Foreclosure Spillovers and Individual Well-being: Evidence from the Great Recession

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For Review

Abstract

Using microdata from CoreLogic and Gallup between 2008 and 2014, we study the impact of foreclosures on individual well-being. We identify the causal effect of foreclosures by instrumenting them using variation in interest rate spikes associated with different types of adjustable rate mortgages (ARMs), conditional on controls and zipcode and time fixed effects. We find that a 10% rise in foreclosures is associated with a 0.51% and 0.26% decline in current and expected future life satisfaction. These effects are driven by a combination of lower local amenities (e.g., neighborhood quality) and greater uncertainty (e.g., income risk).

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Keywords: foreclosures, housing market, life satisfaction, well-being.

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1. Introduction

Moreover, it is important to recognize that the costs of foreclosure may extend well beyond those borne directly by the borrower and the lender. Clusters of foreclosures can destabilize communities, reduce the property values of nearby homes, and lower municipal tax revenues. (Bernanke, 2008)

Understanding the transmission of economic shocks to individual outcomes has been an important objective for economists at least since Friedman (1957). While the bulk of the literature has focused on characterizing the effect on consumption (Hall, 1978; Flavin, 1981; Campbell, 1987; Cochrane, 1991), a more recent literature has taken a broader view of welfare to include measures of happiness (Frijters et al., 2004; Bayer and Juessen, 2015). Empirical characterizations of how these shocks affect individual well-being may be important for disciplining structural business cycle models to incorporate realistic sources of heterogeneity (Lucas, 1987; Krusell et al., 2009) and the pass-through of risk onto households (Blundell et al., 2008; Heathcote et al., 2014).

This paper exploits the rapid surge in foreclosures on home mortgages during the Great Recession to examine whether and why individual well-being and sentiment change in response to local (zipcode) foreclosure shocks. In Q2:2009, 1.47% of all open mortgages were in foreclosure, which was up 400% from the average in 2000 of 0.36% (Figure 1). The rise in foreclosures is associated with many persistent and adverse real outcomes: heightened mortality (Currie and Tekin, 2015) and crime (Cui and Walsh, 2015; Ellen et al., 2013), depressed housing values (Mian et al., 2015) and labor markets (Makridis and Ohlrögge, 2017). On top of these associations, a major concern associated with these foreclosures was that they would “destabilize communities”, prompting au-

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1See Jappelli and Pistaferri (2010) for a survey on income and consumption.

2As we will discuss later, we implement a variant of Attanasio and Davis (1996) by regressing perceptions of current life satisfaction growth on income growth, controlling for consumption expenditure growth, by producing a synthetic panel from the Gallup and Consumption Expenditure Survey (CES) data. Figure 12 in Appendix Section A2. shows that there is significant variation in life satisfaction growth in response to income growth even after controlling for consumption growth, suggesting that there are other channels of pass-through.

3While there is skepticism about the validity of measures on well-being (e.g., Bertrand and Mullainathan (2001)), it has been linked with an array of real outcomes, including GDP (di Tella et al., 2001; Stevenson and Wolfers, 2013), marginal rates of substitution between goods (Benjamin et al., 2014a), health (Hafner et al., 2015; Danzer and Danzer, 2016), and productivity in the work-place (Oswald et al., 2015). There have also been applications of happiness data in other areas of public economics, e.g., environmental economics (Levinson, 2012; Anderson et al., 2016). See Frey and Stutzer (2002) for a discussion about the importance of happiness data in economics and Krueger and Schkade (2008) for an analysis of the reliability of well-being measures. Krueger and Schkade (2008) emphasize that, while well-being tends to contain sizable measurement error, it should be reliable in the presence of large samples and estimates of average treatments across groups.
uthorization of $75 billion through the Home Affordable Modification Program (HAMP) to help renegotiate loans among seriously delinquent borrowers. This paper provides the first evidence behind Bernanke's (2008) concern: local foreclosures produced non-pecuniary spillovers that affected individuals in those communities even conditional on housing and labor market shocks.

Unfortunately, empirically estimating the causal effects of local foreclosures is fraught with identification problems. Even conditional on location fixed effects, there are two opposing forces of endogeneity. On one hand, individuals who experience unemployment will find it much more difficult to make their monthly mortgage payments, raising their probability of entering foreclosure (Tian et al., 2016; Hsu et al., forthcoming; Gerardi et al., forthcoming); this overestimates the effect of foreclosures since unemployment shocks also depress well-being. On the other hand, individuals who experience large declines in housing values and eventually become seriously delinquent are not necessarily at risk of immediate foreclosure since banks during the crisis faced a strategic incentive to avoid recognizing underwater homes on their balance sheet, which would have placed many of them in insolvency; this underestimates the effect of foreclosures since banks may delay foreclosure until economic activity and, therefore, well-being return to trend. A related explanation behind foreclosure delay was administrative backlogs since the surge in foreclosures took place in such a short window of time.

The first part of our paper uses an identification strategy introduced in our companion work to instrument for foreclosures and identify their causal effect on self-reported current and perceived future well-being (Makridis and Ohlrogge, 2017). Our identification strategy exploits quasi-experimental variation in the institutional structure of the mortgage market, leveraging over 97 million loan × month observations of adjustable rate mortgages (ARMs) originated prior to the

4We 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.

5See Figure 1 for an illustration of the sudden spikes and subsequent declines in the share of new foreclosures entering the market. Former Treasury Secretary Tim Geithner is famously reported as describing federal programs for homeowners in foreclosure to have been principally designed to slow the pace of foreclosures so as to “foam the runway” for banks, enabling them to avoid recognizing losses on their housing portfolios all at once. For evidence of significant backlogging, see: http://www.nytimes.com/2011/06/19/business/19foreclosure.html?mcubz=3. Failing to account for these influences, therefore, could lead to an under-estimation of the negative impact of foreclosures on well-being, since the increases in well-being stemming from economic recovery would be spuriously correlated with the foreclosures which do not hit peak levels until after the worst of an economic downturn has passed.
Great Recession. These loans specify 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, which we show is associated with a discontinuous change in the probability of foreclosure. These foreclosures on ARM borrowers also raise the probability of foreclosure among nearby borrowers with fixed rate mortgages (FRM). After matching quarterly zipcode mortgage data with proprietary micro-data from Gallup between 2008 and 2014, we use the quasi-experimental variation from interest rate resets to identify the effects of foreclosures on well-being.\footnote{Unlike prior studies that have been constrained by sample size and the breadth of survey questions, Gallup is the United States’ premier polling institution, surveying 1,000 individuals each day using a reliable survey methodology on a wide scale. Interviewers receive eight hours of classroom instruction conducted by a learning and development specialist over a two-day period. The specialist also works one-on-one with new interviewers, offering contextualized instruction for the first six weeks on the job. For example, the specialist will often tape interviews and replay them to help the new interviewer refine their approach. We also note that, while there is a psychology literature on the effects of foreclosure on mental and emotional well-being, all these studies are based on small sample focus groups; see Tsai (2015) for a meta-analysis.}

The exclusion restriction for our baseline instrumental variables strategy requires that there is no systematic relationship between the contractually pre-determined ARM interest rate resets and other time-varying unobserved shocks to individual well-being. While the variation in the share of ARMs across locations is driven by largely quasi-random historical factors that led to different national bank lending strategies, the presence of other time-varying unobserved shocks to dispersion in ARM origination would not on their own violate our identifying assumption that, for example, the relative proportion of 5-1 to 7-1 ARM originations is uncorrelated with unobserved shocks to well-being five or seven years in the future.

Our baseline specifications exploit variation in predicted foreclosures, controlling for zipcode / time fixed effects and employment and housing growth. We find that a 10% rise in foreclosures is associated with a 0.51% and 0.26% decline in current and expected future life satisfaction. Interestingly, these effects are nearly twice as large in states with judicial foreclosure laws (0.72% and 0.53% for current life satisfaction in judicial and non-judicial), which may reflect that foreclosure is a more drawn out process in these states (Pence, 2006), making foreclosure more salient and emotionally detrimental for seriously delinquent borrowers. We also find that these effects are nearly twice as large in states with recourse laws than those without (0.80% and 0.42% for current life satisfaction in recourse and non-recourse), which may reflect that foreclosure in areas where banks can seize an individual’s assets will induce larger effects (Ghent and Kudlyak, 2011).

To understand how these declines in well-being compare with the increases in well-being from housing price appreciation before the the financial crisis, we exploit within-zipcode housing price appreciation.
growth, we find that a percentage point (pp) rise in housing price growth is associated with a 0.14% rise in life satisfaction. When we instrument using historical county housing price growth between 2000 and 2005 (pre-determined with respect to post-2008 housing price growth), we obtain estimates of 0.097-0.127%. Interestingly, however, we do not find that housing price growth is associated with higher expected future well-being. We interpret these results as evidence that, while housing price growth may have led to an increase in life satisfaction between 2000 and 2007, the subsequent surge in foreclosures during the crisis erased the bulk of those gains.

Even if our exclusion restriction is satisfied (see Makridis and Ohlrogge (2017)), there are two primary concerns associated with these results. First, since we use variation in foreclosures arising from interest rate resets, it is possible that increases (decreases) in a borrower’s monthly mortgage payment lead to reductions (increase) in consumption (Di Maggio et al., 2017), which indirectly affect well-being. In this sense, we would attribute variation in well-being to foreclosures, rather than the increase or decrease in a borrower’s disposable income. While our companion work showed that state consumption expenditures are associated with interest rate resets only for the housing and utilities category, our data allows us to directly control for individual daily consumption expenditures and monthly income and/or examine its correlation with local interest rate resets. These controls do not alter our results. Second, since we are unable to match individuals in Gallup with their exact address in our mortgage data, it is possible that the individuals driving our elasticities are precisely those who are foreclosed upon. We perform specific bounding exercises that show, for instance, that even under extreme assumptions, individuals being directly foreclosed upon could only account for a small percentage of our estimated effects.\(^7\)

The second part of our paper examines two possible mechanisms that may explain the observed decline in well-being. First, we show that increases in foreclosures are associated with declines in local sentiment. For example, a 10% rise in foreclosures is associated with a 1.5% decline in perception about the current state of the economy, as well as a 0.2% rise in the dispersion of the current state of the economy, as well as a 2.5% decrease in perceptions about the future state of the economy and a 0.5% increase in the dispersion of beliefs about the future state of the economy.

\(^7\)We also conduct additional placebos to examine the potential for these mechanical effects since they are important to the interpretation of our results. Specifically, suppose that we are looking at zipcode A where 50% of the homeowners are in foreclosure and zipcode B where 5% are in foreclosure. For a given individual in our Gallup data located in zipcode A, the probability that the individual is foreclosed upon is much higher than a comparable person in zipcode B. Motivated by this logic, we restrict the sample of our zipcodes to those with 3% of homeowners in foreclosure and obtain very similar foreclosure elasticities. If the mechanical response explained our results, we would find a much more attenuated foreclosure elasticity since we would necessarily have fewer individuals from our Gallup data who were foreclosed upon in that zipcode.
even after controlling for the first moment. Second, we show that increases in foreclosures are associated with declines in local amenities. For example, a one pp rise in the growth rate of foreclosures is associated with a 0.02pp decline in the probability that an individual feels satisfied with their city and a 0.046pp decline in the probability an individual feels safe walking at night. One reason for this is that foreclosures raise crime by raising vacancy rates (Immergluck and Smith, 2006; Ellen et al., 2013; Cui and Walsh, 2015). Interestingly, these foreclosure gradients are slightly larger than the employment (and in one case the housing price) growth gradient(s), suggesting that foreclosures have uniquely large spillover effects on local communities.

