Consumption Slowdown After the Great Recession

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Abstract

Consumption growth in the United States slowed markedly following the 2007–2009 financial crisis. We argue that costly regulatory interventions targeting banks with foreclosure-related misconduct contributed to this decline by constraining credit supply. Using variation in county-level exposure to affected banks, we show that tighter regulatory controls reduced mortgage loan origination, leading to weaker house price recoveries and lower household wealth. We find that consumption growth slowed more in counties more exposed to these banks, consistent with a wealth effect transmitted through housing markets. The decline in mortgage lending reflects a reduction in the number of loans rather than in average loan size, suggesting that regulation operated primarily through extensive-margin credit supply.
JEL classification: G21, L26. **Keywords:** Consumption, Mortgage, Banking Regulation, Economic Growth

1   Introduction

The cyclical behavior of GDP and consumption growth is a central focus of macroeconomic research. Typically, periods of negative growth during recessions are followed by rebounds during recoveries, resulting in sustained positive growth over time. However, following the 2008 financial crisis, the U.S. economy did not fully return to its pre-crisis growth trajectory (Pistaferri, 2016).

From the perspective of the expenditure approach to GDP, either consumption or investment must account for this persistent slowdown, given the relatively small contributions of government spending and net exports. Figure 1 documents the behavior of consumption and investment growth across recent business cycles. The shaded areas highlight the two last expansionary periods (2002 Q1 - 2007 Q3 and 2009 Q3 - 2018 Q4). Although investment and durable goods consumption rebounded strongly after 2009, growth in services and nondurable consumption lagged significantly behind. This paper explores a new demand-side explanation for the sluggish recovery in consumption.

Following the financial crisis, a wave of foreclosures swept the United States, peaking in 2010. In response to widespread concerns about improper foreclosure practices, the Federal Reserve launched a major review of foreclosure processes, resulting in consent orders that imposed significant costs on several mortgage lenders. We refer to these institutions as affected banks. Using microdata on mortgage originations and county-level outcomes, we show that banks subject to consent orders curtailed mortgage lending, leading to weaker house price recoveries in counties with higher exposure to these banks. Lower housing wealth, in turn, contributed

Figure 1: Consumption and Investment Growth
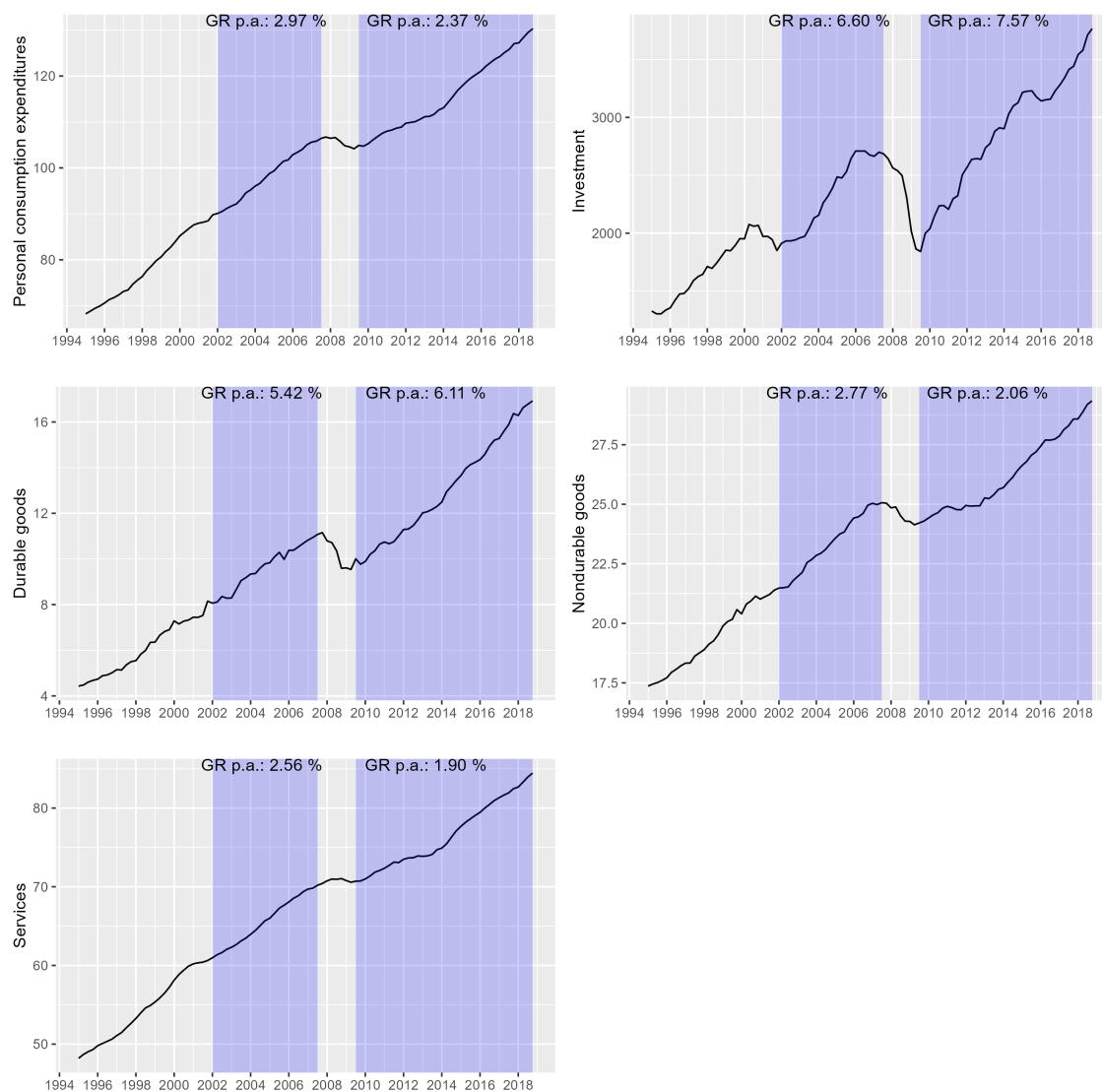
Notes: These figures illustrate the trajectories of the components of consumption and investment. The purple shading indicates the periods used for the growth rate calculations. The data stem from BEA and FRED.

to slower consumption growth during the recovery. Our main result suggests that a 10 percentage points higher exposure to affected banks is associated with a decrease of around 2.3 percent in consumption in the aftermath of the recession.

This paper's contribution is twofold. First, it documents a previously unexamined link between foreclosure-related regulatory interventions and the post-crisis consumption slowdown. While prior work (Dagher and Sun, 2016; McGowan and Nguyen, 2023; Pence, 2006) examines how differences in state foreclosure laws affected credit supply, this paper studies a distinct mechanism: a regulatory cost shock imposed on banks for improper foreclosure practices. By focusing on lender-side penalties rather than borrower-side foreclosure rules, the paper connects regulatory credit frictions directly to house prices and consumption dynamics. Second, methodologically, the paper departs from spatial regression discontinuity designs used in earlier studies by exploiting differences across banks over time within counties. This bank-county level approach allows us to control for local demand conditions more effectively and to trace the consequences of credit disruptions for both asset prices and consumption behavior.

Our empirical analysis consists of three parts. First, we examine the effect of the foreclosure policy on banks' aggregate mortgage lending. A key challenge is that affected banks may operate in regions with weaker growth, which could bias estimates due to lower loan demand. To address this, we use Home Mortgage Disclosure Act (HMDA) data aggregated at the bank-county level, allowing us to control for local demand conditions by comparing banks within the same county. We find that affected banks experienced a 25 percentage point lower growth rate in mortgage lending. Loan-level data further indicate that this reduction occurred primarily on the extensive margin: while average loan sizes remained stable, applicants at affected banks were 9 percentage points less likely to receive a loan, conditional on observables.

Second, we show that from 2012 to 2017, counties more exposed to affected banks experienced slower house price recoveries. Controlling for county-level characteristics, we find that a 1 percentage point increase in exposure is associated with a 0.05 percentage point lower annual house price growth rate. Given the average county exposure of 20 percent, this implies a 1 percentage point lower annual growth rate. To address concerns that county exposure may be endogenous, we instrument for exposure using historical patterns of interbank deregulation (Hoffmann and Stewen, 2019). We also show that results are robust to controlling for debt-to-income ratios and housing supply elasticity (Saiz, 2010). Our findings suggest that slower house price growth, rather than high household leverage, accounts for weaker consumption via the housing wealth channel (Mian et al., 2013).

