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AN IV HAZARD MODEL OF LOAN DEFAULT WITH AN APPLICATION TO SUBPRIME MORTGAGE COHORTS

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ABSTRACT

This paper develops a control-function methodology accounting for endogenous or mismeasured regressors in hazard models. I provide sufficient identifying assumptions and regularity conditions for the estimator to be consistent and asymptotically normal. Applying the estimator to the subprime mortgage crisis, I quantify what caused the foreclosure rate to triple across the 2003-2007 subprime cohorts. To identify the elasticity of default with respect to housing prices, I use various home-price instruments including historical variation in home-price cyclicality. Loose credit played a significant role in the crisis, but much of the increase in defaults across cohorts was caused by home-price declines unrelated to lending standards, with a 10% decline in home prices increasing subprime mortgage default rates by 50%.

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Code implementing the IV hazard model is available at http://github.com/palmercj/ivhazard
and on Stata via ssc install ivcloglog

1 Introduction

Hazard models specify the likelihood that a duration-ending event occurs conditional on it not yet having occurred. This modeling approach is valuable when studying spells such as unemployment that are affected by ongoing shocks, resulting in survivorship bias and censoring.[1] A particularly important application of hazard modeling in finance is estimating loan default models, which are an essential component of underwriting and credit rating, loan and bond pricing, risk management, stress testing, and regulating credit markets. However, often the covariates capturing potential default determinants are mismeasured or correlated with unobservable factors. In this paper, I demonstrate how a control-function estimator that extends the canonical loan default hazard model can account for endogeneity or measurement error in the regressors. Ignoring potential misspecification of the covariates can significantly bias estimated loan-default parameters, prevents accurate attribution of observed defaults to their determinants, and inhibits out-of-sample default prediction.

To demonstrate the utility of combining hazard-model and control-function estimation, I apply the method to characterize the drivers of the subprime foreclosure crisis. Subprime residential mortgage loans were ground zero in the Great Recession, comprising over 50% of all 2006-2008 foreclosures and triggering trillions of dollars of losses in the financial sector despite the fact that only 13% of outstanding residential mortgages were subprime at the time.[2] The subprime foreclosure rate—the number of new subprime foreclosure starts as a fraction of outstanding subprime mortgages—tripled from under 6% in 2005 to 17% in 2009.[3] By 2013, more than one in five subprime loans originated since 1995 had ended in a distressed termination, and by 2017, the five largest banks in the United States had paid a collective $150 billion in fines to settle allegations related to subprime mortgage defaults (Scannell, 2017).

Why did the performance of subprime loans decline so sharply? A focal point of the debate around the crisis has been the observation that subprime mortgages originated in 2005-2007 performed significantly worse than subprime mortgages originated in 2003-2004.[4] This is visible in Figure 1, which uses data from subprime private-label mortgage-backed securities to show this pattern for 2003-2007 borrower cohorts.[5] Each line shows the cumulative fraction of borrowers in the indicated cohort that defaulted within a given number of months from origination.[6] The pronounced pattern is that the speed and frequency of default are higher for later cohorts; within any number

[1]See section 3 for a discussion of the advantages of hazard modeling over standard methods.

[2]Statistics derived from the Mortgage Bankers Association National Delinquency Survey. For the purposes of this paper, subprime mortgages are defined as those in private-label mortgage-backed securities marketed as subprime as in Mayer et al. (2009).

[3]AAA subprime residential mortgage-backed securities—widely held by institutional investors and an important source of repo collateral at the time—had lost 60% of their value by 2009.

[4]For examples contrasting earlier and later borrower cohort default rates, see JEC (2007), Krugman (2007b), Gerardi et al. (2008), Haughwout et al. (2008), Mayer et al. (2009), Demyanyk and Van Hemert (2010), Krainer and Laderman (2011), Bhardwaj and Sengupta (2012, 2014), and Davis et al. (2022).

[5]By 2008, the subprime market was virtually nonexistent—the number of subprime loans originated in 2008 in the securitization data fell by 99% from the number of 2007 originations.

[6]Following Mayer et al. (2009) and Sherlund (2011), I measure default as the first time that its delinquency status is marked as in the foreclosure process or real-estate owned provided it ultimately terminated without being paid off in full, although I show below that my results hold using more sensitive default definitions.

of months since origination, each cohort has defaulted at a higher rate than the one previous to it (with the exception of the 2007 cohort in later years). For example, within two years of origination, approximately 20% of subprime mortgages originated in 2006-2007 had defaulted, in contrast with approximately 5% of 2003-vintage mortgages. Because a disproportionate share of subprime defaults come from later cohorts, understanding why these cohorts performed so poorly informs debate on the causes of the subprime crisis and is important for designing effective policy.

The canonical double-trigger model of mortgage default (Riddiough, 1991) guides the exercise of understanding the cohort pattern. Default in this model is the joint result of cash-flow shocks and negative equity. If there is an increase in ex-ante riskiness across origination cohorts (e.g., from a decline in lending standards, an increase in fraud, or an increase in demand from risky borrowers), an aggregate shock (e.g., to property values or unemployment) could produce a cohort pattern in default rates. However, even without changes in selection, aggregate shocks will have a stronger incidence on recent borrowers because they are more likely to have negative equity. The extent to which the cohort pattern was generated by ex-ante selection or differential exposure to circumstances is an important input into both ex-ante and ex-post policies (e.g., macroprudential credit market regulation and loan modification programs, respectively). On the selection side, a popular explanation for the increase in cohort default rates over time is that loosening lending standards led to a change in the composition of subprime borrowers, potentially on both observable (e.g., JEC, 2007; COP, 2009) and unobservable dimensions (Keys et al., 2010; Rajan et al., 2015). Relatedly, an important source of unobserved credit quality declines across cohort appears to have been the positive correlation between fraud and default (Ben-David, 2011; Piskorski et al., 2015; Griffin and Maturana, 2016). Others blame an increase in the popularity of non-traditional mortgage products (Krugman, 2007a), some arguing that if distressed borrowers had less exotic mortgage products, their distress wouldn't have happened in the first place (Bair, 2007). These selection-based explanations are consistent with the observed heterogeneity in cohort-level outcomes seen in Figure 1, which could be generated by deteriorations in borrower creditworthiness or mortgage product characteristics, and motivate policies that place restrictions on allowable mortgage contracts.

