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 0.53% and 0.23% 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).
1. Introduction

Understanding the transmission of shocks to individual behavior 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 individual well-being and happiness (Frijters et al., 2004; Bayer and Juessen, 2015). Understanding how financial shocks affect individual well-being is essential for understanding not only the welfare costs of business cycles, which have been subject to intense debate (Lucas, 1987; Krusell et al., 2009), but also heterogeneity in insurance at the micro-level (Blundell et al., 2008; Heathcote et al., 2014).

This paper exploits the rapid rise of foreclosures during the Great Recession as a type of location-specific financial shock to characterize their causal effect on individual well-being and examine the underlying transmission mechanisms. In Q2:2009, approximately 1.47 percent of all open mortgages were in foreclosure, which was up 400% from the average in 2000 of 0.36 percent. The surge in foreclosures has been associated with an array of persistent, adverse outcomes, such as increases in mortality (Currie and Tekin, 2015) and crime (Cui and Walsh, 2015), and declines in housing values (Mian et al., 2015). Concern about the welfare effects of the financial crisis led to the Troubled Asset Relief Program (TARP) and Home Affordable Modification Program (HAMP), which helped ease the liquidity crisis banks found themselves in and helped homeowners stay in their homes and refinance their rates, respectively.

Unfortunately, empirically identifying the causal effects of foreclosures on well-being is fraught with identification problems. Since areas that experienced foreclosure shocks also experienced housing and labor market shocks, standard least squares estimators will, at best, confound variation in foreclosures with variation in other unobserved shocks, such as housing value and/or employment declines, producing downwards biased estimates. Especially at the height of the Great Recession, foreclosing on some homeowners would have required that banks value properly

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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 10 in Appendix Section A1.3. 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), health (Hafner et al., 2015), and productivity in the work-place (Oswald et al., 2015). See Frey and Stutzer (2002) for a discussion about the importance of happiness data in economics.
ties at their true value, which would have thrust many of them into insolvency. Instead, many banks waited until local labor market conditions improved and housing prices began recovering until they foreclosed on delinquent home owners.\(^4\) Administrative backlogs of local governments, mortgage servicers, and banks also led to a great many foreclosures not being processed until economic recoveries in local areas were already underway.

Using data on over 170 million loans, we introduce an identification strategy based on companion work that exploits the institutional structure of adjustable rate mortgages (ARMs) originated prior to and during the Great Recession (Makridis and Ohlrogge, 2017). These loans had the feature of specifying a fixed (“teaser”) interest rate on mortgage payments for a certain number of years. However, after the specified period, the interest rates would abruptly change. We non-parametrically show that interest rate resets on these loans are associated with discontinuous changes in foreclosure probabilities. Since zipcodes vary in their dispersion of ARMs and their type for largely historical reasons relating to the expansion of banks in different geographies, we leverage variation arising from heterogeneity in their interest rate reset changes.\(^5\) We also control flexibly for housing price and employment shocks to guarantee that our estimates are not confounded by other time-varying shocks.

While a valid concern is that changes in the dispersion of ARMs at the zipcode-level are correlated with shocks to individual well-being, we implement a number of exercises that suggest otherwise.\(^6\) First, we show that owners with 5-1, 7-1, and 10-1 ARM loans have almost completely overlapping FICO scores, suggesting that differences in the type of ARM are not correlated with individual-specific unobserved heterogeneity.\(^7\) Second, we show that county-level changes in income, housing prices, unemployment, and the share of college graduates between 1990 and 2000 are

\(^4\)While we discuss this identification problem in much greater detail later, former Treasury Secretary Tim Geithner is famously reported as describing federal programs for homeowners in foreclosure to have been principally designed to slow the pace of foreclosures so as to “foam the runway” for banks, enabling them to avoid recognizing losses on their housing portfolios all at once. http://dealbook.nytimes.com/2014/05/06/what-tim-geithner-got-right/

\(^5\)The fact that the resets on these loans are contractually determined at the origination of the loan means that they are, by construction, exogenous with respect to contemporaneous economic shocks following origination. Even if counties were strategically targeted, violations to our identifying assumption would require a discontinuous change in an unobserved variable at precisely the same time as the interest rate resets.

\(^6\)Our companion paper contains the full set of exercises (Makridis and Ohlrogge, 2017), which we will reference throughout various components of this paper.

\(^7\)This is important since our identification strategy is based on the fact that these different types of loans are experiencing interest rate resets at different times, thereby causing exogenous variation in when different counties experience foreclosures. The similarity between these plots shows that, for instance, individuals with 5-1 ARM loans, who experience rate resets sooner, do not appear to be meaningfully different from those with 7-1 or 10-1 loans, who experience resets later.
uncorrelated with the dispersion in ARMs, suggesting that areas that experienced more growth than others were not more or less likely to have a particular type of ARM. Third, we provide evidence that local dispersion in different ARMs is driven by historical factors that led banks to adopt different national corporate strategies (e.g., loan portfolios), suggesting that changes in a geography’s mortgage products may be orthogonal to contemporaneous shocks to well-being.\footnote{For example, regressions of bank deposits on bank-by-CBSA fixed effects produce an $R^2$-squared of 0.93. Moreover, we use 23 years of data between 1994 and 2014, meaning that the high $R^2$-squared from the spatial variation is not driven by having data on too short of a time horizon. That banks’ geographical footprints are remarkably stable suggests that they do not strategically move into different areas based on short-run changes in demographic or economic characteristics of those areas.}

Using new micro-data from Gallup’s U.S. Daily poll between 2008 and 2014, we produce the most comprehensive database to date on realized foreclosures and self-reported measures of well-being and perceptions of the economy.\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.}\footnote{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.} Our main specifications regress indices of logged current and future well-being on instrumented logged foreclosures, controlling for a wide array of individual characteristics and time-varying county factors, conditional on zipcode and time fixed effects. Our preferred estimates suggest that a 10% rise in foreclosures is associated with a 0.53% and 0.23% decline in current and expected future life satisfaction. We also document variation across states, which emerges due to, for example, differences in the length of foreclosure processes.\footnote{Judicial foreclosure laws require that lenders process mortgage foreclosures through the court system, thereby adding time and difficulty to the process. See Mian et al. (2015) for an application of the cross-sectional variation to identify the effect of foreclosures on housing prices.} We find some suggestive evidence that, in states with longer foreclosure processes, foreclosures have a larger adverse effect on well-being, which may emerge because the final-point impact is more severe.

Perhaps the main counterargument to our results is that, rather than representing genuine spillover effects, the estimated gradient is simply capturing the direct effect of a foreclosure on an individual. Although this possibility is highly unlikely since the share of foreclosures per open mortgage never goes beyond 6% (meaning that 94% of the respondents in our sample will not be foreclosed upon), we show that our results are robust across two margins. First, we directly control for income and consumption on non-durable goods. To the extent our estimated gradient is merely an income effect, adding these controls would completely absorb the effect. Second, we
restrict our sample to zipcodes with shares of foreclosures below 5% and below 3%, finding that our estimated gradients are similar. Our results are remarkably robust.

Using the same instrumental variables strategy, we subsequently present two possible mechanisms that contribute to the decline in self-reported well-being. We find evidence of two channels at the micro-level. First, we show that increases in foreclosures are associated with perceptions of lower neighborhood quality and other community amenities. One channel consistent with this evidence is that foreclosures raise the frequency and/or intensity of crime, which depresses overall well-being if community engagement becomes more difficult or dangerous. Second, we show that increases in foreclosures are associated with perceptions of greater uncertainty both about the current and future state of the economy. When we investigate plausible sources, we find that individuals are more likely to be concerned about their future income and/or employment status.