Third, we document the effects on consumption. While county-level consumption data are unavailable, we first identify the sectors driving consumption weakness and then examine employment changes by sector at the county level. We find that a 10 percentage point higher exposure to affected banks is associated with a 0.5 percentage point lower employment growth rate in these sectors. At the state level, we find that a 10 percentage point higher exposure leads to a 2.3 percent decrease in consumption, indicating a sizeable and

economically meaningful effect.

We conduct several robustness checks. One concern is that the results may reflect a decline in business lending, as observed after the Dodd-Frank act (Bordo and Duca, 2018). Using county-level data on small business loans—critical for financially constrained firms (Chen et al., 2017; Cortes et al., 2018)—we find no significant impact of exposure on small business lending. Bank-level analyses suggest that affected banks may even reallocate funds toward small business lending, but these effects are largely explained by county-level demand conditions. We also test whether household debt dynamics could explain the results, finding no evidence that households mitigate the financial constraints and switch to non-affected banks. A confounding factor might be the stress tests in 2011, controlling for which we also do not find significantly different results. Additionally, our findings suggest that the foreclosure regulation, rather than shifts in borrower risk profiles or permanent income revisions, is the key driver of the observed consumption slowdown.

Our paper contributes to the literature on the slow recovery following the Great Recession, which explores the role of monetary, fiscal, and regulatory policies in propagating the recession's effects (Taylor, 2014). Pistaferri (2016) provides an overview of GDP and consumption dynamics, emphasizing uncertainty and distributive channels as key drivers of the consumption slowdown post-recession. Our analysis supports this, highlighting an indirect wealth effect, particularly through housing markets. Pistaferri's finding that the debt service ratio sharply decreased during the recession but stabilized by 2012 aligns with our results, where we find no significant impact of debt overhang on consumption, suggesting that other factors contributed to the slowdown. This paper further expands on the role of regulatory policy in the recovery, focusing on the unintended consequences of the Fed's actions regarding illegal foreclosure practices.

We also engage with the literature on banking and foreclosure regulation. Acharya et al. (2018) investigate how stress tests have reduced credit supply to particularly risky borrowers, while Pierret and Steri (2017) document how stress tests have encouraged banks to invest more prudently since 2011. Other studies have explored how foreclosure laws influence loan supply. For example, Pence (2006) finds that laws favoring borrowers result in a 3-7% reduction in loan sizes, and McGowan and Nguyen (2023) show that stricter laws increase mortgage loan securitization. Unlike these studies, our focus is not on foreclosure regulation itself, but rather on the negative cost shock to banks stemming from improper foreclosure practices and the subsequent regulatory responses.

This study is closely related to the work of Mian, Sufi, and colleagues. Mian et al. (2015) demonstrate that foreclosures during the 2007-2009 period led to lower house prices, residential investment, and auto sales, which they use as proxies for consumption. While they note that house prices began recovering in 2010, our study shifts the focus to the policy response to wrongful foreclosures and its indirect effects on housing demand and consumption. Our data also suggest that vehicle purchases, which Mian et al. (2015) use as a consumption proxy, did not explain the consumption slowdown after 2009. Mian et al. (2013) investigate the geographic distribution of wealth shocks, finding a strong consumption response in regions with larger

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housing wealth declines. Additionally, Mian and Sufi (2014) highlight the impact of household balance sheets on non-tradable employment, which is closely tied to local demand. Our findings show a similar pattern, with slow growth observed in the housing and financial sectors, which are directly tied to wealth shocks. Auroba et al. (2022) decompose the effect of house prices on consumption in 2009. They find that the direct effect of house prices on consumption makes up more than half of the total effect of a change in house prices in consumption.

Our paper also ties into research on credit constraints and sentiment. Studies have shown that credit constraints amplified the Great Recession's effects and delayed recovery (Makridis, 2019; Malmendier and Shen, 2018). In line with these findings, our study highlights how regulatory shocks to the banking sector contributed to persistent credit constraints during the recovery phase, shedding light on their role in slowing consumption growth.

This paper is structured as follows. Section 2 describes the institutional setup; Section 3 describes the data. Section 4 provides the main empirical results regarding the effect of the foreclosure policy on house prices, the labor market, and consumption. Section 5 tests the robustness of our results to alternative explanations and Section 6 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.

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,

Figure 2: Timeline of Events
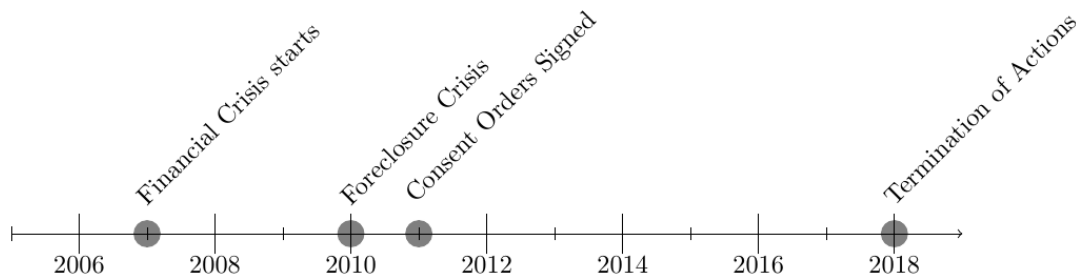
Notes: This figure depicts the chronological sequence of events leading to consent orders being signed and the termination of orders.

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. (Fed, 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 thus included in our sample[2] (Governors of the Federal Reserve System, 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. (Governors of the Federal Reserve System, 2012)

"The actions [...] require each servicer [...] to make 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." (Governors of the Federal Reserve System, 2011)

The Independent Foreclosure Review (IFR) process imposed substantial financial burdens on banks. As of December 31, 2012, IFR administrator costs for Federal Reserve-regulated servicers ranged from $1.3 million for smaller servicers to over $10.2 million for larger institutions, totaling approximately $16.7 million (GAO, 2014). Moreover, if the IFR had continued, Federal Reserve-regulated servicers projected combined future costs between $760 million and $822 million, with final expenses expected to exceed these estimates (Governors of the Federal Reserve System, 2014).

In 2013, the original consent orders were replaced by a settlement agreement requiring banks to pay approximately $4 billion in direct cash compensation to affected borrowers and to commit an additional $6 billion

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

[2]Goldman Sax, Morgan Stanley

to foreclosure prevention programs (GAO, 2014). These commitments entailed not only immediate financial outlays but also lasting increases in operational costs associated with loan servicing and compliance.

Crucially, the impact of these regulations extended beyond one-time payments. The consent orders mandated significant improvements in banks' oversight, internal controls, and foreclosure processes, thereby raising the ongoing cost structure of mortgage origination and servicing (Governors of the Federal Reserve System, 2017). Higher compliance costs and operational risks likely tightened lending standards, raised borrowing costs, and reduced credit availability. Thus, the foreclosure-related regulations meaningfully altered banks' operations, imposing a persistent financial burden that goes well beyond the headline figures of cash payments and settlement amounts.

In 2015, the orders were terminated against three institutions[3], while they were made stricter for six institutions that have not met the requirements[4] (Governors of the Federal Reserve System, 2015). We define our treatment group as the banks that were affected by the foreclosure actions[5], independently of when the consent orders were terminated.

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 affected institutions by dividing the loan amounts granted by affected 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.

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.