On the circumstances side, however, house prices declined nationally by 37% from 2005-2009 and the unemployment rate increased from 4.5% to 10% from 2007-2009. Borrowers who can no longer afford their mortgage payments can sell their homes or refinance *if* they have sufficient equity. Such alternatives are generally unavailable to distressed underwater homeowners—lenders are often reluctant to refinance underwater mortgages or allow short sales (where the purchase price is insufficient to cover liens against the property). Even if aggregate shocks (e.g., labor-market shocks) affect borrowers uniformly, house price declines should therefore differentially affect more recent cohorts, who have accumulated less equity when property values decline.

The counterfactual question I pose is what cohort default rates would have been if the better-performing early cohorts had instead faced the housing-market conditions experienced by later cohorts. Conversely, how strong would the cohort pattern have been if the composition of borrowers and loans hadn't changed across cohorts? If 2003 borrowers would have mimicked the performance of

2006 borrowers if they hadn't experienced mid-2000s home price appreciation—or if 2006 borrowers would have had similar default patterns even with 2003 borrower characteristics—then this limits the scope of mortgage-lending regulation to select resilient borrowers who can withstand significant home price shocks.

To answer these questions, I apply the semiparametric control-function hazard model estimator developed below using a panel of subprime loans that combines borrower and loan characteristics with monthly updates on loan balances, imputed property values, default timing, and local price changes. However, measuring default elasticities with respect to asset prices presents an identification challenge because housing prices are an equilibrium outcome that depend on other factors related to default risk; the potential for both price changes and defaults to be caused by a third factor may induce bias. In other words, some of the sources of price shocks may also have direct effects on the (static) unobserved quality of borrowers or the (dynamic) subsequent economic environment faced by borrowers and hence on defaults. For example, subprime penetration itself may subsequently have caused price declines and defaults: a credit expansion could amplify the price cycle, initially increasing prices from the positive demand shock as the pool of potential buyers grows.[7] However, the decrease in average borrower quality from the credit expansion could eventually lead to an increase in defaults, accelerating price declines (Kermani, 2012). Thus, even though individual borrowers are price takers in the housing market, a cohort's average unobserved quality may be correlated with the magnitude of the price declines its borrowers face, biasing estimates of the causal effect of prices on default risk. Likewise, a simple reverse causality story—defaults cause price declines—could bias default elasticity estimates. These endogeneity challenges complicate identifying the sources of default-rate differences across cohorts.

To isolate the portion of cohort default rates causally driven by price changes, I exploit several sources of plausibly exogenous variation in home prices. First, as observed by Sinai (2013), there is persistence in the amplitude of home-price cycles—cities with strong price cycles in the 1980s were more likely to have strong cycles in the 2000s. I use this historical variation in local home-price cyclicality as an omnibus measure proxying for all of the features of a city that make its housing market more prone to larger boom-bust cycles. Using this historical cyclicality measure, I construct counterfactual price indices, which, crucially, are unrelated to housing market shocks unique to the 2000s price cycle—intuitively because price volatility in the 1980s occurred well before the widespread adoption of subprime mortgages. Indeed, I show below that my instrument does not predict differential subprime expansion. I also verify that my results are robust to alternative instruments for local house prices, including local sensitivities to regional housing-market shocks, belief shocks to a given housing market from its social network, a predictor of local housing supply inelasticity, and exposure to local publicly traded companies.

I find that differential exposure to price declines explains about 60% of the heterogeneity in cohort default rates. I also estimate that the changing product characteristics of subprime mortgages

[7] See Mayer and Sinai (2009), Mian and Sufi (2009), Pavlov and Wachter (2011), Di Maggio and Kermani (2017), and Justiniano et al. (2019). A parallel literature uses country-level data to show the simultaneity of asset-price and credit bubbles (e.g., Jorda et al., 2011).

(and correlated changes in unobservable borrower quality) play an important role, accounting for 30% of the rise in defaults across cohorts. Conditioning on price changes and loan and borrower characteristics explains almost the entire deterioration in cohort-level default rates, suggesting that the model captures the cohort pattern well. My counterfactual simulations predict that if 2003 borrowers had faced the prices that the average 2006 borrower did, 2003 borrowers would have defaulted more than twice as frequently as they did, at an annual default rate of 17% instead of 8% ceteris paribus. These results call into question the practice of inferring the success or failure of lending-standards regimes from cohort-level outcomes (see section 2 for a discussion of the complications caused by vintage effects in credit modeling). Overattribution of a cohort pattern to dynamics in lending practices could lead to an overreliance on tighter lending rules as a policy response. Similarly, using the low rate of foreclosures for cohorts originated since the crisis as evidence that stronger mortgage regulation was a success would overlook the likely role of the house price recovery in explaining much of that improvement.