Our paper contributes directly to three main literatures. The first is a literature on the impact of financial shocks on individual labor market and consumption outcomes. While it is perhaps not surprising that job displacement has large and persistent effects on individual earning dynamics (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 their mechanism by showing that a large financial shock affects individual well-being and can be the channel through which health outcomes deteriorate. 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). Our results illustrate that financial shocks generate variation in well-being even after controlling for differences in consumption, suggesting an additional pass-through channel.

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

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

13 While 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
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. Our paper also joins a concurrent series of papers that have used similar variation in interest rates for different loans to infer causal relationships; see, for example, Fuster and Willen (2017), Gupta (2016), and Di Maggio et al. (2017). Our paper, however, uses a much larger sample of loans (170 million versus 22 million) and uses resets to predict foreclosure probabilities using a loan-level model allowing variation in the slopes by state to account for heterogeneity in the foreclosure process. 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).

The third is a literature about the role of sentiments in generating self-fulfilling business cycles. Macroeconomic models of self-fulfilling expectations, for example, study the impact of sentiments on aggregate productivity (Angeletos and La’O, 2013; Benhabib et al., 2015), currency unions (Morris and Shin, 1998), and the diffusion of information (Morris and Shin, 2002; Angeletos and Pavan, 2007; Angeletos et al., 2016). Saving a few empirical contributions, such as Benhabib and Spiegel (2016) and Makridis (2017), there is no microeconomic evidence that validates the mechanism in these models. Our results, however, show how the surge in foreclosures contributed to worsening well-being and, therefore, may have amplified the Great Recession and the protracted recovery period. Our results are also consistent with the fact that well-being is procyclical (di Tella et al., 2001, 2003), which sheds light into the welfare costs of business cycles (Wolfers, 2003). Finally, our results are closely linked with an emerging literature on the role of beliefs in the formation of expectations about housing market fluctuations (Bailey et al., forthcoming) and leverage (Bailey et al., 2017).

2. Data, Measurement, and Institutional Details

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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/
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 2000 and 2014, covering 3,189,640 million unique hybrid and balloon mortgage loans, although for our purposes in this paper we start the sample in 2008 given the starting point of our Gallup micro-data. For each loan, we observe a vector of initial characteristics, giving information such as the contract type (Hybrid ARM, Balloon, etc.), the initial interest rate, and the schedule for interest rate resets and balloon payments. We also observe monthly performance updates, giving information on factors such as a loan’s current interest rate and whether it has prepaid, been foreclosed upon, or is still current. Our data set covers a total of 158,674,405 such loan-month observations. An advantage of our data is that we focus on the universe of loans, rather than a subset (e.g., sub-prime loans), which might be subject to selection problems. For calculating the total number of foreclosures in our non-IV specification, we use the entire CoreLogic dataset for all loans. This covers approximately 170 million unique mortgage loans and 5 billion loan-month observations.\footnote{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 the second, third, and fourth quarters of 2008; an additional 656,000 prime borrowers lost their homes between 2009-2012 (relative to subprime borrowers).}

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

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.16

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].

Despite 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, for example, the 2008 wave of the survey. While the main results are qualitatively robust to excluding 2008 from the sample, it is included for two reasons. First, there is important variation in foreclosure shocks between 2008 and 2009, so discarding it would reduce the efficiency of our estimator. Second, to the extent measurement error is introduced, there is no reason to suspect it would be correlated with foreclosures since individuals are randomly sampled throughout the U.S.
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 Details

A fundamental feature of the financial crisis, and a key identification challenge for our paper, 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 media. 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

\footnote{See https://www.bloomberg.com/view/articles/2014-02-26/banks-prefer-losses-they-don-t-have-to-talk-about, for mortgage-specific rules on realizing losses, see https://www.federalreserve.gov/bankinforeg/srletters/sr1210a1.pdf}
second lien interests and in continuing to receive mortgage servicing fees.\textsuperscript{19}

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.\textsuperscript{20} Even setting aside the strategic rationale of banks, many courts, banks, and mortgage servicers were simply back-logged with far too many foreclosures to process in a short time span.\textsuperscript{21} By the time most foreclosures occurred, enough time had passed that the economy as a whole was starting to improve.

Consistent with these descriptive facts is evidence from Herkenhoff and Ohanian (2015) on the way many individuals delayed foreclosure to maintain an implicit credit line that allowed them to search longer for the right job match. Foreclosure delay, therefore, creates a spurious correlation between higher foreclosures and improving conditions, which is compounded by the fact that increased foreclosures lead to a reduction in the effective unemployment subsidy and thus leads more individuals to accept jobs even if they are below their reservation values. We will also allow for heterogeneous effects for states with different stringencies of judicial foreclosure processes.

\section*{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 1. 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 1, we see that there is massive heterogeneity in the mean foreclosures at a county-by-quarter level. That is, there is a very large right tail of the distribution.\textsuperscript{22}

We now examine whether there is any descriptive evidence over a relationship between foreclosures and well-being in the cross-section. Figure 2 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

\begin{itemize}
  \item \textsuperscript{19}http://www.realtytrac.com/news/home-prices-and-sales/banks-delay-foreclosures-and-pray-property-values-increase/
  \item \textsuperscript{20}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/
  \item \textsuperscript{21}http://www.creditslips.org/creditslips/2012/11/where-are-the-foreclosures.html
  \item \textsuperscript{22}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.
\end{itemize}
Figure 1: 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].

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.

3. Foreclosures and Reported Well-being

3.1. Identification and Dynamic Selection

Consider a naive regression where we regress an individual’s self-reported well-being onLogged foreclosures, controlling for other individual demographic characteristics and local economic shocks

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

where \( y \) denotes our outcome variable (e.g., logged current or future life satisfaction), \( D \) denotes a vector of individual covariates, \( g(h, \theta) \) denotes a semi-parametric vector of time-varying location controls, specifically housing prices and unemployment rates, \( f \) denotes logged foreclosures, and \( \phi \) and \( \lambda \) are fixed effects on location (zipcode) and time (year and quarter). In practice, we semi-parametrically control for time-varying local shocks by using not only logged zipcode housing...
Figure 2: 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”. 

\[
\text{community index, z-score = } -0.15 - 0.218 \ln(\text{foreclosures, z-score})
\]
prices and the county unemployment rate, but also 10 bins on the annual growth rate of housing prices. These controls help assure that our estimated $\gamma$ is not driven by variation in housing price declines, which are highly correlated with foreclosures.

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, the housing crash was largely an unforeseen period of financial distress. Instead, our primary concerns with Equation 1 are a combination of omitted variables bias and dynamic selection. First, even after controlling for location and time fixed effects, it is possible that there are other time-varying unobserved individual and/or local factors that affect life satisfaction besides foreclosures, but are simply correlated with them. Motivated by the fact that stagnating housing prices were one of the main catalysts for the surge in foreclosures in 2009-2010 (Edmiston and Zalneraitis, 2007), our semi-parametric controls on housing prices helps mitigate this concern. Given that Di Maggio et al. (2017) show that interest rate changes affect individuals’ disposable income, we also control for the total value of mortgage payments due at any point in time for a given county.23

Second, assuming we control for all relevant omitted variables, we still face a serious dynamic selection problem: the behavior among banks to strategically delay foreclosure to avoid realizing losses on their assets until local economic activity and housing prices rebounded. To understand the dynamic selection problem, consider the following setting. Suppose a county is hit hard by the Great Recession: employment is down, investment is down, and housing prices are down. Since banks are required to take physical possession of a property following foreclosure, a bank will have to write off considerable losses on its asset if the property is valued lower than the value of the original equity-based loan. That is, upon foreclosure, the bank is forced to value the value of its asset, rather than simply keeping the value of the loan backed by the property as “loans outstanding” on its balance sheet (Antoniades, 2015). The process created perverse incentives, which were higher for banks in more precarious positions, to value assets at their true value. In this sense, an unobserved shock that raises employment might signal to banks that the housing market is turning the corner, thereby raising their incentive to foreclose on the loan, creating a dynamic selection problem (i.e., upwards bias on $\gamma$).

