FORECLOSURES AND THE LABOR MARKET:
EVIDENCE FROM MILLIONS OF HOUSEHOLDS
ACROSS THE UNITED STATES, 2000-2014*

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In Review

Abstract

Foreclosures per open mortgage grew from 0.20% in 2000 to 2.0% by 2009. Using quarterly county-by-industry and loan-level data between 2000 and 2014, we estimate the impact of foreclosures on local labor markets. 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 1.1% decline in employment, but significant heterogeneity exists across sectors and states. Our estimates imply that the surge in foreclosures during the Great Recession can account for 3-11% and 7-22% of the decline in employment and turnover, respectively. We identify two plausible mechanisms that are independent of the canonical housing price channel and could explain our observed effects: (i) a decline in business investment due to greater local uncertainty, and (ii) a decline in inflows of high skilled workers due to greater local crime and dis-amenities. Our results suggest that embedding foreclosure externalities into macroeconomic models is an important area for future research.

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1. Introduction

Foreclosures per open mortgage spiked by a factor of ten between 2000 and 2009 (see Figure 1). The correlation between foreclosures and housing prices, unemployment, investment, and real GDP also switched dramatically during this period. For example, the correlation between foreclosures and unemployment (housing prices) was 0.02 (0.88) pre-2008, but 0.56 (-0.39) post-2008. Foreclosures were also associated with considerable declines in the growth of investment and real GDP during the Great Recession. While the traditional view of foreclosures is that they affect real outcomes by eroding the value of the home and its surrounding properties (Campbell et al., 2011; Gupta, 2016; Anenberg and Kung, 2014; Mian et al., 2015), we ask whether the stark reversal of these macro relationships is indicative of a new channel whereby foreclosures amplify financial shocks and slow the subsequent recovery. Using the Great Recession as an experiment, the primary aims of this paper are to: (i) quantify the causal effect of foreclosures on labor market outcomes (conditional on house prices), and (ii) identify plausible mechanisms behind these effects.¹

We develop a comprehensive database on foreclosures, housing prices, and labor market outcomes using a wide array of unique data sources.² Empirically estimating the causal effect of foreclosures on the labor market, however, is fraught with identification problems. In addition to spatial heterogeneity across locations, there are two opposing forces of endogeneity. On one hand, counties that experience declines in employment will experience increases in foreclosures since homeowners will be less able to pay their mortgage absent a job. On the other hand, banks had a strategic incentive to delay foreclosing on areas with large housing price declines since doing so would have required valuing the homes and their market value, which would have placed many

¹The 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.

²We 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.
banks in insolvency.\textsuperscript{3} A related explanation behind foreclosure delay was administrative backlogs since the surge in foreclosures took place in such a short window of time.\textsuperscript{4}

We introduce an identification strategy that exploits the contractual structure of adjustable rate mortgages (ARMs) originated prior to and during the Great Recession. These loans had the feature of specifying a fixed (“teaser”) interest rate on mortgage payments for a certain number of years. However, after the specified period, the interest rates would abruptly change. We show non-parametrically that positive (negative) interest rate resets on these loans are associated with discontinuous spikes (drops) in foreclosure probabilities. Using three million unique ARM loans and nearly 160 million loan-month observations from proprietary CoreLogic data, we estimate the probability of foreclosure on the universe of 5-1, 7-1, and 10-1 loans as a function of the net interest rate spike, allowing for resets of each loan type to impact foreclosure probabilities differently.\textsuperscript{5} We aggregate these predicted foreclosures to the county-level and use them as an instrument for realized foreclosures. Since resets on these loans are contractually determined at the origination of the loan, they are, by construction, exogenous with respect to contemporaneous economic shocks following origination. Even if counties were strategically targeted, exclusion restriction violations would require a discontinuous change in an unobserved variable at precisely the same time as the interest rate resets, which were contractually set at least five years ahead of time.

While a valid concern is that changes in the dispersion of county ARMs are correlated with shocks to local labor market outcomes, even controlling for county fixed effects, housing prices and other local demographics, we provide evidence that the dispersion is driven by historical features of the lending market. First, we find no correlation between the 2003-04 county share of ARMs and county shocks (e.g., household income) between 1990 and 2000, suggesting that dispersion in ARMs is not driven by a particular series of economic shocks. Second, we find that the bulk of the

\textsuperscript{3}Former Treasury Secretary Tim Geithner is famously reported as describing federal programs for homeowners in foreclosure to have been principally designed to slow the pace of foreclosures so as to “foam the runway” for banks, enabling them to avoid recognizing losses on their housing portfolios all at once. http://dealbook.nytimes.com/2014/05/06/what-tim-geithner-got-right/.


\textsuperscript{5}Our identification strategy is similar to several papers. First, Di Maggio et al. (forthcoming) 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. (forthcoming), 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.
variation in bank lending is driven by spatial variation among particular banks, rather than time-varying decisions to enter or exit specific geographic areas. Third, we show that the distribution of FICO scores among individuals with 5-1, 7-1, and 10-1 ARMs are almost identical, suggesting that differences in lending practices is not driven by confounding factors correlated with income.\footnote{Although we discuss each in greater detail in the text that follows, full details are in Table 4 for the first point, Appendix Section A2.1. for the second point, and Appendix Section A2.2. for the third.}

We also control for mortgage payments, which shuts off the income effect that would arise from higher interest rates reducing disposable income (Di Maggio et al., forthcoming).

Despite the evidence of plausibly exogenous variation induced by historical factors that prompted the expansion of banks into different geographies, we implement several other robustness exercises. First, using data from the Internal Revenue Service (IRS), we show that the share of ARMs originated in each of the different loan categories has no pattern with the number of filers in different income brackets at the county-year-level. 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. Third, we implement an alternative Bartik-like instrumental variables strategy, which exploits the heterogeneous exposure counties have to different banks as in Mondragon (2015), finding similar results.\footnote{Our approach is conceptually similar to Nguyen (2016) in that we construct a Bartik-like instrument using bank-level data that exploits heterogeneity in the pre-recession exposure of each area to banks with different portfolio strategies and obtain similar results.}

Using two-digit county-by-industry data from the Longitudinal Employer-Household Dynamics (LEHD), we find that a 10% rise of foreclosures is associated with a 1.1% decline in employment, a 0.08 percentage point decline in employment growth, and a 0.05 percentage point decline in the turnover rate. We also find stronger foreclosure gradients for firms in the tradables versus non-tradables sector and for firms in states that do not require judicial foreclosure proceedings as compared to states that do. These dimensions of heterogeneity create very different experiences for counties during the Great Recession. We find no evidence, however, that foreclosures raised intra-county income inequality. These results are all conditional on a semi-parametric function of housing prices, which mitigates concerns that declines in employment are driven by the housing sector (Mian and Sufi, 2014), and on mortgage payments and local bank deposits, which mitigates concerns that interest rate resets are mechanically affecting disposable incomes and employment (Agarwal et al., 2017; Di Maggio et al., forthcoming).\footnote{We have also experimented without housing prices as a control based on the potential concern that we are “over controlling”. Our results are quantitatively similar, but slightly smaller in magnitude since housing prices behave as an omitted variable that amplify the dynamic selection problem that we detail in our section on identification.}
We subsequently introduce two plausible mechanisms to explain our results. The first focuses on the impact of foreclosures on economic perceptions, forecasts, and uncertainty. Using unique micro-data from Gallup’s U.S. Daily poll, we show that foreclosures are associated with declines in perceptions of current and future economic activity, which are linked with uncertainty and declines in business investment. Using county–by-industry measures of investment from the Internal Revenue Service (IRS), together with loans from the Small Business Administration (SBA) obtained through a freedom of information request, we subsequently show that foreclosures depress entrepreneurship and investment through this economic perception and uncertainty channel. These results complement a large literature about the impact of uncertainty on investment on firms and the macroeconomy (Hassler, 1996; Bloom, 2009; Bachmann et al., 2013). The second focuses on the impact of foreclosures on the skill composition of workers. Using employment information on college versus non-college graduates from the LEHD, we show that a 10% rise in foreclosures is associated with a 0.31% decline in the relative composition of college to non-college graduates. Higher income college workers are the most likely to leave following a foreclosure shock. To understand the driving force behind this result, we show that the out-migration of skilled workers is driven by a decline in local amenities (Makridis and Ohlrogge, 2017) and rise in crime (Ellen et al., 2013; Cui and Walsh, 2015), which skilled workers seek to avoid.

Our paper is most closely related with an emerging literature at the intersection of macroeconomics and housing finance; 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. (forthcoming) on consumption, Herkenhoff and Ohanian (2015) on searching and matching in the labor market, and Agarwal et al. (2017) on policy interventions. However, much less attention has been allocated towards the macroeconomic effects of foreclosures themselves (Corbae and Quintin, 2015; Mitman, 2016). Most of the literature to date has focused on how foreclosures impact housing prices (Campbell et al., 2011; Mian et al., 2015; Guren and McQuade, 2013; Gupta, 2016). Our results complement these papers by showing that foreclosures can generate spatial externalities, especially during crisis times (Brunnermeier and Sannikov, 2014). Our results also supplement a related literature on the empirical effects of debtor protections. For example, Dobbie and Song (2015) show that these protections reduce

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9See Brown and Earle (2017) for an evaluation of the causal effects of SBA loans on job growth, which amount to roughly 3 to 3.5 jobs for every million dollars invested.

10Herkenhoff (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.
foreclosure rates and increase long-run earnings and Dobbie and Goldsmith-Pinkham (2015) show that they mitigated the decline in consumption and employment during the recession.

Our paper also 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) shows that individuals might leave an area with declining home prices even if they are not foreclosed upon; see Bernstein and Struyven (2016) and Veldhuizen et al. (2016) for further evidence on the impact of negative home equity. Our results complement these papers by showing that foreclosures can lead to larger scale exits even after controlling for housing prices and even among individuals who are not foreclosed upon. One of the mechanisms behind this result is the fact that areas experiencing a surge in foreclosures also experience an increase in crime (Immergluck and Smith, 2006; Cui and Walsh, 2015), which lowers local amenities and makes individuals more likely to relocate. Anenberg and Kung (2014) also show that the disamenity effects associated with foreclosures are strong in neighborhoods with high housing density and low property values.

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

\[11\] Although Ferreira et al. (2010) finds that a decline in home equity reduces mobility, Schulhofer-Wohl (2012) shows that their result is driven by their dropping some observations with negative home equity homeowner moves; see Coulson and Greico (2013) and Bucks and Bricker (2013) for additional evidence from the Panel Study of Income Dynamics and Survey of Consumer Finances.
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 plausible mechanisms that could be at play in the data.

Below, we examine two major mechanisms through which foreclosures could affect real economic outcomes. We omit a discussion of how foreclosures impact housing prices, i.e. inducing price discounts on neighboring homes (Campbell et al., 2011; Mian et al., 2015; Gupta, 2016). These contributions generally focus on the role of credit as a major determinant of employment and its growth rate (Chodorow-Reich, 2014; Mondragon, 2015) or the role of local demand as a determinant of employment in the non-durables sector Mian and Sufi (2014). We view these as very important mechanisms, but our paper is distinct in that we are examining how foreclosures can affect real labor market outcomes independent of the housing and, therefore, credit channels.

2.1.1. Business Expansion and Entrepreneurship

We begin by discussing how foreclosures can affect business investment and entrepreneurship, particularly for small and new businesses, thereby impacting labor market outcomes independent of the housing price channel. Startups play a fundamental role in explaining job growth (Haltiwanger, 2012). Startups are especially important in the non-tradables sector, where they account for 90% of total net job creation (Adelino et al., 2015a). The housing market crash had a large effect on the supply of credit for not just the overall economy (Mian and Sufi, 2009), but also the supply of credit towards small businesses (Chen et al., 2017). These results are also consistent with Adelino et al. (2015b) who show that the effect of housing price shocks was concentrated among small firms; employment in big firms was largely unaffected.