The paper proceeds as follows. I contextualize the paper in the context of literatures on hazard modeling, loan defaults, and the subprime mortgage crisis in section 2. Section 3 presents the identification concerns triggered by endogenous covariates in the context of a hazard model, along with a description of how my estimator solves these challenges. Section 4 discusses the empirical application to subprime mortgages. I describe the data and compare the observable characteristics of borrower cohorts in section 5. Section 6 presents estimates of the determinants of cohort default rates, including using instrumental-variables control-function hazard regressions. Using my preferred empirical specification, I estimate cohort-level default rates under several counterfactual scenarios in section 7. In section 8, I conclude by summarizing my main findings and briefly discussing policy implications. The appendix covers theoretical results and several additional robustness exercises and provides evidence on mechanisms.

2 Literature Review

In this section, I explain my contribution to both the econometrics and statistics literatures on hazard modeling and the finance literature on loan default and the subprime mortgage crisis.

Hazard Model Literature The econometrics literature on hazard model identification usually assumes exogenous, correctly specified covariates and instead focuses on the conditions necessary for nonparametric identification (e.g., Elbers and Ridder, 1982; Han and Hausman, 1990; Abbring and van den Berg, 2003b). Other work in economics uses competing risks models—a form of hazard modeling with multiple failure types—to study unemployment or mortgage default (e.g., Heckman and Honoré, 1989; Meyer, 1990; Han and Hausman, 1990; McCall, 1996; Deng et al., 2000), again assuming covariate exogeneity and no measurement error. A literature in statistics and biostatistics examines the implications of functional form misspecification or classical covariate measurement error on duration-model inference (e.g., Prentice, 1982; Lin and Wei, 1989; Huang and Wang, 2000, 2006, 2018; Li and Ryan, 2004; Song and Wang, 2014; Wang and Song, 2016). Another collection of papers studies instrumenting in duration models (Abbring and van den Berg, 2003b, 2005; Bijwaard, 2009; Terza et al., 2008; Atiyat, 2011; Wan et al., 2015; Kianian et al., 2021) but lacks ingredients essential to typical applications of hazard models in economics and finance (e.g., time-varying covariates, continuous instruments and treatments, discretely measured durations and covariates, and right-censoring).

In contrast to these literatures, this paper demonstrates how to use instrumental variables to address potential covariate endogeneity in hazard models. Correlation between included covariates and omitted variables potentially prevents causal inference of parameters that could otherwise be used to simulate counterfactuals. I show how to apply control-function estimation (Heckman and Robb, 1985; Newey, 1987) to semiparametric hazard models, illustrating how to diagnose and relax deviations from the distributional assumptions that underlie control function estimators. The loan-default application showcases some of the distinguishing features of the model relative to the prior literature: loan performance and prices are measured monthly instead of continuously, some loans are outstanding in the data (right-censored durations), and important default factors are continuous (e.g., price changes).[8]

Loan Default Literature Many loan default models used in asset pricing are based on the assumption that borrowers optimally exercise a loan's embedded default and prepayment options.[9] However, when researchers want to empirically test the assumption of the options-based framework or to allow for deviations therefrom, they use hazard models (e.g., Schwartz and Torous, 1989; Deng et al., 2000; Fuster and Willen, 2017).[10] Building on the usual hazard approach, I demonstrate how estimating the loan-termination model at the core of many MBS pricing models can be extended to allow for covariate endogeneity or measurement error along with a nonparametric baseline hazard function and unobserved heterogeneity.

More broadly, there is an extensive empirical literature on the determinants of mortgage default—see Foote and Willen (2018) for a review. While many of these papers consider the role of negative equity, this paper is the first to instrument for home prices in a hazard model to address the joint endogeneity of local home price declines and defaults.[11] For example, the common practice of imputing changes in property values using a metropolitan area home price index, although arguably purged from property-specific price shocks, does not address the potential concern that price changes at the metropolitan-area level are themselves the outcome of local demand and supply shocks that are plausibly correlated with unobserved local borrower quality.[12] Using a control-function approach

[8] See also a related approach by Carson et al. (2020), who use a control-function Cox model to account for endogenous selection in life insurance take-up.

[9] For influential early examples, see Findley and Capozza (1977), Dunn and McConnell (1981), Foster and Van Order (1984), and Epperson et al. (1985).

[10] Discrete choice models can also be used to characterize the determinants of default. For example, Haughwout et al. (2008), Rajan et al. (2015), and Clapp et al. (2006) use linear-probability, logit, and multinomial logit models, respectively. As I discuss in section 3, hazard models are particularly well suited to the analysis of time-varying covariates and to samples that contain still-outstanding loans.

[11] For work on negative equity and default, see Foote et al. (2008), Pennington-Cross and Ho (2010), Goodman et al. (2010), Campbell and Cocco (2015), Bhutta et al. (2017), Fuster and Willen (2017), Gerardi et al. (2018), Low (2018), Schelkle (2018), and Ganong and Noel (2020, 2023).

[12] Valuable exceptions to the treatment of home equity as exogenous include Gupta (2019) and Gupta and Hansman

to account for the endogeneity of covariates in a hazard model setting, I confirm that prices are endogenous, they are an important determinant of default, and they account for over half of the cohort pattern in default rates.[13]

Motivating the need to scrutinize a model's identification strategy when prices are a covariate, several literatures in both macroeconomics and finance document how price levels both depend on and affect lending conditions—see Mian and Sufi (2018) for a review. In financial accelerator models (e.g., Kiyotaki and Moore, 1997; Bernanke et al., 1999; Iacoviello, 2005), shocks to housing prices loosen collateral constraints, expanding credit. More recently, Foote et al. (2012), Favilukis et al. (2017), Justiniano et al. (2019), Kaplan et al. (2020), and Greenwald and Guren (2021) study the co-determination of price beliefs, credit conditions, house prices, and mortgage default in equilibrium. Jorda et al. (2015) assemble a historical panel of house prices and credit conditions across countries and find credit shocks have strong predictive power for house-price cycles. In finance, several studies document effects of credit conditions on prices in cross-sections of borrowers and regions (Adelino et al., 2012; Glaeser et al., 2013; Favara and Imbs, 2015; Di Maggio and Kermani, 2017; Argyle et al., 2021). Taking this evidence of the joint endogeneity of prices and credit quality seriously, I develop instruments to isolate home-price variation plausibly unrelated to credit conditions and demonstrate how to address the endogeneity of any covariate in a hazard model.

