore, FE is the more appropriate estimator.[63]

Second, I examine the robustness of the results to the influence of banks of certain geographic footprints and size. Numerous small banks have branches located in one MSA, a relatively small geographic footprint. These banks may face few competitors; therefore, the impact of capital accumulation to signal lending strength to borrowers could prove insignificant. In specification (2) in Table 3.5, I limit the sample to small banks with branches in only one MSA in 2007. The lagged HHI coefficient still proves significant and negative; thus, I conclude the results are robust for small banks in limited footprints. Analogously, I analyze whether my results hold for large banks spanning several MSAs. Large banks with wide branch footprints may not need to use capital as a signal for lending viability, instead relying other strengths related to bank size such as more branches to consult and widespread reputation.[64] However, the lagged HHI

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[63] All results may be driven by my choice of capital definition, Tier capital ratio. This is a regulatory measure of capital rather than one of leverage. Leverage may be more binding than regulatory. If I substitute tier 1 capital ratio, the results are similar to Table 3.5.

[64] Remember the HHI is weighted by the proportion of deposits a bank holds in each MSA. This should reduce the impact of banks spanning multiple MSAs on my estimation results. The HHI MSA measure is not perfect, but this common due to the complexity of banking institutions.

coefficient is negative and significance, see Table 3.5 specification (3). This is further evidence that capital ratio decisions are impacted by competitive conditions despite the geographic footprint or size of the bank.

Table 3.6 Wald Tests for HHI

Wald test	
Hypothesis	p value
lag(MSA HHI)= ($1 bn< and ≤$3 bn indicator *lag(MSA HHI))	0.00***
lag(MSA HHI)= (>$3 bn indicator*lag(MSA HHI))	0.03**
($1 bn< and ≤$3 bn indicator *lag(MSA HHI))= (>$3 bn indicator*lag(MSA HHI))	0.00***

Notes:*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Next, I consider the robustness of my results to the measure of competition. In Table 3.7, I estimate equation (3) using the H-statistic, the bank level revenue elasticity to changes in factor prices, as a replacement for MSA HHI. My results indicate a larger H-statistic significantly predicts a higher capital ratio in specification (1). Similar to specification (1) in Table 3.5 the size interactions with the competition proxy, H-statistic, are significant. Wald tests in Table 3.8 test for differences in magnitude between the size interaction effects. The results in Table 3.7 and 3.8 confirm that heterogeneous effects exist between lagged H-statistic and Tier 1 capital ratios by bank size.

Table 3.7 H-Statistic Regression Results

Dependent Variable	
Tier 1 capital ratio	All Banks
Variables	(1)
lag(H-Statistic)	1.132896***
	(0.058841)
$1 bn< and ≤$3 bn indicator *lag(H-Statistic)	-0.274509***
	(0.005421)
>$3 bn indicator*lag(H-Statistic)	-0.600072**
	(0.299951)
lag(Allowance for loan losses)	0.675392***
	(0.146366)
lag(Pre-tax profit/assets)	0.341176***
	(0.066309)
lag(Liquidity ratio)	0.271103***
	(0.055299)
$1 bn< and ≤$3 bn indicator *lag(Liquidity ratio)	-0.318621***
	(0.062329)
>$3 bn indicator*lag(Liquidity ratio)	-0.394538***
	(0.083307)
lag(Interest rate spread)	-0.002711**
	(0.001088)
lag(merger)	0.025499**
	(0.011290)
$1 bn< and ≤$3 bn indicator *lag(merger)	0.000600**
	(0.000273)
>$3 bn indicator*lag(merger)	0.000525**
	(0.000211)
lag (Size)	-0.040227***
	(0.008301)
$1 bn< and ≤$3 bn indicator *lag(size)	0.014365***

Table 3.7 Continued

Variables	(1)
	(0.006130)
>$3 bn indicator*lag(size)	0.026832**
	(0.016991)
Failed	-0.077703***
	(0.011974)
<=$3 bn and >$1 bn indicator *lag(Failed)	0.018773**
	(0.009819)
>$3 bn *lag(Failed)	0.045501**
	(0.012123)
lag(Real GDP)	0.003393***
	(0.000753)
R^2	0.3489
N	63,896

