The Effects of Foreclosures on the Labor Market*

Christos Andreas Makridis and Michael Ohlrogge

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Abstract

Foreclosures per open mortgage grew by over a factor of five between 2000 to 2009. Using quarterly county-by-industry labor and loan-level data between 2000 and 2014, we estimate how the surge in foreclosures affected local labor markets 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 0.85% and 2.13% decline in employment and hiring, respectively, but these declines are concentrated among firms in the tradables sector and among small firms with 20-249 employees. Our estimates imply that the surge in foreclosures during the Great Recession can account for 10-16% of the decline in the hiring rate during the Great Recession. Using additional individual and loan-level data, we provide suggestive evidence that the rapid and unprecedented surge in foreclosures led to a decline in local optimism and a rise in uncertainty, triggering declines in not only hiring, but also bank lending. We show that these effects are not driven by the direct effect of foreclosures on bank loan portfolios, but rather by the indirect effects of increases in local foreclosures.

Keywords: employment; foreclosures; housing, labor markets; mortgages; uncertainty.

JEL: G21, J21, J23, R31

*Stanford University, Working Paper. Corresponding Author: Christos Makridis, Department of Economics and Department of Management Science & Engineering, Stanford University, 579 Serra Mall, Stanford, CA 94305, http://stanford.edu/~cmakridi/. Michael Ohlrogge: Stanford University, Management Science & Engineering, Huang Engineering Center 475 Via Ortega, ohlrogge@stanford.edu. Christos thanks the NSF Graduate Research Fellowship and the SIEPR Shultz Fellowship for financial support. Michael thanks the Olin Foundation for Law and Economics for financial support. We thank Asaf Bernstein, Pat Bayer, Raj Chetty, John Cochrane, Andreas Fuster, Steven Grenadier, Caroline Hoxby, Kyle Herkenhoff, Erik Hurst, John Mondragon, Atif Mian, Pascal Noel, Elena Pastorino, Markus Pelger, Daniel Silverman, Luke Stein, Johannes Stroebel, Amir Sufi, Constantine Yannelis, Rose Wang, as well as seminar audiences at Arizona State University, George Mason University, Stanford University, Yale University, the 2017 American Real Estate Society, and the 2017 All-Round Labor Economics Meetings for suggestions and feedback. Christos is especially thankful to his advisers (Nicholas Bloom, Mark Duggan, Luigi Pistaferri, and Amit Seru) for conversations, reviews, and suggestions.
1 Introduction

Housing prices fell by nearly 20% between 2007 and 2009, leading to 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 nearly a factor of five (see Figure 1), generating significant policy attention with, for example, the passage of the Home Affordable Mortgage Program (HAMP) to help decrease the number of, and slow the process of, foreclosures.\footnote{While the purpose of HAMP was to facilitate loan renegotiation of seriously delinquent loans (as a way of mitigating the number of foreclosures), and it did achieve some of these aims, it was not as successful as it could have been at developing a framework for loan renegotiation for the broader set of seriously delinquent loans (Agarwal et al., 2017).}

While current models predict that foreclosures affect real activity through a housing market channel (Campbell et al., 2011; Mian et al., 2015), this paper examines whether their rapid and significant surge may have independently affected local labor markets and prolonged local recoveries following the Great Recession. We specifically provide evidence that foreclosures led to a contraction of bank lending by raising local uncertainty, consistent with Christiano et al. (2014) and Brunnermeier and Sannkiov (2014) about the macroeconomic effects of credit supply shocks during the financial crisis and their consequences for employment (Chodorow-Reich, 2014).\footnote{Krishnamurthy and Muir (2017) show that these boom/bust credit cycle features are not unique to the United States’ experience during the financial crisis, but rather resembles the experience of many other countries across the world over time.}

We examine how the sudden and sharp rise in foreclosures may have affected labor market outcomes at a county-level, particularly among firms dependent on external financing, such as small businesses and firms that produce tradable goods.\footnote{See Figure 15 in Appendix Section A.1 for the time series of employment growth in non-tradables and tradables sectors. As we will discuss shortly, our contribution is heavily complementary to Mian and Sufi (2014) who show that housing wealth shocks were integral for explaining the decline in employment among firms in the non-tradables sector through a demand-side channel.}

Unfortunately, empirically estimating the causal effects of foreclosures on the labor market, however, is fraught with identification problems.\footnote{We 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.}

[INSERT FIGURE 1]
have more foreclosures since unemployment leads to such a sudden and persistent drop in earnings (Jacobson et al., 1993; Couch and Placzek, 2010), making it more difficult to pay down mortgage debt. 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 and their market value, which would have placed many banks in insolvency since the mortgages were “under water”.

A related explanation behind foreclosure delay was administrative backlogs since the surge in foreclosures took place in such a short window of time.6

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 spikes on 5-1, 7-1, and 10-1 ARMs to predict realized foreclosures within a county and assess how these predicted foreclosures affect labor market outcomes.7 We show non-parametrically that positive (negative) interest rate resets on these loans are associated with discontinuous spikes (drops) in foreclosure probabilities.8 We also show that these interest rate resets on ARMs are associated with subsequent foreclosures on additional fixed rate mortgages (FRMs), consistent with recent evidence on foreclosure-induced spillover effects (Anenberg and Kung, 2014; Gupta, 2016). Since resets on these loans are contractually determined at the origination of the loan, they

5See 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/.


7Our 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 (forthcoming) 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.

8One 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.
are plausibly exogenous with respect to contemporaneous economic shocks. The fact that resets are influenced only by national interest rates also allows us to rule out the bank-lending channel as a confounding factor. 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.

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 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.}

Using industry $\times$ county and firm size $\times$ county data from the Longitudinal Employer-Household Dynamics (LEHD), the second part of our paper uses these predicted interest rate resets to show that a 10% rise of foreclosures is associated with a 0.85% decline in employment, a 2.13% decline in hiring, and a 0.13 percentage point (pp) decline in job turnover. However, we find evidence of significant heterogeneity. For example, a comparable rise in foreclosures is associated
with a 0.89%, 2.20%, and 0.14pp decline in employment, hiring, and job turnover among firms in
the non-tradables sector, but a 2.63%, 3.88%, and 0.11pp decline among firms in the tradables
sector. We also find similarly large magnitudes of foreclosures on employment and hiring among
small firms with 20 to 249 employees. We do not find evidence, however, that foreclosures raised
income inequality within or across counties; in fact, they decreased inequality. We also show how
these results help understand the slow recovery in many counties following the recession. Given
our estimated elasticities, a back-of-the-envelope calculation suggests that the rise of foreclosures
can explain 10-20% of the decline in the hiring rate during the financial crisis.

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 stronger for the tradables sector, even if an aggregate demand channel is present, it
cannot explain all of the observed effects. Since we also control for housing prices and aggregate
mortgage payments for each county × quarter, our estimates remove variation in labor market
outcomes that is driven by time-varying shocks to housing prices and/or loan volume. We further
examine the correlation between state × 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 to
primarily 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 banks’ willingness to provide external financing.}

\footnote{We by no means provide an exclusive interpretation for the slow recovery; see Fernald et al. (2017) for a survey of recent explanations.}

\footnote{There 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).}

\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.}

\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...}
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 0.54pp and 0.38pp 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.\footnote{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.} 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. (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.\textsuperscript{15} 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 discussion 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

\textsuperscript{15} Herkenhoff (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.
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 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.

16Although 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 Grieco (2013) and Bucks and Bricker (2013) for additional evidence from the Panel Study of Income Dynamics and Survey of Consumer Finances.
2 Background

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.\(^{17}\)

Before turning to the data, we begin with a theoretical framework for understanding the
potential mechanisms at play.\(^{18}\) 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).\(^{19}\)

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.,
\(^{17}\)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.
\(^{18}\)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).
\(^{19}\)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.
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

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20While 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).

21First, 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

22For 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

23While 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.
investment in labor. Second, the “risk premium effect” means that uncertainty can affect the 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, Figure 2 plots the employment growth rate for firms of different sizes. 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. 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).

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 in 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.

Startups, 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.

We 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.

Standard models used to predict default systematically failed during the Great Recession (Rajan et al., 2015).
Our mechanism most closely complements a series of recent contributions emphasizing the importance of credit in explaining fluctuations in investment and employment (Gilchrist and Zakrajsek, 2012; Gilchrist et al., 2014). 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.