Motivated by the effects of uncertainty on investment at both a macro (Hassler, 1996; Bloom, 2009) and micro (Bachmann et al., 2013) level, foreclosures can affect investment and employment growth by lowering perceptions of local economic conditions and raising uncertainty about the future economic state. Newspapers and policymakers have pointed towards significant uncertainty associated with how long it would take for homes to get off the market following foreclosure.

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12 Harding et al. (2009) finds that the contagion discount drops rapidly in distance. Calomiris et al. (2013) use a panel vector auto-regression approach to study the linkages between foreclosures and housing prices, finding that the impact of housing prices on foreclosures is 79% larger than the impact of foreclosures on housing prices.

13 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.

Since standard quantitative metrics became less informative for discerning the credit worthiness of borrowers (Keys et al., 2012) and were amplified by asset quality misrepresentation (Piskorski et al., 2015), big banks substituted away from meeting small business credit demand, which was harder to screen for default probabilities (Chen et al., 2017).\footnote{Standard models used to predict default systematically failed during the Great Recession (Rajan et al., 2015).}

In addition to depressing investment from larger firms, the rise of local uncertainty following a foreclosure shock can also reduce the incentive local banks have to lend to businesses, especially small businesses that are credit constrained.\footnote{The increase in uncertainty does not necessarily 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. However, standard stochastic processes will give rise to our results. We thank Steven Grenadier for pointing out these features of stochastic processes in models of real options models.} One obvious reason arises from the fact that a bank’s expected losses on a loan backed by real estate can be represented as the probability of default times the loss given default. While housing price declines increase the loss given default, banks are also likely to factor in information, like perceptions of nearby foreclosures and drops in consumer sentiment, into their assessments of default probabilities. The effects on small businesses might be especially large. For example, Figure 2 plots the employment growth rate for firms of different sizes, illustrating that, although the height of the decline in employment was greatest among firms with 20-49 and 50-249 employees with means of 4.8% and 4.7%, respectively, large firms of 500+ employees closely followed with a decline of 1.2% in the growth rate of employment.\footnote{These results are consistent with those from Patnaik (2016). In this sense, 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.}

Given that foreclosures can create a “brain drain” effect and raise local dis-amenities (which we discuss in the next section), big firms may be deterred from expanding in areas that experience many foreclosures. For example, foreclosures might induce ambiguity about the distribution of local risk, which could curtail investment (Ilut and Schneider, 2014). Higher uncertainty on its own could also discourage investment; see, for example, Bloom (2014) for a survey of available evidence. Firms in the tradables sector may be especially likely to allocate relatively more towards areas with fewer foreclosures since their demand for skilled workers is higher (e.g., indicated by higher average wages) and because they have far more choices of where to operate than non-tradable firms which must operate in the areas where they sell their products. Nonetheless, as a
robustness check to ensure that our data is comparable Mian and Sufi (2014), we replicate their findings using our own dataset according to their sample restriction (see Appendix Section A3.1.).

2.1.2. Mobility and the Composition of Skill

We now turn towards an explanation of how foreclosures can affect the relative skill composition of a labor market, thereby impacting labor market outcomes independent of the housing price channel. Knowledge spillovers and attracting top talent is viewed as a major determinant of local economic development (Glaeser et al., 1992; Moretti, 2004) and a source of endogenous growth (Lucas and Moll, 2014). We specifically omit a discussion of a related literature on negative home equity and mobility, which has found that accepting an offer in another location with better job and housing prospects tends to outweigh the cost of moving (Demyanyk et al., 2017; Bernstein and Struyven, 2016; Veldhuizen et al., 2016).18 We have in mind two potential mechanisms that could explain a decline in the share of skilled workers in a labor market.

First, foreclosures can depress local amenities, which are valued by skilled workers (Glaeser et al., 2001; Glaeser and Mare, 2001), and endogenously change the skill composition of a labor market Diamond (2016). For example, in our companion work, we show that increases in foreclosures are associated with declines in the probability that an individual reports that they are satisfied with their city and that they feel safe walking at night. We also document a negative association between increases in foreclosures and a much broader index of community amenities.

Additionally, foreclosures can raise crime, which may be a particularly large consideration for skilled workers considering moving to or from an area.20

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18 We recognize that there is some controversy over this conclusion. For example, Ferreira et al. (2010) find that homeowners with negative equity move 30% less than their counterparts with positive equity. Karahan and Rhee (2013) develop an equilibrium model with housing lock in matching the spatial dispersion of unemployment observed during the Great Recession, finding that the housing bust explains roughly 10% of the observed increase in unemployment.

19 The index is a score from 1 to 100 that consists of answers to six questions: “I can’t imagine living in a better community than the one I live in today”, “Are you satisfied or dissatisfied with the city or area where you live?”, “The city or area where I live is a perfect place for me”, “I am proud of my community or area where I live”, “I always feel safe and secure”, “The house or apartment that I live in is ideal for me and my family”, and “In the last 12 months, I have received recognition for helping to improve the city or area where I live”. We also do not find an association between foreclosures and, for example, the physical well-being score, which behaves as a placebo test that we are not simply picking up some unobserved heterogeneity.

20 For example, Immergluck and Smith (2006) find that a percentage point rise in foreclosures is associated with a 2.33% increase in violent crimes, but their estimates are identified off of cross-sectional variation. Using more plausibly exogenous variation, Cui and Walsh (2015) and Ellen et al. (2013) refine this analysis, showing that the effects are largely driven by vacancies that occur as a result of foreclosures.
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. These types of loans were encouraged to raise home ownership and became quite common.\footnote{http://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 the share of 5-1, 7-1, & 10-1 ARMs experiences a decline earlier and does not vanish. One of the reasons 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.

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.\footnote{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 4 the correlation between the share of ARMs and the number of tax filers in different income brackets, showing that there is no systematic correlation, suggesting that there is not evidence of income targeting (at least after controlling for location fixed effects).}

If the shares of ARMs in 2003 are not correlated with growth in economic and/or demographic variables in the preceding decade, then why does the dispersion exist? As we document, certain
banks had a preference for issuing one type of ARM versus others. Moreover, banks are remarkably stable in the geographic locations of where they operate. Thus, since banks tend to continue operating where they always have operated, and since some banks favored one type of loan over another, areas that happened to have banks that preferred 5-1 ARMs over 7-1 ARMs tended to get more of the former and fewer of the latter. To formally document these phenomena, we use national bank-by-year data on logged originations of different adjustable rate mortgage types and 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 A2.1.). 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 A2.2., we also plot the distributions of FICO scores across different types of ARMs. 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. 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. (forthcoming) also present additional tests documenting the comparability of borrowers with 5-1 and 7-1 ARM loans.

3. Data and Measurement

3.1. Sources

County and Zip-code Panel of Demographics. – We access complete county and zip-code 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.

**County-by-industry Panel of Employment and Earnings.**—Our main measure of employment and earnings comes from the Longitudinal Employer-Household Dynamics (LEHD), specifically the Quarterly Workforce Indicators (QWI), which is publicly accessible at an aggregated level from the Census Bureau website (http://lehd.ces.census.gov/data/). The LEHD covers over 95% of jobs in the U.S. and consists of a unique federal-state data sharing collaboration called the Local Employment Dynamics (LED) partnership. It is a partnership whereby all state agencies voluntarily submit quarterly data files from existing administrative records. These administrative records, for example, 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.

**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 (i.e. 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.

We focus on foreclosures between 2000 and 2014. For each loan, we observe a vector of initial characteristics, giving information such as the contract type (Hybrid ARM, Balloon, etc.), the initial interest rate, and the schedule for interest rate resets and balloon payments. We also observe monthly performance updates, giving information on factors such 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).

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23See Abowd et al. (2009) for details.
To provide a glimpse of the variation we observe in the data, Figure 5 begins by plotting the standard deviation of logged foreclosures across states and quarters for three separate time periods (2000-2004, 2007-2009, and 2010-2012). There is a remarkable amount of heterogeneity. In particular, since we are plotting the distribution of standard deviations, rather than just the level of foreclosures, the fact that the 2007-2009 period (orange line) in Figure 5 is centered substantially to the right of 2000-2004 (green line) and even 2010-2012 (blue line) highlights the fact that the surge in foreclosures as not just concentrated in one location during the Great Recession. The fact that each of the different time periods are non-overlapping in many locations illustrates that there is significant time-varying heterogeneity in the intensity of foreclosures.

[Insert Figure 5]

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) have pointed out already in the context of unemployment, foreclosure is a long drawn-out process that may take months, or even years in some cases, before actual loss of the home takes place, which allows those individuals an implicit credit line. We also observe a larger universe of approximately 170 million mortgages (ARM and fixed rate), which allow us to see the total monthly payments due on all loans in each county-quarter (i.e., over 5 billion total loan-month observations). Since Di Maggio et al. (forthcoming) 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.

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). 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).

**Small Business Administration Lending Data.**—We access a database containing 1.4 million loans made by banks to small businesses through the US Small Business Administration’s (SBA) 7(a) and 504 loan programs. The former allows banks to make loans to small businesses and to purchase partial (up to 85%) default insurance on those loans from the SBA.\(^\text{25}\) 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.\(^\text{26}\) See Appendix Section A1. for further details.

**County Panel of Business Tax Returns Data.**—We license detailed data from the IRS tax returns of all 27 million public and private businesses in the United States from the data services company Powerlytics. This data contains total statistics available at, for instance, the county-year-industry level for all line items on the 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. 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.

**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

\(^{25}\)https://www.sba.gov/category/lender-navigation/sba-loan-programs/7a-loan-programs

\(^{26}\)https://www.sba.gov/offices/headquarters/oca/resources/5991
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 three 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?”, (ii) “Right now, do you think that economic conditions in this country, as a whole, are getting better or getting worse?”, and (iii) “Now thinking more generally about the company or business you work for, including all of its employees. Based on what you know or have seen, would you say that, in general, your company or employer is (a) hiring new people and expanding the size of its workforce, (b) not changing the size of its workforce, or (c) letting people go and reducing the size of its workforce.” In companion work, we show that these first two measures do a remarkably good job of tracking the volatility index (VIX) and the Baker et al. (2016) index of economic policy uncertainty. Our results are also robust to using the standard deviation of individuals’ self-reported measures on these questions within the local area, rather than the individual’s self-reported level.27

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

27Our 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.
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 7 begins by plotting the dispersion in 5-1, 7-1, and 10-1 ARM originations across time and geography throughout the United States. We compute the mean time to reset for loans originated in each county and year between 2002 and 2007. A county with mostly 5-1 ARM loans originated in a given year will thus have a mean close to 5 and will appear more blue in the figure; a county with mostly 7-1 ARM loans originated will have a mean closer to seven and will appear more orange; a county with more 10-1 ARMs will appear dark red.

These plots provide an illustration of the relative composition of loan types in each geography both within and across time. The amount of dispersion is striking. Throughout the map, counties are checker-boarded red and blue in a seemingly random pattern. This dispersion is evident 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 7]

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 8 plots the relative interest rate changes starting from 2006:Q1 until 2009:Q1, which precipitated the apex of the surge in foreclosures. We construct the plot in the following way. We first sum across all the interest rate changes on individual loans in a given county. We subsequently identify, for each county, the quarter that had the highest net interest rate delta. We finally assign each county-quarter observation a rank of quarters across the 16 quarters (2006:Q1 to 2009:Q1) to focus the attention on the within-county intensity of interest rate changes. The highest quarter is assigned a value of 16 and

28 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.
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 8 demonstrates that there is still considerable heterogeneity in 2006 and 2009, especially in the mid-West. Appendix Section A3.3. documents the evolution of interest rates for loans originated between 2000 and 2006.

[Insert Figure 8]

Motivated by these geographic differences, Figure 9 illustrates that there is also considerable heterogeneity in the timing of when counties experience foreclosures. Just as in Figure 8, 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.