[3]Bank of America, Citibank, and PNC Bank
[4]EverBank, HSBC, JPMorgan Chase, Santander Bank, U.S. Bank National Association
[5]Aurora Bank, EverBank, OneWest Bank, Sovereign Bank (later Santander Bank), Bank of America, Citigroup, HSBC, JPMorgan Chase, MetLife Bank, PNC Bank, Wells Fargo, Ally Financial (previously GMAC), SunTrust Bank, U.S. Bank National Association, Goldman Sax, Morgan Stanley

Figure 3: Exposure to Affected Banks pre-2012

(.2404931,.6847223]
(.1821501,.2404931]
(.1286998,.1821501]
[0,.1286998]
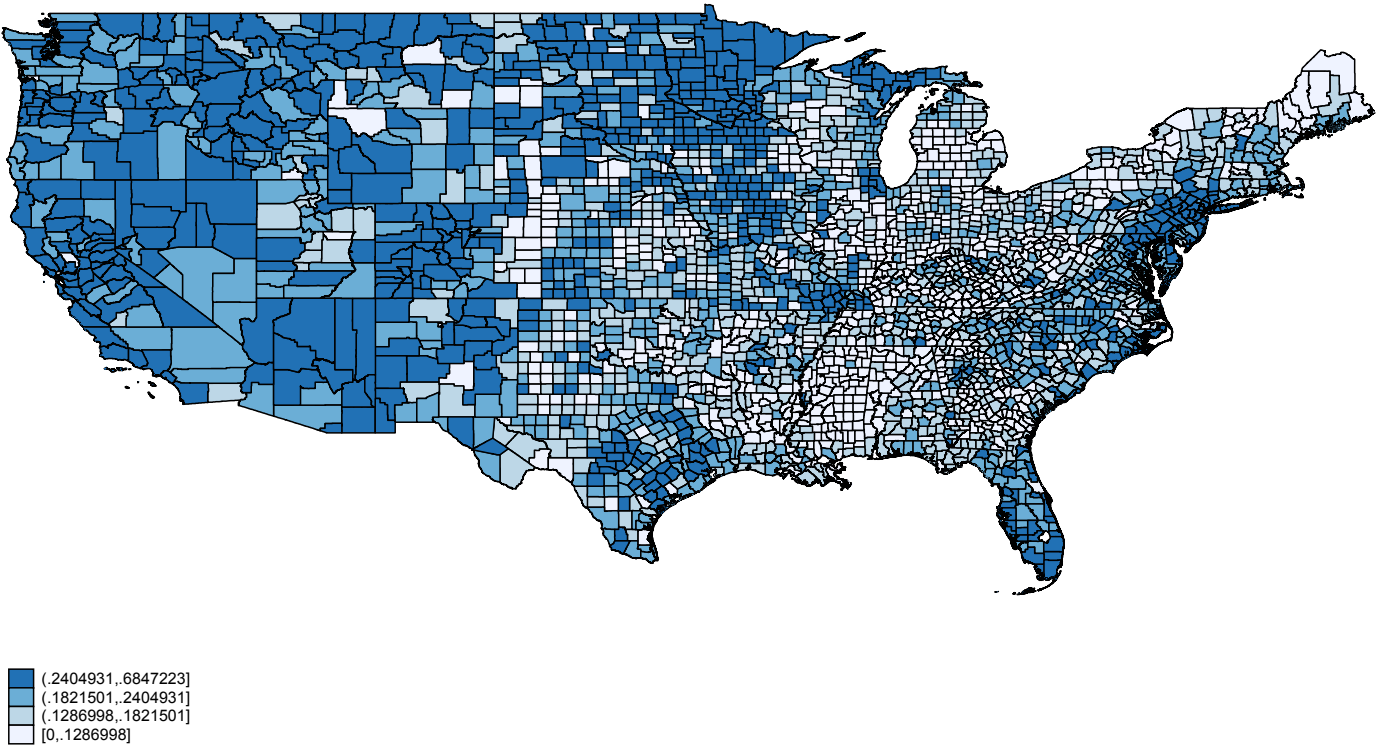
Notes: This figure illustrates the exposure of U.S. counties to institutions affected by government intervention. County exposure reflects the dependency of a region on mortgage loans issued by these institutions. Exposure is quantified by aggregating exposures to each affected bank annually and dividing by the number of years preceding government action.

House prices could also be affected by the high leverage of households after the recession. To control for this channel we obtain the data on debt-to-income on a county level from the Fed. Additionally, it could be the case that the exposure of counties is highly correlated with the housing supply elasticity. Affected institutions could have decided to establish more presence in counties with lower housing supply elasticity. We control for this channel using the housing supply elasticity 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 the 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 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 structure our empirical analysis in several steps. We first examine how foreclosure-related regulation influenced the mortgage loan growth of affected banks. Next, we assess the impact of county-level exposure to affected banks on local house prices. To explore potential effects on consumption, we pursue two approaches: we analyze employment dynamics across sectors at the county level, and we examine state-level consumption growth in relation to exposure.

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 affected and non-

Figure 4: Mortgage Loans Originated
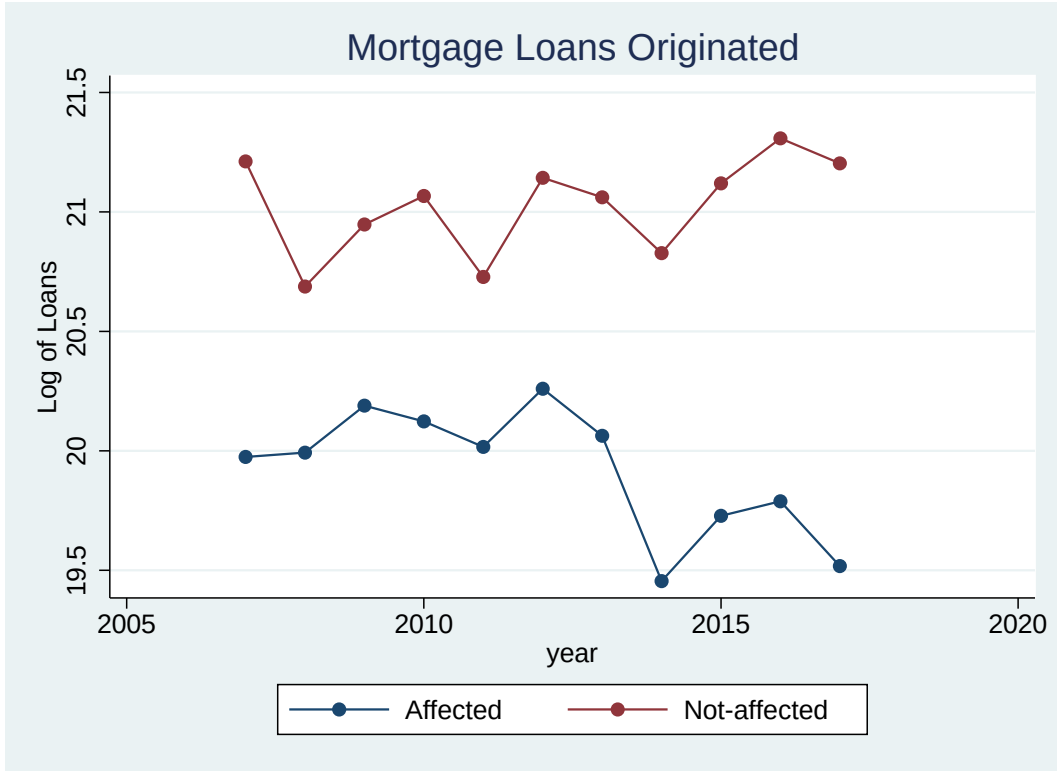
Notes: This figure shows the mortgage loan origination for the two groups of banks. The blue line represents the logarithm of the total sum of mortgage loans issued annually by affected institutions, while the red line represents loans issued by non-affected institutions.

affected institutions. The mortgage loan amounts originated decrease after 2012 for affected institutions.

We run the following differences-in-differences regression, where $\Delta log(loan_{bct})$ is the growth of mortgage loans originated by bank b in county c at time t, $\mathbf{1}_{cp}$affected is the dummy variable if the bank has signed a consent order with the Fed or the OCC after 2011, X_{bt} are the last period growth rates of control variables available from the FDIC data set (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:

$$\Delta log(loan_{bct}) = \beta_0 + \beta_1 \mathbf{1}_{cp}\text{affected}_{bt} + \theta X_{bt} + \phi_t + \sigma_{ct} + \eta_{bc} + \epsilon_{bct}$$

The $\mathbf{1}_{cp}$affected$_{bt}$ variable corresponds to the treatment*post interaction in a standard differences-in-differences setting. Instead of the usual post variable, we include year dummies and show different interactions for the fixed effects.[6] In some specifications, we also include the interaction term $\mathbf{1}_{cp}$affected$_{bt}Asset_{<10bn}$ to investigate the possibility that affected, 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,

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[6]Running a standard differences-in-differences regression yields similar results (not reported).

we also include HF_b, the Herfindahl-index of bank b loans in the pre-consent order period. We calculate the Herfindahl index in the following way:

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

where $B(b)$ is the set of locations in which the lending institution is active, and $\alpha_t^{b,l}$ 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 the level of metropolitan statistical areas (MSA) and the state-level.

Table 1: Bank-County-Year Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\Delta log(loan)$	$\Delta log(loan)$	$\Delta log(loan)$	$\Delta log(loan)$	$\Delta log(loan)$	$\Delta log(loan)$	$\Delta log(loan)$
$\mathbf{1}_{cp} affected_{bt}$	-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)
$\mathbf{1}_{cp} affected_{bt} Asset_{<10bn}$			-0.4930	-0.5208		-0.3448	
			(0.5614)	(0.5676)		(0.4892)	
$\mathbf{1}_{cp} affected_{bt} HF_b$							-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 (1)-(4) feature data on mortgage lending at the bank-county-year level. Column (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 the regression at the bank-level, and columns (6) and (7) control for the interaction between herfindahl index of banks mortgage portfolio and asset size, respectively. Standard errors are clustered at the holding level.