Our paper contributes directly to two main literatures. The first is a literature studies the pass-through of shocks (i.e., wage income) onto households, typically measured through consumption fluctuations. While the large and persistent effects of job displacement on earning dynamics is not surprising (Jacobson et al., 1993; Couch and Placzek, 2010), Sullivan and Wachter (2009) and Eliason and Storrie (2009) have shown that job displacement also has a significant effect on mortality outcomes. Our results complement this literature by showing that individuals might be indirectly affected by local shocks even if they are not the primary target. Closely related is a literature on the impact of income shocks on consumption and estimates of the marginal propensity to consume. While early papers implemented tests of the permanent income hypothesis (Cochrane, 1991; Attanasio and Davis, 1996), a more recent series of papers have estimated the extent of partial insurance (Blundell et al., 2008; Kaplan and Violante, 2010; Heathcote et al., 2014). The fact that local shocks can create large spillovers that affect others in the area even after controlling for differences in consumption and/or income points towards other pass-through channels that are important for understanding welfare. For example, as in the case with Chernobyl, only well-being indices displayed a persistent welfare decline (Danzer and Danzer, 2016).

The second is a literature on the causal effects of foreclosures. While there is some evidence on their effects on crime (Ellen et al., 2013; Cui and Walsh, 2015) and mortality (Currie and Tekin, 2015), most contributions are on the housing market effects. For example, Campbell et al. (2011)

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8Early evidence focused on the relationship between recessions and health; see, for example, Ruhm (2000), Ruhm and Black (2002), Ruhm (2003), and Neumayer (2004).

9While highly complementary, our paper differs from Currie and Tekin (2015) in several ways. First, we exploit a source of plausibly exogenous variation, which is vital in our setting since well-being is closely connected with local economic shocks. Indeed, if we simply use county by quarter fixed effects as in Currie and Tekin (2015), we would wrongly conclude that foreclosures have no significant effect on well-being. Second, we provide a conceptual narrative and two concrete mechanisms that explain their result, in addition to providing a series of new results. In particular, the decline in well-being could trigger a decline in health status and, therefore, mortality. For example, see: https://www.washingtonpost.com/news/to-your-health/wp/2017/06/20/in-just-one-year-nearly-1-3-million-americans-needed-hospital-care-for-opioid-related-issues/
and Guren and McQuade (2013) examine how fire sales amplify housing price declines, Mian et al. (2015) estimate a housing-foreclosure price elasticity, and Gupta (2016) examine the spillover effects of foreclosures on housing prices. Using variation in interest rate resets in a way related to Fuster and Willen (2017), Gupta (2016), and Di Maggio et al. (2017), we focus on an alternative set of outcome variables, namely individual well-being and perceptions about economic activity, and how foreclosures generate local spillovers beyond those being directly foreclosed upon. In this sense, our paper is also related more broadly with recent work examining the effects of housing fluctuations on real economic activity, including the decline in credit (Mian and Sufi, 2009; Adelino et al., 2016), consumption (Mian and Sufi, 2011; Mian et al., 2013), employment (Mian and Sufi, 2014), and small business entrepreneurship (Adelino et al., 2015; Chen et al., 2017).

2. Data, Measurement, and Institutional Details

2.1. Data Sources

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 hybrid ARM loans (i.e. ARM loans with initial fixed rates that then reset to floating rates after an interval between five and ten years), as well as balloon mortgages.

We focus on foreclosures between 2008 and 2014, covering 2,438,444 million unique hybrid and balloon mortgage loans. For each loan, we observe a vector of initial characteristics, giving information such as the contract type (Hybrid ARM, Balloon, etc.), the initial interest rate, and the schedule for interest rate resets and balloon payments. We also observe monthly performance updates, giving information on factors such a loan’s current interest rate and whether it has prepaid, been foreclosed upon, or is still current. Our data set covers a total of 97,109,762 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 might be subject to selection problems.\(^\text{10}\)

\(^{10}\)For example, Ferreira and Gyourko (2015) emphasize that focusing purely on subprime mortgages misses the broader and more adverse set of foreclosures that followed 2008 after the subprime mortgages had already been dumped. In particular, over 40,630 more prime borrowers (relative to subprime borrowers) lost their homes in
**Zipcode Panel of Housing and Labor Market Outcomes.**—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 tends to cover “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). We also draw on the Zipcode Business Patterns to measure employment, employment growth, and establishments at a zipcode level. We restrict the sample to those zipcodes that do not have disclosure problems due to too small of a sample. Both our housing and labor market outcomes are at an annual frequency.

**Gallup Daily Polling Repeated Cross-section.**—To understand how foreclosures impact local investment, we draw on a newly licensed data 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 interviewers with randomly sampled respondents (age 18 or over) from all 50 states and the District of Columbia. Detailed location data, such as the zipcode and metro area, is also available with corresponding sample weights. Gallup also routinely incorporates questions on specific topics, such as voting intentions and perceptions of current events.

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

the second, third, and fourth quarters of 2008; an additional 656,000 prime borrowers lost their homes between 2009-2012 (relative to subprime borrowers).


12Despite the reliable methodology, Gallup’s implementation of the U.S. Daily in the early years was challenged by the fact that all the economics and politics questions were asked for each respondent. As Deaton (2011) discusses, that approach had the risk of creating fatigue among respondents, thereby introducing noise into their answers in,
The main questions we focus on to measure well-being are current and future life satisfaction: (i) “Please imagine a ladder with steps numbered from zero at the bottom to ten at the top. The top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you. On which step of the ladder would you say you personally feel you stand at this time?” [0-10 index]; (ii) “On which step do you think you will stand about five years from now?” [0-10 index]. We also leverage respondent answers to two questions about the current and future state of the economy: (i) “How would you rate economic conditions in this country today: as excellent, good, only fair, or poor?” [1-4 index]; (ii) “Right now, do you think that economic conditions in this country, as a whole, are getting better or getting worse?” [1-3 index].

While some have argued against the use of self-reported measures of well-being (Bertrand and Mullainathan, 2001), and indeed there are serious drawbacks to these measures, these measures have also been an active area of interest for understanding the marginal utility of income (Layard et al., 2008), the relationship between income and happiness (Easterlin, 1996; Stevenson and Wolfers, 2008, 2013), productivity in the work place (Oswald et al., 2015), the persistence of income shocks (Bayer and Juessen, 2015), the presence of self-fulfilling business cycles (Benhabib and Spiegel, 2016; Makridis, 2017), among other topics. Our stance is that, despite measurement error in these individual-level measurements, since our main independent variable of interest will vary at the location level, we do not need to be as concerned with measurement error as a chronic endogeneity problem. If, for example, we were using life satisfaction to compute gradients with health status, we would worry that measurement error in both variables at the individual-level would create upwards bias.

2.2. Institutional Context

Foreclosures grew rapidly during the Great Recession, growing by 300-400% relative to trend in many zipcodes and geographies (Figure 1). The sudden and significant worried many policymakers in the federal government and federal reserve, prompting the passage of the Troubled Asset Relief Program (TARP), which authorized expenditures of $700 billion on October 3, 2008 to help stabilize the financial system. One component of TARP was the Making Home Affordable (MHA)
program, which was designed to maintain home ownership and mitigate foreclosure rates. The Home Affordable Modification Program (HAMP) was the largest of these sub-programs in MHA, helping seriously delinquent borrowers renegotiate their loans to avoid foreclosure. The original authorization for HAMP was $50 billion as measured by the Government Accountability Office (2014) to help 3-4 million homeowners avoid foreclosure, but they later updated the assessment to include an additional $27.8 billion since 2009 (GAO, 2016).\(^\text{13}\)

A fundamental feature of the financial crisis, and a key identification challenge, was a confluence of factors that led many foreclosures to be delayed until after local economic began improving. One driver of this was behavior among banks to strategically delay foreclosure to avoid realizing losses on their assets until local economic activity and housing prices rebounded. That is, rather than foreclosing on delinquent homeowners immediately, many banks faced incentives to defer foreclosures until their local economy improved and the market value of properties had recovered (at least in part) to pre-recession levels. While we are not aware of formal academic literature on this point, there is a wide array of qualitative evidence articulated among policymakers and the press.\(^\text{14, 15}\) Amplifying this incentive is the fact that many mortgage 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. Thus, mortgage servicers would at times have an incentive to delay foreclosure in the hopes of receiving more payments on their second lien interests and in continuing to receive mortgage servicing fees.\(^\text{16}\) In fact, part of the Home Affordable Modification Program (HAMP) aid was specifically designed to help banks avoid recognizing their losses immediately, saving (according to some estimates) roughly over 435,000 distressed homes (Agarwal et al., 2017).\(^\text{17}\) Even setting aside the strategic rationale of banks, many courts, banks, and mortgage servicers were simply back-

\(^\text{13}\)Besides HAMP, four programs were included in the MHA program: (i) Principal Reduction Alternative, which helped homeowners with a loan-to-value (LTV) ratio over 115% make payments, (ii) Home Affordable Unemployment Program, which provided temporary relief for unemployed homeowners, (iii) Second Lien Modification Program, which helped bank servicers modify a second lien on a loan when a homeowner received a first lien modification through HAMP, and (iv) the Home Affordable Foreclosure Alternatives Program, which helped foreclosed homeowners transition from their old homes to new properties through a short sale or deed-in-lieu of foreclosure.


\(^\text{17}\)That HAMP was designed to help banks delay foreclosures was also a conclusion reached by the Special Inspector General of the TARP program. See http://billmoyers.com/content/book-excerpt-neil-barofskys-bailout/2/
logged with far too many foreclosures to process in a short time span. After most foreclosures occurred, enough time had passed that the economy was starting to improve.

2.3. Descriptive Evidence

There is significant dispersion in both well-being and foreclosures across states between 2008-2014. We begin by plotting the kernel density distributions of both current and expected future life satisfaction in Panel A of Figure 2. While individuals tend to be optimistic—thinking that their future will improve, relative to their present condition—we also see considerable heterogeneity across the average scores over each state. Turning towards Panel B of Figure 2, we see that there is massive heterogeneity in the mean foreclosures at a county-by-quarter level. In particular, there is a very large right tail of the distribution.

Our sentiment measures about the current and future state of the economy are asked with respect to the national economy. However, even though individuals are all asked about the same national economy, there is incredible variation in self-reported perceptions (Makridis, 2017; Das et al., 2017). One major reason, for example, is that individuals rely more on information from individuals similar to themselves (Ansolabehere et al., 2014), which often occurs at a local level because of sorting. We examine the evidence in two ways. First, Panel A of Figure 3 reports the distribution of coefficients obtained from regressing logged perceptions of the current state of the economy on monthly national housing price growth, controlling for individual covariates and both county and time fixed effects. Second, Panel B of Figure 3 shows the distribution of logged residualized perceptions about the current and future state of the economy averaged across metropolitan areas with at least 250 respondents between 2008-2011. In this sense, while our survey questions are about the national economy, they still contain significant heterogeneity.

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18 http://www.creditslips.org/creditslips/2012/11/where-are-the-foreclosures.html
19 Although CoreLogic does not cover the entire universe of the residential loan market, it goes cover roughly 80%. Our estimates of total loans is, therefore, an underestimate, but not by a vast amount.
20 We include all our controls used in our baseline specification: housing price growth, day of the week fixed effects, age, marital status, gender, education fixed effects, employment status, and local bank deposits and monthly payments due.
We now examine whether there is any descriptive evidence over a relationship between foreclosures and well-being in the cross-section. Figure 4 plots standardized measures of Gallup’s community well-being index with foreclosures at the state-level for 2014 only. We see, for example, that there is a strong negative correlation: a standard deviation rise in logged foreclosures is associated with a 0.218 standard deviation decline in logged community well-being. Practically, that is equivalent to moving a state that is at the 50th percentile in the distribution of logged community well-being and moving it to roughly the 40th percentile. While we do not interpret these pieces of evidence as causal, they allude to the main result that we shortly document.

