Abstract

Foreclosures per open mortgage grew by over a factor of five between 2000 to 2009. Using geographic and loan-level data on labor market flows and housing prices between 2000 and 2014, we estimate how the surge in foreclosures affected local economic activity during the financial crisis. Our identification strategy exploits the staggered and discontinuous changes in interest rates among holders of adjustable rate mortgages (ARMs). We find that a 10% rise in foreclosures is associated with a 0.77% decline in employment, 1.85% decline in hiring, 0.09 percentage point decline in job turnover, 0.46% decline in housing prices, and 0.82 percentage point decline in housing price growth. Our partial equilibrium estimates imply that the surge in foreclosures during the Great Recession can account for up to 20% of the decline in the hiring rate and 10% of the decline in housing prices over the Great Recession. Using additional individual and loan-level data, we examine potential mechanisms, focusing on the role of bank lending and the supply of credit to small businesses. The adverse effects of foreclosures on credit supply are driven by not only the effect of foreclosures on the health of bank balance sheets, but also the effect of foreclosure externalities on local sentiment.
Keywords: employment; foreclosures; housing, labor markets; mortgages; uncertainty.

JEL: G21, J21, J23, R31

1 Introduction

Housing prices fell by nearly 20% between 2007 and 2009, associated with a decline in consumption (Mian and Sufi, 2011), consumer demand (Mian et al., 2013), and employment (Mian and Sufi, 2014). Moreover, foreclosures per open mortgage grew by over a factor of three (see Figure 1). Motivated by a concern that foreclosures would introduce large spillovers in local communities and the banking sector (Bernanke, 2008), the federal government responded to their rapid rise by offering banks incentives to renegotiate seriously delinquent loans, culminating in over $75 billion for the Home Affordable Mortgage Program (HAMP) on February 19, 2009 (see Appendix Section A.1 for institutional details). During these years, the Federal Reserve actively worked with mortgage servicers to facilitate loan renegotiation and with non-profits to develop materials, tools, and training programs that would help mitigate the rise in foreclosures.

[INSERT FIGURE 1]

This paper examines whether the rapid and significant surge in foreclosures affected local outcomes and prolonged the recovery from the Great Recession independent of the canonical housing price channel (Campbell et al., 2011; Mian et al., 2015; Guren and McQuade, 2013). Unfortunately, empirically estimating the causal effects of local foreclosures is fraught with identification problems. Even conditional on location fixed effects, there are two opposing forces of endogeneity. On one hand, increases in unemployment made it difficult for many borrowers to pay down their mortgage, increasing the number of foreclosures (Tian et al., 2016; Hsu et al., forthcoming; Gerardi et al., forthcoming). On the other hand, banks had a strategic incentive to delay foreclosing immediately on individuals with large housing price declines since doing so would have required valuing the homes at their market value, which would have placed many banks in insolvency since the

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1We define a foreclosure as occurring when a homeowner loses possession of their home at the end of the foreclosure process, rather than the start of the foreclosure process, as is common in many other research settings. The CoreLogic data tracks this information, reporting, for instance, when a house goes from being in foreclosure to REO (Real Estate Owned), meaning that ownership has been transferred to the lender who then records the assets on their books as REO.
mortgages were under water.\textsuperscript{2} A related explanation behind foreclosure delay was administrative backlogs since the surge in foreclosures took place in such a short window of time.\textsuperscript{3}

The first part of the paper introduces an identification strategy that exploits the contractual structure of adjustable rate mortgages (ARMs) originated prior to and during the Great Recession. These loans specify a fixed (“teaser”) interest rate on mortgage payments for $k$ years. However, after the specified $k$ years, the interest rate abruptly changes. Using over three million unique ARM loans from proprietary CoreLogic data, we exploit the timing and intensity of interest rate fluctuations on 5-1, 7-1, and 10-1 ARMs to predict realized foreclosures within a county and assess how these predicted foreclosures affect local labor and housing market outcomes.\textsuperscript{4} These loan resets are contractually determined at the origination of the loan and influenced only by national interest rates, allowing us to rule out the bank-lending channel as a confounding factor. We show non-parametrically that positive (negative) interest rate resets on these loans are associated with discontinuous spikes (drops) in foreclosure probabilities.\textsuperscript{5} We also find that local foreclosures on ARM borrowers also have large spillovers on borrowers with fixed rate mortgages (FRM): a 10% rise in ARM foreclosures is associated with a 5.8% rise in FRM foreclosures in a zipcode. Under our preferred estimates, a 10% rise in interest rate resets is associated with a 1.9% rise in foreclosures. A back-of-the-envelope calculation implies that the rise in interest rate resets from loans originated prior to the financial crisis accounts for nearly 20% of the overall rise in foreclosures.

The second part of our paper uses these interest rate resets to produce a simulated instrument

\textsuperscript{2}See Figure 1 for an illustration of the sudden spikes and subsequent declines in the share of new foreclosures entering the market. Former Treasury Secretary Tim Geithner is famously reported as describing federal programs for homeowners in foreclosure to have been principally designed to slow the pace of foreclosures so as to “foam the runway” for banks, enabling them to avoid recognizing losses on their housing portfolios all at once. http://dealbook.nytimes.com/2014/05/06/what-tim-geithner-got-right/.


\textsuperscript{4}Our identification strategy is similar to several papers. First, Di Maggio et al. (2017a) examine the impact of interest rate resets on individuals’ disposable income, finding that households use increased income following interest rate declines to finance additional consumption. (From here on out, we will refer to Di Maggio et al. (2017a), which is a combined version of Keys et al. (2014) and Di Maggio et al. (2015).) Second, Fuster and Willen (2017) examine the impact of payment size on repayment behavior, finding that reducing payment by a half reduces the delinquency hazard by roughly 55 percent. Third, Gupta (2016) examines the effect of these interest rate changes on default probabilities and housing market spillovers. While our paper is conceptually similar in that it exploits quasi-experimental variation in adjustable rate mortgages, we provide a new methodological and empirical application of them. We discuss these similarities and differences in greater detail later.

\textsuperscript{5}One concern is that these interest rate resets primarily affect higher income borrowers, which would prevent us from recovering heterogeneous treatment effects. However, we also consider specifications that use 2-1 and 3-1 ARM holders to compute interest rate resets, finding very similar results—slightly lower in magnitude because they exacerbate the dynamic selection problem. The fact that our estimates are similar with the inclusion of these loans, which are directed towards lower income borrowers, suggests that our main estimates generate sufficient variation in the propensity to fall into foreclosure following an interest rate shock.
for realized foreclosures. Our exclusion restriction requires that borrowers and banks, for example, do not originate a 5-1 ARM versus a 7-1 ARM in anticipation of local employment growth five, seven, or ten years in the future, conditional on all of our controls. Using county data from the Federal Housing Administration (FHA) and industry × county and firm size × county data from the Longitudinal Employer-Household Dynamics (LEHD), we find that a 10% rise of foreclosures is associated with a 0.77% decline in employment, a 2.1% decline in hiring, and a 0.09 percentage point (pp) decline in job turnover. However, we find considerable heterogeneity. These declines come primarily from the non-tradables sector, small firms between 20 to 249 employees, and states with judicial status laws. We also find that a 10% rise in foreclosures is associated with a 0.46% decline in housing prices and 0.82% decline in house price growth. However, we do not find evidence that foreclosures raised income inequality within or across counties; in fact, if anything, they decreased inequality. We also show how these results help understand the slow recovery in many counties following the recession (Fernald et al., 2017). Given our estimated elasticities, a back-of-the-envelope calculation suggests that the rise of foreclosures can explain 10% of the decline in housing prices and 10-20% of the decline in the hiring rate during the financial crisis.6

While a valid concern is that banks strategically target counties with different types of loans in ways that are correlated with contemporaneous labor market conditions, we implement a wide array of diagnostics to understand the reliability of our exclusion restriction. First, we show that the bulk of the variation in ARM dispersion is driven by historical variation in the formation of banks with different lending strategies in different areas. It is, therefore, not surprising that we find no correlation between the 2003-04 share of ARMs and various county economic shocks (e.g., income) between 1990-2000. Second, we show that borrowers with 5-1, 7-1, and 10-1 ARMs are homogeneous in their FICO scores, suggesting that borrowers are not self-selecting into different types of loans for reasons that are potentially correlated with beliefs about future economic growth. Third, we show that changes in the county income distribution are not systematically correlated with the share of ARMs. If banks were strategically targeting different areas, we would expect to see a pattern. Fourth, we show that, while the share of ARMs in year $t$ is correlated with

6There are two potential concerns about our aggregation exercise. First, it is a partial equilibrium exercise, which fails to take into account general equilibrium feedback mechanisms (Beraja et al., 2016). We examine the potential for reallocation by regressing logged employment in neighboring counties on logged foreclosures in the main county under our baseline approach, finding that, if anything, foreclosures in one county actually lead to negative employment spillovers on neighboring counties. Second, we recover a marginal effect of foreclosures, which might be an underestimate in the presence of many other contemporaneous factors during the financial crisis (Brunnermeier and Sannikov, 2014).
employment growth, it is uncorrelated with other potential confounders, such as income or housing price growth in year $t+5$. Even though many individuals form beliefs about the future, they are not doing so in a way that interacts with their loan purchasing decisions. Fifth, we use an alternative Bartik-like instrumental variables strategy that exploits a county’s pre-recession exposure to banks that are more likely to experience interest rate resets. While our estimates are less precise, this approach does not use variation that could be correlated with other contemporaneous factors.\footnote{We also implement a number of additional robustness exercises. For example, our baseline estimates are robust to controlling for industry \times county earnings and/or delinquencies, which further mitigate concerns about an income effect and/or other unobserved contemporaneous factors correlated with interest rate resets on ARMs. Second, insofar as selection on unobservables is no more than selection on observables (which holds in our data due to an $R$-squared of $\approx 0.90$), we follow Oster (forthcoming) and show that omitted variables cannot reverse our estimates.}

Even if these interest rate resets are orthogonal to unobserved shocks to the labor market, one potential concern with these results is that they do not reflect the effects of foreclosures, but rather a disposable income channel (Di Maggio et al., 2017a). For example, if interest rates for ARM borrowers decline, they will experience a positive shock to disposable income, which could affect local economic activity by stimulating aggregate demand. However, since our foreclosure gradient is present also in the tradables sector, an aggregate demand channel does not explain all of our observed effects. Since we control aggregate mortgage payments for each county \times quarter (and can include housing prices), we also remove variation in labor market outcomes that is driven by time-varying shocks to housing prices and/or loan volume.\footnote{We have also experimented without housing prices as a control based on the potential concern that we are “over controlling”. Our results are quantitatively similar, but slightly smaller in magnitude since housing prices behave as an omitted variable that amplify the dynamic selection problem that we detail in our section on identification.}

We further examine the correlation between state \times year per capita consumption for different expenditure categories, finding that the gradient is only significant for housing and utilities, consistent with the evidence from Di Maggio et al. (2017a) that borrowers use interest rate resets downwards primarily to deleverage from high levels of debt accumulated during boom years.

The third part of our paper explores the plausible mechanisms behind our results. We focus on the impact of local foreclosures on credit supply and the provision of external financing by banks.\footnote{The cost and availability of credit affects firm employment in several ways. First, if labor has a quasi-fixed component of costs (e.g., training), then adjustments to the stock of labor in the firm requires investment; see Oi (1962) and Hamermesh (1989) for early estimates of these adjustment costs. Second, since labor is typically used together with other inputs, like capital, which is a stock, then changes in the price of capital alter the optimal allocation of labor; see Oberfield and Raval (2014) for estimates of the elasticity of substitution between capital and labor. Third, since there is often a delay between the realization of revenues and payment of compensation, credit is required (Greenwald and Stiglitz, 1988). See Benmelech et al. (2015) for further evidence on the importance of credit for financing labor investments.}
Using loan-level data from the Small Business Administration (SBA) obtained through a Freedom of Information Act (FOIA) request, we show that a 10% rise in foreclosures is associated with a 0.38-0.54pp decline in the share of a loan that banks are willing to lend to small businesses. We also find a shift in the composition of loans that get funded, consistent with a flight to quality in the face of large unanticipated shocks (Caballero and Krishnamurthy, 2008).

To understand the decline in credit supply, we focus on the impact of foreclosure-induced declines in optimism and increases in uncertainty. Using proprietary data from Gallup’s U.S. Daily Poll, which surveys 1,000 individuals each day about their perceptions of the current and future state of the economy, we find that a 10% rise in foreclosures is associated with a 1.8% decline in the perception of the future state of the economy and a 1.1% rise in the dispersion of local beliefs about the future state of the economy. Our measures of perception of economic activity compare well with other frequently used measures of sentiment, including the volatility index, the economic policy uncertainty index (Baker et al., 2016), and the investor sentiment index (Baker and Wurgler, 2006). One concern, however, with these results is that they simply reflect the direct effect of foreclosures on bank portfolios. Using additional data from the Call Reports restricted to the set of local banks, we show that increases in local foreclosures reduce the growth rate of bank lending even after controlling for changes in bank assets and deposits. The fact that local foreclosures are systematically associated with declines in lending after controlling for their balance sheets suggests that we are capturing a complementary source of variation behind the bank lending channel (Peek and Rosengren, 2000; Chodorow-Reich, 2014; Bentolila et al., 2017; Amiti and Weinstein, forthcoming). Our results are consistent with recent evidence from Di Maggio et al. (2017b) who show, using a separate measure, that local shocks to uncertainty also affect credit supply. These results also complement a large literature about the impact of uncertainty on investment on firms and the macroeconomy (Bernanke, 1983; Hassler, 1996; Bloom, 2009) and a flight to quality in the presence of uncertainty (Caballero and Krishnamurthy, 2008).

Our paper is most closely related with an emerging literature on the effects of macroeconomic shocks on household finance and the housing market; see, for example, Mian and Sufi (2014) and Adelino et al. (2015b) on employment, Mian and Sufi (2009), Mian et al. (2013), and Adelino et al.  

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10 As we discuss later, our approach is complementary to Di Maggio et al. (2017b) who use the excess returns of public firms in a county after controlling for common sectoral four-digit industry shocks. One of the reasons we defer to our measurement approach comes from the fact that publicly traded companies are highly concentrated in certain metropolitan areas. Since our focus is on foreclosures (rather than credit), which are an inherently local phenomenon and were distributed across a wide array of counties, we defer to the more comprehensive data and direct measurement from Gallup.
(2016) on credit, Mian and Sufi (2011) and Di Maggio et al. (2017a) on consumption, Herkenhoff and Ohanian (2015) on searching and matching in the labor market, and Agarwal et al. (2017) on policy interventions. While there has been some study over the macroeconomic effects of foreclosures (Corbae and Quintin, 2015; Mitman, 2016), most of the literature has focused on how foreclosures impact real economic outcomes through housing prices (Campbell et al., 2011; Mian et al., 2015; Guren and McQuade, 2013; Gupta, 2016; Anenberg and Kung, 2014). Our results complement these papers by showing that foreclosures can directly affect labor market outcomes. There is an increasingly large body of empirical contributions that highlight the role of credit disruptions in explaining the decline in employment during both the Great Depression (Benmelech et al., 2017a) and Great Recession (Chodorow-Reich, 2014), which also impacted consumption (Mian and Sufi, 2011; Mian et al., 2013; Benmelech et al., 2017b). However, these contributions have focused on the role of idiosyncratic loan supply shocks. Our results show that foreclosures create not only a direct loan supply shock—that is, a rise in foreclosures deteriorates bank balance sheets—but also an indirect shock to the supply of credit—that is, by raising risk aversion among lenders and affecting the credit risk of different projects. The fact that we also find heterogeneity based on states with and without judicial status laws, which affect the cost and length of foreclosure (Pence, 2006), relates with recent work on the empirical effects of debtor protections. For example, Dobbie and Song (2015) show that these protections reduce foreclosure rates and increase long-run earnings and Dobbie and Goldsmith-Pinkham (2015) show that they cushioned against the decline in consumption and employment during the Great Recession.

Our results also relate with policy discussions on the role of foreclosure delay. While delays in the foreclosure process serve as a source of credit for home owners, which helps them wait for higher quality job matches (Herkenhoff, 2015; Herkenhoff and Ohanian, 2015), they might also raise uncertainty on both the consumer and bank side, and delay new home construction (Calomiris and Higgins, 2011). The fact that we find states with non-judicial status laws do not have as large of a decline in employment and hiring in response to foreclosure shocks, even in spite of the fact that they have many more foreclosures, is consistent with the view that shorter foreclosure processes help lead to the realization of uncertainty more quickly. The pace of loan renegotiation and the potential for foreclosure, therefore, joins a recent literature on the design of mortgage markets. For example, Guren et al. (2017) develop and estimate an equilibrium model

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11Herkenhoff (2015) argues that credit allows individuals to search for better jobs and higher quality of matches. Cohen-Cole et al. (2016) assess the implications of this link between credit and employment to quantify aggregate effects.
of the mortgage market, finding that loans that front load payment reductions to borrowers during a crisis outperform loans that spread the benefit over the life cycle of the mortgage (i.e., a fixed rate mortgage). Similarly, Piskorski and Tchistyi (2010) find that an optimal contract resembles an adjustable rate mortgage and Piskorski and Tchistyi (2011) find that mortgage modifications are optimal in settings with stochastic housing prices.

Our paper finally complements an emerging literature on foreclosures and mobility in the labor market (Demyanyk et al., 2017; Brown and Matsa, 2016). This literature generally focuses on foreclosure at an individual-level—that is, an individual is foreclosed upon and has to search for a job in another local labor market if they cannot find a job in their current location. In fact, Demyanyk et al. (2017) show that individuals might leave areas with declining home prices even if they are not foreclosed upon—what matters is their outside option (see Bernstein and Struyven (2016) and Veldhuizen et al. (2016) for further evidence on the impact of negative home equity). The fact that we find foreclosures are associated with declines in local amenities, such as neighborhood quality (Makridis and Ohlrogge, 2017) and crime (Immergluck and Smith, 2006; Cui and Walsh, 2015), is consistent with these prior contributions given that high skilled workers value non-market amenities and contribute to the endogenous formation of them (Diamond, 2016).

The structure of the paper is as follows. Section 2 introduces background on the potential mechanisms through which foreclosures might affect the labor market and institutional details about adjustable rate mortgages (e.g., their incidence). Section 3 introduces the data sources, characterizes the variation in the data, and estimates the effects of interest rate resets on foreclosure probabilities at the loan-level. Section 4 introduces the research design, containing the identification strategies and evidence behind the relevant assumptions. Section 5 documents our main results, quantifies their aggregate effects, and implements an array of robustness exercises. Section 6 examines the mechanisms behind our results, focusing on the effect of foreclosures on sentiment and the associated ramifications for bank lending. Section 7 concludes.

## 2 Background

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12 Although Ferreira et al. (2010) finds that a decline in home equity reduces mobility, Schulhofer-Wohl (2012) shows that their result is driven by their dropping some observations with negative home equity homeowner moves; see Coulson and Grecce (2013) and Bucks and Bricker (2013) for additional evidence from the Panel Study of Income Dynamics and Survey of Consumer Finances.
2.1 How Could Foreclosures Affect Employment?

While there is a recent macroeconomic literature on foreclosures, none study the local labor market effects apart from the housing price channel. Mitman (2016) builds a dynamic heterogeneous agent model where individuals can default on their homes and uses the model to analyze the effects of the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) and Home Affordable Modification Program (HAMP) policy interventions. He finds that, while HAMP reduced foreclosures by one percentage point and led to large welfare gains among high loan-to-value mortgage holders, BAPCPA actually increased foreclosures when housing prices fell. Corbae and Quintin (2015) build a dynamic heterogeneous agent model as well, finding that the rise of high-leverage loans originated prior to the crisis can explain over 60% of the rise in foreclosure rates. None of the literature thus far, however, examines how foreclosures might affect employment. We now turn towards to plausible mechanisms that could be at play in the data.\footnote{The only evidence on the impact of foreclosures on the labor market we are aware of to date is from Rana and Shea (2015) who estimate a series of vector auto-regressions. They find that increases in foreclosures are associated with increases in unemployment at a state-level, but do not exploit plausibly exogenous variation and do not provide a potential mechanism explaining the results.}

Before turning to the data, we begin with a theoretical framework for understanding the potential mechanisms at play.\footnote{We omit discussion of the conventional housing price channel whereby foreclosures lead to housing price declines over nearby homes, which has been subject to significant study in recent years (Campbell et al., 2011; Mian et al., 2015; Guren and McQuade, 2013; Gupta, 2016).} The first channel is the direct effect of foreclosure on bank balance sheets. For example, if a bank forecloses on a home owner who was seriously delinquent, the bank updates their balance sheet with the new price of the home. However, since many of these homes were “under water,” foreclosure was a source of financial distress for many banks and behaved as an idiosyncratic shock to their balance sheets, curtailing their willingness to lend and provide credit. There has already been extensive study of this channel in the literature (Peek and Rosengren, 2000; Chodorow-Reich, 2014; Bentolila et al., 2017; Amiti and Weinstein, forthcoming).\footnote{See Bernanke and Gertler (1995) for an introduction to the balance sheet channel—when changes in monetary policy affect borrowers’ balance sheets and income statements—and the bank lending channel—when changes in monetary policy affect the supply of loans by depository institutions.}

The second channel, and the one that we focus on, arises from the indirect effects of local foreclosures on the willingness to provide credit. While it is well-known that banks use an array of local information to determine loan terms and whether to lend to local businesses (Harle et al., 2015), recent evidence from Sirignano et al. (2016) shows that banks respond to local (e.g., county or zipcode) foreclosures in their modeling of credit risk. For example, when local foreclosures rise,
a bank might adjust their lending practices even if it was not the entity holding the loans since foreclosures signal deteriorating neighborhood amenities and value. However, to the extent that foreclosures also reduce local optimism and raise local uncertainty, banks might also respond to local foreclosure shocks by becoming more risk averse. For example, Bernstein et al. (2017) show that housing wealth shocks make employees less likely to patent and innovate.

Why might foreclosures raise uncertainty, in addition to reducing local optimism? While many market participants expected foreclosures to rise at the beginning of the financial crisis due to the decline in housing prices, arguably no one expected the surge in foreclosures to be so significant and rapid. Given that the surge in foreclosures was far above any increase that had happened in the post-war U.S. era, both firms and banks were confronted with Knightian uncertainty about the realm of possible damages. For example, banks were uncertain about who had taken larger losses and the extent of remaining exposure to loan defaults (Pritsker, 2013). Similarly, firms were uncertain about the availability of credit and consumer demand (Murillo et al., 2010). During these times of grave uncertainty, banks and firms may exhibit a flight to quality in the form of both physical and human capital assets (Caballero and Krishnamurthy, 2008), turning towards more conservative lending and hiring strategies.

There are three specific channels through which uncertainty might affect firm hiring and bank lending. First, the “real options effect” means that uncertainty (Bernanke, 1983; Hassler, 1996), especially in the presence of irreversible investments (e.g., firm-specific human capital) (MacDonald and Siegel, 1986; Abel et al., 1996; Bloom, 2009), can decrease the amount of and timing of investment in labor. Second, the “risk premium effect” means that uncertainty can affect the

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16 While we use the term “uncertainty,” an equally plausible scenario is that the surge in foreclosures generated “ambiguity” about the distribution of local risk since they were so unprecedented, relative to historic levels (Ilut and Schneider, 2014).

17 First, part of the rationale for the passage of the Home Affordable Modification Program (HAMP) was to delay the rise of further foreclosures in March 2009. Second, the relationship between housing prices and foreclosures fundamentally flipped in 2009. For example, if we regress logged foreclosures on logged housing prices at the county-level, we obtain coefficients of 0.504, 0.937, and 0.604 for 2006, 2007, and 2008, respectively, whereas we obtain coefficients of -0.623, -1.289, -2.367, -2.935, and -1.905 for 2009, 2010, 2011, 2012, and 2013, respectively. Third, there was uncertainty about the legal status of foreclosed homes and how they would be dealt with given the fault paper work associated with the sale of mortgages prior to the crisis. http://www.nytimes.com/2010/10/28/business/28housing.html

18 For example, newspapers and policymakers have pointed towards significant uncertainty associated with how long it would take for homes to get off the market following foreclosure. http://www.npr.org/templates/transcript/transcript.php?storyId=130469981

19 While firms might also contract physical investment, labor is an important determinant of firm value (Merz and Yashiv, 2007) and, therefore, they may also contract hiring.

20 Kehoe et al. (2016) develop and estimate a quantitative search and matching model with quasi-fixed costs of labor (human capital). Since the benefits of a match are long-lived with human capital formation, then changes
cost of financing (e.g., interest rates) associated with financing an otherwise productive investment (Arellano et al., 2016; Gilchrist et al., 2014; Hall, 2017). Third, the “risk aversion effect” means that uncertainty, especially in the presence of nominal rigidities (Basu and Bundick, 2017), can reduce the desire to pursue high return and innovative projects (Bernstein et al., 2017). Later, we examine evidence for each of these possible channels, finding the strongest evidence for them among firms in the tradables sector, which we show is more dependent on external financing.

Much of our microeconomic evidence will focus on a sample of small and medium sized businesses based on data obtained from the Small Business Administration (SBA). While we provide suggestive evidence that the mechanism is operational for larger businesses too, focusing on smaller businesses has a legitimate precedent in the existing literature since the decline in employment took place primarily among small businesses and small businesses play a major role in explaining job growth (Haltiwanger, 2012). For example, small firms with 20-49 and 50-249 employees experienced the biggest employment declines of roughly 7% during the financial crisis, whereas firms with 250-499 and 500+ employees exhibited only a 3% decline (see Figure 13 in Appendix Section A.1). Small businesses may have been especially susceptible to these first and second moment shocks as standard quantitative metrics became less informative for discerning the credit worthiness of borrowers (Keys et al., 2012) together with asset quality misrepresentation (Piskorski et al., 2015). Big banks may have simply decided to substitute away from small business lending, which are harder to screen for default risk (Chen et al., 2017).

Our mechanism most closely complements a series of recent contributions emphasizing the importance of credit in explaining fluctuations in investment and employment (Gilchrist and Zain credit affect firm investment activity, which leads to larger employment declines. However, the increase in uncertainty does always imply that net investment declines. In particular, certain stochastic processes may give rise to a larger threshold for investment, but a threshold that happens sooner.

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21Startups, which are potentially different from small businesses, are especially important in the non-tradables sector, where they account for 90% of total net job creation (Adelino et al., 2015a). For example, Chen et al. (2017) show that the decline in credit heavily affected employment among small businesses and Adelino et al. (2015b) show that the effect of housing price shocks was concentrated among small firms; employment in big firms was largely unaffected. There is, however, some controversy. For example, Greenstone et al. (2014) do not find strong evidence of credit shocks on small businesses using an alternative identification strategy.

22We omit a discussion of very small firms with 0-19 employees since they are less likely to make large scale investments. For example, the Bureau of Labor Statistics refers to firms with ten or fewer employees as “micro firms.” Even though the growth rate decline is smaller in magnitude for big firms, their contribution to the level of the employment decline is actually quite large. During the height of the recession between 2008:Q4 and 2009:Q3, average employment was 14.7 million among firms with 20-49 employees, 24.5 million among firms with 50-249 employees, and 82.8 million among firms with 500+ employees, then the implied decline in employees is roughly 705,600, 1,151,000, and 1,000,000, respectively.

23Standard models used to predict default systematically failed during the Great Recession (Rajan et al., 2015).
The current literature has, to our knowledge, focused only on the bank lending channel from the perspective of negative shocks to financially exposed banks (Khwaja and Mian, 2008; Chodorow-Reich, 2014). For example, during a liquidity crisis, some banks will be more exposed than others, which leads to a contraction in their lending relative to others (Iyer et al., 2014). Our mechanism, however, focuses on how the rapid and significant surge in foreclosures could behave as an optimism and uncertainty shock, which can cause a flight to quality and contraction in lending, particularly among small businesses in the tradables sector (Bernanke et al., 1996; Caballero and Krishnamurthy, 2008). These results join an emerging literature examining the effects of uncertainty on credit and other macroeconomic aggregates (Christiano et al., 2014; Di Maggio et al., 2017b), how foreclosures can further accelerate housing price declines (Campbell et al., 2011; Gupta, 2016), and how these foreclosure-induced housing price declines can ultimately affect consumption and employment (Mian et al., 2015).