[Insert Figure 9]

3.3. Interest Rate Resets and Default Probabilities

We now turn towards characterizing the determinants of foreclosures. Edmiston and Zalneraitis (2007) discuss three main factors behind the recent surge in foreclosures: housing price declines, greater proportions of sub-prime loans, and interest rates that discontinuously changed at specified points in time. 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. His identification strategy exploited the fact that core based statistical areas (CBSAs) that had more cyclical housing prices in 1980 also had more cyclical housing prices precipitating the financial crisis. Motivated
by the fact that housing price declines do not explain all of the variation in default rates—that is, the role of borrower and loan characteristics—we leverage the fact that the interest rate on loans affects the homeowner’s default probability, specifically among the subset of ARMs. We focus on the discontinuous changes in interest rates as a source of plausibly exogenous variation.

Figure 10 non-parametrically plots the foreclosure probability by month since loan origination separately for different vintages of loans and ARMs. However, since in some cases, the interest rate can decline, we also distinguish between instances where interest rates were reset up versus down given that increases versus decreases will have have opposite effects on the foreclosure probability.\(^{29}\) 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 A3.3.): 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.\(^{30}\) Gupta (2016), Di Maggio et al. (forthcoming), and Agarwal et al. (2017) also provide extensive evidence documenting the impact that interest rate reductions have on the incidence of foreclosures.

\[\text{[Insert Figure 10]}\]

Is it valid for us to examine the foreclosure probability directly following an interest rate change since the foreclosure process often takes time after a borrower initially defaults on a mortgage? For example, foreclosure delay is common and behaves as a source for additional credit (Herkenhoff and Ohanian, 2015; Gerardi et al., 2015). First, some loans may enter default or foreclosure prior to the reset date. In these cases, a discontinuous spike in the interest rate can accelerate the foreclosure process, making it even more difficult, if not impossible, to pay. Conversely, when

\(^{29}\)In these situations, the default probability decreases dramatically on the reset rate. See Fuster and Willen (forthcoming) for a discussion of this mechanism. However, we are able to measure the direction and magnitude of the interest rate change because our data is at the loan/individual level. As described below, our methodology fully considers both the direction and magnitude of interest rate changes experienced by each individual loan.

\(^{30}\)In formal regression specifications that match these plots, we obtain t stats of 10 or greater on the interest rate reset variables.
the interest rate spikes downwards, a homeowner will have an easier time meeting the payment on the loan contract and leaving default or foreclosure.\textsuperscript{31} Second, foreclosure takes place more quickly in some states over others (e.g., those without judicial foreclosure laws), meaning that the relationship between interest rate spikes and foreclosure will be even starker in them. Finally, as we will discuss later, our loan-level specifications allow for a continued effect of the changed interest rate after the initial reset date; that date is simply the starting point of the effect.

4. Research Design

4.1. Empirical Specification

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

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

(1)

where \( y_{jct} \) denotes the industry-by-county outcome variable (e.g., logged employment, employment growth, or job turnover) in industry \( j \), county \( c \), and period \( t \). \( f(X, \beta) \) 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. 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.

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

\textsuperscript{31}See Appendix Section A3.3. for the path of interest rates for loans originated in different years.
to control flexibly for areas with heterogeneous different levels and rates of housing price growth.\footnote{Our main quantitative estimates only decline slightly when we interact the 10 bins with logged housing prices, 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. In particular, absent housing price controls, banks have a greater incentive to strategically delay foreclosure on homeowners with underwater mortgages to avoid valuing those assets at their true market value.}

\section*{4.2. Identification Strategy}

The most obvious form of endogeneity in Equation 1 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.\footnote{https://www.bloomberg.com/view/articles/2014-02-26/banks-prefer-losses-they-don-t-have-to-talk-about} When a bank forecloses on
a mortgage, it must take physical possession of the underlying property and value that asset at its market value, rather than keeping it simply on its books under the “loans outstanding” category in the hope that it will become current again. 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. In fact, part of the Home Affordable Modification Program (HAMP) aid was specifically designed to help banks avoid recognizing their losses immediately. 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.

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. It was only after the peak of the economic crises that these servicers and local governments expanded their administrative capacity to process more foreclosures and worked their way through the initial glut of foreclosures in their systems. Administrative backlog effects of these sorts, combined with strategically delayed foreclosures, will cause significant numbers of foreclosures to be delayed until local economic conditions begin improving, producing upwards biased estimates by underestimating the negative impact due to foreclosures.

36That 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/.  
38http://www.creditslips.org/creditslips/2012/11/where-are-the-foreclosures.html
4.2.1. **Strategy # 1: A Loan-level Model**

Our primary solution is to exploit a unique feature of the design of adjustable rate mortgage (ARM) loans and how they affect foreclosure probabilities. Many hybrid ARMs were initially offered to individuals with “teaser” rates for an initial period. The rates on these loans, however, would frequently spike after the first reset date such that they were in excess of the prevailing interest rate (e.g., LIBOR) by as much as 8% or more.\textsuperscript{39} We restrict our sample to individuals with 5-1, 7-1, and 10-1 ARMs. In our Appendix Section A2.2., we plot the distribution of FICO scores for individuals with each of these loans. The distributions are nearly identical, which is consistent with our earlier evidence that, due to historical reasons, banks simply accelerated lending using more of one type of loan over another for exogenous reasons—that is, they were not targeting low income earners with 5-1 vs. 7-1 loans, for example. We account for these interest rate effects by estimating loan-by-month logit regressions

\[
P(\text{foreclosure}_{it}) = \Lambda \left( \alpha + \sum_k \gamma^k \text{Loan}^k_{it} + \zeta \text{Reset}_{it} + \sum_k \rho^k (\text{Loan}^k_{it} \times \text{Reset}_{it}) \right) \quad (2)
\]

where \(i\) indexes the loan and \(t\) the month-year, \(\text{Loan}^k\) denotes an indicator for the \(k\)-th type of ARM loan, \(\text{Reset}\) denotes the difference between the loan’s initial interest rate at origination and the interest rate in time \(t\), and \(\text{Loan}^k \times \text{Reset}\) captures the differential effect on foreclosure probability that the reset has for different types of ARMs.

There are two important remarks we want to pause on with respect to Equation 2. First, motivated by the role of foreclosure delay as an implicit credit channel (Herkenhoff and Ohanian, 2015), we estimate Equation 2 separately for each state to allow for heterogeneity in the link between interest rate resets and foreclosures. For example, as we will discuss later, states with and without judicial foreclosure laws have different frequencies of foreclosures and their potential impact on the labor market may vary. Second, recognizing that the interest rate resets may have a dynamic effect on foreclosure probabilities, that is, a reset may have different effects depending on whether it is occurring in the current month or has occurred some time in the past, we have experimented with a variant of Equation 2 where we replace \(\text{Reset}\) with a vector of predictors, each measuring the amount of the interest rate on the loan changed due to rate resets at a different

\textsuperscript{39}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 foreclose on the property.
point in time for up to 36 months following the initial reset. 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 \( Z_{jt} = P(\hat{f}_{jt}) \). We use these predictions to instrument actual foreclosures through 2SLS

\[
\begin{align*}
  f_{jt} &= f(X_{ct}, \beta) + g(h_{ct}, \theta) + \pi Z_{jt} + \eta_j + \psi_c + \lambda_t + \epsilon_{ijt} \\
y_{ijt} &= f(X_{ct}, \beta) + g(h_{ct}, \theta) + \gamma \hat{f}_{jt} + \eta_j + \psi_c + \lambda_t + \epsilon_{ijt}
\end{align*}
\]

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 10). 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, and local bank deposits, which removes the potentially confounding role of credit shocks on employment (Chodorow-Reich, 2014; Mondragon, 2015). We also show robustness controlling 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 and Di Maggio et al. (forthcoming) who examine the impact of interest rate changes on consumption and voluntary deleveraging. Our paper (developed concurrently with these) also contains several novel features.

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40 While the results of this alternative specification are available upon request, they are almost identical to those we present in the body of this paper.

41 See Di Maggio et al. (forthcoming) 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.

42 A potential concern remains that shocks to housing prices may affect the incentive to default. If this were true, we may also need to instrument for housing prices to overcome their simultaneous endogeneity. While we are already controlling for housing prices, Gerardi et al. (2015) use the Panel Study of Income Dynamics (PSID) to show that there is only a limited scope for strategic default.
First, we use the entire universe of CoreLogic data, containing 170 million loans, of which 3.2 million ARM loans are resetting.\footnote{In 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. (forthcoming) both use the Blackbox sample which containing 22 million loans (Di Maggio et al. (forthcoming) 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.} Using only a subset of loans (even if randomly chosen)—especially if the subset focuses more heavily on sub-prime borrowers in predicting default—can create significant bias (Ferreira and Gyourko, 2015; Albanesi et al., 2017). Second, we estimate a loan-level model that extracts only the variation in foreclosure predicted from ARM resets, whereas past papers have focused on interest rate changes as the primary independent 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 not publicly available. 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 $m_{i,k,t_0}^P$, 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 $m_{i,c,t_0}^C$, i.e., the percentage of all
of a bank’s total loans that were originated in a given county.\textsuperscript{44, 45} 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 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.\textsuperscript{46}

Let $O_{ickt}$ denote the “predicted” number of originations from bank $i$ in county $c$ for loan type-k in period $t$ and $O_{kt}$ 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

$$O_{ickt} = \sum_i (O_{kt} \times m_{i,kt}^P \times m_{i,c,t}^C)$$

In the second stage, we compute the implied number of resets that occurred due to the origination $O_{ickt}$. 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

\begin{footnotesize}
\textsuperscript{44}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.

\textsuperscript{45}By a bank’s county market share, we mean the percent of a bank’s total loan portfolio that falls within a designated county.

\textsuperscript{46}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.
\end{footnotesize}
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 $20,000 \times 0.03 = 600$ 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 A4.6., 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 fact that banks were geographically concentrated prior to the recession and simply expanded their operations in those initial areas.

The 1980s and 1990s experienced a significant amount of banking deregulation, culminating in the 1994 Interstate Banking and Branching Efficiency Act (Kroszner and Strahan, 2014). Through a series of legislations, 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 quality 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 information on a partial $F$-statistic. To make the plots easy to view and interpret in Figure 11, 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.

[Insert Figure 11]

5. Quantitative Estimates

5.1. Main Results

We turn towards estimating our baseline equation when our outcome variable is logged employment. Panel A in Table 1 documents our results. The unconditional correlation in column 1 is clearly biased as it shows that a 1% increase in foreclosures is associated with a roughly 0.71% rise in employment. The bias arises from the fact that counties with more foreclosures have more people, which tend to be the more productive cities. However, demographic controls help in reducing the bias. For example, the coefficient declines to -0.046 once our semi-parametric controls are added. Our coefficient declines further to -0.010 once we control for time-invariant sources

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48Kroszner and Strahan (1999) discuss the political economy forces that led to these deregulations.
49In the Appendix Section A3.2., 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.
of heterogeneity across counties, industries, and time. While the fixed effects help reduce the bias arising from spatial heterogeneity—the fact that highly productive locations are less likely to experience as many foreclosures—they do not resolve the two sources of time-varying endogeneity.

We now turn towards our instrumental variables results in column 4, which overcomes these problems by comparing observationally equivalent counties before and after idiosyncratic shocks to the reset date of home owners on adjustable rate mortgages. Under our baseline, a 10% rise in foreclosures is associated with a 1.16% decline in employment. As long as the timing of resets, which are contractually fixed years in advance, is uncorrelated with these potentially compounding factors, we have a window of variation that allows us to make valid comparisons between a county that has yet to experience an interest rate reset with a county that just received one.

Since we are controlling for total mortgage payments due and bank deposits, we are removing the potentially mechanical correlation between interest rate shocks and disposable income (Di Maggio et al., forthcoming), which could affect employment by altering individuals’ search behavior. However, column 5 implements an even more restrictive test by controlling for a quadratic in gross county wage income. In this sense, we are examining how the marginal effect of a foreclosure shock varies across counties with similar income shocks, purging the potential effect that variation in income has on searching and matching in the labor market. While our coefficient not surprisingly declines in magnitude (lower employment naturally leads to lower net income, so we are effectively conditioning on part of our response by including income), it still remains negative significant at the 10% level. We have also found that our results are robust to including state-level leave one out estimates of local employment and earnings shocks—that is, recognizing that what is happening in neighboring counties might lead to some spillovers. Even if these spillovers did not decay quickly in distance (which current evidence suggests they are not, i.e. Gupta (2016)), a violation to our exclusion restriction would require these spillovers to be highly correlated with the timing of interest rate resets between two neighboring counties, which seems unlikely given our earlier evidence on the geographic concentration of banks.