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28See Gertler and Kiyotaki (2011) for a survey of recent literature.
29Garriga 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.
30http://bebusinessed.com/history/history-of-mortgages/
We begin by documenting their incidence throughout our sample, starting in 2000. Figure 3 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 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 19) and that counties range in their relative proportions of 5-1 to 7/10-1 ARMs between -3% and 3% (Figure 20).

[INSERT FIGURE 3]

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 4 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.\(^3\)

[INSERT FIGURE 4]

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

\(^{31}\)In 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 3 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).
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 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 $R^2$-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. 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

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32 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.
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+), 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/).\textsuperscript{33} 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).\textsuperscript{34} 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

\textsuperscript{33}See Abowd et al. (2009) for details.

\textsuperscript{34}One 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.
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 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.

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. 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

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35Only 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.
36The 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.
37We see, for example, these measures at a county × industry level for all line items on standard business tax forms. Many firms operate 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.
resets and balloon payments. We also observe monthly performance updates, giving information on factors such as 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 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{38}

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 × month observations) for each county × 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{39} 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 con-

\textsuperscript{38}See Mayer et al. (2009) for an additional discussion of these differences.

taining 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.\footnote{https://www.sba.gov/category/lender-navigation/sba-loan-programs/7a-loan-programs} 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 (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.\footnote{https://www.sba.gov/offices/headquarters/oca/resources/5991} 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.

\textit{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 local information. 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 6 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

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43 http://www.pewinternet.org/2012/04/12/72-of-americans-follow-local-news-closely/

44 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.
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 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 6]

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 7 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.\(^{44}\) 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.\(^{45}\) 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 7 demonstrates that there is still

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\(^{44}\)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.

\(^{45}\)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.
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.

Motivated by these geographic differences, Figure 8 illustrates that there is also considerable heterogeneity in the timing of when counties experience foreclosures. Just as in Figure 7, the darkness of the shading is based on a within-county comparison that takes the total number of 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.

### 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 a 2% increase following an interest rate reset in 2007, then they may be more likely to go into default.46

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 9 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.47 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 9 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.

46Edmiston and Zalneraitis (2007) focus on the latter three main factors behind the recent surge in foreclosures. 47Although our application of interest rate resets is similar to Fuster and Willen (forthcoming), 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.
\[ f_{ict} = \beta X_{it} + \phi D_{ct} + \sum_k \gamma^k l^k_{it} + \zeta \Delta r_{it} + \sum_k \rho^k (l^k_{it} \times \Delta r_{it}) + \epsilon_{ict} \] (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, 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).\(^{48}\) 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., 2015), 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.

Table XX documents the results associated with Equation 1. In addition to the variation in foreclosures induced by these interest rate resets, Table 11 in Appendix Section A.3.5 explores how foreclosures on ARM borrowers generate spillovers into the broader FRM market for loans. Consistent with Anenberg and Kung (2014) and Gupta (2016), we find that a 10% rise in foreclosures on ARM borrowers is associated with a 1.08% rise in foreclosures on FRM borrowers. 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

\(^{48}\) We 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.
foreclosures on other loans more likely.

[INSERT TABLE XX]

4 Research Design

4.1 Empirical Specification

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

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

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 \( j \), \( g(h, \theta) \) denotes a flexible semi-parametric vector of housing price controls, and \( \eta, \psi, \) and \( \lambda \) denote fixed effects on two-digit industry, county, and time.\(^{49}\) 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 also follow Solon et al. (2015) by estimating Equation 2 without weights since we observe the full population.

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). Motivated by the fact that stagnating housing prices were one of the main catalysts for the surge in foreclosures in 2009-2010 (Edmiston and Zalneraitis, 2007), we condition on county housing prices to examine whether foreclosures have a unique and direct effect on labor market outcomes. We measure \( g(h, \theta) \) by including logged housing prices and 10 bins that span the distribution of housing price growth, allowing us to control flexibly for areas with heterogeneous different levels and rates of housing price growth.\(^{50}\)

\(^{49}\)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.

\(^{50}\)Our main quantitative estimates only decline slightly when we interact the 10 bins with logged housing prices,
### 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. When a bank forecloses on a mortgage, it must take physical possession of the underlying property and value that asset at its

\[\text{which allows us to estimate separate group-specific coefficients for areas with different housing price growth in response to housing price shocks. One potential concern is that we are “over controlling” by including housing prices. However, our baseline results are quite similar even when we omit housing prices, although they are lower in magnitude because the dynamic selection problem is amplified. We underscore that it is not theoretically clear whether to include housing prices as a control or not. Including them introduces a concern of “over controlling”, but excluding them raises the potential for a dynamic selection problem since banks have a greater incentive to strategically delay foreclosure on homeowners with underwater mortgages to avoid valuing those assets at their true market value. We find it comforting that our results are similar in both cases.}\]

51 https://www.bloomberg.com/view/articles/2014-02-26/banks-prefer-losses-they-don-t-have-to-talk-about
market value, rather than keeping it simply on its books under the “loans outstanding” category in the hope that it will become current again.\textsuperscript{52} 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 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{53} In fact, part of the Home Affordable Modification Program (HAMP) aid was specifically designed to help banks avoid recognizing their losses immediately.\textsuperscript{54} 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{55,56}

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{57} 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

\textsuperscript{52}(Antoniades, 2015). For specific accounting rules on this, see: https://www.federalreserve.gov/bankinforeg/srletters/sr1210a1.pdf
\textsuperscript{54}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{56}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 \texttimes{} 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 \texttimes{} quarter, producing a coefficient of -13.55 (\textit{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{57}http://www.creditslips.org/creditslips/2012/11/where-are-the-foreclosures.html
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.

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.\footnote{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.} 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

\[
f_{ict} = \alpha + \sum_k \gamma^k l_{it}^k + \zeta \Delta r_{it} + \sum_k \rho^k (l_{it}^k \times \Delta r_{it}) + \epsilon_{ict}
\]  

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 160 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 \(Z_{ct} \equiv P(f_{ct})\). We use these predictions to instrument actual foreclosures through 2SLS

\[
f_{jt} = f(X_{ct}, \beta) + g(h_{ct}, \theta) + \pi Z_{jt} + \eta_j + \psi_c + \lambda_t + \epsilon_{ijt}
\]

\[
y_{ijt} = f(X_{ct}, \beta) + g(h_{ct}, \theta) + \gamma f_{jt} + \eta_j + \psi_c + \lambda_t + \epsilon_{ijt}
\]
where \( \hat{f}_{jt} \) 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 9). 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). We also show robustness controlling control for county income to further mitigate the concern that the composition of workers is changing for other reasons related to spatial mismatch and/or other dynamics that were in flux during the Great Recession.

Our identification strategy is related to several recent contributions, in particular Gupta (2016) who examines the impact of foreclosures on housing price declines, as well as Fuster and Willen (forthcoming) 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. 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

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59See 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.

60A 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. (2015) use the Panel Study of Income Dynamics (PSID) to show that there is only a limited scope for strategic default.

61In contrast, Fuster and Willen (forthcoming) 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.
primary independent variable of interest. These combined approaches have wide applicability for future research.

4.2.2 Strategy # 2: A Bartik-like Measure

Even though the distribution of FICO scores is nearly identical across individuals with these different types of ARMs, it is possible that, given the increasing importance of soft information during the time leading up to the Great recession (Piskorski et al., 2015). We therefore now turn towards another strategy that is less susceptible to concerns about our exclusion restriction.

We create a Bartik-like instrument that exploits the heterogeneous reset dates of loans originated in different periods and under different contractual arrangements—that is, 5-1, 7-1, and 10-1 loan contracts. Our goal is to predict loan origination based on factors that are largely exogenous to a county’s time-varying economic fundamentals—a county’s exposure to a particular bank that has more of one type of ARM than another. This is in many ways parallel to Agarwal et al. (2017) who use pre-existing geographic variation in the location of different loan servicers to measure the macro-economic impact of programs that incentivize mortgage loan renegotiation and Mondragon (2015) who uses a county’s exposure to the collapse of large and previously health lenders.