Accounting for endogenous covariates also helps address the important challenge of vintage effects in credit modeling (Haughwout et al., 2017), which frustrate counterfactual mortgage performance forecasting and stress testing (Foote and Willen, 2018). Etymologically, the term vintage effect suggests static variation across loan cohorts in the conditions at origination. However, a plausible source of eventual credit performance differences across vintages is age-dependent sensitivity to ongoing shocks.[14] Accordingly, the exercise of decomposing vintage effects into lending standards and subsequent economic conditions is complicated by the correlation between unobserved cohort quality and exposures to shocks that potentially bias estimated vintage effects. In the case of subprime mortgage defaults, the corresponding concern is that home-price declines themselves were partially caused by loose lending standards (Di Maggio and Kermani, 2017), preventing credible inference about the degree to which lending standard changes explain the default wave. As I illustrate below, using an instrumental-variables hazard model to account for endogenous covariates permits unbiased estimation of default-model coefficients by isolating variation in home prices unrelated to lending standards. The resulting estimates can then be used to determine the drivers of vintage effects.

(2022), who use shocks to otherwise similar interest-rate benchmarks to isolate variation in mortgage payments and balances, respectively.

[13] As an example of how this methodology has been useful in the literature, Ganong and Noel (2023) follow the control-function strategy developed here to instrument for time-varying leverage with the historical cyclicality instrument for house prices used below and find similar effects of negative equity on default using a sample of prime mortgages and including later vintages.

[14] For example, younger cohorts of student-loan borrowers are more sensitive to labor-market shocks (Looney and Yannelis, 2015).

Subprime Crisis Literature The empirical application in this paper also contributes to the literature studying the subprime foreclosure crisis' proximate causes (e.g., Mian and Sufi, 2009; Hubbard and Mayer, 2009; Keys et al., 2010; Dell'Ariccia et al., 2012; Nadauld and Sherlund, 2013; Ferreira and Gyourko, 2015; Adelino et al., 2016). Several papers have estimated the relative contributions of subprime underwriting standards and housing market conditions in the increase in the subprime default rate over time (e.g., Bajari et al., 2008; Sherlund, 2011; Kau et al, 2011; Bhardwaj and Sengupta, 2012, 2014; Corbae and Quintin, 2015). In contrast to the set of valuable post-mortem studies on the subprime foreclosure crisis that traditionally takes imputed negative equity levels as given and treats metropolitan area home price changes as exogenous, I explicitly consider how to isolate variation in prices and current leverage unrelated to the contemporaneous housing boom and bust. As I discuss in Appendix D, the observation that price momentum is a more powerful predictor of foreclosure than current leverage is consistent with the literature on the role of beliefs in housing and mortgage markets and on housing-market liquidity being positively correlated with prices.[15]

Finally, this paper's methodology and findings are particularly relevant to analysis of differences in default across borrower cohorts. Haughwout et al. (2008) and Mayer et al. (2009) document that loosening downpayment requirements and declining home prices are both highly correlated with increases in early defaults across cohorts. Demyanyk and Van Hemert (2011) consider vintage effects in borrower quality and find that that the bulk of the early deterioration in vintage quality was due to unobservables. Krainer and Laderman (2011) and Bhardwaj and Sengupta (2012, 2014) find that prepayment rate declines across cohorts mirror cohort default rate increases. Lam et al. (2013), Ferreira and Gyourko (2015), and Haughwout et al. (2017) document vintage effects in non-subprime mortgages. Davis et al. (2022) construct a counterfactual exercise similar to section 7 below to study prime cohort performance holding constant mortgage product and borrower characteristics. These papers all generally find significant unexplained quality differences across cohorts and ignore both the endogeneity of prices and the measurement error from imputing property values with price indices. In contrast, I isolate quasi-exogenous variation in price changes and use a specification that explains nearly all of cohort-level heterogeneity in default rates. I also demonstrate how a borrower's current equity position is not a sufficient statistic for the role of price declines on loan performance, showing that the cohort default pattern is more successfully explained by recent price declines than a borrower's current leverage.

In summary, existing work in economics and statistics on hazard models has not dealt with covariate endogeneity. I demonstrate how to use a control-function approach in a hazard model, including its identifying assumptions and how to relax them and how to conduct inference. Although robust literatures in macroeconomics and finance establish the joint endogeneity of credit conditions and housing prices, empirical work on mortgage default has mostly taken price levels and negative equity positions as given. However, in a world where price declines are themselves caused by lending

[15] Extrapolative beliefs and falling housing prices can spook potential buyers, further depressing prices and exacerbating illiquidity (Glaeser and Nathanson, 2017; Gennaioli and Shleifer, 2018; Armona et al., 2019; Bailey et al., 2019; Liu and Palmer, 2021).

activity because of the feedback between asset and credit markets, these results are somewhat hard to interpret without testing the robustness of their conclusions to addressing price endogeneity. In contrast to these papers, I apply my methodology to separate the effect of price changes from the effect of borrower composition on cohort-level default rates and explain virtually all of the default pattern across subprime borrower cohorts.