Notes: Results from equation (3). Equation (3) is estimated using fixed effects over the 2001-2013 panel using within estimator to account for unobservable state effects (i.e., fixed effects).Observations are end of Q2 measures. Time fixed effects are included in the specification, but omitted in the results to save space. The standard errors (shown in parentheses) are clustered at the bank level to account for clustering. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 3.8 Wald Test for H-statistic

Wald test

Hypothesis	p value
lag(H-Statistic)= ($1 bn< and ≤$3 bn indicator *lag(H-Statistic))	0.01**
lag(H-Statistic)= (>$3 bn indicator*lag(H-Statistic))	0.02**
($1 bn< and ≤$3 bn indicator *lag(H-Statistic))= (>$3 bn indicator*lag(H-Statistic))	0.00***

Notes: *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

I find the economic significance in Table 3.7 to be slightly smaller compared to Table 3.5. A 1% rise in the H-statistic of a small bank increases capital ratios by +1.13% points. After a 1% increment in the H-statistic, medium and large banks see a rise of +0.86% points and +0.53% points in capital ratios, respectively. The magnitude of this impact is larger than the one Schaeck and Cihak (2012) estimates. They find 1% increase in the H-statistic lifts capital ratios by +0.3% points for OECD banks in the pre-crisis period.

Thus far, I assume the impact of competition is constant across the panel period 2001-2013. I can test whether there is a homogenous impact of competition over the most recent business cycle by rewriting equation (3):

$$capital_{it} = \alpha + \beta_1 competition_{it-1} + \beta_2 Crisis + \beta_3 PostCrisis + \beta_4 Crisis * competition_{it-1} + \beta_5 PostCrisis * competition_{it-1} + X\beta + \delta_i + \gamma_t + \varepsilon_{it} \quad (4)$$

Equation (4) includes crisis (2008-2009) and post-crisis (2010-2013) period indicators interacted with the variable of interest, competition. If β_4 or β_5 is significant, then the effect of competition on capital ratios differs depending on the period relative to the financial crisis. I include the bank size interactions in equation (4) to account for possible heterogeneous size effects on capital ratios from covariates and the competition variable of interest.

The results of estimating equation (4) are in Table 3.9. Consistent with previous specifications, lagged HHI remains negative and significant in specification (1). I find evidence of additional significant impacts to capital from HHI during the crisis and post-crisis periods compared to the pre-crisis period. Indeed, in equation (4) the coefficients of the interaction terms of HHI with crisis and post-crisis are significant and negative. Wald tests in Table 8 confirm these effects are statistically different from one another. This indicates that the impact of HHI is amplified during the crisis and post-crisis, and peaks during the economic downturn. The economic impact of lagged

HHI on capital ratios is strongest during the crisis for banks with less than or equal to $1 bn in assets; a 1% point increase in small bank HHI predicts a -4.84 % points fall in Tier 1 capital ratio. After the crisis, the effect of a 1% point increase in lagged small bank HHI on capital ratios diminishes to -3.96% points. However, the post-crisis impact remains larger than the pre-crisis. The interaction between each size class and lagged HHI remains positive and significant from Table 3.5 specification (1). Wald tests in Table 3.10 confirm statistical differences among the coefficients. Like small banks, medium and large banks show similar shifts in capital ratio sensitivity to competition over the financial crisis timeline.