[INSERT FIGURE 4]

3. Foreclosures and Reported Well-being

3.1. Identification and Dynamic Selection

Our primary focus is a statistical model that relates individual sentiments or well-being, indexed by \( i \), with zipcode foreclosures, indexed by \( j \), at a quarterly frequency, indexed by \( t \)

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y_{ijt} = \gamma f_{jt} + \beta D_{it} + g(h_{jt}, \theta) + \phi_j + \lambda_t + \epsilon_{ijt}
\]

where \( y \) denotes our outcome variable (e.g., logged current or future life satisfaction), \( f \) denotes logged foreclosures, \( D \) denotes a vector of individual covariates, \( g(h, \theta) \) denotes a semi-parametric vector of time-varying location controls, specifically zipcode housing price growth, logged employment, logged establishments, and \( \phi \) and \( \lambda \) are fixed effects on location (zipcode) and time (year and quarter). Our inclusion of logged employment or employment growth guarantees that the declines in well-being that we document are not driven by the adverse effects of foreclosures on labor market outcomes (Makridis and Ohlrogge, 2017). We follow Bertrand et al. (2004) in clustering our standard errors at the zipcode level, allowing errors to be arbitrarily correlated within a location. Location and time fixed effects remove time-invariant heterogeneity that make some areas systematically worse to live in (e.g., due to climate related reasons).

In practice, we control for zipcode housing price growth, but \( g(h, \theta) \) can be arbitrarily flexible.\textsuperscript{21} Controlling for housing prices may be important since foreclosures lead to declines in housing price

\textsuperscript{21}For example, we also experiment with partitioning housing price growth into 100 bins, which we introduced fixed effects over.
growth, which we later show affects individual well-being. However, what matters is whether our instrument is correlated with housing price growth. If so, then our predicted foreclosures will pick up, in part, the effect on housing prices. Using a regression discontinuity, Bhutta and Ringo (2017) show this is unlikely: the reduction in the FHA’s mortgage insurance premium (MIP) led to a rise in home purchases, but no decline in housing prices. Including housing price controls may be important for mitigating the dynamic selection problem that we discuss below—that banks delay foreclosure in areas where housing prices drop significantly since recognizing the market value of these properties would have adversely affected their balance sheets.

While one concern associated with Equation 1 could be the presence of reverse causality, in reality this is highly unlikely. Recent work from Bhutta et al. (forthcoming) and Gerardi et al. (forthcoming) highlights the fact that the main determinant of foreclosure during the crisis was the inability among consumers to pay their bill. While there is some psychological evidence that problems with emotional well-being precipitate foreclosure due to the fact that these problems tend to cause homeowners to become reclusive, engage in risky behavior, and/or delay payments, income shocks, especially lay offs (Tian et al., 2016), were the largest culprit. In this sense, changes in well-being cannot induce changes in foreclosure. Instead, we have two primary concerns.

The first main concern is that there are other time-varying shocks that are correlated with foreclosures and well-being. For example, credit market shocks induced by declines in housing values could constrain borrowing among households (Mondragon, 2015). One way we address this is by controlling for zipcode housing and labor market (e.g., employment and number of business establishments) shocks to avoid attributing variation in sentiments to local establishment closures or declines in housing equity. Since there could, in theory, still be other unobserved and time-varying zipcode economic phenomena that influence both foreclosures and well-being, we also draw on our instrumental variables strategy, described shortly.

The second main concern, however, is more serious. Bank accounting practices, and payment arrangements for mortgage servicers, created incentives for each type of entity to delay foreclosures in certain circumstances to minimize their balance sheet losses (Antoniades, 2015). Avoiding foreclosure (i.e., taking physical possession of the property) allowed banks to keep the

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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. http://www.realtytrac.com/news/home-prices-and-sales/banks-delay-foreclosures-and-pray-property-values-increase/
property in the “loans outstanding” category of their balance sheet; we refer to this as a “dynamic selection effect.” A related source of foreclosure delay was the massive influx of foreclosures in such a short window of time, which overwhelmed bank administrative systems, servicers, and local governmental authorities; we refer to this as a “backlogging effect”. In this sense, even if there are no unobserved and time-varying shocks to well-being that are correlated with foreclosures, estimating Equation 1 will still fail to account for the presence of backlogging and dynamic selection, which motivates our instrument in the section that follows.

We discuss the presence of foreclosure delay in our companion paper (Makridis and Ohlrogge, 2017). However, to quantitatively test whether worse local economic conditions create conditions that lead (through lender incentives and administrative backlog) to longer delays before foreclosure, we perform the following test. We consider mortgages that have already become seriously delinquent (90+ days delinquent) and predict how many months will elapse between this delinquency and eventual foreclosure. Specifically, for each county × quarter observation in our dataset, we consider all mortgages that become seriously delinquent and calculate the mean time (in months) between this delinquency and eventual foreclosure. We then regress this on employment growth measured in each county × quarter, producing a coefficient of -13.55 (p-value = 0.00). In other words, better (worse) economic conditions are associated with mortgages taking significantly less (longer) time to move from serious delinquency to foreclosure. While this quantitative test is clearly imperfect, it complements a reality that comes through loud and clear through the actions and statements of the federal reserve and banks.

Finally, even if we could randomly assign households to areas with more versus fewer foreclosures, one concern with Equation 1 is that it potentially confounds the direct and indirect effects of foreclosures. The direct effect, for example, is the fact that an individual who gets foreclosed will naturally experience a sudden decline in emotional, financial, and overall well-being. The indirect effect, for example, is the potential for an individual who has observed neighboring foreclosures to experience a decline in their well-being. While we unfortunately cannot match individuals between CoreLogic and Gallup, we argue, and present additional exercise to show, that our results are identifying the indirect effect of foreclosures on life satisfaction, which we believe is more interesting and novel. First, we conduct a bounding exercise that shows that even under extreme assumptions, the direct effect of individuals being foreclosed upon could only account for a very

\footnotesize{\textsuperscript{23}https://www.bloomberg.com/view/articles/2014-02-26/banks-prefer-losses-they-don-t-have-to-talk-about
https://www.federalreserve.gov/bankinfo/srletters/sr1210a1.pdf}
small portion of our measured effects. We also show that our results are robust to restricting the sample to areas with under 3% of homeowners in foreclosure. If we were only picking up individuals who were foreclosed upon, restricting our sample to areas with lower foreclosure shares would produce attenuated results. Second, we control for both consumption expenditures and income bracket fixed effects, which removes variation arising from pure income effects. We discuss these exercises later, but they are presented in Table 7 in Appendix Section A2.

3.2. Empirical Strategy

We draw on our companion work in Makridis and Ohlrogge (2017) by exploiting quasi-experimental variation in abrupt interest rate changes on 5-1, 7-1, and 10-1 adjustable rate mortgages (ARMs). We focus on these because they were not targeted towards subprime borrowers and because the borrowers are highly homogeneous with respect to their FICO scores. Many of these hybrid ARMs were initially offered to individuals with special “teaser” rates for an initial period. After the initial period, the interest rates on these loans would shift discontinuously up or down based on the change in the value of the reference interest rate index (e.g. Treasury rate or LIBOR) between the time of origination and reset.24

Before outlining the details of our estimation strategy and the discontinuity in foreclosure probabilities following interest rate shocks, we want to discuss the sources of variation. First, we leverage the timing of interest rate resets. For example, if one zipcode has more 5-1 relative to 7-1 ARMs, then it will experience interest rate resets sooner than the latter, thereby providing us with a treatment and control group. Appendix Section A1. provides descriptive evidence on the dispersion of ARMs across locations. Second, we leverage the intensity of ARMs in a given location. For example, if one zipcode has 5% of borrowers with ARMs, whereas another has 15%, then the interest rate resets in the latter will be more severe. Although one concern is that the variation in intensity is non-random, Kroszner and Strahan (1999) argue that much of the variation in bank lending practices emerged from political factors that prompted deregulation during the 1980s and 1990s. We can, however, exploit variation only in the timing of interest rate resets by controlling for the share of 5-1, 7-1, and 10-1 ARMs in a given location.

We now show that these interest rate shocks following a reset affect the probability of foreclosure (i.e., by changing the borrower’s monthly payment). Figure 5 non-parametrically plots

\[ \text{Gorton (2008) argues that these loans were designed to make it impossible for borrowers to afford payments after the reset date so that lenders could decide whether to refinance the loans or foreclosure on the property.} \]
the foreclosure probability by month since loan origination separately for different vintages of loans and ARMs. We also distinguish between instances where interest rates were reset up versus down given that increases versus decreases will have opposite effects on the foreclosure probability. 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. In contrast, the bottom right panel illustrates that there was a steep decline in foreclosure probabilities following the decline in the interest rate. In Appendix Section A1., we plot the corresponding interest rate movements for each loan type by their year of origination.

To leverage the discontinuities in foreclosure probabilities induced by changes in interest rate resets, we implement a simulated instrumental variables strategy whereby we first extract the variation in foreclosures emerging from these interest rate resets and subsequently aggregate them to a zipcode level as an instrument for realized foreclosures. Using our loan × month ARMs, we estimate the following logit regressions

$$f_{ict} = \alpha + \sum_k \gamma^k l^k_{it} + \zeta \Delta r_{it} + \sum_k \rho^k (l^k_{it} \times \Delta r_{it}) + \epsilon_{ict}$$  \hspace{1cm} (2)

where $l^k$ denotes an indicator on the $k$-th type of ARM (5-1, 7-1, and 10-1) and $\Delta r$ denotes the change in the interest rate induced by these resets. We specifically omit other characteristics and local controls so that we extract only the variation in foreclosures that is predicted from the quasi-random timing and intensity of these interest rate changes.$^{25}$ After fitting these regressions to 97 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 zipcode for each period during out study, denoted $Z_{jt}^{SIM} \equiv P(f_{jt})$. We use these predictions

$^{25}$One concern with this approach is that our predicted resets might be confounded by an individual’s decision to refinance their loan to avoid foreclosure. First, to the extent that individuals refinance rather than foreclosure, our first-stage correlation will simply be weaker. Second, as Fuster and Willen (2017) discuss, all of the interest rate resets that came due after 2008 were net resets down, meaning that borrowers did not have an incentive to refinance since they were getting a “better deal” by staying with their new rate. Third, Bucks and Pence (2008) use the Survey of Consumer Finances, showing that many borrowers with ARMs underestimate or do not know how much their interest rates are likely to change before the resets kick in.
to instrument actual foreclosures through 2SLS

\[
f_{jt} = \beta D_{it} + \pi Z_{jt}^{SIM} + g(h_{jt}, \theta) + \eta_j + \lambda_t + \epsilon_{ijt}
\]

\[
y_{ijt} = \beta D_{it} + \gamma \hat{f}_{jt} + g(h_{jt}, \theta) + \eta_j + \lambda_t + \epsilon_{ijt}
\]

where \( \hat{f}_{ct} \) denotes the predicted foreclosures based on the ARM resets from our reset instrument.\(^{26}\) 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 5).\(^{27}\) To guarantee that the timing of these discontinuous jumps is not driven by time-varying unobservables that co-move with employment (such as macro interest rates), we we control for quarterly mortgage payments over all loans (from the CoreLogic dataset), which removes the potentially mechanical effect of interest rate resets on disposable income (Di Maggio et al., 2017), which could affect search intensity (and thus employment) (Cohen-Cole et al., 2016).\(^{28}\)

One concern with with these interest rate resets is that the variation is inherently local to ARM borrowers, which are a minority of the sample. However, we show in Makridis and Ohlrogge (2017) that foreclosures on ARM borrowers generate spillovers into the broader FRM market for loans. Consistent with Agarwal et al. (2017) and Gupta (2016), we find that a 10% rise in foreclosures on ARM borrowers is associated with a large 5.8% rise in foreclosures on FRM borrowers in a zipcode. The elasticity is robust to controlling for county \( \times \) year fixed effects and zipcode employment and establishment counts. As a placebo, we also show that these spillovers are larger in zipcodes with greater proportions of ARM borrowers. These results suggest that ARM resets can set in motion a chain of events that make foreclosures on other loans more likely.