We finally turn towards sources of heterogeneity—that is, foreclosure shocks separately for firms in the non-tradables versus tradables sectors and non-judicial foreclosure and judicial foreclosure states. Beginning with the former dimension of heterogeneity, we see that a 10% rise of foreclosures is associated with a 0.6% decline in employment in the non-tradables sector (significant at the 1% level), but a 1.52% decline in the tradables sector (significant at the 10% level). The fact that we find a stronger gradient for the tradables sector points towards a different mechanism than
the consumer demand channel in Mian and Sufi (2014). In particular, they argued that housing price declines are associated with declines in local employment only in the non-tradable sector since these are the jobs that are most dependent on local disposable income. Since we were able to replicate their results earlier, our stronger gradient for tradables suggests that foreclosures may affect the labor market through a wholly different channel.

We now turn towards our second dimension of heterogeneity—state judicial foreclosure laws. We find that a 10% increase in foreclosures is associated with a 1.71% decline in employment among states without judicial foreclosure laws, but a 1.25% decline for states with judicial foreclosure laws. States with these laws have a slower foreclosure process and fewer foreclosures (Mian et al., 2015). 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.\(^{50}\) One reason for the 37% difference (\(=\frac{1.71}{1.25}\)) in these estimates arises from the potential non-linear effects of foreclosures on labor market outcomes. While the heterogeneity may reflect other institutional features of these states, Appendix Section A4.5. provides evidence of strong non-linear foreclosure effects in states without judicial status laws, which could explain our larger gradient when we restrict the sample to these states.

Before turning towards our results when the outcome variable is employment growth or the turnover rate, we remark briefly on the coefficient on logged housing prices. While it is the case that housing price increases are associated with increases in employment over the 2006-2009 period (e.g., see our earlier replication of Mian and Sufi (2014)), we found that they are negatively associated with employment in other years. One explanation is that higher housing prices raise local rents, making it more expensive to do business (higher costs for facility purchase/lease, higher wages needed for employees to compensate for higher rents, etc.). That is especially true for firms in the tradables sector where they do not benefit from higher local housing prices. In contrast, a firm in the non-tradables sector may benefit since house price growth increases wealth and thus potentially spending power of residents and because higher housing prices indicate that the geography is welcoming more affluent earners.

We now explore the associations between foreclosure shocks and other measures of labor market outcomes, namely employment growth and turnover rates. We begin by remarking that there is more similarity between our fixed effects and instrumental variables results, likely because

\(^{50}\)Loans in states with judicial foreclosure laws also tend to be 3-7% smaller, which could also contribute to these differences (Pence, 2006).
the outcome variable is a rate, which differences out any confounding variation in the level of employment. Our preferred specification in column 4 suggests that a 10% rise in foreclosures is associated with a 0.11 percentage point decline in the growth rate of employment. We again see a stronger effect of foreclosures on firms in the tradables sector than the non-tradables sector, but our estimated gradient for non-judicial states fades. Through a series of diagnostics, we discovered that states with judicial foreclosure laws tend to have lower employment growth than those without judicial foreclosure laws, suggesting that one possibility for the attenuated correlation comes from the fact that firms in these states are more resilient.

We finally turn towards our results with the turnover rate as the outcome variable. Under our preferred specification, we find that a 10% rise in foreclosures is associated with a 0.05 percentage point decline in the turnover rate. We find some suggestive evidence that the gradient is stronger in the tradables sector, relative to non-tradables, but we cannot statistically reject the null that they are equal. We again do not find a stronger gradient for non-judicial states, which is likely driven by the fact that their turnover rates are slightly higher and thus contain potentially more dynamic labor markets. Nonetheless, the fact that foreclosures depress turnover rates and employment growth is consistent with microeconomic evidence that foreclosure delay is a mechanism individuals use to extend their search time in the labor market (Herkenhoff and Ohanian, 2015).

[Insert Table 1]

Before continuing, we make two remarks. First, we have also estimated similar models where we altered our outcome variables to logged earnings and the Gini coefficient motivated by recent work by Bernstein (2016) and Rugh and Massey (2010), respectively. For the former models with logged earnings, we find that a 10% rise in foreclosures is associated with a 0.46% decline in earnings. However, once employment is included as a control, the estimate declines to 0.35% (p-value is 0.336). The fact that we cannot reject the null of no effect is consistent consistent with Bernstein (2016) that foreclosures affect household income, but through the extensive margin.51 For the latter models with the Gini coefficient, we find that there is no association between foreclosures and inequality after controlling for basic demographic characteristics, which is robust to using our fixed effects and IV estimators. Appendix Section A4.1. provides a complete discussion of these results.52 Second, we have also examined several dimensions of heterogeneity where we estimate

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51In 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.

52While we have focused on understanding the sources behind the adverse labor market outcomes, there are a
our gradients separately by bins on: the employment share in construction, median county income in 2000, and establishment size. We also examine the potential for an asymmetric gradient based on whether the economy is in a boom or bust, but do not find any evidence. Appendix Section A4.3. provides a complete discussion of these results.

5.2. Supplementary Instrumental Variables Strategy

We now turn towards our results that exploit our alternative Bartik-like instrumental variables strategy. Here, identification is premised on the fact that different counties will have different pre-recession exposure levels to bank lending strategies. 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 2 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. We will also provide additional robustness exercises subsequently.

[Insert Table 2]

range of related issues that also arise. In particular, do foreclosures have other non-pecuniary labor market effects? For example, if foreclosures rise in an area, are there indirect effects that feed back into household behavior? One possibility is that deteriorating labor market conditions cause parents to work longer hours and/or make more sacrifices that trades off with investments in their children’s human capital. Appendix Section A4.2. also uses the American Time Use Survey (ATUS) to explore the impact of foreclosures on the allocation of time. We find no association between foreclosures and parental time allocated to children, but we do find that increases in foreclosures are associated with meager declines in time allocated towards leisure.
5.3. Aggregate Impact

How much of the decline in employment growth, job turnover, and rise in inequality can be attributed to the surge in foreclosures during the Great Recession? We compute these partial equilibrium approximations separately for judicial and non-judicial states, as well as tradables and non-tradables industries, since the elasticities vary significantly between the different groups. 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.

We begin by stating the aggregate facts based on these start/end dates. Using our quarterly county-level foreclosures, we find that foreclosures per open mortgage grew by 302% in states without judicial laws constraining the foreclosure process, whereas they grew by 173% in states with these laws. Using the Mortgage Bankers Association (MBA) foreclosures time series, we find that they grew by 202% on average, which we will use when computing the aggregate effects for the tradables and non-tradables sectors. Turning towards employment, its growth rate, and turnover, we document similar growth rates in Table 3. We specifically note that employment growth declined remarkably over the time period we are looking at, especially in tradables sectors.

Table 3 summarizes the aggregate effects separately by group. We detail the aggregate effects for the non-judicial states as an example to illustrate the mechanics of the back-of-the-envelope approximation. We begin by taking our estimated elasticity for the group under the baseline model, which is \( \gamma = -0.116 \) and multiply it by the ratio of the growth rates between foreclosures and employment, which is 302/4.16. Putting these facts together, we find that the surge in foreclosures explains 11.3% of the overall decline in employment in non-judicial foreclosure law states. While these results provide a back-of-the-envelope approximation, they are likely to underestimate the aggregate effect since only the marginal effects are identified. Given that foreclosures tripled in states without judicial foreclosure laws, the assumption of only taking “marginal” changes around the neighborhood of our preferred \( \hat{\gamma} \) will be violated.\(^{53}\)

\(^{53}\)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 A2.2. and A3.2., 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
5.4. Robustness and Extensions

5.4.1. 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 our estimated coefficient on foreclosures could potentially be contaminated from other omitted variables.

Using administrative records from the IRS, we estimate regressions of the share of type-$k$ hybrid ARMs (e.g., 7-1 ARM) on a semi-parametric measure of the logged number of filers by seven different income bracket bins, conditional on controls. We restrict our sample to the pre-2008 period. 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 any 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 4 documents these results. Consider, first, columns 1 and 2 where we regress the share of 2-1 ARMs on semi-parametric income measures. While the OLS results point towards a plausible endogeneity problem—that is, increases in the number of filers are statistically associated with shares of ARMs—the FE results point towards no significant correlations outside of filers earning under $10,000 per year, which is not a threat to our baseline results since we are looking at full-time workers (e.g., earners above $10,000 per year). Moreover, we would only overestimate the adverse effect of foreclosures if an increase in low income brackets is strongly positively associated with these types of ARMs.

Turning towards the share of 3-1 ARMs as the outcome variable, we see that there is less of a statistical relationship for the FE results, relative to the OLS results. Although increases in the income earners, as well as with recent work from Antoniades (2015); 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).
number of filers with incomes between $50,000-75,000 are associated with increases in the share of 3-1 ARMs, we also see that increases in the number of filers between $25,000-50,000 and between $100,000-200,000 are associated with decreases. In this sense, there is no discernible pattern. To the extent that targeting exists, conditional on controls, it is not taking place solely based on income and, therefore, violations to our exclusion restriction would have to take place through a channel other than income. There is also no discernible association between increases in the number of filers (in any income bracket) and the share of 5-1 ARMs. There are some statistically significant associations for 7-1 and 10-1 ARMs, but they comprise a relatively small share of hybrid loans (7.5% and 4.6%, respectively), whereas 2-1, 3-1, and 5-1 comprise much larger shares of 39%, 19%, and 29%, respectively. In this sense, even if there were a pattern in the displayed correlations, they are not our primary source of identifying variation in our instrument.

[Insert Table 4]

Our second diagnostic exercises gauges the role of unobservables. One concern, for example, is that credit market frictions for employers (e.g., Christiano et al. 2015)) are somehow correlated with both employment and foreclosures in unobserved ways. Given that we have large $R$-squares in our results, the margin for unobserved heterogeneity is relatively low. Nonetheless, we pursue a strategy initially introduced by Altonji et al. (2005), and recently operationalized by Oster (forthcoming), to gauge the potential for remaining unobserved heterogeneity.

Table 5 presents an application of partial identification under our fixed effects and instrumental variables estimators. We restrict the degree of selection on unobservables to be no more than 20% the magnitude of selection on observables since our $R$-square is generally around 0.89, which means there is only about 10% margin for unobservables to exert an effect on the outcome variable. For both employment and turnover, our results suggest that our estimates, if anything, are very conservative—allowing for complete selection on unobservables would bias us towards finding starker negative association between foreclosures and employment.

[Insert Table 5]

5.4.2. Examining the Possible Endogeneity of Credit shocks

A wide array of papers have emerged examining the role of credit shocks in amplifying financial frictions (Mian and Sufi, 2009; Jermann and Quadrini, 2012) and the decline in employment
(Chodorow-Reich, 2014; Mondragon, 2015; Chen et al., 2017) during the Great Recession.\textsuperscript{54} Although our baseline estimates control for mortgage payments and local bank deposits, and our instrumental variables strategy exploits quasi-random variation in the timing of interest rate resets on ARM loans, there is still a potential concern that we are not controlling sufficiently for credit shocks, which may be correlated with foreclosures. We now examine this potential concern explicitly by gathering data on lending among local banks that operate only in a given county\textsuperscript{55} and their capital structure (assets net of liabilities scaled by assets).

Appendix Section A4.4. documents these results using both the fixed effects and instrumental variables approaches. We find that there is no statistically significant association between measures of local credit (e.g., lending and capital) and foreclosures, consistent with our identifying assumption that our main results are not confounded by contemporaneous credit shocks. Although the lack of a significant correlation may appear peculiar, we are clearly not claiming that foreclosures are disconnected from credit in the real economy. Rather, the evidence is consistent with the claim that foreclosures affect credit indirectly through other channels, such as local housing prices and employment; we provide evidence of this also in Appendix Section A4.4.

