Consumption Slowdown After the Great Recession

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December 10, 2020

Abstract

Consumption growth in the US has considerably decreased since the last financial crisis. We argue costly regulation imposed on a group of banks that were facing foreclosure issues played a role in that decline. Additional regulatory controls resulted in a lower supply of mortgage loans. Counties that were more exposed to controlled banks faced a slower recovery of house prices and therefore wealth. Using data on employment and consumption we argue that it is the wealth effect originating from bank controls that decreased consumption growth. Banks decrease their mortgage loan origination by issuing a lower number of loans and not by decreasing the average amount of loans. **JEL classification:** G21, L26

Keywords: Consumption, Mortgage, Banking Regulation, Economic Growth

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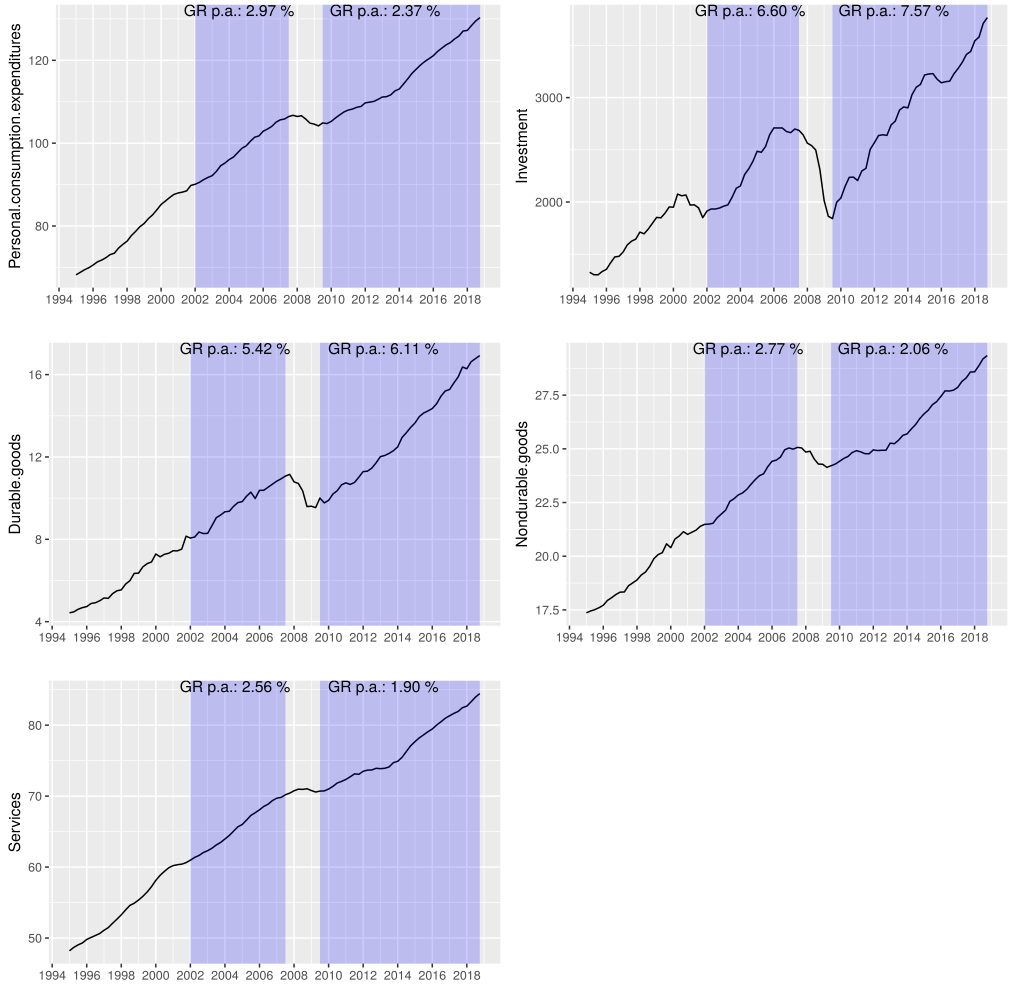
1 Introduction

The cyclical behavior of GDP and consumption growth is an important and well-studied area of macroeconomics. Normally, negative growth in the bust phase is followed by positive growth in the boom phase of a cycle. Over the long term, the boom phase outweighs the bust and we end up with positive economic growth as a general trend. However, the average US GDP growth did not reach pre-crisis levels since the end of the financial crisis in 2009 (Pistaferri, 2016).

Through the lens of the expenditure approach, either consumption or investment should be to blame as government expenditures and net exports constitute only a small part of GDP. Figure 1 depicts US consumption expenditures over time. The shaded areas highlight the two last expansionary periods (2002 Q1 - 2007 Q3 and 2009 Q3 - 2018 Q4). While the growth rates of investment and durable consumption increased when comparing this expansionary period to the last, services and non-durable consumption are lagging behind. This paper sheds light on the question of slow GDP and consumption growth by advocating a demand-side channel.

Figure 1: Consumption and Investment Growth



Notes: These figures show the paths of consumption, components of consumption and investment. Purple areas are the periods we use to calculate the growth rates. While investments have recovered after the previous recession, this does not seem to be the case for consumption

The 2008 financial crisis was followed by a foreclosure crisis which peaked in 2010. Many people were evicted from their homes after not being able to service their mortgages anymore. Since some of this practice was illegal, the Federal Reserve System (Fed) sent reviewers to track the foreclosure processes. Several mortgage issuers signed consent orders that involved big costs for the banks. In our analysis, this translates into a lower growth of mortgage loans originated by lenders which were affected by this

policy. We argue that the decreased mortgage lending slowed down the increase of personal wealth through lower house prices, which slowed the consumption recovery. Our main result suggests that a 10 percentage points higher exposure to controlled banks is associated with a decrease of around 2.4 percent in consumption in the aftermath of the recession. We refer to these lending institutions as controlled banks.

Our empirical analysis consists of three parts. First, we show the effect of foreclosure regulation on the aggregate lending of banks. One challenge is that controlled banks could be operating in regions with slower growth. This would reduce the demand for mortgage loans and bias our coefficients. We use data on mortgage loan applications and aggregate the loans granted by each lending institution on a county level. This allows us to analyze the impact of foreclosure regulation on banks that operate within one county and that are affected by the same demand conditions. Banks that were affected by the policy had a 0.32 percentage point lower mortgage lending growth. We further investigate whether the reduction of loan supply affected all borrowers similarly. Our loan-level data suggests that this reduction happened on the extensive margin rather than the intensive margin. While the average loan amount stays the same, the probability of receiving a loan is 8 percentage points lower for loan applicants with the same characteristics applying at one of the controlled banks.

Second, we show that from 2012 until 2017, counties that were more exposed to these institutions in terms of mortgage loans suffered from slower house prices recovery after the foreclosure crisis. Controlling for county-level characteristics, we find that counties that were more exposed to controlled banks had a slower growth of house prices. Counties with a 1 percentage point higher exposure had a lower growth rate of house prices of on average 0.05 percentage points. Given that the average exposure of counties to controlled banks is around 20 percent, this would amount to a 1 percentage point lower growth rate per year. Endogeneity of the county-level exposure to controlled institutions is one potential issue to our identification strategy. Our solution is to use an alternative measure of exposure to controlled institutions based on interbank state deregulation history (Hoffmann and Stewen, 2019). This helps to overcome the endogeneity arising from the correlation of exposure and the business cycle. We add

the debt-to-income ratio in our regressions and show that the decrease of mortgage loans is not the consequence of a deleveraging process by highly indebted counties. Our results also remain unaffected when controlling for the housing supply elasticity of a county (Saiz, 2010). We hypothesize that the slower growth of house prices led to slower growth of wealth and slower consumption in the affected counties through the housing net worth channel (Mian et al., 2013). Alternatively, accumulated household debt could be responsible for the consumption slowdown. As household leverage does not seem to play an important role according to our analysis, we claim that the lack of house price recovery slows down consumption growth.

Third, we provide evidence on the effect of the foreclosure policy on consumption. One challenge of our approach is that the consumption data is not available at the county level. We tackle this issue by first decomposing consumption growth into various sectors and identifying the sectors responsible for the lack of growth. Then, we show that employment on a county level decreases in counties that were more affected by exposed banks and in the previously identified sectors. The results indicate that a 10 percentage point higher exposure led to a decrease of around 0.5 percentage points in employment growth. Turning to consumption data, we show that state-level consumption decreases as well in more exposed areas. The results indicate that a 10 percentage points higher exposure to controlled banks is associated with a decrease of around 2.4 percent in consumption in the aftermath of the recession. This effect has high magnitude and high economic significance.

We conduct several robustness checks. One immediate concern is that our results could be driven by a decrease in business lending in the areas affected by the foreclosure regulation similarly to the Dodd-Frank act (Bordo and Duca, 2018). We gather data on small business loans, as small businesses are most affected by financing cuts (Chen et al., 2017; Cortes et al., 2018). Moreover, they play an important role in innovation and growth (Haltiwanger, 2015; Alon et al., 2018). Given that small businesses tend to be financially constrained and use real estate as collateral, the decrease in house prices will likely affect their ability to borrow and reduce their output and employment. We investigate the impact of the exposure to controlled banks on small business lending

using county-level data and find that small business loans are not significantly affected by the foreclosure regulation. Turning to a bank-level analysis, we investigate whether controlled banks reduce their supply of small business loans. As controlled banks face additional constraints for mortgage loan origination, more funds might be available for other activities. While there does seem to be an increase in the growth rate of small business loans by controlled banks, our results indicate that much of it is driven by county-specific demand. Next, we investigate whether the effect of consent orders on household debt is in line with our analysis. We showed that controlled banks decreased their lower mortgage loan growth. The effect on households might be mitigated if they can obtain a mortgage from another bank which was not controlled. If the foreclosure regulation had a big impact in exposed counties, they should have lower leverage. Our analysis suggests that the increase in exposure to controlled banks on average decreases the debt-to-income ratio by 4 percentage points. Further, we provide evidence that different pre-existing trends for controlled and non-controlled banks in terms of creditworthiness of customers do not bias our results. We investigate the loan to income ratio of borrowers and find that the patterns are similar for both groups of banks. Different portfolio risks based on the ethnicities or sexes of borrowers do not seem to play a role either. All in all, we do not find evidence that the decrease in mortgage loans was caused by differences in the bank's portfolios. Instead of the foreclosure policy, revisions in agents' permanent income in the aftermath of the financial crisis might have led to changes in consumption. While the revisions in permanent income might have an impact, they cannot fully explain the decrease in consumption in the control period.

Our paper is part of a broad literature that tries to identify the causes of the slow recovery. Pistaferri (2016) gives an overview of the behaviors of GDP and consumption during and after the Great Recession and notes that even after its official end, both variables exhibit lower growth than expected. He prefers the uncertainty and distributive channels to explain the recent consumption slowdown. Our analysis shows that an indirect wealth effect - namely the effect of "feeling poorer" - explains part of the slowdown. In his analysis, the debt service ratio sharply decreases at the onset of the recession and stabilizes at the beginning of 2012. He concludes that the deleveraging

process might explain the lower growth patterns right after the end of the recession, but does not seem to play a role for more recent years. As the debt to income ratio is not significant in our employment regressions, we do not find evidence of a direct effect of debt overhang either.

One reason for the indirect wealth effect described in our paper could be sentiments. There is evidence that they amplified and propagated the Great Recession and lead to a delayed recovery (Makridis, 2019; Malmendier and Shen, 2018). Exploring new measures of credit constraints, Amromin et al. (2017) find that the share of constrained households increased during the recession and remained high during the recovery. Our results are in line with the idea that credit constraints matter and shed light on the causes of these constraints.

Our work is also related to the literature on banking regulation and its effect on loans. Acharya et al. (2018) investigate how the stress tests have reduced credit supply to particularly risky borrowers. Pierret and Steri (2017) study the risk-taking of stress-tested banks since 2011. They document that stress tests encourage banks to invest prudently.

A decrease in house prices could also lead to a decrease in the value of collateral and therefore to a decrease in loans to firms. Thereby, GDP growth might be lower due to decreased productivity because of financial frictions (Duval et al., 2017). However, for the volume of industrial loans, we do not find any effect in the exposed counties. While Fernald et al. (2017) argue that slow TFP growth and falling labor force participation are to blame for the slowdown of GDP, our findings are in line with their patterns of consumption. We also find that housing and financial services exhibit a lower growth rate than before the recession.

This paper is closely related to the research agenda of Mian, Sufi, and their co-authors. Mian et al. (2015) find that foreclosures in 2007-2009 resulted in declining house prices, residential investment, and auto sales which serves as a proxy for consumer demand. They use an instrumental variable approach to find a causal effect between foreclosures and house prices. While the foreclosure crisis seemed to last until 2011, its effect on house prices is negative only until 2009. From 2010 onward, prices seem to in-

crease in response to foreclosures. Instead of analyzing the direct effects of foreclosures, our study deals with the unintended consequences of a policy which was a response to illegal foreclosure activities by banks. Our mechanism highlights higher costs for banks, lower mortgage issuance and thus, lower housing demand. While they take car sales as a proxy for consumption, our data suggest that vehicle purchases growth rose in later years and thus cannot explain the consumption slowdown.

Mian and Sufi (2014) argue that a deterioration in household balance sheets (the housing net worth channel) had an impact on US unemployment between 2007 and 2009. They test the prediction that non-tradable employment should be positively correlated with a change in household net worth as it relies more on local demand. Our results have a similar pattern, as the two sectors that have slower growth in our regressions are the housing and the financial sector.

Mian et al. (2013) provide evidence on the effect of debt and the geographic distribution of wealth shocks on consumption from 2006 to 2009. They identify the housing collapse as a major contributor to the consumption decline in the aftermath of the crisis. Moreover, the effects are stronger for poorer and more levered households. They use cross-sectional variation in the housing wealth shock across the US and the housing supply elasticity and an instrument for the exposure to the cycle. As for the consumption response, they use both auto sales data and county-level consumption data from MasterCard Advisors.

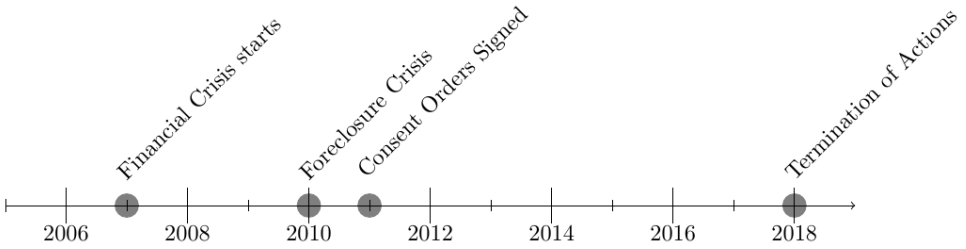
Monetary, fiscal, and regulatory policy might have played a role in propagating the effects of the recession (Taylor, 2014). Our paper provides further evidence on the role of regulatory policy in the slow recovery. This paper is the first to study the unintended consequences of the Fed's policy to illegal foreclosure practices in the aftermath of the financial crisis.

This paper is structured as follows. Section 1 describes the institutional setup; Section 2 describes the data. Section 3 provides the main empirical results regarding the effect of the foreclosure policy on house prices, the labor market, and consumption. Section 4 tests the robustness of our results to alternative explanations and Section 5 concludes.

2 Institutional Setup

The 2008 financial crisis was followed by a foreclosure crisis in 2010. Since our focus is on the effect of the Fed's consent order policy on the demand side of the economy, we study the period from 2011 to 2017.

Figure 2: Timeline of Events



Notes: This figure shows the order of events that lead to the government reaction and the termination of orders.