Our identification strategy is closely related with several recent contributions, including Gupta (2016) who examines the impact of foreclosures on housing price declines, as well as Fuster and

\(^{26}\)While our measure of sentiments (e.g., life satisfaction) in the second-stage of Equation 3 is a categorical variable ranging from zero to ten, we estimate a log-log model because the properties are well-known and the estimates have a common interpretation as an elasticity. In contrast, estimating negative binomial models with instrumental variables and fixed effects remains an active area of econometric study. We have also experimented with measuring foreclosures as a share, i.e., foreclosures per open mortgage, but doing so does not alter our qualitative results.

\(^{27}\)See Di Maggio et al. (2017) 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.

\(^{28}\)A potential concern remains that shocks to housing prices may affect the incentive to default. If this were true, we may also need to instrument for housing prices to overcome their simultaneous endogeneity. While we are already controlling for housing prices, Gerardi et al. (forthcoming) use the Panel Study of Income Dynamics (PSID) to show that there is only a limited scope for strategic default.
Willen (2017) who examine the impact of loan size on mortgage default and Di Maggio et al. (2017) who examine the impact of interest rate changes on consumption and voluntary deleveraging. Our paper (developed concurrently with these) also contains several novel features. First, we use the entire universe of CoreLogic data.\footnote{In contrast, Fuster and Willen (2017) use a sample of 221,000 loans from January 1 2005 to June 30 2006, Gupta (2016) and Di Maggio et al. (2017) both use the Blackbox sample which containing 22 million loans (Di Maggio et al. (2017) 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 over 17,000 zipcodes.} While these differences in sample size might seem innocuous, we have found that using only a subset of loans (even if randomly chosen) can create significant variability in causal estimates. Second, we implement a simulated instrumental variables approach that extracts the component of variation in realized foreclosures predicted by these ARM resets and, therefore, allows us to examine local (rather than just individual) outcomes.

While a valid concern is that banks strategically target counties with different types of loans in ways that are correlated with contemporaneous shocks to well-being, we discuss a wide array of diagnostics that we implement in Makridis and Ohlrogge (2017) to understand the reliability of our exclusion restriction. First, we show that the bulk of the variation in ARM dispersion is driven by historical variation in the formation of banks with different lending strategies in different areas. We find no correlation between the 2003-04 share of ARMs and various county economic shocks (e.g., income) between 1990-2000. Second, we show that borrowers with 5-1, 7-1, and 10-1 ARMs are homogeneous in their FICO scores, suggesting that borrowers are not self-selecting into different types of loans for reasons that are potentially correlated with beliefs about future economic growth. Third, we show that changes in the county income distribution are not systematically correlated with the share of ARMs. If banks were strategically targeting different areas, we would expect to see a pattern. Fourth, we show that, while the share of ARMs in year $t$ is correlated with employment growth, it is uncorrelated with other potential confounders, such as income or housing price growth in year $t + 5$. Even though individuals form beliefs about the future, they are not specifically forming beliefs of national interest rates and subsequently deciding to take out a 5-1 versus 7-1 ARM in ways that are correlated with contemporaneous well-being.
3.3. Main Results

Table 1 documents the estimates associated with Equation 3 under our instrumental variables specification with and without local housing price growth and employment as controls.\textsuperscript{30} Since foreclosures affect both housing and labor market outcomes (Makridis and Ohlrogge, 2017), our baseline specifications include them as controls. We interpret the gradient on logged foreclosures as a causal estimate under the orthogonality restriction that unobserved shocks to individual well-being are uncorrelated with pre-determined interest rate resets based on national lending conditions. We also estimate our foreclosure gradients separately by states with and without judicial foreclosure laws and with and without state recourse laws.

We begin by reporting the results in Table 1 associated with Equation 1 when the outcomes are measured as logarithms of our 10-point index of current and future life satisfaction. Starting with columns 1 and 7, we find that a 10% rise in foreclosures is associated with a 0.51% and 0.26% decline in current and future life satisfaction, respectively. The fact that future life satisfaction also declines suggests that foreclosures are picking up not only a decline in an individual’s attitude today, but also a belief that their life will be systematically worse in the future (i.e., five years from now). To address the potential concern that we are “over controlling”, we omit local housing price growth and our economic controls (employment and establishments) in columns 2 and 8, but our results are statistically indistinguishable from one another.\textsuperscript{31} In all our specifications our $F$-statistic from the first-stage is well above the rule of thumb (Stock and Yogo, 2005).

We next investigate whether the effects of foreclosures on well-being vary across states based on how state law dictates lenders proceed with foreclosure actions. We find that a 10% rise in foreclosures is associated with a 0.53% decline in current life satisfaction for states without judicial foreclosure laws (“NJUD”), but a 0.72% decline in those states that have the laws (“JUD”); corresponding magnitudes exist for future life satisfaction of 0.25% and 0.44%, respectively.\textsuperscript{31}

\textsuperscript{30}Simply as a reference, we find a least squares estimate of -0.005 ($p$-value = 0.00) when our outcome is logged current life satisfaction. As we discussed earlier, the naïve estimator is subject to endogeneity in both directions, so the resulting estimate could be an over or under estimate depending on the relative magnitudes of the two forms of bias.

\textsuperscript{31}Why are these two estimates so similar to one another? In auxiliary diagnostics, we have explored the role of zipcode and state housing and employment shocks measured as both logged levels and year-to-year growth rates. We find that, while individuals are responsive to state housing and employment shocks, they are less responsive to zipcode housing and employment shocks. One potential reason is that they are less salient events, relative to foreclosures. Given that our instrument is identified off of heterogeneity in the timing and intensity of interest rate resets, the invariance to the inclusion of these local controls is sensible since individuals may not notice or even be aware of zipcode housing price and employment changes immediately.
sistent with evidence about the costly and long drawn-out nature of foreclosure in some states (Pence, 2006), one reason they are larger in judicial foreclosure states is because foreclosure is a more salient and, therefore, significant process for local communities. We also find that a corresponding 10% rise in foreclosures is associated with a 0.42% decline in life satisfaction for states without recourse laws ("NREC"), but a 0.80% decline for states with the laws ("REC"). Not surprisingly, states with recourse laws will tend to have more intense foreclosure processes whereby banks attempt to seize the assets of the delinquent homeowner.

Although not reported, we also experimented with specifications containing metropolitan × year × quarter fixed effects, which exploit time-varying shocks to predicted foreclosures within-zipcodes at any given point in time. The identifying assumption without instruments—as estimated Currie and Tekin (2015), for example—is that zipcodes vary over time in their shares of foreclosures for largely idiosyncratic reasons (with respect to well-being) after controlling for all shocks common to the metropolitan area over a year. However, other time-varying unobserved zipcode-specific factors may still remain. Therefore, using our baseline instrumental variables specification, we recover a coefficient of -0.082 (p-value = 0.00), which is slightly larger than our previous baseline. One reason for this is that likely time-varying unobserved shocks that jointly affect interest rate resets and well-being are likely to produce upwards bias. For example, an increase in historical zipcode per capita income growth that could be correlated with future life satisfaction may have led to more ARM loan originations and, therefore, resets.

[INSERT TABLE 1]

As a point of comparison, we also examine how these foreclosure gradients relate with housing price growth gradients. In other words, given that the run-up in housing may have led to a rise in life satisfaction indices, how much did the rise in subsequent foreclosures depress life satisfaction back to trend? Table 2 documents these results by regressing logged current life satisfaction on county housing price growth, conditional on the usual controls. However, to deal with the potential for endogeneity, we also instrument contemporaneous housing price growth between 2008 and 2014 with historical housing price growth between 2000 and 2005 within a county.

Column 1 reports the least squares estimator conditional on controls, suggesting that a one

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32We also experiment with the Saiz (2010) elasticity, but it produced an upwards biased estimate of 0.44 likely because we did not also introduce zipcode controls. However, demographic zipcode data from census is only available over five-year intervals, which prevents us from exploiting all of the time variation. We also find no statistically significant positive gradient between housing price growth and perceptions of future life satisfaction in the cross-section using either OLS or our IV approach.
percentage point rise in housing prices is associated with a 0.133% rise in life satisfaction. While our gradient declines marginally in magnitude with our historical housing price growth instrument in column 2, it declines further to an imprecise 0.097% in column 3 once we add in our long-difference controls that purge potentially correlated omitted variables, like the growth in the share of college workers between 2000 and 2007. Column 4 simply uses location and time fixed effects, producing a statistically precise gradient of 0.070. From these diagnostics, we conclude that, while housing prices led to a large uptick in local well-being, the subsequent surge in foreclosures effectively offset most of the gains from the housing boom.

While we will examine several robustness exercises later, we discuss two important concerns about the interpretation of our results thus far. First, these foreclosure gradients might simply reflect the mechanical effect that an individual is foreclosed upon and experiences a decline in self-reported well-being following foreclosure. However, since foreclosures are infrequent—the average zipcode share of homeowners in our data in foreclosure is 1% between 2008 and 2011—the mechanical effect would have to be implausibly large to account for our observed elasticities.33

Second, these foreclosure gradients might not reflect actual foreclosures, but rather variation in disposable income based on interest rate resets. In particular, since interest rate resets following 2008 were effectively all decreasing, thereby making monthly payments easier to make, interest rate resets raised disposable income and may have influenced individual well-being simply through allowing homeowners to finance additional consumption (Di Maggio et al., 2017). Given the quality of our data, we can explicitly control for, in addition to zipcode monthly mortgage payments, individual non-durable consumption expenditures and a coarse measure of income; these results are presented in Table 7 in Appendix Section A2, and our baseline estimates are unaffected.

We interpret the negative association between foreclosures and life satisfaction as evidence of spillovers and, more generally, the pass-through of risk onto households: even if an individual is not

33Under the “worst case” scenario we can assume that individuals who experience foreclosure move from a life satisfaction index level of seven to one, or a decline of 85%. Almost certainly, those who get foreclosed upon would start out below average satisfaction and probably would not drop completely to the bottom of the scale, so this is quite conservative. We also assume that 75% of those surveyed are homeowners, which is a conservative estimate that well exceeds the U.S. national home ownership rate which peaked at 69.2% in 2004. Thus, an increase in foreclosures per open mortgage from 1% to 2% would mean an additional 7.5 people out of the 1000 surveyed would be in foreclosure. If all 7.5 of those people went from a score of 7 to 1, it would mean a net reduction in geographic average satisfaction of 45 points attributable to the direct effect. Even in this extreme scenario, 45 out of the 371 point reduction we observe in life satisfaction (12% of the total decline) could be attributed to the direct effect of foreclosures on well-being.
foreclosed on directly, foreclosures among neighbors can indirectly affect their well-being. These spillovers are consistent with two classes of models. The first class are built around external habit formation (Pollak, 1970) and positional externalities. Especially since houses are a conspicuous and large consumption good for homeowners (Heffetz, 2011), when a zipcode experiences a surge in foreclosures, then the decline in neighborhood quality can lower its residents positional status and lead to a corresponding decline in perceived life satisfaction. The second class are built around risk sharing in social networks (Ambrus et al., 2014). Since connections among individuals creates social collateral to help enforce informal insurance payments, a common local shock makes it more difficult for neighbors to help one another. While these mechanisms are easy to see in developing countries (e.g., Fafchamps and Lund (2003)), which often lack efficient financing channels, social networks also play an important role in developed economies (Fafchamps, 2008).