### 5.4.3. Foreclosure Intensity and Non-linearities

The new macroeconomics of finance models emphasize the importance of non-linearities in times of crises; see, for example, 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 logged foreclosures, and (ii) replacing logged contemporaneous foreclosures with cumulative logged foreclosures to date. Appendix Section A4.5. documents these results, suggesting that the inclusion of either non-linearities or foreclosure intensity raises the quantitative magnitude of our estimates.

\textsuperscript{54}See Brunnermeier and Oehmke (2013) for a comprehensive survey of macroeconomic models in this area.

\textsuperscript{55}We identify banks operating in a single county based on the FDIC’s Statement of Deposits Data. We then use Call Reports to obtain bank-wide information on these institutions, which fully describes the activities of these banks in the particular county that they solely operate within. County-level data is not available for banks operating in multiple counties.
5.4.4. Examining the Potential for Reverse Causality

While labor market shocks undoubtedly affect the type and frequency of loan originations, whether they constitute a violation of our exclusion restriction depends on whether these contemporaneous labor market shocks are correlated with the interest rate changes that are induced as a result of loan originations that took place five, seven, or ten years ago as per the contractual terms of the ARMs.\textsuperscript{56} Based on earlier null correlation between income and housing shocks between 1990-2000 and the share of ARMs in 2003-2004 in Figure 4, the potential for these violations should be small.

We nonetheless examine the potential for labor market shocks to affect ARM shocks by implementing a placebo test. If it is the case that a rise in foreclosures merely reflects a rise in unobserved local productivity, we should see a decline in the share of renters in response to economic activity due to the strong correlation between income and home ownership.\textsuperscript{57} Using county-level data from the ACS between 2006 and 2014, we regress logged rental payments on logged employment, controlling semi-parametrically for a number of demographic covariates. While we find that a 1% rise in employment is associated with a 0.15% decline in rental payments ($p$-value = 0.00) in the cross-section, the coefficient declines to 0.01 ($p$-value = 0.209) once we add county and year fixed effects. In this sense, even if labor market shocks affect loan originations and eventual ARM interest rate resets, the magnitude of the channel within-county is small.\textsuperscript{58}

6. Disentangling the Mechanisms

We now explore two candidate mechanisms that help explain the observed negative association between foreclosures and labor market outcomes. While we do not claim that these two are exhaustive, they simply help rationalize our main results and put them in a broader context of

\textsuperscript{56}If such a correlation exists and is persistent, then our county fixed effects should absorb the variation. Even in these cases, since employment growth tends to be relatively mean reverting, an unobserved shock in period $t - s$ would begin to fade by period $t$, coinciding with the ARM interest rate spike and causing us to underestimate the true effect of foreclosures.

\textsuperscript{57}For example, Flavin and Yamashita (2011) show the important link between net wealth and home ownership. For more informal evidence, see https://www.zillow.com/research/homeownership-by-income-9419/.

\textsuperscript{58}As an additional piece of evidence, we residualize both employment shocks and loan originations using our standard controls. We residualize in two ways: with and without location and time fixed effects. When we regress residualized logged loan origination without fixed effects on logged employment, we recover a coefficient of 0.049 ($p$-value = 0.00). However, when we regress our residualized value with fixed effects on logged employment, we find a coefficient of 0.00086 ($p$-value = 0.00). In this sense, while positive economic shocks are intuitively associated with greater loan originations—arguably because marginal renters are taking on ARM loans to purchase a home—the bulk of the reverse causality appears to fade when we remove time-invariant location heterogeneity.
the major fluctuations observed during the recession. The presence of additional mechanisms does not invalidate our analysis, but rather supplements it.

6.1. Business Expansion and Entrepreneurship

We begin by examining the impact of foreclosures on business expansion and entrepreneurship. While one channel discussed in prior literature arises from the impact of housing prices on consumer credit (Mian and Sufi, 2009; Mian et al., 2013), which is amplified by the impact of foreclosures on housing prices (Campbell et al., 2011; Anenberg and Kung, 2014; Mian et al., 2015; Gupta, 2016), we focus on an alternative channel that is independent of housing prices.59 We instead propose an alternative channel based on an increase in local uncertainty in response to foreclosure shocks.

While there is already a literature on the link between uncertainty and investment for broader macroeconomic analysis (Hassler, 1996; Bloom, 2009; Bachmann et al., 2013), there is less literature about the role of local uncertainty shocks (Shoag and Veuger, 2016). Especially with the surge in foreclosures during the Great Recession, our hypothesis is that areas experiencing foreclosure shocks became more uncertain for investors and firms. The fact that the variance of foreclosure shocks also grew over this period also potentially raised investor and firm ambiguity about the time it would take for houses to become occupied again, which may also be linked with a further decline in investment (Ilut and Schneider, 2014). We begin by providing evidence that there is a causal effect of foreclosures on uncertainty.

Using our novel data from Gallup’s U.S. Daily between 2008 and 2014, we examine how measures of economic sentiment at a micro-level are affected by foreclosures.60 We focus on three individual-level measures (perceptions of current economic activity, perceptions of future economic activity, and hiring/firing at the employee’s firm), denoted $S_{ict}$, which we estimate through

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59 Particularly for small businesses, real estate is often an important source of collateral when securing credit (Schmalz et al., 2017). In this sense, foreclosures may affect lending to small businesses not only through foreclosure on an entrepreneur’s (or possible entrepreneur’s) home that they could use for collateral, but also through price discounts on neighboring homes. As we discuss above, we do not focus on this mechanism since it is already intuitive based on the current literature and is inconsistent with our focus on foreclosures affecting real outcomes independent of housing prices.

60 As we discussed earlier, we measure uncertainty based on individuals’ self-reported replies to three questions about their perception of the current state of the economy (1-4 index), future state of the economy (1-3 index), and hiring at their current company (1-3 index). We also validate these measures in our companion work by plotting the daily measures with the volatility index and the Baker et al. (2016) index of economic policy uncertainty (Makridis and Ohlrogge, 2017).
\[ S_{ict} = \alpha D_{ct} + \beta X_{it} + \gamma f_{ct} + g(h_{ct}; \theta) + \phi_c + \lambda_t + \epsilon_{ict} \]  

(4)

where \( S \) denotes the logarithm of these sentiment indices, \( D \) denotes the usual county controls (e.g., logged mortgage payments), \( X \) denotes a vector of individual controls (race, education, marital status, gender, and in some cases income), \( f \) denotes logged county foreclosures, \( g(h; \theta) \) denotes the usual semi-parametric measure of housing prices, and \( \phi \) and \( \lambda \) are fixed effects on county and year/quarter.\(^{61}\) Since our data does not have full coverage of every county in the United States, we also use foreclosures predicted by 2-1 and 3-1 ARMs as a source for additional identifying variation in our estimation. In addition to controlling for mortgage payments, we also add county adjustable gross income and individual monthly income to further mitigate concerns that our negative associations are driven by income effects or the mechanical association between higher interest rates and lower disposable income (Di Maggio et al., forthcoming).\(^{62}\)

Table 8 documents these results. We see that a 10% rise in foreclosures is associated with a 1.2% decline in perceptions of current economic activity, a 1.4% decline in perceptions of future economic activity, and a 0.4% decline in employer hiring intensity. There are a couple of notable points to observe. The first observation is the remarkable consistency across each of the estimated coefficients. Regardless of whether we include county adjustable gross income or eight different income brackets for an individual, the coefficients are barely affected. The second observation is that foreclosure shocks affect perceptions of future activity just as much as current economic activity. While the latter is not surprising—potentially due to the impact of foreclosures directly on housing prices—the former is surprising since it is consistent with a model where foreclosures induce heightened uncertainty about future growth.

[Insert Table 8]

While it would be reasonable to suspect that the rise in uncertainty following foreclosure shocks would led to a decline in bank lending (Alessandri and Bottero, 2017) and investment (Bernanke, 1983; Bloom, 2009), we now provide explicit evidence that the decline in lending and investment is driven specifically by foreclosures, rather than other contemporaneous correlates. First, we compile the universe of loans made by banks through the Section 7(a) and 504 lending programs of the US

\(^{61}\)In our companion work (Makridis and Ohlrogge, 2017), we show these measures are reliable proxies for uncertainty by correlating them with the volatility index and Baker et al. (2016) index of economic policy uncertainty.

\(^{62}\)We have also experimented with controlling for non-durables consumption expenditures on top of income, which does not alter our quantitative estimates; see our companion work for further discussion (Makridis and Ohlrogge, 2017).
Small Business Administration (SBA). This data was acquired through a Freedom of Information Act (FOIA) request and encompasses detailed records on approximately 1.4 million individual loans. Each loan record includes information on the NAICS industry of the loan recipient, the amount of the loan, and the portion of the loan guaranteed by the SBA. We focus on the fraction of unguaranteed loans—that is, the amount of risk that banks are willing to take on—compared to the guaranteed portion of loans which the government backs. Both Section 7(a) and 504 programs aim to make credit available to small businesses that would find it difficult or impossible to obtain through traditional lending channels.

Using these data, we now consider the logged ratio of unguaranteed to total loan amounts for each county-quarter-NAICS pair. This ratio represents how willing local banks are to risk their own money investing in an area, as compared to funds guaranteed by the government. Table 7 documents these results. We see that a 10% rise in foreclosures is associated with a 0.7% and 1.6% decline in bank lending through the 504 and 7(a) loan programs. We also find it reassuring that a 10% rise in housing prices and total mortgage loan payments due are associated with a 4.3% decline and 1.5% rise in unguaranteed (bank-backed) loans, respectively. Since entrepreneurs tend to use housing as collateral to finance credit lines (Schmalz et al., 2017), the negative coefficient on housing prices is consistent with self-financing through housing collateral versus small business backed loans being substitutes. Similarly, positive shocks to mortgage payments make banks more willing to lend since they are earning more in interest from the mortgage loans. These results are consistent with models where foreclosures induce greater uncertainty in risk profiles even after controlling for other time-varying covariates, such as housing prices and overall mortgage loans that come due.63

[Insert Table 7]

We next investigate a measure of business investment using administrative Internal Revenue Service (IRS) data on advertising and rental expenditures at a local industry-by-county level

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63We also examine the degree of heterogeneity by sector. We document these results in the Appendix Section A5.2.. 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.
between 2004 and 2014. Following the style of our earlier models, we now regress logged advertising and rental expenditures on logged foreclosures, controlling for the usual demographics and using the baseline instrumental variable for foreclosures.

We find tremendous variation in the association between foreclosures and investment by major industry (see Figure 13). Whereas some industries have a null association (e.g., agriculture / forestry, finance, insurance, and real estate, education and health services), others have a very negative association (e.g., mining, utilities, construction, manufacturing, wholesale / retail trade, and information). In particular, for these latter industries, a 10% rise in foreclosures is associated with approximately a 1% decline in investment. Others, specifically transportation and administrative support / waste management, experience a large rise. One rationale for the phenomenon with the latter two industries is that the Great Recession led to an abrupt decline in state / local government revenues, which led to an expansion in services provided by somewhat substitutable sectors to fill the gap (e.g., waste management contractors, transportation and warehousing).

6.2. Mobility and the Composition of Skill

We now turn towards examining the impact of foreclosures on mobility, focusing in particular on the composition of skilled workers within a local labor market. We use educational attainment, specifically whether an individual has a college degree, as a proxy for skill and produce 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 1 with them and estimate these models using the same identification strategy.

64 Even with administrative data, information on investment is generally most available for manufacturing firms, which is only a small subset of the overall economy. However, both advertising and rental expenses are reported directly to the IRS as a line item on their business tax returns, which we exploit as a proxies for investment. For businesses whose operations span multiple counties, there is no reliable way to differentiate between which areas a firm’s advertising expenses were concentrated. Therefore, we consider only the universe of sole proprietorship, which are likely to have operations in just the single county in which they are incorporated.