2.2 The Rise (and Fall) of Adjustable Rate Mortgages

Adjustable rate mortgages (ARMs) became a popular tool for banks to increase lending to borrowers in the 1980s, but did not start expanding in use until the late 1990s and early 2000s. There are two main theories for explaining the use of ARMs over fixed rate mortgages (FRMs). The first is that borrowers look at the expected future costs they expect to face over the life cycle of the loan, relative to the risk that these costs will be higher or lower than expected. If the risks are relatively stable, then borrowers will look at the spread between the current fixed rate and expectation of the adjustable mortgage (Koijen et al., 2009; Botsch and Malmendier, 2017). The second is that credit constraints cause borrowers to care primarily about current interest rates, rather than the costs over the life cycle of the loan, meaning that the spread between the FRM and current ARM rate is what should matter (Campbell and Cocco, 2003, 2015). However, we do not need to take a stand on either of the theories for our identification strategy.

We begin by documenting their incidence throughout our sample, starting in 2000. Figure 2 plots the share of ARMs—separated into two categories of 2-1 & 3-1 ARMs and 5-1, 7-1, & 10-1 ARMs—from 2000 to 2014. The share of 2-1 & 3-1 ARMs peaks around roughly 2007, but declines

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24 See Gertler and Kiyotaki (2011) for a survey of recent literature.
25 Garriga et al. (2014); Duygan-Bump et al. (2015) show that credit shocks played an important role in accounting for declines in employment among small businesses.
26 http://bebusinessed.com/history/history-of-mortgages/
rapidly during the height of the Great Recession, whereas 5-1, 7-1, & 10-1 ARMs experience declines earlier, but do not vanish. One reason we will exploit variation only among 5-1, 7-1, and 10-1 ARMs is precisely because of the collapse in the share of 2-1 and 3-1 ARM lending—they provide no identifying variation after 2008. Appendix Section A.3.1 provides further evidence of the degree of variation across counties in the share of ARMs, showing that counties range in dispersion of ARMs between 0 and 10% of originated loans (Figure 18) and that counties range in their relative proportions of 5-1 to 7/10-1 10-1 ARMs between -3% and 3% (Figure 19).

While these ARMs have the unique feature of inducing heterogeneity in the timing of spikes in individuals’ interest rates, one concern is that their spatial incidence is not random—that is, banks may have strategically increased and decreased certain types of ARMs in certain areas. We examine the plausibility of this concern by introducing two series of exercises. The first set of exercises examines the correlation between changes in economic and demographic outcomes between 1990-2000 and the 2003 and 2004 average share of 5/7/10-1 ARMs at a county-level. If a county-level correlation exists, then it is possible that the incidence of these ARMs in the years preceding the housing boom were driven by economic and demographic shifts—that is, they are not quasi-random. Figure 3 documents these correlations for four sets of variables: growth in county-level housing prices, household incomes, unemployment rates, and the share of college graduates. In each case, the gradient is zero: economic shocks are uncorrelated with ARM dispersion.27

If the shares of ARMs in 2003 are not correlated with growth in economic and/or demographic variables in the preceding decade, then why does the dispersion exist? As we document, certain banks had a preference for issuing one type of ARM versus others. Moreover, banks are remarkably stable in the geographic locations of where they operate. Thus, since banks tend to continue operating where they always have operated, and since some banks favored one type of loan over another, areas that happened to have banks that preferred 5-1 ARMs over 7-1 ARMs tended to get more of the former and fewer of the latter. To formally document these phenomena, we use national bank-by-year data on logged originations of different adjustable rate mortgage types and

27In the Appendix, we also document more formal regressions results where we include a more comprehensive set of controls. Later in the paper, we also examine in Table 4 the correlation between the share of ARMs and the number of tax filers in different income brackets, showing that there is no systematic correlation, suggesting that there is not evidence of income targeting (at least after controlling for location fixed effects).
compute the fraction of loans that a bank lends as 5-1 and 7-1. We subsequently find a correlation of -0.34, suggesting that banks choose one or the other type of loan primarily to focus on (see Appendix Section A.3.1). We next use information on the distribution of bank deposits in each CBSA to measure their area of operations. We obtain this from the Federal Deposit Insurance Corporation’s Statement of Deposits. Using this data, we regress the share of a bank’s total deposits within each CBSA, over each year from 1993 to 2014, on bank-by-CBSA fixed effects. We recover an \( R \)-squared of 0.93, suggesting that banks tend to remain within their narrowly defined geographic areas, rather than frequently moving to strategically target new areas.

In the Appendix Section A.3.2, we also plot the distributions of FICO scores across different types of ARMs both pre and post the Great Recession. While individuals with 2-1 and 3-1 ARMs have lower average FICO scores than those with 5-1, 7-1, and 10-1 ARMs, the distribution of FICO scores among those with 5-1, 7-1, and 10-1 overlap almost entirely. The near overlap suggests that individuals undertaking these different types of loans look remarkably similar, at least with respect to FICO scores.\(^{28}\) We also show that our 5-1, 7-1, and 10-1 ARMs exhibit slightly larger FICO scores than their fixed rate mortgage (FRM) counterparts even though on average ARMs (including the non 5-1, 7-1, and 10-1 ARMs) have lower FICO scores than ARMs. Although we recognize that lenders look at soft information on top of FICO scores, we find it assuring that banks with different lending strategies are not also targeting systematically different types of borrowers. Di Maggio et al. (2017a) also present additional tests documenting the comparability of borrowers with 5-1 and 7-1 ARM loans.

3 Data and Measurement

3.1 Sources

*County Panel of Demographics.*—We access complete county demographic measurements from SocialExplorer, which is based on the Census Bureau’s American Community Survey (ACS) and Decennial Census. We specifically extract the following measures to produce semi-parametric controls: the fraction of individuals in different age brackets (0 to 18, 19-34, 35-64, and 65+),

\(^{28}\)Even if this were the case, selection into these ARMs would cause us to underestimate the effects of foreclosures. In particular, as Campbell (2013) discusses, “the preference for ARMs should be greatest among mortgage borrowers with increasing income who are buying large houses relative to their current income.” If we leverage variation in interest rate resets among counties that are on an upward trend, this will dilute the negative effects of foreclosures.
the fraction of individuals in different education brackets (no high school, only high school, some college, college, and post-graduate), the fraction that are male, married, and race (white and black), and total population. These capture time-varying shocks in the composition of individuals and tastes in a given area. We obtain these measures for 2000, 2005-2009, and 2010-2014.

**County-by-industry Panel of Employment and Earnings.**—Our main measure of employment and earnings comes from the Longitudinal Employer-Household Dynamics (LEHD), specifically the Quarterly Workforce Indicators (QWI), which is publicly accessible at an aggregated level from the Census Bureau website (http://lehd.ces.census.gov/data/). The LEHD covers over 95% of jobs in the U.S. and consists of a unique federal-state data sharing collaboration called the Local Employment Dynamics (LED) partnership. In partnership, all state agencies voluntarily submit quarterly data files from existing administrative records, which combine information from employers’ quarterly earnings reports that are required for state unemployment insurance agencies, the Quarterly Census of Employment and Wages, the Business Dynamics Statistics, and other demographic sources from the Census Bureau and Social Security Administration.

We aggregate to the county $\times$ firm size level and two-digit industry $\times$ county level, classifying industries as non-tradable and tradable in following Mian and Sufi (2014). Our first measure classifies an industry as tradable if it has net exports of at least $10,000 per worker or if the sum of exports and imports for the whole industry exceeds $500$ million. Under this definition, we use four-digit industry disaggregated data to compute the share of sub-sectors at a two-digit level that are classified as tradable. Agriculture has 25%, mining has 81%, and manufacturing has upwards of 76% (depending on NAICS 31, 32, or 33). All others have a share of zero with the exception of information, which has a share of 5%. Our second measure classifies an industry based on its geographical concentration based on the intuition that non-tradables firms are needed everywhere.

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29See Abowd et al. (2009) for details.

30One obvious alternative approach is to work at a more disaggregated level, i.e. three-digit industry $\times$ county. However, the LEHD restricts access to employment records for counties where there are too few of firms in a particular industry. Once we go down to a three-digit level, there are many more missing observations—nearly half. There are also many more zeros, which creates a truncation problem. In diagnostic exercises in Appendix Section A.2.6, we present our main results at a three-digit industry $\times$ county level, showing that our conclusion that tradables are more adversely affected actually changes. However, we investigated the source for the difference in detail by computing the difference of logged missing employment records in the tradables sector net of logged missing employment records in the non-tradables sector. We regressed that measure on logged foreclosures, conditional on controls, and recovered a coefficient of -0.05 ($p$-value = 0.00). In other words, foreclosures appear to be highly correlated with the presence of missing observations—even more so in counties with greater shares of tradables. We, therefore, opt to work at a two-digit industry $\times$ county level of aggregation since it reduces the number of zeros and missing observations.

31Only the first two change substantially to 6.25% and 60%, respectively, if employment shares are used to weight the share of tradables in sub-sectors.
and, therefore will tend to be more geographically dispersed (versus tradables having a national market that can concentrate in a particular location). The main sectors that are now considered tradable include: finance and insurance, information, and professional and technical services (although transportation and warehousing also have somewhat high geographic concentrations). We choose not to include these latter sectors under our baseline results, but doing so does not alter our conclusion that the tradables sector is more adversely affected by foreclosures.32

For a subset of our analysis, we also use licensed data from IRS tax returns (provided by Powerlytics) of all 27 million public and private businesses to quantify the impact of foreclosures on investment through advertising and rental expenditure proxies.33 We extract from information on total advertising and rental expenses, which we take as proxies for firm investment in our investigation of potential causal mechanisms behind our results.

*Loan-level Panel of Foreclosures and Characteristics.*—We license detailed, loan-level mortgage data from CoreLogic, which gathers the data from loan servicing companies. Based on comparisons of total loan counts in the CoreLogic data to figures of total outstanding loans from the Mortgage Bankers Association, we estimate that the CoreLogic data covers approximately 82% of the residential mortgage market in the United States. We consider all 5-1, 7-1 and 10-1 hybrid ARM loans—that is, ARM loans with initial fixed rates that then reset to floating rates after an interval between two and ten years, as well as balloon mortgages. This gives us a total set of 3,189,640 million unique hybrid and balloon mortgage loans between 2000 and 2014.

For each loan, we observe a vector of initial characteristics, giving information such as the contract type (hybrid ARM, balloon, etc.), the initial interest rate, and the schedule for interest rate resets and balloon payments. We also observe monthly performance updates, giving information on factors such a loan’s current interest rate and whether it has prepaid, been foreclosed upon, or is still current. Our data set covers a total of 158,674,405 such loan × month observations. An advantage of our data is that we focus on the universe of loans, rather than a subset (e.g., sub-prime loans), which recent literature shows is subject to selection problems (Ferreira and Gyourko, 2015; Albanesi et al., 2017). Appendix Section A.2 provides evidence on the significant variation in our

32The notion of geographic concentration is implemented through a Herfindahl index of employment. We find a correlation of 0.42 between the Herfindahl index and share of four-digit sub-sectors in a two-digit industry that are tradable. However, including finance, insurance, information, and professional / technical services does not alter our results. Doing so changes our gradient on foreclosures from -0.26 to -0.18 for tradables.

33We see, for example, these measures at a county × industry level for all line items on standard business tax forms. Many firms operation in multiple locations, and do not explicitly break out their tax line items by each different region in which they operate. We therefore focus on sole proprietorship, as they are most likely to operate just in the single county which contains their primary business address.
data across different periods, together with other summary statistics detailing differences between adjustable and fixed rate mortgages (e.g., borrower and other loan characteristics).\textsuperscript{34}

In addition to observing millions of loans for over a decade, an important feature of our data is the set of characteristics we observe about a loan. In particular, we classify foreclosures as occurring when a homeowner loses possession of their home, which contrasts with an approach in prior studies that classify it as such when a loan first enters the foreclosure process. As Herkenhoff and Ohanian (2015) and Pence (2006) have pointed out, foreclosure can be a long drawn-out process that may take months, or even years in some cases, before the actual home loss takes place. Because we also observe a larger universe of approximately 170 million mortgages (ARM and fixed rate), we compute a measure of mortgage payments due (across all loans, totaling over five billion loan \times month observations) for each county \times quarter. Since Di Maggio et al. (2017a) show that interest rate resets affect consumers’ disposable incomes, we total monthly payments due to help control any potential mechanical effect that the rate resets have on labor market outcomes through a disposable income channel.

\textit{County and Zip-code Panel of Housing Prices.}—We use the Federal Housing Agency’s (FHAs) house price index (normalized to 2000 as the base year).\textsuperscript{35} The HPI captures movements in the price of single-family housing prices that is constructed from repeat sales or refinancings on the same properties specifically on the set of mortgages purchased or securitized by Fannie Mae or Freddie Mac. We use it as an alternative to, for example, Zillow’s median housing price per square foot since the FHA data is more comprehensive; Zillow only covers “larger” counties. While we recognize that it may vary with respect to other measures of housing prices, it has a high correlation with, for example, the Zillow indices (above 90%), and our statistical estimates are robust to using the Zillow series (on a subset of counties).

\textit{Small Business Administration and Other Bank Lending Data.}—We access a database containing 1.4 million loans made by banks to small businesses through the US Small Business Administration’s (SBA) 7(a) and 504 loan programs. The former allows banks to make loans to small businesses and to purchase partial (up to 85%) default insurance on those loans from the SBA.\textsuperscript{36} The latter involves a partnership with a Certified Development Company (a nonprofit set to contribute to the economic development of its community) to work with the SBA and private-sector lenders, providing a senior lien covering at least 50% of the project cost, a loan from a CDC

\textsuperscript{34}See Mayer et al. (2009) for an additional discussion of these differences.
\textsuperscript{36}https://www.sba.gov/category/lender-navigation/sba-loan-programs/7a-loan-programs
(backed by the SBA) with a junior lien covering up to 40% of total costs, and a contribution from the borrower of at least 10% equity.\textsuperscript{37} See Appendix Section A.2 for further details. To isolate the effects of local foreclosures on the supply of credit, we also draw on capital levels (e.g., assets) and lending data from the Call Reports. These are comprehensive regulatory disclosures made by banks, which are made available to the public. The data is made available through the Federal Financial Institutions Examination Council (FFIEC). We focus primarily on the set of local banks, which are most likely to experience a decline in their balance sheets in response to local foreclosures. We also combine these data with the Federal Reserve’s Senior Loan Officer Survey.

Gallup Daily Polling Repeated Cross-section.—To understand how foreclosures impact local investment, we draw on data newly licensed from Gallup, Inc. to Stanford University. Gallup is the United States’ premier polling service and conducts daily surveys of 1,000 U.S. adults on various political, economic, and well-being topics. In particular, 200 Gallup interviewers conduct computer-assisted telephone interviews with randomly sampled respondents (age 18 or over) from all 50 states and the District of Columbia. Detailed location data, such as the zip-code and metro area, is also available with corresponding sample weights. Gallup also routinely incorporates questions on specific topics, such as voting intentions and perceptions of current events.

Gallup’s polling relies on live, not automated, interviews with dual-frame sampling (including random-digit-dial [RDD]) landline and wireless phone sampling. Half of the respondents receive the “well-being track” version (with a 9% survey response) of the survey questions, whereas the other half receives the “politics and economy track” (with a 12% survey response). The two surveys contain different topical questions, but both contain the same identifying demographic information. Gallup also conducts the survey in Spanish to record replies from those Spanish speakers who do not also speak English. The sampling methodology also uses a three-call design to reach respondents who do not pick up on the original attempt.

The two main sampling questions that we use are: (i) “How would you rate economic conditions in this country today: as excellent, good, only fair, or poor?”, and (ii) “Right now, do you think that economic conditions in this country, as a whole, are getting better or getting worse?”. Later, we explore comparisons between these two measures and several measures of economic uncertainty, including: the volatility index (VIX), the Baker et al. (2016) index of economic policy uncertainty, and the Baker and Wurgler (2006) index of investor sentiment. Although both questions are worded about the national economy, there is growing evidence that most individuals respond to

\textsuperscript{37}https://www.sba.gov/offices/headquarters/oca/resources/5991
local information.\textsuperscript{38,39} In Appendix Section A.2, we provide descriptive evidence on the dispersion of beliefs across metropolitan areas over time, which shows that perceptions vary even in response to the same national conditions.

3.2 The Geographic Incidence of Foreclosures

While county variation in the frequency and timing of foreclosures is an important source of variation, our empirical strategy exploits a feature of the institutional environment that precipitated the financial crisis. In particular, we leverage the fact that different counties had different proportions of different types of adjustable rate mortgages (ARMs). ARMs are unique in that lenders used low ("teaser") rates to attract homeowners, but the interest rate would discontinuously reset up or down after a point in time (e.g., five years after the origination of the loan). The changes occurred because the rates after reset were tied to certain common interest rate metrics, such as treasury rates or LIBOR, plus an additional spread. If the reference rate increased (decreased) significantly since loan origination, the loan’s rate would reset up (down). These abrupt changes in interest rates are associated with discontinuous changes in foreclosure probabilities.

Before implementing our empirical strategy, however, we document the significant heterogeneity in the geographic dispersion and timing of these loan origination and interest rate shocks. Different lending companies used different strategies, and these companies were clustered in different locations of the United States. Figure 5 begins by plotting the dispersion in 5-1, 7-1, and 10-1 ARM originations across time and geography throughout the United States. We compute the mean time to reset for loans originated in each county and year between 2002 and 2007. A county with mostly 5-1 ARM loans originated in a given year will thus have a mean close to 5 and will appear more blue in the figure; a county with mostly 7-1 ARM loans originated will have a mean closer to seven and will appear more orange; a county with more 10-1 ARMs will appear dark red.

These plots provide an illustration of the relative composition of loan types in each geography both within and across time. The amount of dispersion is striking. Throughout the map, counties are checker-boarded red and blue in a seemingly random pattern. This dispersion is evident

\textsuperscript{38}http://www.pewinternet.org/2012/09/26/how-people-get-local-news-and-information-in-different-communities/
\textsuperscript{39}http://www.pewinternet.org/2012/04/12/72-of-americans-follow-local-news-closely/

Our primary rationale for using this data is that the measures vary by location. While we could theoretically construct a measure of equity shocks that capture volatility, we would have to generate a local exposure to interact it with. However, since many publicly traded companies are consolidated in larger metropolitan areas, we would lose the bulk of our sample and sacrifice external validity.
temporally, as well as spatially. For example, many counties that are dark red in the 2003 plot turn to dark blue in the 2005 plot, and vice versa. There are some additional macro trends apparent in the figure too. The balance tends to shift from 5-1 loans to 7-1 and 10-1 loans between 2002 and 2007. Yet, to the extent these trends are non-random, these are precisely what our time fixed effects will be able to control for. What the plots vividly depict is that there remains a strikingly large amount of temporal and spatial variation in the timing of which types of loans are made in which counties, which is the variation we seek to exploit in our empirical strategy.

[INSERT FIGURE 5]

Given the nature of these different ARMs, the geographic heterogeneity induces heterogeneity in both the timing and magnitude of loan interest rate resets. Using all ARM loans that experience an interest rate reset within a given month, Figure 6 plots the relative interest rate changes starting from 2006:Q1 until 2009:Q1, which precipitated the apex of the surge in foreclosures. We construct the plot in the following way. We first sum across all the interest rate changes on individual loans in a given county. We subsequently identify, for each county, the quarter that had the highest net interest rate delta. We finally assign each county-quarter observation a rank of quarters across the 16 quarters (2006:Q1 to 2009:Q1) to focus the attention on the within-county intensity of interest rate changes. The highest quarter is assigned a value of 16 and the lowest quarter is assigned a value of 1. This focus on within-county, rather than between county, variation mirrors the geographic fixed effects we use in our empirical specifications. While many interest rate spikes took place in 2007 and 2008, Figure 6 demonstrates that there is still considerable heterogeneity in 2006 and 2009, especially in the mid-West. Appendix Section A.3.4 documents the evolution of interest rates for different vintages of ARM loans.

[INSERT FIGURE 6]

Motivated by these geographic differences, Figure 7 illustrates that there is also considerable heterogeneity in the timing of when counties experience foreclosures. Just as in Figure 6, the darkness of the shading is based on a within-county comparison that takes the total number of

\[\text{For example, if a county had four loans that experienced an interest rate reset in a given period equal to +1, +3, +2, and -1, then the relative (“delta”) net interest rate would be +5.}\]

\[\text{As we discuss above, we restrict the set of loans only to those adjustable rate mortgages that we use to identify interest rate resets. Moreover, once the foreclosure process has finished on a loan, it is removed from the sample. In this sense, only 5-1, 7-1, and 10-1 ARM loans that are going into foreclosure or finishing the process are considered in the sample.}\]
foreclosures in a given quarter and ranks it relative to foreclosures in each of the other quarters between 2006 and 2009. To reiterate, the shading has nothing to do with the absolute number of foreclosures a county experiences, only with the relative timing of when the bulk of a county’s foreclosures occur. In this sense, the darkest shading implies that a county experienced its most adverse foreclosure shock in a given quarter, whereas the lightest shading implies the county experienced low (if any) foreclosures. The fact that foreclosure shocks are relatively staggered within-county and distributed across locations provides a great deal of plausibly exogenous variation. Given our geographic fixed effects and instrumental variable specification, our identification strategy is based entirely on exploiting heterogeneity in when counties experience foreclosures, and not on the absolute numbers of foreclosures in different counties.

[INSERT FIGURE 7]

### 3.3 Interest Rate Resets and Foreclosure Probabilities

What are the determinants of foreclosure? First, an individual might be laid off if, for example, local demand declines and their company no longer needs as many employees. Given that layoffs are associated with significant and persistent declines in income (Jacobson et al., 1993; Couch and Placzek, 2010), especially during the Great Recession (Davis and von Wachter, 2011), these individuals might have been especially likely to default on their loans and eventually experience foreclosure. Second, housing price declines might make an individual more likely to strategically default and escape their underwater investment. However, Demyanyk et al. (2017) show that what empirically matters more is the individual’s outside option, rather than the decline in their home equity. Third, subprime loans were uniquely more likely to experience foreclosure arguably because of negative selection among borrowers into these contracts (Mian and Sufi, 2009) and the falsification of metrics used to gauge credit quality (Keys et al., 2012). For example, focusing on the set of subprime borrowers, Palmer (2016) finds that changes in borrower and loan characteristics account for 40% of the difference in default rates between 2003 to 2007 with the remaining variation being driven by housing price declines. Fourth, among the set of borrowers with adjustable rate mortgages (ARMs), discontinuous changes in their interest rate make it harder or easier to pay down their mortgage depending on the direction of the change. For example, if an individual originated a 5-1 ARM loan with a 1% teaser rate in 2002, but subsequently experiences at 2%
increase following an interest rate reset in 2007, then they may be more likely to go into default.\textsuperscript{42}

We focus on providing evidence of the fourth determinant of foreclosure since we will use it as a source of quasi-experimental variation in our empirical estimation. Before providing more formal evidence, we begin by non-parametrically plotting in Figure 8 the foreclosure probability by month since loan origination separately for different vintages of loans and ARMs. We distinguish between interest rate increases and decreases since they may have asymmetric effects on the probability of foreclosure.\textsuperscript{43} Consider, for instance, the top left panel. For these 5-1 ARMs originated in 2002, the plot illustrates that the foreclosure probability—given by the fraction of individuals who are foreclosed upon in a given month—is constant up until the five-year mark (60 months) when the foreclosure probability spikes from roughly 0.01\% to 0.05\% and all the way up to 0.15\% in the following months. The pattern in foreclosure probabilities resembles the pattern in interest rates (see Appendix Section A.3.4): precisely when interest rates spike, foreclosure probabilities rise. After the original reset date, the loan remains at an elevated risk of default, though this rate consistently declines through the remainder of the life of the loan, reaching, for example, its pre-reset risk levels. In contrast, the bottom right panel illustrates that there was a steep decline in foreclosure probabilities following the decline in the interest rate.

While Figure 8 provides graphical evidence of a discontinuity in the probability of foreclosure following an interest rate reset, and we have shown that there exists significant variation in the share of ARMs across locations (see Appendix Section A.3.1), we now provide evidence that the discontinuity is robust to a range of other controlling individual and local covariates by estimating a loan-level model restricted to the set of ARMs

\[ f_{ict} = \beta X_{it} + \phi D_{ct} + \sum_k \gamma_k l_{ikt} + \zeta \Delta r_{it} + \sum_k \rho_k (l_{ikt} \times \Delta r_{it}) + \epsilon_{ict} \]  

(1)

where $f$ denotes an indicator for whether the loan is in foreclosure, $X$ denotes a vector of borrower characteristics (e.g., FICO score), $D$ denotes a vector of county controls (e.g., housing prices and share of subprime borrowers), $l_k$ denotes the $k$-th type of ARM loan (e.g., 5-1, 7-1,

\textsuperscript{42}Edmiston and Zalneraitis (2007) focus on the latter three main factors behind the recent surge in foreclosures.

\textsuperscript{43}Although our application of interest rate resets is similar to Fuster and Willen (2017), Di Maggio et al. (2017a), and Di Maggio et al. (2017b), we leverage both interest rate resets up and down, which provides us with greater variation in the probability of foreclosure within a county. Gupta (2016) uses interest rate resets in a similar setting as ours, but focuses on different outcomes at a different layer of aggregation. Eberly and Krishnamurthy (2014) also discuss interest rate reset declines as an effective mechanism for reducing defaults.
or 10-1 normalized to 5-1), and $\Delta r$ denotes the interest rate reset (new interest rate net of the interest rate at origination). Our primary coefficients of interest from Equation 1 are $\zeta$ and $\rho^k$ since they characterize the probability of foreclosure following interest rate resets for the different types of loans after controlling for other potential determinants of foreclosure.

Before turning towards our results, we first discuss an important potential concern with our estimation of Equation 1. Given that foreclosure delay is common and behaves as a source for additional credit (Herkenhoff and Ohanian, 2015; Gerardi et al., forthcoming), especially in states with judicial status laws (Pence, 2006), some loans may not enter foreclosure immediately following the interest rate reset. First, the severity of foreclosure delay will bias us against finding a first-stage correlation, simply making our instrument weaker. As we will show later, our instrument gives us a first-stage $F$-statistic well above the rule of thumb of 10 (Stock and Yogo, 2005). Second, we can replicate all our results by estimating Equation 1 separately for states with and without judicial status laws, allowing for heterogeneous relationships. Although this may introduce the potential for endogeneity, and therefore it is simply a robustness exercise, Mian et al. (2015) show that judicial status is plausibly exogenous. Third, we can also replicate all our results by modifying Equation 1 to allow for a continued effect of interest rate adjustments following the initial reset date. These provide stronger predictions, but nearly identical second-stage results in our subsequent empirical application.

In addition to the variation in foreclosures induced by these interest rate resets, Table 12 in Appendix Section A.3.5 explores how foreclosures on ARM borrowers generate spillovers into the broader FRM market for loans. Consistent with Agarwal et al. (2017) and Gupta (2016), we find that a 10% rise in foreclosures on ARM borrowers is associated with a large 5.8% rise in foreclosures on FRM borrowers in a zipcode. The elasticity is robust to controlling for county × year fixed effects and zipcode employment and establishment counts. As a placebo, we also show that these spillovers are larger in counties with greater proportions of ARM borrowers. These results suggest that ARM resets can set in motion a chain of events that make foreclosures on other loans more likely. While one likely source of foreclosure spillovers is a housing price channel (Anenberg and Kung, 2014), there at least two other possible reasons: (i) exposure to others nearby who have defaulted increases one’s own propensity to default (Guiso et al., 2013), and (ii) local foreclosures affect the perception of risk by banks, affecting their interest rates and loan

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44We keep loans in the dataset up until the foreclosure process ends (i.e., an individual is forced to leave their home). We keep, for example, loans where the foreclosure process has started.
volume (Sirignano et al., 2016). These factors explain the strong first-stage we will use.

4 Research Design

4.1 Empirical Specification

Our primary focus is a statistical model that relates local labor market outcomes with foreclosures

\[ y_{jct} = \gamma f_{ct} + f(X_{ct}, \beta) + \eta_j + \psi_c + \lambda_t + \epsilon_{jct} \]  

(2)

where \( y_{jct} \) denotes the industry-by-county outcome variable (e.g., logged employment, hiring, or job turnover) in industry \( j \), county \( c \), and period \( t \), \( f \) denotes logged (flow) foreclosures, \( f(X, \beta) \) denotes a flexible semi-parametric vector of demographic controls over the location \( j \), and \( \eta, \psi, \lambda \) denote fixed effects on two-digit industry, county, and time.\(^{45} \) Although our outcome varies at the industry \( \times \) county level, we follow Bertrand et al. (2004) in clustering our standard errors at the county level, allowing errors to be arbitrarily correlated at the broadest level of aggregation. We do not include weights since we have effectively the population of counties (Solon et al., 2015).