3.4. Heterogeneity

While the media and some others have frequently pointed towards the tendency for low income and minority individuals to have been targeted by predatory loan practices (Calem et al., 2004; Bocian et al., 2008; Squires, 2008; Rugh and Massey, 2010), it is not necessarily the case that they will experience a much stronger foreclosure gradient for at least two reasons.

First, recent evidence from Adelino et al. (2016) shows that many middle income earners also took out subprime loans. Our data covers the entire universe of foreclosures that took place within CoreLogic’s records, guaranteeing that we are not looking at only a small subset of loans. Second, many of these prior sociological studies do not address selection problems—the fact that these individuals have lower income are inherently more likely to default. Our instrumental variables strategy addresses this selection problem by exploiting plausibly exogenous variation in the reset times of these different ARMs.

Nonetheless, we now examine the potential for heterogeneous treatment effects across several margins: education (college attainment), race (black), age (less than 40 years old), and gender (male). We estimate variants of Equation 1 instrumenting for logged foreclosures and its inter-

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34Positional externalities also have implications for public policy; see, for example, Frank (2008) who discusses how a more progressive income tax could address these challenges. While one may argue that, because everyone in a zipcode is affected similarly by foreclosure shocks, that their relative happiness should not decline, what matters is the reference case. Since individuals interact with others in a broader county or metropolitan area, a foreclosure in one zipcode could lead to a relatively lower status when compared with another individual at work who lives in another zipcode. For evidence on happiness and reference income using German data, see Ferrer-i Carbonell (2005).
action with our demographic indicator using our standard interest rate reset instruments also interacted with our demographic indicator.

Table 3 documents these results. Generally speaking, while we find statistically significant sources of heterogeneity, their magnitudes are quite small, relative to the direct effect between foreclosures and life satisfaction. However, our results point towards several interesting findings. First, while individuals with college degree have 5.1% higher life satisfaction than their counterparts, they have a stronger foreclosure gradient. In particular, a 10% rise in foreclosures is associated with a 0.052% decline in life satisfaction with an additional 1.5% decline among college degree workers. One reason for the statistically significant and large gradient comes from the fact that college degree workers tend to live in areas with higher amenities (Diamond, 2016), which means the margin for negative shocks during the recession was larger. Second, we find no statistically significant interaction between being black and foreclosures. To the extent that one group has a larger response to foreclosures than another, then we would expect to find a large negative gradient in the interaction. However, through supplementary exercises where we partition our sample based on counties based on the share of individuals who are black, we do find a larger negative gradient. We also find a positive, but small, interaction effect for young (under 40) individuals, which is consistent with the fact that their mobility costs are likely low. We find no significant effect for males.

[INSERT TABLE 3]

3.5. Exclusion Restriction

While we refer readers to our companion paper for a full examination of the exclusion restriction (Makridis and Ohlrogge, 2017), we summarize them here briefly and subsequently turn towards an additional direct test. The motivating concern behind the diagnostics that follow is that banks strategically target counties with different types of loans in ways that are correlated with contemporaneous shocks to individual well-being. However, because we are controlling for both zipcode housing and labor market conditions, we are confident that we are capturing responses only to these idiosyncratic fluctuations in foreclosures predicted by our interest rate shocks.

First, we show that the bulk of the variation in ARM dispersion is driven by historical variation in the formation of banks with different lending strategies in different areas. For example, there is no correlation between the 2003-04 share of ARMs and various county economic shocks (e.g.,
income) between 1990-2000. Second, we show that borrowers with 5-1, 7-1, and 10-1 ARMs are homogeneous in their FICO scores. While 2-1 and 3-1 borrowers tend to have very low credit scores, these latter borrowers are much more similar (at least along the FICO score dimension), suggesting that they are not self-selecting into different types of loans for reasons that are potentially correlated with beliefs about future economic growth. Third, we show that changes in the county income distribution are not systematically correlated with the share of ARMs. If banks were strategically targeting different areas, we would expect to see a pattern.

We now exploit an alternative source of variation by constructing a Bartik-like instrument that interacts a county’s pre-recession exposure to a particular bank with the national interest rate resets that the bank was experiencing at any given point in time. We fix the base year to 2006 and compute a county’s exposure by taking the ratio of bank $i$’s 2006 loan volume in county $c$, denoted $v_{i,c,2006}$, and county $c$’s 2006 loan volume, denoted $v_{c,2006}$. Given the county exposure in 2006, then the Bartik-like instrument is given by

$$Z_{ct}^{BARTIK} = \sum_i \left[ \left( \frac{v_{i,c,2006}}{v_{c,2006}} \right) \times \Delta r_{it} \right]$$

(4)

where $\Delta r_{it}$ denotes bank $i$’s average interest rate resets in their national portfolio. Equation 4, therefore, identifies the causal effect of foreclosures on individual well-being by exploiting plausibly exogenous variation in a county’s predicted exposure to banks that were more versus less likely to experience interest rate resets from 2008 onward. Our approach shares many features of the identification strategies in Agarwal et al. (2017) and Mondragon (2015).

These results are documented in Table 4. Column 1 replicates our main results using our simulated instrumental variables strategy simply as a convenient reference case; our estimated gradient is lower in magnitude because we are working here at the county-level, rather than zipcode-level, so foreclosure spillovers are smaller through the aggregation. Column 2 subsequently uses our baseline formulation of the Bartik instrument in Equation 4 by defining a county’s exposure to a bank based on total bank loans, rather than just their ARM loans. We find that a 10% rise in foreclosures is associated with a 0.90% decline in current life satisfaction. Since our first-stage $F$-statistic is slightly above the benchmark rule of thumb (Stock and Yogo, 2005), we also formulate a variant of the instrument using the county’s exposure to a bank’s ARM loan volume. While estimated our magnitude declines, it is statistically indistinguishable from our simulated IV estimates. The fact that our baseline and Bartik-like instruments produce nearly identical
estimates obviates the concern that our estimates are driven by time-varying contemporaneous shocks to well-being that are simply correlated with the interest rate resets that are generating predictable variation in foreclosures.

[INSERT TABLE 4]

### 3.6. Relation to Current Work

The rise of “big data” has helped make possible a series of conventionally infeasible quasi-experimental exercises. Our statistical exercises are based on observing 97 million loans, which covers the bulk of the residential housing market, which we have linked at the zipcode-level to individuals with an array of well-being measures from the U.S. Daily Poll. Although we recognize self-reported measures of well-being as potentially noisy indicators of actual welfare, they still convey important information (Krueger and Schkade, 2008) and help provide greater understanding about the effects of macroeconomic shocks on welfare (di Tella et al., 2001) and the marginal rates of substitution across different goods (Benjamin et al., 2012, 2014b).

We extend the application of social well-being indices into the mortgage market to understand how local foreclosure shocks affect individuals’ current and expected future life satisfaction and why. While there has been some recent work on how foreclosures affect the real economy through their effects on housing prices (Mian et al., 2015; Gupta, 2016), this paper provides causal evidence that local foreclosures can generate spillovers on those who are not foreclosed upon. While we view this result as important on its own—since well-being is an integral metric of aggregate welfare (Frey and Stutzer, 2002) and for understanding the persistence of income shocks (Bayer and Juessen, 2015)—our results are also closely related with microeconomic evidence on the mortality effects of unexpected shocks. For example, Sullivan and Wachter (2009) document a rise in mortality following an unemployment spell and Currie and Tekin (2015) document a rise in mortality following a rise in zipcode foreclosures. Our results point towards a common mechanism behind the mortality effects of financial shocks: they induce persistent declines in physical and emotional well-being, often triggering a decline in optimism and aspirations about the future. Given that worry and uncertainty about the future are important determinants of stated well-being (Makridis, 2017), these unanticipated local shocks can raise stress hormones, thereby affecting blood pressure and cardiovascular health (McEwen, 1998a,b) and physical and mental illnesses (Goldberger and Breznitz, 1993; Schneiderman et al., 2005).
The evidence of spillovers that we have documented also connects with a broader-literature about the pass-through of risk onto households and the role of partial insurance against aggregate risk (Blundell et al., 2008; Heathcote et al., 2014). While this literature has largely relied on consumption to identify the cost of partial insurance on households, our data enables us to conduct an additional exercise. In Appendix Section A3., we combine data from the Consumption Expenditure Survey with our Gallup data to produce a synthetic panel that enables us to test how labor market shocks potentially affect well-being, even after controlling for consumption. After computing growth rates of consumption, life satisfaction, and wage income, we show that there is considerable variation in life satisfaction growth in response to wage income growth, conditional on consumption growth; see Figure 12. These results, therefore, provide guidance on the ways through which macroeconomic shocks affect individual well-being through channels other than simply consumption.

4. Understanding the Mechanisms

4.1. Foreclosures Depress Local Amenities

One way foreclosures can influence current and expected future life satisfaction is by altering the quality of local amenities, which enter into individuals’ preferences by shaping the marginal utility of leisure and/or consumption activities. For example, an individual who enjoys going to the park might be less willing if the visual appeal of the neighborhood changes and/or crime rates rise following foreclosure.35

While Gallup’s U.S. Daily poll has several good measures of these amenities, they are not available throughout our entire sample, in particular when foreclosures reached their peak in 2009-2010. We, therefore, begin by estimating probit regressions of an indicator for whether an individual is satisfied with their city and perceives that it is safe to walk through the city on zipcode logged foreclosures (zipcode), logged employment and establishments, housing price growth, and the usual individual covariates.36

35Previous research has documented an increase in crime following foreclosure, but only in specific geographic areas (Cui and Walsh, 2015). Our companion work illustrated this throughout the entire United States, obtaining a gradient that was strongest in low and middle income communities (Makridis and Ohlrogge, 2017).

36The variables are in response to questions along the lines of: (i) “Overall, I am satisfied with this city”, and (ii) “I feel safe walking alone”. Unfortunately, we do not have enough power to implement detailed fixed effects regressions in this setting.
Table 5 documents the marginal effects associated with our three main regressors of interest: logged foreclosures, housing price growth, and employment growth. We find that a 1% rise in foreclosures is associated with a 0.02 percentage point decline in the probability that an individual reports being satisfied with their city and a 0.046pp decline in the probability that an individual feels safe walking outside at night. As a point of comparison, these gradients are weakly greater than the marginal effects of employment growth; a one pp rise in employment growth is associated with a 0.019pp and 0.016pp rise in the corresponding probabilities. Similarly, we find that a one pp rise in housing price growth is associated with roughly a four times larger increase in city satisfaction—reflecting the fact that higher property values lead to higher property tax revenue, which is partially converted into city amenities—but not associated with an increase in city safety. While these are only conditional correlations, they highlight that foreclosures are a salient phenomenon within neighborhoods and have a statistically and economically meaningful effect on perception of city amenities.