The first four columns of Table 1 report regressions at the bank-county-year level. While we are interested in the effect of foreclosure-related regulation, a decline in mortgage lending could also reflect affected banks' exposure to economically weaker counties. To address this, we include county-year fixed effects to control for local demand conditions. We also use bank-level FDIC data to control for factors correlated with being affected, such as declining assets or high non-performing loan shares, which could otherwise bias our estimates.

Column 1 shows that affected banks experience, on average, a 25 percentage point lower growth in mortgage

lending when bank-county, year, and county fixed effects are included. Given an average growth rate of around 4 percent, this corresponds to a decline in mortgage loan growth of approximately 21 percentage points. While sizable, this effect is broadly consistent with related studies, which report loan supply reductions of 6–14% following comparable shocks (Berrospide et al., 2013; Calem et al., 2013). Our estimate is somewhat larger, although the confidence intervals overlap with those in the existing literature.

A potential concern is that small banks, which are less able to diversify geographically, may be driving the effect. To test this, columns 3 and 4 interact the affected-bank indicator with a small-bank dummy (defined as assets under $10 billion; about one-third of our sample). While the coefficient for small affected banks is negative and sizable, it is not statistically significant. Moreover, the main coefficient increases somewhat but remains statistically indistinguishable from earlier estimates.

Columns 5–7 shift to the bank-year level to assess aggregate lending effects. Column 5 shows that affected banks had 31 percentage points lower mortgage loan growth, controlling for year and bank fixed effects. Column 6 confirms that this effect is not driven by small banks. In column 7, we interact the affected-bank indicator with a Herfindahl index of mortgage loan concentration to test whether poor geographic diversification explains the result. The interaction is small and insignificant, indicating that diversification does not account for the decline.

Taken together, these results suggest that foreclosure-related regulatory requirements significantly reduced mortgage loan growth at affected banks, both at the county and aggregate level.

Table 2: Bank-Year Regressions - Diversification Robustness

	(1)	(2)
	$\Delta log(loan)$	$\Delta log(loan)$
$\mathbf{1}_{cp}affected_{bt}$	-1.5089*	-0.5748**
	(0.8369)	(0.2907)
$\mathbf{1}_{cp}affected_{bt}HF_{b,MSA}$	0.4755	
	(0.3835)	
$\mathbf{1}_{cp}affected_{bt}HF_{b,State}$		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.

Hoffmann and Stewen (2019) caution that using a Herfindahl index at the county level may be misleading, since economic shocks are likely correlated across counties within a metropolitan area (MSA) or state. To address this concern, Table 2 reports additional robustness checks.

Column 1 shows the results when we calculate the Herfindahl index at the MSA level. While the coefficient decreases in magnitude, it remains statistically significant. Column 2 repeats the analysis using a state-level index, yielding a similar pattern: the coefficient is smaller but remains significant. Overall, our findings are robust to broader measures of geographic diversification.

4.1.1 Intensive vs Extensive Margin

Our previous considerations suggest that the institutions under consent orders 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. This dataset from HMDA 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:

$$dep_i = \beta_0 + \beta_1 \mathbf{1}_{cp}\text{affected}_{bt} + X_i + \eta_c + \phi_t + \epsilon_{ibt}$$

The dependent variable dep_i is either a loan approval decision or the log loan amount for applicant i. $\mathbf{1}_{cp}$ indicates the consent order period. Both specifications control for borrower income; for the approval decision specification, we also control for the loan amount. All regressions include fixed effects for applicant sex and ethnicity. To account for time-varying bank characteristics that may affect lending, we control for lagged (log) total assets, overhead costs, income, investment securities, non-performing loans, return on assets, total deposits, and deposit interest rates. We further include county-year fixed effects to control for changes in local economic conditions, such as house prices or employment. Finally, county-bank fixed effects account for potential differences in lending standards across counties.

Table 3: Intensive versus Extensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)
	$log(loan_i)$	$log(loan_i)$	$log(loan_i)$	$loanoriginated_i$	$loanoriginated_i$	$loanoriginated_i$
$\mathbf{1}_{cp}affected_{bt}$	-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.

Columns 1–3 of Table 3 examine the impact of the regulation on the intensive margin of loan supply. Column 1 includes bank-county fixed effects and shows no significant reduction in the average loan amount by affected banks. Column 2 adds bank-level controls to account for factors varying at the bank-year level, such as non-performing loans; the coefficient remains insignificant and moves closer to zero. Column 3 further includes county-year fixed effects to control for local demand shocks, with no change in significance. Overall, we find no economically meaningful reduction in the average loan amount among affected banks.

Column 4 shifts focus to the extensive margin, using a binary dependent variable equal to 1 if the loan was

approved and 0 if denied. Including bank-county fixed effects, we find that applicants at affected banks face an 8-percentage-point lower probability of loan approval. This effect could reflect changes in bank-level conditions, such as rising non-performing loans during the crisis, leading to tighter lending. Column 5 adds bank-level controls; the coefficient remains statistically significant and increases in magnitude. To account for possible declines in mortgage demand in harder-hit areas, column 6 adds county-year fixed effects; the estimate remains negative and significant. Together, these results suggest that the decline in mortgage lending post-2012 occurred primarily along the extensive margin.

Our analysis suggests that the regulatory policies disproportionately affected certain borrowers, since the lending reduction occurred primarily on the extensive margin. We expect that riskier borrowers were more likely to be affected and investigate whether this pattern is reflected in the data. While borrower risk is multidimensional and banks consider many factors, we focus on a readily available measure: the borrower's loan-to-income ratio. Specifically, we test whether affected banks became more likely to reject low-quality borrowers.

We use individual-level data on loan-to-income ratios and split mortgage applications between affected and unaffected banks. For simplicity, we divide loan-to-income into four categories and calculate rejection rates across these groups. A challenge in this analysis is that the financial crisis, lasting through 2009, likely raised rejection rates independently of regulatory changes. To address this, we define 2010 as the pre-policy baseline period for Figures 5 and 6. The post-policy period includes the years following the signing of the consent orders. Each bar in the figures shows the change in rejection rates from the baseline to the post-policy period.

Figures 5 and 6 show that the rejection rates have increased substantially for all the loan categories if we look at affected institutions. However, riskier borrowers with a higher loan-to-income ratio (LTI) faced higher rejection rates in the second period than in the first. This is not the case for the non-affected institutions. This suggests that the bulk of the cut in the mortgage lending was taken by the low-quality borrowers.

While the previous graphs 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 depend on how we define the number of bins for loan-to-income. To provide 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 affected 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:

$$decision_i = \beta_0 + \beta_1 \mathbf{1}_{cp}\text{affected}_{bt} + \beta_2 \mathbf{1}_{cp}\text{affected}_{bt}LTI_i + \beta_3 LTI_i + X_i + \eta_c + \phi_t + \epsilon_{ibt}$$

The dependent variable, $decision_i$, is a binary indicator equal to 1 if mortgage loan application i was approved. $\mathbf{1}_{cp}$ denotes the consent order period, and LTI_i is the loan-to-income ratio of applicant i. All