In an ideal world, we would be able to measure each bank’s market value of its type-\( k \) loan (with \( k = 5, 7, 10 \) for each ARM type) in each county and period. However, this data, to our knowledge, is prohibitively costly for us to purchase. We instead approximate it by using two additional terms. First, a bank \( i \)’s (product) market share of loan type-\( k \) in a baseline period \( t_0 \), denoted \( m^P_{i,k,t_0} \), i.e., the percentage of all type-\( k \) loans that were originated (anywhere in the country) by a given bank. Second, a bank \( i \)’s (county) market share in a baseline period \( t_0 \), denoted \( m^C_{i,c,t_0} \), i.e., the percentage of all of a bank’s total loans that were originated in a given county.\(^{62} \), \(^{63} \) Our approximation relies on two assumptions. First, national banks, for reasons of corporate strategy, favored different types of loan products as compared to other banks. Seeing as these are decisions by national banks, they are unlikely to be driven by the differences in economic characteristics between different counties. Second, banks tend to have relatively stable patterns of which geographic areas they operate in, and that these arise for historical reasons. With these

\(^{62} \)The CoreLogic data does not identify the specific lender, therefore, we obtain these bank-product market shares from the Columbia Collateral Files, a set of 8 million privately securitized loans. We obtain bank-county market shares (based on all loans, not just hybrid ARMs) from the Home Mortgage Disclosure Act (HMDA) data.

\(^{63} \)By a bank’s county market share, we mean the percent of a bank’s total loan portfolio that falls within a designated county.
being both stable and based on historical reasons, they are also less likely to be correlated with economic factors within counties that correlate with employment.\footnote{Furthermore, any changes that might occur in bank geographical distribution after our base period ($t_0$) will not impact our results, since we use only the market share calculations as of the baseline periods. For the geographic market shares of banks, we measure as of year 2000, whereas for the national market shares of ARM loan types we measure over years 2000 to 2003, since there were still relatively few such loans originated in year 2000 itself.}

Let $O_{i,c,k,t}$ denote the “predicted” number of originations from bank $i$ in county $c$ for loan type-$k$ in period $t$ and $O_{k,t}$ the national number of originations for loan type-$k$ in period $t$. We then construct our Bartik-like measure in two stages. In the first stage, we predict the number of originations based on a county’s exposure to a particular bank and to a particular loan type

$$O_{i,c,k,t} = \sum_i \left( O_{k,t} \times m_{i,k,t_0}^P \times m_{i,c,t_0}^C \right)$$

In the second stage, we compute the implied number of resets that occurred due to the origination $O_{i,c,k,t}$. In other words, if a 5-1 loan is originated in a given county in Q1 2003, we record a predicted loan reset for Q1 2008 in that same county. We base the amount of the interest rate change in this reset on the median rate change for all loans (nationally) of that given type, originated in a given quarter. Thus, nothing specific to the geographies of different areas that influence the reset amounts will factor into these calculations. We subsequently multiply this interest rate change by the corresponding number of loans originated in that period and county to obtain a metric of total predicted resets at the county-quarter level. We separate between reset increases and decreases to allow for the asymmetric effect that they have on foreclosure probabilities.

For clarity, consider an example. Suppose 100,000 5-1 ARM loans were originated in period $t$. If Wells Fargo had an approximately 20\% share of the national market in 5-1 ARM loans, then, based on the fact that banks operate in historically defined regions, we can compute how much of each bank’s loan volume is allocated towards each county. For example, if Wells Fargo made approximately 3\% of its national loan volume within Los Angeles County, CA, then our instrument infers that Wells Fargo made 20,000*0.03 = 600 of those loans in Los Angeles County.\footnote{We generate both of these calculations as of 2003, prior to the start of our main study period. We predict that the number of loans that will be originated within a given county by a given bank at a given point in time will be proportional to (a) the total number of loans originated nationally at a given point in time, (b) the fraction of the national market share a given bank had, and (c) the fraction of a bank’s total lending that occurred in a given county.}

In this sense, our instrument generates plausibly exogenous variation in the incidence of foreclosures by leveraging the fact that different areas were more likely to experience interest rate resets for different types of ARMs based on their pre-recession exposure to lending strategies by
banks. As we documented earlier, the share of ARMs is uncorrelated with historical shocks (see Figure 4) and appears to be driven by historical factors that led to the entry of banks into different areas. As long as the pre-recession exposure to banks is quasi-random (along the lines of what Mondragon (2015) showed), we will recover unbiased foreclosure gradients. Another advantage of this alternative formulation is that it insulates us from any concerns about selective prepayment of mortgages as discussed by Fuster and Willen (forthcoming). In Appendix Section A.5.6, we outline the identification concern, explain how this second identification strategy directly circumvents the concern, and explains why our first identification strategy also overcomes it.

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, we can directly test the strength of our first-stage assumption by plotting residualized measures of foreclosures and our instruments together. We plot residualized foreclosures with each of our residualized instruments to provide

\footnote{Kroszner and Strahan (1999) discuss the political economy forces that led to this deregulation.}
information on a partial $F$-statistic. To make the plots easy to view and interpret in Figure 10, we partition residualized foreclosures into 1000 bins and average to this group-level. Our baseline instrumental variables strategy using the loan-level model generates very strong first-stage correlations. The $F$-statistics over both the baseline and supplemental Bartik-like instruments are well above the recommended $F$-statistic of 10 from Stock and Yogo (2005). The asymmetric relationship between foreclosures and interest rate resets based on decreases versus increases suggests that interest rate increases are less costly for individuals than interest rate declines are helpful.\footnote{In the Appendix Section A.4.2, we also report 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.}

\[\text{[INSERT FIGURE 10]}\]

5 Quantitative Estimates

5.1 Main Results

Table 1 documents our estimates of Equation 2 for three outcome variables: logged employment, logged hires, and the job turnover rate. 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 0.36% decline in employment. Once we add fixed effects on county, industry, and time, however, the estimated gradient declines considerably in magnitude to a corresponding 0.04% decline. Why? While column 2 mitigates 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. Ignoring the presence of dynamic selection will bias our estimates upwards.

We subsequently turn towards our baseline result in column 4, 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 1% decline in employment. Our $F$-statistic of roughly 60 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. To assess the concern that the variation emerging from focusing purely on 5-1, 7-1, and 10-1 ARMs is limited and/or a local treatment, column 4 subsequently includes resets emerging from 2-1 and 3-1 ARMs as additional instruments. While our coefficient declines in magnitude to -0.071, it is not statistically different from our main estimate in column 3, suggesting that expanding the source of the variation in our IV does little to alter the main results.\footnote{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\% \textit{(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.}

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 our driving our results by implementing three exercises. First, Table 16 in Appendix Section A.5.4 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 41 in Appendix Section A.5.4 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. Third, column 5 in Table 1 controls for logged county × industry earnings. While it reduces our gradient to -0.045, the fact that it still remains significant shows that income fluctuations cannot fully explain the response of employment to foreclosures. Fourth, as we show shortly, we find our effects are even larger in the tradables sector. If income effects were driving our pooled estimate, then we should find a null gradient for tradables since their product demand would not be driven as much by local income fluctuations.

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
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.

We finally turn towards two sources of heterogeneity, specifically heterogeneity in the response of employment in the tradables and non-tradables sectors and states with and without judicial status laws. Beginning with the first, we are motivated by results in Mian and Sufi (2014) that the decline in household net worth led to a decline in disposable income, which affected demand for local non-tradables goods. However, we find that a 10% rise in foreclosures is associated with a 1.14% decline in the non-tradables sector, but a 2.71% decline in the tradables sector. What explains these significant differences? Given that we have replicated their results on our data (see Appendix Section A.4.1), our stronger gradient for tradables suggests that foreclosures affect the labor market through a systematically different channel. While we discuss the mechanism in detail later, we show that the causal effect of foreclosures on the labor market is mediated through a credit supply channel. By depressing local optimism and raising local uncertainty, foreclosures lead to a flight to quality (Caballero and Krishnamurthy, 2008) and, therefore, contraction in credit.\footnote{While we show that foreclosures lead to reductions in credit and increases in uncertainty, prior literature has provided some evidence already that uncertainty leads to reductions in credit (Christiano et al., 2014; Di Maggio et al., 2017b).}

We also show that the tradables sector tends to require greater credit and also exhibits greater credit elasticities. In this sense, our mechanism focuses on credit supply, whereas both Dobbie and Goldsmith-Pinkham (2015) and Mian and Sufi (2014) focus on household demand.