In the aftermath of the financial crisis, many people were evicted from their homes after not being able to service their mortgages. In 2010, a scandal about so-called "robo-signers" emerged: Bank employees deciding on foreclosure cases did not put adequate time and effort into reviewing each case and just signed them (Whelan, 2010). Certain individuals signed as many as 10,000 foreclosure documents per month (Kagan, 2018). Since this business practice led to illegal foreclosures, the Fed, the Office of the Comptroller of the Currency (OCC) and the Office of Thrift Supervision issued a report on a small sample of foreclosure cases of several mortgage servicers. These were "selected based on the concentration of their mortgage-servicing and foreclosure-processing activities" and comprised around 2/3 of the market. (System, 2011)

Based on this report, the regulators initialized reviews by independent consultants of the foreclosure actions in 2009/10 in several institutions[1]. Two banks were not part of these foreclosure actions, but reached a similar arrangement with the Fed and are

[1]Aurora Bank, EverBank, OneWest Bank, Sovereign Bank (later Santander Bank), Bank of America, Citigroup, HSBC, JPMorgan Chase, MetLife, LPS, MERS, PNC, Wells Fargo, Ally Financial (previously GMAC), SunTrust Banks, U.S. Bank National Association

thus included in our sample[2] 2017. Over time, most banks reached an agreement with the regulators to settle in a one-time payment distributed to all borrowers affected. Additionally, action plans were created to improve the foreclosure practices at the institutions[3]. 2012 These measures entailed big costs for the institutions:

"Significant revisions to certain residential mortgage loan servicing and foreclosure processing practices. Each servicer must, among other things, submit plans acceptable to the Federal Reserve that:

- strengthen coordination of communications with borrowers by providing borrowers the name of the person at the servicer who is their primary point of contact;
- ensure that foreclosures are not pursued once a mortgage has been approved for modification, unless repayments under the modified loan are not made;
- establish robust controls and oversight over the activities of third-party vendors that provide to the servicers various residential mortgage loan servicing, loss mitigation, or foreclosure-related support, including local counsel in foreclosure or bankruptcy proceedings;
- provide remediation to borrowers who suffered financial injury as a result of wrongful foreclosures or other deficiencies identified in a review of the foreclosure process; and
- strengthen programs to ensure compliance with state and federal laws regarding servicing, generally, and foreclosures, in particular."2011

In 2015, the orders were terminated against three institutions, while they were made stricter for six institutions that have not met the requirements [4] 2015.

[2]Goldman Sax, Morgan Stanley

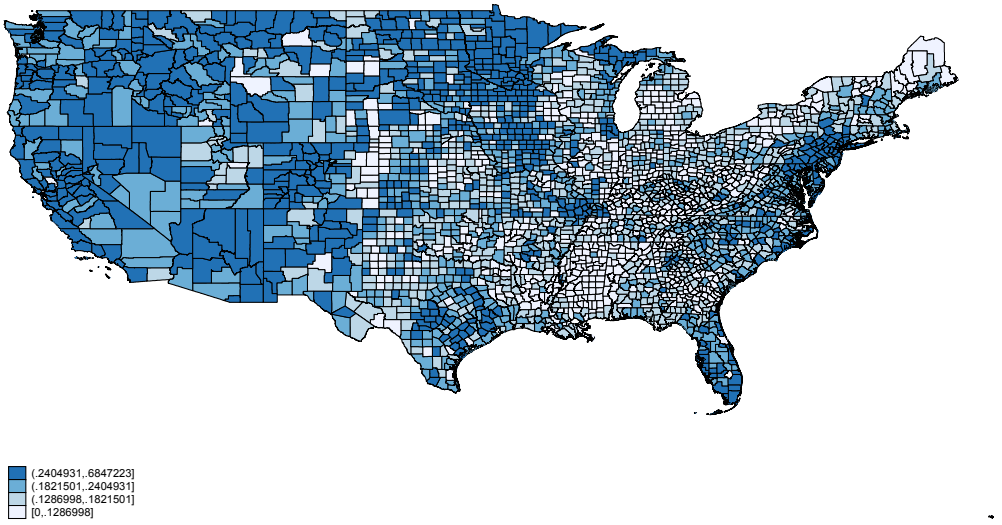
[3]Bank of America, Citigroup, PNC

[4]EverBank, HSBC, JPMorgan Chase, Santander Bank, U.S. Bank National Association

3 Data

We obtain bank-level data on mortgage lending from the Home Mortgage Disclosure Act (HMDA). HMDA provides bank-level data on various characteristics of the mortgage loan originated. This dataset also features information on the year that the application is reviewed, the ID of the institution, the name of the institution, the agency regulating the lender, whether the application for the loan was approved, loan purpose, amount of loan and rate spread. The tract number allows us to determine the county of the applicant. To investigate the impact of the foreclosure policy we construct a variable of exposure of each county to controlled institutions by dividing the loan amounts granted by controlled banks in a county by all the mortgage loans in the respective county, all before 2012. The variation across all US counties is depicted in figure 3.

Figure 3: Exposure to Controlled Banks pre-2012



Notes: This figure shows the exposure of counties in the U.S. to the controlled institutions by the govermnent. Exposure of a county should capture the dependence of this region to the mortgage loans issued by the controlled institutions. Exposure is measured by dividing the sum of exposures to each controlled banks in a year, and divided by the number of years before the government action takes place.

In our analysis, we control for bank characteristics varying across years. These data stem from the Federal Deposit Insurance Corporation (FDIC) and can be matched with HMDA from 2007 onwards. Our characteristics include total assets, total overhead

costs, income, total investment securities, non-performing loans, return on assets, total deposits and the interest rate on deposits. As the data on mortgage loan applications is yearly, all our analysis is on a yearly level. Our analysis does not entail nondepository institutions as they are not part of the FDIC dataset.

To investigate the effect of the foreclosure policy on house price recovery, we use data on house prices from the Federal Housing Finance Agency (FHFA). The data comes on a county level and we obtain the yearly index. House prices could also be affected by the high leverage of households after the recession. To control for this channel we obtain the data from the Fed on debt-to-income on a county level. Additionally, it could be the case that the exposure of counties is highly correlated with the housing supply elasticity. Controlled institutions could have decided to establish more presence in counties with lower housing supply elasticity. We control for it using the measure provided by Saiz (2010). In our county-level regressions, we also include personal income and growth of population from the Bureau of Economic Analysis (BEA) and unemployment from the Bureau of Labor Statistics (BLS). Data on population is obtained from the United States Department of Agriculture. A housing inventory dataset is available at the state-level and comes from the Realtor.

To examine the effects of the foreclosure policy on consumption, we first show evidence for employment on a county level and then proceed to investigate consumption on the state level. Our employment data from BEA is available at the county level and reported yearly. One of the advantages of this data set is that it is disaggregated on the industry level. The consumption data comes from BEA and is only available on the state level. As the industry level aggregation is not the same as for the consumption data, we explain how we relate the two classifications relate to each other in the Appendix.

To rule out that the consumption slowdown originates on the supply side, we gather data on the commercial loan amounts granted to small firms. This data is available from the Community Reinvestment Act (CRA) and features information on the loan amounts granted to small businesses, the unique identifier of the bank, and the number of loans granted. Importantly, this data is available at the county level.

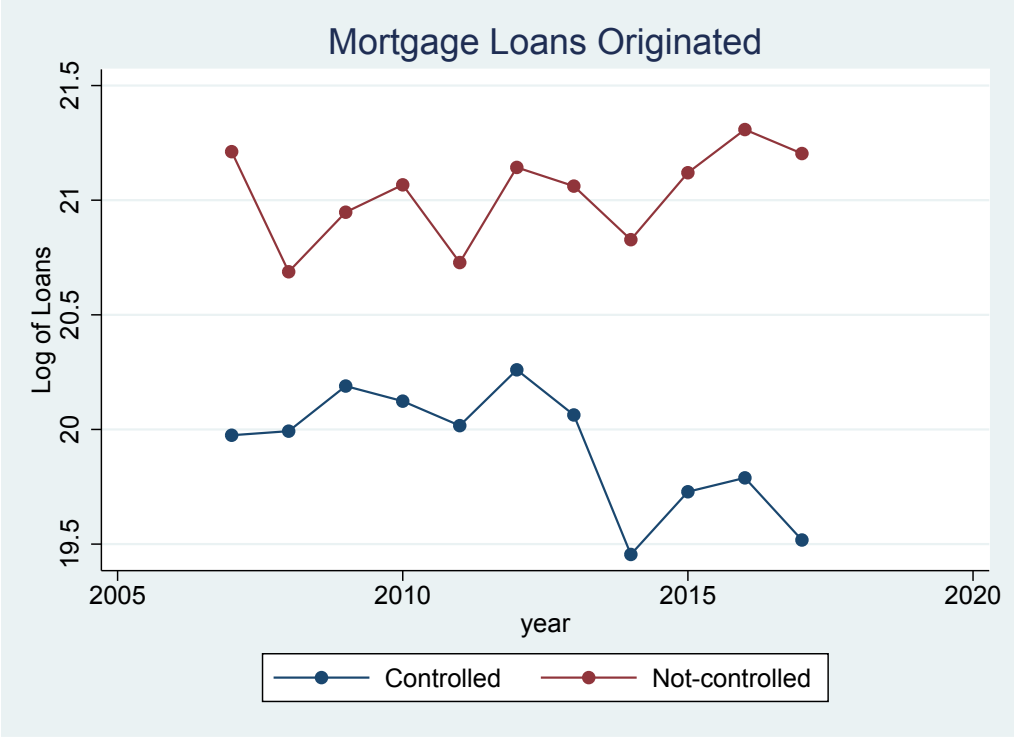
4 Empirical Analysis

We organize our empirical analysis in several steps. We first document the effects of the foreclosure regulation on the mortgage loan growth of banks. Our results suggest that controlled banks experience a decrease in their loan growth. This decrease in mortgage loan growth has important implications for house prices in the areas which are exposed to controlled institutions. In the next step, we investigate the impact of exposure on house prices on a county level. We find that counties that were more exposed to controlled banks had a slower growth of house prices. We hypothesize that the slower growth of house prices led to slower growth of wealth and slower consumption in the aforementioned counties. Since data on consumption on a county level is not available, we provide two analyses to close that gap. First, we remain on a county level and identify sectors that experienced slower growth. We examine the effect of exposure to controlled banks on employment in sectors that we suspect will be affected. We find that employment decreases in the slower-growing sectors in exposed counties. Then, we proceed to the state level and show that our results are consistent with our findings on the county level: States that were more exposed to controlled institutions have experienced lower consumption growth.

4.1 Effect of Consent Orders on Loans Originated

In this section, we show that institutions that were subject to consent orders decrease their mortgage loan origination. Consent orders related to foreclosure actions consisted of many different requirements for the banks that were under scrutiny. Among other measures, the banks had to improve their management of new foreclosures and train their employees to act within the legal framework. All the requirements entailed additional costs for the lenders. Both newly issued mortgage loans and future foreclosure court proceedings are more expensive for the affected banks. Figure 4 shows the loan amounts originated for controlled and non-controlled institutions. The mortgage loan amounts originated decrease after 2012 for controlled institutions.

Figure 4: Mortgage Loans Originated



Notes: Blue line represents the log of the sum of mortgage loans issued by controlled institutions in a year. Red line are the loans issued by the non-controlled institutions. It is noticeable that there is a large decline in mortgage loans issued by controlled banks after the government measures were introduced.

We run the following regression, where $\Delta log(loan_{bct})$ is the growth of mortgage loans originated by bank b in county c at time t, $controlled$ is the dummy variable if the bank has signed a consent order with the Fed or the OCC, X_{bt} are the last period growth rates of control variables such as total assets, total overhead costs, income, total investment securities, non-performing loans, return on assets, total deposits and interest rate on deposits, θ is the vector of coefficients related to those controls. In our regressions we also include the interaction term $cont_{bt}Asset_{<10bn}$ for lending institution being small to investigate the possibility that the small lenders are driving our result. This measure might not be able to capture the extent of diversification of the lender's portfolio. For that reason, we also include HF_b, the Herfindahl-index of bank b loans in the pre-control period. We calculate the Herfindahl index in the following way:

Table 1: Bank-County-Year Regressions

	(1) $\Delta log(loan)$	(2) $\Delta log(loan)$	(3) $\Delta log(loan)$	(4) $\Delta log(loan)$	(5) $\Delta log(loan)$	(6) $\Delta log(loan)$	(7) $\Delta log(loan)$
$cont_{bt}$	-0.2506***	-0.2579***	-0.2147***	-0.2192***	-0.3191**	-0.2571*	-0.2898*
	(0.0915)	(0.0940)	(0.0812)	(0.0826)	(0.1423)	(0.1388)	(0.1524)
$cont_{bt}Asset_{<10bn}$			-0.4930	-0.5208		-0.3448	
			(0.5614)	(0.5676)		(0.4892)	
$cont_{bt}HF_b$							-0.0261
							(0.0623)
N	684967	684161	684967	684161	38024	38024	37430
Bank-County FE	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	no	yes	no	yes	yes	yes
County-Year FE	no	yes	no	yes	no	no	no

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the bank-county-year level. Columns 5,6 and 7 show that these results translate to aggregate lending by controlled banks. Columns (1)-(4) feature data on mortgage lending at the bank-county-year level. Columns (1) includes bank-county fixed effects. Column (2) adds both bank-county and county-year fixed effects. Columns (3) and (4) control for small banks by including the dummy for asset size lower than 10 bn. Column (5) repeats regression at the bank-level, and columns (6) and (7) control for interaction between herfindahl index of banks mortgage portfolio and asset size, respectively. Standard errors are clustered at the holding level.

$$HF_b = \frac{1}{T} \sum_{t=2007}^{2011} \sqrt{\sum_{l \in B(b)} (\alpha_t^{b,l})^2}, \tag{1}$$

where $B(b)$ is the set of locations in which the lending institution i active, $\alpha_t^{b,l}$ denotes the share of the lender's portfolio issued in year t. In our main specification, we compute the HF_b on a county level. We also provide results with HF_b calculated at an MSA-level and the state-level.

$$\Delta log(loan_{bct}) = \beta_0 + \beta_1 controlled_{bt} + \theta X_{bt} + \phi_t + \sigma_{ct} + \eta_{bc} + \epsilon_{bct} \tag{2}$$

The first four columns of Table 1 are on the bank-county-year level. While we are interested in the effect of the foreclosure regulation, a decrease in mortgage loans could also stem from controlled banks being more exposed to counties that are affected by deteriorating economic conditions. The county-year fixed effects control for the county-specific demand conditions. Additionally, bank-level data from the FDIC enables us to control for various factors that could be correlated with being controlled. For example, institutions that are controlled could be the ones that had a large decrease in assets or

a high share of non-performing loans, which would bias our results.

Column 1 in Table 1 shows that if we include bank-county, year and county fixed effects, the controlled banks experience on average 25 percentage points lower growth of mortgage loans. Including county-year fixed effects in column 2, we find that our results do not change significantly. Therefore, regional economic conditions do not seem to influence our estimate. Our results could be driven by the presence of small banks in our sample of controlled institutions. It might be that small banks decrease their mortgage loan growth, while big banks could be decreasing their loan supply in some counties, and increasing it in others. Put differently, small banks cannot diversify their loan portfolio as easily as big banks. Columns 3 and 4 suggest that this is not the case. In both columns, we interact the dummy for being controlled with the dummy if the institution has assets that are lower than 10 billion dollars. Although the coefficient on banks with lower assets is negative and large in magnitude, it is not significantly different from 0. While the coefficient on our main variable of interest does increase, it is not significantly different from the one in columns 1 and 2.