[INSERT TABLE 5]

Starting in 2014, we have a much more granular index of neighborhood quality—a community well-being index—which draws on respondent answers on several questions: (i) “I can’t imagine living in a better community than the one I live in today”, (ii) “Are you satisfied or dissatisfied with the city or area where you live”, (iii) “The city or area where I live is a perfect place for me”, (iv) “I am proud of my community”, (v) “I always feel safe and secure”, (vi) “The house or apartment that I live in is ideal for me and my family”, and (vii) “In the last 12 months, I received recognition for helping to improve the city or area where I live”. Based on respondent answers, Gallup produces a 100-point index. The mean is 62.44 and the standard deviation is 19.74. Here, we also find a negative association with foreclosures: a 10% rise in foreclosures is associated with a 0.64% decline in community well-being. While we also find smaller gradients on other well-being indices (e.g., social), we do not interpret these gradients as causal due to the lack of time series variation, which prevents us from applying our baseline instrumental variables strategy.

What might induce these declines in neighborhood quality? Crime is one such reason. 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) find, using data from Pittsburgh and New York City, that foreclosures affects crime only when a property stays vacant (versus if a new tenant moves in). Foreclosures might also raise other types
of dis-amenities. For example, foreclosures have been linked with declines in property tax revenues (Alm et al., 2014), driven by the fact that foreclosures amplify housing price declines. Since local and state tax revenues are allocated towards public infrastructure and community development, declines in revenues can reduce various non-market amenities in cities.

4.2. Foreclosures Depress Sentiment

We now turn towards an alternative explanation of the decline in life satisfaction. A recent vein of macroeconomic models has began embedding unemployment risk into self-fulfilling business cycles (Heathcote and Perri, 2016; Ravin and Sterk, 2017). In these papers, an important mechanism the way in which large financial shocks generate greater uncertainty and/or worry among individuals. Since worry and life satisfaction are closely linked (Makridis, 2017), local foreclosures can lower well-being by raising perceptions risk and/or uncertainty about the future even if an individual is not directly foreclosed upon. Before delving into our results, we briefly discuss our measurement of sentiment in greater detail.

We primarily draw on Gallup’s questions about an individual’s perception about the current state of the economy, which they can rank on a scale from one to four, and about the future state of the economy, which they can rank on a scale of one to three. While these measures are somewhat coarse, they contain considerable variation. For example, Figure 13 in Appendix Section A4. shows that there is massive variation in perceptions of current and future economic states across locations in the same year, suggesting that individuals may be responding more towards local, rather than national, information. Figures 14 and 15 in Appendix Section A4. also compare the Gallup measure of sentiments with the volatility index and economic policy uncertainty index from Baker et al. (2016).

Having validated our main measure of uncertainty, we now examine whether the surge in foreclosures potentially drove declines in sentiment and increases in uncertainty. We measure sentiment at the individual-level using both perceptions current and future economic activity; we measure uncertainty at the county-level by taking the dispersion of beliefs across all individuals.

\(^{37}\) However, Lutz et al. (2010) find that property tax revenues do not decline in response to housing price declines. One difference between these results is likely the fact that Alm et al. (2014) focus on the contribution of foreclosures to property tax declines, whereas Lutz et al. (2010) just focus on housing price declines. Pooling foreclosed versus non-foreclosed properties may generate heterogeneous treatment effects.

\(^{38}\) Although show that our sentiment measure corresponds to uncertainty, these results are also consistent with models of ambiguity aversion where investment declines (Ilut and Schneider, 2014).
in a county $\times$ year. We use the same identification strategy as before, but now include 2-1 and 3-1 ARMs in our simulated instrument to increase the strength of our first-stage. We continue controlling for mortgage payments, logged housing prices, and bank deposits, on top of our usual individual covariates.

Table 6 documents these results. We begin by focusing on Panel A, which reports the estimated coefficients when the outcome variable is individual logged perception of the current or future state of the economy. We find that a 10% rise in foreclosures is associated with a 1% (column 1) and 1.5% (column 3) decline in the perception of the current and future state of the economy, respectively. Housing prices enter negatively since they tend to raise rents, which reduces individual disposable incomes. Similarly, mortgage payments due enter negatively for the same reason. Bank deposits, however, enter positively.

One concern with these results, however, is that the presence of income effects induced by the ARM interest rate resets in our instrument could simultaneously affect sentiment. For example, if an individual experiences a spike in their interest rate, thereby lowering their disposable income, then the individual might not be able to purchase as much consumption, thereby reducing their mood and perception of the economy. Columns 2 and 4 control directly for income effects by introducing logged individual income and non-durable consumption expenditures as controls. In fact, our estimated gradients rise to 1.5% and 2.5% for a corresponding 10% rise in foreclosures. The fact that individual perceptions about the future state of the economy change (not just the current state) is consistent with the claim that banks and other firms update their expectations about local economic development in response to foreclosures. To further rule out the possibility that there are other confounding factors correlated with sentiments, we exploit only very local variation by controlling for county $\times$ quarter $\times$ year fixed effects.\footnote{Under this specification, we find that a 10% rise in foreclosures is associated with a 0.67% ($p$-value = 0.00) decline in sentiment coefficient about the current state of the economy. We also find a corresponding 0.1% decline in perception of the future state of the economy, but our estimate is not statistically significant at conventional levels since this index has less variation—it can only take on one of three values.}

We now turn to Panel B, which reports the estimated coefficients when the outcome variable is the standard deviation of logged sentiments within a county $\times$ quarter $\times$ year. We find that a 10% rise in foreclosures is associated with a 0.2% and 0.5% rise in the standard deviation of the current and future state of the economy, respectively. Unlike our estimates from Panel A, our theory here is that foreclosures raise uncertainty, which we proxy by taking the dispersion of beliefs within a local area. The fact that our estimates are stronger for perceptions of the future
(versus perceptions of the current state) are further evidence that we are identifying uncertainty, not just level shocks. We also deal with income effects by introducing logged county adjustable gross income (AGI) as a control in columns 2 and 4. Perhaps surprisingly, the inclusion of county income does not alter our results, reflecting the fact that the increase in dispersion of beliefs is due not to changes in income, but rather the salient fluctuations in foreclosures.

[INSERT TABLE 6]

We now turn towards one possible reason behind the spike in uncertainty by introducing a measurement of unemployment risk. Respondents answer whether they have enough money to meet their basic needs. Using this indicator variable, we now estimate linear probability models of the indicator regressed on logged foreclosures and other controls. Figure 6 plots the estimated coefficients by income bracket. We find the largest effect in the middle, especially for those earning between $1,500-6,499 per month. Unfortunately, we do not have income data on all individuals in our sample, and those earning less than $1450/month are especially sparsely populated bins in our data, so our estimated confidence intervals are large. However, the fact that the estimated gradients are relatively negative for all groups except the top income bracket suggests that foreclosure shocks have an important effect on individuals’ perception of unemployment risk. One possible reason is that middle income individuals between $1,500-6,499/month earners tend to own homes, but may also face liquidity constraints, meaning that they are more likely to adjust their expectations in response to foreclosure shocks.

[INSERT FIGURE 6]

While our primary goal here was to highlight how foreclosures produce local spillovers that affect well-being—potentially by shaping individuals’ worry about current and/or future economic activity—heightened uncertainty and lower sentiment are also potential mechanisms behind the rise in mortality following foreclosure shocks documented by Currie and Tekin (2015). For example, unanticipated spikes in uncertainty can raise stress hormones, thereby affecting blood pressure and cardiovascular health (McEwen, 1998a,b) and physical and mental illnesses (Goldberger and Breznitz, 1993; Schneiderman et al., 2005). Gallup’s survey design enables us to test the mechanism more precisely through the following question: “During the past 30 days, for about how many days did poor health keep you from doing your usual activities?” Using our baseline strategy, we regress the logged number of days with health problems on logged foreclosures and find that a 10% rise in the number of foreclosures is associated with 0.496% fewer days of good health (p-value =
0.00.\textsuperscript{40} We, therefore, interpret the rise of foreclosures as one potentially important mechanism behind the rise of mortality following zipcode foreclosures in Currie and Tekin (2015).

5. Conclusion

While there is now clear evidence that labor and housing market shocks have persistent effects on individual welfare through heightened consumption volatility, we provide the first microeconomic evidence that local (zipcode) foreclosure shocks can have large adverse effects on well-being even among individuals not directly foreclosed upon. Importantly, these results hold even after controlling for income, consumption, and other local housing and/or labor market shocks, suggesting that foreclosures may have had destabilizing effects for reasons beyond the pass-through of risk onto consumption. Using the Great Recession as a natural experiment, we use new and proprietary micro-data from Gallup’s U.S. Daily poll matched with mortgage data across zipcodes between 2008 and 2014 to quantify how and why the surge in foreclosures affected individual well-being.

Although the quality of our data enables us to introduce many granular controls, many banks had an incentive to delay foreclosing on homeowners until local housing values returned to trend so that they could avoid recognizing underwater homes on their balance sheets. To deal with strategic delay and additional omitted variables problems, we exploit plausibly exogenous variation in the timing and intensity of abrupt interest rate changes on adjustable rate mortgages (ARMs) across zipcodes. The fact that individuals across zipcodes originated their loans at different points in time and at different contract terms based on the banks in their area produces significant variation in the interest rate resets on ARM loans. We show that these abrupt interest rate changes induce discontinuities in the probability that an individual enters into foreclosure since their monthly payment can become more or less expensive depending on the direction of the reset.

The first part of the paper shows that local foreclosures have a causal effect on individual well-being: a 10% rise in foreclosures is associated with a 0.51% and 0.26% decline in current and expected future life satisfaction. We find that these foreclosure gradients are nearly twice as strong in magnitude in states without judicial foreclosure laws and with recourse laws. These estimates are robust to controlling for individual income and consumption expenditures, implying

\textsuperscript{40}We also control for logged life satisfaction since foreclosures affect life satisfaction, which in turn is associated with health problems. We do not take a stance on the mechanism through which life satisfaction affects health, which could arise from a causal channel (e.g., mood affecting physical well-being), a selection channel (e.g., lower health individuals also have lower life satisfaction), or a reverse causality channel (e.g., worse health reduces life satisfaction).
that the observed declines in well-being are not driven by the potential direct effects of interest rate resets on disposable income. These results are consistent with models of external habit formation whereby individual utility depends in part based on the outcomes of peers and social insurance networks whereby members of communities provide forms of partial insurance for each other.

The second part of the paper provides two plausible mechanisms behind these observed declines in well-being. First, we show that foreclosures lead to declines in local amenities. To the extent that individuals value these amenities, such as parks or neighborhood quality, local foreclosures will affect their utility flow by eroding the quality of these amenities (e.g., by raising crime or by reducing the visual appeal of neighborhoods). Second, we show that foreclosures not only reduce individuals’ perception about the current and future state of economic activity, but also raise the dispersion of beliefs about the current and future states (even after controlling for the first moment). These results are robust to controlling for income, suggesting that the decline in sentiment is not mechanically driven by movements in disposable income. We also show that heightened individual uncertainty about the future is associated with more health problems.

Our results provide microeconomic evidence that local shocks can generate significant spillovers that affect individual well-being even after controlling for consumption. While consumption is an important metric for evaluating the pass-through of housing wealth (Campbell and Cocco, 2007; Mian and Sufi, 2011; Berger et al., 2017) or labor income (Attanasio and Davis, 1996; Blundell et al., 2008; Kaplan and Violante, 2010) risk onto households, it is not a sufficient statistic. We also provide evidence that beliefs about economic activity matter and are informed by local information. As recent household finance contributions from Bailey et al. (forthcoming) and leverage Bailey et al. (2017) emphasize, beliefs play an important role in influencing expectations and real activity, especially in the housing market where individuals make decisions about financing their mortgages. Overall, our results suggest that allowing for spillovers and/or externalities emerging from local shocks is important for quantitative macroeconomic models of the housing market.