Turning towards heterogeneity in states with and without judicial status laws, we are motivated by Mian et al. (2015) who show that the degree of foreclosures are much lower in states with these laws since the mere process of foreclosing on a seriously delinquent borrower is longer and more costly.\footnote{For example, states without these laws experienced over a 300% rise in foreclosures between 2006:Q1-2009:Q3 versus a 173% increase in states with these laws. Loans in states with judicial foreclosure laws also tend to be 3-7% smaller, which could also contribute to these differences (Pence, 2006).} We regress logged employment on logged foreclosures and an interaction with non-judicial
state status, finding that a 10% rise in foreclosures is associated with a 0.97% decline in states without judicial state status and a 1.23% decline in states with it. The fact that states without these laws exhibit better outcomes is consistent with evidence from Pence (2006) that the longer foreclosure process raises costs for all involved parties. Longer foreclosure delays and higher legal costs may be especially costly for banks during times of uncertainty. Given that states with judicial status laws experience roughly half as many foreclosures as their counterparts, we find their greater marginal effect especially interesting.

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. We now discuss these results briefly. Under our baseline specification in column 3, we find that a 10% rise in foreclosures is associated with a 2.13% decline in hiring and a 0.12 percentage point decline in job turnover. Again, these results are strongly robust to using variation in 2-1 and 3-1 loans and/or robust to controlling for logged county × industry earnings as a proxy for other local contemporaneous shocks. While we do not find much heterogeneity between the tradables and non-tradables sector when job turnover is our outcome variable, we do find that the tradables sector is impacted 77% more with a 4.55% decline in hires for a corresponding 10% rise in foreclosures (versus a 2.57% decline in hiring for non-tradables). We again find similar evidence of heterogeneity across states with and without judicial status laws under both outcome variables—for example, states without judicial foreclosure laws have nearly a 1% and a 0.05 percentage point weaker foreclosure-hiring and foreclosure-turnover gradient, 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. 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 lay offs 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

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71 We have also experimented with the 90/10 income ratio and the Gini coefficient from the Census both at a county level.
-0.03 when we use the dispersion in logged adjustable gross income (see Appendix Section A.5.1).

Appendix Section A.5.2 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 34). 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 employment growth and hiring rate can be attributed to the surge in foreclosures? 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 three different samples of observations: (i) firms with between 20 and 249 employees, (ii) firms in the non-tradables sector, and (iii) firms in the tradables sector. 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 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,

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72 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.
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 2 documents these results. We find that the rise of foreclosures explains roughly 10% of the decline in the hiring rate overall. However, the effects are highly 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% of the decline in the hiring rate, whereas it explains nearly 13% in states with 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 us to underestimate our aggregate effects.

5.3 Robustness and Extensions

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).

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.5.3, we find that increases in foreclosures have, if anything, negative spillovers on neighboring counties.
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 1, 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.\footnote{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.}

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 3 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}
\caption{Table 3}
\label{tab:table3}
\end{table}

Our second diagnostic exercises gauges the 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 15 in Appendix Section A.5.4 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. 39 in Appendix Section A.5.4 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 $R^2$-squared of our unemployment growth regression from 7.5% to 24%, whereas it keeps the $R^2$-squared for our income growth regression from 8.2% to 8.4%. In this sense, duration choices and their corresponding interest rate shocks predicts labor market outcomes, but not potential confounders, like income.

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.5.5 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.

5.3.3 Examining the Potential for Reverse Causality

We provide further evidence that our main results are not driven by contemporaneous shocks to both the labor market and interest rates by using our Bartik-like instrumental variables strategy. By exploiting a county’s pre-recession exposure to banks that were originating different types of loans, we can hold fixed subsequent compositional changes within a county. For example, consider two counties that see a large number of mortgage originations in 2003. One county has a high market share of banks that tend to lend more 5-1 ARMs, and thus will tend to see interest rates
reset in 2008, whereas the other county has a high market share of banks that favor 7-1 ARMs, and as a result will tend to see interest rates reset in 2010. Our identifying assumption is that these initial 2003 bank shares are orthogonal to changes in our outcome variable between 2005-2010.

Table 4 documents these results. We find a close correspondence between our baseline results and those displayed here. For example, a 10% rise in foreclosures is associated with a 0.36% decline in employment when we use our Bartik-like instrument, but our main results suggest the association is closer to a 1.16% decline, on average. Similarly, a 10% rise in foreclosures is associated with a 0.7 and 0.8 percentage point decline in the growth rate of employment and employee turnover, which are strikingly similar to our main results of 0.8 and 0.5 percentage points, respectively. The only difference is that our employment growth rate regressions are statistically insignificant at conventional levels. While our Bartik-like measure generates coefficients that are less precise than those in our baseline instrumental variable strategy, they still illustrate that our main results are not driven by strategic income targeting among banks.

[INSERT TABLE 4]

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. 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, especially for firms in the tradables sector. Using a sample

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76 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.
of local banks, we also show that our estimates are not driven by contemporaneous and direct effects of foreclosures on bank portfolios, which could also reduce lending.

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 11 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 5 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.

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77We 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.
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. Not surprisingly, increases in local bank deposits have a positive association with sentiment, whereas mortgage payments due have a negative association.

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.\(^{78}\) 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.

What if there are other confounding local factors that are correlated with individual beliefs? We now use the full granularity of our data by controlling for county × quarter × year fixed effects, exploiting within-zipcode variation in interest rate resets that give rise to higher or lower probabilities of foreclosure through our instrument. Under this specification, we find that a 10% rise in foreclosures is associated with a 0.67% \((p\text{-value} = 0.00)\) decline in sentiment coefficient about the current state of the economy.\(^{79}\)

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

\(^{78}\) This is on top of our mortgage payments that come due in a given quarter.

\(^{79}\) 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.
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 5]

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 12, 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.

While our above results provide 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—they do not provide a rationale for heterogeneity in our elasticity between foreclosures and labor market outcomes in tradables versus non-tradables sectors. The fact that we find a stronger gradient among tradables suggests that the explanation cannot rely on local demand as in Mian and Sufi (2014). We, therefore, turn towards evidence over the risk premium and precautionary channels using our loan-level data from the SBA. While all small businesses tend to depend on bank lending as a source of financing (Cole et al., 1996), firms in the tradables sector are especially dependent on credit and may, therefore, experience a greater foreclosure gradient. For example, loans are roughly $85,000 greater in the tradables sector, which amounts to 36% of the average loan value.\footnote{Although our focus on small and medium sized businesses is motivated by the fact that the bulk of the decline in employment took place within these businesses during the recession, one possible concern is that the bank lending channel is less relevant for larger firms. To address this concern, we examined differences in credit using the set of Compustat firms and found that current liabilities per employee are 17% higher in the tradables sector, relative to non-tradables, even with these larger set of firms (see Figure 46 in Appendix Section A.6.2).}

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) the interest rate on the loan, and (iii) the riskiness of a loan. While the first two are given in our data, we obtain the riskiness of a loan by taking the predicted values from a logit regression of loan default on loan-level characteristics, including: the logged value of the loan, the logged amount of SBA backing, fixed effects for the type of business (individual [20.78%], partnership [2.56%], corporation [76.66%]), and an indicator for whether the loan is a continuing line of credit or a one-time lump sum.

Table 6 documents these results. Beginning with columns 1 and 2, we find that a 10% rise in foreclosures is associated with a 0.58% and 0.38% decline in the share of the loan that a bank is willing to lend to a small business in the tradables and non-tradables sectors, respectively. We can reject the null that they are equal to one another. The fact that the average loan is also 36% larger in the tradables sector further highlights the more adverse effects of foreclosures on tradables firms with loans. Turning towards columns 3 and 4, we find that a 10% rise in foreclosures is associated with a 0.59% and 0.46% decline in interest rates on initial loans in the tradables and non-tradables sectors, respectively. Although the negative association appears counter intuitive at first since foreclosures raise default risk, which should affect the cost of external financing, it is consistent with imperfect pass-through of risk and points towards the a change in the composition of loans. To further address the concern that our results are driven by spurious declines in housing net worth, which behaves as 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).

\[82\] The fact that a 1% rise in foreclosures is associated with a 0.058pp and 0.038pp decline in the share a bank lends is significant given that the mean for tradables and non-tradables is 0.348 and 0.363, respectively.
While our estimates in columns 1 and 2 show that banks are less willing to lend, we examine more explicit evidence for a composition effect by turning towards our estimated default probabilities. We find that a 10% rise in foreclosures is associated with a 0.023 and 0.009 percentage point rise in the default risk of loans among firms in the tradables and non-tradables sectors. First, the magnitude on the foreclosure coefficient in the tradables sector is over 2.5 times as large as the coefficient on foreclosures in the non-tradables sector, reflecting the fact that foreclosures are more heavily affecting credit in the former sector. Second, the fact that the gradients are both positive reflects the fact that foreclosures are priced into the default risk of a loan. 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.