The previous results suggest that the requirements imposed on institutions that were controlled had a significantly negative impact on mortgage loan growth on the county level. In Columns 5, 6 and 7 we perform regressions on the bank-year level, where we investigate what happened to the aggregate loan growth of banks that were controlled. Our first specification in Column 5 presents the results where we include year and bank fixed effects. They suggest that controlled banks had around 31 percentage points lower mortgage loan growth. Column 6 does not suggest that the results are driven by small banks. Further, it could be that the banks that are responsible for the decrease in the growth of mortgage loans are not well diversified. In that case, those banks would depend on giving out loans in areas with deteriorating economic conditions without the possibility to reallocate their lending to other counties. We construct the Herfindahl-index for the banks' mortgage loan diversification and interact it with the dummy for being controlled. Column 7 shows that our results do not change and that the interaction is insignificant and small in magnitude. Overall, our results suggest that the regulations imposed on the controlled banks had a significantly negative impact on

Table 2: Bank-Year Regressions - Diversification Robustness

	(1) $\Delta log(loan)$	(2) $\Delta log(loan)$
$cont_{bt}$	-1.5089*	-0.5748**
	(0.8369)	(0.2907)
$cont_{bt}HF_{b,MSA}$	0.4755	
	(0.3835)	
$cont_{bt}HF_{b,State}$		0.1912
		(0.1456)
N	37430	37430
	(0.3835)	(0.3835)
N	37430	37430
Year FE	yes	yes
Bank FE	yes	yes

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the bank-year level using two additional measures of diversification of mortgage portfolio. Column 1 uses Herfindahl index calculated at an MSA-level. Column 2 uses Herfindahl index calculated at the state level. Standard errors are clustered at the holding level.

their mortgage loan supply.

Hoffmann and Stewen (2019) argue that using a Herfindahl index calculated at the county level can be misleading, as economic shocks affecting counties are likely to be correlated within MSA or a state. For that reason, our next table provides additional robustness checks.

Table 2 shows that our results do not change qualitatively, although our coefficients do change. Column 1 presents our results when we calculate the Herfindahl index at the MSA-level. Our coefficient of interest remains significant and the coefficient is lower. The same is true in Column 2, where we calculate the diversification measure at the state-level.

Previous regressions have three sets of results that try to determine the role of small banks in driving the results that controlled institutions decrease their lending due to consent orders. First, we have included a dummy variable if a bank has total assets smaller than 10 billion dollars. The coefficient, even though it is not significant, is very negative. This could as well be because the number of small banks to big controlled

institutions is relatively small. However, this is not the case, as the number of small controlled institutions is 12 and the total number of controlled institutions is 39, so it is unlikely that the small sample is driving the result. Additionally, results that directly use the measure of diversification of an institution's portfolio further strengthen being controlled is what causes these institutions to decrease their lending and not big losses due to low diversification.

4.2 Intensive vs Extensive Margin

Our previous considerations suggest that the institutions controlled by the Fed reduced their supply of mortgage loans. We now investigate whether the reduction of loan supply affected all borrowers similarly, or whether loan applications at the threshold were reduced more. Our data is loan-level and obtained from HMDA. This dataset features information on each application such as the income of the borrower, the loan amount requested, the institution where the borrower applied, type of loan, state and county of the borrower, applicant sex, and applicant ethnicity. Given that our dataset is loan-level we can run the following regressions:

$$dep_i = \beta_0 + \beta_1 \mathbf{1}_{cp} controlled_b + X_i + \eta_c + \phi_t + \epsilon_{ibt} \tag{3}$$

The dependent variable dep_i is either the decision by the bank whether to approve the loan or the log of the loan amount of applicant i. $\mathbf{1}_{cp}$ stands for the control period and $controlled_b$ is the dummy for controlled banks. In both of our specifications, we add the income of the borrower as a control variable. In the case where the dependent variable is the decision on the application, we additionally include the loan amount. All our specifications include fixed effects for the sex and the ethnicity of the applicant. To control for bank characteristics that vary at the year level and might affect loan supply, we include bank-level controls such as total assets, total overhead costs, income, total investment securities, non-performing loans, return on assets, total deposits and the interest rate on deposits. All of these are in logs and lagged one period. Further, the loan amount or the decision on the loan application could be influenced by county characteristics that change over the years, such as house prices in the county or em-

Table 3: Intensive versus Extensive Margin

	(1) $log(loan_i)$	(2) $log(loan_i)$	(3) $log(loan_i)$	(4) $loanoriginated_i$	(5) $loanoriginated_i$	(6) $loanoriginated_i$
$cont_{bt}$	-0.0723	-0.0309	-0.0362	-0.0808***	-0.0935***	-0.0945***
	(0.0631)	(0.0503)	(0.0361)	(0.0171)	(0.0064)	(0.0084)
N	79136833	46990176	46990099	79136833	46990176	46990099
Bank-County FE	yes	yes	yes	yes	yes	yes
County-Year FE	no	no	yes	no	no	yes
Bank Controls	no	yes	yes	no	yes	yes

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the individual application level. First three columns have the log of an individual loan as an independent variable. Columns (4),(5) and (6) use the application decision by the institution as an independent variable. Standard errors are clustered at the holding level. Results suggest that the cut in the loan supply is on the extensive margin.

ployment. We include county-year fixed effects to alleviate this concern. County-bank fixed effects control for the possibility that the banks have different lending standards across different counties.

Columns 1-3 of Table 3 analyze the impact of the controls on the intensive margin of loan supply. Column 1 includes bank-county fixed effects and shows that controlled institutions do not significantly reduce the average amount of loans. Column 2 includes bank-level variables to control for the possibility that our coefficient is biased by characteristics that vary at the bank-year level such as non-performing loans. Our coefficient of interest is not significant and even closer to zero. Column 3 includes county-year fixed effects, to control for the possibility that our results are biased by county-specific demand. The coefficient remains insignificant. All in all, we reject economically important magnitudes for a reduction of the average loan amount by controlled banks. In Column 4, the dependent variable is a dummy variable indicating whether the loan was approved and thus represents the extensive margin. 1 stands for mortgage loan originated and 0 for the denial of a mortgage loan application. We include bank-county level fixed effects to control for the possibly different lending standards of banks across counties. The coefficient of interest suggests that the probability of receiving a loan is 8 percentage points lower for loan applicants with the same characteristics applying at

one of the controlled banks. This result might be biased due to various factors changing at the bank level. For example, many banks experienced an increase in non-performing loans during the crisis. This could have lead to more cautious lending policy. In column 5 we additionally include bank-level controls. Our results stay statistically significant and even higher in magnitude. A decrease in demand for mortgage loans in areas heavily affected after the crisis might also be the reason for these results. To control for this possibility we include the county-year fixed effects. Our estimate remains statistically significant and negative. Overall, these results suggest that the cut in loans after 2012 manifests on the extensive rather than the intensive margin.

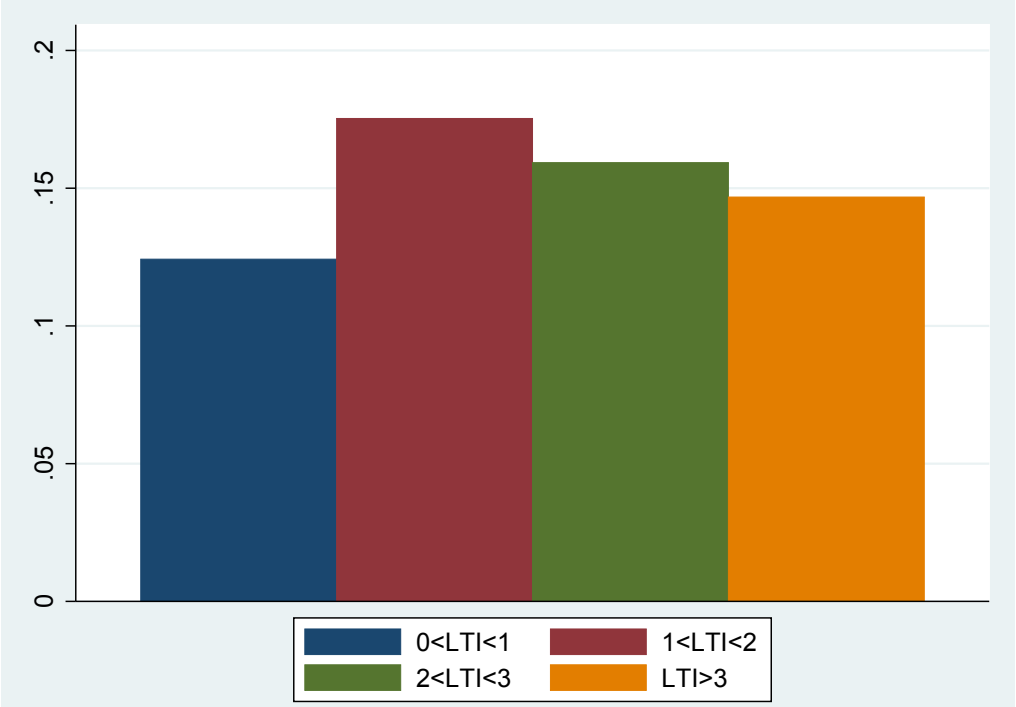
These results point in the direction that certain borrowers were affected by the regulation policies more than other since the cut in lending was on the extensive margin. It also raises a question on what was the reasoning of financial institutions when they made a decision to decrease the loan supply, after being informed that they will be controlled by government institutions. There are many reasons one can think that would be a good reason to cut lending. For example, it could be the case that the portfolio of controlled institutions was such that the losses due to foreclosure were too high, which depleted their capital and the possibility to lend. Another possibility is that the controlled institutions have anticipated an increase in the costs of future foreclosures, which meant an unprofitable business for them with lower quality borrowers. There is much evidence that the latter is the case.

As we have previously described, when the foreclosure crisis started, the government was determined to slow down this process by making certain institutions sign consent orders, which involved significant costs for banks. However, these measures included not only institution-side policies, but also many other state-level policies, which were aimed at making the foreclosure a costly, uncertain, and unattractive process for lenders. According to Realty Trac (Henry, 2014), at the beginning of the crisis, it took on average 120 days to foreclose on a home. The average time increased by more than 4 times in the first quarter of 2014. Not only was there an increase in the time it took to go through the process, but also the costs of such process have risen astronomically for them, according to Kevin Watters, chief executive of JPMorgan. Additionally, they

lost its trust in the insurance provided by the government against these loans defaulting (Henry, 2014), which made them more reluctant to lend. The mortgages that were most likely to be affected by this development are the ones with low-quality borrowers.

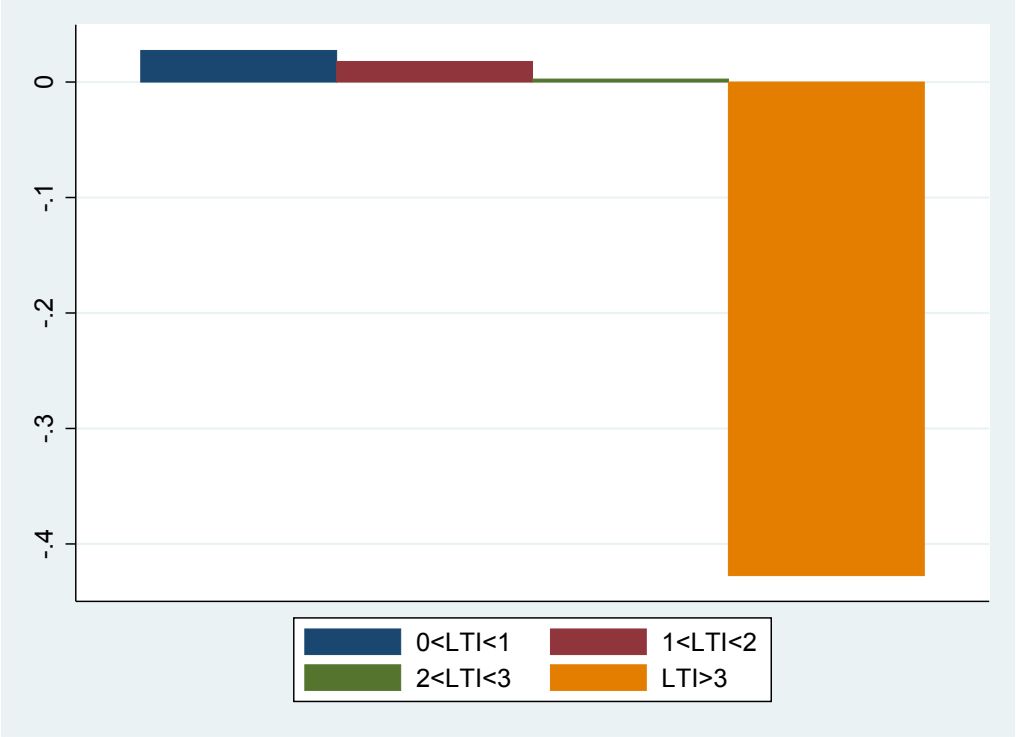
We further investigate whether this decision can be captured in the data. Of course, the risk of a borrower is a multidimensional measure, and banks rely on many variables to determine a borrower's risk. The measure that is readily available to us is the loan-to-income of a borrower. We want to know whether controlled banks have increased the rejection rates for low-quality borrowers. We obtain data on loan-to-income on an individual level. We split all mortgage loan applications into the ones received by controlled and non-controlled institutions. For simplicity, we split the loan-to-value ratio into four sections and obtain rejection rates of mortgage applications across these 4 ranges. One problem is that the crisis lasted until 2009, which will automatically increase the rejection rates. For that reason, we take only year 2010 as a first period, when constructing the graphs. The second period is calculated using the years after signing the consent orders. Each bar represents the difference in rejection rates in the second period compared to the first period.

Figure 5: Difference in Rejection Rates for Controlled Institutions



Notes: This figure shows the difference in rejection rates for the controlled institutions, separated in 4 sections using LTI, to proxy for the riskiness of the borrower. First, we calculate the rejection rates for the pre-control and the post-control period. Then we subtract the rejection rates in the first period from the second period. This means, for example, that the rejection rates for borrowers with LTI between 0 and 1 have increased by around 12.5 percentage points after the measures were introduced.

Figure 6: Difference in Rejection Rates for Non-Controlled Institutions



Notes: This figure shows the difference in rejection rates for the non-controlled institutions, separated in 4 sections using LTI, to proxy for the riskiness of the borrower. First, we calculate the rejection rates for the pre-control and the post-control period. Then we subtract the rejection rates in the first period from the second period. This means, for example, that the rejection rates for borrowers with LTI between 0 and 1 have increased by around 0.4 percentage points after the measures were introduced.

Figures 5 and 6 show that the rejection rates have increased substantially for all the loan categories if we look at controlled institutions. However, it is noticeable that the borrowers with higher LTI — higher risk — have faced higher rejection rates in the second period, compared to the first. This is not the case for the non-controlled institutions. This suggests that the bulk of the cut in the mortgage lending was taken by the low-quality borrowers.

While the previous graphs certainly suggest that the loan-to-income as a measure of risk has been an important determinant in the lender's decision to approve a loan, these results certainly depend on how we define the number of bins for loan-to-income. To

provide a more robust results, we resort to regression analysis as a way to quantify the importance of loan-to-income in determining its importance for the application process for controlled institutions. In order to investigate the hypothesis that the decline in the amount of loans generated was mostly due to the cut in lending to risky borrowers we run the following regression:

$$decision_i = \beta_0 + \beta_1 \mathbf{1}_{cp} controlled_b + \beta_2 \mathbf{1}_{cp} controlled_b LTI_i + \beta_3 LTI_i + X_i + \eta_c + \phi_t + \epsilon_{ibt} \quad (4)$$

The dependent variable $decision_i$ is a binary variable, equal to one if the mortgage loan application was accepted. $\mathbf{1}_{cp}$ stands for the control period and $controlled_b$ is the dummy for controlled banks. All our specifications include fixed effects for the sex and the ethnicity of the applicant. To control for bank characteristics that vary at the year level and might affect loan supply, we include bank-level controls such as total assets, total overhead costs, income, total investment securities, non-performing loans, return on assets, total deposits and the interest rate on deposits. All of these are in logs and lagged one period. It is also possible that the controlled banks are more exposed to counties whose indebted population has decreased their demand for loans. For this reason, in our last specification we include county-year fixed effects. Additionally, we also include bank-county fixed effects.