Finally, even if we could randomly assign households to areas with more versus fewer foreclo-

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23We also control for bins over an individual’s monthly income, although we omit them from our baseline results since income brackets are missing for some of our sample.
sures, 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, since foreclosures are still a small portion of an overall community—roughly 1.4% of homes were in foreclosure during the peak of the Great Recession—it is highly unlikely for an individual in foreclosure to appear in our data in a systematic way. Second, we will conduct robustness that restricts the sample to counties with lower shares of foreclosures to help reduce the aforementioned probability even further, illustrating that our results are robust to these restrictions. Third, we will 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 6 in Appendix Section A1.3.

3.2. Empirical Strategy

We deal with these time-varying shocks by exploiting a novel feature of the design of loans over the past decade. Our instrumental variables strategy focuses specifically on the important role of a loan’s interest rate in generating these predicted probabilities. Many hybrid adjustable rate mortgages (ARMs) were initially offered to individuals with special “teaser” rates for an initial period. The rates on these loans, however, would frequently spike after the first reset date such that they were in excess of the prevailing interest rate (e.g., LIBOR) by as much as 8% or more.\footnote{Gorton (2008) argues that these loans were designed to make it impossible for borrowers to afford payments after the reset date so that lenders could decide whether to refinance the loans or foreclosure on the property.}

Figure 3 non-parametrically plots 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 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 A1.1., we plot the corresponding interest rate movements for each loan type by their year of origination.

To account for these interest rate effects, we begin by estimating loan-level logit regressions

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

where \(i\) indexes the loan and \(t\) the month-year, \(\text{Loan}^k\) denotes an indicator for the \(k\)-th type of ARM loan, \(\text{Reset}\) denotes the difference between the loan’s initial interest rate at origination and the interest rate in time \(t\), and \(\text{Loan}^k \times \text{Reset}\) captures the differential effect on foreclosure probability that the reset has for different types of ARMs. Before continuing, we emphasize two details about our estimation of Equation 2. First, we purposefully omit typical controls to avoid contaminating our predictions in foreclosure probabilities with other time-varying shocks and/or time-invariant characteristics of the location.\(^{25}\) Second, while we estimate Equation 2 separately for each state—since the foreclosure process and judicial laws vary in lenience and speed—our results are robust to pooling all states together.

Is it valid for us to examine the foreclosure probability directly following an interest rate change since the foreclosure process often takes time after a borrower initially defaults on a mortgage? For example, foreclosure delay is common and behaves as a source for additional credit (Herkenhoff and Ohanian, 2015; Gerardi et al., 2015). First, some loans may enter default or foreclosure prior to the reset date. In these cases, a discontinuous spike in the interest rate can accelerate the foreclosure process, making it even more difficult, if not impossible, to pay. Conversely, when the interest rate spikes downwards, a homeowner will have an easier time meeting the payment on the loan contract and leaving default or foreclosure. Second, foreclosure takes place more quickly in some states over others (e.g., those without judicial foreclosure laws), meaning that the relationship between interest rate spikes and foreclosure will be even starker in them. Finally, and most importantly, our loan-level specifications allow for a continued effect of the changed reset after the initial reset date; that date is simply the starting point of the effect.

After fitting these regressions to our 158 million loan-month observations, we recover predicted foreclosure probabilities for each observation. Since the occurrence of a foreclosure is a binary

\(^{25}\)Doing so, however, produces similar (and slightly stronger) results, which should not be surprising given the reason we stated above.
Figure 3: 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.
outcome, its expectation equals its probability. We sum over the loans in a given zipcode to obtain predicted numbers of foreclosures in that zipcode for each period during our study, denoted $Z_{jt} \equiv P(\hat{f}_{jt})$. We subsequently use these predictions to instrument actual foreclosures through two-staged least squares regressions

$$
Z_{jt} = \beta D_{it} + \alpha X_{jt} + \gamma \hat{f}_{jt} + \phi_j + \lambda_t + \epsilon_{ijt}
$$

$$
y_{ijt} = \beta D_{it} + \alpha X_{jt} + \gamma \hat{f}_{jt} + \phi_j + \lambda_t + \epsilon_{ijt}
$$

(3)

where $\hat{f}_{jt}$ denotes the predicted foreclosures based on the ARM resets from our reset instrument. Importantly, our estimates do not use borrower characteristics (e.g., FICO scores, incomes) or geographic attributes (e.g., share of college degree workers) since our goal is to capture only the variation in foreclosures driven by these idiosyncratic reset shocks. We emphasize that our 2SLS procedure uses the predictions from the loan-level model as its first stage instrument, the loan level model is not the first stage of a 2SLS procedure. We also use only 5-1, 7-1, and 10-1 ARMs and balloon mortgages, which are not subject to endogeneity concerns relating to income targeting—that is, firms strategically targeting lower income earners with teaser rates for 2-1 and 3-1 ARMs. In Figure 3 of Appendix Section A1.1., we illustrate that the distribution of FICO scores are nearly identical across these three types of ARMs.

Figure 4 provides an assessment of the strength of our first-stage by plotting the residualized endogenous variable (logged foreclosures) with the our instrument: logged predicted foreclosures based on interest rate resets for the sum of 5-1, 7-1, and 10-1 ARMs and balloon mortgages. The partial $F$-statistic on the instruments is quite large—far above the recommended $F$-statistic of 10 from Stock and Yogo (2005) and the $R$-squared is 0.75. In practice, we use a cubic in the instrument to capture the non-linearities between these interest rate resets and realized foreclosures. We also examined the geographical source of the identifying variation by computing correlations between realized and predicted foreclosures separately by group, but we did not find any systematic evidence that it was being driven from some locations over others.26

Our identification strategy is closely related with several recent contributions, in particular Gupta (2016) who examines the impact of foreclosures on housing price declines, as well as Fuster and Willen (2017) who examine the impact of loan size on mortgage default and Di Maggio

---

26For example, we split county household income into four quantiles and found correlations of 0.389, 0.348, 0.398, and 0.465 for quantiles 1 to 4, respectively. We also explored whether it varied based on areas with more versus fewer males in the population, but the correlations were 0.406 and 0.397, respectively. We finally looked for differences across areas with different age distributions, but consistently found a correlation of 0.40.
Actual Foreclosures = −.13 + 3.46 loan prediction

Figure 4: First-stage Partialed Correlation of Instruments and Foreclosures
Notes.–Sources: CoreLogic and U.S. Daily from Gallup. The table reports the coefficients associated with regressions of the residualized instrument (predicted foreclosures from interest rate resets on 5-1, 7-1, and 10-1 ARMs plus balloon mortgages). Controls used for residualizing include: county unemployment, logged housing prices, day of the week fixed effects on the interview, age, marital status, gender, education fixed effects, and zipcode and time fixed effects. These controls are included in our second-stage of the main results, so we include them here for completeness. Standard errors are clustered at the zipcode-level and observations are weighted by the sample weights.
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, containing 170 million loans of which 3.2 million ARM loans are resetting. 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 loan-level model that extracts the component of variation in realized foreclosures that is predicted from these ARM resets, whereas past papers have focused on interest rate changes as the primary independent variable of interest. These combined approaches have wide applicability for future research.