3 Hazard Model, Identification, and Estimation

In this section, I discuss assumptions sufficient for identification, introduce a control-function strategy when assumptions around the exogeneity and measurement of covariates are not met, and explain how to conduct consistent estimation and inference.

There are two main advantages to hazard modeling over specifications that do not condition on an event not yet having occurred, both related to sample selection concerns. First, researchers often seek to estimate the effects of time-varying covariates on the likelihood an event occurs. Studying the effect of time-varying covariates on duration data creates an unbalanced panel where the sample's observations disproportionately represent individuals with long durations. Second, economic data often contain many spells whose durations have not yet ended, resulting in censored observations where the econometrician only observes a lower bound on the duration. Besides censoring caused by a sample being collected before all durations have ended, a common form of censoring arises when there are multiple failure types. When a duration ends because of one type of failure (e.g., loan prepayment), this censors the observability of the duration until another type of failure (e.g., loan default)—known as a competing risks model. Specifying the problem as a hazard model directly accounts for both forms of sample selection.

The workhorse hazard model in econometrics and statistics is a proportional hazard model where the effects of covariates multiply a baseline hazard function. For this paper's application, I specify the duration from loan origination until default as a proportional hazard model with time-varying covariates and a nonparametric baseline hazard function. Although loan-performance data are often grouped into monthly observations, the proportional hazards functional form estimates a continuous-time hazard model using discrete data (Prentice and Gloeckler, 1978; Allison, 1982)—see further discussion of interval censoring below.[16]

Let the latent continuous time-to-default random variable be denoted τ, and let the instantaneous probability of a borrower defaulting at month t given that the borrower has not yet defaulted be specified as

$$\lim_{\xi \to 0^+} \frac{\Pr\left(\tau \in (t-\xi,\, t] \middle| \tau > t-\xi\right)}{\xi} \quad \equiv \quad \lambda(X(t),\, t) \tag{1}$$

[16]The Cox (1972) partial likelihood hazard model is particularly popular because it allows conditioning out the baseline hazard function to avoid its specification or estimation. While estimation using the Cox model is likely sufficiently reliable in many applications, the approach I use here has three main advantages over the Cox model. The model below does not require any special treatment for "ties" in the data (multiple observations with the same duration) as in the Cox model and allows for nonparametric estimation of the baseline hazard function.

$$= \exp(X(t)'\beta)\lambda_0(t) \quad (2)$$

where $\lambda_0(\cdot)$ is the baseline hazard function that depends only on the time since origination t, and $X(t)$ is a vector of time-varying covariates that in practice will be measured at discrete monthly intervals. The proportional hazards framework in (2) assumes that the conditional default probability depends on the elapsed duration through a baseline hazard function that is shared by all loans and is scaled up and down by covariates to capture the effects of observable individual heterogeneity. A convenience of this framework is that the coefficient vector β is readily interpretable as measuring the effect of the covariates on the log hazard rate. That is, β_j captures the proportional increase in the likelihood of default conditional on not yet having defaulted associated with a one unit change in the j^{th} element of X holding all other covariates fixed. When some covariates in X are potentially measured with error or plausibly covary with omitted default factors not included in X, econometricians can better interpret the magnitude of estimated coefficients $\hat{\beta}$ when accounting for such endogeneity with a control function, as discussed below.

3.1 Identification

The proportional hazard model is identified—implying that the population objective function is uniquely maximized at the true parameter values—under the assumptions that 1) conditional on current covariates, past and future covariates do not enter the hazard (often termed strict exogeneity), and 2) any sample attrition is unrelated to the covariates (Lancaster, 1979; Wooldridge, 2002).[17] Stated in terms of the conditional distribution $F(\cdot|\cdot)$ of failure times τ, the strict exogeneity and non-informative censoring assumptions are met provided the following assumption holds.

Assumption 1 (Strict Exogeneity and Non-informative Censoring)**.** *The distribution $F(\cdot|\cdot)$ of duration τ until default conditional on the full vectors of time-varying covariates X and censoring indicators c satisfy the following relationship*

$$F\left(\tau \middle| \tau > t-1, \{X(s), c(s); 0 \leq s \leq T\}\right) = F(\tau|\tau > t-1, X(t)) \quad (3)$$

where $X(s)$ is a vector of covariates measured at time s and $c(s)$ is an indicator for whether a given loan's duration was censored at time s.

This standard assumption in hazard modeling requires that only contemporaneous variables matter, any censoring is independent, and individual observations are independent conditional on the covariates. In principle, if lags or leads of the covariates enter into λ, the strict exogeneity condition can be satisfied by including them as explanatory variables in the vector $X(t)$.

An important form of censoring in loan-performance data arises from borrowers prepaying their loans in full. Loans that have been prepaid are treated as censored because all that can be learned about their latent time until termination by default is that it would have been at least as long as the observed elapsed time until prepayment. Technically, any such hazard model with multiple failure

[17] The linear-index functional form assumption that the effect of covariates on the hazard is linear in logs is not necessary for identification and is made for the sake of parsimony and convenience in interpreting the coefficients.

types is a competing risks model, which can be generalized to accommodate the potential dependence of one risk on shocks to another. Under the assumption there is no unobserved individual heterogeneity in the default hazard (or that unobserved heterogeneity in the default and prepayment hazards are independent at the individual level), competing risks models can be estimated as separable hazard models with observations representing other failure types treated as censored.[18] As in Gerardi et al. (2008), Sherlund (2011), Foote et al. (2010), and Demyanyk and Van Hemert (2011), I adopt this approach and focus on estimation of the default hazard.[19] In Appendix B, I further validate this independent competing risks approach by verifying that my main results are unaffected by allowing for unobserved heterogeneity in the default hazard.