In practice, we measure \( f(X, \beta) \) by including bins of the fraction of households in a county falling within different age, education, gender, and race brackets, as well as logged county population. We also include measures of loan volume and credit deposits to control for the fact that certain counties may have had a greater expansion of credit than others, thereby affecting employment and/or consumption outcomes (Mian et al., 2013). We have also experimented with including county housing price and housing price growth as a control to address the concern that declining housing prices were a determinant of foreclosures (Edmiston and Zalneraitis, 2007), which therefore could affect our outcome variable through housing prices. However, for our baseline estimates, we omit housing price controls to avoid “over controlling” (Angrist and Pischke, 2009).\(^{46} \)

\(^{45}\) Whether foreclosures should be measured as foreclosures per open mortgage and employment as employment per population is theoretically ambiguous. While our results are quite robust to either of these other measures, our primary rationale for using a log-log model is that it fits the data well and delivers an intuitive interpretation (an elasticity) of our coefficient on foreclosures. We later examine the potential for non-linearities by interacting intensity bins from the financial crisis with contemporaneous foreclosures.

\(^{46}\) What matters is whether our instrument (interest rate resets) is correlated with housing prices through a channel other than foreclosures. Using evidence from a regression discontinuity, Bhutta and Ringo (2017) show this is unlikely: the reduction in the FHA’s mortgage insurance premium (MIP) led to a rise in home purchases, but no decline in housing prices. Including housing price controls may be important for mitigating the dynamic selection problem that we discuss below—that banks delay foreclosure in areas where housing prices drop significantly since recognizing the market value of these properties would have adversely affected their balance sheets. That our
4.2 Identification Strategy

The most obvious form of endogeneity in Equation 2 arises from cross-sectional differences across locations. For example, more productive counties and industries will tend to have higher employment and churn. In turn, individuals will tend to be wealthier and more mobile, reducing the probability of being foreclosed upon. In this sense, ignoring cross-sectional unobserved heterogeneity will produce downwards bias on \( \gamma \), making it more negative than the truth.

However, these concerns are easily addressed through our inclusion of demographic controls and fixed effects. The more pressing sources of bias are inherently time-varying. We focus on two. The first endogeneity problem arises from reverse causality. Drops in employment may lead to foreclosures since a worker getting unemployed means that their income plummets, eroding their ability to stay solvent and pay off the loan. Recent work by Hsu et al. (forthcoming), for example, has shown that unemployment insurance played an important role in stabilizing housing markets by providing liquidity to laid off workers. Failing to account for reverse causality will produce downwards biased estimates, overestimating the negative impact due to foreclosures.

The second endogeneity problem arises from the presence of two phenomena that led a delay in foreclosures until recoveries began taking place. First, bank accounting practices, and payment arrangements for mortgage servicers, created incentives for each type of entity to delay foreclosures in certain circumstances; we refer to this as a “dynamic selection effect”. Second, the glut of foreclosures during the crisis overwhelmed administrative systems of banks, servicers, and local governmental authorities, causing significant delays in processing foreclosures that did not resolve themselves until the worst of the crisis had passed; we refer to this as a “backlogging effect”. We explore both of these channels below.

Banks, particularly those in precarious financial conditions, have strong incentives to delay and minimize the losses they need to recognize on their accounting books.\(^{47}\) When a bank forecloses on a mortgage, it must take physical possession of the underlying property and value that asset at its market value, rather than keeping it simply on its books under the “loans outstanding” category in the hope that it will become current again.\(^{48}\) If the market value of the foreclosed property is below the book value assigned to the loan, this can mean taking a large loss on the bank’s balance.

\(^{47}\)https://www.bloomberg.com/view/articles/2014-02-26/banks-prefer-losses-they-don-t-have-to-talk-about

sheet. This creates an incentive for banks, particularly those in precarious financial conditions, to delay foreclosures until after economic activity in a region begins to improve.\textsuperscript{49} In fact, part of the Home Affordable Modification Program (HAMP) aid was specifically designed to help banks avoid recognizing their losses immediately.\textsuperscript{50} These incentives of banks were compounded by those of mortgage servicers. Many servicers also owned interests in second lien mortgages on the primary mortgages they were servicing. If the first lien mortgage were foreclosed upon, the second lien would almost certainly receive no value in the foreclosure sale, meaning that mortgage servicers would at times delay foreclosure in the hopes of receiving more payments on their second lien interests and in continuing to receive mortgage servicing fees.\textsuperscript{51, 52}

An additional explanation behind emerges from administrative backlogs at numerous points in the foreclosure process. Many mortgage servicers encountered significant difficulties due to missing or fraudulent documentation accompany mortgages (Calem et al., 2016). Apart from this, many servicers simply lacked the personnel and experience to handle a large number of foreclosures in a short amount of time. Local governments, which are also required to act as part of the foreclosure process, likewise often lacked capacity to handle the unprecedented number of foreclosures.\textsuperscript{53} It was only after the peak of the economic crises that these servicers and local governments expanded their administrative capacity to process more foreclosures and worked their way through the initial glut of foreclosures in their systems. Administrative backlog effects of these sorts, combined with strategically delayed foreclosures, will cause significant numbers of foreclosures to be delayed until local economic conditions begin improving, producing upwards biased estimates by underestimating the negative impact due to foreclosures.

\textsuperscript{50}That HAMP was designed to allow banks to delay losses from foreclosures was a conclusion reached by the Special Inspector General of the TARP program. See http://billmoyers.com/content/book-excerpt-neil-barofskys-bailout/2/.
\textsuperscript{52}To quantitatively test whether worse local economic conditions create conditions that lead (through lender incentives and administrative backlog) to longer delays before foreclosure, we perform the following test. We consider mortgages that have already become seriously delinquent (90+ days delinquent) and predict how many months will elapse between this delinquency and eventual foreclosure. Specifically, for each county × quarter observation in our dataset, we consider all mortgages that become seriously delinquent and calculate the mean time (in months) between this delinquency and eventual foreclosure. We then regress this on employment growth measured in each county × quarter, producing a coefficient of -13.55 (p-value = 0.00). In other words, better (worse) economic conditions are associated with mortgages taking significantly less (longer) time to move from serious delinquency to foreclosure.
\textsuperscript{53}http://www.creditslips.org/creditslips/2012/11/where-are-the-foreclosures.html
4.2.1 Strategy # 1: A Loan-level Model

Our primary solution is to exploit a unique feature of the design of adjustable rate mortgage (ARM) loans and how they affect foreclosure probabilities. Many hybrid ARMs were initially offered to individuals with “teaser” rates for an initial period. The rates on these loans, however, would frequently spike after the first reset date such that they were in excess of the prevailing interest rate (e.g., LIBOR) by as much as 8% or more. Like our estimation of Equation 1, we restrict our sample to individuals with 5-1, 7-1, and 10-1 ARMs because these borrowers are relatively homogeneous in their characteristics; see Appendix Section A.3.2 for a comparison of FICO scores across each loan category, displaying almost identical overlap in their distributions. Homogeneity among borrowers helps us mitigate concerns about selection into these ARM loans, focusing instead on the historical variation that led banks to adopt different lending practices.

Motivated by our earlier results from Equation 1, we now estimate a variant of it through

$$f_{ict} = \alpha + \sum_k \gamma^k l^k_{it} + \zeta \Delta r_{it} + \sum_k \rho^k (l^k_{it} \times \Delta r_{it}) + \epsilon_{ict}$$ (3)

where the difference from Equation 1 is that we omit borrower and location characteristics since we only want to recover variation in foreclosure that is predicted by the timing and intensity of ARM resets. After fitting these regressions to 170 million loan-month observations, we recover predicted foreclosure probabilities for each observation. Since the occurrence of a foreclosure is a binary outcome, its expectation equals its probability. We sum over the loans in a given county to obtain predicted numbers of foreclosures in that county for each period during our study, denoted $Z_{ct}^{SIM} \equiv P(f_{ct})$. We use these predictions to instrument actual foreclosures through 2SLS.

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54 Gorton (2008) argues that these loans were designed to make it impossible for borrowers to afford payments after the reset date so that lenders could decide whether to refinance the loans or foreclosure on the property. Ben Bernanke, in a speech at the Independent Community Bankers of America Annual Convention in March 2008 comments: “In the past, subprime borrowers were often able to avoid resets by refinancing, but currently that avenue is largely closed. Borrowers are hampered not only by their lack of equity but also by the tighter credit conditions in mortgage markets. New securitizations of nonprime mortgages have virtually halted, and commercial banks have tightened their standards, especially for riskier mortgages. Indeed, the available evidence suggests that private lenders are originating few nonprime loans at any terms.”

55 One concern with this approach is that our predicted resets might be confounded by an individual’s decision to refinance their loan to avoid foreclosure. First, to the extent that individuals refinance rather than foreclosure, our first-stage correlation will simply be weaker. Second, as Fuster and Willen (2017) discuss, all of the interest rate resets that came due after 2008 were net resets down, meaning that borrowers did not have an incentive to refinance since they were getting a “better deal” by staying with their new rate. Third, Bucks and Pence (2008) use the Survey of Consumer Finances, showing that many borrowers with ARMs underestimate or do not know how much their interest rates are likely to change before the resets kick in.
\[ f_{ct} = f(X_{ct}, \beta) + \pi Z_{ct}^{SIM} + \eta_j + \psi_c + \lambda_t + \epsilon_{ict} \]
\[ y_{ict} = f(X_{ct}, \beta) + \gamma \tilde{f}_{ct} + \eta_j + \psi_c + \lambda_t + \epsilon_{ict} \]

where \( \tilde{f}_{ct} \) denotes the predicted foreclosures based on the ARM resets from our reset instrument. Importantly, our estimates do not use borrower characteristics (e.g., FICO scores) or geographic attributes (e.g., college attainment) since our goal is to capture only the variation in foreclosures driven by these idiosyncratic reset shocks. Our first-stage correlation is driven by the discontinuous change in the probability of foreclosures following interest rate resets (see Figure 8).\(^{56}\)

To guarantee that the timing of these discontinuous jumps are not driven by time-varying unobservables that co-move with employment (such as macro interest rates), we control for quarterly county mortgage payments over all loans (from the CoreLogic dataset), which removes the potentially mechanical effect of interest rate resets on disposable income (Di Maggio et al., 2017a), which could affect search intensity (and thus employment) (Cohen-Cole et al., 2016).\(^{57}\)

Our identification strategy is related to several recent contributions, e.g., Gupta (2016) who examines the impact of foreclosures on housing price declines, Fuster and Willen (2017) who examine the impact of loan size on mortgage default, Di Maggio et al. (2017a) who examine the impact of interest rate changes on consumption and voluntary deleveraging, and Cloyne et al. (2017) who use similar staggered refinancing duration to identify the effects of housing on borrowing in the United Kingdom. Our paper (developed concurrently with these) also contains several novel features. First, we use the entire universe of CoreLogic data, containing 170 million loans, of which 3.2 million ARM loans are resetting.\(^{58}\) Using only a subset of loans (even if randomly chosen)—especially if the subset focuses more heavily on sub-prime borrowers in predicting default—can create significant bias (Ferreira and Gyourko, 2015; Albanesi et al., 2017). Second, we estimate a loan-level model that extracts only the variation in foreclosure predicted from ARM resets, whereas past papers have focused on interest rate changes as the primary independent

\(^{56}\)See Di Maggio et al. (2017a) and Gupta (2016) for recent applications that use a variant of our approach to identify the causal effect of foreclosures on disposable income and housing price discounts, respectively.

\(^{57}\)A potential concern remains that shocks to housing prices may affect the incentive to default. If this were true, we may also need to instrument for housing prices to overcome their simultaneous endogeneity. While we are already controlling for housing prices, Gerardi et al. (forthcoming) use the Panel Study of Income Dynamics (PSID) to show that there is only a limited scope for strategic default.

\(^{58}\)In contrast, Fuster and Willen (2017) use a sample of 221,000 loans from January 1 2005 to June 30 2006, Gupta (2016) and Di Maggio et al. (2017a) both use the Blackbox sample which containing 22 million loans (Di Maggio et al. (2017a) focus primarily on 5-1 ARM loans originated between 2005 and 2007). Moreover, Gupta (2016) has roughly 682,000 resetting ARM loans and roughly 54 counties, whereas we cover 3.2 million resetting ARMs and nearly 2000 counties with over 100 respondents from our data.
variable of interest. Third, we show how they can be used to construct a Bartik-like measure that exploits a county’s exposure to banks that exhibited more versus fewer resets; we examine this next.

4.2.2 Strategy # 2: A Bartik-like Measure

Although we have presented evidence on the quasi-experimental variation that gave rise to dispersion in ARMs across space and time, two possible concerns remain. First, although we show later that interest rate resets are not correlated with future income growth, individual beliefs about the future could still be correlated with interest rate resets (e.g., banks strategically target borrowers with different types of loans in a way that is correlated with local employment growth). Second, Fuster and Willen (2017) discuss the fact that better borrowers might be more likely to refinance their mortgages, thereby avoiding abrupt interest rate changes in a way that is correlated with local county fundamentals (see Appendix Section A.5.7). We now introduce an additional identification strategy that overcomes these concerns by constructing an alternative Bartik-like instrument for foreclosures based on the interaction between county exposures to different banks prior to the Recession and the bank’s national interest rate resets on ARMs.

Given a base year $t_0$, we can compute a county’s exposure by taking the ratio of bank $i$’s $t_0$ loan volume in county $c$, denoted $v_{i,c,t_0}$, and county $c$’s $t_0$ loan volume, denoted $v_{c,2006}$. Given the county exposure in the baseline period $t_0$, then the Bartik-like instrument is given by

$$Z_{ct}^{BARTIK} = \sum_i \left[ \left( \frac{v_{i,c,t_0}}{v_{c,t_0}} \right) \times \Delta r_{it} \right]$$

where $\Delta r_{it}$ denotes bank $i$’s average interest rate resets in their national portfolio. Equation 5, therefore, identifies the causal effect of foreclosures on local outcomes by exploiting plausibly exogenous variation in the exposure a county had to a particular bank prior to the Great Recession. Since we fix the baseline exposure to $t_0$, counties will experience a greater shock if they have a greater proportion of banks that are experiencing interest rate resets up or down on their ARM mortgages. Our approach shares many features of the identification strategies in Agarwal et al. (2017) and Mondragon (2015).
4.3 Discussion of Relevance and Validity

Given that our first identification strategy is based off of the heterogeneous intensity and staggered timing of interest rate resets on ARMs, and our second identification strategy is based on the heterogeneous exposure of counties to banks with these different loan portfolios, a natural question is the underlying source of this variation leading up to the recession. The main concern is that the variation is merely a function of economic shocks that led to the expansion of banks and particular types of lending strategies in some areas over others. While we will implement exercises that directly gauge the plausibility of our exclusion restriction, we are exploiting the geographical concentration and subsequent expansion of banks around their initial hubs prior to the recession.

The 1980s and 1990s experienced a significant amount of banking deregulation, culminating in the 1990 Interstate Banking and Branching Efficiency Act (Kroszner and Strahan, 2014). Through a series of legislation, out-of-state banks were allowed to enter new markets and intra-state branching restrictions were relaxed. The expansion of originally concentrated banks into new areas led to a causal rise in credit (Favara and Imbs, 2015). As we discussed earlier, banks had different lending strategies, which meant that consumers exposed to these banks based on their location were offered different types of loan packages. In this sense, the source of the dispersion in our ARMs and their reset times is based off of this plausibly exogenous historical variation, which has been exploited in several recent papers (Favara and Imbs, 2015; Mian et al., 2017). Unlike the exclusion restriction, which is inherently untestable, our baseline specifications that follow include the first-stage $F$-statistic, which is well above the rule of thumb from Stock and Yogo (2005).

5 Quantitative Estimates

5.1 Main Results

We begin by validating the well-known result that foreclosures are associated with housing price declines. We estimate Equation 2 with both logged housing prices and the annual growth rate in housing prices as our outcome variables. We find that foreclosures are robustly negatively

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59Kroszner and Strahan (1999) discuss the political economy forces that led to this deregulation.

60Appendix Section A.4.2 plots both the residualized correlations in the pooled sample and the correlation between residualized foreclosures and our residualized instruments separately by year. The correlations are quite strong throughout, suggesting that our identifying variation is not coming from a single period and that the variation is not truncated after the Great Recession.
correlated both housing price levels and year-to-year growth rates. In our least squares specification (columns 1 and 5), we find that a 10% rise in foreclosures is associated with a 0.63% decline in housing prices and a 0.29 percentage point (pp) decline in housing price growth at a zipcode-level. However, under our preferred instrumental variables specification (columns 3 and 7), we find that a corresponding increase in foreclosures is associated with a 0.46% decline in housing prices and 0.82pp decline in housing price growth. These elasticities are identified by interest rate resets among ARM borrowers who experience a discontinuous change in the probability of foreclosure; the estimates are also robust to controlling for all shocks common to the same zipcode within a state over time (i.e., state × year fixed effects).

[INSERT TABLE 1]

Appendix Section A.5.1 examines several additional diagnostics. First, Figure 33 examines heterogeneity in the elasticity across zipcodes with different employment shares in the non-tradable sector. Consistent with the intuition coming from Mian and Sufi (2014) and Adelino et al. (2015b), areas with greater employment shares in non-tradables experience a greater decline in response to foreclosure shocks—nearly double when comparing the top versus bottom of the employment share distribution. Second, to make our estimates more comparable to those from Mian et al. (2015), Table 14 replicates our estimates using foreclosures per open mortgage. We find stronger elasticities in comparison with Mian et al. (2015) largely because judicial status laws produce different incentives for banks to strategically foreclose on seriously delinquent homeowners.

We now turn towards our main estimates of Equation 2 in Table 2 where we examine how foreclosures affect the labor market as defined by employment, hires, and job turnover. We begin with our employment results, which are displayed in Panel A. Beginning with the OLS estimator in column 1, we find that a 10% rise in foreclosures is associated with a 0.41% decline in employment. However, once we add fixed effects on county, industry, and time, however, the estimated gradient declines considerably in magnitude to a corresponding 0.11% decline. The decline in magnitude of the fixed effects estimator emerges from the fact that, while it helps mitigate the downwards bias emerging from unobserved heterogeneity—that areas with more foreclosures likely differ in other negative and unobservable ways—it does nothing to mitigate dynamic selection—that banks in areas with negative labor and housing market conditions are more likely to defer foreclosing on borrowers until their economy improves. Dynamic selection produces upwards bias in our setting.

We subsequently turn towards our baseline result in column 3, which instruments realized foreclosures with those predicted based on ARM interest rate resets. We find that a 10% rise in
foreclosures is associated with a 0.77% decline in employment. Our \( F \)-statistic of roughly 95 is well above the “rule of thumb” suggested by Stock and Yogo (2005), suggesting that our estimates are not driven by a weak first-stage. Moreover, as we discussed earlier, as long as the timing of these resets, which are contractually fixed years in advance, is uncorrelated with other contemporaneous factors, we have a window of variation that allows us to compare counties that just experienced an interest rate shock (and thus differences in foreclosures) with those that have yet to experience it. Motivated by Mian and Sufi (2014) and Dobbie and Goldsmith-Pinkham (2015) that the decline in household net worth led to a decline in disposable income (see Appendix Section A.4.1 for a replication of Mian and Sufi (2014)), column 4 restricts the sample to the set of non-tradables firms and finds that a 10% rise in foreclosures is associated with a 1.01% decline in employment.\textsuperscript{61} Column 5 subsequently restricts the sample to 2007-2014, producing a similar elasticity.

We now explore two potential concerns. The first concern is that interest rate resets might affect labor market outcomes through a disposable income channel. One way this could happen is by altering individuals’ search intensity by easing their credit constraints (Cohen-Cole et al., 2016), which could affect labor supply. Another way this could happen is by raising consumption expenditures (Di Maggio et al., 2017a), which could raise local demand. While we are already controlling for housing prices and mortgage payments over time within each county, which help control for correlates of local demand, we rule out the possibility that unobserved income effects are driving our results by implementing three exercises. First, Table 18 in Appendix Section A.5.5 uses the American Time Use Survey (ATUS) to show that interest rate resets are not correlated with individual time use data, suggesting that these disposable income shocks do not affect search intensity or labor supply. Second, Figure 42 in Appendix Section A.5.5 uses the Bureau of Economic Analysis (BEA) state consumption data to show that the only aggregate consumption category that responds to interest rate resets are expenditures on housing and utilities, suggesting that the main way these disposable income shocks affect consumption is by helping borrowers on these ARMs deleverage from high levels of debt.

The second concern is the fact that our baseline IV estimate is larger in magnitude than the OLS estimate. Although conventional wisdom suggests that IV estimates are lower than OLS estimates, this requires that bias is one-sided—that is, that OLS estimates are only overestimating

\textsuperscript{61}We also find that a 10% rise in foreclosures is associated with a 0.46% decline in earnings. However, once employment is included as a control, the estimate declines to 0.35% (\( p \)-value is 0.336). The fact that we cannot reject the null of no effect is consistent with Bernstein (2016) that foreclosures affect household income, but through the extensive margin. In particular, Bernstein (2016) finds that loan shocks are only associated with large declines in wealth, rather than small declines that might be driven by an adjustment of hours worked.
the effects of foreclosures. However, dynamic selection runs in the opposite direction and is likely to be more important after having already controlled for other contemporaneous factors (e.g., housing prices, mortgage payments, etc), which mitigate the presence of reverse causality. To examine the potential role of dynamic selection more carefully, we restrict the sample to mortgages that have become seriously delinquent (90+ days) and predict how many months will elapse between this delinquency and eventual foreclosure. We subsequently compute the mean time in months between this delinquency and eventual foreclosure and regress it on employment growth, producing a coefficient of -13.55 (p-value = 0.00), which suggests that better (worse) economic conditions are associated with mortgages taking less (more) time to move from delinquency to foreclosure.

Motivated by recent policy discussions (Calomiris and Higgins, 2011) and research on the role of foreclosure delay (Herkenhoff and Ohanian, 2015), we subsequently examine heterogeneity across states with and without judicial status laws (“JUD” and “NJUD”), which lengthen the foreclosure process and raise the costs associated with going to court (Pence, 2006). Columns 6 and 7 restrict the sample to states without and with judicial status laws, producing elasticities of 0.91% and 1.34%, respectively. The fact that states without judicial status laws exhibit a weaker foreclosure gradient in spite of the fact that they experienced roughly twice as many foreclosures is surprising. While we explore the potential channels behind these results later, our results are consistent with models where foreclosure delay generates uncertainty for banks, thereby affecting lending to businesses (and therefore employment). Our results are also related with Ghent and Kudlyak (2011) who find that borrowers are 30% more likely to default in non-recourse states.

Having discussed the results associated with employment as the outcome variable, we now turn to Panels B and C, which replace the outcome variable with logged hiring and job turnover rates. Under our baseline specification in column 4 for non-tradables, we find that a 10% rise in foreclosures is associated with a 2.18% decline in hiring and a 0.10 percentage point decline in job turnover. These results are again robust to restricting the sample to post-2007 data and/or controlling for housing prices or housing price growth. We find similar sources of heterogeneity between states with and without judicial status laws. For example, a 10% rise in foreclosures is associated with a 1.6% decline in hiring and 0.05pp decline in job turnover among non-judicial status states, but a 3.58% and 0.21pp decline in judicial status states, respectively.

Before continuing, we also discuss the effects of foreclosures on income inequality. Motivated by recent work in the sociology literature by Bernstein (2016) and Rugh and Massey (2010), we
now switch our outcome variable to the standard deviation of logged income—measured with total income and adjustable gross income—across zipcodes within the same county as a proxy for income inequality.\footnote{We have also experimented with the 90/10 income ratio and the Gini coefficient from the Census both at a county level.} One concern is that these foreclosures are concentrated on low income borrowers, or that the incidence of foreclosures affect low income workers at firms that are forced to implement layoffs due to a lack of credit availability. We do not find evidence that either of these channels are operational. In fact, we find a statistically insignificant, but negative, association when we use the dispersion in logged total income and a statistically significant and negative coefficient of -0.03 when we use the dispersion in logged adjustable gross income (see Appendix Section A.5.2).

Appendix Section A.5.3 examines our results across several dimensions of heterogeneity. First, we find the largest foreclosure gradients of roughly -0.18 to -0.22 for employment and -0.28 to -0.33 for hiring among firms with between 50 and 499 employees (see Figure 35). While these are not “micro businesses” with under 10 employees, they are medium scale businesses that do not have access to capital markets and are more likely to rely on external financing from banks. Second, we do not find much heterogeneity in the exposure of a county to employment in the construction sector or across counties with different average household incomes, which is consistent with our earlier results that foreclosures did not amplify, but rather compress, the income distribution. Third, we examine the potential for extensive margin effects on establishment closures, finding that small (10-99 employees) and medium (250-499 employees) establishments were the most heavily affected. Fourth, we examine the potential asymmetric effects of foreclosures depending on whether the housing market is in a boom or bust, but do not find evidence. However, we cannot rule out the possibility that these null results are driven by a lack of variation.

## 5.2 Aggregate Impact

How much of the decline in the hiring rate can be attributed to the surge in foreclosures? Although we cannot identify the aggregate level effect of foreclosures on employment since we are using a within-county estimator, we can focus on employment growth and the hiring rate since they are relative. Since our estimated foreclosure elasticities are based on the underlying population of counties in the United States between 2000 and 2014, we can use them to conduct a back-of-the-envelope aggregation exercise to gauge the relative importance of foreclosures, relative to other phenomena during this period. We compute these partial equilibrium approximations for
the pooled sample and various subsets (non-tradables sectors, judicial and non-judicial states, and small firms). We focus on 2007:Q4 and 2009Q3:2010:Q1 as the start and end of the Great Recession, respectively, based on the National Bureau of Economic Research dating methodology. We, however, take 2006Q1 as the start when looking at foreclosures since they began before the effects of the Great Recession became apparent.

Using our quarterly county foreclosures data, we find that foreclosures grew by 264% overall, but 379% in states without judicial status laws and 111% in states with judicial status laws. We also find that that the hiring rate declined by roughly 28% across different partitions of the labor market, which reflects the fact that, although employment fell more in some sectors than others, hires fell by a proportional amount. Given our estimated elasticities between foreclosures and the hiring rate, we can subsequently compute the aggregate effects via $\gamma \times \Delta f/\Delta y$ where $\Delta$ denotes the “growth in” and $y$ denotes our outcome of interest (i.e., hiring rate).

Table 3 documents these results. We find that the rise of foreclosures explains 10.45% of the decline in the hiring rate overall. However, the effects are heterogeneous: even though states without judicial status laws had many more foreclosures, their estimated elasticity is lower and, therefore, the rise of foreclosures only explains 4.24% of the decline in the hiring rate, whereas it explains 9.38% in states with the judicial status laws. We also find that the rise in foreclosures can explain 16% of the decline in the hiring rate among small businesses (20-249 employees).