The first row indicates being a controlled bank is associated with a decrease in the chance of being approved a mortgage loan. In the second row, we have an interaction between a bank that is controlled and the loan-to-income ratio of a borrower. Variable in the third row represents a loan-to-income of a borrower i. Results in the third row suggest that, over the whole period, applicants with a higher loan-to-income were more likely to be rejected. Moreover, in the second column we can see that during the control period, banks who were subject to regulations were less likely to approve mortgage loans to riskier borrowers. This observation further substantiates our hypothesis that those banks that were faced with stricter regulation on foreclosures, have reacted by cutting down on those who were most likely to default, and potentially present an additional cost to a bank.

Table 4: Borrower's Risk and Lending

	(1)	(2)	(3)
	$loanoriginated_i$	$loanoriginated_i$	$loanoriginated_i$
$cont_{bt}$	-0.0416**	-0.0712***	-0.0914***
	(0.0171)	(0.0150)	(0.0085)
$LTI_i x cont_{bt}$	-0.0007*	-0.0009***	-0.0013***
	(0.0004)	(0.0003)	(0.0003)
LTI_i	-0.0012***	-0.0010***	-0.0011***
	(0.0004)	(0.0003)	(0.0004)
N	79569408	47426320	46990099
Bank-County FE	yes	yes	yes
County-Year FE	no	no	yes
Bank Controls	"no"	Controls "yes"	Controls "yes"
Controls			

Note: Standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on an individual level. The purpose is to investigate whether the banks that were controlled, were decreasing the amount of loans originated by refusing to give out loans to higher risk borrowers.

These observations can explain why the decrease in consumption that we saw was so strong. Mortgage loans can be used for home purchase, home improvement or refinancing. For those consumers that are poor, a declined mortgage means more savings to either repay other debt, or apply with larger downpayment, in order to increase the chances of being accepted in the future. Both of these lead to a decrease in consumption. On the other hand, this decrease in the supply of mortgage loans further decreases the value of houses of other homeowners, decreasing their possibility to use the money from refinancing for consumption.

In the Appendix we show that the rejection rates for refinance loans have significantly increased for both groups of institutions, which suggests that that a second-order effect of consent orders exsists. Rejection rates for other mortgage loans have increased significantly for controlled institutions in 2012 and remained high for two more years, and after that decreased.

4.3 Effect of Consent Orders on House Prices

Before, we showed that the foreclosure regulation led to a decrease in mortgage loan supply. In this section, we provide evidence that the cut in mortgage supply led to lower growth of house prices. It is not surprising that house prices decrease if there are not enough mortgage loans since it is harder to buy a house due to more financially constrained households (Imbs and Favara, 2011). This does not necessarily have to be the case though since improving economic conditions after the crisis could counteract this effect.

We run the following regressions on a county-year level, where we control for the possible factors that could influence the house prices in the region:

$$\Delta log(HPI_{ct}) = \beta_0 + \beta_1 \mathbf{1}_{cp} exp_c + \theta X_{ct} + \eta_c + \phi_t + \epsilon_{c,t} \tag{5}$$

The dependent variable is the growth rate of the house price index in county c in year t. $\mathbf{1}_{cp}$ is a dummy for periods in which the consent orders are active and starts in 2012, exp_c is the average share of controlled institutions in total mortgage loans originated from 2007-2011, X_{ct} is a vector of other county-specific controls such as log population, labor force participation rate, unemployment rate, and log income per capita, all lagged by one period.

We hypothesize that counties that are more exposed to controlled banks experience a slower growth of house prices. It is possible that counties with higher exposure to the controlled banks had a large increase in mortgage loan supply before 2012 and during the crisis. As a result, high exposure of a county to these banks is a consequence of the high growth of mortgage loans by controlled banks due to favorable economic conditions. If this was true, the slower growth of house prices in more exposed counties in our regressions could happen because these counties are just experiencing a cyclical downturn. We control for this possibility in two ways. The first method is to take the mean of exposures over all years varying on the county level:

$$exp_c = \frac{\sum_{t=2007}^{2011} exposure_{ct}}{5}, \tag{6}$$

where the variable exp_{ct} is the mortgage loan amount granted by controlled banks at time t to county c. The second way is to follow Hoffmann and Stewen (2019), where we make use of the interstate banking deregulation that took place from 1980-1995. Intuitively, the newly constructed variable shows the exposure of a county to controlled banks that arises as a result of interstate banking deregulation and is not correlated with business cycle fluctuations which could affect the exposure variable. Details of how we calculate $exp_{d,c}$ can be found in the Appendix, subsection 4.7.7.

Our results in Table 5 suggest that counties with high exposure to the controlled institutions had lower growth in house prices. Column 1 presents the baseline results and includes only county fixed effects and controls. Counties with a 1 percentage point higher exposure had on average a 0.05 percentage points lower growth rate of house prices. Given that the average exposure of counties to controlled banks is around 20 percent, this would amount to a 1 percentage point lower growth rate. Column 2 includes controls for time fixed effects and the results are identical to the previous regression. In the last two columns, we use our alternative measure of exposure to controlled banks $exp_{d,c}$. The results in Column 3 show that the effects of the foreclosure regulation have a higher impact on the house prices in counties exposed to controlled banks. Column 4 suggests that the effect of being exposed to controlled banks is on average almost 3 times higher than in column 1 and that the average county could experience a decrease in the growth rate of house prices of almost 3 percentage points.

4.4 Consumption Slowdown by Components

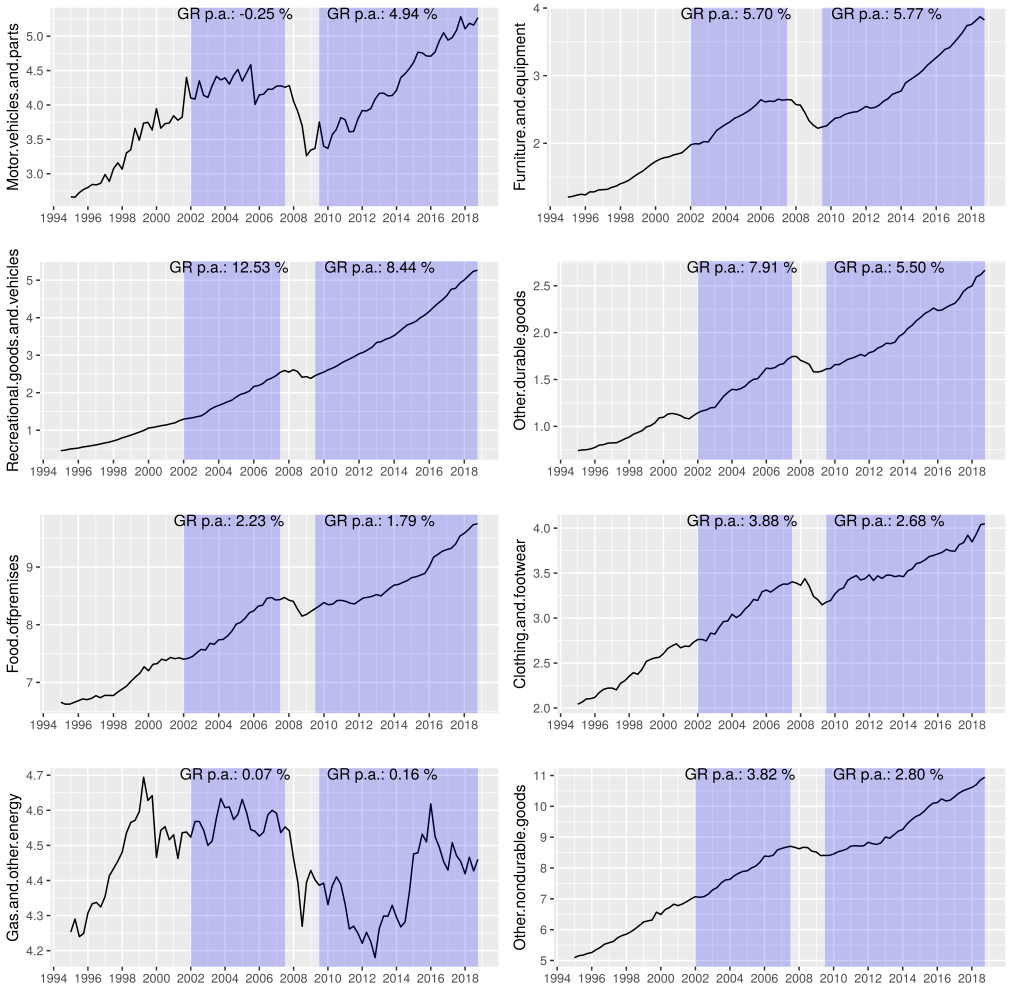
Figure 1 from the introduction shows that consumption growth is lower than in the previous recovery period. Our hypothesis is that the cut in mortgage lending has decreased consumption growth, as the household balance sheets deteriorate due to falling house prices (Mian et al., 2015; Mian and Sufi, 2014). To further investigate this claim we divide consumption growth into categories. We obtain disaggregated data on consumption from BEA. This disaggregation entails data on 15 different categories.

Table 5: House Prices and Exposure to Controlled Banks

	(1) $\Delta log(HPI_{ct})$	(2) $\Delta log(HPI_{ct})$	(3) $\Delta log(HPI_{ct})$	(4) $\Delta log(HPI_{ct})$
$\mathbf{1}_{cp} exp_c$	-0.0553***	-0.1394***		
	(0.0167)	(0.0167)		
$\mathbf{1}_{cp} exp_{d,c}$			-0.1384**	-0.1654**
			(0.0196)	(0.0655)
N	7369	7369	7369	7369
County FE	yes	yes	yes	yes
Year FE	no	yes	no	yes

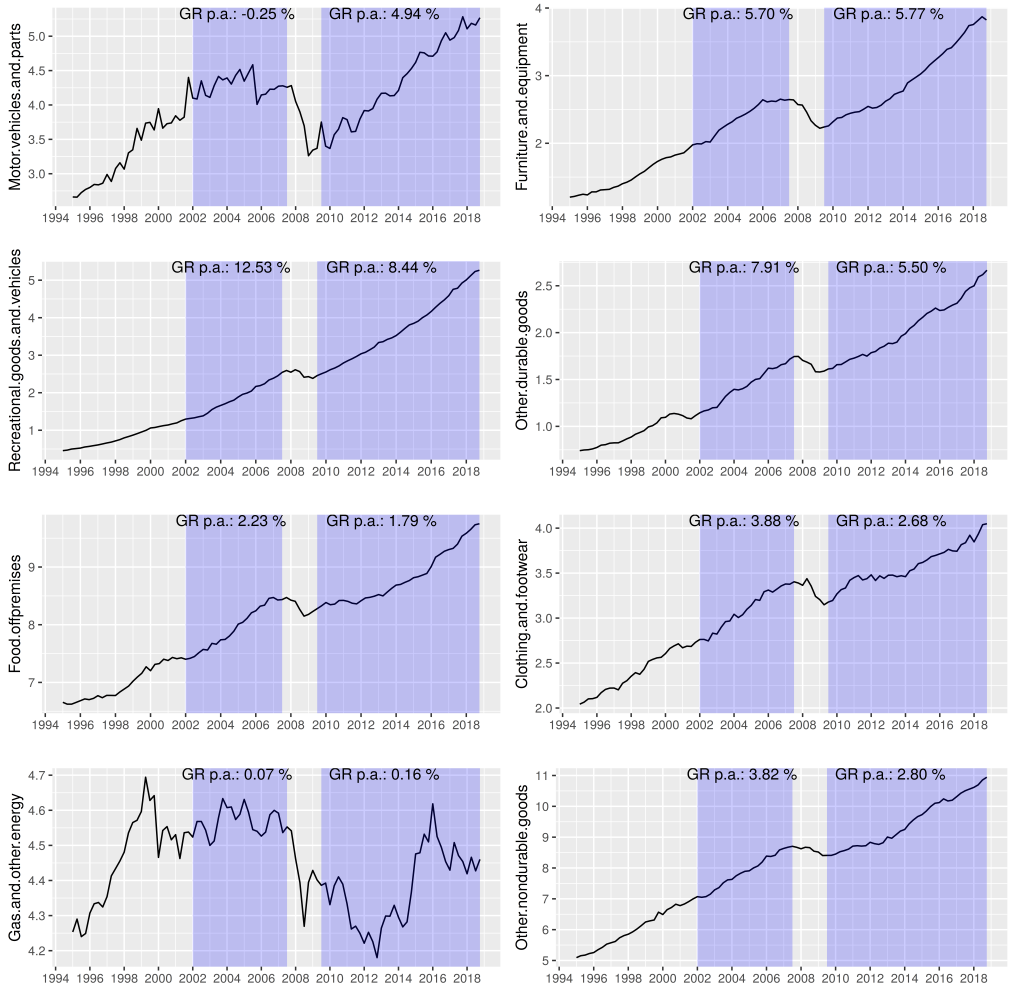
Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the county-year. Columns (1) and (2) use a standard measure of exposure, obtained by dividing the average exposure of each county to controlled banks divided by number of years. Columns (3) and (4) use the exposure measure described in the Appendix. In both specifications, first regression is always without year fixed effects. Columns (1) and (2) show that counties with higher exposure to controlled banks experienced slower recovery of house prices. This result does not change when we use alternative exposure measure and add time fixed effects. Standard errors are clustered at the county level.

Figure 7: Components of Durable and Nondurable Goods Consumption



Notes: These figures show the paths of durable and nondurable goods consumption. Purple areas are the periods we use to calculate the growth rates. In the durables category, recreational goods and other goods exhibit a decline. In the nondurable category, all the components experience a slowdown, except for the gasoline.

Figure 8: Components of Services Consumption



Notes: These figures show the paths of different categories of services. Purple areas are the periods we use to calculate the growth rates. Two major subsectors have experienced a big decline — financial services and the housing subsector. The only category that does not decline is transportation and other services.

Figure 7 shows that the durables (upper four graphs) exhibit a lower growth rate this expansionary period for recreational goods and other goods, but not for vehicles and furnishing. Previous work found that car sales were slower than usual in this expansionary period (Mian et al., 2015). Here, we show that over a longer period after the recession, vehicles cannot be to blame for the slow consumption growth. The lower four graphs in Figure 7 show decreased growth in all sectors except gasoline.

The services components in Figure 8 experienced the major decrease in two subsectors: housing declined from 2 to 1.2 percent and the financial services sector from 2.6 to 0.59 percent. Most other components declined as well, except for transportation and other services.

We claim that the household balance sheet effect caused by a cut in lending due to the foreclosure regulations led to a decrease in consumption. If this is true, we expect consumption growth on a sectoral level to be slower in counties that were more exposed to controlled banks. Since consumption data is not available on a county level, we first turn to employment to investigate our claims.