Figure 5: Difference in Rejection Rates for Affected Institutions
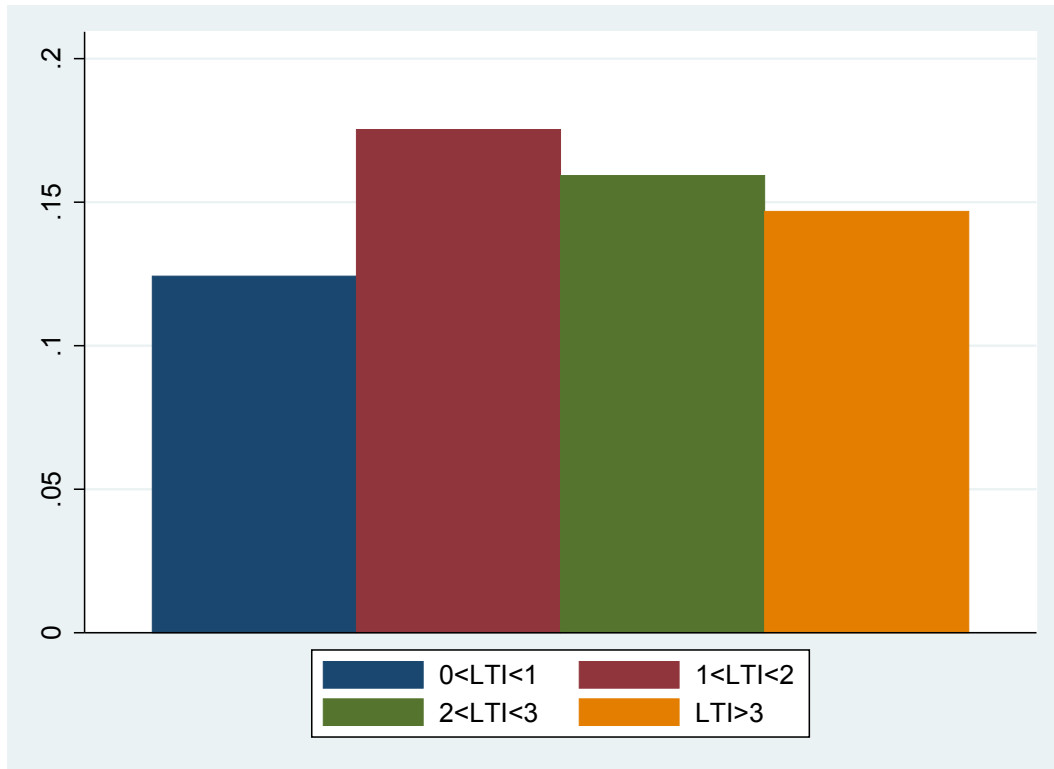
Notes: This figure illustrates the difference in rejection rates among affected institutions, divided into four sections using LTI (Loan-to-Income ratio) to proxy for borrower riskiness. We first calculate rejection rates for both the pre-control and post-consent order periods. Subsequently, we subtract rejection rates in the first period from those in the second period. For example, borrowers with an LTI between 0 and 1 experienced an increase of approximately 12.5 percentage points in rejection rates following the implementation of the measures.
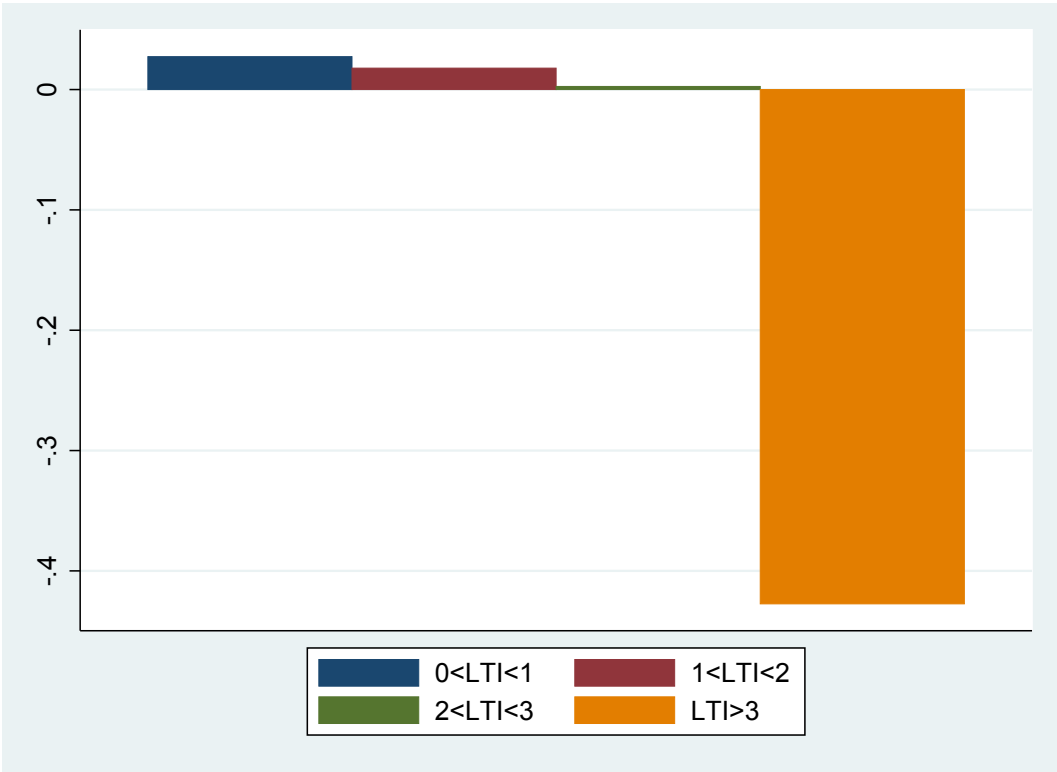
Figure 6: Difference in Rejection Rates for Non-Affected Institutions

Notes: This figure illustrates the difference in rejection rates among affected institutions, divided into four sections using LTI (Loan-to-Income ratio) to proxy for borrower riskiness. We first calculate rejection rates for both the pre-control and post-consent order periods. Subsequently, we subtract rejection rates in the first period from those in the second period. For example, borrowers with an LTI between 0 and 1 experienced an increase of approximately 0.4 percentage points in rejection rates following the implementation of the measures.

specifications include fixed effects for the applicant's sex and ethnicity. To account for bank characteristics that vary over time and may influence loan supply, we include a set of lagged, logged bank-level controls: total assets, total overhead costs, income, total investment securities, non-performing loans, return on assets, total deposits, and the deposit interest rate.

It is also possible that affected banks are more exposed to counties where declining borrower indebtedness reduces loan demand. To address this, our final specification includes county-year fixed effects to absorb local demand shocks, as well as bank-county fixed effects to control for persistent differences in lending standards across banks and counties.

Table 4: Decision to Lend

	(1)	(2)	(3)
	$loanoriginated_i$	$loanoriginated_i$	$loanoriginated_i$
$\mathbf{1}_{cp}affected_{bt}$	-0.0416**	-0.0712***	-0.0914***
	(0.0171)	(0.0150)	(0.0085)
$\mathbf{1}_{cp}LTI_i affected_{bt}$	-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"	"yes"	"yes"

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.

The first row of Table 4 indicates applying for a loan at a affected bank is associated with a decrease in the chance of being approved a mortgage loan. In the second row, we include an interaction between a bank that is affected and the loan-to-income ratio of a borrower. The coefficient in the third row suggest that, over the whole period, applicants with a higher loan-to-income were more likely to be rejected. In the second column we can see that during the consent order period, affected banks were less likely to approve mortgage loans to riskier borrowers. This observation further substantiates our hypothesis that those banks that faced stricter regulation on foreclosures reacted by cutting lending to those who were most likely to default.

In the Appendix we show that the rejection rates for refinance loans have significantly increased for both groups of institutions, which suggests that a second-order effect of consent orders exists. Rejection rates for

other mortgage loans have increased significantly for affected institutions in 2012 and remained high for two more years, and after that decreased.

4.2   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. With fewer mortgage loans, households face more financial constraints. This leads to lower housing demand and lower prices (Imbs and Favara, 2011).

We run the following regression at the county-year level, controlling for local economic factors that could influence house prices:

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

The dependent variable is the growth rate of the house price index in county c in year t. $\mathbf{1}cp$ is a dummy equal to one in periods when consent orders are active (starting in 2012), and exp_c is the average exposure of county c to affected banks based on mortgage loan origination shares from 2007 to 2011. Xct is a vector of other county-specific controls — log population, labor force participation rate, unemployment rate, and log income per capita — all lagged by one period.

We hypothesize that counties more exposed to affected banks experience slower growth in house prices. A potential concern is that counties with higher exposure to affected banks may have seen unusually rapid mortgage growth before 2012, for reasons unrelated to regulation. If so, the slower house price growth we observe could reflect a cyclical downturn rather than a causal effect of mortgage supply constraints. Another concern is that regulatory exposure may itself be endogenous to county-specific housing cycles.

We address these issues in two ways. First, we average exposure over the five years prior to the consent orders to minimize the influence of short-term fluctuations:

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

where the variable exp_{ct} is the mortgage loan amount granted by affected banks at time t to county c.

Second, we construct an alternative exposure measure based on banks' historical branch networks and deposit shares prior to the financial crisis, following Hoffmann and Stewen (2019). This measure exploits the gradual interstate banking deregulation from 1980–1995 and captures variation in county exposure to affected banks that is unlikely to be driven by local economic conditions. Details of the construction of $exp_{d,c}$ are provided in the Appendix.