While our formal robustness section later, together with our companion work in Makridis and Ohlrogge (2017) and Appendix Section A1.2., addresses various dimensions of the exclusion restriction in detail, we specifically discuss two of them here. First, since changes in interest rate resets affect disposable income, are our effects merely picking up an income effect? While our robustness exercises later control directly for both income and consumption expenditures, which does not alter our main estimates, our default specification controls for loan payments. In this sense, we are identifying the effect of interest rate resets on foreclosure probabilities, conditional on any potential effects on disposable income (Di Maggio et al., 2017). Second, since our identifying variation comes from individuals being more or less able to make mortgage payments due to discontinuous changes in the interest rate on their mortgage, are our estimates contaminated by strategic default? Bajari et al. (2008) show that short-term liquidity constraints for many homeowners were at least as important as the decline in housing prices in explaining the increase in defaults and Guiso et al. (2013) show that strategic default played a small role (on the order of 26%), implying that speculation is not a driving force behind our variation.28

\[27\text{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 Blacbox 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 nearly 2000 counties with over 100 respondents from our data.}

\[28\text{Credit bureau data between 2009-2011 suggests an even lower magnitude of 12-19\% (Experian and Wyman, 2009). Foote et al. (2008) also provided evidence that the share was as low as 6.4\% in Massachusetts between 1990-1991.}]}
3.3. Main Results

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 the cross-sectional conditional correlations between current life satisfaction and foreclosures are small and ambiguous signs: a 10% rise in foreclosures is associated with a 0.04% decline in current life satisfaction, but a 0.04% rise in future life satisfaction. Once we add county unemployment rates and housing prices as controls, the coefficient on current life satisfaction grows in magnitude to 0.07% for a comparable rise in foreclosures, but becomes insignificant for future life satisfaction. Adding zipcode and time fixed effects attenuates both coefficients down to zero because of the dynamic selection problem.\(^{29}\)

Columns 4 and 10 present our preferred baseline estimates, instrumenting logged foreclosures using our predicted measure of foreclosures based on variation in the interest rate resets for 5-1, 7-1, and 10-1 ARMs. Here, we find that a 10% rise in foreclosures is associated with a 0.53% and 0.23% decline in current and future life satisfaction, respectively. We also see that shocks to housing prices are negatively associated with life satisfaction, which is not surprising since higher housing prices imply larger mortgages or rental rates.\(^{30}\) These gradients are also more statistically associated with changes in life satisfaction, which suggests that housing market shocks may account for more of the variation in life satisfaction than labor market shocks. In either case, the fact that our gradients on foreclosures are more precise and larger in magnitude is consistent with our concern about dynamic selection.

Although not reported, we have also experimented with both least squares and instrumental variables regressions containing metropolitan \(\times\) year fixed effects, which exploits time-varying fluctuations in foreclosures at the zipcode level. 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, conditional on zipcode housing prices and after controlling for all shocks common to the metropolitan area over a year. We recover a coefficient of 0.0022 (\(p\)-value = 0.046) for our least squares estimator, which is quite similar to our estimate in

\(^{29}\)Are our instrumental variables estimates too high relative to our least squares results? While often IV results have larger standard errors than OLS estimates, it depends on the correlation between the underlying covariates and the omitted characteristics and source of bias (Greene, 2012).

\(^{30}\)The full set of effects is slightly more complicated since higher housing prices can create a wealth effect. If individuals finance consumption through home equity loans, housing prices can in theory raise life satisfaction by helping allow for greater spending.
column 3, suggesting that county × year fixed effects do little to deal with the dynamic selection problem. However, we recover a coefficient of -0.077 (p-value = 0.00) for our instrumental variables estimator, which is actually greater than our baseline estimate in column 4.

Recognizing that there is a lag between foreclosure proceedings and actual foreclosure (Molloy and Shan, 2012), and that these lengths of time vary by state, we also estimate our main specification separately for states with (“JUD”) and without (“NJUD”) judicial foreclosure laws. These laws make foreclosures more difficult and lengthy to process since additional rules must be followed when obtaining a lawful foreclosure in the court system; see Mian et al. (2015) for an application of these in identifying the effects of foreclosures on housing prices. Surprisingly, however, we find that states without these laws have a weaker foreclosure gradient: a 10% rise in foreclosures is associated with a 0.53% decline in current life satisfaction among these states versus a 0.75% decline in states with judicial foreclosure laws. One explanation is that, since foreclosures are much less frequent in states with judicial foreclosure laws, when foreclosures happen, they have a bigger community impact. A comparable rise in foreclosures is associated with a 0.19% and 0.49% decline in future life satisfaction in non-judicial status and judicial status states, respectively.

While we have worked hard to mitigate endogeneity problems arising from unobserved shocks—either time-invariant or time-varying—there are two related concerns with our results. First, it is possible that they simply capture the direct effect of foreclosures on individual well-being. If we were merely capturing the fact that individuals who experience foreclosures suffer a decline in well-being, the result would be much less interesting. Second, it is possible that interest rate fluctuations reduce life satisfaction directly, independent of the foreclosure channel, which would violate the exclusion restriction of our instrument. Both concerns amount to an income effect. Table 6 in Appendix Section A1.3. implements robustness that assuage this concern, affirming our interpretation of these gradients as spillover effects for broader communities.

We begin by controlling for individual income in column 1 and daily consumption expenditures of non-durable goods in column 2. Individuals are classified into six monthly income bins, so we produce a continuous version by averaging the upper and lower bounds that determine an individual’s group. Individuals also report their consumption expenditures on non-durable goods for the day before the survey, which proxies for a more permanent level of non-durables consumption since we are controlling for day of the week fixed effects and applying the appropriate sample weights. Columns 1 and 2 continue to point to statistically significant negative gradients equal to -0.051 and -0.048, which are almost statistically indistinguishable from our baseline estimates.
<table>
<thead>
<tr>
<th></th>
<th>logged current life satisfaction</th>
<th></th>
<th>logged future life satisfaction</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ALL</td>
<td>ALL</td>
<td>ALL</td>
<td>ALL</td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>-.004***</td>
<td>-.007***</td>
<td>-.001</td>
<td>-.053***</td>
</tr>
<tr>
<td>unemployment rate</td>
<td>.002***</td>
<td>-.002***</td>
<td>-.001</td>
<td>-.001</td>
</tr>
<tr>
<td>ln(housing prices)</td>
<td>-.010***</td>
<td>-.044***</td>
<td>-.088***</td>
<td>-.069***</td>
</tr>
<tr>
<td>R-squared</td>
<td>.04</td>
<td>.04</td>
<td>.06</td>
<td>.06</td>
</tr>
<tr>
<td>Sample Size</td>
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<td>1583698</td>
<td>1583698</td>
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</tr>
<tr>
<td>Controls</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Zipcode FE</td>
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</tr>
<tr>
<td>Time FE</td>
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<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Instruments</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

**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, and education fixed effects. “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.
We subsequently turn towards two additional exercises. Column 3 controls instead for housing price growth, rather than the logged level. While we are already controlling for fixed effects on housing price growth using 10-bins, the continuous measure adds additional granularity. Again, we find a gradient of -0.04. We finally restrict the sample to zipcodes with less than 3% of households in foreclosure computed using a measure of foreclosures per open mortgage. Since the 99th percentile of foreclosures per open mortgage observed in our sample is 5.3%, we view the restricted sample as a way of mitigating the probability that an individual in our sample is actually being foreclosed upon. Under this new restriction, we obtain a gradient of -0.063 in column 4.