[INSERT TABLE 6]

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. Unlike the 7(a) lending program, this one works by distributing the loan among three parties: the business owner (a minimum of 10%), a conventional lender (i.e., bank), and a Certified Development Company. The average loan is $585,205 and firms in the tradables sector receive $134,251 (23%) more relative to their non-tradables counterparts. Table 20 in Appendix Section A.6.2 provides results along the lines of Table 6 for these set of loans. Overall, our results also reinforce those from Gupta (2016) who finds suggestive evidence of a lender-driven response to local foreclosure shocks whereby many individuals continue paying their mortgage payment, but neither default nor refinance.

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.

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83 We also examined logit regressions of default on logged foreclosures, conditional on controls, finding that a 10% rise in foreclosures is associated with a 0.6 percentage point rise in the probability of default. County controls include: population, housing prices, bank deposits, mortgage payments. Loan controls include: business type fixed effects and revolver status.

84 Although national banks are much larger than local banks—for example, we find that they loan 109% more in
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 7 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).85 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.86

[INSERT TABLE 7]

### 6.3 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; 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.

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86We 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.
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 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 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 x industry data on advertising and rental expenditures.

### 6.4 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

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87 However, there are some exceptions. For example, Mondragon (2015) who focuses on the decline in household credit.
better outside options and a willingness to pay for higher amenity areas.\textsuperscript{88}

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 8 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 8]

In Appendix Section A.6.3, we also examine an analogue of Table 8, but with earnings, rather

\textsuperscript{88}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).
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{89} 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.

While college attainment is a useful proxy for skill, it is still crude. We now refine our strategy further by turning towards micro-data from over eight 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.\textsuperscript{90} Figure 13 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.

\[\text{[INSERT FIGURE 13]}\]

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 \textsuperscript{89}Sorkin (2015) shows that approximately two-thirds of the variation in earnings is driven by compensating differentials.\textsuperscript{90}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.\]
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.\footnote{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.}

### 6.5 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 1). 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, forthcoming; 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 0.85% decline in employment and 2.13% decline in hiring, but these declines are larger among firms in the tradables sector and small businesses. Using our estimated elasticities, we find that the observed surge in foreclosures during the financial crisis explains 10-16% of the decline in the hiring rate. However, we find significant heterogeneity. For example, the rise of foreclosures explains more of the decline in hiring in states with judicial status laws, which slowed the process of foreclosure, in spite of the fact that they had a smaller increase in foreclosures during the crisis years, consistent with evidence from Pence (2006) that these laws raise costs. Our results complement the evidence from Mian and Sufi (2014) that housing wealth shocks drove the employment decline in non-tradables by providing additional evidence for understanding the employment decline, especially among the tradables sector.

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 leads to a 0.58 and 0.38 percentage point decline in the share that a bank is willing to lend to local small businesses in the tradables and non-tradables sectors. 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).

7.1 References

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: Employment Rate by Firm Size, 1998-2015

Figure 3: 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 4: 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 5:** 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.
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).
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 8: 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 9: 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.
\[
\ln(\text{foreclosures}) = -0.0084 + 7.1 \ln(2^{-1/3} - 1 \text{ resets})
\]

\[
\ln(\text{foreclosures}) = -0.031 + 4.75 \ln(\text{other resets})
\]

\[
\ln(\text{foreclosures}) = 0.0012 + 0.864 \ln(\text{reset increase})
\]

\[
\ln(\text{foreclosures}) = 0.0026 - 1.35 \ln(\text{reset decrease})
\]

**Figure 10:** 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.

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<thead>
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</tr>
<tr>
<td>All</td>
</tr>
<tr>
<td>Tradables</td>
</tr>
<tr>
<td>Non-Tradables</td>
</tr>
<tr>
<td>Judicial</td>
</tr>
<tr>
<td>Non-Judicial</td>
</tr>
<tr>
<td>Small Firms</td>
</tr>
<tr>
<td>262%</td>
</tr>
<tr>
<td>262%</td>
</tr>
<tr>
<td>262%</td>
</tr>
<tr>
<td>111%</td>
</tr>
<tr>
<td>379%</td>
</tr>
<tr>
<td>262%</td>
</tr>
<tr>
<td>Hiring Rate</td>
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<tr>
<td>24.4%</td>
</tr>
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</tr>
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<td>26.78%</td>
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**Table 2:** 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 × industry data is used to obtain tradables and 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: \( \hat{\gamma} \times (\Delta f/\Delta y) \) 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 1: Baseline Effects of Foreclosures on Employment, Hiring, and Job Turnover

<table>
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<th>Dep. var.</th>
<th>logged county-by-industry employment</th>
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<tbody>
<tr>
<td>all</td>
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</tr>
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</table>

**Panel A**

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<th>ln(foreclosures)</th>
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<tr>
<td>× non-judicial st</td>
<td>.026**</td>
<td>[.011]</td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
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<td>.85</td>
<td>.85</td>
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<td>1470055</td>
<td>1470055</td>
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**Panel B**

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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 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.
Table 3: Examining the Correlation between ARMs and Income Fluctuations

<table>
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<th>type-k ARMs as a share of total hybrid ARMs</th>
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<th>2-1</th>
<th>3-1</th>
<th>3-1</th>
<th>5-1</th>
<th>5-1</th>
<th>7-1</th>
<th>7-1</th>
<th>10-1</th>
<th>10-1</th>
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<td>ln(filers, under 10K)</td>
<td>-.020***</td>
<td>-.008**</td>
<td>-.010***</td>
<td>-.002</td>
<td>-.014***</td>
<td>-.003</td>
<td>-.003***</td>
<td>.000</td>
<td>-.001</td>
<td>.001</td>
</tr>
<tr>
<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>-.011</td>
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<td>ln(filers, 75-100K)</td>
<td>-.007</td>
<td>-.003</td>
<td>.004</td>
<td>.010**</td>
<td>-.017***</td>
<td>-.009*</td>
<td>-.002*</td>
<td>.002</td>
<td>-.003***</td>
<td>-.000</td>
</tr>
<tr>
<td>ln(filers, 100-200K)</td>
<td>.002</td>
<td>.009</td>
<td>-.007***</td>
<td>-.000</td>
<td>.015***</td>
<td>.004</td>
<td>.003***</td>
<td>-.006***</td>
<td>.003***</td>
<td>-.000</td>
</tr>
<tr>
<td>ln(filers, above 200K)</td>
<td>-.005***</td>
<td>-.005</td>
<td>-.001</td>
<td>-.002</td>
<td>.001</td>
<td>-.003</td>
<td>.000</td>
<td>.001</td>
<td>.000</td>
<td>.001</td>
</tr>
<tr>
<td>R-squared</td>
<td>.07</td>
<td>.48</td>
<td>.05</td>
<td>.38</td>
<td>.20</td>
<td>.64</td>
<td>.09</td>
<td>.45</td>
<td>.06</td>
<td>.37</td>
</tr>
<tr>
<td>Sample Size</td>
<td>11828</td>
<td>11794</td>
<td>11828</td>
<td>11794</td>
<td>11828</td>
<td>11794</td>
<td>11828</td>
<td>11794</td>
<td>11828</td>
<td>11794</td>
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<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</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 4:** Exploiting Initial Bank Exposure to Identify Labor Market Effects

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>ln(employment)</th>
<th>employment growth</th>
<th>turnover rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>-0.036**</td>
<td>-0.007</td>
<td>-0.008***</td>
</tr>
<tr>
<td>[0.017]</td>
<td>[0.006]</td>
<td>[0.002]</td>
<td></td>
</tr>
<tr>
<td>R-squared</td>
<td>0.87</td>
<td>0.09</td>
<td>0.55</td>
</tr>
<tr>
<td>Sample Size</td>
<td>744543</td>
<td>700943</td>
<td>688299</td>
</tr>
<tr>
<td>County Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>2-digit Industry FE</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>
</tr>
<tr>
<td>Time FE</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, 2005-2010. The table reports the coefficients associated with regressions of two-digit industry-by-county logged employment, the change in logged employment, and the turnover rate on logged county foreclosures, logged housing prices (index with 2000 base year), 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. 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 a Bartik-like measure that takes the sum over the inner product of a county’s 2003 market share over a type-\(k\) ARM (2-1, 3-1, 5-1, 7-1, 10-1), the national time-varying market share of bank \(i\) for a type-\(k\) loan, and the bank’s logged origination. We sum over all banks within a county and multiply these predicted origination by the average reset increase and decrease in a county for a given point in time. Standard errors are clustered at the county-level and county population is used as the sample weight.