Because branch networks adjust slowly and consent orders were assigned based on banks' national foreclosure practices — not county-specific loan performance — the variation in regulatory treatment across counties is plausibly exogenous to contemporaneous local shocks. Our identification strategy therefore relies on predetermined differences in exposure across counties, while flexibly controlling for local economic conditions through county and time fixed effects.

Table 5: House Prices and Exposure to Affected 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 level. Columns (1) and (2) use a standard measure of exposure, obtained by dividing the average exposure of each county to affected banks divided by number of years. Columns (3) and (4) use the exposure measure based on interstate banking deregulation based on Hoffman and Stewen (2019). In both specifications, first regression is always without year fixed effects. Standard errors are clustered at the county level.

Our results in Table 5 suggest that counties with high exposure to the affected 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 affected 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 similar to column 1. In the last two columns, we use our alternative measure of exposure to affected 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 affected banks. Column 4 suggests the average county could experience a decrease in the growth rate of house prices of almost 3 percentage points.

4.3 Effect of Consent Orders on Consumption Growth

The previous sections provided evidence that the consent orders decreased loan origination and house price growth. This section analyzes the effect of consent orders on consumption growth. Due to a lack of consumption data on the county level, we proceed in three steps. First, we stay on the county level and provide visual evidence for the consumption slowdown by categories. Second, we investigate employment on the county level for the different components of consumption. Third, we analyze the effect of consent orders on consumption growth on the state level.

4.3.1 Consumption Slowdown by Components

Figure 1 in the introduction shows that consumption growth was weaker than in the previous recovery period. We hypothesize that reduced mortgage lending contributed to this slowdown by weakening household balance sheets through falling house prices Aruoba et al. (2022). To explore this mechanism, we disaggregate consumption growth into categories using data from the BEA, covering 15 distinct consumption categories.

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 argue 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 affected banks. Since consumption data is not available on a county level, we first turn to employment to investigate our claims.

Figure 7: Components of Durable and Nondurable Goods Consumption
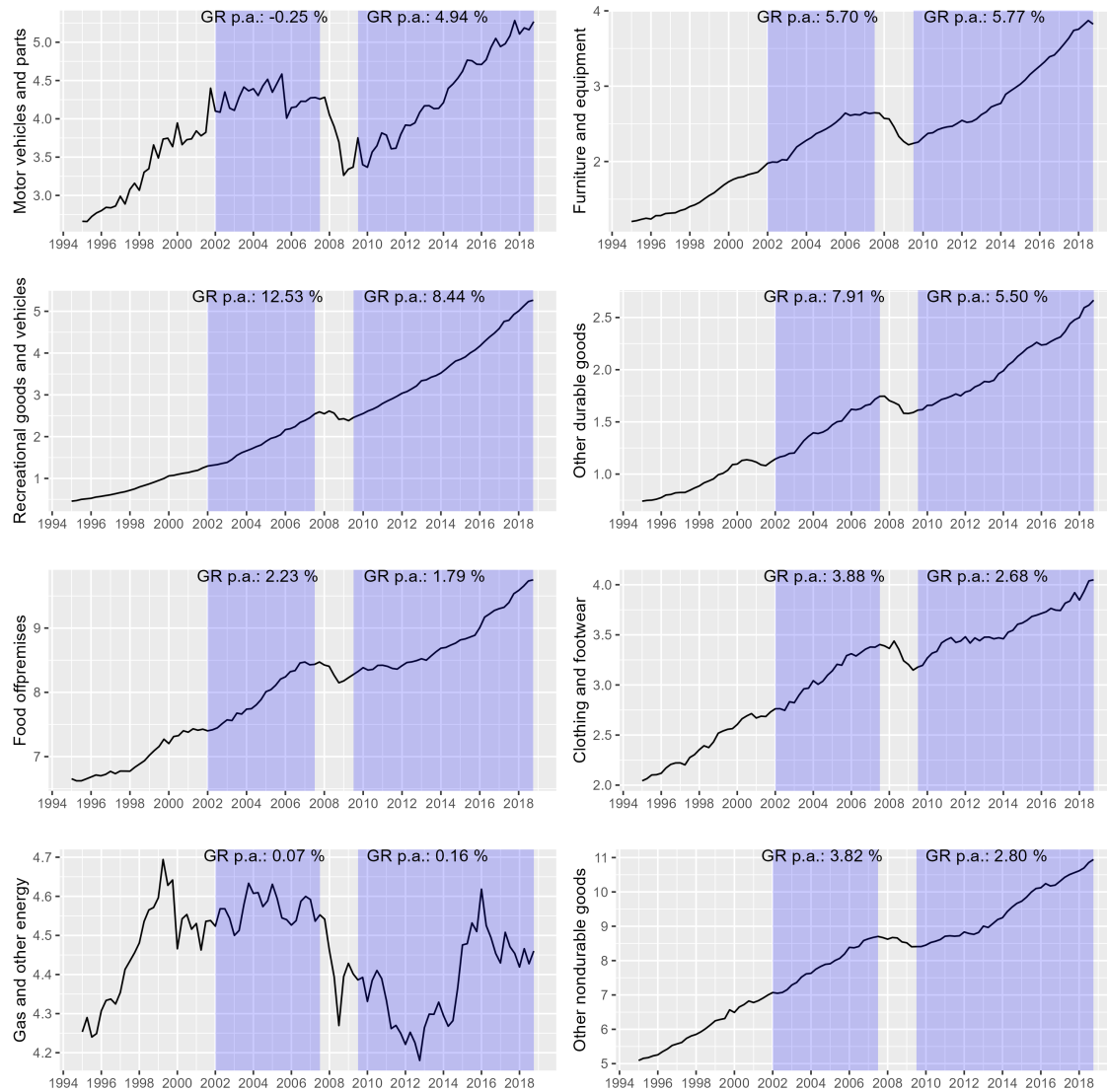
Notes: These figures illustrate the trajectories of the components of durable and nondurable goods consumption. The purple shading indicates the periods used for the growth rate calculations. The data stem from BEA and FRED.

Figure 8: Components of Services Consumption
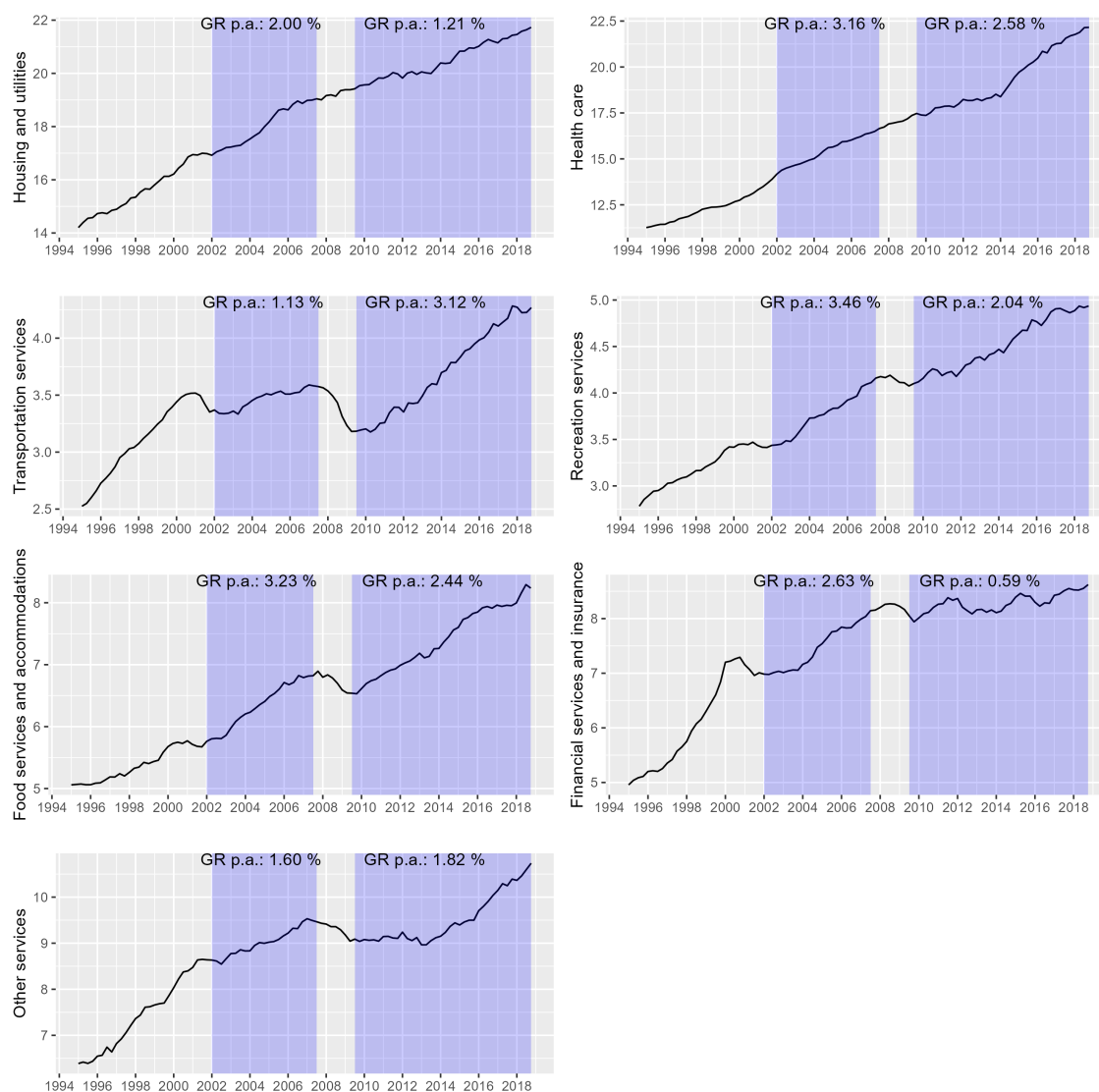
Notes: These figures illustrate the trajectories of various service categories. The purple shading indicates the periods used for the growth rate calculations. The data stem from BEA and FRED.