65 The IRS defines rent as any amount paid for the use of property that the firm does not own, such as office space and machinery. Of particular interest are incidental repairs, maintenance, and costs of materials and supplies that are consumed during the year. For more detail, see https://www.irs.gov/pub/irs-pdf/p535.pdf.

Table 6 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 6]

In Appendix Section A5.1., we also examine an analogue of Table 6, but with earnings, rather than employment, as a further diagnostic to gauge the plausibility of our proposed mechanism. Theoretically, if the relative composition of college to non-college workers declines since the areas are becoming less attractive to live in, then we should also see a rise in the relative earnings premium for these workers as a compensating differential. 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 con-

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67Sorkin (2015) shows that approximately two-thirds of the variation in earnings is driven by compensating differentials.
trols, and estimated separately by income bracket. Figure 12 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.

But, what is it about foreclosures that generate the decline in skilled workers? We argue that the primary channel is a demand for local amenities, which we will show decline in response to foreclosure shocks. Building on earlier findings 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). As we discussed in our background section, there are at least two channels. The first is that foreclosures can depress local amenities, which skilled workers value; we explore how foreclosures affected various measures of well-being, including city satisfaction, in our companion work (Makridis and Ohlrogge, 2017). The second is that foreclosures can raise crime, which skilled workers want to avoid; see Appendix Section A5.1. for additional evidence where we find that a 10% rise in foreclosures is associated with a 2.52% rise in crime.69

68 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.

69 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.
7. Conclusion

An emerging body of literature has successfully quantified the real effects of housing shocks on house prices, and the subsequent impacts those have on consumption and employment. Nevertheless, we are not aware of any literature that has examined the impacts of foreclosures directly on labor market outcomes, such as employment and inequality. We develop a comprehensive database containing county-level variation in labor market outcomes, lending and investment, crime, foreclosures, and housing prices between 2000 and 2014.

We confront two major sources of endogeneity problems: (i) reverse causality—the fact that increases in unemployment make foreclosures more likely—and (ii) dynamic selection—the fact that banks have an incentive to wait until local conditions improve before foreclosing on delinquent home owners. We overcome these sources of bias by exploiting heterogeneity in the incidence of different adjustable rate (ARM) mortgage loans across counties. We specifically use the fact that two observationally equivalent counties have different shares of 5-1, 7-1, and 10-1 ARMs, which will induce differences in foreclosure probabilities at different dates, granting us windows of time where one county can serve as a control for the other. We show that these shares of ARMs are uncorrelated with historical labor and housing market shocks. We also provide evidence that banks expanded into areas based on plausibly exogenous historical factors.

Our results suggest that a 10% rise in foreclosures is associated with a 1.1% decline in employment, as well as a 0.05 and 0.08 percentage point decline in employment growth and labor market turnover. We find that firms in the tradables sector are hit harder than those in the non-tradables sector, and firms in states without judicial foreclosure laws are hit harder than those in states with the laws. These results point towards a fundamentally different mechanism than the demand-side forces discussed in the existing literature. We do not, however, find an effect of foreclosures on inequality. If anything, foreclosures reduced inequality by compressing the income distribution. These results are robust to a battery of exercises, including a supplemental instrumental variables strategy that exploits a county’s pre-recession exposure to different bank portfolios.

Our estimated foreclosure effects appear to be driven by a combination of two channels. First, a decline in local skill whereby more productive workers leave. The main mechanism here is that skilled workers are willing to pay for non-market amenities, so an increase in dis-amenities induces flight from those on the margin. Second, a decline in business investment whereby firms expand in areas that contain fewer foreclosures. The main mechanism here is that a rise in local economic
uncertainty deters business investment and lending, so there are fewer jobs available. These results point towards a new class of macroeconomic models in the vein of Corbae and Quintin (2015) and Mitman (2016)—models that incorporate search in the labor market and allow for the presence of foreclosure externalities.

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8.1. References

References


9. Tables and Figures

Foreclosures and the Macroeconomy, 1979-2016

**Figure 1:** 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.
Figure 2: Employment Rate by Firm Size, 1998-2015

Notes. - Sources: Longitudinal Employer-Household Dynamics (LEHD). The figure plots the quarterly employment growth rate for firms of different sizes.
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: 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.
employment growth = 0.0044 + 0.266 housing price growth

employment growth = −0.017 + 0.104 housing price growth

Figure 6: Housing Shocks and Employment Growth in Non-tradables and Tradables Sectors

Notes. - Sources: Longitudinal Employer-Household Dynamics, Federal Housing Administration. The figure plots employment growth and housing price growth (using an index normalized to 2000 prices) at the county-level for non-tradables and tradables sectors averaged between 2007 and 2010. Our classification scheme follows Mian and Sufi (2014) and we restrict the sample of counties to those with over 50,000 individuals. We also trim the data at the 5th and 95th percentiles.
Figure 7: Heterogeneity in ARM Loan Origination

Notes. – Sources: CoreLogic. The figure plots the dispersion in originations of different types of hybrid loans across counties for between 2002 and 2007. For each county-year combination, we compute the mean years to origination of all of the 5-1, 7-1, and 10-1 ARM loans originated in that county and year. Blue on the graph indicates a relatively high concentration of 5-1 ARMs, red a relatively high concentration of 7-1 or 10-1 ARMs. We gray out counties that do not have enough observations to produce reliable loan shares (e.g., no or very few hybrid loan origination were observed in that year/county).
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 9: 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 10: 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 11:** 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: the fraction of individuals in the county who are male, married, between ages \( k \in [k, k+1) \) where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education \( k \in [k, k+1) \) 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.

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Aggregate Effects

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

Notes. – Sources: Longitudinal Employer-Household Dynamics, CoreLogic. The table reports the mean employment growth, job turnover, inequality (90-10 logged wage difference) growth, and foreclosure growth for the respective years using county population as the weight. 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 Estimates Foreclosure Shocks on Industry Employment and Job Reallocation

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<td>.87</td>
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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, the change in logged employment, 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 controls. Demographic controls include: the fraction of individuals in the county who are male, married, between ages \( k \in [k, k'] \) 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. 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.
### Table 2: Exploiting Initial Bank Exposure to Identify Labor Market Effects

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</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, between ages \( k \in [k, \bar{k}] \) where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education \( k \in [k, \bar{k}] \) 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. 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.
Table 4: Examining the Correlation between ARMs and Income Fluctuations

<table>
<thead>
<tr>
<th></th>
<th>2-1</th>
<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>
</tr>
</thead>
<tbody>
<tr>
<td>ln(filers, under 10K)</td>
<td>-.145***</td>
<td>-.062**</td>
<td>-.040***</td>
<td>.007</td>
<td>.099***</td>
<td>.010</td>
<td>.054***</td>
<td>-.001</td>
<td>.032***</td>
<td>.037***</td>
</tr>
<tr>
<td>ln(filers, 10-25K)</td>
<td>.240***</td>
<td>.044</td>
<td>.102***</td>
<td>-.006</td>
<td>-.256***</td>
<td>.087</td>
<td>-.061***</td>
<td>-.043</td>
<td>-.027*</td>
<td>-.055*</td>
</tr>
<tr>
<td>ln(filers, 25-50K)</td>
<td>-.241***</td>
<td>.051</td>
<td>-.158***</td>
<td>-.134**</td>
<td>.265***</td>
<td>-.038</td>
<td>.102***</td>
<td>.077*</td>
<td>.039</td>
<td>.043</td>
</tr>
<tr>
<td>ln(filers, 50-75K)</td>
<td>-.073</td>
<td>-.092</td>
<td>.017</td>
<td>.185***</td>
<td>.083</td>
<td>-.084</td>
<td>-.033</td>
<td>.062*</td>
<td>-.004</td>
<td>-.091***</td>
</tr>
<tr>
<td>ln(filers, 75-100K)</td>
<td>.259***</td>
<td>-.026</td>
<td>.459***</td>
<td>-.006</td>
<td>-.478***</td>
<td>.013</td>
<td>-.097***</td>
<td>-.038</td>
<td>-.131***</td>
<td>.065***</td>
</tr>
<tr>
<td>ln(filers, 100-200K)</td>
<td>-.175***</td>
<td>.045</td>
<td>-.402***</td>
<td>-.094***</td>
<td>.371***</td>
<td>.049</td>
<td>.094***</td>
<td>-.046**</td>
<td>.105***</td>
<td>.047**</td>
</tr>
<tr>
<td>ln(filers, above 200K)</td>
<td>-.022</td>
<td>.025</td>
<td>.048***</td>
<td>-.009</td>
<td>-.015</td>
<td>.015</td>
<td>-.005</td>
<td>-.009</td>
<td>-.005</td>
<td>-.021</td>
</tr>
<tr>
<td>R-squared</td>
<td>.39</td>
<td>.76</td>
<td>.35</td>
<td>.74</td>
<td>.45</td>
<td>.78</td>
<td>.24</td>
<td>.58</td>
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<td>.59</td>
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<td>11059</td>
<td>11059</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>County FE</td>
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<td>Yes</td>
<td>No</td>
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<td>Time FE</td>
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<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, between ages k ∈ [k, k], where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education k ∈ [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.
Table 5: Partial Identification of Baseline Results

Notes. - The table reports point estimates obtained from an application of selection on observables from Oster (forthcoming) to the baseline fixed effect and instrumental variables results. $\delta$ denotes the degree of bias, i.e. $\delta = 0.20$ means that selection on unobservables must be no more than 20% of selection on observables. All contain fixed effects on county, industry, year, quarter, and 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. 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. All observations are weighted by county population.

<table>
<thead>
<tr>
<th>Outcome = logged employment</th>
</tr>
</thead>
<tbody>
<tr>
<td>FE</td>
</tr>
<tr>
<td>$\delta = 0$</td>
</tr>
<tr>
<td>-0.048</td>
</tr>
<tr>
<td>-0.060</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Outcome = turnover rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>FE</td>
</tr>
<tr>
<td>$\delta = 0.20$</td>
</tr>
<tr>
<td>-0.0021</td>
</tr>
<tr>
<td>-0.003</td>
</tr>
</tbody>
</table>

Table 6: Foreclosure Shocks and Net Migration Flows Across Skilled Brackets

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 $k \in [k, k]$ 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.

\[
\begin{array}{cccc}
\text{Dep. var.} & \text{logged college employment net of ...} & \text{college employment growth net of ...} \\
& \text{non-college} & \text{some-college} & \text{non-college} & \text{some-college} \\
\text{ln(foreclosures)} & -0.0308*** & -0.0135* & -0.0010** & -0.0020*** \\
& [0.0089] & [0.0075] & [0.0005] & [0.0004] \\
\text{R-squared} & .91 & .91 & .01 & .01 \\
\text{Sample Size} & 1872742 & 1847734 & 1814384 & 1788954 \\
\text{Controls} & Yes & Yes & Yes & Yes \\
\text{County FE} & Yes & Yes & Yes & Yes \\
\text{Year FE} & Yes & Yes & Yes & Yes \\
\text{Instruments?} & Yes & Yes & Yes & Yes \\
\end{array}
\]
Figure 12: Foreclosures and Skill Composition, by Income Bracket

Notes.—Sources: Longitudinal Employer-Household Dynamics, CoreLogic, Census, Federal Housing Administration. 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 7: Foreclosure Shocks and Small Business Lending

<table>
<thead>
<tr>
<th></th>
<th>ln(unguaranteed 504 loan difference)</th>
<th>ln(unguaranteed 7(a) loan difference)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>-.07***</td>
<td>-.16***</td>
</tr>
<tr>
<td></td>
<td>[.02]</td>
<td>[.02]</td>
</tr>
<tr>
<td>ln(housing price)</td>
<td>-.13***</td>
<td>-.43***</td>
</tr>
<tr>
<td></td>
<td>[.05]</td>
<td>[.06]</td>
</tr>
<tr>
<td>ln(loans due)</td>
<td>.09**</td>
<td>.15***</td>
</tr>
<tr>
<td></td>
<td>[.04]</td>
<td>[.04]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.09</td>
<td>.29</td>
</tr>
<tr>
<td>Sample Size</td>
<td>43929</td>
<td>174984</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>
</tbody>
</table>

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, 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 13: Foreclosures and Advertising & Rent Expenditures, by Major 2-digit Industry

Notes. - Sources: IPUMS Census, CoreLogic, Internal Revenue Service. The figure plots the coefficients from regressions of the logged advertising expenditures on rent (paid on machinery and other categories) on logged foreclosures, conditional on housing prices and controls. The instrumental variables specification uses the number of foreclosures predicted by the loan resets of adjustable rate and balloon mortgage loans based on plausible exogeneity of their reset times, which discontinuously raise the foreclosure probabilities. The controls include: the fraction of individuals in the county who are male, married, between ages $k \in [k_1, k_2]$ where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education $k \in [k_1, k_2]$ where the brackets are less than high school, only high school, some college, college, and graduate school, race (white and black), and logged total population. All observations are weighted by county population and standard errors are clustered at the county-level.