There are two caveats about these aggregation results. First, since our estimates of $\gamma$ are identified for marginal changes in foreclosures, we are likely to underestimate the true general equilibrium effects of the surge in foreclosures, which may interact with other system non-linearities (Brunnermeier and Sannikov, 2014). Second, we assume that the within-county variation “aggregates up”—that is, there is no reallocation. Given that regional elasticities vary (Beraja et al., 2016), the aggregate effects will depend on the ways in which the regional shocks interact with one another. While we agree that this partial equilibrium assumption is strong, we view the exercise as a useful one since it helps discipline the predictions of macroeconomic models and find that it likely causes

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63 One other possible concern is that our estimates are local average treatment effects (LATE) since they are identified off of ARM-based variation. While we document that FICO scores are remarkably similar across individuals with different ARMs in Appendix Sections A.3.2 and A.4.2, we recognize that there may be differences among these individuals and others who do not use ARMs (i.e., those with 30 year fixed rate mortgages), but note that an examination of our FICO distributions shows that many ARM holders were not low income. This is consistent with evidence from Adelino et al. (2016) that many sub-prime loans were targeted towards middle income earners, as well as with recent work from Antoniades (2015) and Albanesi et al. (2017) that the rise in credit growth was concentrated in the middle of the credit score distribution, making our estimate the potentially more relevant policy parameters of interest. At worst, however, we identify a LATE, which simply requires that the monotonicity assumption is satisfied (Heckman et al., 2006).
us to underestimate our aggregate effects.\textsuperscript{64}

\begin{table}[h]
\centering
\caption{Table 3}
\end{table}

\section*{5.3 Robustness and Extensions}

\subsection*{5.3.1 Examining the Exclusion Restriction}

We implement several diagnostics to provide assurance that our main results are not biased due to unobserved time-varying shocks to contemporaneous labor market outcomes and predicted foreclosures based on ARM resets. Our first diagnostic begins by examining whether changes in the share of a particular type of adjustable rate mortgage originations are correlated with changes in income. If, for example, there are large swings in income fluctuations that coincide with changes in the share of different ARM originations, then the intensity component of our variation is likely endogenous. We test this in two ways. First, we include the share of ARMs in our baseline specification used to produce Table 2, which does not alter our results. Second, and more directly, we regress the share of type-$k$ hybrid ARMs on a semi-parametric measure of the logged number of filers by seven income bracket bins, conditional on controls, between 2004 and 2007.\textsuperscript{65}

We report our coefficients with and without county and year fixed effects to underscore the importance of controlling for time-invariant factors that are correlated with both the share of hybrid loans and income. Table 4 documents these results. While our OLS results tend to point towards some correlation between the number of filers and the share of ARMs, albeit there is no systematic correlation across ARM types, these correlations vanish once location fixed effects are introduced. We only find a few instances where the number of filers is correlated with the share of 5-1, 7-1, or 10-1 ARMs (e.g., filers between $10,000-25,000 for 7-1 ARMs). However, these minor correlations exhibit no systematic pattern and, therefore, are inconsistent with a view that variation in the shares of ARM borrowers is a major source of concern.

\begin{table}[h]
\centering
\caption{Table 4}
\end{table}

\textsuperscript{64}We can still assess the extent to which our partial equilibrium result holds by estimating the presence of reallocation. We do this by regressing a weighted average of job flows (normalized to their prior quarter) in the counties that neighbor county $c$ on foreclosures in county $c$, thereby asking whether an increase in foreclosures is associated with an increase in nearby employment. We report variants of these regressions in Appendix Section A.5.4, we find that increases in foreclosures have, if anything, negative spillovers on neighboring counties.

\textsuperscript{65}Loans originated after this period will start to have reset dates after the period of our data ends, since our final year is 2014. In this sense, they do not provide significant identifying variation.
Our second diagnostic exercises gauges the role of unobservables. One concern, for example, is that credit market frictions for employers (e.g., Christiano et al. (2015)) are somehow correlated with both employment and foreclosures in unobserved ways. Given that we have large $R$-squares in our results, the margin for unobserved heterogeneity is relatively low, allowing for partial identification using an approach introduced by Oster (forthcoming). Table 17 in Appendix Section A.5.5 presents these results, showing that our estimates are similar even when we restrict the degree of selection on unobservables to be 20% of the magnitude of selection on observables.

Our third diagnostic exercises examines the exclusion restriction more carefully. While prior evidence suggests that homeowners tend to have biased beliefs about future housing prices (Case and Shiller, 1989; Shiller, 2007; Kaplan et al., 2016), and thus might not be likely to develop beliefs about future employment growth, we can gauge the potential role that anticipation may have played in affecting the types of contracts that individuals select into or that banks offer. 40 in Appendix Section A.5.5 illustrates how much residual unemployment growth in year $t$ and income growth in year $t+5$ variation can be explained by the baseline controls versus the baseline controls with the interest rate resets arising from the 5-1, 7-1, and 10-1 ARMs. We show, for example, that including our instruments raises the $R$-squared of our unemployment growth regression from 7.5% to 24%, whereas it keeps the $R$-squared for our income growth regression from 8.2% to 8.4%. In this sense, duration choices and their corresponding interest rate shocks predict labor market outcomes, but not potential confounders, like income.

Fourth, using our Bartik-like instrument, we show our main results are not driven by contemporaneous shocks to both the labor market and interest rates. By exploiting a county’s pre-recession exposure to banks that were originating different types of loans, we can hold constant all subsequent compositional changes within a county following the baseline year. Table 5 documents these results, which are very similar: a 10% rise in foreclosures is associated with a 0.68% (versus 0.77%) decline in overall employment and a 1.23% decline in the non-tradables sector.66

66While we find non-judicial states exhibit a greater foreclosure elasticity than the judicial states, in contrast to our baseline, these results are driven by a weaker first-stage, evident by the $F$-statistic right below 10.

[INSERT TABLE 5]
5.3.2 Foreclosure Intensity and Non-linearities

Macro-finance models emphasize non-linearities in times of crises (e.g., Brunnermeier and Sannikov (2014)). Especially during the height of the crisis in 2009-2010, foreclosures were roughly 200-400% as large as their trend levels in 2006 or before. It is also possible that what matters is not the single foreclosure shock, but rather the cumulative foreclosure stock. For example, it is possible that the first ten foreclosures have a small effect on a county in comparison to an additional ten from a starting point of 500. We now examine the potential for non-linearities by: (i) including a squared term for foreclosures per open mortgage, and (ii) replacing logged contemporaneous foreclosures with cumulative logged foreclosures to date. Appendix Section A.5.6 documents these results, suggesting that the inclusion of either non-linearities or foreclosure intensity raises the quantitative magnitude of our estimates for the areas that were hit hardest.

6 Understanding the Mechanisms

The fact that foreclosures grew was no surprise given early indications of housing price declines in 2007:Q3. However, the fact that foreclosures grew so rapidly and significantly in such a short window was unanticipated, surprising even many banks as their foreclosure processes became backlogged. This section begins by providing causal evidence that foreclosures led to a decline in optimism and rise in uncertainty at a local level. An important feature of our strategy is that we measure sentiment at an individual level, which allows us to exploit within-zipcode variation and control for county \( \times \) time fixed effects as an additional layer of robustness.\textsuperscript{67} We subsequently provide evidence that foreclosures raise uncertainty and depress local optimism, which lead to declines in not only the hiring rate among firms, but also lending among banks. The credit freeze feeds back into employment declines. Using a sample of local banks, we also show that our estimates are not driven by contemporaneous and direct effects of foreclosures on bank portfolios.

\textsuperscript{67}Di Maggio et al. (2017b) is conceptually related in that they measure local uncertainty by weighting four-digit NAICS uncertainty shocks by the county employment share in each of those four-digit industries. While our approaches differ, they each have their own advantages and disadvantages. A disadvantage of our approach is that we do not have data on every county in the United States. However, an advantage of our approach is that we measure individual beliefs, allowing us to examine not only different dimensions of heterogeneity, but also more parsimonious specifications that control for county \( \times \) time unobserved factors that are potentially correlated with local credit conditions.
6.1 Foreclosures and Local Optimism and Uncertainty

Our main measure of local optimism and uncertainty is based on micro-data responses from the U.S. Daily Poll from 2008 to 2014 over perceptions of the current and future state of the economy, which vary on a scale of 1-4 and 1-3, respectively. Before presenting our empirical specification relating foreclosures with sentiments, Figure 9 begins by comparing our measure of optimism with the volatility index (VIX) at a daily frequency between 2008 and 2015. We see that increases in volatility have a -0.63 correlation with mean perceptions of the current state of the economy. Appendix Section A.6.1 also reports additional comparisons with mean perceptions of the future state of the economy ($\rho = -0.59$), together with their standard deviations ($\rho = -0.42$ and $\rho = -0.54$), with the VIX. We also compare between these and the economic policy uncertainty index from Baker et al. (2016) and investor sentiment from Baker and Wurgler (2006).

Having validated our main measure of uncertainty, we now turn towards quantifying the effect of foreclosures on the mean and standard deviation of county sentiment. We estimate Equation 2 under four sets of outcome variables: the mean and standard deviation of perceptions on both the current and future state of the economy. Our data also provides us with the flexibility to account explicitly for income effects by controlling for individual income brackets and daily consumption expenditures. Like before, we control for mortgage payments due in a county and instrument for foreclosures using our baseline strategy, but using zipcode-level variation this time.

Table 6 documents these results. We begin by focusing on Panel A, which reports the estimated coefficients when the outcome variable is individual logged perception of the current or future state of the economy. We find that a 10% rise in foreclosures is associated with a 1% (column 1) and 0.8% (column 3) decline in the perception of the current and future state of the economy, respectively. The fact that the gradient on housing prices is marginally larger and negative suggests that higher housing prices raise rents, which may affect individual sentiment too. Increases in local bank deposits have a positive association with sentiment, whereas mortgage payments have a negative

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68 We are specifically cognizant that both household and investor sentiment matter for economic activity. While firms and banks are ultimately comprised of households, the set of individuals who make purchasing decisions in firms may vary in systematic ways from the average individual selected to participate in Gallup’s daily poll. We, therefore, examine the correlation between our measure of sentiment about the current state of the economy with the measure of investor sentiment from Baker and Wurgler (2006), producing a correlation of 0.47 between 2008 and 2015. While our indices are highly correlated, they diverge in 2011:Q3 for a few months before returning to having very similar time series patterns.
One concern with these results, however, is that the presence of income effects induced by the ARM interest rate resets in our instrument could simultaneously affect sentiment. For example, if an individual experiences a spike in their interest rate, thereby lowering their disposable income, then the individual might not be able to purchase as much consumption, thereby reducing their mood and perception of the economy. Columns 2 and 4 control directly for income effects by introducing logged individual income and non-durable consumption expenditures as controls. In fact, our estimated gradients rise to 1.4% and 1.8% for a corresponding 10% rise in foreclosures. The fact that individual perceptions about the future state of the economy change (not just the current state) is consistent with the claim that banks and other firms update their expectations about local economic development in response to foreclosures. To further rule out the possibility that there are other confounding factors correlated with sentiments, we exploit only very local variation by controlling for county × quarter × year fixed effects.

We now turn to Panel B, which reports the estimated coefficients when the outcome variable is the standard deviation of logged sentiments within a county × quarter × year. We find that a 10% rise in foreclosures is associated with a 0.1% and 1.1% rise in the standard deviation of the current and future state of the economy, respectively. Unlike our estimates from Panel A, our theory here is that foreclosures raise uncertainty, which we proxy by taking the dispersion of beliefs within a local area. The fact that our estimates are stronger for perceptions of the future (versus perceptions of the current state) are further evidence that we are identifying uncertainty, not just level shocks. We also deal with income effects by introducing logged county adjustable gross income (AGI) as a control in columns 2 and 4, which keep our results in tact. Although show that our sentiment measure corresponds to uncertainty, these results are also consistent with models of ambiguity aversion where investment declines (Ilut and Schneider, 2014).

[INSERT TABLE 6]

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69 This is on top of our mortgage payments that come due in a given quarter.

70 Under this specification, we find that a 10% rise in foreclosures is associated with a 0.67% (p-value = 0.00) decline in sentiment coefficient about the current state of the economy. We also find a corresponding 0.1% decline in perception of the future state of the economy, but our estimate is not statistically significant at conventional levels since this index has less variation—it can only take on one of three values.
6.2 Firm Hiring and Bank Lending Channels

Why might local optimism and uncertainty affect employment? As we discussed earlier, there are three classes of channels. The first is the real option channel whereby a rise in uncertainty can raise the option value associated with delaying investment of an otherwise productive asset (Bernanke, 1983; MacDonald and Siegel, 1986; Dixit and Pindyck, 1994). The second is the risk premium channel whereby a rise in uncertainty can raise the cost of financing and affect interest rates associated with financing otherwise productive investment (Arellano et al., 2016; Gilchrist et al., 2014; Hall, 2017). The third is the risk aversion channel whereby a rise in uncertainty in the presence of nominal rigidities can reduce the desire to pursue high return and other innovative projects (Basu and Bundick, 2017; Bernstein et al., 2017).

We begin by examining evidence over the real options channel. We do so by implementing two diagnostic exercises. First, leveraging the fact that the adverse effects of uncertainty on hiring is greater when the cost of creating a job (or terminating an existing job) is greater (Riegler, 2014), we examine whether the effects of foreclosures are greater in states with stronger enforcement of non-compete contracts. Enforcement of non-compete contracts make it costly for states to hire and reduce the degree of labor market mobility (Starr et al., 2016), meaning that the option value associated with delaying additional hiring should be greater in states where it is harder to recruit workers from one employer to another. We now estimate Equation 2 by interacting foreclosures with an indicator for high state enforcement of non-competes and instrumenting for foreclosures using interest rate resets interacted with the indicator. Setting the hiring rate as our outcome, we find that a 10% rise in foreclosures leads to a 1.1 percentage point overall decline in hiring and an additional 0.6pp decline in high enforcement states (both p-value = 0.00).\footnote{We also explored other measures of firing costs, like the presence of wrongful discharge laws. We define a state as having a wrongful discharge law if they have either public policy or good faith laws. We focus on these versus implied contract laws since they pose the greatest legal risk for companies if sued for wrongful termination; see Autor et al. (2006) for details. In Appendix Section A.6.2, we provide additional evidence for both wrongful discharge laws and non-compete enforcement. While the evidence for non-competes is strong, we find no evidence of heterogeneity based on states with wrongful discharge laws. However, one possibility is that the lack of heterogeneity is simply driven by low power in the effect of these laws on firing costs.}

Second, we examine the effects of foreclosures on the time it takes a county to recover from the Great Recession. For each county, we count the number of months (starting in 2008:Q1) it took for it to return to its 2006 employment level. We subsequently regress it on the mean logged number of foreclosures between 2008 and 2011 at a county-level, plotting the two in Figure 10, which suggests that a 1% rise in foreclosures between 2008 and 2011 is associated with an additional 23 months.
before the county recovers to its 2006 employment level. Of course, recognizing that counties differ in many ways, in particular that foreclosures took place with a number of other correlated negative shocks, we exploit cross-sectional variation in state judicial status laws as in Mian et al. (2015), controlling for average housing price growth over those years, and use a matching estimator based on state median county income and housing values in 2006. Doing so reduces our estimate down to 6.27, although our estimate is imprecise ($p$-value = 0.681). In this sense, we find suggestive evidence that foreclosures led to a slower recovery, but cannot reject the null that they had no effect on their long-run economic prospects.

[INSERT FIGURE 10]

Having provided evidence of a real options effect—consistent with, for example, Stein and Stone (2013) who find that a 10% rise in implied volatility is associated with a 1.65% decline in hiring—we now turn towards evidence over the risk premium and precautionary channels using our loan-level data from the SBA. Small businesses are especially dependent on bank lending as a source of financing (Cole et al., 1996). A necessary ingredient in establishing the presence of a credit channel is the presence of sticky bank lending relationships. As Chodorow-Reich (2014) discusses, lending relationships could be sticky for a variety of reasons. For example, adverse selection in the market for borrowers could affect who decides to switch lenders (Sharpe, 1990). Similarly, there could exist a signaling equilibrium whereby lending to the same borrower reduces moral hazard problems (Holmstrom and Tirole, 1997; Sufi, 2007). It is also possible that declines in ex-ante (due diligence) or ex-post (state verification) monitoring costs affects the returns to lending to the same borrower (Williamson, 1987; Montoriol-Garriga and Wang, 2011).

Given the presence of sticky lending relationships, our argument is that foreclosures lead to increases in uncertainty and declines in optimism, which lead to a local credit freeze. Although there is a wide recognition that credit matters, especially for small businesses, we also show that the credit-foreclosure elasticity is stronger among firms in the tradables sector since they are more credit dependent. We focus on three loan-level measures from our SBA data, including: (i) the share of the loan that the bank guarantees (versus the SBA), (ii) overall loan size, and (iii) the probability of default. Our specifications control for loan-level factors, such as the type of business (individual, partnership, corporation) and whether the loan has a continuing line of credit.

Table 7 documents these results. Beginning with column 1, we find that a 10% rise in foreclosures is associated with a 0.38pp decline in the share of the loan that a bank is willing to guarantee
independent of the amount that the SBA is lending. Column 2 shows that a corresponding increase in foreclosures is associated with a 2.40% decline in loan volume. These two elasticities reflect the fact that foreclosures affect not only overall bank lending, but also the relative amount that a bank is willing to risk. Column 3 shows that a 10% rise in foreclosures is associated with a 0.84pp rise in the probability of default. Importantly, these elasticities are not driven by spurious declines in housing net worth, which is a source of collateral for small business owners (Schmalz et al., 2017), we have controlled even more flexibly for housing price growth (e.g., introducing a hundred bins on housing price growth as controls). These results are consistent Sirignano et al. (2016) who show that local foreclosures have non-linear effects on bank lending strategies, the composition of loans, and interest rates. Our results are quite similar when we use loans from the 504 lending program in the SBA, which provides financing for the purchase of fixed assets (e.g., buildings and machinery) at below market rates (see Table 22 in Appendix Section A.6.2).

[INSERT TABLE 7]

### 6.3 Loan Supply Shocks versus Uncertainty

Even if we are capturing the causal effect of foreclosures on uncertainty, how do we know that uncertainty is a primary culprit behind the decline in bank lending? For example, the alternative theory is that foreclosures lead to a deterioration of a bank’s loan portfolio. Although we have shown that foreclosures raise uncertainty, and prior literature has shown that uncertainty reduces bank lending (Buch et al., 2015; Bordo et al., 2016; Alessandri and Bottero, 2017), we now provide explicit evidence that we are not simply capturing the direct effect of foreclosures on a bank’s loan supply. Using the Call Reports data, we restrict the sample to local banks for two reasons.\footnote{Although national banks are much larger than local banks—for example, we find that they loan 109% more in loan volume and have 95% larger assets, on average—the ratio of their loans to assets and loan growth are much more comparable to the same statistics among local banks. See Appendix Section A.6.2 for a comparison between the two sets of banks.}

First, these data allows us to examine how local foreclosures affect banks that have their entire loan portfolio in the local economy. Given that firms find it easier to borrow from local banks (Petersen and Rajan, 2002; Agarwal and Hauswald, 2010), these are precisely the banks that should exhibit the greatest response to declines in their loan portfolio. Second, these data contain measures of bank assets and deposits, which allow us to directly control for how foreclosures might also correlated with contemporaneous correlates of bank balance sheet health.
We now estimate regressions of the growth in bank commercial lending on logged county foreclosures, controlling for the usual covariates (demographics, mortgage payments, housing prices) and bank assets and deposits. Table 8 documents these results. First, we find that the inclusion of bank assets and deposits does not alter our estimated gradient on local foreclosures: a 10% rise in foreclosures is associated with an approximate 0.45 percentage point decline in lending. Not surprisingly, under our preferred specifications in columns 4 and 8, assets are positively related with lending and deposits are negatively related with it, which reflects the flight to quality and empirical regularity that banks hoarded deposits during the financial crisis (Caballero and Krishnamurthy, 2008; Cornett et al., 2011; Berrospide, 2012). Second, although omitted for brevity, we obtained statistically indistinguishable coefficients when we use growth in total bank lending as our outcome variable. If the credit channel was not the primary culprit of the employment decline in response to foreclosures, we would expect to see a wedge between the foreclosure gradients when we define lending based on commercial versus total loan volume.

[INSERT TABLE 8]

### 6.4 Comparison to the Literature

The role of credit over the Great Recession has received considerable attention given how much it declined during the financial crisis (Murillo et al., 2010). Most contributions examining the decline in credit have focused on the bank lending as the primary channel (Brunnermeier, 2009; Ivashina and Scharfstein, 2010; Santos, 2011; Chodorow-Reich, 2014)—that is, financially exposed banks experienced a negative shock, which led to a contraction in credit supply to firms that were connected with them. Both Mian and Sufi (2011) and Mian et al. (2013) show that the expansion of credit to subprime borrowers eventually led to large declines in consumption and other real outcomes, which largely was driven by declines in local demand (Mian et al., 2017). However, there remains some controversy over the magnitude of the bank lending channel since

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74We do not interpret these coefficients—for example, columns 1 versus 4—as evidence that the direct effects are immaterial; clearly they matter. Because we do not see bank × county × year measures of foreclosures, lending, and balance sheet characteristics, we cannot isolate the two channels—all we can do is provide evidence that the indirect effects arising from uncertainty are present.
75However, there are some exceptions. For example, Mondragon (2015) who focuses on the decline in household credit.
capital expenditures among firms evolved in similar ways during the crisis irrespective of how they financed themselves before the crisis (Kahle and Stulz, 2013).

Our results provide a complementary economic mechanism for the decline in credit during the financial crisis. Even if a firm was not directly connected with a bank that experienced a negative shock during the crisis, credit may have still been constrained due to the rise in uncertainty and decline in optimism induced by local foreclosures. For example, Caballero and Krishnamurthy (2008) analytically characterize a setting where investors exhibit a flight to quality during a time of crisis. Similarly, Bernanke (1983) and Bloom (2009) develop quantitative models and estimates of the impact of uncertainty shocks on investment. Although labor is arguably the most significant type of investment good for firms (Merz and Yashiv, 2007), we also provide evidence in Appendix Section A.6.2 that foreclosures are associated with declines in physical investment, which we proxy using county × industry data on advertising and rental expenditures.

A final issue is whether our foreclosure gradients are identified off of a demand versus supply response. Recent work by Mian et al. (2017) exploits variation across the timing of banking regulations to understand whether the expansion of credit primarily affects aggregate demand versus labor productivity among firms. While their result that the expansion of credit was consistent more with an aggregate demand channel, we argue that our effects are consistent with more of a supply-based response. Using SBA micro-data, we showed that local foreclosures reduce not only loan volume, but also bank exposure to small businesses as measured by the share of a loan that a bank is guaranteeing relative to the total loan amount. Using Call Report micro-data, we also showed that local foreclosures reduce bank lending, driven almost exclusively through commercial lending, even after controlling for bank assets and deposits. If we were identifying only an aggregate demand channel, then we would expect to see three differences: (i) a decline in loan volume, but not a decline in bank exposure to small businesses, (ii) a larger decline in lending to households relative to commercial entities, and (iii) a decline in lending that vanishes after controlling for bank balance sheet shocks based on the deterioration of local household mortgages.

6.5 Placebos from Skill Composition and Amenities

If we are capturing a truly causal effect of foreclosures on the labor market and credit, we would expect to see an exodus of skilled workers, relative to unskilled workers for at least two reasons. First, if the concern is that foreclosures are associated with lower productivity areas, then there
should be a greater proportion of unskilled (e.g., non-college) workers, which would be biasing our estimated coefficients. In this sense, we should be less likely to find a relatively larger effect on skilled workers if we are merely picking up other negative contemporaneous factors associated with interest rate resets. Second, since foreclosures have been linked with an increase in crime (Immergluck and Smith, 2006; Cui and Walsh, 2015) and decline in city amenities (Makridis and Ohlrogge, 2017), then we would expect to see a larger decline among skilled workers who have better outside options and a willingness to pay for higher amenity areas.\footnote{Prior research has already established that higher skilled workers value local amenities (Glaeser et al., 2001; Glaeser and Mare, 2001) and that these amenities endogenously change the skill composition of a labor market Diamond (2016) potentially through knowledge spillovers (Glaeser et al., 1992; Moretti, 2004) or social interactions (Lucas and Moll, 2014). These results are also consistent with the stronger gradient that we find in the tradables sector given that they tend to hire higher skilled workers (see Figure 32).}

We subject our data to this additional test by exploiting heterogeneity in educational attainment from the LEHD. Our theory is that a sudden rise in dis-amenities, controlling for price effects, reduces the attractiveness of an area and, in particular, for skilled workers. We use college attainment as a proxy for skill, producing two measures of mobility: (i) logged employment among college graduates net of logged employment among non-college graduates, and (ii) logged employment among graduates net of logged employment among workers with some college experience. We also compute their growth rates. Based on these new variables, we replace the outcome variable from Equation 2 with them and estimate these models using the same identification strategy.

Table 9 documents these results. We find that a 10% rise in foreclosures is associated with a 0.308% decline in the logged ratio of college graduates to non-graduates in the labor force, but only a statistically imprecise 0.15% decline among college graduates net of those with some college experience. The fact that the former is over twice as large and much more precisely estimated suggests that the bulk of the individuals who stay within a local area are those with less than a high school degree or just a high school degree, consistent with the “sheepskin effect” for individuals holding at least some college experience (Hungerford and Solon, 1987; Card and Krueger, 1992).

Turning towards employment growth, we find that a 10% rise in foreclosures is associated with a 0.001pp and 0.002pp decline in employment growth among college degree workers net of those without a college degree and college degree workers net of those with some college experience, respectively. While the magnitudes on our employment growth estimates is in some ways reversed relative to our estimation in levels, they are statistically indistinguishable from one another, suggesting that there increases in foreclosures are affecting the rate at which employment among college degree workers are exiting a county net of workers with no college and some college equally
over this period. Since the mean growth rates for college net of no-college and net of some college employment are 0.0011 and 0.0018, then the marginal effects evaluated are 9.1% and 11.1% of the mean flows, which we view as quantitatively significant.

[INSERT TABLE 9]

Appendix Section A.6.3 examines an analogue of Table 9, but with earnings, rather than employment, as a further diagnostic to gauge the plausibility of our proposed mechanism. Theoretically, if the relative composition of college to non-college workers declines since the areas are becoming less attractive to live in, then we should also see a rise in the relative earnings premium for these workers as a compensating differential.\textsuperscript{77} Indeed, we find that a 10% rise in foreclosures is associated with a 0.29% and 0.27% increase the relative earnings premium between college & non-college and college & some-college workers, as well as a comparable 0.051pp and 0.043pp increase in the growth rate of the earnings premium. Appendix Section A.6.3 also provides more detailed evidence by turning towards micro-data from over eight million individuals from the 2000 Decennial Census and 2005-2014 annual American Community Survey (ACS). We find that increases in the growth rate of foreclosure are associated with declines in the probability of college attainment that increases in income—that is, areas with increases in foreclosure are not only less likely to have fewer college workers, but also increasingly so among higher income workers.

But, what is it about foreclosures that generate the decline in skilled workers? We argue that the primary channel behind the differential effect is based on a demand for local amenities. Given that higher income workers demand better city amenities (Glaeser et al., 2001; Glaeser and Mare, 2001), more recent evidence also suggests that skill and local amenities are endogenously related (Diamond, 2016). Higher foreclosures can depress amenities in at least two ways. The first is by reducing overall city satisfaction—that is, the way a neighborhood looks and feels (Makridis and Ohlrogge, 2017). The second is by raising crime due to the increase in vacancies following foreclosure (Immergluck and Smith, 2006; Ellen et al., 2013; Cui and Walsh, 2015); see Appendix Section A.6.3 for additional evidence where we find that a 10% rise in foreclosures is associated with a 2.52% rise in crime.\textsuperscript{78} Anenberg and Kung (2014) decompose these two effects, finding that

\textsuperscript{77}Sorkin (2015) shows that approximately two-thirds of the variation in earnings is driven by compensating differentials.

\textsuperscript{78}While the gradient is much smaller for higher income counties, our results are robust to controlling for a county’s adjustable gross income, which proxies for the fact that counties may vary with respect to their unobserved productivity. We include the control since we did not have enough power when we only use 5-1, 7-1, and 10-1 ARMs as instruments. However, by including 2-1 and 3-1 ARMs, we are vulnerable to the concern that these counties systematically vary in the type of workers residing with them.
prices matter most.

6.6 Implications for Foreclosure Policy

Following the financial crisis, an emerging literature began analyzing the optimal design of mortgages. The bulk of these contributions have emphasized the benefits of adjustable rate mortgages; see, for example, Piskorski and Tchistyi (2010), Piskorski and Tchistyi (2011), and Guren et al. (2017). While the negative effects of foreclosures on labor market outcomes in our earlier results might allude to macro prudential policies that slow the process of foreclosures, these ex-post policies may be ineffective at best and counterproductive at worst. A prime example is the Home Mortgage Modification Program (HAMP). Agarwal et al. (2017) find that, while HAMP led to a decline in the rate of completed foreclosures, it was largely ineffective and could have had a much larger impact if it had created a broader framework for loan renegotiation.

Setting aside the increased costs arising from administrative burdens and legal fees often associated with policies that attempt to reduce the frequency of foreclosure (Pence, 2006), one concern is that merely slow the process of foreclosure. In particular, rather than resulting in a sustainable renegotiation of delinquent loans, they may raise foreclosure volatility. Exploiting variation from our zipcode-level data, we compute the standard deviation of logged foreclosures across zipcodes in the same county and examine whether greater volatility has adverse effects on labor market outcomes, instrumenting using the standard deviation of logged predicted ARM resets and controlling for our usual fixed effects, county covariates / demographics, and mean logged foreclosures. We find that a standard deviation increase in logged county foreclosures is associated with a 0.021% and 0.012% decline in hiring among firms in the tradables and non-tradables sectors, respectively.