References


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

Notes.– Sources: Mortgage Bankers Association. The figure plots seasonally adjusted foreclosures that were started as a share of open mortgages. The time series shows the rapid run up in the fraction of mortgage loans that went into foreclosure during the financial crisis.

Figure 2: Dispersion in Life Satisfaction and Foreclosures, state/year

Notes.– Source: U.S. Daily from Gallup and CoreLogic, 2008-2014. The left panel plots the average current and future life satisfaction for each state-year combination. The right panel plots the average foreclosures for each county-quarter combination. Current and future life satisfaction are measured based on the answers to the following questions: (i) “Please imagine a ladder with steps numbered from zero at the bottom to ten at the top. The top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you. On which step of the ladder would you say you personally feel you stand at this time?” [0-10 index]; (ii) “On which step do you think you will stand about five years from now?” [0-10 index]. The figures both show considerable heterogeneity in the distribution of well-being and foreclosures.
### Table 1: The Effects of Foreclosures on Current and Future Life Satisfaction

<table>
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<th>ALL</th>
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<th>JUD</th>
<th>NREC</th>
<th>REC</th>
<th>ALL</th>
<th>ALL</th>
<th>NJUD</th>
<th>JUD</th>
<th>NREC</th>
<th>REC</th>
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<td>-.053***</td>
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<td>.06</td>
<td>.06</td>
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<td>Yes</td>
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<td>Yes</td>
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<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Extra Controls</td>
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<td>Yes</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<td>Yes</td>
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Notes. Sources: CoreLogic, U.S. Daily from Gallup, Zipcode Business Patterns, 2008-2014. The table reports the coefficients associated with regressions of logged current and expected future life satisfaction (from an index of 1 to 10) on zipcode logged foreclosures, conditional on controls. “Controls” refers to individual covariates, including: day of the week fixed effects on the interview, age, marital status, gender, and education fixed effects. “Extra Controls” refers to other zipcode covariates, including: logged zipcode employment, logged establishments, logged total payments due (on mortgages across the county-quarter), and housing price growth. “NJUD” and “JUD” denote non-judicial and judicial foreclosure states, which are laws governing the foreclosure process. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the zipcode-level and observations are weighted by the sample weights.
Panel A: Distribution of HPI Gradients

Panel B: Distribution of Sentiments

Figure 3: Heterogeneity in Individual Sentiments

Notes.– Source: U.S. Daily from Gallup, Federal Housing Administration Monthly series. Panel A plots the distribution of regression coefficients from regressions of logged perceptions about the current and future state of the economy on national housing price growth (year-to-year at a monthly frequency), conditional on controls, including: day of the week fixed effects, age, marital status, education fixed effects. The FHA purchase-only housing price index is measured using sales price data. Panel B plots the distribution of residualized logged perceptions about the current and future state of the economy where housing price growth, income bracket fixed effects, day of the week fixed effects, age, marital status, education fixed effects, local bank deposits and monthly payments, and employment status are included controls. Our two questions used to measure the current and future state are given by: (i) “How would you rate economic conditions in this country today: as excellent, good, only fair, or poor?” [1-4 index]; (ii) “Right now, do you think that economic conditions in this country, as a whole, are getting better or getting worse?” [1-3 index].

Table 2: Comparison of Effects of Housing Price Growth on Current Life Satisfaction

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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
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<td>∆ ln(housing prices)</td>
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<td>.097</td>
<td>.070***</td>
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<td>[.011]</td>
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</table>

Notes.– Sources: CoreLogic, U.S. Daily from Gallup, Census Bureau, 2008-2014. The table reports the coefficients associated with regressions of logged current life satisfaction (from an index of 1 to 10) on county housing price growth, zipcode logged bank deposits, zipcode monthly loan payments, conditional on individual controls, including: day of the week fixed effects on the interview, age, marital status, gender, and education fixed effects. The long-difference controls include the growth in the college share, ages between 0-33 years old, race (white and black), marital status, and population from 2000 to 2007 at a county level. Contemporaneous housing price growth is instrumented using historical 2000 to 2007 housing price growth. Standard errors are clustered at the county-level and observations are weighted by the sample weights.
Figure 4: Foreclosures and Community Well-being, by State

Notes.– Source: U.S. Daily from Gallup and CoreLogic, 2014. The figure plots standardized logged foreclosures (total across all counties in each state) with standardized logged community well-being (averaged across all individuals in the state). The community index is a 100-point scale based on responses to the following questions: (i) “I can’t imagine living in a better community than the one I live in today”, (ii) “Are you satisfied or dissatisfied with the city or area where you live”, (iii) “The city or area where I live is a perfect place for me”, (iv) “I am proud of my community”, (v) “I always feel safe and secure”, (vi) “The house or apartment that I live in is ideal for me and my family”, and (vii) “In the last 12 months, I received recognition for helping to improve the city or area where I live.”
Figure 5: Interest Rate Spikes and Foreclosure Probabilities, by Vintage & ARM

Notes. Source: CoreLogic. The figures plot, for different vintages of loans and adjustable rate mortgages, the non-parametric probabilities of foreclosure for each month since the origination period. Each observation is the share of individuals who were foreclosed upon corresponding to the month following origination. Reset up refers to increases in interest rates, while reset down refers to decreases in interest rates.
### Table 3: Heterogeneity in the Effects of Foreclosures on Current Life Satisfaction

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<td>[.001]</td>
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<td>× male</td>
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<td>Yes</td>
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</table>

Notes: Sources: CoreLogic and U.S. Daily from Gallup, 2008-2014. The table reports the coefficients associated with regressions of logged of current life satisfaction (from an index of 1 to 10) on logged foreclosures, demographic indicators, their interactions, and other controls, including county unemployment, logged total payments due (on mortgages across the county-quarter), logged housing prices, day of the week fixed effects on the interview, and marital status. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the zipcode-level and observations are weighted by the sample weights.
Table 4: Robustness of Foreclosure Shocks and Life Satisfaction Using a Bartik Instrument

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<td>1600328</td>
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<td>Time FE</td>
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<tr>
<td>Bartik IV</td>
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Notes.—Sources: CoreLogic and U.S. Daily from Gallup, 2008-2014. The table reports the coefficients associated with regressions of logged of current life satisfaction (from an index of 1 to 10) on logged foreclosures, and controls, including zipcode logged employment, zipcode logged total payments due (on mortgages across the county-quarter), zipcode housing price growth, day of the week fixed effects on the individual’s interview, marital status, age, education fixed effects, gender, and race. Our simulated instrument is computed by taking 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, subsequently aggregated to the zipcode × quarter frequency. Our Bartik instrument is computed by taking the ratio of bank i’s 2006 loan volume in county c and county c’s 2006 loan volume, subsequently taking the sum over the product of the ratio and bank i’s national average interest rate resets. Column 2 measures loan volume based on all loans, whereas column 3 measures it based on 5-1, 7-1, and 10-1 ARM loans. Standard errors are clustered at the zipcode-level and observations are weighted by the sample weights.

Table 5: The Marginal Effects of Foreclosures on City Perceptions (Probit Model)

<table>
<thead>
<tr>
<th></th>
<th>1[satisfied with city]</th>
<th>1[safe walking]</th>
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</thead>
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<tr>
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<td>-.046***</td>
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<td></td>
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<td>∆ ln(HPI)</td>
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<td></td>
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<td>Time FE</td>
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</table>

The table reports the coefficients associated with probit regressions of an indicator for whether the individual is satisfied with their city and for whether they perceive that it is safe to walk outside at night on zipcode logged foreclosures, logged employment, logged establishments, logged total payments due (on mortgages across the county-quarter), housing price growth, day of the week fixed effects on the interview, age, marital status, gender, and education fixed effects. Standard errors are clustered at the zipcode-level and observations are weighted by the sample weights.
### Table 6: Foreclosure Shocks and Economic Sentiments

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<tr>
<th>Dep. var. = ln(state of economy)</th>
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<td></td>
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<tr>
<td>ln(payments due)</td>
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<td>[.03]</td>
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<tr>
<td>ln(deposits)</td>
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<td></td>
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<td>ln(housing price)</td>
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</thead>
<tbody>
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<td><strong>Panel B: Standard Deviation</strong></td>
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<td>[.00]</td>
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<td>ln(deposits)</td>
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<td>Time FE</td>
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<tr>
<td>Income Controls</td>
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Notes.—Sources: CoreLogic, Gallup, Federal Housing Administration, 2008-2014. Panel A reports the coefficients associated with regressions of individual logged logged sentiments (current state of the economy [1-4] and future state of the economy [1-3]) on logged county foreclosures, conditional on controls and fixed effects. Our controls include:10 bins of fixed effects on housing price growth, logged local bank deposits, logged mortgage payments due, day of the week of the interview fixed effects, logged housing prices, and a set of individual covariates, including age, marital status, education fixed effects (no high school, high school, technical, some college, and college), and race. Our income controls include logged income (discrete twelve bins, which we average to create a continuous variable) and logged consumption expenditures on non-durables from the day before. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the county-by-year level and include a cubic as instruments. Observations are weighted by sample weights Panel B reports the coefficients associated with the standard deviation of logged individual sentiments within a county × quarter on logged foreclosures, conditional on controls. Controls include: logged housing prices, logged local bank deposits, and logged mortgage payments due. Observations are weighted by the number of individuals observed in the county × quarter × year before computing the standard deviation. Standard errors are always clustered at the county-level.
**Figure 6: Unemployment Risk and Foreclosures, by Income Bracket**

*Notes.* Sources: CoreLogic and U.S. Daily from Gallup, 2008–2014. The figure reports the coefficients obtained from regressions of an indicator for whether the respondent has had enough money to meet their basic needs on logged foreclosures, logged total payments due (on mortgages across the county-quarter), logged bank deposits, logged housing prices, the unemployment rate, day of the week fixed effects on the interview, age, marital status, gender, and education fixed effects. Standard errors are clustered at the zipcode-level and observations are weighted by the sample weights.
6. Tables and Figures

A Online Appendix (Not for Print)

A1. Concentration of and Variation in Adjustable Rate Mortgages

As we discuss in our companion work (Makridis and Ohlrogge, 2017), we now characterize the extent of the variation in ARMs that aids us in identification. Since an important component of our identification strategy is that we have variation in the set of counties containing different types of ARMs and the same type of ARMs originated at different periods, we begin by plotting the distribution of the difference in 5-1 v. 7-1 and 5-1 v. 10-1 ARMs across counties in Figure 7. There is significant variation in the relative proportions with some counties having nearly 10% more 5-1 ARMs than 7-1 ARMs, for example. We also plot the distribution of different types of ARMs in Figure 8, showing that there is also quite a bit of variation in the incidence of these mortgages across space.

Figure 9 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.