To estimate causal relationships, the key identifying assumption for the estimated coefficients in equation (2) to be interpretable as the causal effect of the regressors is that the covariates must be independent of unobserved shocks to default risk. For example, to estimate the causal effect of collateral value changes on default, fluctuations in collateral values need to be unrelated to any other change in default risk not captured by the other regressors. The exogeneity condition necessary for the maximum likelihood estimates of the hazard model parameters to represent causal effects is that the probability of failure (conditional on reaching a given period) is correctly specified by $\lambda(X,t)$ in (2) and the functional form of how X enters the hazard function. Assumption 2 states this necessary covariate exogeneity assumption formally. Combined with Assumption 1, estimates of β will represent the partial effects of X on the conditional likelihood of failure under these two assumptions.

Assumption 2 (Proportional Hazards)**.** *The hazard $\lambda(\cdot,t)$ specified in (2) satisfies the following relationship with the expectation of an indicator for failure at duration τ conditional on covariates X and not having yet failed*

$$\lim_{\xi\to 0^+} E\left[\frac{1\left(\tau\in(t-\xi,t]\right)}{\xi}\Big| X,\, \tau>t-\xi\right] \equiv \lambda(X,t) = \exp(X'\beta)\lambda_0(t), \tag{4}$$

where $1(\cdot)$ is the indicator function.

This condition would be violated if there were an omitted factor ω which affects failure risk and is not independent of X. However, unlike classic omitted variables bias in a linear model, even independent ω would bias estimates of β, as in many nonlinear models. In either case, unobserved heterogeneity leads to violation of the proportional hazards assumption in (2) because ω affects failure, is not included in λ, and survives conditioning on X or $\tau>t-\xi$. To see this, suppose that the true instantaneous conditional probability of default is not $\lambda(X,t)$ but is $\tilde{\lambda}(X,\omega,t)$ where

$$\tilde{\lambda}(X,\omega,t) = \exp(X'\beta+\omega)\lambda_0(t). \tag{5}$$

[18] See Heckman and Honoré (1989) and Abbring and van den Berg (2003a) for a full discussion of identification in competing risks models.

[19] The best-known example of allowing for correlated default and prepayment unobserved heterogeneity is Deng et al. (2000), who jointly estimate a competing risks model of mortgage termination using the mass-points estimator of McCall (1996).

In this case, the condition in (4) becomes to

$$\begin{aligned}\lim_{\xi\to 0^+} E\left[\frac{1\,(t-\xi<\tau\leq t)}{\xi}-\lambda(X,t)\middle|X,\tau>t-\xi\right] &= E\left[\tilde{\lambda}(X,\omega,\tau)\middle|X,\tau\geq t\right]-\lambda(X,t)\\ &= \exp(X'\beta)\lambda_0(t)\left(E\left[e^{\omega}|X,\tau\geq t\right]-1\right)\\ &\neq 0\end{aligned}$$

When ω and X are not independent then the omission of ω from (2) leads to a violation of equation (4) because $E(e^{\omega}|X,\tau\geq t)$ depends on X, the proportional hazards assumption fails, and the estimated $\hat{\beta}$ will be inconsistent for the marginal effect of X on default. However, even when ω and X are unconditionally independent, conditioning on $\tau\geq t$ induces a conditional correlation between ω and X. Similar to how selecting on the dependent variable in a linear regression induces a correlation between observed and unobserved variables (because high values of y are likely to have both high X and high ε), long-surviving units selected by conditioning on $\tau\geq t$ are more likely to have high $X'\beta$ and high ω. Failing to control for ω in estimating this model results in estimated coefficients $\hat{\beta}$ not converging to the marginal effect of X on the log hazard and instead combining both the direct effect of X on default and the indirect effect of ω on default after projecting onto X and $\tau\geq t$. While independent unobserved heterogeneity can be addressed with random effects, known in this setting as the mixed proportional hazard model estimated in Appendix B, correlated omitted variables require instruments, as developed in section 3.2.

A special case of Assumption 2 being violated is when components of X are measured with error such that a variable x^* is not observed and instead the econometrician observes $x=x^*+v$. As in linear models, this measurement error can cause attenuation bias in hazard models (Prentice, 1982; Li and Ryan, 2004; Song and Wang, 2014). While alternative measures of a mismeasured covariate are suitable instruments to address the errors-in-variables problem, such instruments may not meet the conditions necessary to address covariate endogeneity. An instrument that addresses covariate endogeneity, however, will also address covariate measurement error using the approach below.

3.2 Control-Function Approach

To address violations of Assumption 2 arising from omitted variable correlated with observable variables, I use the control-function approach (Heckman and Robb, 1985; Newey, 1987). Control-function estimators condition on an approximation of the endogeneity in the endogenous explanatory variables. Labeling the right-hand side endogenous variables x, let their reduced-form (equivalent to the 2SLS first stage) be specified as

$$x = Z_1\Pi_1 + Z_2\Pi_2 + v, \tag{6}$$

where Z_1 is a vector of excluded instruments, and Z_2 is a vector containing the included right-hand side exogenous controls such that $X=[x \quad Z_2]$.[20] As discussed above, the misspecification arises because of some omitted variable ω. However, the identifying assumption behind control-function

[20] The control-function approach is best suited to endogenous covariates that are continuous (or are functions of observed continuous variables). For a LIML approach applicable to discrete covariates, see Wooldridge (2014).

estimators is that conditioning on the control function $c(v)$ restores the ability to estimate the partial effect of X. Formally, the replacement for Assumption 2 is

Assumption 3 (Control Function Specification)**.** *The hazard $\lambda(\cdot,t)$ specified in (2) satisfies the following relationship after conditioning on the control function $c(v)$ with the expectation of an indicator for failure at duration τ conditional on covariates X and not having yet failed*

$$\lim_{\xi\to 0^+} E\left[\frac{1\left(\tau\in(t-\xi,t]\right)}{\xi}\Big| X,\,\omega,\,\tau>t-\xi\right]=\lambda(X,c(v),t)=\exp(X'\beta+c(v))\lambda_0(t), \tag{7}$$

where $1(\cdot)$ is the indicator function.