The fact that we find a robust negative association between the volatility of foreclosures even after controlling for the mean suggests that a reduction in the rate at which foreclosures are processed have their own negative effect on the labor market. This is on top of the fact that we find the rise of foreclosures explains nearly 50% more of the decline in the hiring rate between states with and without judicial status laws (see Table 2). Our results are also consistent with Guren and McQuade (2013) who develop and estimate an equilibrium search model with foreclosures, finding that slowing down foreclosures prolongs the crisis by reducing the rate that the backlog of foreclosures gets cleared out. Unlike Guren and McQuade (2013), however, our results point towards an additional channel through which foreclosure volatility can adversely affect the labor
market: altering the incentives for banks to strategically delay foreclosures can raise uncertainty, which raises the real option effect for firms and banks. In this sense, counties might be better served by just “biting the bullet” versus prolonging the foreclosure process.

7 Conclusion

Do foreclosures affect the real economy? We provide the first evidence, to our knowledge, that foreclosures affect labor market outcomes outside of the conventional housing channel (Campbell et al., 2011; Anenberg and Kung, 2014; Mian et al., 2015; Gupta, 2016; Guren and McQuade, 2013). We examine the causal effects of local county foreclosures on labor market outcomes by assembling a comprehensive database on foreclosures from CoreLogic (covering over 80% of the mortgage market) and employment, hiring, and job turnover from the Longitudinal Employer Household Dynamics datasets. To address reverse causality, we exploit plausibly exogenous variation in the timing and intensity of interest rate resets on adjustable rate mortgages (ARMs).

The first part of our paper shows that these interest rate resets are associated with discontinuous changes in the probability of foreclosure. Even though interest rates might change by “only” a few percentage points, these changes can drastically affect borrowers’ monthly schedule of payments, consistent with several recent empirical studies (Fuster and Willen, 2017; Di Maggio et al., 2017a; Gupta, 2016; Di Maggio et al., 2017b). For example, given an initial interest rate of 3.62% on a $300,000 mortgage, a 2% interest rate reset up changes the payment from $10,860 to $16,860—an increase of 55.24%. While the main source of our identifying variation comes from the timing of these interest rate resets, we also show that the variation in intensity emerging from dispersion in ARMs is plausibly exogenous. In particular, we provide suggestive evidence that geographic dispersion in ARMs is driven by the expansion of banks with different national lending strategies, consistent with political economy evidence from Kroszner and Strahan (1999) and Kroszner and Strahan (2014). These discontinuities in foreclosure probabilities persist even after controlling for an array of location-specific and borrower characteristics.

The second part of our paper uses these plausibly exogenous movements in interest rates to predict foreclosures and identify their causal effect on county employment, hiring, and job turnover. We find that a 10% rise of foreclosures is associated with a 0.77% decline in employment, 2.10% decline in hiring, a 0.09 percentage point decline in job turnover, a 0.46% decline in housing prices, and 0.82pp decline in housing price growth. These effects are most robust and apparent
in the non-tradables sector, small businesses, and states with judicial status laws, consistent with
evidence from Mian and Sufi (2014) on housing wealth shocks and from Pence (2006) on the costs
of foreclosure delay. Using our elasticity estimates, a back-of-the-envelope calculation implies that
the surge in foreclosures explains roughly 10% of the decline in housing prices and 10-20% of the
decline in the hiring rate during the financial crisis.

The third part of our paper explores the role of credit in accounting for these observed labor
market declines. We begin by providing evidence that foreclosures affect the underlying sentiment
in a county. Using Gallup’s U.S. Daily Poll, we find that a 10% rise in foreclosures is associated
with a 1.4% and 1.8% decline in perceptions about the current and future state of the economy,
respectively. We also find that a comparable increase in foreclosures is associated with a 1.1% rise
in the dispersion of beliefs about the future state of the economy within a county. Given that the
surge in foreclosures was largely unexpected, our results are consistent with recent evidence on
the effects of uncertainty on sentiments and aggregate output (Benhabib et al., 2015; Benhabib
and Spiegel, 2016; Makridis, 2017). Motivated by these results, we subsequently use the universe
of loans from the Small Business Administration’s 7(a) and 504 lending programs and show that
a 10% rise in our instrumented measure of foreclosures is associated with a 0.38pp decline in the
share a bank is willing to risk on a loan, a 2.40% decline in loan volume, and a 0.84pp rise in
the probability of default. Using the Call Reports, we show that local banks reduce their lending
in response to foreclosures even after controlling for the health of their balance sheets, consistent
with models featuring flight to quality and risk aversion (Caballero and Krishnamurthy, 2008).

Our results raise several exciting areas for future research. First, how do network effects
help explain the origination of different types of loans and at different points in time? While our
evidence indicates that the variation that gave rise to variation in interest rate resets on adjustable
rate mortgages is plausibly exogenous, how these networks developed and influenced the financial
crisis remains an open question. Second, how did the decline in employment affect the frequency
of foreclosure? While we have focused on the opposite relationship using variation in interest rate
resets, an equally interesting and important question that we are working on is quantifying how
much of the rise in foreclosures can be explained by the rise in lay offs. Third, how much did the
rise in foreclosures contribute to the deterioration of bank loan portfolios? While we showed that
the decline in lending driven by increases in local foreclosures cannot be fully explained by balance
sheet effects, we are working on isolating the contribution of bank-specific decisions to foreclose
on loans to their credit lending during the crisis. Fourth, how do foreclosures potentially create
spatial externalities? While we have abstracted from general equilibrium and reallocation effects, neighboring counties have strong labor market ties and changes in one will unambiguously affect the other (Beaudry et al., 2012).

References


8 Tables and Figures

Figure 1: Evolution of Foreclosures Started as a Share of all Mortgages, 1979-2015

Notes: Sources: Mortgage Bankers Association. The figure plots seasonally adjusted foreclosures that were started as a share of open mortgages. The time series shows the rapid run up in the fraction of mortgage loans that went into foreclosure during the financial crisis.
Figure 2: Share of Adjustable Rate Mortgages, 2000-2014

Notes: Sources: CoreLogic. The figure plots the share of 2-1 & 3-1 and 5-1 & 7-1 & 10-1 adjustable rate mortgages (ARMs), relative to total loan origination, by year weighted by county population.
Figure 3: 5-1, 7-1, 10-1 Adjustable Rate Mortgage Shares and 1990-2000 Growth Rates

Notes.—Sources: CoreLogic and Census Bureau. The figure plots the share of 5-1, 7-1, and 10-1 ARMs in 2003 and 2004, relative to total loans for the county, with the growth rate of median housing prices (for specified owner-occupied houses), median household income, the unemployment rate, and the college share. Observations are weighted by the county’s 2000 population and standard errors are clustered at the county-level.
Figure 4: Housing Shocks and Employment Growth in Non-tradables and Tradables Sectors

Notes.– Sources: Longitudinal Employer-Household Dynamics, Federal Housing Administration. The figure plots employment growth and housing price growth (using an index normalized to 2000 prices) at the county-level for non-tradables and tradables sectors averaged between 2007 and 2010. Our classification scheme follows Mian and Sufi (2014) and we restrict the sample of counties to those with over 50,000 individuals. We also trim the data at the 5th and 95th percentiles.
Figure 5: Heterogeneity in ARM Loan Origination

*Notes.* Sources: CoreLogic. The figure plots the dispersion in originations of different types of hybrid loans across counties for between 2002 and 2007. For each county-year combination, we compute the mean years to origination of all of the 5-1, 7-1, and 10-1 ARM loans originated in that county and year. Blue on the graph indicates a relatively high concentration of 5-1 ARMs, red a relatively high concentration of 7-1 or 10-1 ARMs. We gray out counties that do not have enough observations to produce reliable loan shares (e.g., no or very few hybrid loan origination were observed in that year/county).
Figure 6: Heterogeneity in ARM Loan Resets

Notes. Sources: CoreLogic. The figure plots the within-county intensity of interest rate resets between 2006:Q1 and 2009:Q1 for all hybrid ARM loans that experience an initial interest rate reset during that quarter, and then sum the total amounts by which each loan resets. For a fixed county, we have a particular quarter in which it experiences the least net resets and a quarter in which it experiences the greatest net resets, and everything in between. We assign the quarter with the least net resets for a specific county the value of 1, and the quarter with the most a value of 16, with intervening values assigned accordingly. We then plot the geographic distribution of these rankings at several points in time in our study. The first of these plots is for 2006:Q1. Counties that are darkly shaded in red experienced proportionally more of their net resets in that respective year.
Figure 7: Heterogeneity in Foreclosures

Notes.—Sources: CoreLogic. The figure plots the within-county intensity of foreclosures between 2006:Q1 and 2009:Q1. For each county in our sample, we observe a total of 16 quarterly observations between 2006 and June 2009. We assign the quarter with the least number of foreclosures for a specific county the value of 1 and the quarter with the most a value of 16. We then plot the geographic distribution of these rankings at several points in time in our study. The first of these plots is for Quarter 1, 2006. Counties that are darkly red shaded experienced proportionally more of their total foreclosures, whereas those shaded cyan experienced less.
Figure 8: Interest Rate Spikes and Foreclosure Probabilities, by Vintage & ARM

Notes. – Source: CoreLogic. The figures plot, for different vintages of loans and adjustable rate mortgages, the non-parametric probabilities of foreclosure for each month since the origination period. Each observation is the share of individuals who were foreclosed upon corresponding to the month following origination. Reset up refers to increases in interest rates, while reset down refers to decreases in interest rates.
### Table 1: Baseline Effects of Foreclosures on the Housing Market

<table>
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<tr>
<th>Dep. var. = ln(housing price)</th>
<th>ln(heat waves)</th>
<th>ln(mortgage payments)</th>
<th>ln(heat waves)</th>
<th>ln(housing price)</th>
<th>ln(heat waves)</th>
<th>ln(mortgage payments)</th>
<th>ln(heat waves)</th>
<th>ln(housing price)</th>
</tr>
</thead>
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<tr>
<td>ln(foreclosures)</td>
<td>-0.063***</td>
<td>-0.074***</td>
<td>-0.046***</td>
<td>-0.023***</td>
<td>-0.029***</td>
<td>-0.033***</td>
<td>-0.082***</td>
<td>-0.044***</td>
</tr>
<tr>
<td>ln(mortgage payments)</td>
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<td>0.123***</td>
<td>0.051***</td>
<td>0.017***</td>
<td>-0.027***</td>
<td>0.002</td>
<td>0.008***</td>
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<tr>
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<td>.16</td>
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<td>.90</td>
<td>.12</td>
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<td>191005</td>
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<td>1725</td>
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<td>Yes</td>
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<td>No</td>
<td>Yes</td>
<td>No</td>
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<td>No</td>
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<td>Instruments?</td>
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<td>No</td>
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<td>Yes</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
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</tbody>
</table>

Notes.—Sources: CoreLogic, Federal Housing Administration, 2000-2014. The table reports the coefficients associated with regressions of logged housing prices (normalized to 2000) and housing price growth on logged foreclosures, conditional on logged loan payments due and fixed effects. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode $\times$ year level and include a cubic as instruments. Standard errors are clustered at the zipcode-level.

---

### Table 3: Quantifying the Aggregate Effects of Foreclosure Shocks on the Labor Market

Notes.—Sources: Longitudinal Employer-Household Dynamics, CoreLogic. The table reports the mean hiring rate (hires divided by employment) growth and foreclosure growth for the respective years. State $\times$ industry data is used to obtain non-tradables following the Mian and Sufi (2014) baseline definition with three-digit 2000 NAICS employment shares as weights, together with 2000 state employment shares to weight in the aggregation to the national level. The elasticities are summarized from earlier results using the IV strategy. Aggregate effects are obtained by computing: $\gamma \times (\Delta f/\Delta y)$ where $\Delta f$ denotes the change in foreclosures between 2009-2014 and 2000-2005 and $\Delta y$ denotes the corresponding change in outcome variable $y$.

---

<table>
<thead>
<tr>
<th>% Decline from 2007:Q4 to 2009Q3:2010:Q1</th>
<th>All</th>
<th>Non-Tradables</th>
<th>Judicial</th>
<th>Non-Judicial</th>
<th>Small Firms</th>
</tr>
</thead>
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<tr>
<td>Foreclosures</td>
<td>262%</td>
<td>262%</td>
<td>111%</td>
<td>379%</td>
<td>262%</td>
</tr>
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<td>Hiring Rate</td>
<td>28.3%</td>
<td>29.1%</td>
<td>28.3%</td>
<td>28.3%</td>
<td>26.78%</td>
</tr>
</tbody>
</table>

Foreclosure Elasticities with Hiring Rates

| $\gamma$ | -0.0133 | -0.0121 | -0.0219 | -0.0034 | -0.0164 |

Implied Aggregate Effects in Hiring Rate

| 10.45% | 10.09% | 9.38% | 4.24% | 16.06% |
Table 2: Baseline Effects of Foreclosures on the Labor Market

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>logged county-by-industry employment</th>
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</thead>
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<td>(1)</td>
</tr>
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</tr>
<tr>
<td>ln(foreclosures)</td>
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<td>R-squared</td>
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<td>Sample Size</td>
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<tr>
<td>F-statistic</td>
<td>96.9</td>
</tr>
<tr>
<td>Dep. var. =</td>
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</tr>
<tr>
<td>ln(foreclosures)</td>
<td>-.069***</td>
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<tr>
<td>R-squared</td>
<td>.51</td>
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<tr>
<td>Sample Size</td>
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<tr>
<td>F-statistic</td>
<td>96.9</td>
</tr>
<tr>
<td><strong>Panel C</strong></td>
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<td>ln(foreclosures)</td>
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<td>95.5</td>
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<td>Baseline Controls</td>
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<td>2-dig Industry FE</td>
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<td>Time FE</td>
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<tr>
<td>Sample?</td>
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<tr>
<td>Instruments?</td>
<td>No</td>
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</table>

Notes.—Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration, 2000-2014. The table reports the coefficients associated with regressions of two-digit industry logged employment, logged hiring, and the turnover rate on logged county foreclosures, logged mortgage payments due, and logged bank deposits, and demographic controls: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level.
Table 4: Examining the Correlation between ARMs and Income Fluctuations

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<th>7-1</th>
<th>10-1</th>
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<tr>
<td>ln(filers, under 10K)</td>
<td>-.020***</td>
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<td>-.010***</td>
<td>-.002</td>
<td>-.014***</td>
<td>-.003</td>
<td>-.003***</td>
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<td>-.001</td>
<td>.001</td>
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<td>ln(filers, 10-25K)</td>
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<td>.007</td>
<td>.010***</td>
<td>.011</td>
<td>-.003</td>
<td>-.010</td>
<td>-.001</td>
<td>.002</td>
<td>-.002**</td>
<td>.001</td>
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<tr>
<td>ln(filers, 25-50K)</td>
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<td>.030**</td>
<td>-.002</td>
<td>-.011</td>
<td>.025***</td>
<td>.015*</td>
<td>.008***</td>
<td>.009*</td>
<td>.003***</td>
<td>.001</td>
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<tr>
<td>ln(filers, 50-75K)</td>
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<td>-.019*</td>
<td>.003</td>
<td>.001</td>
<td>-.011***</td>
<td>.003</td>
<td>-.004***</td>
<td>.000</td>
<td>-.001</td>
<td>.001</td>
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<tr>
<td>ln(filers, 75-100K)</td>
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<td>-.003</td>
<td>.004</td>
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<td>-.009*</td>
<td>-.002*</td>
<td>.002</td>
<td>-.003***</td>
<td>-.000</td>
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<tr>
<td>ln(filers, 100-200K)</td>
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<td>-.007***</td>
<td>-.000</td>
<td>.015***</td>
<td>.004</td>
<td>.003***</td>
<td>-.006***</td>
<td>.003***</td>
<td>-.000</td>
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<tr>
<td>ln(filers, above 200K)</td>
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<td>-.005</td>
<td>-.001</td>
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<td>.001</td>
<td>-.003</td>
<td>.000</td>
<td>.001</td>
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</table>

Notes. - Sources: Internal Revenue Service, CoreLogic, Census, 2004-2007. The table reports the coefficients associated with regressions of the share of type-k adjustable rate mortgages (2-1, 3-1, 5-1, 7-1, and 10-1 ARMs) relative to total hybrid loans on a semi-parametric measure of logged number of filers by income bracket, conditional on controls. Demographic controls include: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Standard errors are clustered at the county-level and county population is used as the sample weight.
Table 5: Robustness Using County Exposure to Adjustable Rate Mortgage Resets

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<th>Dep. var. = logged county-by-industry employment</th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
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<td>ln(foreclosures)</td>
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<td>-.123***</td>
<td>-.150***</td>
<td>-.093***</td>
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<td>[.039]</td>
<td>[.038]</td>
<td>[.058]</td>
<td>[.042]</td>
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<td>.94</td>
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<td>Instruments?</td>
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Notes.—Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration, 2007-2014. The table reports the coefficients associated with regressions of two-digit industry logged employment, logged hiring, and the turnover rate on logged county foreclosures, logged mortgage payments due, and logged bank deposits, and demographic controls: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using a Bartik-like instrument that interacts a county’s exposure to a bank with the average interest rate resets on 5-1, 7-1, and 10-1 ARMs; we set the baseline period to 2006 for constructing county exposures. Standard errors are clustered at the county-level.
Figure 9: Perceptions of the Current State of the Economy with the Volatility Index

Sources.—St. Louis Fed and U.S. Gallup Daily Poll, 2008-2015. The figure plots the mean sentiment about the current state of the economy (1-4 scale) with the volatility index at a daily frequency.
Table 6: Foreclosure Shocks and Economic Sentiments

<table>
<thead>
<tr>
<th></th>
<th>ln(state of economy)</th>
<th>ln(future of economy)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
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<td>(2)</td>
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<tr>
<td>Panel A: Mean</td>
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<tr>
<td>ln(foreclosures)</td>
<td>-.10***</td>
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<td></td>
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<td>ln(housing price)</td>
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<td>Dep. var. =</td>
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<td>ln(future of economy)</td>
</tr>
<tr>
<td>Panel B: Standard Deviation</td>
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<td>ln(foreclosures)</td>
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<td>.01**</td>
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<td>Controls</td>
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<td>Yes</td>
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<tr>
<td>County FE</td>
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<tr>
<td>Time FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Income Controls</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes.–Sources: CoreLogic, Gallup, Federal Housing Administration, 2008-2014. Panel A reports the coefficients associated with regressions of individual logged sentiments (current state of the economy [1-4] and future state of the economy [1-3]) on logged county foreclosures, conditional on controls and fixed effects. Our controls include: 10 bins of fixed effects on housing price growth, logged local bank deposits, logged mortgage payments due, day of the week of the interview fixed effects, logged housing prices, and a set of individual covariates, including age, marital status, education fixed effects (no high school, high school, technical, some college, and college), and race. Our income controls include logged income (discrete twelve bins, which we average to create a continuous variable) and logged consumption expenditures on non-durables from the day before. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year level and include a cubic as instruments. Observations are weighted by sample weights Panel B reports the coefficients associated with the standard deviation of logged individual sentiments within a county × quarter on logged foreclosures, conditional on controls. Controls include: 10 bins of fixed effects on housing price growth, logged housing prices, logged local bank deposits, and logged mortgage payments due. Observations are weighted by the number of individuals observed in the county × quarter × year before computing the standard deviation. Standard errors are always clustered at the county-level.
Figure 10: Foreclosures and the Protracted Recovery

Notes.– Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration. The figure plots the number of months it takes for a county to recover to its 2006 level of employment starting from 2008 with the logged number of foreclosures between 2008-2011.

A Online Appendix (Not for Print)

A.1 Supplement to Introduction

We start with several institutional details. Through the Troubled Asset Relief Program (TARP), which authorized expenditures of $700 billion on October 3, 2008, the Making Home Affordable (MHA) program was designed to maintain home ownership and mitigate foreclosure rates. The Home Affordable Modification Program (HAMP) was the largest of these sub-programs in MHA, helping seriously delinquent borrowers renegotiate their loans to avoid foreclosure. The original authorization for HAMP was $50 billion as measured by the Government Accountability Office (2014) to help 3-4 million homeowners avoid foreclosure, but they later updated the assessment to include an additional $27.8 billion since 2009 (GAO, 2016).

Besides HAMP, four programs were included in the MHA program: (i) Principal Reduction Alternative, which helped homeowners with a loan-to-value (LTV) ratio over 115% make payments, (ii) Home Affordable Unemployment Program, which provided temporary relief for unemployed
Table 7: Examining the Impact of Foreclosures on Credit Among Small Businesses

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
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<tbody>
<tr>
<td>ln(share of bank guarantee)</td>
<td>-.038***</td>
<td>-.240***</td>
<td>.084***</td>
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<td>ln(loan volume)</td>
<td>[.005]</td>
<td>[.045]</td>
<td>[.012]</td>
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<td>ln(loan defaulted)</td>
<td>.183***</td>
<td>-3.688***</td>
<td>2.140***</td>
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<td>R-squared</td>
<td>.25</td>
<td>.37</td>
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<td>Sample Size</td>
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<td>F-statistic</td>
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<td>297</td>
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<td>Time FE</td>
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<tr>
<td>Instrument?</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes.– Sources: CoreLogic, Small Business Administration, Federal Housing Administration, 2000-2014. The table reports the coefficients associated with regressions of the share of the loan a bank guarantees (gross approval net of the SBA guarantee divided by the gross approval), logged loan volume, and an indicator for whether the loan went into default on logged zipcode foreclosures, controlling for mortgage payments due, bank deposits, fixed effects on ten bins of housing price growth, fixed effects on the business type, and zipcode and time (quarter and year) fixed effects. (The logit regressions do not contain zipcode fixed effects.) Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode × quarter level and include a cubic as instruments. Standard errors are always clustered at the zipcode-level.
Table 8: Local Foreclosure Shocks Controlling for Bank Balance Sheet Quality

<table>
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<td>ln(foreclosures)</td>
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<td></td>
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<td>ln(assets)</td>
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<td></td>
<td>[.001]</td>
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<td>ln(deposits)</td>
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<td>[.002]</td>
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<td>R-squared</td>
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<td>Time FE</td>
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</tr>
<tr>
<td>Instruments?</td>
<td>Yes</td>
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</table>

Notes.–Sources: CoreLogic, Small Business Administration, Federal Housing Administration, Call Reports, 2003-2013. The table reports the coefficients associated with regressions of the bank-level growth in commercial loans on logged county foreclosures, controlling for logged bank assets, logged bank deposits, mortgage payments due, fixed effects on ten bins of housing price growth, fixed effects on county and time (quarter and year). The sample is restricted to the set of banks that operate locally (no national operations). Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county × quarter level and include a cubic as instruments. Standard errors are always clustered at the county-level.
Table 9: Foreclosure Shocks and Net Migration Flows Across Skilled Brackets

<table>
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<tr>
<th>Dep. var. =</th>
<th>logged college employment net of ...</th>
<th>college employment growth net of ...</th>
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<td>ln(foreclosures)</td>
<td>-0.0308***</td>
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<tr>
<td>[0.0089]</td>
<td>[0.0005]</td>
<td>[0.0004]</td>
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<td>R-squared</td>
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<td>Year FE</td>
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<td>Yes</td>
</tr>
<tr>
<td>Instruments?</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes.—Sources: Census Bureau, CoreLogic, Federal Housing Administration, Longitudinal Employer-Household Dynamics, 2000-2014. The table reports the coefficients associated with regressions of the logged employment among college graduates net of non-college graduates (and separately for individuals with some college experience) on logged foreclosures, conditional on fixed effects and demographic controls: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. All observations are weighted by county population and standard errors are clustered at the county-level.

homeowners, (iii) Second Lien Modification Program, which helped bank servicers modify a second lien on a loan when a homeowner received a first lien modification through HAMP, and (iv) the Home Affordable Foreclosure Alternatives Program, which helped foreclosed homeowners transition from their old homes to new properties through a short sale or deed-in-lieu of foreclosure.

The introduction motivates our focus on foreclosures by quoting some of their time series properties, specifically their correlations with other macroeconomic aggregates. We now present these in Figure 11. While we clearly do not interpret these as causal, we simply point out that there was a fundamental shift in the correlation between foreclosures and major macroeconomic growth indicators from 2008 onward. In the introduction, we also discuss differences in employment declines among non-tradables and tradables firms. Using the LEHD, we plot the average employment growth rate for firms in these sectors in Figure 12. We define tradables firms according to the baseline definition in Mian and Sufi (2014) based on whether firms in the sector export internationally (Panel A), but these differences in employment declines are also robust to using their second definition based on geographic concentration (Panel B).
Figure 11: Macroeconomic Aggregates and Foreclosures, 1979-2015

Notes.—Sources: Mortgage Bankers Association and St. Louis Federal Reserve. The figure plots seasonally adjusted foreclosures started (share of homes) with the housing price index (1980 normalized to unity), the change in investment, the change in the unemployment rate, and the change in real (2009 base year) GDP.

Panel A: Narrow Definition of Tradables

Panel B: Broad Definition of Tradables

Figure 12: Employment Growth in Tradables and Non-Tradables, 1995-2015

Notes.—Sources: Longitudinal Employer-Household Dynamics. We define non-tradables and tradables sectors according to the baseline definition in Mian and Sufi (2014) based on the international classification in Panel A and based on the geographical concentration classification in Panel B.
We also briefly discuss employment growth across the firm size distribution. Using the LEHD, Figure 13 plots employment growth for four categories of firm size bins. The largest declines are among firms with 20-49 and 50-249 employees, whereas the smallest are for under 20 employees (not displayed) and over 250 employees. These descriptive statistics are consistent with the fact that very small businesses do not invest much and very large businesses have access to many forms of credit.

Figure 13: Employment Rate by Firm Size, 1998-2015

Notes.—Sources: Longitudinal Employer-Household Dynamics, 2000-2015. The figure plots the quarterly employment growth rate for firms of different sizes by weighting across state-level averages using 2000 state population shares.
Figure 14: Annual Employment & Hiring Growth, by Industry, 2000-2006 and 2008-2013

Notes.– Sources: Longitudinal Employer Household Dynamics (LEHD). The figure plots the average annual growth rate employment and hiring between 2000-2006 and 2008-2013 separately by two-digit industry

A.2 Data Construction

A.2.1 Small Business Administration (SBA) Loan-level Data

We use loan-level data on the SBA’s approval of loans through the 7(a) and 504 lending programs, which we obtained through a freedom of information act (FOIA) request, but has since then become publicly available.\textsuperscript{79} The 7(a) lending program has several features. First, loans have a maximum amount of $5 million and the SBA will guarantee up to 85% of the loan for those that are $150,000 or less and 75% for those over $150,000. Second, loans have different maturities.

\textsuperscript{79}https://www.sba.gov/about-sba/sba-performance/open-government/foia/frequently-requested-records/sba-7a-504-loan-data-reports
depending on the investments that the loan will be used to finance (e.g., working capital versus real estate). Third, the SBA sets a maximum interest rate that decreases in the amount of the loan, but increases with the maturity. Fourth, while the SBA does not directly assess loan risk, there are still basic standards the SBA uses to vet loans—for example, prior history of default can be used to reject a loan application. The Preferred Lender Program (PLP) is also a feature whereby the SBA delegates the credit decision and servicing to these PLP lenders. In the event of a default, the SBA will honor its commitments, but the PLP will have to liquidate all of its assets first. The 7(a) lending program also contains an additional sub-category of loans through the express loan program, which allows for a $350,000 maximum amount and 50% maximum SBA guaranty with approval decisions on the loans made within 36 hours.

Although the 504 lending program contains many similarities, there are also some substantive differences. First, the SBA offers loan guarantees up to $5.5 million, but a lender will typically cover 50% of the project and a Certified Development Company (CDC) will provide up to 40% of the financing; borrowers must provide at least 10%. The CDCs are maintained as nonprofits designed to promote local economic development through the distribution of the 504 loans. Similar to the 7(a) lending program’s PLP, the 504 program has the Premier Certified Lender Program (PCLP). All lenders must pay a fee and sign an eligibility form that states that they would not have been able to make the loan if it were not for the SBA assistance. We also refer readers to Brown and Earle (2017) who use the data in a separate context.