In addition to these aforementioned tests, we can also partially identify the source our estimates. Suppose that we are capturing both direct and indirect effects. Suppose that one thousand people are surveyed in a given county and they report an average life satisfaction of 7, for a total life satisfaction among the sample of 7000. Suppose that from one quarter to the next, foreclosure rates in the county go from 1% of open mortgages to 2%, or a 100% increase. Our coefficient estimate from Table 1 of -.053 suggests this would be accompanied by a 5.3% decline in life satisfaction, or in other words, a drop of 371 points among the total sample, from 7000 to 6629. How much of this could be attributed to the direct effects of people being foreclosed upon?

Under 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%.\textsuperscript{31} We also assume that 75% of those surveyed are homeowners.\textsuperscript{32} 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.\textsuperscript{33} 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.

Given that this 12% is a “doomsday scenario” for our estimates, what is our interpretation of the remaining 88 or more percent? While we explore potential mechanisms shortly, we view the negative association between foreclosures and life satisfaction as evidence of spillovers and, more generally, the pass-through of risk onto households through general equilibrium channels: even if an individual is not foreclosed on directly, foreclosures among neighbors can indirectly affect their

\textsuperscript{31}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

\textsuperscript{32}A conservative estimate that well exceeds the U.S. national home ownership rate which peaked at 69.2% in 2004

\textsuperscript{33}With our zipcode fixed effects, it is only deviations from a county’s mean rates that impact our estimation.
well-being. These spillovers are consistent with two classes of models: external habit formation (Pollak, 1970; Abel, 1990) and risk sharing in social networks (Ambrus et al., 2014).

In the first class of external habit formation models, individual preferences depend in part on consumption among peers. While most of these models have taken the form of “catching up with the Jonses,” an equally plausible setting is one where a negative shock to a neighbor (i.e., foreclosure) leads to a reduction in an individual’s life satisfaction. Such outcomes can emerge if individuals have preferences for community. In the second class of risk sharing in social networks models, connections among individuals creates social collateral to help enforce informal insurance payments. 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). In this sense, our results are consistent with models where financial shocks generate externalities that indirectly affect individuals.

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 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 interaction with our demographic indicator using our standard interest rate reset instruments also interacted with our demographic indicator.

Table 2 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.

3.5. Robustness

We now discuss several core robustness exercises that address potential concerns with the exclusion restriction in our instrumental variables strategy. Before turning to these in greater detail, we reiterate that all our specifications contain controls for the total loan payments that are due within a given county and quarter, which obviates the concern that our results are driven by income effects when interest rate changes affect homeowners disposable income (Di Maggio et al., 2017). We also provide several pieces of evidence in Table 6 and Figure 11 in Appendix Section A1.3. that directly control for income / consumption and estimate the gradients separately by income bracket, leaving our main results unchanged.

We now implement several more direct diagnostics that help us gauge the credibility of our exclusion restriction. For brevity, we only summarize them, but defer to Appendix Section A1.3. (with some references to our companion work in Makridis and Ohlrogge (2017)). First, the distribution of FICO scores among those with 5-1, 7-1, and 10-1 ARMs are nearly identical. To the extent that FICO scores are a reasonable proxy for individual heterogeneity, these similarities are consistent with a story where different banks simply have different lending strategies (e.g., con-
Table 2: Heterogeneity in the Effects of Foreclosures on Current Life Satisfaction

<table>
<thead>
<tr>
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<th>ln(current life satisfaction)</th>
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<tbody>
<tr>
<td></td>
<td>(1)</td>
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<tr>
<td>college attainment</td>
<td>.051***</td>
</tr>
<tr>
<td></td>
<td>[.002]</td>
</tr>
<tr>
<td>black</td>
<td>.001</td>
</tr>
<tr>
<td></td>
<td>[.002]</td>
</tr>
<tr>
<td>young (&lt;40 years)</td>
<td>.028***</td>
</tr>
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<td></td>
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<tr>
<td>male</td>
<td>-.038***</td>
</tr>
<tr>
<td></td>
<td>[.001]</td>
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<tr>
<td>ln(foreclosures)</td>
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</tr>
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<td></td>
<td>[.005]</td>
</tr>
<tr>
<td>× college</td>
<td>-.015***</td>
</tr>
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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, 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.
tracts on their loans). However, since we recognize that FICO scores are not a complete measure of individual heterogeneity, especially since soft information became quite important (Piskorski et al., 2015), we also implement a more restrictive series of regressions where we examined the association between the share of ARMs at a county-level with the number of filers in different income brackets. Consistent with our assumption that changes in the dispersion of ARMs and their implied interest rate resets are exogenous, we find no pattern between ARMs and income.

Second, we compute county-level growth rates in income, housing prices, unemployment rates, and the share of college graduates between 1990 and 2000. Perhaps surprisingly, they are uncorrelated with the share of ARMs. If, in contrast, they were highly correlated, then we might be concerned that areas with particularly high ARMs prior to the Great Recession were areas that experienced a large boom at the turn of the century, thereby making the dispersion in ARMs that we exploit non-random.

Third, we provide evidence that dispersion in ARMs is due to largely historical factors; see Stanton et al. (2014) for more institutional details. Banks tend to be remarkably stable in the geographic areas in which they operate. We examine this hypothesis by using bank-by-CBSA-by-year deposits from 1994 to 2016 from the Federal Deposit Insurance Corporation’s (FDIC) Statement of Deposits. After recovering each bank’s share of its deposits for each metropolitan area, we regress it on bank-by-CBSA fixed effects, producing an $R^2$-squared of 0.93. The fact that the spatial variation alone explains so much of the variation in actual lending activity suggests that banks are not, in general, making large moves to enter or exit specific geographic areas. Our companion work in Makridis and Ohlrogge (2017) also provides several additional pieces of evidence, including the fact that banks that tend to use more 5-1 ARMs use fewer 7-1 ARMs.

### 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 170 million loans, which covers nearly the universe of residential housing market loans, which we 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 and are an essential ingredient for understanding macroeconomic fluctuations and aggregate welfare (di Tella et al., 2001).
While there has been an emergence of recent literature on foreclosures, it has primarily focused on the effects on housing prices (Mian et al., 2015; Gupta, 2016). We, however, focus on another important measure for understanding the real effects of the financial crisis, namely the impact on individual well-being. In Figure 10 in Appendix Section A1.3., we produce a synthetic panel and show that there is considerable variation in life satisfaction growth in response to wage income growth even after controlling for consumption growth. Motivated by this evidence, we use the surge of foreclosures during the Great Recession as a type of location-specific financial shock to examine how financial shocks pass-through and affect individuals, related with the literature on partial insurance against labor market shocks (Blundell et al., 2008; Heathcote et al., 2014).