Assumption 3 requires that the control function $c(\cdot)$ itself is known. While control-function practitioners often assume a linear form, an arbitrary $c(\cdot)$ can be approximated and estimated semiparametrically with series or splines. As a robustness check, Appendix C considers third- and fifth-order polynomial approximations to $c(\cdot)$ and finds that the results are insensitive to this semiparametric flexibility.

Identification further requires the following two familiar assumptions on the relevance and exogeneity of the instruments Z_1 and included controls Z_2.

Assumption 4 (Instrument Relevance)**.** *The reduced-form relationship for endogenous variable x given instruments Z_1 and included controls Z_2 is linear, and the coefficients Π_1 on Z_1 in the first-stage equation satisfy $\Pi_1\neq 0$. Moreover, there are at least as many instruments as endogenous explanatory variables such that* $\operatorname{rank}\{E(Z_1'x|Z_2)\}\geq \dim x$.

Assumption 5 (Instrument Exogeneity)**.** *The set of instruments $Z=[Z_1 \quad Z_2]$ consisting of excluded instruments Z_1 and controls Z_2 is independent of (v,ω), where v are the first-stage residuals and ω are the variables omitted from the hazard equation.*

Assumption 4 is the usual relevance condition that the instruments Z_1 have sufficient predictive power for x, X is full rank, and there are more instruments than endogenous covariates.[21] If there is only a weak first stage in the sense that $\Pi_1\approx 0$, then conditioning on v and Z_2 will absorb all of the variation in x, and β will not be identified.[22] Assumption 5 is the key instrument exogeneity condition that the instruments Z_1 and controls Z_2 are independent from v and ω. In practice, empiricists replace v with a consistent estimate $\hat{v}$. Assumption 5 also motivates the estimation of v by OLS as the most parsimonious estimator imposing the orthogonality of Z and v.

I use these identification conditions to state a formal consistency theorem below after introducing the quasi-ML estimator in section 3.3.

[21] In models with linear second stages, Kelejian (1971) and Angrist and Pischke (2009) recommend estimating a linear first stage even when the true relationship must be nonlinear, as with a discrete endogenous variable. However, allowing for a discrete endogenous explanatory variable using the control-function method with a nonlinear second stage requires stronger assumptions (Wooldridge, 2014).

[22] The statement of Assumption 4 ignores any weak-instruments issues where $\Pi_1\neq 0$ but the relationship is not significantly strong (Stock et al., 2002). However, the first-stage F-statistics in the empirical application below suggest weak instruments are not a concern in my setting.

3.3 Estimation and Inference

Arranging the data into a monthly panel with a dependent variable $default_{icgt}$ equal to one if existing mortgage i defaulted in month t, the likelihood $h(t)$ of observing failure for a given monthly observation must take into account the sample selection process. Namely, loans are not observed after they have defaulted such that the likelihood of sampling a given month is a discrete hazard, which conditions on failure not having yet occurred. Suppressing dependence on X momentarily for ease of exposition, the discrete hazard can be derived from the continuous density of default time as

$$\begin{aligned} h(t) &\equiv \Pr(default_{icgt} = 1) \\ &= \Pr(\tau \in (t-1,\, t] \big| \tau > t-1) \\ &= (F(t) - F(t-1))/S(t-1) \\ &= 1 - S(t)/S(t-1) \end{aligned}$$

where $F(\cdot)$ is the cumulative density of τ, the random variable representing mortgage duration until failure, and $S(\cdot) = 1 - F(\cdot)$ is the survivor function, the unconditional probability that observed mortgage duration exceeds the given amount of time. Using the identity that $S(t) = \exp(-\Lambda(t))$, where $\Lambda(\cdot)$ is the integrated hazard function $\Lambda(t) = \int_0^t \lambda(\tau)d\tau$, I can rewrite the discrete likelihood of observing failure for a given observation as

$$\begin{aligned} h(t\big|X) &= 1 - \exp(-\Lambda(t) + \Lambda(t-1)) \\ &= 1 - \exp\left(-\int_{t-1}^{t} \exp(X(\tau)'\beta)\lambda_0(\tau)d\tau\right) \end{aligned} \quad (8)$$

using the proportional hazards assumption from (2). I incorporate the control function approach into the log likelihood (10) in two steps by first estimating (6) to estimate residuals $\hat{v} = x - Z_1\hat{\Pi}_1 - Z_2\hat{\Pi}_2$ and then augmenting X_{icgt} in (10) with $\hat{v}$ or, in robustness-checks, a higher-order polynomial of $\hat{v}$ to approximate potentially nonlinear $c(v)$. If time-varying covariates are constant within each discrete time period (for example if the observed value of X_t represents the average of $X(\tau)$ for $\tau \in (t-1,\, t]$),

$$h(t\big|X) = 1 - \exp\left(-\exp(X_t'\beta)(\Lambda_0(t) - \Lambda_0(t-1))\right) \quad (9)$$

where $\Lambda_0(\cdot)$ is the integrated baseline hazard $\Lambda_0(t) = \int_0^t \lambda_0(\tau)d\tau$.