4.3.2 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 affected 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.

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

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 affected, sec_d is a dummy, which is equal to 1 if the sector is one of the previously identified sectors[7]. Additionally, we include DTI_{2011}, the debt-to-income ratio by county 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.

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[7]Housing and Utilities, Health Care, Recreation services, Food services and accommodation and Financial Services and Insurance

Table 6: County Level Exposure and Employment

	(1)	(2)	(3)	(4)	(5)	(6)
	$log(emp_{ict})$	$log(emp_{ict})$	$log(emp_{ict})$	$log(emp_{ict})$	$log(emp_{ict})$	$log(emp_{ict})$
$\mathbf{1}_{cp}exp_c$	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)
$\mathbf{1}_{cp}exp_{c,d}$				0.0416	0.0000	0.0000
				(0.0423)	(.)	(.)
$\mathbf{1}_{cp}exp_{c,d}sec_d$				-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 the standard measure of exposure, obtained by dividing the average exposure of each county to affected banks divided by number of years. Columns (4), (5) and (6) use the exposure measure based on interstate banking deregulation (Hoffmann and Stewen, 2019). In both specifications, the first regression is always without year fixed effects. Standard errors are clustered at the county-industry level.

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 $\mathbf{1}_{cp}exp_c sec_d$. Our specification in column 1 includes industry-year and county-industry fixed effects. Industry-year fixed effects capture everything that would 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 affected 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 affected institutions.

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

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

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 affected, exp_s is the exposure of state s to the affected 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_{ds} is the measure constructed by aggregating the county-level exposure. 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: State-level Evidence on Consumption

	(1)	(2)	(3)	(4)	(5)	(6)
	$log(con_{ist})$	$log(con_{ist})$	$log(con_{ist})$	$log(con_{ist})$	$log(con_{ist})$	$log(con_{ist})$
$\mathbf{1}exp_s$	0.1049	0.0000	0.0000			
	(0.0952)	(.)	(.)			
$\mathbf{1}exp_s sec_d$	-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)
$\mathbf{1}exp_{s,d}$				0.0073	0.0000	0.0000
				(0.0137)	(.)	(.)
$\mathbf{1}exp_{s,d}sec_d$				-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-sector-year level. Columns (1),(2) and (3) use a standard measure of exposure, obtained by dividing the average exposure of each county to affected banks divided by number of years. Columns (4), (5) and (6) use the exposure measure based on the interstate banking deregulation (Hoffmann and Stewen, 2019). Standard errors are clustered at the state-sector level.

Table 7 confirms our results from the previous section. All our regressions are weighted by population, as in Mian and Sufi (2010)[8]. Column 1 presents the results from the baseline regression where we include industry-state fixed effects and industry-year fixed effects to control for a national trend. The results indicate that a 10 percentage points higher exposure to affected 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

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

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 consent order 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. 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. One natural question is how lower house prices translate into lower consumption, especially if households are not selling their homes and the deleveraging channel is controlled for. Several mechanisms may be at play. First, lower housing wealth reduces the value of collateral and can tighten credit constraints, particularly for liquidity-constrained households. Second, housing market shocks can have local general equilibrium effects, reducing employment and income in related sectors. Third, households may respond to perceived wealth losses or increased uncertainty with more precautionary savings. Lastly, buyer-seller asymmetries may lead to an aggregate consumption decline even if homes are resold. We acknowledge that these channels are difficult to disentangle with our data and caution against a single-mechanism interpretation. For a broader discussion of equilibrium effects operating through house prices, see (Aruoba et al., 2022).

5 Robustness

In this section, we provide robustness checks and 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, we examine whether the decrease in mortgage lending by affected 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 affected banks was not binding for households. Third, we investigate whether the stress tests that occurred simultaneously drive our results. Fourth, we analyze whether differing pre-existing trends for affected and non-affected banks in terms of creditworthiness of customers might bias our results. Fifth, we investigate the possibility that the decrease in consumption is a consequence of a decrease in permanent income.

5.1 Loans to Firms

On a broader level, we find that economic policies can have unintended 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.

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 several reasons why we use small business lending data to test the hypothesis of a collateral channel. 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 (Alon et al., 2018; Haltiwanger, 2015). 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. Additionally, we face a data limitation in so far as data on lending to big firms do not exist at the level of disaggregation needed for this analysis. We run the following regression:

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

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 consent order period and the exposure of a county to affected 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 affected banks also experienced a decrease in small business lending in the consent order period.

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)		
$\mathbf{1}_{cp}exp_d$			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 level. Columns (1) and (2) use a standard measure of exposure, obtained by dividing the average exposure of each county to affected banks divided by number of years. Columns (3) and (4) use the exposure measure based on interstate banking deregulation (Hoffmann and Stewen 2019). In both specifications, the first regression is always without year fixed effects. Standard errors are clustered at the county level.

Column 1 in Table 8 presents the results of a regression where we include county-level controls and the county fixed effect. Column 2 includes time fixed effects and the previously positive coefficient is rendered insignificant here. Column 3 uses our second measure of exposure to affected 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 affected banks. Overall, these findings suggest that the decrease in consumption was not a consequence of a decrease in lending to firms.

The previous result holds for overall lending in the counties exposed to affected banks. Now, we perform a similar analysis, but on the bank level. We use data on the lending of affected banks to small firms to investigate whether affected 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:

$$\Delta log(smeloan_{bct}) = \beta_0 + \beta_1 \mathbf{1}_{cp}affected_{bt} + \theta X_{bt} + \phi_t + \sigma_{ct} + \eta_{bc} + \epsilon_{bct},$$

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, $\mathbf{1}_{cp}affected_{bt}$ is the dummy variable for whether bank was affected, X_{bt} is the set of controls, σ_{ct} are county-year fixed effects and η_{bc} are bank-county fixed effects. Our controls at the bank level are the last period growth rates of 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 affected banks increased their loan supply to small businesses during the period of conset orders. When we include county-year fixed effects in column 2, this positive effect becomes insignificant and smaller in magnitude. As affected banks face additional constraints for mortgage loan origination, more funds might be available for other activities. However, our results indicate that much of this is driven by county-specific demand.

Table 9: Loans to Firms - Bank Level

	(1)	(2)
	$\Delta log(smeloan_{bct})$	$\Delta log(smeloan_{bct})$
$\mathbf{1}_{cp}affected_{bt}$	0.2776***	0.1710
	(0.1021)	(0.1371)
N	142418	137748
Bank-County FE	yes	yes
Year FE	yes	no
County-Year FE	no	yes

Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows regression results for small business lending at the bank-county-year level. Column (1) includes bank-county fixed effects. Column (2) adds both bank-county and county-year fixed effects. Standard errors are clustered at the holding level.