**Figure 11:** 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.
months till recovery = $30 + 23 \ln(\text{foreclosures})$

**Figure 12:** 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.

**Figure 13:** 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.
**Table 5: Foreclosure Shocks and Economic Sentiments**

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>ln(state of economy)</th>
<th>ln(future of economy)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Panel A: Mean</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>-.10***</td>
<td>-.14***</td>
</tr>
<tr>
<td></td>
<td>[.02]</td>
<td>[.02]</td>
</tr>
<tr>
<td>ln(payments due)</td>
<td>-.09***</td>
<td>-.15***</td>
</tr>
<tr>
<td></td>
<td>[.02]</td>
<td>[.03]</td>
</tr>
<tr>
<td>ln(deposits)</td>
<td>.03***</td>
<td>.04**</td>
</tr>
<tr>
<td></td>
<td>[.01]</td>
<td>[.02]</td>
</tr>
<tr>
<td>ln(housing price)</td>
<td>-.13***</td>
<td>-.12***</td>
</tr>
<tr>
<td></td>
<td>[.04]</td>
<td>[.04]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.04</td>
<td>.03</td>
</tr>
<tr>
<td>Sample Size</td>
<td>1124294</td>
<td>515882</td>
</tr>
<tr>
<td>Dep. var. =</td>
<td>ln(state of economy)</td>
<td>ln(future of economy)</td>
</tr>
<tr>
<td><strong>Panel B: Standard Deviation</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>.01*</td>
<td>.01**</td>
</tr>
<tr>
<td></td>
<td>[.01]</td>
<td>[.01]</td>
</tr>
<tr>
<td>ln(payments due)</td>
<td>.01**</td>
<td>.00</td>
</tr>
<tr>
<td></td>
<td>[.00]</td>
<td>[.00]</td>
</tr>
<tr>
<td>ln(deposits)</td>
<td>-.02***</td>
<td>-.01***</td>
</tr>
<tr>
<td></td>
<td>[.00]</td>
<td>[.00]</td>
</tr>
<tr>
<td>ln(housing price)</td>
<td>.05***</td>
<td>.05***</td>
</tr>
<tr>
<td></td>
<td>[.01]</td>
<td>[.01]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.14</td>
<td>.14</td>
</tr>
<tr>
<td>Sample Size</td>
<td>51637</td>
<td>51530</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<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 logged sentiments (current state of the economy [1-4] and future state of the economy [1-3]) on logged county foreclosures, conditional on logged housing prices, 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. 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.
Table 6: Examining the Impact of Foreclosures on Credit Among Small Businesses

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>share of bank guarantee</th>
<th>ln(interest rate)</th>
<th>default probability</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>TR</td>
<td>NTR</td>
<td>TR</td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>-0.058***</td>
<td>-0.038***</td>
<td>-0.059**</td>
</tr>
<tr>
<td>[0.005]</td>
<td>[0.002]</td>
<td>[0.007]</td>
<td>[0.001]</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.28</td>
<td>0.18</td>
<td>0.38</td>
</tr>
<tr>
<td>Sample Size</td>
<td>54399</td>
<td>688594</td>
<td>15350</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Zipcode 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>
<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), the logged interest rate in percentage points, 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. 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 7: Local Foreclosure Shocks Controlling for Bank Balance Sheet Quality

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>commercial loans, growth rate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>-0.049***</td>
</tr>
<tr>
<td>[0.010]</td>
<td>[0.010]</td>
</tr>
<tr>
<td>ln(assets)</td>
<td>-0.012***</td>
</tr>
<tr>
<td>[0.001]</td>
<td>[0.011]</td>
</tr>
<tr>
<td>ln(deposits)</td>
<td>-0.017***</td>
</tr>
<tr>
<td>[0.002]</td>
<td>[0.012]</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.02</td>
</tr>
<tr>
<td>Sample Size</td>
<td>129429</td>
</tr>
<tr>
<td>F-statistic</td>
<td>16.5</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
</tr>
<tr>
<td>County 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, 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, logged county housing prices, 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 8: Foreclosure Shocks and Net Migration Flows Across Skilled Brackets

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>logged college employment net of ...</th>
<th>college employment growth net of ...</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>-.0308*** [-.0089]</td>
<td>-.0135* [.0075]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.91</td>
<td>.91</td>
</tr>
<tr>
<td>Sample Size</td>
<td>1872742</td>
<td>1847734</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year FE</td>
<td>Yes</td>
<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 logged housing prices, controls, and fixed effects. The controls include: the fraction of individuals in the county who are male, married, between ages $k \leq k < k$, where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education $k \leq k < k$, 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. 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 Online Appendix (Not for Print)

#### A.1 Supplement to Introduction

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 14. 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 15. 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 14: 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 15: 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.
A.2 Data Construction

A.2.1 Small Business Loans

In order to be approved as an SBA 7a loan, the application must meet certain criteria, such as maximums on loan amount, interest rate, and so on. Any loan meeting the basic criteria, and initiated by a lender accredited by the SBA to make such loans, is automatically approved by the SBA. Once approved, the bank can choose to purchase default insurance from the SBA to cover up to 75% or 85% of the loan’s value (depending on loan size). The SBA charges a fixed portion of the guaranteed amount for this insurance. While the SBA does not directly assess loan risk when either approving a loan to guarantee, or when deciding the price to charge for its guarantee, if a lender consistently has default levels that far exceed program guidelines then their ability to further participate in the program may be curtailed. Small businesses may use 7a loans for a variety of operational expenses. Similar provisions apply for the 504 loans, such that the SBA is not involved in directly reviewing the loan prior to issuing its guarantee of the CDC’s bond, but a CDC that consistently performs poorly may be removed from the program. The bank receives no default insurance from the SBA for its contribution to a 504 loan.

We obtained this data via a Freedom of Information Act (FOIA) request, giving us detailed information on, for instance, the location of the business taking out the loan, the industry that business is in, the total amount of the loan, and the amount of insurance purchased by the bank originating the loan from the SBA. We use this data to tabulate county-quarter panels measuring both total business lending (in aggregate and by industry), as well as the average percent of total loan balances that are not guaranteed by the SBA. This allows us to test both whether foreclosures led banks to make fewer total of these type of loans in areas impacted by foreclosures as well as whether the foreclosures caused the banks to be willing to place less of their own money at risk in those loans.

A.2.2 Census Bureau

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, 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 Data

We begin by providing a glimpse of the variation in foreclosures we observe. Figure 16 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 16: 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 17. 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 17: 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 tradables observations net of the logged number of missing non-tradables 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 9 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 related,
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 18 shows a similar result partitioning the Herfindahl index into ten bins and presenting the foreclosure elasticities separately for each.

**Table 9:** Foreclosure Heterogeneity in Tradables and Non-Tradables

<table>
<thead>
<tr>
<th>Dep. var. = ln(hiring)</th>
<th>all</th>
<th>TR</th>
<th>NTR</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>-.126***</td>
<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>
<td>.77</td>
<td>.57</td>
<td>.80</td>
</tr>
<tr>
<td>Sample Size</td>
<td>2128265</td>
<td>222570</td>
<td>1905695</td>
</tr>
<tr>
<td>Baseline Controls</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>
</tr>
<tr>
<td>Time FE</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 county $\times$ 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 18: 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 19. 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 20, showing that there is also quite a bit of variation in the incidence of these mortgages across space.