4.5 Employment by Components

So far, we provided evidence that the foreclosure policy affected mortgage loan supply and house prices in counties that were more exposed to the controlled banks. Relying on previous results from the literature (Mian et al., 2015; Mian and Sufi, 2014), we expect to see the housing net worth channel: household's balance sheets deteriorate due to a decrease in house prices and thus consumption decreases. Since there is no consumption data available on a county level, we now turn to county-level employment data. If consumption decreases, employment should decrease as well due to a lack of demand. We investigate whether this is true for the sectors we found to be detrimental for consumption growth. Particularly, we are interested in industries with the slowest consumption growth and a high share in consumption. Our correspondence between the two sectoral classifications and the identification of the sectors of interest can be found in the Appendix.

$$log(emp_{ict}) = \beta_0 + \beta_1 \mathbf{1}_{cp} exp_c + \beta_2 \mathbf{1}_{cp} exp_c sec_d + \beta X_{it} + \delta_c + \phi_t + \eta_{ct} + \sigma_{it} + \kappa_{ic} + \epsilon_{ict} \quad (7)$$

The dependent variable is the employment growth rate in sector i, county c at time t. $\mathbf{1}_{cp}$ is a dummy for the period when banks are controlled, sec_d is a dummy, which is equal to 1 if the sector is one of the previously identified sectors[5]. Additionally, we

[5]Housing and Utilities, Health Care, Recreation services, Food services and accommodation and

Table 6: County Level Exposure and Employment

	(1) $log(emp_{ict})$	(2) $log(emp_{ict})$	(3) $log(emp_{ict})$	(4) $log(emp_{ict})$	(5) $log(emp_{ict})$	(6) $log(emp_{ict})$
$\mathbf{1}_{cp} exp_c$	0.0156	0.0000	0.0000			
	(0.0139)	(.)	(.)			
$\mathbf{1}_{cp} exp_c sec_d$	-0.0485**	-0.0480**	-0.0462**			
	(0.0205)	(0.0204)	(0.0205)			
$\mathbf{1}_{cp} DTI_{2011}$			0.0000			0.0000
			(.)			(.)
$\mathbf{1}_{cp} DTI_{2011} sec_d$			-0.0001			-0.0001**
			(0.0001)			(0.0001)
$\mathbf{1}_{cp} exp_d$				0.0416	0.0000	0.0000
				(0.0423)	(.)	(.)
$\mathbf{1}_{cp} exp_d sec_d$				-0.1546**	-0.1466**	-0.1576**
				(0.0701)	(0.0699)	(0.0686)
N	411606	411543	411543	411606	411543	411543
Industry-Year FE	yes	yes	yes	yes	yes	yes
County-Industry FE	yes	yes	yes	yes	yes	yes
County-Year FE	no	yes	yes	no	yes	yes

Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the county-industry-year level. Columns (1),(2) and (3) use a standard measure of exposure, obtained by dividing the average exposure of each county to controlled banks divided by number of years. Columns (4), (5) and (6) use the exposure measure described in the Appendix. In both specifications, first regression is always without year fixed effects. Standard errors are clustered at the county-industry level. Results show that those counties that had higher exposure to controlled banks experienced a larger slowdown in the sectors we previously identified as slow-growing.

include DTI_{2011}, debt-to-income ratio in 2011 in our regressions. We want to investigate the possibility that the cross-county level of indebtedness in 2011 has been responsible for the difference in employment changes. All regressions in table 6 include county-level controls such as log population, labor force participation rate, unemployment rate, and log income per capita, all lagged by one period.

Columns 1 and 2 of Table 6 investigate our main hypothesis that employment decreased in the sectors that we have identified as growing slowly. Our variable of interest is the interaction $exp_c sec_d$. Our specification in Column 1 includes industry-year and county-industry fixed effects. Industry-year fixed effects capture everything that would

Financial Services and Insurance

happen to a certain industry in one year and help us eliminate the possibility that our results are driven by other policies affecting the chosen sectors at the national level. County-industry fixed effects control for county-industry specifics. For example, the financial sector could be larger in some counties, which could be related to exposure and bias our analysis. The results indicate that a 1 percentage point higher exposure leads to around 0.05 percent decrease in employment growth. Column 2 additionally controls for the county-level demand effects and leaves our results unchanged. Column 3 includes the debt-to-income ratio in 2011. Our estimates point in the same direction and the coefficient is similar. This speaks against the hypothesis that the previous leverage of households was responsible for the slow growth in the period we look at. Our coefficient on the exposure dummy suggests that there is no effect of exposure to controlled institutions on the other sectors. This means that there are no reallocation effects and that the employees do not just switch to other sectors. Columns 4, 5 and 6 perform the robustness check with the alternative exposure share predicted by the interstate banking deregulation. Column 4 uses industry-year fixed effects and county-industry fixed effects and suggests that a 1 percentage point higher exposure led to around 0.15 percent decrease in employment. This is a substantial effect and has high economic significance. Column 5 controls for the local demand effects. The coefficient is still significant and almost unchanged. Column 6 controls for debt-to-income interacted with slowly growing sectors. Our results indicate that deleveraging does not seem to be driving our results. Overall, the employment analysis suggests that the slow-growing sectors we identified previously had lower employment in counties that were exposed to controlled institutions.

4.6 Consumption on the State Level

Because of a lack of consumption data on the county level, we identified specific sectors that are responsible for the slowdown in consumption and showed that employment in those sectors decreased due to the foreclosure regulation. To provide further evidence that consumption decreased in the affected sectors, we use state-level data on personal consumption expenditures from BEA. Although not as finely grained, the state-level ex-

posure to controlled banks has enough variation to make statements about consumption growth after the cut in lending. We run the following regressions:

$$\log(con_{ist}) = \beta_0 + \beta_1 \mathbf{1}_{cp} exp_c + \beta_2 \mathbf{1}_{cp} exp_c sec_d + \beta X_{st} + \eta_{st} + \sigma_{st} + \kappa_{is} + \epsilon_{ist} \quad (8)$$

The dependent variable is the log of consumption in sector i, state s at time t. $\mathbf{1}_{cp}$ is a dummy for the period when banks are controlled, exp_s is the exposure of state s to the controlled banks, sec_d is a dummy if the sector is one of the sectors considered in our analysis and X_{st} are state-level control variables. $exp_{d,c}$ is the measure constructed by aggregating the county-level exposure. Details are provided in the Appendix. We include controls such as disposable income, labor participation, logged population, debt-to-income ratio of a state, and the unemployment rate. All controls are lagged one period.

Table 7 confirms our results from the previous section. All our regressions are weighted by population, as in Mian and Sufi (2010)[6]. Column 1 presents the results from the baseline regression where we include industry-state fixed effects and industry-year fixed effects control for a national trend. The results indicate that a 10 percentage points higher exposure to controlled institutions is associated with a decrease of around 2.3 percent in consumption in the slowly growing sectors. This effect has a high magnitude and high economic significance. In Column 2, we include state-time fixed effects to control for local demand effects. As our results do not change significantly, we can rule out the possibility that more exposed states were subject to state-level policies that dampened demand for certain goods. Next, we again investigate whether the decrease in consumption could be explained by high leverage in the states that were also hit by a cut in lending. To test this hypothesis, we include an interaction of the debt-to-income ratio and a dummy for the control period. Our results in Column 3 suggest that states that had relatively high leverage compared to other states in 2011 did not experience significantly different nor economically important changes in consumption.

[6]They additionally restrict their analysis to the top 450 counties. We do not exclude any of the states from our analysis.

Table 7: State-level Evidence on Consumption

	(1) $log(con_{ist})$	(2) $log(con_{ist})$	(3) $log(con_{ist})$	(4) $log(con_{ist})$	(5) $log(con_{ist})$	(6) $log(con_{ist})$
$\mathbf{1}exp_c$	0.1049	0.0000	0.0000			
	(0.0952)	(.)	(.)			
$\mathbf{1}exp_c sec_d$	-0.2322**	-0.2314**	-0.1840*			
	(0.0999)	(0.0994)	(0.0935)			
$\mathbf{1}DTI_{2011}$			0.0000			0.0000
			(.)			(.)
$\mathbf{1}DTI_{2011} sec_d$			-0.0122			-0.0236
			(0.0185)			(0.0179)
$\mathbf{1}exp_d$				0.0073	0.0000	0.0000
				(0.0137)	(.)	(.)
$\mathbf{1}exp_d sec_d$				-0.0097	-0.0090	-0.0133
				(0.0169)	(0.0168)	(0.0165)
N	10244	10244	10244	10244	10244	10244
Industry-Year FE	yes	yes	yes	yes	yes	yes
State-Industry FE	yes	yes	yes	yes	yes	yes
State-Year FE	no	yes	yes	no	yes	yes

Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the state-category-year level. Columns (1),(2) and (3) use a standard measure of exposure, obtained by dividing the average exposure of each county to controlled banks divided by number of years. Columns (4), (5) and (6) use the exposure measure described in the Appendix. In both specifications, first regression is always without year fixed effects. Standard errors are clustered at the state-category level. Results show that those states that had higher exposure to controlled banks experienced a larger slowdown in the sectors we previously identified as slow-growing. This does not translate to our alternative measure of exposure, possibly due to low variation of exposure across states.

Columns 4, 5 and 6 use the newly constructed variable for exposure as in Hoffmann and Stewen (2019). While the sign on the coefficient of interest is the same, the results are not significant and small in magnitude. This is possibly due to lower variation in the measure at the state level.

Our results suggest that the cut in lending due to the foreclosure regulation had a negative and important effect on consumption.

5 Robustness

In this section, we provide robustness checks and try to assess alternative mechanisms for our effects. First, we explore the supply side of the economy by investigating whether the costly foreclosure policy also impacted lending to firms and thus could operate in other ways than the housing net worth channel. Second, different pre-existing trends for controlled and non-controlled banks in terms of creditworthiness of customers might bias our results. Third, we examine whether the decrease in mortgage lending by controlled banks was binding for households. If so, we expect the debt-to-income ratio of a more exposed county to decrease due to the effect of the foreclosure regulation. Otherwise, this might indicate that the decrease in mortgage lending by controlled banks was not binding for households. Fourth, we investigate the possibility that the decrease in consumption came as a consequence of a decrease in permanent income.

5.1 Loans to Firms

On a broader level, we find that economic policies can have unpredictable consequences. Any change in economic policy might affect several agents in the economy. While our results suggest effects on the demand side, we now turn to the supply side and investigate the consequences of the foreclosure policy for firms. As shown before, we find evidence that the cut in lending due to regulation decreased the recovery of house prices. Lower house prices could also affect the value of the collateral that firms use to obtain loans. As a result, firms would be less likely to invest and both employment and consumption might decrease as a consequence.

This channel is prominently featured in the literature. Chen et al. (2017) show that stress testing in the post-crisis period led to a decrease in small business lending. Similarly, Cortes et al. (2018) argue that stress tests decrease small business lending due to higher costs of pro-cyclical loan classes. Another stream of papers documents the importance of the collateral for firm lending. For example, Chaney et al. (2010) show that real estate shocks can have adverse effects on firm investment. This result is much stronger for firms facing more financial frictions.

To test this channel we obtain data on small business lending. While total consumption does not only consist of goods and services supplied by small firms, there are two reasons why we use small business lending data to test the hypothesis of a collateral channel. First, small businesses are more financially constrained and more likely to be affected by decreasing house prices. Second, data on lending to big firms do not exist at the level of disaggregation needed for this analysis. We run the following regression:

$$\Delta \log(sme_{ct}) = \beta_0 + \beta_1 \mathbf{1}_{cp} exp_c + \beta X_{ct} + \eta_c + \sigma_t + \epsilon_{ct}, \tag{9}$$

where the dependent variable is the growth of the loan amount lent to small businesses in county c at time t. Our variable of interest is the interaction of a dummy for the control period and the exposure of a county to controlled banks. Our county-level controls X_{ct} are debt-to-income, unemployment, log population, personal income, all lagged. We want to test whether counties that had higher exposure to controlled banks also experienced a decrease in small business lending in the control period.

Column 1 in Table 8 presents the results of a regression where we include county-level controls and the county fixed effect. It suggests that counties with higher exposure experienced an increase in small business loans rather than a decrease. Column 2 includes time fixed effects and shows that the previous result does not survive the additional control. Although the sign is now negative, it is small in magnitude and insignificant. Column 3 uses our second measure of exposure to controlled banks. The result is similar to the one presented in Column 1. Once we control for the time fixed effects in Column 4, we again find no economically meaningful effect on small business lending in counties that were more exposed to controlled banks. Overall, these findings

Table 8: Industrial Loans to Firms

	(1) $\Delta log(sme_{ct})$	(2) $\Delta log(sme_{ct})$	(3) $\Delta log(sme_{ct})$	(4) $\Delta log(sme_{ct})$
$\mathbf{1}_{cp} exp_c$	0.3414***	-0.0270		
	(0.0219)	(0.0509)		
$\mathbf{1}_{cp} exp_c$			1.0779***	0.0045
			(0.0573)	(0.1754)
N	25558	25558	25558	25558
County FE	yes	yes	yes	yes
Year FE	no	yes	no	yes

Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the county-year. Columns (1) and (2) use a standard measure of exposure, obtained by dividing the average exposure of each county to controlled banks divided by number of years. Columns (3) and (4) use the exposure measure described in the Appendix. In both specifications, first regression is always without year fixed effects. Standard errors are clustered at the county level. Results show that there was no decrease in small business loans to those areas that were more exposed to the controlled institutions.

suggest that the decrease in consumption was not a consequence of a decrease in lending to firms, but a wealth effect due to lower house prices in the affected areas.

The previous result holds for overall lending in the counties exposed to controlled banks. Now, we perform a similar analysis, but on the bank level. We use data on the lending of controlled banks to small firms to investigate whether controlled banks reduce their supply of small business loans. The empirical strategy is the same as in the first section, where we investigate the effect of regulation on the mortgage loan origination.

Our regression specification is:

$$\Delta log(smeloan_{bct}) = \beta_0 + \beta_1 cont_{bt} + \theta X_{bt} + \phi_t + \sigma_{ct} + \eta_{bc} + \epsilon_{bct}, \tag{10}$$

where $\Delta log(smeloan_{bct})$ is the growth rate of the loan amount lent to small businesses by bank b in county c at time t, $cont_{bt}$ is the dummy variable for whether bank was controlled, X_{bt} is the set of controls, σ_{ct} are the county-year fixed effects and η_{bc} are the bank-county fixed effects. Our controls at the bank level are the last period growth rates of control variables such as total assets, total overhead costs, income, total investment securities, non-performing loans, return on assets, total deposits and the interest rate on deposits. Column 1 of Table 9 presents the results of the regression with bank-county fixed effects and year fixed effects. The results show that controlled banks increased their loan supply to small businesses during the period of control. When we include county-year fixed effects in Column 2, this positive effect becomes insignificant and smaller in magnitude. This suggests that the increase in small business loans is a result of an increase in demand. Column 3 adds the interaction with being controlled and a dummy if the bank has assets lower than 10 billion dollars. Our result is similar to the previous specification, but in Column 4 it loses significance again when we include county-year fixed effects. Finally, Columns 5 and 6 are regressions at the bank-year level, where we investigate what happens to the growth of small business loans at an aggregate level. Overall, there does seem to be an increase in the growth rate of small business loans by controlled banks due to being controlled. As controlled banks face additional constraints for mortgage loan origination, more funds might be available for

Table 9: Loans to Firms - Bank Level

	(1) $\Delta log(smeloan_{bct})$	(2) $\Delta log(smeloan_{bct})$	(3) $\Delta log(smeloan_{bct})$	(4) $\Delta log(smeloan_{bct})$	(5) $\Delta log(smeloan_{bt})$	(6) $\Delta log(smeloan_{bt})$
$cont_{bt}$	0.2776***	0.1710	0.2776***	0.1710	0.4799***	1.1979***
	(0.1021)	(0.1371)	(0.1021)	(0.1371)	(0.1296)	(0.3041)
$cont_{bt}\mathbf{1}_{Asset<10bn}$			0.0000	0.0000		0.0000
			(.)	(.)		(.)
N	142418	137748	142418	137748	5695	2907
Bank-County FE	yes	yes	yes	yes	no	no
Year FE	yes	no	yes	no	no	yes
County-Year FE	no	yes	no	yes	no	no

Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the bank-county-year level. Columns 5 and 6 show that these results translate to aggregate lending by controlled banks. Columns (1)-(4) feature data on small business lending at the bank-county-year level. Columns (1) includes bank-county fixed effects. Column (2) adds both bank-county and county-year fixed effects. Columns (3) and (4) control for small banks by including the dummy for asset size lower than 10 bn. Columns (5) and (6) are at the bank-year level. Standard errors are clustered at the holding level.

other activities. However, our results indicate that much of this is driven by county-specific demand.