5.2 Effect of Consent Orders on Household Debt

We provided evidence that the foreclosure regulation led to lower mortgage loan growth for affected banks. This suggests that counties that are more exposed to the affected 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 affected. 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 affected banks, X_{ct} is a vector of other county specific controls:

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

Table 10: Exposure and Household Debt

	(1)	(2)	(3)	(4)
	DTI_{ct}	DTI_{ct}	DTI_{ct}	DTI_{ct}
$\mathbf{1}_{cp}exp_c$	-0.4041***	-0.1394**		
	(0.0323)	(0.0586)		
$\mathbf{1}_{cp}exp_{c,d}$			-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 level. Columns (1) and (2) use a standard measure of exposure, obtained by dividing the average exposure of each county to affected banks divided by number of years. Columns (3) and (4) use the exposure measure based on interstate banking deregulation (Hoffmann and Stewen 2019). In both specifications, the first regression is always without year fixed effects. Standard errors are clustered at the county level.

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 affected 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 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 affected banks of 10 percentage points reduced their debt-to-income ratio by around 21 percentage points.

Table 11: Bank-County-Year Regressions - Stresstest

	(1)	(2)	(3)
	$\Delta log(loan)$	$\Delta log(loan)$	$\Delta log(loan)$
$\mathbf{1}_{cp}affected$	-0.1928**	-0.1928**	-0.1937**
	(0.0878)	(0.0878)	(0.0874)
stresstest		0.1057	0.1045
		(0.2354)	(0.2356)
captialratio			343.4107
			(392.8693)
N	758380	758380	758380
BankCountyFE	yes	yes	yes
YearFE	yes	yes	yes
CountyYearFE	no	no	no

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.3 Confounding effect: Stress tests

In 2011, the Comprehensive Capital Analysis and Review (CCAR) stress tests were introduced. The initial stress tests applied to banks with total consolidated assets of $50 billion or more, creating some overlap with banks that were subject to consent orders[9] (Fed, 2012). Calem et al. (2013) show that the stress tests reduced the supply of mortgage loans. While all banks in the stress tests were also affected by the foreclosure regulation, not all affected banks underwent stress testing. To disentangle the effects of these two policies, we include controls for whether a bank was subject to the stress test and for the bank's capital ratio. The capital ratio is included because the impact of the stress test should be concentrated among undercapitalized banks, whereas the foreclosure regulation should affect banks regardless of their capital adequacy. Table 11 shows that neither controlling for the stress test nor the capital ratio changes our main results, and the coefficients of interest remain stable. This gives us confidence that our findings are not driven by the effects of the stress tests.

5.4 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. 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, as highly indebted borrowers are less likely to repay the loan. Similarly, riskier borrowers might be less able to repay the loan. 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

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[9]Specifically, Aurora Bank, EverBank, OneWest, Santander Bank, HSBC, and US National Bank signed consent orders but did not participate in the stress tests.

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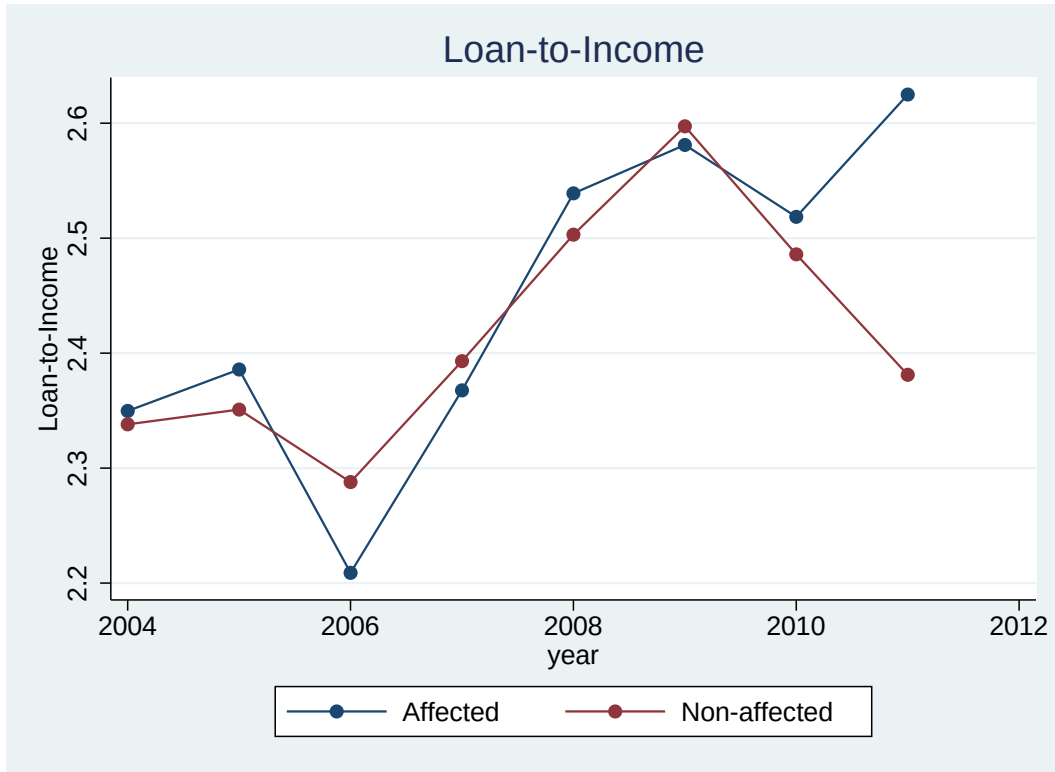
applicant is only available from 2018, we calculate the average loan-to-income (LTI) ratio and look at how this measure differs across affected and non-affected banks. Figure 9 shows that in both groups of banks the loan amount was around 2.4 times higher on average than the yearly income. Both in the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

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 affected 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 in the Appendix show that the distribution of the 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 affected 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 12 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:

$$\Delta log(loan_{bct}) = \beta_0 + \beta_1 \mathbf{1}_{cp}\text{affected}_{bt} + \theta X_{bt} + \phi_t + \sigma_{ct} + \eta_{bc} + \epsilon_{bct},$$

where $\Delta log(loan_{bct})$ is the growth of mortgage loans originated by bank b in county c at time t, $mathbf1_{cp}\text{affected}_{bt}$ is the dummy variable if the bank has signed a consent order with the Fed or the OCC after 2011, 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.

Column 1 of Table 12 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-consent order 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 and view certain ethnicities as riskier borrowers, 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 consent orders 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.

Table 12: Bank Robustness Check

	(1) $\Delta log(loan)_{bt}$	(2) $\Delta log(loan)_{bt}$	(3) $\Delta log(loan)_{bt}$	(4) $\Delta log(loan)_{bt}$
$\mathbf{1}_{cp}affected_b$	-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 first three columns include each control variable separately, and the last column includes all. Standard errors are clustered at the holding level.

5.5 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.

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:

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

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

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

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 13 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 consent order period.

Table 13: Consumption and Permanent Income

	(1)	(2)	(3)
	$log(con_{ist})$	$log(con_{ist})$	$log(con_{ist})$
$\mathbf{1}_{cp}exp_c$	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.

6 Conclusion

This paper investigates the effect of foreclosure regulation on the slowdown in U.S. consumption following the 2008 financial crisis. We argue that a series of stricter foreclosure requirements imposed on certain banks contributed to the decline in consumption. We find that institutions subject to stricter foreclosure regulations reduced the growth of mortgage loan origination during the regulatory period. This decline in lending is not explained by county-level demand for mortgages or by banks' ability to diversify their mortgage portfolios. Instead, the reduction occurs on the extensive margin.

We further show that house prices in areas exposed to affected banks recovered more slowly. Consistent with this finding, counties more exposed to these banks also experienced a decline in their debt-to-income ratio in the post-regulation period. This supports our hypothesis that mortgage lending from affected banks

was not fully replaced by other lenders. Due to the lack of county-level consumption data, we examine the impact of reduced mortgage lending on employment and state-level consumption. Both analyses corroborate our hypothesis. We test the robustness of our results against alternative explanations, including supply-side factors and changes in permanent income, assess the role of pre-existing trends, and provide evidence that the reduction in mortgage lending was indeed binding for households.

Our findings suggest that foreclosure regulation contributed to the slower recovery in consumption following the crisis. A natural policy question is whether stricter foreclosure rules are detrimental in the aftermath of a financial crisis. The answer depends on the broader effectiveness of the policy. Foreclosure regulation reduces mortgage lending and affects consumption through household balance sheets. While our results indicate that the policy slowed the recovery of key economic aggregates, it may also promote higher long-run output and enhance the economy's resilience to future crises. If foreclosure policies lead banks to adopt more cautious foreclosure practices, they could improve outcomes in future downturns. We leave it to future research to evaluate whether the long-term benefits of foreclosure regulation outweigh its short-term costs.

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