A.2.2 Census Bureau County Demographics

We use SocialExplorer as the primary source to extract Census demographic controls at a tract-level. Specifically, we use the 2000 decennial census, 2005-2009, and 2009-2013 5-year estimates to obtain the most comprehensive coverage over our main time series of interest. We cross-walk over tracts to zip-codes using the HUDS database. The 2000 and 2005-2009 year groups share the common 2000 Census codes (in HUDS, up to Q1 2012 contains 2000 Census codes), whereas the 2009-2013 year group uses the 2010 Census codes. To match the Census demographic controls to our crosswalk, we match one to many since there is only one observation per tract within a year group, but many potential zip-codes. Our measures include: race (fraction of individuals who are white, black), age (fraction of individuals within different age brackets), marital status, gender, marital status, gender, marital status, gender,
population, and education (fraction of individuals within different education brackets, i.e., less than high school, high school, some college, or college or more).

### A.2.3 Longitudinal Employer Household Dynamics (LEHD)

We use the publicly available version of the LEHD at the two-digit industry, county, and quarterly level (https://ledextract.ces.census.gov/static/data.html). We drop all employment and turnover cells that are missing or flagged as potentially inaccurate, but keep those cells that are flagged as more reliable imputations.

### A.2.4 CoreLogic Loan-level Data

We begin by providing a glimpse of the variation in foreclosures we observe. Figure 15 plots logged foreclosures and housing prices with their corresponding growth rates for 2004 and 2010, highlighting the massive pre and post differences over the Great Recession. While our housing price data is obtained from the Federal Housing Administration (FHA), we can see that counties exhibit significantly different outcomes during these periods.
**Figure 15**: Distribution of Foreclosures and Housing Prices, 2004 and 2010

*Notes.* Sources: CoreLogic and Federal Housing Administration. The figure plots the annual county logged number of foreclosures, the growth rate of foreclosures, housing prices, and the growth rate of housing prices.

### A.2.5 Gallup U.S. Daily Poll

A crucial feature of this data is that it contains local measures of sentiment, which help proxy for both local optimism and uncertainty. However, one concern is that the wording of the question (with reference to national economic conditions) makes it a poor proxy for perceptions of local economic activity. To examine this concern, we highlight the extent of the variation in the data. We first residualize our two measures of perceptions of current and future economic activity by regressing the z-scores on income bracket fixed effects, educational bracket fixed effects, age, marital status, gender, and race (black/white) to control for selection effects.

We subsequently plot the distribution of these variables pre and post the Great Recession pooling across metropolitan averages in Figure 16. Perhaps surprisingly, there is massive variation across locations, as well as over time, reflecting the fact that different areas experienced bigger economic shocks during the financial crisis. The fact that these distributions are so disbursed in...
both the cross-section and panel is consistent with the view that individuals respond more to local economic conditions versus the national ones.

Panel A: Perception of the Current State

Panel B: Perception of the Future State

Figure 16: Distribution of Sentiments about Current and Future Economic States

Notes. – Sources: Gallup U.S. Daily Poll, 2008-2016. The figure plots the distributions of residualized z-scores of perceptions of the current and future economic state, which are ranked on scales of 1-4 and 1-3, respectively. We residualize these variables by regressing on income and education fixed effects (four each), age, marital status, gender, and race. We subsequently average across individuals in a metropolitan area and plot the distribution across locations, restricting the set to areas with over 250 individual observations.

A.2.6 Classification of Tradables Sectors

Our main text discusses our rationale for working at a two-digit industry × county level of aggregation, rather than a deeper three-digit level. Unfortunately, the LEHD restricts data for counties with too few firms in a particular industry, introducing significant noise and measurement error into our estimating equation. In fact, if we compute the logged number of missing tradable observations net of the logged number of missing non-tradable observations at a county level, we find a high correlation with foreclosures even after controlling for our baseline covariates. It is, therefore, possible that working with this greater disaggregation would actually introduce much more bias. We now show that is the case.

Table 10 presents our baseline estimates pooling all observations and separating between the first classification of tradables from Mian and Sufi (2014). Although our main result that foreclosures have a negative effect on hiring remains in tact, we find opposite results for the tradables and non-tradables sectors, which reflects the significant measurement error we discussed above. One obvious reason for the correlation comes from Mian and Sufi (2014) that local housing shocks had a large impact on non-tradables. Since foreclosures and housing prices are non-linearly re-
lated, non-tradables will be less likely to be observed (since there will be more non-tradables firms that go out of business and, therefore, raise the number of missing observations at a county level. Figure 17 shows a similar result partitioning the Herfindahl index into ten bins and presenting the foreclosure elasticities separately for each.

**Table 10:** Foreclosure Heterogeneity in Tradables and Non-Tradables

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<tbody>
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<td>.019</td>
<td>-.124***</td>
</tr>
<tr>
<td></td>
<td>[.013]</td>
<td>[.035]</td>
<td>[.013]</td>
</tr>
<tr>
<td>R-squared</td>
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<td>.57</td>
<td>.80</td>
</tr>
<tr>
<td>Sample Size</td>
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<td>222570</td>
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<td>Time FE</td>
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</tbody>
</table>

*Notes.* Sources: Census Bureau, CoreLogic, Federal Housing Administration, Longitudinal Employer-Household Dynamics, 2000-2014. The table reports the coefficients associated with regressions of county × three-digit industry logged hiring on logged foreclosures for tradables and non-tradables using the first measure from Mian and Sufi (2014), conditional on logged housing prices, logged mortgage payments due, logged local bank deposits, the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education (brackets are less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. All observations are weighted by county population and standard errors are clustered at the county level.
Figure 17: Foreclosure Heterogeneity in Hiring, by Tradability Herfindahl Index

Notes. Sources: Census Bureau, CoreLogic, Federal Housing Administration, Longitudinal Employer-Household Dynamics, 2000-2014. The table reports the coefficients associated with regressions of county × three-digit industry logged hiring on logged foreclosures separately by the Mian and Sufi (2014) Herfindahl index of tradability geographic concentration, conditional on logged housing prices, logged mortgage payments due, logged local bank deposits, the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education (brackets are less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. All observations are weighted by county population and standard errors are clustered at the county-level.

A.3 Supplemental Evidence the Rise (and Fall) of Adjustable Rate Mortgages

A.3.1 Concentration of and Variation in Adjustable Rate Mortgages

We begin by characterizing the extent of variation. Since an important component of our identification strategy is that we have variation in the set of counties containing different types of ARMs and the same type of ARMs originated at different periods, we begin by plotting the distribution of the difference in 5-1 v. 7-1 and 5-1 v. 10-1 ARMs across counties in Figure 18. There is
significant variation in the relative proportions with some counties having nearly 10% more 5-1 ARMs than 7-1 ARMs, for example. We also plot the distribution of different types of ARMs in Figure 19, showing that there is also quite a bit of variation in the incidence of these mortgages across space.

![Figure 18: Difference in the Share of 5-1 v. 7-1 and 5-1 v. 10-1 ARMs](image)

**Notes.** Sources: CoreLogic, 2000-2014. The figure plots the difference between the share of 5-1 v. 7-1 and 5-1 v. 10-1 ARMs at a county-level, excluding counties with a zero share. Out of the 47,250 observations, 18,080 have differences of zero.
$$\text{Panel A: 2-1 and 3-1 ARMs}$$

$$\text{Panel B: 5-1 and 7-1 ARMs}$$

**Figure 19:** Distribution of 2-1, 3-1, 5-1, 7-1 ARM Shares

*Notes:* Sources: CoreLogic, 2000-2014. The figures plot the distribution of the share of different mortgage duration across counties, excluding counties with a zero share.

Figure 20 plots their distributions and, perhaps surprisingly, suggests a negative correlation of -0.34, meaning that banks with more 5-1 ARMs tend to use fewer 7-1 ARMs, and vice versa. In other words, banks tended to pick one or the other type of loan to focus on, meaning for instance that areas with historical concentrations of banks focusing on 5-1 loans would tend to get more of those types of loans as compared to other areas.
Figure 20: Distribution of Bank ARM Shares, by ARM Type

Notes. Sources: CoreLogic. The figure plots the distribution of bank shares for a particular type of adjustable rate mortgage between 2000 and 2014 with most of the mass between 2005 and 2008. If, for example, a bank makes 50 5-1 ARMs and 50 7-1 ARMs, then its share would be 0.50 for both.

A.3.2 Distribution of FICO Scores

While we focus on 5-1, 7-1, and 10-1 ARMs as a source of quasi-experimental variation in predicting foreclosures, we now examine a broader comparison across loan types and over time. We begin by plotting the distribution of mean FICO scores for 5-1 and 7-1 and 10-1 ARMs across zipcodes in Figure 21. Panels A and B distinguish between pre (2004-2007) and post (2008-2014) recession to examine the possibility that borrowers were asymmetrically targeted before versus after. Importantly, the distributions exhibit a significant degree of overlap. In fact, Figure 22 plots histograms across individuals, showing that the high degree of overlap in Figure 21 is not emerging due to a faulty aggregation issue, for example.

However, motivated by the vast evidence on low income targeting for 2-1 and 3-1 ARMs, we now explore the distribution of FICO scores across zipcodes for 2-1 & 3-1 ARMs versus the scores for 5-1, 7-1, and 10-1 ARMs. Figure 23 illustrates that there is a massive discrepancy between the two sets of ARM loans, suggesting that there is likely negative selection into 2-1 and 3-1 ARMs. Moreover, while the FICO score distribution for 5-1, 7-1, and 10-1 ARMs remains stable over
time, it shifts dramatically for 2-1 and 3-1 ARMs. Our focus on the more stable set of ARMs also provides greater reliability that our interest rate resets are not driven by composition changes.

We finally compare differences between fixed rate mortgages (FRMs) and ARMs. While FRMs have a mean FICO score of 702 and a median of 707 across zipcodes, and ARMs have a mean of 675 and median of 677, our comparison of FRMs with the specific set of 5-1, 7-1, and 10-1 ARMs in Figure 24 suggests that our ARMs actually exhibit larger FICO scores. In this sense, while there is negative selection into ARMs more generally, the set that we use for our instrument actually exhibits many more similarities to FRMs (mean 702 for FRMs and 725 for our set versus a standard deviation of 47 for FRMs and 55 for our set).


Panel B: 2008-2014

**Figure 21:** Comparison of Mean 5-1, 7-1, 10-1 FICO Scores Across Zipcodes

*Notes.*–Sources: CoreLogic. The figure plots the distribution of mean FICO scores for borrowers with 5-1 and the combination of 7-1 and 10-1 ARMs (which are even more similar) across zipcodes between 2004-2007 and 2008-2014. The figure shows that there is a high degree of overlap in both periods between the two distributions— that is, zipcodes with different sets of borrowers have a high degree of overlap.
Figure 22: Comparison of Mean 5-1, 7-1, 10-1 FICO Scores Across Individuals

Notes. – Sources: CoreLogic. The figure plots the distribution of mean FICO scores for borrowers with 5-1, 7-1, and 10-1 ARMs (which are even more similar) across individuals between 2004-2007 and 2008-2014. The individual data shows even more clearly that there is full overlap in the distribution of FICO scores across individuals in these different loans.

Figure 23: Comparison of Mean 2-3-1 and 5,7,10-1 FICO Scores Across Zipcodes

Notes. – Sources: CoreLogic. The figure plots the distribution of mean FICO scores for borrowers with an average of 2-1 & 3-1 ARMs with an average of 5-1, 7-1, and 10-1 ARMs across zipcodes between 2004-2007 and 2008-2014. The figure shows that many 2-1 and 3-1 ARMs were targeted towards low credit score borrowers and that the distribution shifted significantly following the crisis.

Panel B: 2008-2014

Figure 24: Comparison of Mean FRMs and 5-1, 7-1, 10-1 ARMs FICO Scores Across Zipcodes

Notes. Sources: CoreLogic. The figure plots the distribution of mean FICO scores for borrowers with fixed rate mortgages (FRMs) and 5-1, 7-1, and 10-1 ARMs across zipcodes between 2004-2007 and 2008-2014. The figure shows that there is a high degree of overlap in both periods between the two distributions—that is, zipcodes with different sets of borrowers have a high degree of overlap.

A.3.3 Rise of Adjustable Rate Mortgages

One concern is that dispersion in ARMs during our time series is endogenous—that is, some areas are more likely to have more ARMs based on their historical sequence of shocks. To the extent these shocks influence their ability to adapt to housing shocks during the Great Recession, our instrument’s exclusion restriction could be violated. We show that this is not the case by formally examining the correlation between the share of 5-1, 7-1, and 10-1 adjustable rate mortgages (ARMs), relative to the total supply of loans, between 2000-2001 with the growth rate of different demographic and economic characteristics from 1990 to 2000. Table 11 documents these results. These shocks provide no predictive power for understanding the dispersion in the share of ARMs.

A.3.4 Additional Loan and Foreclosure Descriptive Statistics

We begin by plotting the time series evolution of loan originations for each of the ARM categories and all of them together, illustrating that there is a significant run up between 2000 and 2006, followed by a large decline. These parallel in many ways the time series patterns in Figure 3 discussed in the main text. We see that originations of 2-1 and 3-1 loans collapsed to zero following the financial crisis, but originations of 5-1 and 7-1 ARMs generally remained afterwards. In this
Table 11: 5-1, 7-1, and 10-1 Adjustable Rate Mortgage Shares and 1990-2000 Growth Rates

<table>
<thead>
<tr>
<th>Dep. var. = share of 5-1, 7-1, 10-1 loans</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
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</thead>
<tbody>
<tr>
<td>income, 90-00 growth</td>
<td>.01</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[.01]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>housing price, 90-00 growth</td>
<td>.01</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[.01]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>college, 90-00 growth</td>
<td>-.00</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[.00]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>unemployment, 90-00 growth</td>
<td>.00</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[.00]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>R²</td>
<td>.00</td>
<td>.01</td>
<td>.00</td>
<td>.00</td>
</tr>
<tr>
<td>Sample Size</td>
<td>2786</td>
<td>2783</td>
<td>2781</td>
<td>2783</td>
</tr>
</tbody>
</table>

Notes.—Sources: CoreLogic and Census Bureau. The table reports the coefficients associated with regressions of the share of 5-1, 7-1, and 10-1 ARMs in 2003-2004, relative to total loans for the county, on the 1990-2000 growth rate of median housing prices (for specified owner-occupied houses), median household income, the unemployment rate, and the college share. Observations are weighted by the county’s 2000 population and standard errors are clustered at the county-level.

sense, the bulk of our variation is coming from the period leading up to the financial crisis and the resets that were happening until roughly 2010.
Figure 25: Hybrid Loan Originations, 2000-2014

Notes.—Sources: CoreLogic. The figure plots the total number of loan originations by hybrid adjustable rate mortgage (ARM) type between 2000 and 2014 at a quarterly frequency.

Figure 26 characterizes the variation we have at our disposal across ARM loan types. Starting with 2-1 ARMs, we see that all loan vintages experience an interest rate reset up—most of which are upward of 2%. The largest resets are among those originated in 2003 that were facing the height of the crisis between 2005-2006. Turning towards 3-1 ARMs, we see a similar interest rate
reset up among some loan vintages, but for those that were originated later the interest rates reset down. We observe a similar story for 5-1 ARMs where only two years experience an interest rate reset up, but following 2003 all loan originations (resets taking place from 2009 onward) experience resets down. All 7-1 ARMs also experienced resets down. Overall, these plots provide evidence of the significant heterogeneity in interest rate shocks among borrowers.
Figure 26: Median National Interest Rate Changes, by ARM Type

Notes. Sources: CoreLogic. The figures plot the median interest rate change for holders of ARMs based on the type of ARM and across all loan vintages between 2002 and 2008.
A.3.5 Foreclosure Spillovers

While there is already some evidence that foreclosures create spillovers on neighboring homes (Anenberg and Kung, 2014; Gupta, 2016), we now provide additional evidence that foreclosures specifically on 5-1, 7-1, and 10-1 adjustable rate mortgages (ARMs) are associated with foreclosures on fixed rate mortgages (FRMs). The underlying mechanism is based on contagion—when one foreclosure takes place, it generates a disamenity and/or price effect that influences neighboring homes. Figure 27 illustrates that foreclosures on ARMs are associated with foreclosures on FRMs when residualizing based on the usual county controls and location / time fixed effects.

**Figure 27:** Foreclosure Spillovers from Adjustable Rate Mortgages

*Notes.* Sources: CoreLogic, Census, Federal Housing Administration. The figure reports a scatterplot obtained by regressing residualized logged foreclosures on borrowers with fixed rate mortgages (FRMs) on residualized logged foreclosures on borrowers with 5-1, 7-1, and 10-1 adjustable rate mortgages (ARMs), controlling for county and year fixed effects, logged median home value per square foot, a quadratic in the total mortgage payments due, and a vector of demographics. Demographic controls include: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population.

However, one concern with Figure 27 is that it overestimates the effects of foreclosures on ARMs on foreclosures on FRMs because these counties simply are experiencing other negative shocks. While we have controlled for housing prices and other time-varying correlates, like mortgage expenditures, we now turn towards more parsimonious regressions at a zipcode level that exploit our plausibly exogenous variation arising from interest rate resets. In our preferred specification, we specifically regress the logged number of foreclosures on FRMs on the logged number
of foreclosures on ARMs, instrumenting for the ARMs, and controlling for housing price growth, logged mortgage payments, and both zipcode and year fixed effects.

Table 12 documents these results. Beginning with our least squares estimate in column 1, we find that a 10% rise in ARM foreclosures is associated with a 6.14% rise in foreclosures on FRMs in a zipcode. As expected, housing price growth is robustly associated with a large decline in the number of foreclosures since rising property values make it easier for individuals to take out equity in their homes, refinance, or sell. Loan payments also is perhaps surprisingly associated with increases in foreclosures, which may reflect the fact that foreclosures were more common in areas that had originated many loans. Including zipcode and year fixed effects in column 2 reduces the magnitude of our estimate by roughly a half, reflecting the fact that areas with more foreclosures on FRMs are lower quality areas that also experienced increases in foreclosures on ARMs.

Turning towards our preferred estimates in column 3, our instrumental variables specification suggests that a 10% rise in foreclosures on ARMs is associated with a 5.87% rise in foreclosures on FRMs. Our estimate is greater than that obtained from the OLS, but larger than the FE estimate, which suggests that dynamic selection and reverse causality may have been offsetting each other to varying extents. Recognizing that foreclosures may have been generated in part by housing labor market declines (e.g., plant closures), we implement two exercises. First, column 4 includes county × year fixed effects to focus specifically on local variation in interest rate resets on ARM borrowers. Second, column 5 uses the zipcode business patterns data to control for logged employment and establishments. Perhaps surprisingly, our estimate in column 5 is statistically indistinguishable from our baseline in column 3. These results are closely related with the evidence of spillovers from Anenberg and Kung (2014) and Gupta (2016).

As an additional placebo, we examine how the elasticity under our IV specification varies across the distribution of counties with ARMs. We focus on counties, rather than zipcodes, since our main results are at a county level and we have already demonstrated the presence of these spillovers at a granular level. We specifically compute the share of ARMs in foreclosure relative to total foreclosures between 2006 and 2010. We subsequently estimate the elasticity separately for each group, pooling across counties within the group. Figure 28 plot these estimated coefficients, illustrating that the spillovers are only present in counties with a higher proportion of ARM foreclosures, which is where spillovers should emerge.
Table 12: Evidence of Foreclosure Spillovers from ARMs to FRMs

<table>
<thead>
<tr>
<th></th>
<th>Dep. var. = ln(foreclosures on FRMs)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) (2) (3) (4) (5)</td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>.61*** .30*** .59*** .76*** .59***</td>
</tr>
<tr>
<td></td>
<td>[.00] [.00] [.01] [.02] [.01]</td>
</tr>
<tr>
<td>Δ HPI</td>
<td>-1.76*** -.96*** -.16*** .12*** -.14***</td>
</tr>
<tr>
<td></td>
<td>[.03] [.02] [.03] [.04] [.03]</td>
</tr>
<tr>
<td>ln(loan payments)</td>
<td>.12*** .21*** .12*** .27*** .13***</td>
</tr>
<tr>
<td></td>
<td>[.00] [.01] [.01] [.02] [.01]</td>
</tr>
<tr>
<td>ln(employment)</td>
<td>-.02</td>
</tr>
<tr>
<td></td>
<td>[.01]</td>
</tr>
<tr>
<td>ln(establishments)</td>
<td>-.04*</td>
</tr>
<tr>
<td></td>
<td>[.02]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.48 .81 .79 .83 .79</td>
</tr>
<tr>
<td>Sample Size</td>
<td>178208 178201 178201 166531 175075</td>
</tr>
<tr>
<td>F-statistic</td>
<td>2788 858 2776</td>
</tr>
<tr>
<td>Zipcode FE</td>
<td>No Yes Yes Yes Yes</td>
</tr>
<tr>
<td>Year FE</td>
<td>No Yes Yes Yes Yes</td>
</tr>
<tr>
<td>County x Year FE</td>
<td>No No No Yes No</td>
</tr>
<tr>
<td>Instruments?</td>
<td>No No Yes Yes Yes</td>
</tr>
</tbody>
</table>

Notes.—Sources: CoreLogic, Census, Federal Housing Administration, Zipcode Business Patterns, 2000-2014. The table reports the coefficients associated with regressions of logged foreclosures among borrowers with fixed rate mortgages on logged foreclosures among borrowers with adjustable rate mortgages, conditional on controls. Column 5 also includes logged employment and establishments in a zipcode. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the zipcode-level.
Figure 28: Elasticity of Foreclosure Spillovers between ARMs and FRMs

Notes.–Sources: CoreLogic, Census, Federal Housing Administration, 2000-2014. The figure reports the coefficients associated with regressions of logged foreclosures among borrowers with fixed rate mortgages on logged foreclosures among borrowers with adjustable rate mortgages separately by group (where each group is based on the share of foreclosures on ARMs to total foreclosures between 2006-2010 in a county), conditional on controls. These controls include: logged housing prices (index with 2000 base year), logged mortgage payments due, and logged bank deposits, the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level.

A.4 Supplemental Evidence on Main Results

A.4.1 Replication of Mian and Sufi (2014)

The central contribution of Mian and Sufi (2014) was the illustration that housing wealth shocks, measured as the change in county-level housing prices, largely affected the non-tradables sector. We begin by plotting employment growth and housing price growth at the county-level between 2006-2010 for non-tradables and tradables separately in Figure 4. As in Mian and Sufi (2014), the gradient between employment growth and housing price growth in the non-tradables sector is statistically larger than the gradient in the tradables sector: a one percentage point rise in housing price growth is associated with a 0.24 percentage point rise in employment in the non-tradables...
sector, whereas it is associated with only a 0.05 percentage point rise in the tradables sector.

\[ y_{ict} = \beta X_{ct} + \gamma \Delta h_{ct} + \eta_i + \psi_c + \lambda_t + \epsilon_{ict} \]

where \( y \) denotes the outcome (logged employment and the growth in employment), \( X \) denotes county demographics, \( \Delta h \) denotes housing price growth, and \( \eta, \psi, \) and \( \lambda \) are two-digit industry, county, and year/quarter fixed effects. Table 13 documents these results. Beginning with logged employment as the outcome variable, a one percentage point rise in housing prices is associated with a large 0.36% rise in employment. The inclusion of fixed effects reduces the estimate to 0.16. However, separating the observations into non-tradables and tradables sectors produces heterogeneous coefficients of 0.16 and 0.27. Turning towards employment growth as the outcome variable, a one percentage point rise in housing prices is associated with a 0.04 percentage point rise in employment.
rise in employment growth. The inclusion of fixed effects reduces the magnitude marginally to 0.03. We do not find significant heterogeneity between non-tradables and tradables sectors here.

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>logged employment</th>
<th>employment growth</th>
</tr>
</thead>
<tbody>
<tr>
<td>ALL</td>
<td>ALL</td>
<td>NTR</td>
</tr>
<tr>
<td>∆ ln(housing price)</td>
<td>.36***</td>
<td>.16***</td>
</tr>
<tr>
<td>[.06]</td>
<td>[.02]</td>
<td>[.02]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.58</td>
<td>.89</td>
</tr>
<tr>
<td>Sample Size</td>
<td>1759046</td>
<td>1759046</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry FE</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes. Sources: Longitudinal Employer Household Dynamics (LEHD), Census, Federal Housing Administration. The table reports the coefficients associated with regressions of two-digit industry logged employment and employment growth on housing price growth (index with 2000 base year). Columns 1 and 2 are on the pooled sample; columns 3 and 4 are on the non-tradables and tradables sectors, respectively, based on Mian and Sufi (2014) classification. Controls include: the fraction of individuals in the county who are male, married, between ages \( k \in [k, F] \) where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education \( k \in [k, F] \) where the brackets are less than high school, only high school, some college, college, and graduate school, race (white and black), and logged total population. Standard errors are clustered at the county-level and county population is used as the sample weight.

A.4.2 First-stage Correlation by Year

Figure 30 begins by plotting the residualized realized and predicted foreclosures (based on the interest rate resets in the two approaches), displaying a high correlation between the two.
Figure 30: First-stage Partial Correlation of Instruments and Foreclosures

Notes.– Sources: Longitudinal Employer-Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration. The figure reports a scatterplot obtained by partitioning residualized logged foreclosures into 1000 bins and separately plotting it against the baseline loan-level instruments (2000-2014) and the supplementary Bartik-like instruments (2007-2014). Controls include county, industry, year, quarter fixed effects, logged median home value per square foot, a quadratic in the total mortgage payments due, and a vector of demographics. Demographic controls include: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population.

One concern is the fact that the collapse of the loan market might have led to truncation in the set of loan originations, weakening the predictive power of our instrument in certain years and capturing other time-varying shocks that are correlated with local labor markets. Figure 31 examines this concern by plotting the average correlation between our residualized instrument—that is, foreclosures predicted by the interest rate resets on 5-1, 7-1, and 10-1 ARMs—and residualized foreclosures by year. We see that the correlation is quite constant and significant throughout the sample series and does not show signs of weakening after the Great Recession.
A.4.3 Comparison of Tradables and Non-Tradables

Given that we find heterogeneous effects of foreclosures on labor market outcomes in the tradables and non-tradables sectors, we explore potential sources of these differences. As we discussed in the main text, we interpret the stronger gradient as consistent with the flight to quality channel since the option value of deferring investment in an asset is increasing in the value of the asset, which in our case is labor. There are various ways to illustrate differences in labor quality, but perhaps one of the most straightforward approaches is to compare average earnings between workers in the non-tradables and tradables sectors.

Figure 32 plots the distribution of annual labor income for employed workers in the tradables and non-tradables sectors, which is a classification obtained from Mian and Sufi (2014) at the three-digit NAICS level. Logged annual earnings is 10.69 in the tradables sector, whereas it is 10.40 in the non-tradables sector, totaling a difference of roughly 33% \( (1 - \exp(10.69 - 10.40)) \).\(^{81}\) Individuals in

\(^{81}\)We also matched at a four-digit level, but the results were statistically indistinguishable (logged annual earnings for the tradables was 10.67 on average versus 10.69). Three-digit NAICS codes allowed us to retain a larger sample since some individuals to not report more detailed industry classifications in the census. Interestingly, however,
the tradables sector also work longer hours—2161 annual hours versus 1998 among non-tradables workers. The share of individuals working over $75,000/year is also much larger in the tradables sector—23.4% versus 14.4% in the non-tradables sector.

**Figure 32:** Comparison of Earnings Distribution, Tradables and Non-Tradables

*Notes.* Sources: Census Bureau, 2000, 2005-2014. The figure plots the distribution of logged annual labor income deflated using the 2010 personal consumption expenditure index between the tradables and non-tradables sectors at a three-digit classification obtained from Mian and Sufi (2014).

### A.5 Supplemental Evidence on Main Results and Robustness Exercises

#### A.5.1 Housing Prices Across Zipcodes

The main text documented a robust negative relationship between logged foreclosures and housing prices in logs and growth rates. We begin by examining heterogeneity in the elasticity. Figure 33 examines the foreclosure elasticity with housing price growth across deciles of the employment share in non-tradables. Motivated by prior evidence on the collateral channel (Adelino et al., 2015b; Mian and Sufi, 2014), we find that the elasticity is greatest in zipcodes with larger shares in the non-tradables sector. For example, in zipcodes with low non-tradables shares, the elasticity of college attainment is 33% in the tradables sector versus 36% in the non-tradables sector. But, this masks greater dispersion in the tradables sector, which has a standard deviation of 2.72 for average years of schooling versus 2.54 in the non-tradables sector.
is roughly -0.05, whereas for zipcodes with high non-tradables shares, it is nearly double.