Table 3.9 Regression Results with Financial Crisis Controls

Dependent Variable	
Tier 1 capital ratio	All Banks
Variables	(1)
lag(MSA HHI)	-0.06175***
	(0.013800)
Crisis	-0.237718**
	(0.125930)
Post Crisis	-0.186719**
	(0.099428)
Crisis*lag(MSA HHI)	-0.024732***
	(0.008110)
Post Crisis*lag(MSA HHI)	-0.009100***
	(0.002312)
Crisis*$1 bn< and ≤$3 bn indicator	-0.010482**
	(0.044017)
Post Crisis*$1 bn< and ≤$3 bn indicator	0.080048**
	(0.039108)
Crisis*>$3 bn indicator	0.013669**
	(0.051717)
Post Crisis*>$3 bn indicator	0.095001**
	(0.037719)
$1 bn< and ≤$3 bn indicator *lag(MSA HHI)	0.055538***
	(0.010990)
>$3 bn indicator*lag(MSA HHI)	0.056465***
	(0.006861)
Crisis*$1 bn< and ≤$3 bn indicator *lag(MSA HHI)	-0.002484**
	(0.001355)
Post Crisis*$1 bn< and ≤$3 bn indicator *lag(MSA HHI)	-0.000915**
	(0.000411)
Crisis*>$3 bn indicator *lag(MSA HHI)	-0.002113**
	(0.001180)
Post Crisis*>$3 bn indicator *lag(MSA HHI)	-0.000778**
	(0.000281)
lag(Allowance for loan losses)	1.129060***
	(0.258833)

Table 3.9 Continued

Variables	(1)
lag(Pre-tax profit/assets)	0.485091***
	(0.108642)
lag(Liquidity ratio)	0.280871***
	(0.091471)
$1 bn< and ≤$3 bn indicator *lag(Liquidity ratio)	-0.200095***
	(0.074176)
>$3 bn indicator *lag(Liquidity ratio)	-0.560824***
	(0.089949)
lag(Interest rate spread)	-0.001518**
	(0.000823)
lag(merger)	0.004812**
	(0.000869)
$1 bn< and ≤$3 bn indicator *lag(merger)	0.001098**
	(0.000643)
>$3 bn indicator*lag(merger)	0.0042106**
	(0.001486)
lag (Size)	-0.034580***
	(0.005518)
$1 bn< and ≤$3 bn indicator *lag(size)	0.018003***
	(0.006771)
>$3 bn indicator*lag(size)	0.029170**
	(0.014522)
Failed	-0.045813***
	(0.007901)
$1 bn< and ≤$3 bn indicator *lag(Failed)	0.021995**
	(0.012533)
>$3 bn indicator*lag(Failed)	0.033922**
	(0.025900)
lag(Real GDP)	0.004189***
	(0.000856)
R^2	0.4406
N	63,896

Notes: Equation (4) is estimated withfixed effects over the 2001-2013 panel using the within estimator to account for unobservable state effects (i.e., fixed effects).Observations are end of Q2 measures. Time and MSA fixed effects are included in the specification, but omitted in the results to save space. The standard errors) are clustered at the bank level to account for clustering. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 3.10 Wald Test for Financial Crisis Controls

Wald test

Hypothesis	p value
lag(MSA HHI)= ($1 bn< and ≤$3 bn indicator *lag(MSA HHI))	0.00***
lag(MSA HHI)= (>$3 bn indicator*lag(MSA HHI))	0.04**
($1 bn< and ≤$3 bn indicator *lag(MSA HHI))= (>$3 bn indicator*lag(MSA HHI))	0.01**
Lag (MSA HHI)= Crisis*lag(MSA HHI)	0.02**
Lag (MSA HHI)=Post Crisis*lag(MSA HHI)	0.03**
Crisis*Lag (MSA HHI)=Post Crisis*lag(MSA HHI)	0.02**
$1 bn< and ≤$3 bn indicator *lag(MSA HHI)	0.00***
>$3 bn indicator*lag(MSA HHI)	0.04**
($1 bn< and ≤$3 bn indicator *lag(MSA HHI))=(Crisis*$1 bn< and ≤$3 bn indicator *lag(MSA HHI))	0.00***
(Crisis*$1 bn< and ≤$3 bn indicator *lag(MSA HHI))=(Post Crisis*$1 bn< and ≤$3 bn indicator *lag(MSA HHI))	0.01**
(Post Crisis*$1 bn< and ≤$3 bn indicator *lag(MSA HHI))=(Crisis*$1 bn< and ≤$3 bn indicator *lag(MSA HHI))	0.03**
(>$3 bn indicator*lag(MSA HHI))=(Crisis*>$3 bn indicator *lag(MSA HHI))	0.01**
(>$3 bn indicator*lag(MSA HHI))=(Crisis*>$3 bn indicator *lag(MSA HHI))	0.01**
(Post Crisis*>$3 bn indicator *lag(MSA HHI))=(Crisis*>$3 bn indicator *lag(MSA HHI))	0.03**