5.2 Effect of Consent Orders on Household Debt

We provided evidence that the foreclosure regulation led to lower mortgage loan growth for controlled banks. This suggests that counties that are more exposed to the controlled institutions should have decreased their leverage. The effect on households might be mitigated if they can obtain a mortgage from another bank which was not controlled. In that case, we should not see lower leverage.

We run the following regression, where DTI_{ct} is debt-to-income in county c at time t, exp_c is exposure to controlled banks, X_{ct} is a vector of other county specific controls:

$$DTI_{ct} = \beta_0 + \beta_1 \mathbf{1}_{cp} exp_c + \theta X_{ct} + \eta_c + \phi_t + \epsilon_{ct}, \tag{11}$$

Column 1 in Table 10 includes county fixed effects and county-level controls. The coefficient on our variable of interest suggests that the increase in exposure to controlled banks on average decreases the debt-to-income ratio by 4 percentage points. Adding time fixed effect in Column 2, the result remains significant, although the coefficient is

Table 10: Exposure and Household Debt

	(1) DTI_{ct}	(2) DTI_{ct}	(3) DTI_{ct}	(4) DTI_{ct}
$\mathbf{1}_{cp}exp_c$	-0.4041***	-0.1394**		
	(0.0323)	(0.0586)		
$\mathbf{1}_{cp}exp_{d,c}$			-1.7143***	-2.1291***
			(0.0970)	(0.2582)
N	31188	31188	31188	31188
County FE	yes	yes	yes	yes
Year FE	no	yes	no	yes

Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the county-year. Columns (1) and (2) use a standard measure of exposure, obtained by dividing the average exposure of each county to controlled banks divided by number of years. Columns (3) and (4) use the exposure measure described in the Appendix. In both specifications, first regression is always without year fixed effects. Standard errors are clustered at the county level. This regression shows that the cut in the supply of mortgages by controlled institutions was not replaced by other lenders in the area.

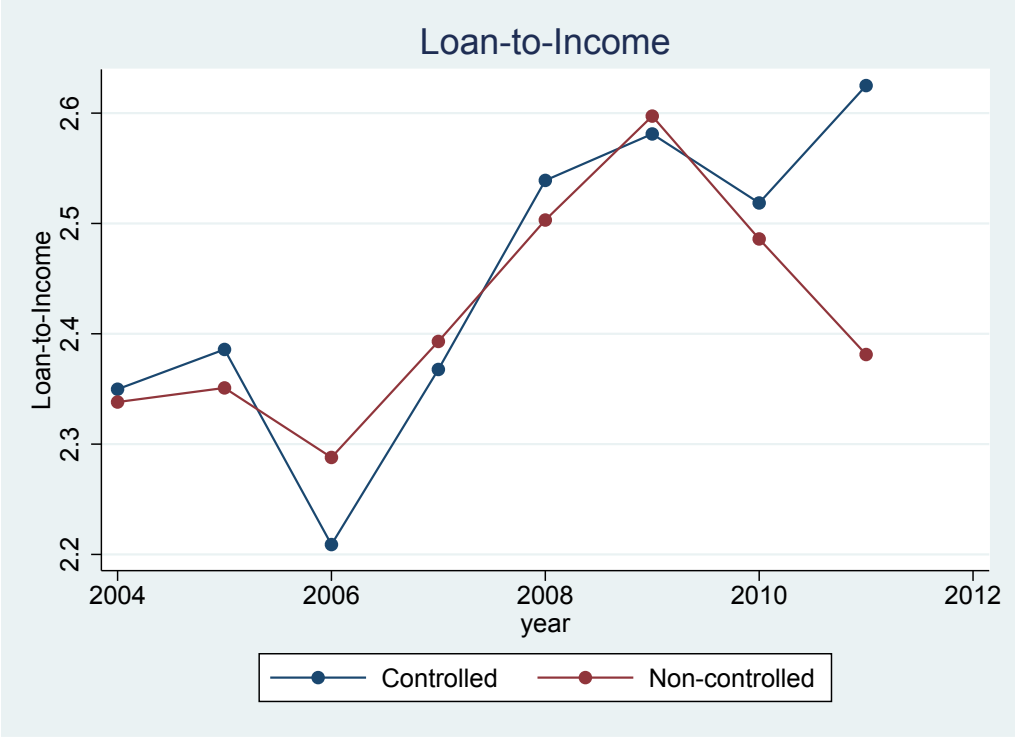
much smaller. Next, we perform our analysis with our alternative exposure variable. This variable helps us control for the indebtedness before the foreclosure regulation. If a county was highly indebted prior to the regulation, a decrease in the debt-to-income ratio could be a consequence of the downturn in the business cycle and not of the foreclosure regulation. Column 3 confirms our prior that more exposed counties experienced higher deleveraging. Column 4 includes year fixed effects and shows that counties with higher exposure to controlled banks of 10 percentage points reduced their debt-to-income ratio by around 21 percentage points.

5.3 Portfolio Differences and Post-Crisis Lending

This section explores the possibility that differences in the portfolio of mortgage loans led to a decrease in the growth of mortgage loans originated after the foreclosure crisis. There are various channels through which the mortgage portfolio could affect mortgage lending. For example, if the portfolio of a bank consisted of borrowers with a very high loan to income ratio, this might present a problem for the bank when the crisis hits. Highly indebted borrowers are less likely to repay the loan, while loans with several borrowers are more likely to be repaid. We gather data on mortgage loan applications from HMDA and describe several measures that could be important for lending.

A natural measure to look at is the ratio of debt and the income of the applicant. Since the debt of the applicant is only available from 2018, we calculate the average loan-to-income (LTI) ratio and look at how this measure differs across controlled and non-controlled banks. Figure 9 shows that in both groups of banks the loan amount was around 2.4 higher on average than the yearly income. Both in the pre-crisis period and during the crisis, LTI was identical in both groups of banks and moving in the same direction. The LTI starts to diverge in 2011, with controlled banks increasing it further and non-controlled decreasing. This suggests that the LTI does not play a big role for the mortgage loans issued after 2011.

Figure 9: Pre-Crisis Loan-to-Income



Notes: This figure shows the loan-to-income ratio for the two groups of lenders. We collect data on the loan-to-income ratio on an individual level for those mortgage applications that were accepted. We then average the LTI across the two groups of banks. We use LTI as a proxy for the riskiness of a loan. There is no large difference in LTI previous to the foreclosure crisis.

The previous arguments hold for the average borrower who is less likely to default on a mortgage loan. We now turn to the distribution of the LTI. If controlled banks engaged in a more dispersed lending strategy and give loans to both high and low-quality borrowers, the means of the two groups of banks could be similar while the tails might be very different. The figures from the Appendix show that the distribution of LTI among the two groups of banks is similar for the two groups. This leads us to conclude that the LTI as such does not play a role in the decrease of mortgage growth for the controlled banks.

The final step towards investigating the effect of portfolio differences is running several regressions to ensure that our previous results are not biased by any of the previous considerations. Table 11 presents the results of sequentially including different

variables at the bank level to control for the possible portfolio differences affecting lending. Our regression specification is:

$$\Delta log(loan_{bct}) = \beta_0 + \beta_1 controlled_{bt} + \theta X_{bt} + \phi_t + \sigma_{ct} + \eta_{bc} + \epsilon_{bct}, \tag{12}$$

Column 1 of Table 11 includes the LTI variable at the bank level controlling for a possible decrease in mortgage loan growth due to an overindebtedness of previous borrowers. Our results suggest that the loan-to-income ratio of previous borrowers does not affect the mortgage lending behavior of banks in an economically meaningful way during the period of control. Column 2 investigates whether the ethnicity of the borrowers in the pre-control period affects the lending behavior after 2012. There is evidence that Hispanics were more likely to receive high-cost loans in the build-up to the crisis (Bayer et al., 2014). If banks want to cut down risky mortgages, they would be decreasing the mortgage loan growth after the crisis for these particular borrowers. As our coefficient of interest remains similar in magnitude, our results suggest that the decrease in mortgage loan growth is not related to a different portfolio of risky loans given to the Hispanic population. Column 3 investigates portfolio differences in terms of the percentage of applications given out to the male population (Goodman and Zhu, 2016). To the extent that banks want to cut down on subprime lending, the share of sexes before the control could be affecting the decision on mortgage loans. This channel does not alter our coefficient of interest, either. All in all, we do not find evidence that the decrease in mortgage loans was caused by differences in the bank's portfolios.

5.4 Permanent Income and Consumption

As the financial crisis was large in magnitude and unanticipated by the main economic actors, it challenged the basic understanding of how the economy works and led to a revision of agents' expectations (Bernanke, 2010). Thereby, revisions in agents' permanent income can manifest in changes in consumption. The permanent income of households is not only determined by the value of their houses or stocks, but also by their job prospects and uncertainty about their future employment and wages.

Table 11: Bank Robustness Check

	(1) $\Delta log(loan)_{bt}$	(2) $\Delta log(loan)_{bt}$	(3) $\Delta log(loan)_{bt}$	(4) $\Delta log(loan)_{bt}$
$\mathbf{1}_{cp} controlled_b$	-0.4355**	-0.4378**	-0.4465**	-0.4480**
	(0.2027)	(0.2030)	(0.2019)	(0.2025)
$\mathbf{1}_{cp} LTI_b$	0.0016			0.0014
	(0.0011)			(0.0011)
$\mathbf{1}_{cp} hispanic_b$		0.1197		0.1149
		(0.0901)		(0.0903)
$\mathbf{1}_{cp} male_b$			-0.1112	-0.0978
			(0.1217)	(0.1221)
N	34328	34328	34328	34328
Year FE	yes	yes	yes	yes
Bank FE	yes	yes	yes	yes

Note: Standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the bank-year level. The purpose of this table is to establish whether inclusion of the factors that are marked as implying certain risk in the context of mortgage loans can affect our results. First three columns include the factors separately, and the last column includes all the factors. Standard errors are clustered at the holding level. This table shows that our previous results at the bank-year level remain unchanged and suggests that the controlled institutions have cut their mortgage loan supply.

Jaimovich and Su (2012) show that the recent recessions in the US were characterized by slow recoveries in employment. These jobless recoveries mostly happened for "routine" jobs. Therefore, recessions might have greatly changed the views of a certain group of people about their future employment or wages.

To investigate this possibility we perform additional checks on whether the revisions in permanent income could be responsible for the decline in consumption we saw during the period of control. Permanent income revisions are traditionally hard to measure. We rely on the modified version of the measure as presented in Straub (2019), where the difference is due to the level of observation.

First, we obtain the parts of disposable income at the state level that are unpredicted with the information known at time t. The regression that we run to obtain the residuals is:

$$log(dinc_{st}) = \beta_0 + \beta X_{st} + \epsilon_{st}, \tag{13}$$

where $dinc_{st}$ is the disposable income in state s at time t and X_{st} are covariates in state s at time t. The covariates used in our regressions are the same as the ones presented in Table 7: disposable income, labor participation, logged population, debt-to-income ratio of a state, and the unemployment rate. All controls are lagged one period. Our unit of observation is not an individual, but a state. Therefore, all the factors we include in the state-level regressions of disposable income are at the state-level.

Then, we use residuals from the previous regression to construct our measure of permanent income. We use a moving window of residuals around the time t:

$$\bar{PI}_{st,T} = \frac{1}{2T+1} \sum_{\tau=t-T}^{t+T} \hat{\epsilon}_{st}, \tag{14}$$

where $2T+1$ is the number of periods we use to calculate our measure of permanent income and $\hat{\epsilon}_{st}$ are the residuals from the previous regression.

Note that the highest moving window we use is 5 years around time t, as our sample gets too small and we lose a third of observations moving from the first specification to

the third.

The first column of Table 12 uses a seven-year time window to construct the permanent income measure. The magnitude of the coefficient decreases compared to the coefficients in Table 7, but the significance remains. Using the nine-year time window in Column 2, we find that the coefficient decreases slightly, but our results remain significant. Our last specification uses eleven-year time windows. We find that our results are not significantly changed. Overall, our results suggest that the permanent income cannot fully explain the decrease in consumption in the control period.

6 Conclusion

This paper investigates the effect of the foreclosure regulation on the consumption slowdown in the US after the financial crisis in 2008. We suggest that a series of requirements for banks that faced stricter foreclosure regulations led to the slowdown in consumption. We find that institutions that faced stricter foreclosure regulation requirements decrease the growth of mortgage loans originated during the period of regulation. Our results are not driven by county-level demand for mortgages or by the possibility of banks to diversify their portfolio of mortgage loans. This decrease in mortgage loan origination happens on the extensive margin.

We further find that house prices in the areas exposed to controlled banks experience a slower recovery. In line with the finding on house prices, counties that are more exposed to controlled banks also experience a decrease in their debt-to-income ratio in the post-regulation period. This further strengthens our hypothesis, as mortgage loans by controlled banks might have been replaced by loans from other banks. Due to a lack of consumption data at the county-level, we investigate the effects of the cut in mortgage lending on employment and state-level consumption. Both results further corroborate our hypothesis.

Small businesses could be driving the decrease in consumption due to being more financially constrained after the decrease in house prices. Using data on small business lending at the county level we find that there is no effect of being exposed to controlled

Table 12: Consumption and Permanent Income

	(1) $log(con_{ist})$	(2) $log(con_{ist})$	(3) $log(con_{ist})$
$\mathbf{1}_{cp} exp_c$	0.0000	0.0000	0.0000
	(.)	(.)	(.)
$\mathbf{1}_{cp} exp_c sec_d$	-0.1344**	-0.1042*	-0.0905*
	(0.0646)	(0.0549)	(0.0529)
$PI_{st,3}$	9.0784***		
	(3.1823)		
$PI_{st,4}$		10.8638**	
		(4.7011)	
$PI_{st,5}$			8.5608
			(8.9188)
N	5746	4680	3640
Industry-Year FE	yes	yes	yes
State-Industry FE	yes	yes	yes
State-Year FE	yes	yes	yes

Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results on the state-category-year level. Standard errors are clustered at the state-category level. PI stands for the measure of permanent income, as described in the paper. This table establishes that previous results are not driven by the change in the expectations about the permanent income.

institutions on small business lending in the post-regulation period.

All county-level regressions focus on exposure to controlled banks, where we used the average of exposure in the pre-regulation period. Since this is potentially endogenous we mitigate this problem using the approach established in the literature (Hoffmann and Stewen, 2019), using the banking interstate deregulation. Our final robustness check leaves our results unaltered.

The results in this paper lead us to the conclusion that the foreclosure regulation harmed the recovery of consumption after the financial crisis. A natural question is whether the regulations which impose stricter control on foreclosure regulations are a bad policy after a crisis. The answer to this question depends on the effectiveness of the policy. Foreclosure regulation does affect mortgage loan origination and consumption through the household balance sheet effect. While our results suggest that this policy slowed down the recovery of the main economic aggregates after the crisis, it could lead to higher long-term output and an economy that is less susceptible to financial crises. If banks are now more reluctant to initiate foreclosure as a result of previous policies, this could have a positive effect on the future recoveries from the crisis. We leave it to future work to explore whether the benefits of this policy outweigh its costs.