Figure 33: Foreclosure Gradient on Housing Price Growth, by Employment Share in Non-tradables

Notes.—Sources: CoreLogic, Federal Housing Administration, 2000-2014. The table reports the coefficients associated with regressions of year-to-year housing price growth (normalized to 2000) on logged foreclosures at the zipcode-level, conditional on logged loan payments due and both zipcode and year fixed effects. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode × year level and include a cubic as instruments. Standard errors are clustered at the zipcode-level.

We subsequently compare our results more closely with Mian et al. (2015) who use foreclosures per homeowner; we use foreclosures per open mortgage, which is effectively the same. Table 14 documents these results. Our least squares estimator suggests that a one percentage point rise in foreclosures per open mortgage is associated with a 3.56% and 3.90pp decline in housing prices and housing price growth, respectively. The magnitude grows to -4.39% in the case of logged housing prices and declines to -1.77pp in the case of housing price growth once we add fixed effects. In our preferred instrumental variables specification, we find that a pp rise in foreclosures is associated with a 6.85% and 7.45pp decline in housing prices and housing price growth, respectively.

To compare properly with Mian et al. (2015), we focus on our estimates for housing price growth. Whereas they find that a pp rise in foreclosures per homeowner is associated with roughly a 2.2pp decline in housing price growth (significant at the 10% level), we find that it is closer to
7.45pp. What explains these differences? First, we are using different identification strategies. Their instrument contains only cross-sectional variation. As they discuss, the cross-sectional variation has limitations (e.g., strength of the first stage). Second, states with differences in judicial status laws also produce heterogeneous incentives for banks to foreclose on individuals, which could be a source of upwards bias.

**Table 14: Robustness of Foreclosures on the Housing Market**

<table>
<thead>
<tr>
<th>Dep. var. = ln(housing price)</th>
<th>housing price growth</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>foreclosures/mortgage</strong></td>
<td>-3.56***</td>
</tr>
<tr>
<td>ln(mortgage payments)</td>
<td>.05***</td>
</tr>
<tr>
<td>[.00]</td>
<td>[.00]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.10</td>
</tr>
<tr>
<td>Sample Size</td>
<td>190991</td>
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<tr>
<td>F-statistic</td>
<td>737</td>
</tr>
<tr>
<td>Zipcode FE</td>
<td>No</td>
</tr>
<tr>
<td>Year FE</td>
<td>No</td>
</tr>
<tr>
<td>State x Year FE</td>
<td>No</td>
</tr>
<tr>
<td>Instruments?</td>
<td>No</td>
</tr>
</tbody>
</table>

Notes.–Sources: CoreLogic, Federal Housing Administration, 2000-2014. The table reports the coefficients associated with regressions of logged housing prices (normalized to 2000) and housing price growth on foreclosures per open mortgage (a percent), conditional on logged loan payments due and fixed effects. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode x year level and include a cubic as instruments. Standard errors are clustered at the zipcode-level.

### A.5.2 Inequality Outcomes Across Counties

Motivated by the main results between foreclosures and the labor market, we now study whether it had similar effects on intra-county income inequality. Before turning to our results, we begin by noting that the theoretical impact is ambiguous ex-ante. On one hand, a decline in employment and labor market dynamism may compress the income distribution for everyone. On the other hand, foreclosures could specifically target one group of individuals. For example, one may suspect, based on the evidence from Mian et al. (2013) that poorer and more levered households experienced larger reductions in credit limits, that the effects of foreclosures were isolated on the poor. Conversely, home ownership overall displays a somewhat positive correlation with income, meaning that larger numbers of low income individuals never had homes in the first place that
could be foreclosed upon. Moreover, whether or not inequality rises within a local labor market in response to foreclosure shocks will depend crucially on the degree of inter-sectoral spillovers.

Table 15 documents the results of our analyses on income inequality outcomes. While not reported, we find that the unconditional correlation between foreclosures and income inequality is positive—areas a 1% rise in foreclosures is associated with a 0.0054% increase in the Gini coefficient.\textsuperscript{82} However, once we introduce basic demographic controls, the correlation is unambiguously negative (see columns 1 and 5). Once we add fixed effects, the coefficient drops to a very precise zero. Given that the $R$-squared is 0.98, one possibility is that there is simply too little variation in these measures of income inequality over the financial crisis.

Turning towards our instrumental variables estimates in columns 3-4 and 7-8, we find a potentially negative association between foreclosures and dispersion in income. However, our first-stage $F$-statistic is very low and our concern is that it runs into a weak instrument problem. One rationale for this arises from the fact that inequality is a longer-run phenomenon, meaning that the short term interest rate shocks used to identify the causal effects of foreclosures do little to push a county into higher versus lower income inequality. We have also experimented with regressions where we use zipcode level data to compute measures of the Gini coefficient over three periods—2000, 2005-2009, and 2010-2014—which we regress on foreclosures, conditional on a rich set of demographic controls (including the unemployment rate), and our results are unchanged.

These results are important in light of recent descriptive evidence among both sociologists (Rugh and Massey, 2010; Dymski et al., 2010) and popular press.\textsuperscript{83} While low income earners were more likely to receive sub-prime loans preceding the Great Recession, existing papers have not distinguished between self-selection into these loans versus strategic targeting. For example, Rugh and Massey (2010) do not include fixed effects in any of their metro-level regressions and their instrument of inter-metro variation in racial differentials is correlated with many other time-varying unobservables, such as the composition of industries in an area and mobility patterns. To the extent that these groups differ in their human capital or match quality, then race differentials are also endogenous. The fact that we discover a negative correlation between intra-county income inequality and foreclosures under our semi-parametric, fixed effects, and instrumental variables regressions helps to address this gap in the literature.

\textsuperscript{82}Given that the mean Gini coefficient is 0.435, then even if the unconditional correlation were an unbiased estimate of the treatment effect, the fact that the coefficient is so small suggests that foreclosures, even if they are positively associated with inequality, play almost no economically significant role in accounting for the rise in intra-county inequality during the Great Recession.

\textsuperscript{83}For example, see http://www.huffingtonpost.com/ray-brescia/when-the-rich-get-risky-i_b_695535.html.
Table 15: Baseline Estimates of Foreclosure Shocks on Inequality

<table>
<thead>
<tr>
<th></th>
<th>s.d. ln(total income)</th>
<th>s.d. ln(AGI)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) (2) (3) (4)</td>
<td>(5) (6) (7) (8)</td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>-0.05*** -0.0 -0.02 -0.01</td>
<td>-0.05*** -0.0 -0.03** -0.02***</td>
</tr>
<tr>
<td></td>
<td>[.01] [.00] [.02] [.01]</td>
<td>[.01] [.00] [.01] [.01]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.13 .98 .98 .98</td>
<td>.12 .98 .98 .98</td>
</tr>
<tr>
<td>Sample Size</td>
<td>22844 22843 22843 22844</td>
<td>22844 22843 22843 22844</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes Yes Yes Yes</td>
<td>Yes Yes Yes Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>No Yes Yes Yes</td>
<td>No Yes Yes Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>No Yes Yes Yes</td>
<td>No Yes Yes Yes</td>
</tr>
<tr>
<td>Housing Controls</td>
<td>No Yes Yes No</td>
<td>No Yes Yes No</td>
</tr>
</tbody>
</table>

Notes. – Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration, 2000-2014. The table reports the coefficients associated with regressions of the standard deviation of logged wage income and adjustable gross income on logged county foreclosures, logged housing prices (index with 2000 base year), logged mortgage payments due, and logged bank deposits, and demographic controls: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level.

A.5.3 Heterogeneous Effects

While the main text presented heterogeneous treatment effects of Equation 4 separately for non-tradables / tradables sectors and non-judicial / judicial states, we turn towards four additional sources of potential heterogeneity. However, before turning to these sources of heterogeneity, we first estimate our baseline specification separately by industry to highlight the fact that there is a negative association between hiring and foreclosures throughout all industries. Figure 34 shows that the negative association is heavily influenced by the manufacturing sector, but even other sectors (e.g., finance, professional services, and information) that might typically fall under a broader definition of tradables experience a strong negative association. The only industry that does not respond much to foreclosures is arts and entertainment.
We now turn towards these other sources of heterogeneity. First, given that firms of different sizes are likely to be heterogeneously dependent on external financing from local banks, we estimate our foreclosure gradients separately by firm size, displayed in Figure 35. We find the weakest gradients on the smallest (0-19 employees) and largest (500+ employees) firms, which reflects the fact that small companies make small investments and large companies have access to more diverse capital markets. In contrast, medium size companies between 50 and 499 employees are affected the most: a 10% increase in foreclosures is associated with between 1.8-2.2% lower employment and 2.8-3.2% lower hiring among these firms. While we can reject the null that these gradients are equal to zero, our confidence intervals for firms with 250-499 employees are large.
Panel A: ln(Employment) Coefficients

Panel B: ln(Hiring) Coefficients

Figure 35: Heterogeneity in Foreclosure Gradients, by Firm Size

Notes.—Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration, 2000-2014. The figures report the coefficients (separately by firm size) associated with regressions of logged county employment and hiring on logged county foreclosures, logged housing prices (index with 2000 base year), logged mortgage payments due, and logged bank deposits, and demographic controls: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level.

Second, we turn towards heterogeneity in a county’s employment share in construction jobs prior to the Great Recession. Motivated by evidence from Charles et al. (2016) that the booming construction sector masked a broader decline in employment prior to the Great Recession, we now ask whether the effects of foreclosures were stronger in areas with more workers employed in the construction sector. We fix employment shares according to the 2000 Census to avoid the simultaneity arising from the run-up of employment in the sector and housing prices between 2004 and 2007. We display these estimates in Figure 36. Perhaps counter intuitively, we do not find evidence of heterogeneity. One reason for this is arises from the fact that heterogeneity in construction employment sectors affects real outcomes is through housing prices, rather than foreclosures. Evidence consistent with this view is found in Figure 37, which plots the gradients on housing price growth (not controlling for foreclosures) across different employment share brackets, suggesting that there is heterogeneity along housing shocks.
Figure 36: Heterogeneity in Foreclosure Gradients, by County Construction Share

Notes. Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration, 2000-2014. The figures report the coefficients (separately by counties with different employment shares in the construction sector) associated with regressions of logged county employment and hiring on logged county foreclosures, logged housing prices (index with 2000 base year), logged mortgage payments due, and logged bank deposits, and demographic controls, and fixed effects on county, year, quarter, and industry. Controls include: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level.

Figure 37: Heterogeneity in Housing Price Gradients, by County Construction Share

Notes. Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration, 2000-2014. The figures report the coefficients (separately by counties with different employment shares in the construction sector) associated with regressions of logged county employment and hiring on housing price growth, logged mortgage payments due, and logged bank deposits, demographic controls, and fixed effects on county, year, quarter, and industry. Controls include: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Standard errors are clustered at the county-level.
Third, motivated by evidence that the rise of mortgage defaults were concentrated in sub-prime zip-codes (Mian and Sufi, 2009), we examine the potential for heterogeneity across counties’ average household income from 2000.\textsuperscript{84} Figure 38 documents these results. While we find some evidence of heterogeneity with low income counties between $12,000-38,300 in annual household income having very small foreclosure gradients—reflecting the fact that housing prices are low and many residents are renters—our foreclosure gradients across the second, third, and fourth quartiles are statistically indistinguishable from one another. One reason for the similarities arises from the fact that our gradients are identified off of variation in interest rate resets on 5-1, 7-1, and 10-1 ARM holders, which tend to be wealthier borrowers. In this sense, the homogeneity might be a result of obtaining a local average treatment effect.

\begin{figure}
\centering
\begin{subfigure}{0.4\textwidth}
\centering
\includegraphics[width=\textwidth]{PanelA.png}
\caption{Panel A: ln(Employment) Coefficients}
\end{subfigure}\hspace{1cm}
\begin{subfigure}{0.4\textwidth}
\centering
\includegraphics[width=\textwidth]{PanelB.png}
\caption{Panel B: ln(Hiring) Coefficients}
\end{subfigure}
\caption{Heterogeneity in Foreclosure Gradients, by County Household Income}
\end{figure}

Notes.-- Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration, 2000-2014. The figures report the coefficients (separately by average 2000 household income) associated with regressions of logged county employment and hiring on logged county foreclosures, logged housing prices (index with 2000 base year), logged mortgage payments due, and logged bank deposits, and demographic controls, and fixed effects on county, year, quarter, and industry. Controls include: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level.

Fourth, we explore heterogeneity in the extensive margin effects of foreclosures on different sizes of establishments. Motivated by evidence from Patnaik (2016) that smaller firms were the most adversely affected by the credit crunch of the Great Recession, we examine whether foreclosures

\textsuperscript{84}There is, however, controversy over the role that sub-prime mortgages played in accounting for the overall decline in consumption and foreclosures; see, for example Adelino et al. (2016).
had a larger effect on the closure of establishments among smaller versus larger firms. In particular, smaller firms might be less equipped to handle foreclosure shocks since they cannot re-allocate resources from one branch to another—that is, to stop expanding in one location that experiences a foreclosure shock and re-allocate resources for expansion to another location that was not hit by a shock of similar magnitude.

Figure 39 subsequently examines the effects of foreclosures on the number of establishments by establishment size. We find that establishments with between 250-999 employees are the ones that experience the greatest number of closures, but we also find relatively large gradients on establishments between 10 and 99 employees. Even though we find small foreclosure gradients with respect to employment and hiring among firms with under 20 employees, the extensive margin effects on them might be larger—that is, perhaps they do not adjust by reducing employment, but rather by shutting down entirely.

Figure 39: Foreclosures and Establishment Closures, by Establishment Size

Notes. Sources: County Business Patterns, CoreLogic, Census, Federal Housing Administration, 2000-2014. The figures report the coefficients associated with regressions of logged number of establishments in different establishment size bins on logged county foreclosures, logged housing prices (index with 2000 base year), logged mortgage payments due, and logged bank deposits, and demographic controls, and fixed effects on county, year, quarter, and industry. Controls include: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level.
The fourth dimension of heterogeneity is in the potential asymmetric effects of foreclosures during housing booms versus busts. Letting $1[\Delta h < 0]$ denote an indicator for whether a county is in a housing bust, we now consider

$$y_{jct} = f(X_{ct}, \beta) + \gamma_1 f_{ct} + \gamma_2 [\Delta h_{ct} < 0] + \delta (f_{ct} \times 1[\Delta h_{ct} < 0]) + \eta_j + \psi_c + \lambda_t + \epsilon_{jct}$$  \hspace{1cm} (6)

where the primary coefficient of interest is now $\delta$, which characterizes how foreclosures affect labor market outcomes during a housing bust. If foreclosures amplify the adverse effects of housing market downturns, we expect $\delta > 0$. However, it is possible that, while housing price declines lower employment, foreclosures may lower employment less during busts, i.e. $\delta < 0$, if housing price declines make foreclosures less likely (since individuals are more likely to pay their mortgage).\footnote{Since housing price growth is endogenous, we have also examined results instrumenting for not only foreclosures, but also housing price growth using the Saiz (2010) instrument.}

We estimate Equation 6 separately for firms in the non-tradable and tradable sectors. For the set of firms in the non-tradable sector, we find that $\gamma_1 = -0.078$, $\gamma_2 = -0.051$, and $\delta = 0.011$, whereas for the set of firms in the tradable sector, we find that $\gamma_1 = -0.173$, $\gamma_2 = -0.137$, and $\delta = 0.03$, all of which are significant at the 1% level. In this sense, we find evidence that housing market declines make the gradient of foreclosures on employment less severe since individuals are more likely to be able to pay and negotiate with a repayment plan on their home.

### A.5.4 Examining the Potential for Reallocation

One of the concerns with our aggregation exercise is that it ignores general equilibrium effects. For example, if an increase of foreclosures in county $c$ reduces employment in county $c$, it is possible that neighboring counties reap the benefits of new entrants. In a world with full reallocation, the aggregate effects may be much smaller, although there would still be distributional considerations. However, we can test the potential for reallocation more explicitly by estimating the following

$$\text{JobGrowth}_{-c,t} = f(X_{ct}, \beta) + \gamma f_{ct} + g(h_{ct}, \theta) + \phi_c + \lambda_t + \epsilon_{ct}$$ \hspace{1cm} (7)

where $-c$ denotes the neighboring counties to county $c$ obtained using the Census Bureau’s adjacent counties file, $\text{JobGrowth}$ denotes job growth (normalized to the county’s prior quarter job growth), $f(\cdot)$ and $g(\cdot)$ denote the usual semiparametric controls, $\phi$ and $\lambda$ are the usual county
and time fixed effects. We estimate Equation 7, asking whether an increase in foreclosures in county \( c \) raises neighboring county job flows. We measure job growth in two ways: a weighted average (based on population) and the maximum job growth in neighboring counties. The latter captures the fact that laid off workers in county \( c \) might be more likely to move to the neighboring county with higher job growth.

Table 16 documents these results. Columns 1 and 3 only present the least squares estimator, whereas columns 2 and 4 present the instrumental variables results with the interest rate resets. If anything, we find that increases in foreclosures in county \( c \) reduce job growth in neighboring counties—both the weighted average of surrounding counties and in the county with the maximum job growth nearby. As usual, we are controlling for housing prices, mortgage payments that come due, bank deposits, and demographic characteristics. The fact that we find a negative association between the two suggests the presence of negative, not positive (reallocation), spillovers.

Table 16: Examining the Potential for Reallocation

<table>
<thead>
<tr>
<th>Dep. var. = job growth, normalized mean</th>
<th>job growth, normalized maximum</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>-0.00**</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.06</td>
</tr>
<tr>
<td>Sample Size</td>
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</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>No</td>
</tr>
<tr>
<td>Time FE</td>
<td>No</td>
</tr>
</tbody>
</table>

Notes.—Sources: CoreLogic, Census, Federal Housing Administration, Longitudinal Employer-Household Dynamics, 2005-2011. The table reports the coefficients associated with regressions of job growth in neighboring counties (using the Census adjacent county file) on logged foreclosures. The controls include: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. The sample consists of three year groups: 2005-2007, 2008-2010, and 2011-2013 obtained through SocialExplorer, which is linked with the American Community Survey. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level and county population is used as the sample weight.

A.5.5 Robustness and Exclusion Restriction

We now examine several robustness exercises that were discussed in the main text. Our second exercise uses a selection on unobservables test to gauge the potential impact that other confounders might have on our baseline estimates. We use the strategy introduced by Oster (forthcoming).
While these approaches to partial identification typically are challenged by the fact that there is too much residual variation to bound, our setting allows us to do so well given that our $R$-squared is upward of 0.80 in many of our employment and hiring regressions. If, for example, we assume that, selection on unobservables cannot be more than 20% of the selection on observables, then our partially identified estimates from our IV specification are guaranteed to be consistent since there is no more residual variation left to explain.

Table 17 documents these results. Across each specification, we see that there are strong negative effects of foreclosures. In fact, the nature of the specification test from Oster (forthcoming) suggests that we are likely to be underestimating the negative effects of foreclosures due to, potentially, negative spillover general equilibrium effects. For example, in a number of cases, our upper bound (in magnitude) contains elasticities close to one, as in the case for the tradables sector. While we do not take these estimates as causal, our only point is that allowing for selection on unobservables appears to, if anything, reinforce our results through the magnitude of the bounds.

<table>
<thead>
<tr>
<th>Outcome = ln(employment)</th>
<th>ln(hiring)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ALL</td>
</tr>
<tr>
<td>$\delta = 0.10$</td>
<td></td>
</tr>
<tr>
<td>upper</td>
<td>-0.510</td>
</tr>
<tr>
<td>lower</td>
<td>-0.065</td>
</tr>
<tr>
<td>$\delta = 0.20$</td>
<td></td>
</tr>
<tr>
<td>upper</td>
<td>-0.750</td>
</tr>
<tr>
<td>lower</td>
<td>-0.065</td>
</tr>
</tbody>
</table>

Table 17: Partial Identification of Employment and Hiring Results

Notes. Sources: CoreLogic, Census, Federal Housing Administration, Longitudinal Employer-Household Dynamics, 2005-2011. The table reports point estimates obtained from an application of selection on observables from Oster (forthcoming) to the baseline fixed effect and instrumental variables results. $\delta$ denotes the degree of bias, i.e. $\delta = 0.20$ means that selection on unobservables must be no more than 20% of selection on observables. All contain fixed effects on county, industry, year, quarter. The controls include: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. The sample consists of three year groups: 2005-2007, 2008-2010, and 2011-2013 obtained through SocialExplorer, which is linked with the American Community Survey.

Our third exercise gauges the potential role that anticipation of future employment growth might play in affecting the duration choice of a loan that an individual opts into or that a bank decides to lend. Figure 40 displays this exclusion restriction test in the following way. We first examine the distribution of unemployment growth between $t$ and $t - 1$, residualizing it on our standard controls and on our controls together with our interest rate reset instrument. We compare the $R$-squared values in both cases. We see, for example, that the $R$-squared jumps from 10%
to 33% once we add our instruments. In contrast, turning towards income growth between $t + 5$ and $t$, we find that the $R$-squared does not change at all when we add in our interest rate reset controls. While we recognize that there is no perfect test for the exclusion restriction, Figure 40 shows very clearly that these interest rate shocks predict employment outcomes, but not future income growth.

\[
\text{Panel A: } \Delta(\text{Unemployment})_t
\]

\[
\text{Panel B: } \Delta(\text{Income})_{t+5}
\]

Figure 40: First-stage Effects of Interest Rate Resets on Unemployment and Income Growth

Notes.–Sources: CoreLogic, Internal Revenue Service, 2004-2010. Panel A plots the residualized unemployment growth rate in year $t$ and the residualized unemployment growth rate including a cubic on interest rate resets from 5-1, 7-1 and 10-1 ARMs. Controls include: a quadratic in logged housing prices, logged mortgage payments due, logged bank deposits, a quadratic in logged population, the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black). Panel B implements the same exercise, but this time using income growth in $t + 5$. The $R$-squared value in the notes compares the $R$-squared obtained from the residualized outcome with and without the interest rate resets. The figures show that interest rate resets predict contemporaneous unemployment rate growth well, but does not predict future income growth, which suggests that the decision to undertake a particular type of loan is not correlated with anticipation about future income growth.

One potential concern with our robustness exercise is that we are using interest rate resets, which are induced by historical originations five, seven, or ten years in advance. While Figure 40 is the relevant exercise since we are using interest rate resets as our instrument—and thus, it shows that interest rate resets do not predict future income growth—a concern still exists that other contemporaneous factors might be affected based on historical originations. We, therefore, implement the same exercise using logged numbers of originations as an alternative for gauging the explanatory power on unemployment growth and income growth. Figure 41 shows that originations of 5-1, 7-1, and 10-1 ARMs predicts unemployment growth very well, raising the $R$-squared from 0.10 to 0.44. However, it does not do nearly as good of a job predicting future income growth, raising the $R$-squared from 0.11 to 0.24. While the $R$-squared clearly rises, the fact it grows nearly four times less than the case with unemployment growth shows that originations are affecting labor
market outcomes much more than other income related outcomes.

Figure 41: First-stage Effects of ARM Originations on Unemployment and Income Growth

Notes.– Sources: CoreLogic, Internal Revenue Service, 2004-2010. Panel A plots the residualized unemployment growth rate in year $t$ and the residualized unemployment growth rate including a cubic on logged number of originations for each type of 5-1, 7-1, and 10-1 ARM loan. Controls include: a quadratic in logged housing prices, logged mortgage payments due, logged bank deposits, a quadratic in logged population, the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black). Panel B implements the same exercise, but this time using income growth in $t + 5$. The R-squared value in the notes compares the R-squared obtained from the residualized outcome with and without the logged originations. The figures show that logged originations of ARMs predict contemporaneous unemployment rate growth well, but does not predict future income growth very well, which suggests that the decision to undertake a particular type of loan is not correlated with anticipation about future income growth.

We now examine the possibility that, because interest rate resets affect disposable income, they could also affect local labor market outcomes by altering individual search intensity (Herkenhoff, 2015; Cohen-Cole et al., 2016). While we cannot measure search intensity directly, we can measure different categories of time use. To do so, we turn towards the American Time Use Survey (ATUS) between 2003 and 2014 and merge foreclosure, interest rate reset, and housing price data at the state $\times$ quarter level. While we could work with metropolitan geographical aggregations, we opt for state to avoid removing 80% of the sample. Our primary concern is that, because interest rate resets affect disposable income, they could affect firm employment and hiring through a labor supply channel. For example, if individuals feel poorer, and thus work harder, this may change the way firms behave. We examine this concern by regressing the time allocated towards leisure and work in minutes per day on logged interest rate resets, controlling for individual characteristics and housing price growth. We measure leisure in three ways according to Aguiar and Hurst (2007).

Table 18 documents these results. Although there is a small, but statistically significant negative association between interest rate resets and the first definition of time allocated to leisure (the baseline), the significance vanishes once fixed effects are introduced. Increases in interest rate
resets is not associated with any measure of time use—whether it is leisure or work time. Housing price appreciation is associated with increases in leisure, but is statistically insignificant, reflecting the fact that positive housing price shocks likely allow individuals to take out more equity on their home, which is a substitute with income generated from work activities.

Table 18: Examining the Potential Effects of Interest Rate Resets on Time Use

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>leisure def 1</th>
<th>leisure def 2</th>
<th>leisure def 3</th>
<th>work</th>
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<tbody>
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<td>ln(ARM reset)</td>
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<td>-.50</td>
<td>-.41</td>
<td>-1.34</td>
</tr>
<tr>
<td>[0.56]</td>
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<td>[4.08]</td>
<td>[0.73]</td>
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<td>Δ ln(housing price)</td>
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<td>34.75</td>
<td>-6.55</td>
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</tr>
<tr>
<td>[11.84]</td>
<td>[24.89]</td>
<td>[17.81]</td>
<td>[31.72]</td>
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<td>.26</td>
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<td>Yes</td>
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<td>Year FE</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes.—Sources: American Time Use Survey, CoreLogic, 2004-2014. The table reports the coefficients associated with regressions of the time allocated to leisure (measured in three ways) on logged interest rate resets on 5-1, 7-1, and 10-1 ARMs (aggregated to the state by quarter level), housing price growth, and a vector of individual covariates, including: day of the week (for the interview) fixed effects, number of children, years of schooling, race, marital status, gender, and age. The three definitions of leisure are defined according to Aguiar and Hurst (2007): the first includes social activities, general leisure, communication, pets, outdoors; the second includes the first together with personal care and eating; the third includes the second together with caring for others in the household and outside of the household. Standard errors are clustered at the state-level and observations are weighted by ATUS sample weights.
Table 19: Examining the Effects of Foreclosure on the Allocation of Time

<table>
<thead>
<tr>
<th>Dep. var. = leisure def 1</th>
<th>leisure def 2</th>
<th>leisure def 3</th>
<th>work</th>
</tr>
</thead>
<tbody>
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<td>ln(foreclosures)</td>
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<td>-.007</td>
<td>-.003</td>
</tr>
<tr>
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<td>[.008]</td>
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<td>.21</td>
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<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes.—Sources: American Time Use Survey, CoreLogic, 2004-2014. The table reports the coefficients associated with regressions of logged time allocated to children (minutes/day) on logged foreclosures, day of the week (for the interview) fixed effects, number of children, years of schooling, race, marital status, gender, and age. The three definitions of leisure are defined according to Aguiar and Hurst (2007): the first includes social activities, general leisure, communication, pets, outdoors; the second includes the first together with personal care and eating; the third includes the second together with caring for others in the household and outside of the household. Standard errors are clustered at the state-level and observations are weighted by ATUS sample weights.

D), it is driven by the correlation between housing expenditures and interest rate resets (Panel C). Both durable and non-durables consumption expenditures are not statistically associated with interest rate resets. In this sense, while Di Maggio et al. (2017a) show that ARM borrowers spend some of their increased disposable income arising from downward interest resets on car purchases, the potential aggregate increase in consumption expenditures from these pale in comparison to the deleveraging of debt that shows up in housing expenditures, which is what provides us with plausibly exogenous variation for foreclosures.
Figure 42: State Consumption Expenditures and Interest Rate Resets, 2000-2014

Notes.—Sources: CoreLogic and Bureau of Economic Analysis, 2000-2014. The figure plots residualized state × year logged interest rate resets and logged consumption expenditures (using state and year fixed effects as controls) across four measures of consumption expenditure categories: durable goods, non-durable goods, housing and utilities, and total consumption expenditures. The figure shows that interest rate resets are only statistically correlated with housing expenditures—durable and non-durable consumption expenditures have p-values above 0.10.