An integral feature behind our application of ARMs is that variation in their interest rate resets is driven by national forces—for example, the LIBOR interest rate. We now provide evidence that the interest rate resets in our data contain considerable variation based on the vintage and specific moment in time. While we only use 5-1, 7-1, and 10-1 ARMs, Figure 10 also plots these median interest rate resets for 2-1 and 301 ARMs. Starting with 2-1 ARMs, we see that all loan vintages experience an interest rate reset up—most of which are upward of 2%. The largest resets are among those originated in 2003 that were facing the height of the crisis between 2005-2006. Turning towards 3-1 ARMs, we see a similar interest rate reset up among some loan vintages, but for those that were originated later the interest rates reset down. We observe a similar story for 5-1 ARMs where only two years experience an interest rate reset up, but following 2003 all loan
Figure 7: Difference in the Share of 5-1 v. 7-1 and 5-1 v. 10-1 ARMs

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

Panel A: 2-1 and 3-1 ARMs
Panel B: 5-1 and 7-1 ARMs

Figure 8: Distribution of 2-1, 3-1, 5-1, 7-1 ARM Shares

Notes. Sources: CoreLogic, 2000-2014. The figures plot the distribution of the share of different mortgage duration across counties, excluding counties with a zero share.
Figure 9: Distribution of Bank ARM Shares, by ARM Type

Notes. – Sources: CoreLogic. The figure plots the distribution of bank shares for a particular type of adjustable rate mortgage between 2000 and 2014 with most of the mass between 2005 and 2008. If, for example, a bank makes 50 5-1 ARMs and 50 7-1 ARMs, then its share would be 0.50 for both.
originations (resets taking place from 2009 onward) experience resets down. All 7-1 ARMs also experienced resets down. Overall, these plots provide evidence of the significant heterogeneity in interest rate shocks among borrowers.

A2. Supplement to Main Results

We now turn towards robustness exercises over the concern that our main effects are driven by income effects—that is, perhaps what we are detecting is not a foreclosure externality, but rather a composition effect of those who actually experience foreclosure. These could result either through zipcode foreclosures representing declines in well-being directly through the person being evicted, or through a mechanical effect of higher interest rates from the ARM shocks on life satisfaction. The first case is especially unlikely because it would require that every, or at least a majority of, individual(s) surveyed by Gallup to be foreclosed upon. However, the 99th percentile of foreclosures per open mortgage across zipcodes is only 5.3%, making it very unlikely for an individual in our survey to actually be foreclosed upon.

We nonetheless investigate this potential in Table 7 through four exercises. Our first two columns provide the most direct tests by controlling for logged income and logged consumption of non-durables at both an individual-level. Monthly income is measured through six bins: less than $750, $750-1500, $1500-5500, $5500-6500, $6500-8499, and $8500 and above. To produce a continuous measure, we take the average of the upper and lower bounds. Consumption is measured at the daily level covering non-durables goods similarly defined as Attanasio and Weber (1995).

Columns 1 and 2 show that our estimated coefficients decline only marginally to -0.049, which is very close to our baseline estimates in the main text. Not surprisingly, both income and consumption enter positively: a 10% rise in consumption and income is associated with a 0.11% and 0.60% rise in current life satisfaction. Column 3, which includes both of them, surprisingly leads to a slightly larger marginal effect, but again the point estimates are not statistically distinguishable from one another. Column 4 subsequently restricts the sample to zipcodes with under 3% of borrowers in foreclosure. The fact that our results are again precise and strong suggests that the mechanical effect is not driving our results. If it were, then we would see a lower gradient when we restrict the sample to areas where our Gallup data is less likely to pick up foreclosed individuals. In all these specifications, our first-stage correlation remains stronger and well above the rule of thumb.
Figure 10: Median National Interest Rate Changes, by ARM Type

Notes.– Sources: CoreLogic. The figures plot the median interest rate change for holders of ARMs based on the type of ARM and across all loan vintages between 2002 and 2008.
Table 7: Robustness Exercises on the Potential for Income Effects

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<th>(4)</th>
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</table>

Notes.—Sources: CoreLogic, Zipcode Business Patterns, Federal Housing Administration, and U.S. Daily from Gallup, 2008-2014. The table reports the coefficients associated with regressions of zipcode logged current life satisfaction (from an index of 1 to 10) on logged foreclosures, housing price growth, employment, establishments, and individual controls, including: day of the week fixed effects on the interview, age, marital status, gender, and education fixed effects. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the zipcode-level and observations are weighted by the sample weights.
**Figure 11:** Heterogeneity in Foreclosure Shocks, by Income Bracket

*Notes.* Sources: CoreLogic and U.S. Daily from Gallup, 2008–2014. The table reports the coefficients associated with regressions of logged current and expected future life satisfaction (from an index of 1 to 10) on logged foreclosures, county unemployment, logged total payments due (on mortgages across the county-quarter), logged housing prices, day of the week fixed effects on the interview, age, marital status, gender, number of children, and education fixed effects. Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the zipcode-level and observations are weighted by the sample weights. The sample is restricted to employed individuals.

We provide one final exercise that imposes an even stricter test to address the concern that income effects could be a culprit behind our estimates. In particular, we estimate Equation 3 separately by income bracket, plotting the estimated gradients on logged foreclosures in Figure 11. Although we do obtain a larger point estimate for lower income individuals earning less than $750 per month, we cannot reject the null that each of the estimated gradients are statistically equal to one another. The fact that they are all within each other’s confidence intervals is consistent with our narrative that we are capturing spillovers, not direct individual effects. Indeed, if the latter were the case, then we would expect to find much sharper and precise gradients for lower income earners who were less likely to pay their mortgage following interest rate rests.
A3. Supplement to Pass-through of Risk

We begin by presenting evidence that wage shocks affect well-being, even after controlling for consumption shocks. To do this, we collapse our Gallup data to the year × 1[children] × 1[white] × 1[male] × 1[married] × age group (20-34, 35-49, 50-65) level of aggregation to produce a synthetic panel. We subsequently use annual 2008-2015 data from the Consumption Expenditure Survey (CES) to produce a similar synthetic panel. We restrict both samples to workers with over 500 hours worked per year and deflate nominal values by the 2010 personal consumption expenditure index. We have two measures of consumption: non-durables (as defined by Attanasio and Weber (1995)) and total expenditures.

Figure 12 plots current (Panels A and C) and future (Panels B and D) life satisfaction growth with wage income growth, residualizing using total (Panels A and B) and non-durable (Panels C and D) expenditure growth. The gradients are all positive, especially for current life satisfaction: a one percentage point rise in income growth is associated with a 0.078-0.082 percentage point rise in life satisfaction growth. The gradients fall to roughly 0.02 on average when using future life satisfaction growth. However, the fact that there is still considerable variation in life satisfaction growth even after controlling for consumption growth illustrates that income shocks pass-through to individuals beyond mere consumption.

A4. Supplement to Understanding the Mechanisms

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

We subsequently plot the distribution of these variables pre and post the Great Recession pooling across metropolitan averages in Figure 13. Perhaps surprisingly, there is massive variation
Figure 12: Life Satisfaction and Wage Income Growth Rates, Residualizing Consumption

Notes.—Sources: Gallup U.S. Daily, Consumption Expenditure Survey (CES), 2008-2015. The figure plots the residualized growth rate of current and future life satisfaction with wage income, residualizing for total and non-durables consumption expenditures. Panel A plots current life satisfaction growth with wage income growth, residualizing both using total consumption expenditures growth; Panel B uses the growth in future life satisfaction. Panel C plots current life satisfaction with wage income growth, residualizing using non-durable consumption expenditure growth. Panel D uses future life satisfaction growth. Both current and future perceptions of life satisfaction are ranked on a scale of 1-10. Nominal consumption and income values are deflated with the 2010 personal consumption expenditure index. Each observation is a synthetic panel consisting of year, indicator for not having children, indicator for being male, indicator for being married, indicator for being white, and age group (20-34, 35-49, 50-65). Observations are weighted by the number of sample points within each cell from the CES data (since it is smaller in sample size). Only synthetic panels with over 20 observations are considered and the distribution is trimmed at the top and bottom percentiles.
across locations, as well as over time, reflecting the fact that different areas experienced bigger economic shocks during the financial crisis. The fact that these distributions are so disbursed in both the cross-section and panel is consistent with the view that individuals respond more to local economic conditions versus the national ones.

To illustrate that our measures of sentiment are not contaminated purely by noise, we also correlate our measure with more common approaches to measuring uncertainty. Figure 14 plots the volatility index with the mean perception of the current state of the economy at a daily frequency, producing a correlation of -0.63. Alternatively, if the perception of the future state of the economy is used, the correlation is -0.59. The correlation is negative since positive values of our index are “good” in the sense that they indicate a stronger economy, whereas positive values of the volatility index are “bad” in the sense that they indicate greater uncertainty.\textsuperscript{41} Figure 15 provides supplementary evidence using the index of economic policy uncertainty from Baker et al. (2016).

We now turn towards evidence on our main results using the dispersion of attitudes about the current and future economic state (in standardized $z$-scores), which more closely aligns with the typical understanding of uncertainty (Bloom, 2014). To produce these measures of dispersion, I take the standard deviation of the $z$-score across all individuals in the same county separately by

\textsuperscript{41}We also find a correlation of -0.39 when we use the standard deviation of perceptions of the current state of the economy.
Figure 14: Volatility Index and Perception of the Current State of the Economy

Notes. – Sources: U.S. Daily from Gallup, 2008-2014. The figure plots daily averages of the z-score on the one to four index of perceptions about the current state of the economy with the volatility index.
Figure 15: Policy Uncertainty and Perception of the Current State of the Economy

Notes.– Sources: Baker et al. (2016) and U.S. Daily from Gallup, 2008-2014. The figure plots monthly averages of the z-score on the one to four index of perceptions about the current state of the economy with the index of economic policy uncertainty.
year; the sample is restricted to counties with over 50 respondents in each year.

Table 8 documents these results. While columns 1 and 4 present the simple least squares estimator with controls, columns 2 and 5 present the fixed effect estimates and columns 3 and 6 present the instrumental variables (with fixed effects) estimates. The instrumental variables estimates are the preferred results for reasons discussed in the main text. Under these specifications, a 10% rise in foreclosures is associated with a 0.032 and 0.032 and 0.041 standard deviation decline in the dispersion of current and future perceptions of the economy, respectively. Just like in the main results at the individual-level, the OLS estimates are heavily biased because of both dynamic selection and omitted variables bias, which operate in competing directions an attenuate the estimate to zero.\footnote{While the specific quantitative estimates differ slightly from those in the main text, that is not surprising given that the measure is itself different!}

\begin{table}[h]
\centering
\caption{Robustness Exercises on the Measurement of Uncertainty}
\begin{tabular}{lcccccc}
\hline
 & s.d. perception of current economy & & & s.d. perception of future economy & \\
 & (1) & (2) & (3) & (4) & (5) & (6) \\
\hline
ln(foreclosures) & .009*** & -.005*** & -.023*** & .000 & -.005** & -.041*** \\
unemployment rate & -.010*** & -.001 & -.002 & .007*** & -.004*** & -.005*** \\
ln(housing prices) & .026*** & -.065** & -.105*** & -.041*** & -.142*** & -.223*** \\
R-squared & .21 & .47 & .46 & .51 & .87 & .86 \\
Sample Size & 4545 & 4545 & 4545 & 4545 & 4545 & 4545 \\
Controls & Yes & Yes & Yes & Yes & Yes & Yes \\
County FE & No & Yes & Yes & No & Yes & Yes \\
Time FE & No & Yes & Yes & No & Yes & Yes \\
Instruments & No & No & Yes & No & No & Yes \\
\hline
\end{tabular}
\end{table}

Notes. – Sources: CoreLogic and U.S. Daily from Gallup, 2008-2014. The table reports the coefficients associated with regressions of the standard deviation on the average \( z \)-score of current (and separately future) perceptions of the state of the economy (rated on a 1-4 index for current and 1-3 index for future) on logged foreclosures, county unemployment, logged housing prices, age, marital status, gender, and education shares (all at the county by year level). Foreclosures are instrumented using the predicted coefficients of logit regressions of an indicator for whether the individual foreclosed on their loan in a given month on a constant, an indicator for the type of loan (5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the zipcode-by-year-quarter level and include a cubic as instruments. Standard errors are clustered at the county-level and observations are weighted by the number of individuals observed in each county-year in the Gallup data.