Notes: *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

I complete the robustness tests with an estimation (4) using the H-statistic as a proxy for competition. Results of this estimation are in Table 3.11. Wald tests for significant differential impacts of size and crisis capital effects are in Table 3.11. I find no significant difference in results from Tables 3.9 and 3.10.

Table 3.11 H-Statistic Regression Results with Financial Crisis Controls

Dependent Variable	
Tier 1 capital ratio	All Banks
Variables	(1)
lag(H-Statistic)	1.160012***
	(0.229903)
Crisis	-0.268802**
	(0.156644)
Post Crisis	-0.200917**
	(0.117407)
Crisis*lag(H-Statistic)	0.35621***
	(0.011678)
Post Crisis*lag(H-Statistic)	0.164994***
	(0.021179)
Crisis*$1 bn< and ≤$3 bn indicator	0.137709**
	(0.076654)
Post Crisis*$1 bn< and ≤$3 bn indicator	0.084430**
	(0.045634)
Crisis*>$3 bn indicator	0.189903**
	(0.115687)
Post Crisis*>$3 bn indicator	0.124440**
	(0.065981)
$1 bn< and ≤$3 bn indicator *lag(H-Statistic)	-0.237501***
	(0.087911)
>$3 bn indicator*lag(H-Statistic)	-0.400120***
	(0.084389)
Crisis*$1 bn< and ≤$3 bn indicator *lag(H-Statistic)	0.285321***
	(0.089923)
Post Crisis*≤$3 bn and >1$ bn indicator *lag(H-Statistic)	0.104573***
	(0.021190)
Crisis*>$3 bn indicator*lag(H-Statistic)	0.234231**
	(0.152003)
Post Crisis*>$3 bn indicator *lag(H-Statistic)	0.085822**
	(0.045932)
lag(Allowance for loan losses)	0.881680***
	(0.127790)

Table 3.11 Continued

Variables	(1)
lag(Pre-tax profit/assets)	0.306409***
	(0.055554)
lag(Liquidity ratio)	0.339010***
	(0.073429)
\$1 bn< and ≤\$3 bn indicator *lag(Liquidity ratio)	-0.174421***
	(0.042232)
>\$3 bn indicator*lag(Liquidity ratio)	-0.252205**
	(0.142221)
lag(Interest rate spread)	-0.004328**
	(0.002199)
lag(merger)	0.020876**
	(0.012830)
\$1 bn< and ≤\$3 bn indicator *lag(merger)	0.001700**
	(0.001039)
>\$3 bn indicator *lag(merger)	0.000333**
	(0.000211)
lag (Size)	-0.048604***
	(0.009993)
\$1 bn< and ≤\$3 bn indicator *lag(size)	0.016537***
	(0.003883)
>\$3 bn indicator*lag(size)	0.030132**
	(0.018120)
Failed	-0.089421***
	(0.022241)
\$1 bn< and ≤\$3 bn indicator *lag(Failed)	0.027280**
	(0.019853)
>\$3 bn indicator*lag(Failed)	0.055774**
	(0.028009)
lag(Real GDP)	0.006639***
	(0.001115)
R2	0.5039
N	63,896

Notes: Equation (4) is estimated with fixed effects over the 2001-2013 panel using the within estimator to account for unobservable state effects (i.e., fixed effects).Observations are end of Q2 measures. Time and MSA fixed effects are included in the specification, but omitted in the results to save space. The standard errors are clustered at the bank level to account for clustering. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 3.12 H-Statistic Wald Test with Financial Crisis Controls