Our results point towards significant local spillovers. Even if an individual is not foreclosed upon, foreclosures in their zipcode are associated with declines in well-being. 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) use administrative data to show that there is a rise in mortality following an individual’s displacement from the labor market. Similarly, Currie and Tekin (2015) use zipcode level data to show that there is a rise in mortality following a rise in 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. Companion evidence has also illustrated that uncertainty about the future is a major determinant of well-being (Makridis, 2017). Putting these factors together, an unexpected rise in stress hormones can affect blood pressure and cardiovascular health (McEwen, 1998a,b), adversely affecting both physical and mental illnesses (Goldberger and Breznitz, 1993; Schneiderman et al., 2005).

4. Understanding the Mechanisms

4.1. Mechanism #1: Local Amenities

We begin by examining the association between foreclosures and neighborhood amenities. While the U.S. Daily 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 logit 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 logged foreclosures (zipcode), the unemployment rate (county), and logged housing prices (county), conditional on controls.\textsuperscript{34}

Table 3 documents these results. We find that a 10% rise in foreclosures is associated with a 1.63% and 2.55% decline in the probability that an individual reports being satisfied with their city and that they perceive it is safe to walk alone. Interestingly, a “comparable” ten percentage point rise in the county unemployment rate is associated with a 0.71pp and 0.46pp decline in the probability of reporting satisfaction with the city and safe walking, respectively, which is lower than the estimated gradients on foreclosure. Comparing these coefficients suggests that foreclosure shocks had a larger impact on individual well-being than overall unemployment.\textsuperscript{35} We also find that increases in housing prices are not associated with city satisfaction, but negatively associated with reports of safe walking environments. Put together, foreclosures are a significant determinant of city amenities.

\begin{table}[h]
\centering
\caption{Foreclosures and Local Amenities}
\begin{tabular}{lcccc}
\hline
 & \text{ln(foreclosures)} & \text{unemployment rate} & \text{ln(housing prices)} \\
\hline
\text{(1)} & -.163\textsuperscript{***} & -.071\textsuperscript{***} & .044 \\
 & [.012] & [.003] & [.048] \\
\text{(2)} & -.255\textsuperscript{***} & -.046\textsuperscript{***} & -.116\textsuperscript{***} \\
 & [.009] & [.003] & [.037] \\
\hline
\end{tabular}
\begin{tabular}{lcccc}
\text{Sample Size} & 1482959 & 1393299 \\
\text{Controls} & Yes & Yes \\
\text{Time FE} & Yes & Yes \\
\end{tabular}
\end{table}

Notes.--Sources: CoreLogic and U.S. Daily from Gallup, 2008-2014. The table reports the coefficients associated with logit regressions of indicators on satisfaction with their city and the perception that it is safe to walk alone in the city on logged foreclosures, county unemployment, logged total payments due (on mortgages across the county-quarter), logged bank deposits, logged housing prices, 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.

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

\textsuperscript{34}The 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.

\textsuperscript{35}We are not claiming that job loss has a lower direct effect on individual well-being, but rather providing suggestive evidence that the spillovers for unemployment shocks are lower than those for foreclosures.
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.

While we are upfront with the fact that these are conditional correlations, we can approximate our baseline specification with these logistic regressions by regressing the logged community index on quarterly foreclosure growth, which differences out the time-invariant heterogeneity that might be correlated with city amenities. Doing so produces a coefficient of -0.012 ($p$-value = 0.00). Importantly, we do not see a correlation between either logged foreclosures or the growth rate of foreclosures with alternative indices, like the social well-being index, behaving like a placebo test given that it captures features that might be viewed as correlated with the community index.

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.\footnote{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.} 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.

\subsection*{4.2. Mechanism #2: Uncertainty}

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), financial shocks can affect well-being through heightened risk and/or uncertainty about the future. Using indices on perceptions of current and future economic activity, we run regressions of these variables on logged foreclosures under our baseline instrumental variables specification.\footnote{We also include 2-1 and 3-1 ARMs in these regressions since Gallup does not have full coverage of every county or zipcode. If we restrict the sample and only use 2-1 and 3-1 loans, we have less identifying variation at our disposal. However, we control for logged total bank deposits and total payments due within a county to control for local credit shocks and overall mortgage fluctuations, respectively.}

Table 4 documents these results. We find strong negative gradients between foreclosures and our two measures of perceptions about economic activity. For example, a 10\% rise in foreclosures is associated with a 0.76\% decline in perceptions of current economic activity and a 0.26\% decline in perceptions of future economic activity. The fact that foreclosures affect not just perceptions of current activity, but also future, is important since it is consistent with models where individuals are ambiguous about the business cycle—that is, they do not know the distribution of risk (Ilut and Schneider, 2014). These results are also robust to controlling for income or consumption, much like the earlier robustness exercises in Table 6 in Appendix Section A1.3..

**Table 4: Foreclosures and Perceptions of Current and Future Economic Activity**

<table>
<thead>
<tr>
<th></th>
<th>ln(current economy)</th>
<th>ln(future economy)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(foresclosures)</td>
<td>-0.076***</td>
<td>-0.026**</td>
</tr>
<tr>
<td></td>
<td>[.010]</td>
<td>[.010]</td>
</tr>
<tr>
<td>unemployment rate</td>
<td>-0.004***</td>
<td>-0.004***</td>
</tr>
<tr>
<td></td>
<td>[.001]</td>
<td>[.001]</td>
</tr>
<tr>
<td>ln(housing prices)</td>
<td>-0.095***</td>
<td>-0.041**</td>
</tr>
<tr>
<td></td>
<td>[.018]</td>
<td>[.019]</td>
</tr>
<tr>
<td>R-squared</td>
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<td>.10</td>
</tr>
<tr>
<td>Sample Size</td>
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<td>961919</td>
</tr>
<tr>
<td>Controls</td>
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<td>Yes</td>
</tr>
<tr>
<td>Zipcode FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

*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 indices of 1-4 and 1-3) on logged foreclosures, county unemployment, logged total payments due (on mortgages across the county-quarter), logged bank deposits, logged housing prices, 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 (2-1 ARM, 3-1 ARM, 5-1 ARM, 7-1 ARM, or 10-1 ARM), the interest rate reset (the interest rate at their point of foreclosure net of the interest rate at origination), and their interactions. We aggregate these predicted foreclosures to the 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.
Recognizing that uncertainty is a multi-dimensional term, Appendix Section A1.4. discusses our measure and provides several robustness exercise. First, we show that our measure is highly correlated with both the volatility uncertainty index (VIX) and the Baker et al. (2016) index of policy uncertainty. Second, we present our estimates using the standard deviation of perceptions of uncertainty at the county-by-year level, which has a potentially closer mapping to a “standard” notion of uncertainty as synonymous with dispersion in beliefs. While our results are largely unaltered, our baseline estimates exploit the individual variation since it is much more granular and representative (whereas aggregating to counties involves dropping a subset of counties with too few observations).

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 5 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.

5. Conclusion

While it is now well-known that financial shocks have a large effect on individual well-being, there is no evidence, to our knowledge, on the unique role that foreclosures played during the Great Recession in potentially depressing well-being. Like loss of a job, foreclosures represent an important shock, which can have long-run effects on an individual’s physical and mental state.

Using novel and proprietary data from Gallup’s U.S. Daily, which surveys 1,000 individuals each day, and CoreLogic’s universe of loan data between 2008 and 2014, we examine the causal effect of foreclosures on individual well-being. Our identification strategy leverages heterogeneity in the
Figure 5: 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.
incidence of different types of adjustable rate mortgages (ARMs) across counties. We specifically compare counties with more, for example, 5-1 ARMs with those having more 7-1 and 10-1 ARMs. These ARMs contain a contractually fixed interest rate that changes on the $k$-th year, producing a discontinuously higher or lower probability of foreclosure for an individual. Since some counties have more 5-1 ARMs spiking at one point in time, while others have more 7-1 ARMs spiking, we are able to exploit plausibly exogenous variation in the window of time when one county is experiencing a rise in foreclosures and the other county is not.