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7 Appendix

7.1 Descriptives

Table A1: Summary Statistics

(1)

Banks Data	Mean	Standard Deviation	Max	Min
Loan amount	219.7159	1217.21	951495	1
Assets	1.18e+08	3.56e+08	2.14e+09	4514
Deposits	8.29e+07	2.52e+08	1.53e+09	0
Non-interest income	2189772	6723177	4.19e+07	-1815900
Interest Expense	1036557	3856667	3.89e+07	-23
Return on Assets	.7466346	1.554866	207.0973	-26.09702
Efficiency ratio	94.97682	1279.735	72439.26	-2459.509
Tier 1 Capital	13.89387	17.35169	3873.022	-58.48638
N	5283753			

(2)

County Data	Mean	Standard Deviation	Max	Min
Exposure	.2039885	.0849828	.5911837	.042017
Debt-to-income	1.887505	.7989944	3.46	.78
Population	121970.5	305615.5	5205723	1057
Labor Force	61512.39	179634.8	5096516	38
Unemployment rate	6.534535	2.747033	28.9	1.1
N	1066482			

Notes: This table shows the summary statistics for the bank level and county level data. Bank-level data comes from the FDIC database. County-level data comes from various sources such as HMDA, FHFA, BLS, BEA.

Table A2: Summary Statistics

(Summary statistics for Controlled Institutions before 2012)

	Mean	Standard Deciation	Max	Min
Amount of Loans	25659132.33	51835461.05	2.44e+08	2101.00
Assets	3.09e+08	5.01e+08	1.81e+09	1616614.00
Deposits	2.05e+08	3.32e+08	1.19e+09	39487.00
Non-interest income	5579853.29	9732814.42	37099000.00	-578632.00
Total Interest Expense	3831294.52	7719609.80	38916000.00	0.00
Return on Assets	0.38	2.16	8.30	-14.15
Efficiency ratio	764.44	7167.40	72439.26	-478.09
Tier 1 capital ratio	12.73	6.18	42.12	5.64
Observations	112			

(Summary statistics for Controlled Institutions after 2012)

	Mean	Standard Deviation	Max	Min
Amount of Loans	27440771.75	41620606.60	1.98e+08	96.00
Assets	5.93e+08	7.19e+08	2.14e+09	3615086.00
Deposits	4.40e+08	5.33e+08	1.53e+09	2911187.00
Non-interest income	9886794.88	12803720.55	41943000.00	-22363.00
Total Interest Expense	1771140.81	2299072.32	9660000.00	12212.00
Return on Assets	0.84	0.45	1.63	-0.37
Efficiency ratio	63.89	10.11	84.39	49.03
Tier 1 capital ratio	13.86	3.91	30.19	9.70
Observations	68			

(Summary statistics for Non-controlled Institutions before 2012)

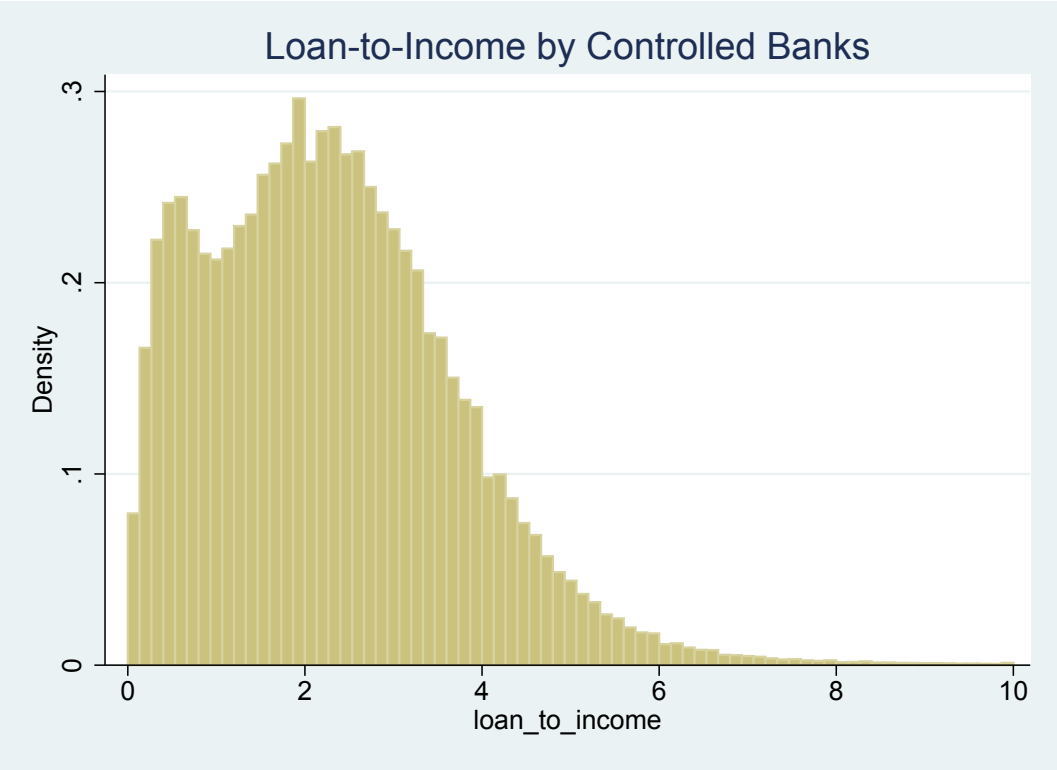
	Mean	Standard Deviation	Max	Min
Amount of Loans	150368.31	1306834.91	1.04e+08	6.00
Assets	1145821.58	7119655.53	3.26e+08	4514.00
Deposits	845935.11	5030831.89	2.14e+08	0.00
Non-interest income	14279.57	144244.68	7278327.00	-1815900.00
Total Interest Expense	18793.33	133805.63	10844151.00	0.00
Return on Assets	0.21	2.12	207.10	-26.10
Efficiency ratio	77.91	66.01	5812.00	-2459.51
Tier 1 capital ratio	14.64	9.29	757.55	-58.49
Observations	40032			

(Summary statistics for Non-controlled Institutions after 2012)

	Mean	Standard Deviation	Max	Min
Amount of Loans	222541.51	1400474.63	90552832.00	0.00
Assets	1752766.80	11123923.18	3.19e+08	19834.00
Deposits	1395089.61	8867691.33	2.71e+08	0.00

7.2 LTI ratio distribution of Controlled Banks

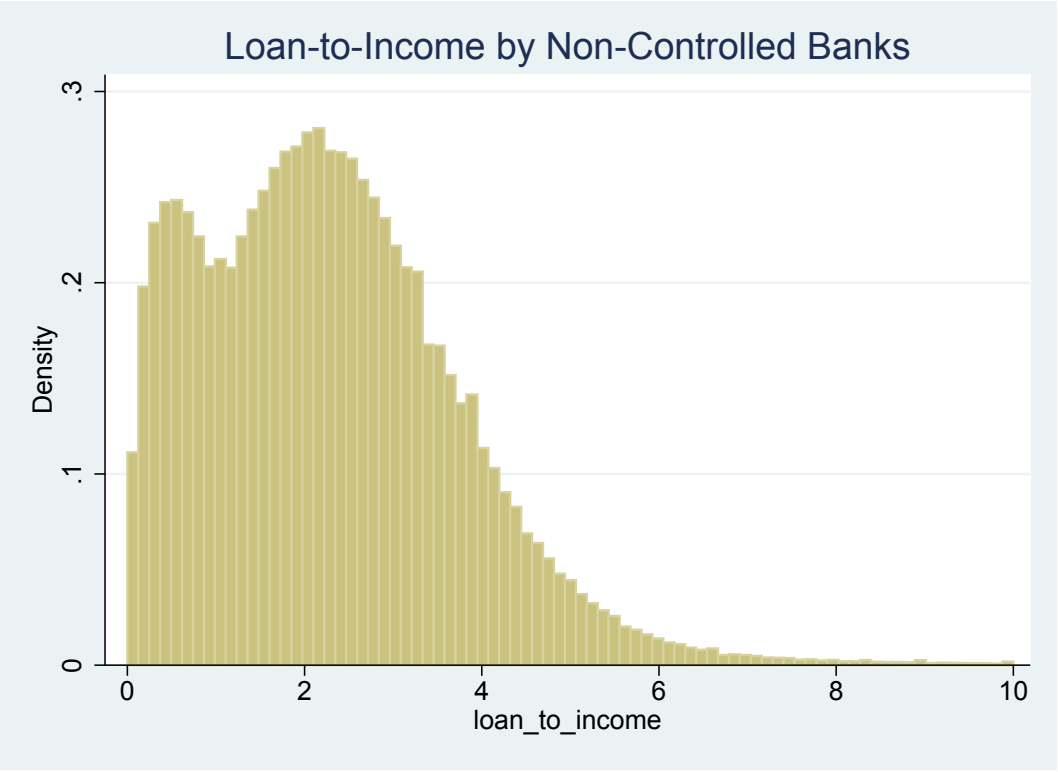
Figure A1: LTI distribution for Controlled Banks



Notes:This graph presents the distribution of loan-to-income ratios of controlled banks. Loan-to-income is calculated on the application level by dividing the loan amount to income of the people applying. It uses the LTI ratio of all the accepted applications. The distribution of LTI is taken unconditionally and presents the distribution for all the banks that are controlled.

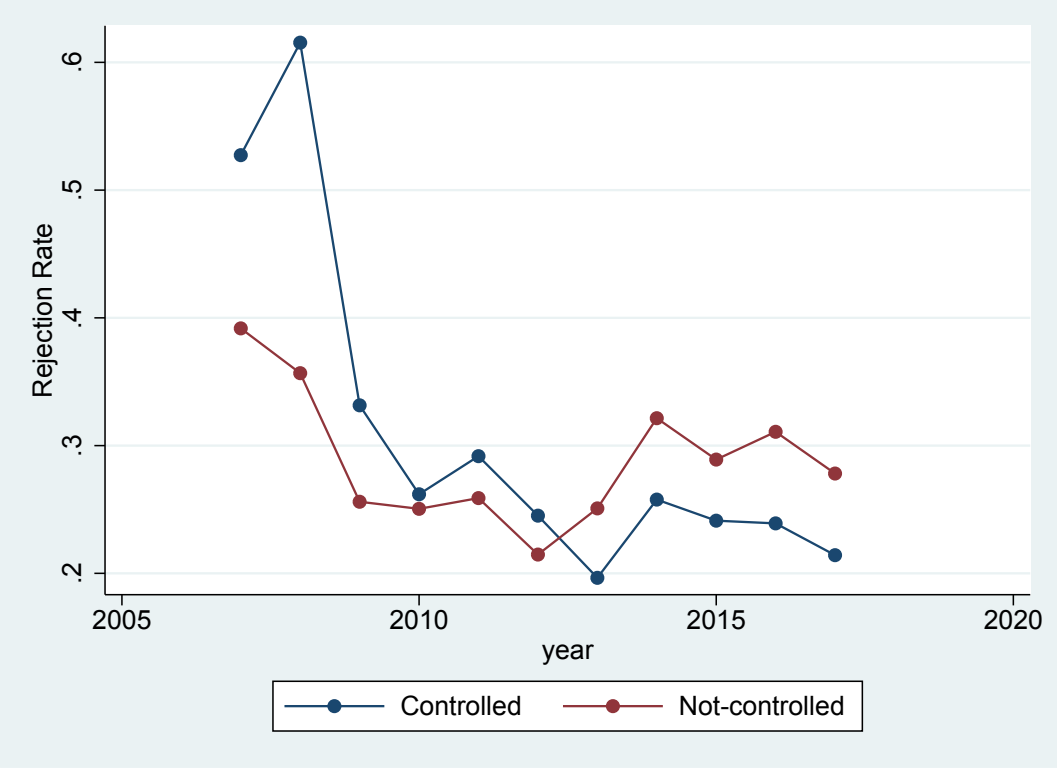
7.3 LTI ratio distribution of Non-Controlled Banks

Figure A2: LTI distribution for Non-controlled Banks



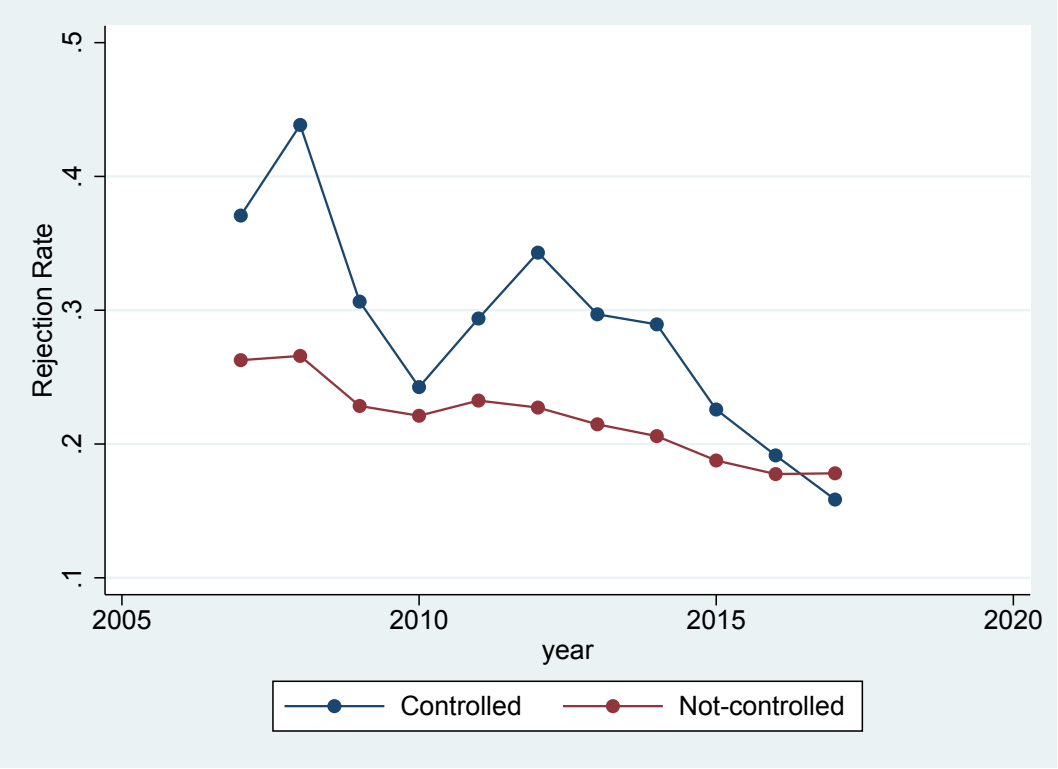
Notes:This graph presents the distribution of loan-to-income ratios of non-controlled banks. Loan-to-income is calculated on the application level by dividing the loan amount to income of the people applying. It uses the LTI ratio of all the accepted applications. The distribution of LTI is taken unconditionally and presents the distribution for all the banks that are not controlled.

Figure A3: Rejection Rates for Refinance Loans



Notes:This graph presents the rejection rates for mortgage applications for refinance loans. It shows development for both controlled and non-controlled institutions.