Wald test	
Hypothesis	p value
lag(H-Statistic)= ($1 bn< and ≤$3 bn indicator *lag(H-Statistic))	0.00***
lag(H-Statistic)= (>$3 bn indicator*lag(H-Statistic))	0.02**
($1 bn< and ≤$3 bn indicator *lag(H-Statistic))= (>$3 bn indicator*lag(H-Statistic))	0.02**
Lag (H-Statistic)= Crisis*lag(H-Statistic)	0.03**
Lag (H-Statistic)=Post Crisis*lag(H-Statistic)	0.01**
Crisis*Lag (H-Statistic)=Post Crisis*lag(H-Statistic)	0.00***
$1 bn< and ≤$3 bn indicator *lag(H-Statistic)	0.00***
>$3 bn indicator*lag(H-Statistic)	0.01**
($1 bn< and ≤$3 bn indicator *lag(H-Statistic))=(Crisis*$1 bn< and ≤$3 bn indicator *lag(H-Statistic))	0.00***
(Crisis*$1 bn< and ≤$3 bn indicator *lag(H-Statistic))=(Post Crisis*$1 bn< and ≤$3 bn indicator *lag(H-Statistic))	0.02**
(Post Crisis*<=$3 bn and >$1 bn indicator *lag(H-Statistic))=(Crisis*$1 bn< and ≤$3 bn indicator *lag(H-Statistic))	0.04**
(>$3 bn indicator*lag(H-Statistic))=(Crisis*>$3 bn indicator *lag(H-Statistic))	0.03**
(>$3 bn indicator*lag(H-Statistic))=(Crisis*>$3 bn indicator *lag(H-Statistic))	0.02**
(Post Crisis*>$3 bn indicator *lag(H-Statistic))=(Crisis*>$3 bn indicator *lag(H-Statistic))	0.04**

Notes:*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

3.7. Discussion

After reviewing the above results, my findings support my first hypothesis: competition disciplines banks to operate at higher capital ratios. If banks face competitive pressures in the form of lower bank concentration, evidence predicts the institutions will accumulate more capital relative to assets. Similarly, banks that demonstrate more intense competition in the form of a higher H-statistic are predicted to increase capital ratios. This provides additional evidence for the capital accumulation theory of Allen et al. (2011), which concludes that banks in competitive environments hold more capital to attract borrowers seeking a stable lender. Furthermore, my results are consistent with Schaeck and Cihak (2012); banks exposed to more competition held additional capital in the pre-crisis period. My results extend to the crisis and post-crisis periods for all banks.

In all of the specifications I estimate size, lag log(assets), has a negative and significant impact on capital ratios. The impact of lag log assets on capital ratios does exhibit significant size class heterogeneity. I find the impact of lag log assets on capital ratios diminishes with increasing bank size class. I conclude my second hypothesis holds: smaller banks operate with higher capital ratios than larger banks. Schaeck and Cihak (2012) identify this relationship in OCED banks, as well. As Berger and Bouwman (2013) suggest, size could be a source of economic strength for a bank allowing it greater diversification and flexibility in lending and borrowing. In Figure 3.2 I trend capital ratios over time by bank asset size. The rank ordering remains the same over time: on average small banks have the highest capital ratios and large banks have the lowest capital ratios. T-tests confirm the means differ among the bank size classifications. This reaffirms support of my second hypothesis.