Using this quasi-experimental variation in interest rate reset shocks and conditioning on location and time fixed effects, we find that a 10% rise in foreclosures is associated with a 0.53% and 0.23% decline in current and expected future well-being. We also find significant evidence of heterogeneity across states, which is consistent with differences in the length and process requirements for actually implementing foreclosures. Interestingly, we find that states with judicial status laws exhibit stronger foreclosure gradients, suggesting that long drawn-out foreclosure processes can be potentially more harmful. We show that these estimated effects are not driven by direct and/or income effects, but rather represent spillover effects that play out in general equilibrium.

We subsequently examine two candidate mechanisms that can explain these results: (a) a decline in local amenities, and (b) a rise in local economic uncertainty. We find that increases in foreclosures are associated with systematic declines in city satisfaction, which are at least in part explained by perceptions of greater criminal activity. We also find that increases in foreclosures are associated with systematic increases in perceptions of uncertainty about both the current and future state of economic activity. Given that both of these are important inputs to individual well-being, we find our link between uncertainty and well-being of interest to not only a more developed literature on the role of sentiments in business cycles (Angeletos and La’O, 2013), but also an emerging literature on the specific role of beliefs in the formation of expectations about housing market fluctuations (Bailey et al., forthcoming) and leverage (Bailey et al., 2017).

When studying the pass-through of risk from either 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) shocks, the typical approach is to focus on the effects on consumption. A related empirical literature, however, has also begun documenting effects of financial shocks on other outcomes, including mortality (Sullivan and Wachter, 2009; Currie and Tekin, 2015). We join this literature by providing the first evidence on the pass-through of foreclosure shocks onto individual well-being, which is even robust to controlling for income
and/or consumption. Incorporating these types of spatial externalities into quantitative models of the housing market will be an important and exciting area for future research.

References


A Online Appendix (Not for Print)

A1. Supplemental Evidence on the Main Results and Empirics

A1.1. Identification

Our identification strategy exploits plausibly exogenous interest rate resets for different types of ARMs, which are occurring at different points in time across counties. In Figure 3, we illustrate that the probability of foreclosure discontinuously changes when these interest rate spikes following the specified contractual date of the ARM. Figure 3 now plots the interest rate changes that correspond to these discontinuous changes in the probability of foreclosure.
There are two important observations. The first observation is that we have variation across origination years. The first panel with 5-1 ARMs illustrates that there are different magnitudes of interest rate increases based on the origination year, e.g., 2002 and 2003 being increases versus the others, which are decreases. The second observation is that we have variation across interest rate increases and decreases, meaning that our results are not identified based purely on a potentially asymmetric effect that a rise or fall has on foreclosure probabilities. For example, all 7-1 ARMs experienced reset rate declines given span of time between their origination in the early 2000s and the national interest rates following the Great Recession.

A1.2. Examining the Exclusion Restriction

The identifying assumption in the main text is that unobserved shocks to well-being are uncorrelated with changes in interest rate resets on different types of adjustable rate mortgages (ARMs). We restrict our sample to foreclosures induced by 5-1, 7-1, and 10-1 ARMs. Our first test begins by providing evidence on the underlying information that lenders had on borrowers, most notably FICO scores. Figures 7 and 8 plot the distributions of 5-1 & 7-1 and 7-1 and 10-1. Both have almost complete overlap, suggesting that there is no evidence of a major difference in credit worthiness of the two sets of borrowers. We have, however, also examined the robustness of our estimates by including variation in 2-1 and 3-1 loans, but err on the side of caution in using 5-1, 7-1, and 10-1 loans as in Keys et al. (2014).

However, we are also aware of the increasing importance of soft information prior to the Great Recession (Piskorski et al., 2015). To more formally examine the possibility of non-random dispersion in ARMs, we estimate regressions of the form

$$s_{ct}^k = \sum_f \psi^f \text{NumFilers}_{ct} + f(X_{ct}, \beta) + \phi_c + \lambda_t + \epsilon_{ct}$$

(4)

where NumFilers denotes the number of filers in bracket $f$. We measure $f$ through seven income brackets: the number of filers with under $10,000 in annual earnings, between $10-25,000, between $25-50,000, between $50-75,000, between $75-100,000, between $100-200,000, and over $200,000. Our goal in estimating Equation 4 is to examine whether there exists a pattern in correlations between income filers with a particular loan category. For example, if we found that only wealthy earners were associated with 7-1 loans, then we would infer that banks targeted individuals based on their income, undermining our identification strategy.
**Figure 6:** Interest Rate Resets for 5-1 and 7-1 ARMs, by Origination Year

*Notes.* Sources: CoreLogic. The figures plot the median interest rate change, defined as the current interest net of the initial interest rate at origination, for 5-1 and 7-1 adjustable rate mortgages.
Figure 7: Distribution of FICO Scores, 5-1 and 7-1 Adjustable Rate Mortgages

Notes.—Sources: CoreLogic. The figure plots the distribution of FICO scores for 5-1 and 7-1 adjustable rate mortgages.
Figure 8: Distribution of FICO Scores, 7-1 and 10-1 Adjustable Rate Mortgages

Notes.–Sources: CoreLogic. The figure plots the distribution of FICO scores for 7-1 and 10-1 adjustable rate mortgages.
We report our coefficients with and without county and year fixed effects to underscore the importance of controlling for time-invariant factors that are correlated with both the share of hybrid loans and income. Table 5 documents these results. While the conditional correlations in columns 1, 3, and 5 produce some statistically significant associations, once we include country and year fixed effects in columns 2 and 4, for example, the correlations decline heavily. Even if there were stronger correlations in the data, we do not observe any pattern. For example, consider the set of 5-1 loans in column 2. We see that there is a statistically insignificant, but negative, association between individuals with incomes between $25-50,000 and $50-75,000 in counties with these ARMs, but these correlations turn positive when we examine 7-1 loans, and negative again when we examine 10-1 ARMs. In this sense, while there are some precise conditional correlations, there is no pattern in the data consistent with a story of income targeting.

Our second test turns towards potential evidence on the correlation between local shocks between 1990-2000 and the dispersion of ARMs in 2003. (For purposes of simplicity, we focus on 2003 and 2004 since it is prior to the acceleration of these loans in the pre-recession period, but our results hold for other years too.) If a county-level correlation exists, then it is possible that the incidence of these ARMs in the years preceding the housing boom were driven by economic and demographic shifts—that is, they are not quasi-random. Figure 9 documents these correlations for four sets of variables: growth in county-level housing prices, household incomes, unemployment rates, and the share of college graduates. In each case, the gradient is economically and statistically trivial.