A Online Appendix (Not for Print)

A1. Data Construction

A1.1. 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
<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>current state of economy</th>
<th>future state of economy</th>
<th>firm hiring</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>-0.12***</td>
<td>-0.12***</td>
<td>-0.13***</td>
</tr>
<tr>
<td></td>
<td>[0.02]</td>
<td>[0.02]</td>
<td>[0.02]</td>
</tr>
<tr>
<td>ln(deposits)</td>
<td>0.04***</td>
<td>0.04***</td>
<td>0.04***</td>
</tr>
<tr>
<td></td>
<td>[0.01]</td>
<td>[0.01]</td>
<td>[0.01]</td>
</tr>
<tr>
<td>ln(payments due)</td>
<td>-0.13***</td>
<td>-0.12***</td>
<td>-0.12***</td>
</tr>
<tr>
<td></td>
<td>[0.03]</td>
<td>[0.03]</td>
<td>[0.03]</td>
</tr>
<tr>
<td>ln(housing price)</td>
<td>-0.17***</td>
<td>-0.19***</td>
<td>-0.18***</td>
</tr>
<tr>
<td></td>
<td>[0.05]</td>
<td>[0.05]</td>
<td>[0.05]</td>
</tr>
<tr>
<td>ln(adj gross income)</td>
<td>0.15***</td>
<td>0.26***</td>
<td>0.17***</td>
</tr>
<tr>
<td></td>
<td>[0.05]</td>
<td>[0.07]</td>
<td>[0.05]</td>
</tr>
<tr>
<td>ln(income)</td>
<td>0.02***</td>
<td>0.01***</td>
<td>0.01***</td>
</tr>
<tr>
<td></td>
<td>[0.00]</td>
<td>[0.00]</td>
<td>[0.00]</td>
</tr>
<tr>
<td>R-squared</td>
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<td>0.03</td>
<td>0.04</td>
</tr>
<tr>
<td>Sample Size</td>
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<td>493984</td>
<td>425709</td>
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<tr>
<td>County FE</td>
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<td>Yes</td>
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</tr>
<tr>
<td>Time FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

The table reports the coefficients associated with regressions of the logged sentiments (current state of the economy [1-4], future state of the economy [1-3], and hiring intensity of their employer [1-3]) 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 $k \in [k, k]$ 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 level and include a cubic as instruments. All observations are weighted by survey sample weights and standard errors are clustered at the county-level.
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.

A1.2. 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 crosswalk over tracts to zip-codes using the HUDS database (http://www.huduser.org/portal/datasets/usps_crosswalk.html). 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).

### A1.3. 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.

### A2. Supplemental Evidence the Rise (and Fall) of Adjustable Rate Mortgages

#### A2.1. Concentration of 5-1 and 7-1 ARMs

Figure 14 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.
A2.2. Distribution of FICO Scores

The main text emphasizes that the distribution of FICO scores among individuals with 5-1, 7-1, and 10-1 loans overlaps nearly completely with one another, suggesting that there is not non-random sorting into different types of loans based on unobservables. We now provide the evidence. Simply as a point of context, we begin with Figure 15, which plots the distribution of FICO scores for individuals with 2-1 and 3-1 ARMs. While those with 2-1 ARMs have a lower mean and slightly left-skewed distribution of scores, there is significant overlap between the two.

We now turn to the main sets of loans used in our instrumental variables specifications. Figures 16 and 17 plot the distributions of 5-1 & 7-1 and 7-1 and 10-1. Both have almost complete overlap, suggesting that there is no evidence of a major difference in credit worthiness of the two sets of borrowers. In this sense, our robustness using variation in ARM interest rate resets driven by non 2-1 and 3-1 loans behaves as a suitable robustness exercise.
Figure 15: Distribution of FICO Scores, 2-1 and 3-1 Adjustable Rate Mortgages

Notes.–Sources: CoreLogic. The figure plots the distribution of FICO scores for 2-1 and 3-1 adjustable rate mortgages.
Figure 16: Distribution of FICO Scores, 5-1 and 7-1 Adjustable Rate Mortgages

Notes: Sources: CoreLogic. The figure plots the distribution of FICO scores for 5-1 and 7-1 adjustable rate mortgages.
Figure 17: Distribution of FICO Scores, 7-1 and 10-1 Adjustable Rate Mortgages

Notes. – Sources: CoreLogic. The figure plots the distribution of FICO scores for 7-1 and 10-1 adjustable rate mortgages.

A2.3. 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 9 documents these results. These shocks provide no predictive power for understanding the dispersion in the share of ARMs.

**Table 9**: 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>0.01</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>housing price, 90-00 growth</td>
<td>0.01</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>college, 90-00 growth</td>
<td>-0.00</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>unemployment, 90-00 growth</td>
<td>0.00</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>R²</td>
<td>0.00</td>
<td>0.01</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
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<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.

**A3. Supplemental Evidence on Main Results**

**A3.1. 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 6. 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.
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 10 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 10:** Replication of Mian and Sufi (2014)

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

Notes.—Sources: Longitudinal Employer Household Dynamics (LEHD), Census, Federal Housing Administration. The table reports the coefficients associated with regressions of two-digit industry logged employment and employment growth on housing price growth (index with 2000 base year). Columns 1 and 2 are on the pooled sample; columns 3 and 4 are on the non-tradables and tradables sectors, respectively, based on Mian and Sufi (2014) classification. Controls include: the fraction of individuals in the county who are male, married, between ages \(k\)\[k,\tilde{k}\] where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education \(k\)\[k,\tilde{k}\] 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.

**A3.2. 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 19 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.
Figure 19: 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.

We also characterize the variation in loan origination across each category in Figure 20. We observe a massive surge in loan origination between 2002 and 2007, which creates significant variation in interest rate resets following the Great Recession for 5-1, 7-1, and 10-1 ARMs. We also see that, while the market for 2-1 and 3-1 loans collapsed almost entirely by 2007, there was still variation in both 5-1 and 7-1 ARMS, which is consistent with our evidence above that there remains predictive power in our instrument even after the Great Recession.
Figure 20: 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.

A3.3. Interest Rates over Time by Vintage

Figure 21 illustrates the change in interest rates—that is, the difference between interest rates in period $t$ and their original interest rate at origination—and the evolution of loan interest rates as
a function of their age. We specifically focus on the most common type of ARMs (2-1, 3-1, 5-1, and 7-1) and different vintages (origination year). The data highlights that there is substantial variation over time and across vintages—both interest rate increases and decreases. The increases occur at precisely the points in time predicted by the terms of the contract for each loan, thereby creating the large discontinuous rises in the probability of foreclosure. These plots also show how the specific size and direction of these interest rate changes varies across loan types and vintages.
Figure 21: Median Interest Rate Change, by Hybrid ARM, Months Since Origination

Notes.—Sources: CoreLogic. The figures plot the interest rate changes (relative to the initial interest rate of the loan) for each separate adjustable rate mortgage (ARM) category since the date of origination.
A4. Supplemental Evidence on Main Results and Robustness Exercises

A4.1. 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 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 11 documents the results of our analyses on economic inequality outcomes. The unconditional correlation is positive: a 1% rise in foreclosures is associated with a 0.0054% increase in the Gini coefficient calculated by comparing incomes of zip codes within a given county. Given that the mean Gini coefficient is 0.435, 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. Furthermore, once basic demographic controls are introduced in column 2, the coefficient switches signs. Moreover, both our fixed effects and instrumental variables estimators point towards a dampening of intra-county income inequality in response to foreclosure shocks.

These results are important in light of recent descriptive evidence among both non-economists (e.g., see Rugh and Massey (2010) and Dymski et al. (2010)) and popular press.\(^\text{70}\) While minority groups were more likely to receive sub-prime loans preceding the Great Recession, non-economists 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, nor does their instrument of inter-metro variation in racial differentials of subprime loans satisfy

\(^{70}\)For example, see http://www.huffingtonpost.com/ray-brescia/when-the-rich-get-risky-i_b_605535.html.
Table 11: Baseline Estimates of Foreclosure Shocks on Inequality

<table>
<thead>
<tr>
<th></th>
<th>gini coefficient</th>
<th>OLS</th>
<th>OLS</th>
<th>OLS</th>
<th>IV</th>
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</thead>
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<tr>
<td>ln(foreclosures)</td>
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<td>-.0012</td>
<td>-.0020***</td>
<td>-.0067***</td>
<td></td>
</tr>
<tr>
<td>ln(home value)</td>
<td>-.0201***</td>
<td>-.0110***</td>
<td>-.0234***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[.0068]</td>
<td>[.0030]</td>
<td>[.0042]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>male</td>
<td>-.1094</td>
<td>-.2208***</td>
<td>-.2367***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[.0675]</td>
<td>[.0664]</td>
<td>[.0667]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>married</td>
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<td>-.0180*</td>
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<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[.0135]</td>
<td>[.0100]</td>
<td>[.0103]</td>
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<td></td>
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<tr>
<td>ln(population)</td>
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<td>-.0184**</td>
<td>-.0123</td>
<td></td>
<td></td>
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<td></td>
<td>[.0035]</td>
<td>[.0084]</td>
<td>[.0087]</td>
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<td>[.0219]</td>
<td>[.0270]</td>
<td>[.0272]</td>
<td></td>
<td></td>
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<tr>
<td>some college</td>
<td>.1256***</td>
<td>-.0353*</td>
<td>-.0401*</td>
<td></td>
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<tr>
<td></td>
<td>[.0244]</td>
<td>[.0213]</td>
<td>[.0215]</td>
<td></td>
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<tr>
<td>college degree</td>
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<td>[.0376]</td>
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<td>[.0284]</td>
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<td>graduate degree</td>
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<td>[.0541]</td>
<td>[.0418]</td>
<td>[.0437]</td>
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<td>R-squared</td>
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<td>.96</td>
<td>.96</td>
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<td>Sample Size</td>
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<tr>
<td>County FE</td>
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<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
</tbody>
</table>

Notes. – Sources: CoreLogic, Census, 2006-2014. The table reports the coefficients associated with regressions of the Gini coefficient on logged county foreclosures and logged home values, conditional on 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. 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.
the basic exclusion restriction. To the extent that these groups differ in their human capital or match qualities, then race differentials are also endogenous. The fact that we discover a negative correlation between intra-county income inequality and foreclosures under our semi-parametric, fixed effects, and instrumental variables regressions helps to address this gap in the literature. At the same time, the analyses we conduct here do nothing to directly investigate important and related effects, such as inter-county income inequality or inequality of wealth (either within or between counties).

A4.2. Foreclosures and Time Use

We now examine whether the rise in foreclosures is associated with changes in the allocation of time. Using the American Time Use Survey (ATUS) between 2003 and 2014, Columns 1-3 regress logged time in leisure (using the first definition in Aguiar and Hurst (2007)) on logged foreclosures, conditional on controls. Column 3 specifically uses the instrumental variable. The results suggest that increases in foreclosures are associated with meager declines in leisure (and increases in hours worked), but not associated with the time parents invest with their children. Table 12 presents these results.