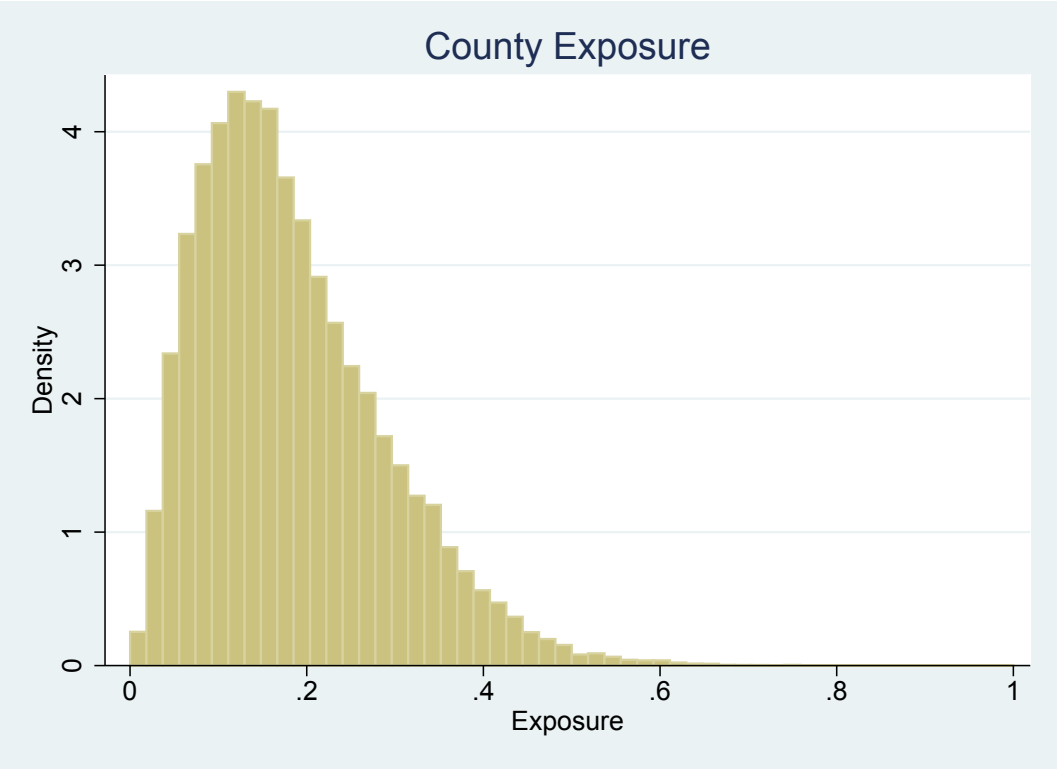
Figure A4: Rejection Rates for Other Loans



Notes:This graph presents the rejection rates for mortgage applications for refinance loans. It shows development for both controlled and non-controlled institutions.

7.4 County Exposure

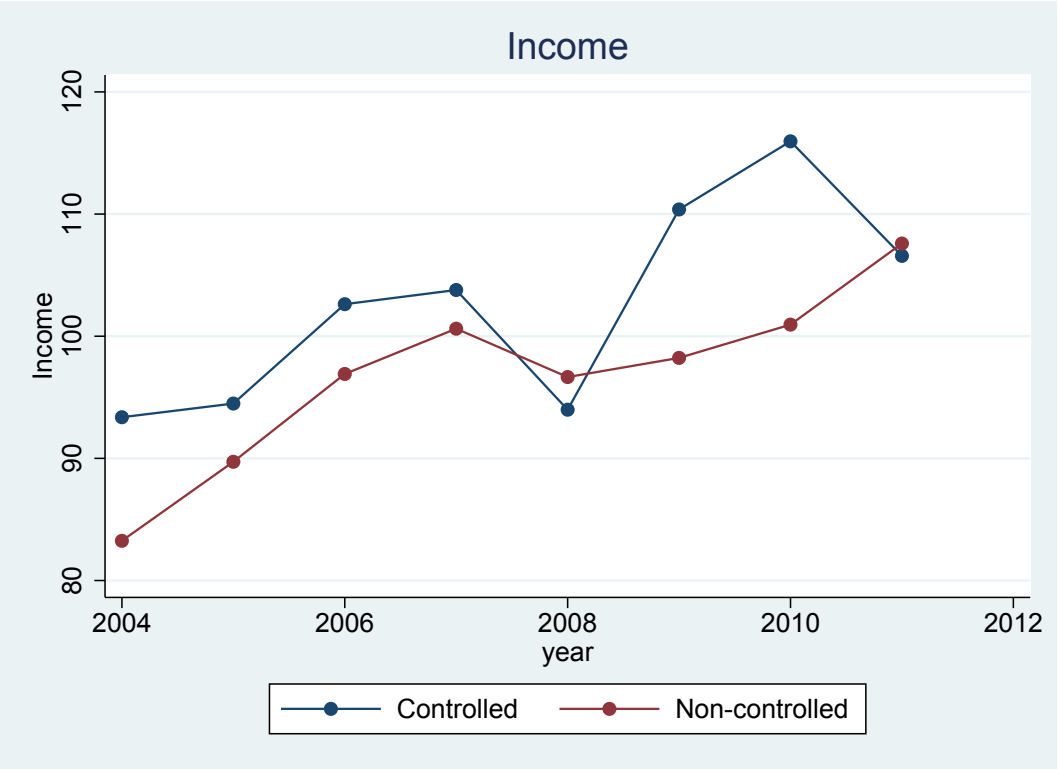
Figure A5: Exposure to Controlled Banks



Notes:This graph presents the distribution of county exposure to controlled institutions. Exposure is calculated by dividing the sum of market shares of each controlled institution in a year by the total amount of mortgage loans, and then divided by the number of years used in calculation. As evident, most of counties are exposed to controlled institutions. Very few of them have exposure above 0.5 and only a few around 0. This means that we expect to see the effects of a cut in mortgage lending of these institutions on the economy.

7.5 Average Income

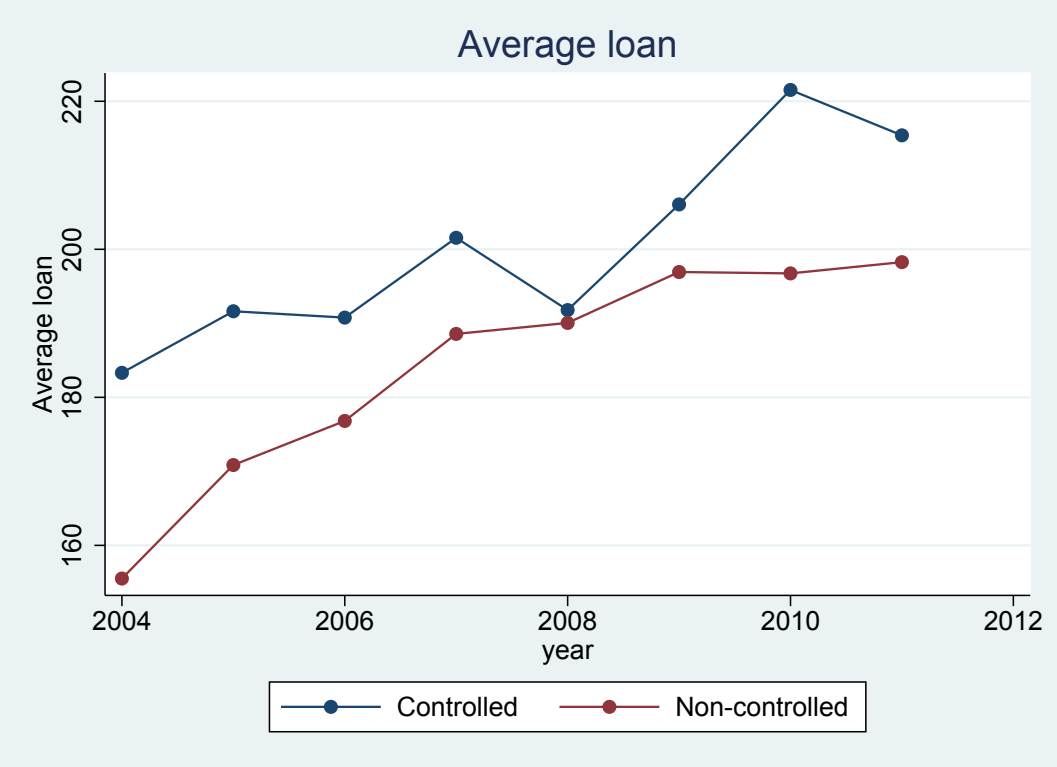
Figure A6: Average Income for Controlled and Non-controlled



Notes:This graph presents average income of borrowers in the both groups of institutions. We take all the accepted mortgage applications and split them in those that were accepted by controlled and non-controlled institutions. Average taken is conditional on being in a group. There is no significant difference in trends prior to the foreclosure crisis.

7.6 Average Loan

Figure A7: Average Loan Amount for Controlled and Non-controlled



Notes:This graph presents average loan amount of borrowers in both groups of institutions. We take all the accepted mortgage applications and split them in those that were accepted by controlled and non-controlled institutions. Average taken is conditional on being in a group. There is no significant difference in trends prior to the foreclosure crisis.

7.7 Alternative Exposure Measure

One of the main problems for the identification strategy in our paper is the fact that the exposure of a county to controlled banks is potentially endogenous. For example, controlled banks could have more assets and the ability to diversify. When a county starts growing, these banks could decide to increase their presence in that county. This would increase the exposure of these counties to controlled banks. Since after every growth period there is a downturn, we will see that counties with high exposure to controlled banks have a slower growth of e.g. house prices and employment.

To deal with this problem we first construct a measure of a bank's hypothetical share in the mortgage market that is based on the regulatory framework as in (Hoffmann and Stewen, 2019):

$$\omega_{bc} = \frac{min\{open^{s(c)}, open^{s(b)}\}\mathbf{1}_{s(c)\neq s(b)} + \mathbf{1}_{s(c)=s(b)}d(c)}{\sum_{b\in B_c} min\{open^{s(c)}, open^{s(b)}\}\mathbf{1}_{s(c)\neq s(b)} + \mathbf{1}_{s(c)=s(b)d(c)d(c)}} \tag{15}$$

Here, $\omega_{b,c}$ is a hypothetical market share, which is supposed to capture how many years did the bank have to build up its presence in the county compared to other lenders. $open^s$ indicates how many years have passed since the interstate banking deregulation took place until 1995 and $d(c)$ is set as if local banks enter market in 1980s (so $d(c)$ will be 15). Intuitively, the numerator measures the number of years passed since deregulation for bank b in county c and represents the time that bank had to build up its presence in the county. The denominator is the sum of the number of years across all banks and is supposed to capture the time that all banks together had to build up their presence. So the hypothetical market share is calculated by dividing the number of years to potentially do business for a specific bank and state with the number of years to potentially do business for all banks in this county.

With this measure, we want to eliminate the part of the exposure of a county to a certain bank that arises due to time-varying factors like cyclical economic conditions. Instead, we are only interested in the part caused by bank-specific factors, county-specific factors and the openness of a county to banks outside of its state. The interstate banking deregulation hopefully provides an exogenous variation in the possibility to establish the presence of a bank in a specific county. If the state in which a county is located has opened up earlier to banks outside of its state, then banks were more likely to build up a presence in this specific county.

We use the following regression to predict shares of bank b in a county c at time t:

$$sh_{bct} = \beta_0 + \beta_1\omega_{bc} + \gamma_b + \gamma_c + \epsilon_{bct} \tag{16}$$

Then, we use our estimates to predict the exposure of a county c to bank b:

$$\hat{sh}_{bc} = \beta_0 + \beta_1\omega_{bc} + \gamma_b + \gamma_c \tag{17}$$

Previously constructed share of banks are hopefully not correlated with cyclicality of output and therefore help us avoid possible endogeneity of the previous measure. In the next step we want to construct a measure of exposure to controlled banks. To do so, we further aggregate the shares of controlled banks and call it:

$$exp_{d,c} = \sum_{b=B_{controlled}} \hat{sh}_{bc}, \tag{18}$$

where $B_{controlled}$ is the set of controlled institutions.

7.8 Industry Correspondence

This section explains the correspondence that we construct between industries in the employment dataset by BEA and products in personal consumption expenditure by BEA. There is no clear cut correspondence between the two classifications, as one entails industries and the other is related to major products in the consumption basked. However, consumption aggregates in the personal consumption expenditure data also rely on the North America Industry Classification System (NAICS). The data on the products produced by each industry in NAICS enables us to construct a correspondence between the two classifications.

The idea behind our correspondence is the following. In one of the previous chapters, we have identified fast and slow-growing consumption aggregates by constructing growth rates in the aftermath of two previous recessions. Our results suggest that most of the categories in the consumption basket have experienced a significantly lower growth. Our analysis requires us to identify those sectors that have slowed down the consumption growth the most. For any of the consumption categories to be able to affect the total consumption it has to have a high share in the total consumption. Additionally, these categories have to grow slower compared to the previous recovery period. This motivates the construction of our correspondence between categories of consumption and employment by industry. We construct the ranking of the importance of each sector in the following way. First, we calculate the change in the growth rate of consumption categories in percentages in the following way:

$$\gamma_{c,j} = \frac{\sum_{i=1}^{T} \phi_{i,c}}{T}, \tag{19}$$

where c is the category of consumption, j is the recovery period, which is either 1 or 2, and T is the total number of periods in each of the recovery periods. $\phi_{i,c}$ is the growth rate of a category c in period i. Then we obtain the change in growth rates by dividing average growth rates over two periods:

$$\eta_c = (\frac{\gamma_{c,2}}{\gamma c, 1} - 1), \tag{20}$$

which gives us the rate of change in growth rates across two periods. Next, we multiply each of the categories of consumption by their respective shares to obtain our measure we us for ranking.

Having obtained this measure, we split all the categories into roughly three groups, that represent high, medium and slow-growing sectors. We then identify the slow-growing sectors as Housing and Utilities, Health Care, Recreation services, Food services and accommodation and Financial Services and Insurance. In the next step, we want to identify sectors that provide these groups of products/services and that are present. First, in our industry-level employment we don't take into account government sub-sectors, but instead, treat them as one. Housing and utilities category of consumption corresponds to two industries in employment classification - utilities and real estate and rental and leasing, which we treat as one sector in our analysis. Health care consumption category corresponds to the health care and social assistance. Recreation services in the consumption category entail purchases such as membership clubs, sports centers, theaters and museums,audio-video, photographic, and information processing equipment services, veterinary and other services. If we look at the employment information, people working in arts, entertainment and recreation include actors, amusement and recreation attendants, fitness trainers and aerobics instructors, gaming supervisors and musicians. These two, therefore, correspond to each other. Food and accommodation category entails purchased meal and beverages, food furnished to employees and accommodation expenses for hotels and motels. Bureau of Labor Statistics

shows that the biggest categories of the accommodation and food industry are cooks in fast food establishments and restaurants, hotel, motel and desk clerks and waiters. These sectors clearly correspond to each other. Financial services consumption category has several categories of consumption: financial services, furnished without payment, financial services charges and fees, securities commissions, portfolio management and investment advice services, trust, fiduciary and custody activities, life insurance, net health insurance, vehicle insurance. Finance and insurance industry in the employment classification has workers in accounting and audit, insurance sales agents, loan officers, securities, commodities and financial services agents and tellers, and corresponds to the previous consumption category. Also, part of the category other services in the employment classification corresponds to household utilities, like water supply and for that reason we include it in our classification.

Product - Industry Correspondence			
Motor vehicles and parts	4	Farm employment	70
Furnishings and durable household equipment	5	Forestry, fishing, and related activities	100
Recreational goods and vehicles	6	Mining, quarrying, and oil and gas extraction	200
Other durable goods	7	Utilities	300
Food and beverages purchased for off-premises consumption	9	Construction	400
Clothing and footwear	10	Manufacturing	500
Gasoline and other energy goods	11	Wholesale trade	600
Other nondurable goods	12	Retail trade	700
Household consumption expenditures (for services)	14	Transportation and warehousing	800
Housing and utilities	15	Information	900
Health care	16	Finance and insurance	1000
Transportation services	17	Real estate and rental and leasing	1100
Recreation services	18	Professional, scientific, and technical services	1200
Food services and accommodations	19	Management of companies and enterprises	1300
Financial services and insurance	20	Administrative and support and waste management and remediation services	1400
Other services	21	Educational services	1500
		Health care and social assistance	1600
		Arts, entertainment, and recreation	1700
		Accommodation and food services	1800
		Other services (except government and government enterprises)	1900
		Government and government enterprises	2000