**Figure 19:** Difference in the Share of 5-1 v. 7-1 and 5-1 v. 10-1 ARMs

*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.
Figure 20: 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 21 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.
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 22. 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 23 plots histograms across individuals, showing that the high degree of overlap in Figure 22 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 24 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 25 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).

**Figure 22:** 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 23: 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 24: 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.
Figure 25: 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 10 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 4 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 10: 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>
</tr>
</thead>
<tbody>
<tr>
<td>income, 90-00 growth</td>
<td>.01</td>
<td></td>
<td></td>
<td></td>
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<td>[.01]</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>housing price, 90-00 growth</td>
<td>.01</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>[.01]</td>
<td></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>[.00]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>unemployment, 90-00 growth</td>
<td></td>
<td>.00</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[.00]</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<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 26: 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 27 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 27: 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 28 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 28: 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 28 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 exploit our interest rate resets, which make it more difficult for borrowers
on these ARMs to pay their mortgage if the interest rate grows and easier to pay if the interest rate declines. Table 11 documents these associations starting with the simple least squares estimator, moving towards the fixed effects estimator, and then finally instrumenting ARM foreclosures with the resets. Not surprisingly, our IV estimate is lower in magnitude than our OLS and FE estimates, but is still highly economically and statistically significant, consistent with the presence of foreclosure spillovers.

Table 11: Evidence of Foreclosure Spillovers from ARMs to FRMs

<table>
<thead>
<tr>
<th>Dep. var. = ln(foreclosures on ARMs)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>.296***</td>
<td>.147***</td>
<td>.108***</td>
</tr>
<tr>
<td>[0.007]</td>
<td>[0.006]</td>
<td>[0.010]</td>
<td></td>
</tr>
<tr>
<td>R-squared</td>
<td>.84</td>
<td>.93</td>
<td>.93</td>
</tr>
<tr>
<td>Sample Size</td>
<td>32110</td>
<td>32109</td>
<td>32109</td>
</tr>
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<td>3791</td>
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<td>Baseline Controls</td>
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<td>Yes</td>
</tr>
<tr>
<td>County FE</td>
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<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Instruments</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes.–Sources: CoreLogic, Census, Federal Housing Administration, 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. 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.

As an additional placebo, we examine how the elasticity under our IV specification varies across the distribution of counties with ARMs. 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 29 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.
Figure 29: 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 5. 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.

![Figure 30: Employment Elasticities of Housing Wealth Shocks, by Year 2000-2014](image)

**Notes.** Sources: Longitudinal Employer-Household Dynamics (LEHD), CoreLogic, Federal Housing Administration. The figure plots the coefficients from regressions of county-by-industry employment growth on county housing price growth separately by year. Standard errors are clustered at the county level and observations are weighted by county population.

We now turn towards more formal regressions where we regress logged employment and employment growth on housing price growth with a comprehensive set of demographic controls and fixed effects on two-digit industry, county, and time (year and quarter)

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

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 12 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 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 12: Replication of Mian and Sufi (2014)

<table>
<thead>
<tr>
<th></th>
<th>Dep. var. = logged employment</th>
<th>employment growth</th>
</tr>
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<tbody>
<tr>
<td></td>
<td>ALL</td>
<td>ALL</td>
</tr>
<tr>
<td>∆ ln(housing price)</td>
<td>.36***</td>
<td>.16***</td>
</tr>
<tr>
<td></td>
<td>[.06]</td>
<td>[.02]</td>
</tr>
<tr>
<td>R-squared</td>
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<td>.89</td>
</tr>
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<td>Sample Size</td>
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<td>Controls</td>
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<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 $k \in [k, K]$ 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. 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

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% ($= 1−\exp(10.69−10.40)$). Individuals in

---

Figure 31: First-stage Correlation Between Foreclosures and Instrument, by Year
Notes. Sources: CoreLogic, 2000-2014. The figure plots the correlations between residualized foreclosures predicted by 5-1, 7-1, and 10-1 ARM interest rate resets and residualized actual foreclosures at the county-level by year.
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](image)

**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 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 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.
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 13 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{94} 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{95} 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

\textsuperscript{94}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{95}For example, see http://www.huffingtonpost.com/ray-brescia/when-the-rich-get-risky_i_b_695535.html.
Table 13: 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>-.05***   -.00  -.02  -.01</td>
<td>-.05***   -.00  -.03**  -.02***</td>
</tr>
<tr>
<td></td>
<td>[.01]     [.00]  [.02]  [.01]</td>
<td>[.01]     [.00]  [.01]  [.01]</td>
</tr>
<tr>
<td>R-squared</td>
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<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>F</td>
<td>.01       .02   .01   .02</td>
<td>.01       .02   .01   .02</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.

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.

A.5.2 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 33 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.
Figure 33: Foreclosure Heterogeneity in Employment Elasticity, by Industry

Notes.– Sources: Longitudinal Employer Household Dynamics (LEHD), CoreLogic, Census, Federal Housing Administration, 2000-2014. The figure reports the coefficients (separately by NAICS two-digit industry) associated with regressions of logged county 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.

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 34. 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.
Figure 34: 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 35. 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 36, 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 35: 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 36: 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. Figure 37 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.

Figure 37: 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 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

\[96\]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 38 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 38: 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

$$y_{ct} = f(X_{ct}, \beta) + \gamma_1 f_{ct} + \gamma_2 1[\Delta h_{ct} < 0] + \delta(f_{ct} \times 1[\Delta h_{ct} < 0]) + \eta_j + \psi_c + \lambda_t + \epsilon_{ct} \quad (5)$$

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).\textsuperscript{97}

We estimate Equation 5 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.5.3 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

$$\text{JobGrowth}_{-c,t} = f(X_{ct}, \beta) + \gamma f_{ct} + g(h_{ct}, \theta) + \phi_c + \lambda_t + \epsilon_{ct} \quad (6)$$

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

\textsuperscript{97}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.
and time fixed effects. We estimate Equation 6, 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 14 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 (reallocations), spillovers.

Table 14: Examining the Potential for Reallocation

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>job growth, normalized mean</th>
<th>job growth, normalized maximum</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>-0.00***</td>
<td>-0.01*</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.05</td>
<td>0.41</td>
</tr>
<tr>
<td>Sample Size</td>
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<td>46377</td>
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<tr>
<td>Controls</td>
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<td>Yes</td>
</tr>
<tr>
<td>County 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: 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.5.4 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 $R^2$ 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 15 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><strong>Δ = 0.10</strong></td>
<td></td>
</tr>
<tr>
<td>upper</td>
<td>-0.510</td>
</tr>
<tr>
<td></td>
<td>-0.380</td>
</tr>
<tr>
<td>lower</td>
<td>-0.065</td>
</tr>
<tr>
<td></td>
<td>-0.102</td>
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<tr>
<td>Δ = 0.20</td>
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<tr>
<td>upper</td>
<td>-0.750</td>
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<tr>
<td></td>
<td>-0.540</td>
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<tr>
<td>lower</td>
<td>-0.065</td>
</tr>
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<td></td>
<td>-0.101</td>
</tr>
</tbody>
</table>

**Table 15:** 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. $Δ$ denotes the degree of bias, i.e. $Δ = 0.20$ 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 39 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 $R^2$ values in both cases. We see, for example, that the $R^2$ 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 39 shows very clearly that these interest rate shocks predict employment outcomes, but not future income growth.

**Figure 39: 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 39 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 40 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.

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

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

Figure 40: 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 16 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 16: 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>
</tr>
</thead>
<tbody>
<tr>
<td>ln(ARM reset)</td>
<td>-3.44***</td>
<td>-.50</td>
<td>-.41</td>
<td>-1.34</td>
</tr>
<tr>
<td>Δ ln(housing price)</td>
<td>-26.98**</td>
<td>34.75</td>
<td>-6.55</td>
<td>35.22</td>
</tr>
<tr>
<td>[11.84]</td>
<td>[24.89]</td>
<td>[17.81]</td>
<td>[31.72]</td>
<td>[16.47]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.18</td>
<td>.18</td>
<td>.26</td>
<td>.26</td>
</tr>
<tr>
<td>Sample Size</td>
<td>49756</td>
<td>49756</td>
<td>49756</td>
<td>49756</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State FE</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<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 17 subsequently examines the effects of foreclosures on the allocation of time, measuring time use in logs to represent an elasticity. These specifications are estimated by instrumenting for foreclosures using state interest rate resets. Although the $F$-statistic is above 10, it is just marginally above since all of the county variation is removed. While there is a potential negative association with the allocation of time towards leisure, even if it was statistically significant and different from zero, it would still not be economically significant.