A.5.6 Foreclosure Intensity and Non-linearities

The main text presents results using the flow of foreclosures as the main measure of foreclosure shocks pooled across 2000 to 2014. Since states without judicial status laws have many more foreclosures than those that have the laws, the potential for non-linearities could explain the difference in our estimate gradients. To examine the potential for these non-linearities, we use the Bureau of Labor Statistic’s annual county unemployment rate series as our primary outcome variable and foreclosures per open mortgage as our primary right hand side variable. We defer to these alternative datasets for two reasons. First, they provide overall robustness to our main results by
showing that our estimated gradient is not simply driven by our functional form (i.e., logarithms). Second, they provide interpretable estimates in a setting where industry-level heterogeneity is not directly relevant.

We begin by ranking counties based on their share of foreclosures per open mortgage between 2008 and 2010 at the county-level to capture the intensity of foreclosure shocks counties faced during the recession. We subsequently regress county unemployment rate, denoted $u_{ct}$, on the interaction between foreclosures per open mortgages, denoted $f_{ct}$, and seven dummies ranking the intensity of a county’s average foreclosures per open mortgages between 2008 and 2010, denoted $d_c$, controlling for housing prices, local bank deposits, mortgage payments, denoted $X_{ct}$, and both county and year fixed effects.

$$u_{ct} = \beta X_{ct} + \gamma f_{ct} + \sum_{k=2}^{7} \delta_k (f_{ct} \times d_c^k) + \psi_c + \lambda_t + \epsilon_{ct} \tag{8}$$

We estimate Equation 8 these separately for states with and without judicial status laws, and we instrument for these endogenous foreclosures per open mortgages with interactions between the dummies and our predicted ARM measures and their quadratic and cubic terms.\(^{86}\)

Figure 43 plots the estimated interactions, $\delta^k$, across the foreclosure intensity distribution. Importantly, for convenience we are not including the direct effect, $\gamma$, which is clearly positive. We see a remarkable asymmetry in the interactions between these two sets of states. For example, at the top two bins of the foreclosure intensity distribution, an additional one percentage point rise in foreclosures per open mortgage is associated with a 1.5-2pp rise in the unemployment rate. Given that the average foreclosure per open mortgage is 0.63pp (median is 0.44pp), the marginal effect evaluated at the mean is 0.94-1.26pp, which is not unreasonable in light of the fact that foreclosures per open mortgage grew by roughly a factor of four between 2006 and 2009. While the trend on the interaction effects is declining across the distribution for judicial status states, the direct effect ($\gamma$) is precisely estimated at 3.81, so the net effect on unemployment even in these judicial status states is still unambiguously positive. In summary, the results in Figure 43 point towards strong non-linearities in the effects of foreclosures on county unemployment rates, but only in states without judicial status laws.

\(^{86}\)We include 2-1 and 3-1 ARMs to gain additional identifying variation, but the results are robust to only using 5-1, 7-1, and 10-1 ARMs.
Figure 43: Foreclosure Interaction Effects Across Intensity Distribution, Non-judicial & Judicial

Notes.– Sources: Bureau of Labor Statistics Local Area Unemployment Statistics, CoreLogic, Federal Housing Administration, 2000-2014. The table reports the coefficients associated with regressions of county unemployment rates on foreclosures per open mortgage, its interaction with seven dummies over the intensity of foreclosures per open mortgage between 2008-2010 county averages (normalizing the first bin to zero), conditional on controls and county and year fixed effects, separately for states with and without judicial state laws on the foreclosure process. Controls include: logged housing prices, logged population, logged mortgage payments due, and logged bank deposits. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (2-1 ARM, 3-1ARM, 5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year level and include a cubic as instruments, together with their interactions with the aforementioned foreclosure bins. We restrict to counties with over 0.05 percent foreclosures per open mortgage between 2008-2010. Standard errors are clustered at the county-level and county population is used as the sample weight.

We now examine the potential for the intensity of foreclosure shocks to impact labor market outcomes, rather than the contemporaneous flow. Turning back towards our LEHD sample, we now estimate our baseline specification using logged cumulative foreclosures on the right hand side. Table 20 documents these results. We find a very similar gradient in the pooled sample, but we find quite a larger gradient when we partition the sample by industry and state. For example, we find that a 10% rise in cumulative foreclosures is associated with a 0.9% employment decline in the non-tradables sector, but a 2.65% decline in the tradables sector. We also find that all of the effect is coming from non-judicial status states with a corresponding 3.1% decline in employment following a 10% rise in foreclosures. While we do not view these cumulative foreclosures as the preferred measure, they suggest that the intensity of foreclosure shocks matters.
Table 20: Examining the Potential for Foreclosure Intensity

<table>
<thead>
<tr>
<th>Dep. var.</th>
<th>All</th>
<th>NonTradable</th>
<th>Tradable</th>
<th>NonJudicial</th>
<th>Judicial</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \ln(\text{cumulative foreclosures}) )</td>
<td>-.170***</td>
<td>-.089***</td>
<td>-.262**</td>
<td>-.307***</td>
<td>-.015</td>
</tr>
<tr>
<td>[ .040 ]</td>
<td>[ .026 ]</td>
<td>[ .106 ]</td>
<td>[ .114 ]</td>
<td>[ .031 ]</td>
<td></td>
</tr>
<tr>
<td>R-squared</td>
<td>.87</td>
<td>.94</td>
<td>.81</td>
<td>.87</td>
<td>.87</td>
</tr>
<tr>
<td>Sample Size</td>
<td>1751422</td>
<td>691756</td>
<td>230084</td>
<td>934276</td>
<td>757484</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Instruments?</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes.—Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration, 2000-2014. The table reports the coefficients associated with regressions of two-digit industry logged employment on logged cumulative foreclosures, conditional on controls, logged housing prices (index with 2000 base year), logged mortgage payments due, and logged bank deposits, and controls. Demographic controls include: the fraction of individuals in the county who are male, married, between ages \( k \in [k_1, k_2] \) where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education \( k \in [k_1, k_2] \) where the brackets are less than high school, only high school, some college, college, and graduate school, race (white and black), and logged total population. Loan controls include: a quadratic in the total mortgage payments due for all loans (measured in dollars), a quadratic in adjustable gross income (county-level from the Internal Revenue Service), and local bank deposits (from bank Call Reports). Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level and county population is used as the sample weight.

A.5.7 Selective Pre-payment and Different Loans

The main text describes one potential concern with our identification strategy discussed in detail by Fuster and Willen (2017): when interest rates reset up, better off borrowers—due either to personal or local economic conditions—may be able to refinance their loans to lower interest rates. This causes the set of borrowers with outstanding loans after the reset to be skewed towards those at greater risk of default, leading us to over estimate the net effect of interest rate resets since some of the variation in the composition of borrowers will be correlated with the interest rate change. This section confronts this concern in two ways.

The first and cleanest reply is to rely on our second identification strategy, which exploits counties’ exposure to banks with more versus less ARMs. Counties exposed to different banks will experience resets at different points in time. In this sense, our sample covers the entire universe of loans originated and our predicted loan resets are not influenced by selective prepayment behavior. This especially holds since we are leveraging counties’ initial exposure, which is less likely to be influenced by unobserved shocks before the recession.
The second reply is to delve into the mechanics of our first identification strategy in greater
detail. Although theory is clear that selective prepayment will create a bias in over-estimating the
impact of rate resets on foreclosures, how this would translate into biases is more ambiguous. We
also note that any biases that might exist would tend to be quite small, since the large majority of
the resets we start are downwards resets, as used by Fuster and Willen (2017). Suppose we have
two types of counties, "Good" and "Bad" that experience upward rate resets. In "Good" counties
(which contain prosperous economic conditions along dimensions that we are unable to capture
using our extensive controls), borrowers are more able to refinance their loans, thus eliminating
them from the pool of borrowers at risk of experiencing foreclosures (from our "stage 0" loan-level
models).

On one hand, if “Good counties” tend to have more productive workers who are less likely
to default, then we will overestimate the number of foreclosures in this county. This is plausible
since unobserved factors that make the counties 'Good' and more likely to refinance also make
borrowers less likely to default, even if they do not refinance. Because what makes a county good
vs. bad is unobservable, we cannot measure the coefficient on our reset variable separately for
each type of county in our stage 0 estimation. Our coefficient will, therefore, tend to capture an
average over the true coefficients for good and bad counties, thereby over-estimating effects of
upwards resets for good counties and under-estimating them for bad counties. When we generate
predicted foreclosures based on the fitted coefficient for the reset variable, this can then lead to
over-predicting foreclosures in “Good” counties. Overpredicting foreclosures in "Good" counties
clearly would lead us to underestimate the association between foreclosures and negative economic
outcomes in counties. On the other hand, if "Bad" counties have more borrowers remaining in the
risk set as compared to the "Good" counties, then we might overpredict the number of foreclosures
simply because of an inflated foreclosure risk that applies to more remaining borrowers in those
counties. Following a similar logic as before, then this would lead us to overestimate the association
between foreclosures and negative economic outcomes in these “Bad” counties.

While either scenario is plausible, the channel that dominates is an empirical question. In
particular, it depends on: (i) how significant the effects of upwards rate resets are in inducing
good quality mortgagees to refinance and how large the over-estimation of the effect of interest
rate resets on defaults is, and (ii) how significant the other unobserved personal or economic
characteristics are in their correlation with rate resets, conditional on controls. In the main text,
we already gauged the potential magnitude of these scenarios by including 2-1 and 3-1 ARMs in
our sample since these reset upwards at greater frequencies than the 5-1, 7-1, and 10-1 ARMs that in our main specifications. We found a high degree of similarity between our baseline estimates, which we report in odd columns again for convenience, with our modified IV estimates containing variation in 2-1 and 3-1 ARMs in even columns.

Given that individuals with 2-1 and 3-1 ARMs tend to have lower FICO scores and incomes, why do we not see a more substantial difference between these estimates? Our diagnostics suggest it is largely a result of our controls and granular fixed effects. Although dispersion in levels of 2-1 and 3-1 ARMs appear to be correlated with measurement error in our instrument (arising from the selective prepayment issue), changes do not. We examine potential differences between counties with more versus fewer 2-1 and 3-1 ARMs further by partitioning the set of counties into high and low levels of 2/3-1 and 5/7/10-1 ARMs based on whether they are in the top versus bottom quartile. Figure 44 shows that there are only minor differences in employment growth across these sets of counties—a phenomenon that holds up across various time periods of our sample.

![Figure 44: Distribution of Employment Growth Across Counties, High/Low ARMs](image)

*Notes.* Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic. The figure plots the distribution of employment growth over time and for counties with high and low levels of the different types of ARMs. High denotes the county is in the 75th percentile; low denotes it is in the 25th percentile.
A.6 Supplemental Evidence on Mechanisms

A.6.1 Measuring Optimism and Uncertainty

Figure 45: Perceptions of the Future State of the Economy with the Volatility Index

Sources.—St. Louis Fed and U.S. Gallup Daily Poll, 2008-2015. The figure plots the mean sentiment about the future state of the economy (1-3 scale) with the volatility index at a daily frequency.
A.6.2 Firm Hiring and Bank Lending Channels

We now examine evidence of the real options channel. As we discussed in the main text, we found strong evidence of heterogeneity in the effects of foreclosures on the hiring rate based on differences in state enforcement of non-compete laws from Starr et al. (2016). However, an additional source of variation in firing costs could arise from wrongful discharge laws as discussed by Autor et al. (2006) who evaluate the effects of these laws on employment. Since they found some evidence of adverse effects on employment, we now examine this additional dimension of heterogeneity together with non-compete enforcement in greater detail.

Table 21 documents these results under our baseline instrumental variables specification, separating between states with and without these laws and between the tradables and non-tradables sectors. Starting with columns 1 and 2, we find no evidence of heterogeneity in the effects of foreclosures: a 10% rise in foreclosures is associated with a 0.08 and 0.11 percent decline in the hiring rate for states with and without wrongful discharge laws in the tradables sector, but we fail to reject the null that they are equal (p-value = 0.49). We also see no evidence of foreclosure heterogeneity among the non-tradables sector. However, we do find evidence that states with stronger enforcement of non-compete contracts have much higher foreclosure gradients: a 10% rise in foreclosures is associated with a 0.25 percent decline in the hiring rate among tradables and non-tradables firms in states with strong enforcement, but no statistically significant effect among states with weak enforcement.
Table 21: Examining the Importance of Real Options in Hiring Declines

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>county-by-industry hiring/employment</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>WD NWD WD NWD HNC LNC HNC LNC</td>
</tr>
<tr>
<td></td>
<td>-.008*** -.011** -.011*** -.016*** -.026*** -.004 -.025*** -.008***</td>
</tr>
<tr>
<td>R-squared</td>
<td>.35 .35 .48 .49 .32 .36 .45 .47</td>
</tr>
<tr>
<td>Sample Size</td>
<td>156358 24015 530911 84544 123613 56760 416656 198799</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes Yes Yes Yes Yes Yes Yes Yes</td>
</tr>
<tr>
<td>Industry FE</td>
<td>Yes Yes Yes Yes Yes Yes Yes Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>Yes Yes Yes Yes Yes Yes Yes Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>Yes Yes Yes Yes Yes Yes Yes Yes</td>
</tr>
<tr>
<td>Instrument?</td>
<td>Yes Yes Yes Yes Yes Yes Yes Yes</td>
</tr>
<tr>
<td>Tradables?</td>
<td>Yes Yes No No Yes Yes No No</td>
</tr>
</tbody>
</table>

Notes.– Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration, 2000-2014. The table reports the coefficients associated with regressions of two-digit industry logged hiring on logged county foreclosures, logged housing prices (index with 2000 base year), logged mortgage payments due, and logged bank deposits, and controls. Demographic controls include: the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education brackets (less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. “WD” and “NWD” stand for wrongful-discharge and non-wrongful-discharge laws, whereas “HNC” and “LNC” stand for high non-compete enforcement and low non-compete enforcement. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level and the regressions are unweighted.
One of the important ingredients for our theory about the credit channel and foreclosures is that credit is more important for firms in the tradables sector. Although we have shown that credit is 23% and 36% higher among tradables firms covered by the 504 and 7(a) Small Business Administration lending programs, and that the foreclosure gradients are stronger for tradables, it is possible that our theory is applicable only to small businesses. We now turn towards Compustat to observe a proxy for the importance of credit among large publicly traded firms. We use debt in current liabilities net of employment as our primary proxy. Figure 47 plots the distribution for both sets of firms and shows that it is 16% large for firms in the tradables sector, which is not driven by differences in firm size.

![Figure 47: Distribution of Debt/Employee in Tradables and Non-Tradables](image)

Sources.—Compustat, 2000-2017. The figure plots the distribution of logged debt (current liabilities) net of logged employees between the tradables and non-tradables sectors using the Mian and Sufi (2014) classification.

Another way of gauging the heterogeneous effects of foreclosures on bank lending is by separately estimating our baseline specification for different bins based on a Herfindahl index of geographic concentration (based on employment) from Mian and Sufi (2014) linked to four-digit NAICS industry codes. Figure 48 plots these estimated coefficients. Consistent with our first definition of tradables, we find that the foreclosure elasticity to bank lending is increasing in the geographic concentration of the index—meaning that sectors that are more tradable exhibit a
greater decline in bank lending in response to foreclosures. While we recognize that the distinction between tradables and non-tradables is not always clear, this evidence shows the robustness of our results to agreed upon and different measures of the same phenomena.

Figure 48: Foreclosure Heterogeneity in SBA Lending, by Tradability Herfindahl Index

Notes. Sources: CoreLogic, Small Business Administration, Federal Housing Administration, 2000-2014. The figure reports the coefficients associated with regressions of the share of the loan a bank guarantees (gross approval net of the SBA guarantee divided by the gross approval) on logged zipcode foreclosures (separately for each bin on the employment concentration index of tradability from Mian and Sufi (2014)), controlling for logged county housing prices, mortgage payments due, bank deposits, fixed effects on ten bins of housing price growth, fixed effects on the business type, and zipcode and time (quarter and year) fixed effects. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode × quarter level and include a cubic as instruments. Standard errors are always clustered at the zipcode-level.

The main text provided evidence on the effects of foreclosures on lending using variation from projects in the 7(a) lending program. While these projects are arguably more representative of average lending activity, we now provide additional evidence from the 504 lending program. Table 22 documents these results. While the coefficients are smaller, we find that a 10% rise in foreclosures is associated with a 0.11 and 0.09 percentage point decline in the share of the loan that banks are willing to guarantee to firms in the tradables and non-tradables sectors, respectively. We also find a decline in default probability, which again signals a composition effect that could be taking place in response to foreclosures.
Table 22: Examining the Impact of Foreclosures on Credit Among Small Businesses (504 Lending Program)

<table>
<thead>
<tr>
<th>Dep. var. = share of bank guarantee</th>
<th>default probability</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>TR</td>
</tr>
<tr>
<td></td>
<td>TR</td>
</tr>
<tr>
<td></td>
<td>NTR</td>
</tr>
<tr>
<td></td>
<td>-.011**</td>
</tr>
<tr>
<td></td>
<td>[.005]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.45</td>
</tr>
<tr>
<td>Sample Size</td>
<td>8537</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
</tr>
<tr>
<td>Zipcode FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Instrument?</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes. – Sources: CoreLogic, Small Business Administration, Federal Housing Administration, 2000-2014. The table reports the coefficients associated with regressions of the share of the loan a bank guarantees (gross approval net of the SBA guarantee divided by the gross approval) and the default probability on logged zipcode foreclosures, controlling for logged county housing prices, mortgage payments due, bank deposits, fixed effects on ten bins of housing price growth, fixed effects on the business type, and zipcode and time (quarter and year) fixed effects. These are for the SBA’s 504 lending program. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode × quarter level and include a cubic as instruments. Standard errors are always clustered at the zipcode-level.

To illustrate that our results are not entirely driven by the direct effects of foreclosures on a bank’s loan portfolio, we use data from the Call Reports and restrict the sample to local banks without national operations. We show that foreclosures are associated with declines in the growth rate of their lending even after controlling for their assets and deposits. One concern, however, is that the sample is not externally valid—national banks might operate much differently. We now examine the similarities and differences between these two sets of banks in Figure 49. While there are large differences in lending and assets—national banks have 109% and 95% higher loan volume and assets relative to their counterparts—the differences between the loan to assets ratio and loan growth are much smaller—roughly 4.8% and 0.17%, respectively. These minor differences between national and local bank-specific deviations from trend suggest that our estimated coefficients are externally valid for the exercises of interest.
Figure 49: Comparison of Lending and Assets between National and Local Banks

Notes. Sources: Call Reports, 2003-2013. The figures report the kernel density distributions of logged loan volume, assets, lending to assets, and lending growth across banks for national and local banks.

Figure 50 examines the effects of foreclosures on the level of advertising and rental expenditures by industry using our baseline estimation strategy using county × industry data. While our estimates are imprecise and these are merely proxies for investment activities, they show an overall decline in investment in response to foreclosure shocks. The only sector that exhibits a positive foreclosure gradient is administration and support, which might benefit when there are foreclosure shocks that require more temporary workers and capacity from temporary help agencies.
Figure 50: Foreclosures and Advertising & Rent Expenditures, by Major 2-digit Industry

Notes.– Sources: IPUMS Census, CoreLogic, Internal Revenue Service, Federal Housing Administration, 2004-2014. The figure plots the coefficients from regressions of the logged advertising expenditures on rent (paid on machinery and other categories) on logged foreclosures, conditional on housing prices and controls. The instrumental variables specification uses the number of foreclosures predicted by the loan resets of adjustable rate and balloon mortgage loans based on plausible exogeneity of their reset times, which discontinuously raise the foreclosure probabilities. The controls include: the fraction of individuals in the county who are male, married, between ages $k \in [k, k]$ where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education $k \in [k, k]$ where the brackets are less than high school, only high school, some college, college, and graduate school, race (white and black), and logged total population. All observations are weighted by county population and standard errors are clustered at the county-level.

A.6.3 Mobility and the Composition of Skill

The main text illustrates that foreclosures are associated with significant declines in net migration flows into a county. Although there are several studies that have argued foreclosures raise local crime, we test the hypothesis more broadly using our comprehensive data. We estimate regressions of the form

$$\text{crime}_{ct} = f(X_{ct}, \beta) + \gamma f_{ct} + g(h_{ct}; \theta) + \phi_e + \lambda_{ct}$$

(9)
where \( \text{crime} \) denotes our logged measure of crime, \( f(X, \beta) \) denotes the usual semiparametric function of controls, \( f \) denotes logged foreclosures, and \( g(h, \theta) \) denotes our semiparametric function of housing prices.\(^{87}\) In estimation of Equation 9, we did not have enough variation when we use variation from predicted foreclosures on 5-1, 7-1, and 10-1 ARMs. We, therefore, also include 2-1 and 3-1 ARMs. However, to address the potential for endogeneity—that areas with more 2-1 and 3-1 ARMs also vary in other unobservable ways that are correlated with lower income, we control for a county’s adjustable gross income.

Table 23 documents these results. These specifications contain all the standard controls, including housing prices, mortgage payments due, local bank deposits, housing bin fixed effects, and so on. When our outcome variable is logged total county crime (across all categories), we find that a 10% rise in foreclosures is associated with a 2.52% rise in crime. Once we add logged adjustable gross income as a control, the gradient rises to 3.67%. The fact that the gradient rises when we control for income is a little surprising since the most plausible story of omitted variables bias is that counties that vary in positive unobserved ways (e.g., productivity) will have fewer foreclosures and less crime. One possible explanation is that wealthy communities were hit harder by foreclosure shocks in absolute value since the average home value is larger.

We also examine how these treatment effects vary based on different quartiles of a county’s median household income. Figure 51 plots these estimated coefficients separately. The estimates are quite large, especially at the bottom of the income distribution and even among the middle income counties in the second quartile. For example, a 10% rise in foreclosures is associated with roughly a 5% rise in local crime. The low correlation between foreclosures and crime rates in wealthier counties is likely driven by the fact that crime rates are simply much higher in poorer neighborhoods.\(^{88}\)


### Table 23: Foreclosure Shocks and Crime

<table>
<thead>
<tr>
<th></th>
<th>ln(total crime)</th>
<th>ln(violent crime)</th>
<th>ln(property crime)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>ln(foreclosures)</strong></td>
<td>.252**</td>
<td>.053***</td>
<td>.060**</td>
</tr>
<tr>
<td></td>
<td>[.112]</td>
<td>[.017]</td>
<td>[.024]</td>
</tr>
<tr>
<td><strong>ln(payments due)</strong></td>
<td>-.337**</td>
<td>-.078**</td>
<td>-.175**</td>
</tr>
<tr>
<td></td>
<td>[.215]</td>
<td>[.034]</td>
<td>[.071]</td>
</tr>
<tr>
<td><strong>ln(deposits)</strong></td>
<td>.000</td>
<td>.008</td>
<td>-.002</td>
</tr>
<tr>
<td></td>
<td>[.058]</td>
<td>[.008]</td>
<td>[.009]</td>
</tr>
<tr>
<td><strong>ln(adj gross income)</strong></td>
<td>-.097</td>
<td>.021</td>
<td>-.012</td>
</tr>
<tr>
<td></td>
<td>[.612]</td>
<td>[.099]</td>
<td>[.089]</td>
</tr>
<tr>
<td><strong>R-squared</strong></td>
<td>.91</td>
<td>.89</td>
<td>.89</td>
</tr>
<tr>
<td><strong>Sample Size</strong></td>
<td>15526</td>
<td>15429</td>
<td>11747</td>
</tr>
<tr>
<td><strong>Controls</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>County FE</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Time FE</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Notes.** – Sources: IPUMS Census, CoreLogic, ICPSR Reported Arrest Files. The figure plots the coefficients from regressions of the logged crime (measured in different ways) on logged county foreclosures, conditional on controls, including logged mortgage payments due, logged local bank deposits, logged housing prices, the fraction of individuals in the county who are male, married, between ages $k \in [k_1,k]$ where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education $k \in [k_1,k]$ where the brackets are less than high school, only high school, some college, college, and graduate school, race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (2-1 ARM, 3-1 ARM, 5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year level and include a cubic as instruments. All observations are weighted by county population and standard errors are clustered at the county-level.
The main text also implemented an analysis that examines how foreclosures affect the relative composition of college to non-college workers in a county (see Table 9). Here, we replicate the analysis using earnings, rather than employment. We ask whether foreclosure shocks affect the relative earnings premium between skilled and unskilled workers. To the extent that the relative composition is affected, we should also see a change in the compensating differentials for workers.

Table 24 documents these results and indeed shows that increases in foreclosures are associated with increases in the relative earnings premium between college and non-college workers. For example, we find that a 10% rise in foreclosures is associated with 0.29% and 0.273% in the relative earnings premium between college & non-college and college & some-college workers, respectively. We find that a comparable 10% rise in foreclosures is associated with a 0.05pp and 0.043pp rise in the growth rate of the earnings premium among these two sets of workers. These results are consistent with the presence of compensating differentials.

We finally turn towards additional evidence that examines how foreclosure growth affects the probability of college attainment across the income distribution. We use micro-data from over eight
### Table 24: Foreclosure Shocks and Relative Earnings Across Skilled Brackets

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>Logged college earnings net of non-college</th>
<th>Logged college earnings growth net of non-college</th>
<th>Some-college</th>
<th>Some-college</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>.0290*** [.0065]</td>
<td>.0273*** [.0056]</td>
<td>.0051*** [.0008]</td>
<td>.0043*** [.0006]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.57</td>
<td>.56</td>
<td>.01</td>
<td>.00</td>
</tr>
<tr>
<td>Sample Size</td>
<td>1718145</td>
<td>1689001</td>
<td>1577645</td>
<td>1547145</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Instruments?</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

*Notes.* – Sources: Census Bureau, CoreLogic, Federal Housing Administration, Longitudinal Employer-Household Dynamics, 2000-2014. The table reports the coefficients associated with regressions of the logged earnings among college graduates net of non-college graduates (and separately for individuals with some college experience) on logged foreclosures, conditional on logged housing prices, logged mortgage payments due, logged local bank deposits, the fraction of individuals in the county who are male, married, age brackets (0-17, 18-34, 35-64, and 65+ years old), education (brackets are less than high school, only high school, some college, college, and graduate school), race (white and black), and logged total population. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year-quarter level and include a cubic as instruments. All observations are weighted by county population and standard errors are clustered at the county-level.

Million individuals from the 2000 Decennial Census and 2005-2014 annual American Community Survey (ACS). We estimate logit regressions of an indicator of college attainment on the growth rate of county foreclosures, conditional on controls, and estimated separately by income bracket.89

Figure 52 plots these estimated coefficients, illustrating that a one percentage point rise in the growth rate of foreclosures is associated with a statistically significant decline in the probability that an individual has a college degree. Among those earning less than $25,000 per year, such an increase in foreclosure growth is associated with a 0.05 percentage point decline in the probability an individual has a college degree, whereas those earning over $75,000 have a 0.085 percentage point decline. The fact that the gradient is monotone in income is intuitive. Put simply, observing a wealthy college degree worker becomes increasingly less likely in areas with greater foreclosure shocks.

89 We use the growth in logged foreclosures to remove the endogeneity that emerges from non-random sorting into areas. For example, areas with a higher fraction of college degree workers may have more foreclosures simply because they are larger. While we control for logged population, we recognize that there are many unobservables we cannot control for absent fixed effects (which are computationally intensive with a probit estimator and this sample size). When we instead use an OLS estimator with county and year fixed effects, we recover coefficients that are closer to zero and imprecise, which is not surprising since least squares estimators routinely do a poor job of capturing non-linearities when the outcome is discrete.
Figure 52: Foreclosures and Skill Composition, by Income Bracket

Notes. Sources: Longitudinal Employer-Household Dynamics, CoreLogic, Census, Federal Housing Administration, 2000-2014. The figure plots the coefficients associated with logit regressions of an indicator for college attainment on the growth in county foreclosures, conditional on demographic controls. The quartiles are over income bracket. These controls include: logged county housing prices, number of children, family size, age, household tenure, male, marital status, and race. Standard errors are clustered at the county-level.