Table 12: Foreclosure Shocks and the Allocation of Time

<table>
<thead>
<tr>
<th>Dep. var. =</th>
<th>logged leisure</th>
<th>logged time w/ children</th>
</tr>
</thead>
<tbody>
<tr>
<td>OLS</td>
<td>OLS IV</td>
<td>OLS OLS IV</td>
</tr>
<tr>
<td>logged foreclosures</td>
<td>-.034***</td>
<td>-.027*</td>
</tr>
<tr>
<td>R-squared</td>
<td>.07</td>
<td>.07</td>
</tr>
<tr>
<td>Sample Size</td>
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<td>49756</td>
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<td>Controls</td>
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<td>Yes</td>
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<tr>
<td>State FE</td>
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<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes. Sources: American Time Use Survey, CoreLogic. 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. Standard errors are clustered at the state-level and observations are weighted by ATUS sample weights.
A4.3. Heterogeneous Effects

While the main text presented heterogeneous treatment effects of Equation 3 separately for non-tradables / tradables sectors and non-judicial / judicial states, we turn towards four additional sources of potential heterogeneity.

The first dimension of heterogeneity is a county’s employment share for the construction sector 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. To avoid the potential simultaneity between employment shares and foreclosures, we use employment shares in the construction sector based on the 2000 Decennial Census.

Figure 22 plots these estimates under our preferred instrumental variables specifications for each of our three main outcome variables. Although our standard errors grow, we find relatively strong evidence of a stronger foreclosure gradient for counties with larger construction sectors. For instance, we find that a 10% rise in foreclosures is associated with a 1.5% decline in employment among counties with 8.9-24% of workers employed in the manufacturing sector in 2000, but we cannot reject the null that the decline is closer to 3%. We also find stronger and more precise point estimates when employment growth and the turnover rate are our outcome variables. For example, a comparable 10% rise of foreclosures is associated with nearly a 0.4 percentage point decline in employment growth and a 0.15 percentage point decline in the turnover rate. These larger estimates suggest that counties with larger employment shares in construction may have been less adaptable to the foreclosure crisis because their labor markets were not as diversified.
Figure 22: Foreclosures and Local Labor Markets, by Construction Employment Shares

Notes. – Sources: Longitudinal Employer-Household Dynamics, CoreLogic, Census, Zillow. The figures plot the estimated coefficients of a measure of the local labor market regressed on logged county foreclosures estimates separately by quartiles on the 2000 employment share in the construction sector. The controls include: logged housing prices, 10 bins on housing price growth, 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. 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.

The second dimension of heterogeneity is a county’s 2000 average household income. Motivated by evidence that the rise of mortgage defaults were concentrated in sub-prime zip-codes (Mian and Sufi, 2009), the effects of foreclosures could have been greater in them.71 Figure 23 plots these estimates again under the baseline specification across the three main outcome variables. While the standard errors are larger, we tend to see the highest income counties having the lowest foreclosure gradient. In contrast, we see that a 10% rise in foreclosures is associated with a

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71There 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).
1.5% decline in employment, a 0.32 percentage point decline in employment growth, and a 0.018 percentage point decline in turnover rates within counties with average incomes between $12,000-$38,300. Nonetheless, our results are not consistent with the claim that higher income counties are unaffected—we still see considerable negative associations for counties with median incomes above $43,700, for example.

Figure 23: Foreclosures and Local Labor Markets, by County Incomes

Notes. –Sources: Longitudinal Employer-Household Dynamics, CoreLogic, Census, Federal Housing Administration. The figures plot the estimated coefficients of a measure of the local labor market regressed on logged county foreclosures estimates separately by quartiles on the 2000 median county household income. The controls include: logged housing prices, 10 bins on housing price growth, the fraction of individuals in the county who are male, married, between ages \(k \in [k, k]\) 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. 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.

The third dimension of heterogeneity is the size of an establishment. 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 had a larger effect on the closure of estab-
lishments 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 24 subsequently examines the effects of foreclosures on establishments of different sizes. We specifically estimated our main regression using ordinary least squares. While we are potentially underestimating the effects of foreclosures, we defer to being conservative in light of the fact that our instrument does not contain within-firm size variation in its treatment effect. Remarkably, both the levels and logs specifications suggest that there is a stark negative impact of foreclosures at the bottom of the firm size distribution, but establishments at the top of the distribution were not affected. Since many small businesses have their collateral and equity vested in their homes, foreclosure on their home often involves abandoning their business.

Figure 24: Foreclosures and Establishment Closures, by Establishment Size

Notes. – Sources: Longitudinal Employer-Household Dynamics, CoreLogic, Census, Federal Housing Administration, County Business Patterns. The table reports the coefficients associated with regressions of establishments in both levels and logs logged on logged foreclosures, conditional on controls and separately for establishment with different sizes (number of employees). The controls include: the fraction of individuals in the county who are male, married, between ages \( k \in [k, k+1) \) where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education \( k \in [k, k+1) \) where the brackets are less than high school, only high school, some college, college, and graduate school, race (white and black), and logged total population. All observations are weighted by county population and standard errors are clustered at the county-level.

The fourth dimension of heterogeneity is in the potential asymmetric effects of foreclosures
during housing booms versus busts. Letting $1[\Delta h < 0]$ denote an indicator for whether a county is in a housing bust, we now consider

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

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). 72

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.

### A4.4. Examining the Possible Endogeneity of Credit shocks

We now explore the potential for omitted variables arising from contemporaneous credit market shocks. Although we fully recognize their importance in explaining the transmission of shocks during the financial crisis, our argument here is that foreclosures do not directly affect credit. Of course, foreclosures can affect credit indirectly through other channels, such as housing prices or employment, and we provide two mechanisms through which these effects on employment can manifest, but our intention here is to rule out credit shocks as a potential omitted variable in our baseline equation. We focus on three measures of credit: logged value of loans to commercial entities, logged total value of loans, and capital structure (assets net of liabilities scaled by assets). We measure these only for local banks since national banks should be unresponsive to local foreclosure shocks.

These results are documented in Table 13. Across the board, we see no association between

72Since 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.
foreclosures and each of the measures of credit (with the exception of an arguably spurious positive correlation between foreclosures and commercial lending). Put simply, increases in foreclosures are not associated with causal increases in the degree of losses banks are taking, which is consistent with our identifying assumption that our observed decline in labor market outcomes is not a function of contemporaneous credit shocks that are affecting the real economy. That does not imply that foreclosures have no indirect effects on credit—indeed, we find that increases in employment are associated with heightened lending and lower capital requirements in auxiliary regressions, which suggests that the way foreclosures affect credit is indirectly through either the labor or housing market.

Table 13: Examining Potential Unobserved Heterogeneity b/w Foreclosures and Credit Shocks

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<tr>
<th>Dep. var.</th>
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<th>ln(total loans)</th>
<th>(assets - liabilities)/assets</th>
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<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>0.09</td>
<td>0.54*</td>
<td>0.03</td>
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<tr>
<td>R-squared</td>
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<td>0.79</td>
<td>0.84</td>
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<tr>
<td>County FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
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<td>No</td>
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</tr>
<tr>
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</tbody>
</table>

Notes.—Sources: CoreLogic, Census, Federal Housing Administration, Longitudinal Employer-Household Dynamics, 2000-2014. The table reports the coefficients associated with regressions of logged total loan value to commercial entities within a county/quarter, logged total loan value across all entities, and the capital structure (assets - liabilities)/assets on logged county foreclosures and logged home values, conditional on 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. 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.

A4.5. Foreclosure Intensity and Non-linearities

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

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

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

We estimate Equation 6 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.\(^{73}\)

Figure 25 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 25 point

\(^{73}\)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.
towards strong non-linearities in the effects of foreclosures on county unemployment rates, but only in states without judicial status laws.

Figure 25: Foreclosure Interaction Effects Across Intensity Distribution, Non-judicial & Judicial

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

We now examine the potential for the intensity of foreclosure shocks to impact labor market outcomes, rather than the contemporaneous flow. Turning back towards our LEHD sample, we now estimate our baseline specification using logged cumulative foreclosures on the right hand side. Table 14 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 14: Examining the Potential for Foreclosure Intensity

<table>
<thead>
<tr>
<th></th>
<th>Dep. var. = logged county-by-industry employment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All NonTradable Tradable NonJudicial Judicial</td>
</tr>
<tr>
<td>ln(cumulative foreclosures)</td>
<td>-.170*** -.089*** -.262** -.307*** -.015</td>
</tr>
<tr>
<td>R-squared</td>
<td>.87 .94 .81 .87 .87</td>
</tr>
<tr>
<td>Sample Size</td>
<td>175142 691756 230084 934276 757484</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes Yes Yes Yes Yes</td>
</tr>
<tr>
<td>Industry FE</td>
<td>Yes Yes Yes Yes Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>Yes Yes Yes Yes Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>Yes Yes Yes Yes Yes</td>
</tr>
<tr>
<td>Instruments?</td>
<td>Yes Yes Yes Yes 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 $k \in [k, K]$ 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. 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.

A4.6. 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. We gauge the potential magnitude of these scenarios by including 2-1 and 3-1 ARMs in our sample since these reset upwards at far
greater rates than the 5-1, 7-1, and 10-1 ARMs that in our main specifications. Table 15 document these results. We find 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.

### Table 15: Comparison of Baseline with 2-1 and 3-1 ARMs in IV

<table>
<thead>
<tr>
<th>Dep. var. = ln(employment)</th>
<th>ln(employment)</th>
<th>employment growth</th>
<th>turnover rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>(.090***)</td>
<td>(.116***)</td>
<td>(.010***)</td>
</tr>
<tr>
<td></td>
<td>[.018]</td>
<td>[.023]</td>
<td>[.004]</td>
</tr>
<tr>
<td>R-squared</td>
<td>.87</td>
<td>.87</td>
<td>.60</td>
</tr>
<tr>
<td>Sample Size</td>
<td>1751422</td>
<td>1751422</td>
<td>1616818</td>
</tr>
<tr>
<td>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>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>
<tr>
<td>2-1, 3-1 ARMs included?</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
</tr>
</tbody>
</table>

Notes. – Sources: CoreLogic, Census, 2006-2014. The table reports the coefficients associated with regressions of logged industry-by-county employment, employment growth, and the turnover rate on logged foreclosures, conditional on controls and fixed effects. 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. 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. In even columns, 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, whereas the odd columns only use 5-1, 7-1, and 10-1 ARMs. 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.

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 26 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.
A5. Supplemental Evidence on Mechanisms

A5.1. 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

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

(7)

where 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.\(^{74}\) In estimation of Equation 7, we did not have enough variation when we use

\(^{74}\)We obtain the crime data from the United States Department of Justice. Office of Justice Programs. Federal

Figure 26: 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.
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 16 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 27 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.75

---


<table>
<thead>
<tr>
<th>Dep. var. = ln(total crime)</th>
<th>ln(violent crime)</th>
<th>ln(property crime)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foreclosures)</td>
<td>.252** (.112)</td>
<td>.367** (.155)</td>
</tr>
<tr>
<td>ln(payments due)</td>
<td>-.337 (-.215)</td>
<td>-.954** (.455)</td>
</tr>
<tr>
<td>ln(deposits)</td>
<td>.000 (.058)</td>
<td>.013 (.057)</td>
</tr>
<tr>
<td>ln(adj gross income)</td>
<td>-.097 (.612)</td>
<td>.021 (.099)</td>
</tr>
</tbody>
</table>

R-squared: .91 .89 .89 .88 .87 .85
Sample Size: 15526 11815 15429 11747 15500 11792
Controls: Yes Yes Yes Yes Yes Yes
County FE: Yes Yes Yes Yes Yes Yes
Time FE: Yes Yes Yes Yes Yes Yes

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 6). 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 17 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 17: 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>.0290*** (.0065)</td>
<td>.0273*** (.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>
<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 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, controls, and fixed effects. 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.

A5.2. 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 28 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 28: 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.