We now examine the possibility that, because interest rate resets affect disposable income, they could also affect local labor market outcomes by affecting local demand (Di Maggio et al., 2017a). While we do not have access to micro-level consumption expenditure data at a location-level, we turn towards the Bureau of Economic Analysis (BEA) state level consumption expenditure data between 2000 and 2014. Figure 41 plots residualized state × year logged interest rate resets with different measures of consumption expenditures (using state and year fixed effects as controls). While there is a correlation between total consumption expenditures and interest rate resets (Panel
Table 17: 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>
<tr>
<td>ln(foreclosures)</td>
<td>-.058</td>
<td>-.007</td>
<td>-.003</td>
</tr>
<tr>
<td></td>
<td>[.063]</td>
<td>[.010]</td>
<td>[.008]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.07</td>
<td>.23</td>
<td>.21</td>
</tr>
<tr>
<td>Sample Size</td>
<td>49756</td>
<td>49756</td>
<td>49756</td>
</tr>
<tr>
<td>F-statistic</td>
<td>11.9</td>
<td>11.9</td>
<td>11.9</td>
</tr>
<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.
\[ \ln(\text{consumption}) = -5.2e^{-11} + 0.024 \ln(\text{reset}) \]

Panel A: Durable goods

\[ \ln(\text{consumption}) = 5.5e^{-11} + 0.027 \ln(\text{reset}) \]

Panel B: Non-durable goods

\[ \ln(\text{consumption}) = -2.0e^{-11} + 0.083 \ln(\text{reset}) \]

Panel C: Housing and utilities

\[ \ln(\text{consumption}) = -5.6e^{-11} + 0.041 \ln(\text{reset}) \]

Panel D: Total consumption

\textbf{Figure 41:} State Consumption Expenditures and Interest Rate Resets, 2000-2014

Notes.– Sources: CoreLogic and Bureau of Economic Analysis, 2000-2014. The figure plots residualized state \times 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.

\textbf{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.

$$u_{ct} = \beta X_{ct} + \gamma f_{ct} + \sum_{k=2}^{7} \delta^k (f_{ct} \times d^k_c) + \psi_c + \lambda_t + \epsilon_{ct}$$ (7)

We estimate Equation 7 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.\(^{98}\)

Figure 42 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 42 point towards strong non-linearities in the effects of foreclosures on county unemployment rates, but only in states without judicial status laws.

\(^{98}\)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.
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 18 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 18: Examining the Potential for Foreclosure Intensity

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>logged county-by-industry employment</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(cumulative foreclosures)</td>
<td>All</td>
</tr>
<tr>
<td>-.170***</td>
<td>-.089***</td>
</tr>
<tr>
<td>R-squared</td>
<td>.87</td>
</tr>
<tr>
<td>Sample Size</td>
<td>1751422</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry FE</td>
<td>Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Instruments?</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 \( k \in [k_3, k_4] \) 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.5.6 Selective Pre-payment and Different Loans

The main text describes one potential concern with our identification strategy discussed in detail by Fuster and Willen (forthcoming): 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 (forthcoming). 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 43 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 43:** Distribution of Employment Growth Across Counties, High/Low ARMs

*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 44: 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.
Figure 45: Perceptions of the State of the Economy with the Economic Policy Uncertainty

Sources.—Baker et al. (2016) 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 19 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 19: Examining the Importance of Real Options in Hiring Declines

<table>
<thead>
<tr>
<th>Dep. var. = county-by-industry hiring/employment</th>
<th>WD</th>
<th>NWD</th>
<th>WD</th>
<th>NWD</th>
<th>HNC</th>
<th>LNC</th>
<th>HNC</th>
<th>LNC</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>-.008***</td>
<td>-.011**</td>
<td>-.011***</td>
<td>-.016***</td>
<td>-.026***</td>
<td>-.004</td>
<td>-.025***</td>
<td>-.008***</td>
</tr>
<tr>
<td>R-squared</td>
<td>.35</td>
<td>.35</td>
<td>.48</td>
<td>.49</td>
<td>.32</td>
<td>.36</td>
<td>.45</td>
<td>.47</td>
</tr>
<tr>
<td>Sample Size</td>
<td>156358</td>
<td>24015</td>
<td>530911</td>
<td>84544</td>
<td>123613</td>
<td>56760</td>
<td>416656</td>
<td>198799</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</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>
<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>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Instrument?</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Tradables?</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>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 46 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 46: 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 47 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 47: Foreclosure Heterogeneity in SBA Lending, by Tradability Herfindahl Index](image)

*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. 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 \( \times \) 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 20 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 20: Examining the Impact of Foreclosures on Credit Among Small Businesses (504 Lending Program)

<table>
<thead>
<tr>
<th></th>
<th>TR</th>
<th>NTR</th>
<th>TR</th>
<th>NTR</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>-.011**</td>
<td>-.009***</td>
<td>-.001*</td>
<td>-.001***</td>
</tr>
<tr>
<td></td>
<td>[.005]</td>
<td>[.002]</td>
<td>[.000]</td>
<td>[.000]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.45</td>
<td>.18</td>
<td>.87</td>
<td>.16</td>
</tr>
<tr>
<td>Sample Size</td>
<td>8537</td>
<td>71958</td>
<td>8537</td>
<td>71958</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Zipcode FE</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>
</tr>
<tr>
<td>Instrument?</td>
<td>Yes</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) 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 48. 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 48: 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 49 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.
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

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

(8)
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.\(^9\) In estimation of Equation 8, 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 21 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 50 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.\(^1\)


\(^1\)https://www.brookings.edu/blog/up-front/2014/04/28/the-unequal-burden-of-crime-and-incarceration-on-americas-poor/
Table 21: Foreclosure Shocks and Crime

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>ln(total crime)</th>
<th>ln(violent crime)</th>
<th>ln(property crime)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>.252**</td>
<td>.367**</td>
<td>.053***</td>
</tr>
<tr>
<td>ln(payments due)</td>
<td>-.337</td>
<td>-.954**</td>
<td>-.078**</td>
</tr>
<tr>
<td>ln(deposits)</td>
<td>.000</td>
<td>.013</td>
<td>.008</td>
</tr>
<tr>
<td>ln(adj gross income)</td>
<td>-.097</td>
<td>.021</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[.612]</td>
<td>[.099]</td>
<td></td>
</tr>
<tr>
<td>R-squared</td>
<td>.91</td>
<td>.89</td>
<td>.89</td>
</tr>
<tr>
<td>Sample Size</td>
<td>15526</td>
<td>11815</td>
<td>15429</td>
</tr>
<tr>
<td>Controls</td>
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<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>County 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: 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 \( k \in [k, k] \) 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. 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 8). 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 22 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.
Table 22: Foreclosure Shocks and Relative Earnings Across Skilled Brackets

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>logged college earnings net of</th>
<th>college earnings growth net of</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>non-college</td>
<td>some-college</td>
</tr>
<tr>
<td></td>
<td>.0290***</td>
<td>.0273***</td>
</tr>
<tr>
<td></td>
<td>[.0065]</td>
<td>[.0056]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.57</td>
<td>.56</td>
</tr>
<tr>
<td>Sample Size</td>
<td>1718145</td>
<td>1689001</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
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<td>County FE</td>
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</tr>
<tr>
<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 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.

### A.6.4 Entrepreneurship and Business Expansion

In the main text, we examined the causal effect of foreclosures on small business lending through the 504 and 7(a) Small Business Administration programs, finding significant adverse associations. We also examine the degree of heterogeneity by sector. Figure 51 plots the estimated coefficients separately by two-digit NAICS industry. Overall, the effect sizes tend to be quite large, although there are a few industries that are unaffected, such as real estate and arts/entertainment, largely because there are very few small business startups applying to the SBA in these sectors.
Figure 51: Foreclosures and 7(a) Small Business Loans, by Industry

Notes. Sources: IPUMS Census, CoreLogic, Small Business Administration. The figure plots the coefficients from regressions of the logged difference between unguaranteed 7(a) loans and total loans (in dollar value) on logged county foreclosures, conditional on housing prices and controls. 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. 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.