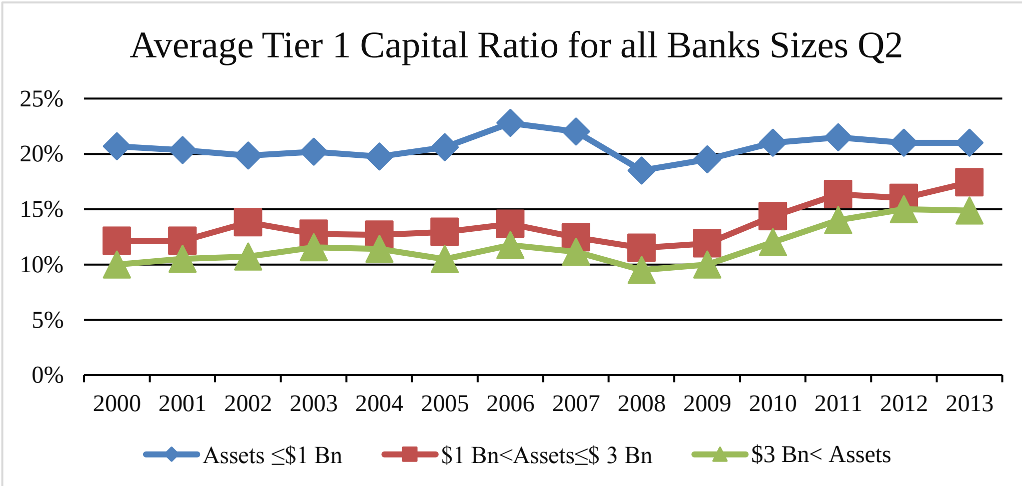

Figure 3-2 Average Tier 1 Capital

My estimates of equation (4) confirm Hypothesis 3: competition has a greater impact on capital ratios during the financial crisis. Indeed, I find significant evidence that after an increase in competition banks are predicted to adjust capital ratios the most during the crisis. Banks are likely more sensitive to competitive pressures during the financial crisis because there are fewer borrowing and lending opportunities, a “credit crunch” (Berger and Udell, 2004). Furthermore, non-performing loans typically remain at low levels during most of the expansion, but begin to appear at the end of the expansion, then increase significantly during the recession. The Allen et al. model explains banks will accumulate capital to signal their resilience to borrowers. Berger and Bouwman (2013) find empirical support that higher capital levels increase the likelihood of survival.

After the crisis, banks' capital ratio sensitivity to competitive pressure remains higher than in the pre-crisis period. One explanation of this lingering sensitivity is that banks' behavior is altered by the crisis.[65]

In a broader context, this flow of borrowers to better capitalized institutions can have industry-level effects. Profits can be maintained on an industry level (Stiroh, 2000) as assets flow from weak to strong performing banks. Multiple channels can be the source of this reallocation. Dick and Lehnert (2010) demonstrate that competition increases lending efficiency while decreasing credit risk. The threat of potential entrants drives banks to devote resources to screening and monitoring of borrowers, and in turn reduces information asymmetries (Mehran and Thakor, 2011). As competition rises lending related information may increase, which improves bank's screening and monitoring (Hauswald and Marquez, 2003). Additionally, Banks may respond to competition by specializing lending to certain types of loans. Specialization can lower the cost of lending through risk pricing and loan differentiation (Zarutskie, 2010). These specific channels better explain the evident relationship between competition and capital.

[65] Alternatively, banks with high capital ratios also can more easily pass regulators' annual Comprehensive Capital Analysis and Review (CCAR) stress test put in place after the financial crisis. Holding additional capital will lower the likelihood regulatory penalties will be placed upon the institution.

3.8. Conclusion

Guided by the asset side driven theory of Allen et al. (2011), this paper empirically addresses the impact of bank competition on bank capital ratios before, during, and after the financial crisis, the most recent U.S. business cycle. I find a heterogeneous impact of competition among small, medium, and large banks' capital accumulation. Alternative definitions of competition do not change the results. More intense competition is associated with higher bank capital ratios at all times (before, during, and after the financial crisis) regardless of bank asset size. Smaller banks tend to hold more capital than medium and large banks. My analysis provides insight into the economic roles of competition and on how its impact can vary in the cross section of banks depending on bank size (small, medium, and large) and time period (before, after, and during a financial crisis).

Strengthening bank capital buffers remains the most common tool to maintain financial system stability. The evidence presented here provides an additional avenue for regulators and policymakers to gauge capital adequacy and the impact of competition on lending and bank soundness outcomes. If the goal is to increase capital ratios, then initiatives that encourage competition should be promoted.

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