A1.3. Robustness Exercises

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 10 plots current (Panels A and C) and future (Panels B and D) life satisfaction growth
Table 5: Examining the Correlation between ARMs and Income Fluctuations

<table>
<thead>
<tr>
<th></th>
<th>type-k ARMs as a share of total hybrid ARMs</th>
<th>5-1</th>
<th>5-1</th>
<th>7-1</th>
<th>7-1</th>
<th>10-1</th>
<th>10-1</th>
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<tbody>
<tr>
<td>ln(filers, under 10K)</td>
<td>.099</td>
<td>.010</td>
<td>.054</td>
<td>-.001</td>
<td>.032</td>
<td>.037</td>
<td></td>
</tr>
<tr>
<td>ln(filers, 10-25K)</td>
<td>-.256</td>
<td>.087</td>
<td>-.061</td>
<td>-.043</td>
<td>-.027</td>
<td>-.055</td>
<td></td>
</tr>
<tr>
<td>ln(filers, 25-50K)</td>
<td>.265</td>
<td>-.038</td>
<td>.102</td>
<td>.077</td>
<td>.039</td>
<td>.043</td>
<td></td>
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<tr>
<td>ln(filers, 50-75K)</td>
<td>.083</td>
<td>-.084</td>
<td>-.033</td>
<td>.062</td>
<td>-.004</td>
<td>-.091</td>
<td></td>
</tr>
<tr>
<td>ln(filers, 75-100K)</td>
<td>-.478</td>
<td>.013</td>
<td>-.097</td>
<td>-.038</td>
<td>-.131</td>
<td>.065</td>
<td></td>
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<tr>
<td>ln(filers, 100-200K)</td>
<td>.371</td>
<td>.049</td>
<td>.094</td>
<td>-.046</td>
<td>.105</td>
<td>.047</td>
<td></td>
</tr>
<tr>
<td>ln(filers, above 200K)</td>
<td>-.015</td>
<td>.015</td>
<td>-.005</td>
<td>-.009</td>
<td>-.005</td>
<td>-.021</td>
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<td>.24</td>
<td>.58</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<td></td>
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<tr>
<td>County FE</td>
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<td>Yes</td>
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Notes.—Sources: Internal Revenue Service, CoreLogic, Census, 2004-2007. The table reports the coefficients associated with regressions of the share of type-\(k\) adjustable rate mortgages (5-1, 7-1, and 10-1 ARMs) relative to total hybrid loans on a semi-parametric measure of logged number of filers by income bracket, conditional on controls. Demographic controls include: the fraction of individuals in the county who are male, married, between ages \(k \in [k, k]\) where the brackets are 0-17, 18-34, 35-64, and 65+ years old, between education \(k \in [k, k]\) where the brackets are less than high school, only high school, some college, college, and graduate school, race (white and black), and logged total population. Standard errors are clustered at the county-level and county population is used as the sample weight.
Figure 9: 5/7/10-1 Adjustable Rate Mortgage Shares and 1990-2000 Growth Rates

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

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.

We now turn towards robustness exercises over the concern that our main effects are driven by income effects. 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 6 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). In both cases, our estimated coefficients of -0.051 and -0.048 are very similar to our baseline estimates. If income effects explained all of the effects, then we would not see variation in life satisfaction after controlling for these direct measures of income.

Our second two columns provide additional indirect tests by controlling instead for the growth in housing prices (rather than the level) and restricting the sample to zipcodes with less than 3% foreclosures per open mortgage. The first test helps capture housing market fluctuations, which may proxy better for income effects at play since credit and housing price changes are so closely connected (Mian and Sufi, 2009). Here, we obtain a gradient of -0.0397, which again is quite close to our baseline estimates. The second test helps mitigate the probability that an individual in the Gallup survey is actually foreclosed upon—that is, that the observed life satisfaction is driven by an actual foreclosure for that specific individual (versus a neighbor). Here, we obtain a gradient of -0.063. While the larger gradient is perhaps counter intuitive since areas with greater foreclosures per open mortgages likely exhibit other time-varying negative shocks, the direction of the bias ultimately depends on the correlation with our vast set of controls.

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
Table 6: Robustness Exercises on the Potential for Income Effects

<table>
<thead>
<tr>
<th></th>
<th>logged current life satisfaction</th>
<th></th>
<th></th>
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</thead>
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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>ln(foreclosures)</td>
<td>-.052***</td>
<td>-.048***</td>
<td>-.040***</td>
<td>-.063***</td>
</tr>
<tr>
<td>unemployment rate</td>
<td>-.002*</td>
<td>-.002***</td>
<td>-.001**</td>
<td>-.001**</td>
</tr>
<tr>
<td></td>
<td>[.001]</td>
<td>[.001]</td>
<td>[.001]</td>
<td>[.001]</td>
</tr>
<tr>
<td>ln(housing prices)</td>
<td>-.089***</td>
<td>-.092***</td>
<td>-.094***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[.020]</td>
<td>[.014]</td>
<td></td>
<td>[.013]</td>
</tr>
<tr>
<td>Δ ln(housing prices)</td>
<td></td>
<td></td>
<td>-.030**</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>[.012]</td>
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<tr>
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<td>.09</td>
<td>.06</td>
<td>.06</td>
</tr>
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<td>1583070</td>
<td>1505436</td>
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<td>Yes</td>
<td>Yes</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Instruments</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes.—Sources: CoreLogic and U.S. Daily from Gallup, 2008-2014. The table reports the coefficients associated with regressions of logged current life satisfaction (from an index of 1 to 10) on logged foreclosures, county unemployment, logged housing prices, 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 thezipcode-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.

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.

A1.4. Measurements of Uncertainty

The main text uses individual-level perceptions of the current and future states of the economy to make statements about a possible mechanism through which foreclosures affect local uncertainty and, therefore, well-being through heightened worry about unemployment and/or earnings risk. We now provide evidence that our measures are genuinely capturing uncertainty and validate them according to more standard definitions.

Figure 12 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. Figure 13 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 year; the sample is restricted to counties with over 50 respondents in each year.

Table 7 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

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38 We also find a correlation of -0.39 when we use the standard deviation of perceptions of the current state of the economy.
Figure 13: 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.
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.39

Table 7: Robustness Exercises on the Measurement of Uncertainty

<table>
<thead>
<tr>
<th>s.d. perception of current economy</th>
<th>s.d. perception of future economy</th>
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<tr>
<td>ln(foreclosures)</td>
<td>(1) (.009***) (.001)</td>
</tr>
<tr>
<td></td>
<td>(2) (.005***) (.002)</td>
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<td></td>
<td>(3) (.023***) (.006)</td>
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<td></td>
<td>(4) (.000) (.003)</td>
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<td></td>
<td>(5) (.005**) (.002)</td>
</tr>
<tr>
<td></td>
<td>(6) (.041***) (.008)</td>
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<tr>
<td>unemployment rate</td>
<td>(1) (-.010***) (.001)</td>
</tr>
<tr>
<td></td>
<td>(2) (-.001) (.001)</td>
</tr>
<tr>
<td></td>
<td>(3) (-.002) (.001)</td>
</tr>
<tr>
<td></td>
<td>(4) (.007***) (.001)</td>
</tr>
<tr>
<td></td>
<td>(5) (-.004**) (.001)</td>
</tr>
<tr>
<td></td>
<td>(6) (.005***) (.002)</td>
</tr>
<tr>
<td>ln(housing prices)</td>
<td>(1) (.026***) (.008)</td>
</tr>
<tr>
<td></td>
<td>(2) (.065**) (.030)</td>
</tr>
<tr>
<td></td>
<td>(3) (-.105***) (.033)</td>
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<tr>
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<td>(4) (-.041***) (.016)</td>
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<td>(6) (-.223***) (.037)</td>
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</tr>
<tr>
<td>Instruments</td>
<td>No No Yes No No Yes</td>
</tr>
</tbody>
</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.

39While the specific quantitative estimates differ slightly from those in the main text, that is not surprising given that the measure is itself different!