Incorporating this likelihood of observing $default_{icgt} = 1$, each month $\times$ loan observation's contribution to the overall sample's log-likelihood is

$$\ell_{icgt} = default_{icgt} \cdot \log(h(t|X_{icgt})) + (1 - default_{icgt}) \log(1 - h(t|X_{icgt})). \quad (10)$$

Note that this framework with time-varying covariates readily accommodates prepaid loans that are essentially censored observations on duration until default. I can then estimate the hazard model parameters of equation (2) by Quasi-Maximum Likelihood in a Generalized Linear Model framework where the link function $G(\cdot)$ satisfying $E(default_t|X_t) = h(t|X_t) = G^{-1}(X_t'\beta + \psi_t)$ is the complementary log-log function

$$G(h(t)) = \log(-\log(1 - h(t))) = X_t'\beta + \underbrace{\log(\Lambda_0(t) - \Lambda_0(t-1))}_{\psi_t}. \tag{11}$$

Estimating coefficients ψ_t on a full set of dummies in event-time t allows for the baseline hazard to be nonparametric (Han and Hausman, 1990).[23] Under some additional technical regularity conditions, estimates of β will be consistent and asymptotically normal, as stated formally by Theorem 1.[24]

Theorem 1 (Consistency and Asymptotic Normality of IV Hazard Model Estimator)**.** *Under Assumptions 1, 3-5, and the standard regularity conditions in Assumption A1 in Appendix A, quasi-maximum likelihood estimates of $\hat{\beta}$ in equation (7) obtained by maximizing the sum of the log likelihoods given by (10) will be consistent and asymptotically normal estimates of β.*

Proof. See Appendix A. □

3.3.1 Inference

Although the resulting quasi-ML estimates correct for potential endogeneity in or mismeasurement of covariate x, the usual MLE standard errors will usually be biased downwards by failing to take into account the uncertainty in $\hat{v}$ (Newey, 1987).[25] Again, consistently estimating Π in a first stage to generate $\hat{v}$ does not affect the consistency of the hazard-model parameters. However, by treating $\hat{v}$ as fixed and failing to account for the correlation between the estimation error in $\hat{\Pi}$ and the error in estimating β, the usual ML asymptotic standard errors will generally be understated unless there is no unobserved heterogeneity or measurement error.

To estimate asymptotically valid standard errors, I recast the estimation problem as a Generalized Method of Moments (GMM) objective function, which establishes the asymptotic normality of the estimates. The moment functions $g_i(\cdot)$ are the stacked together first-order conditions for both the OLS first stage (6) and the quasi-ML hazard model (10), which depend on the parameter vector $\theta = (\beta', \psi', \rho', \Pi')'$, where ρ parameterizes the control function $c(\cdot)$.

$$g_i(\theta) = \begin{pmatrix} \partial \ell_i / \partial \beta \\ \partial \ell_i / \partial \psi \\ \partial \ell_i / \partial \rho \\ (Z_{1i}' \quad Z_{2i}')'(x_i - Z_{1i}\Pi_1 - Z_{2i}\Pi_2) \end{pmatrix} \equiv \begin{pmatrix} g_{\ell i}(\theta) \\ g_{\pi i}(\theta) \end{pmatrix}. \tag{12}$$

Given that the two-step estimator minimizes the GMM objective function (Newey, 1984), asymptotically valid standard errors can be obtained by plugging two-step estimates $\hat{\Pi}$, $\hat{\beta}$, and $\hat{\psi}$ into the usual GMM variance formula.[26] In the results below, I use a cluster-robust estimator for the GMM

[23]The estimates of the baseline hazard function represent the average value of the continuous-time baseline hazard function $\lambda_0(\cdot)$ over each discrete interval $\bar{\lambda}_{0t} = \int_{t-1}^{t} \lambda_0(\tau)d\tau$ and are obtained as $\hat{\bar{\lambda}}_{0t} = \exp(\hat{\psi}_t)$. Alternatively, ψ_t can be thought of as estimating a piecewise-constant baseline hazard function.

[24]See Liu (2023) for more details and a longer discussion of the technical issues involved in the proof.

[25]This understatement of the standard errors is a form of the generated regressor problem (Pagan, 1984; Murphy and Topel, 1985) and arises because v depends on an unknown parameter vector Π.

[26]Stata code implementing my estimator can be accessed via `ssc install ivcloglog`.

standard errors.

3.3.2 Unobserved Heterogeneity

In the general case, even unobserved heterogeneity that is independent of the controls will affect the conditional distribution of $\tau|X$ (and hence the estimated coefficients), a common obstacle in nonlinear models. Lancaster (1979) introduced the Mixed Proportional Hazard (MPH) model where the heterogeneity enters in multiplicatively (additively in logs).[27] Conditional on unobserved heterogeneity ε, the hazard function becomes

$$\lambda(t|X_{icgt}, \varepsilon_i) = \exp(X'_{icgt}\beta + \varepsilon_i)\lambda_0(t). \tag{13}$$

The literature on unobserved heterogeneity in duration models has broadly found that ignoring unobserved heterogeneity biases estimated coefficients down in magnitude. Intuitively, the presence of ε induces survivorship bias—loans with low draws of ε last longer and are thus overrepresented in the sample relative to their observables. Individuals whose observable characteristics put them at a high ex-ante risk of default and yet have lengthy durations are likely observed in the sample because they have low unobserved individual-specific default risk (high latent quality). The negative correlation between X and ε induced by the sample selection process can prevent consistent estimation of β. A related form of potentially problematic unobserved heterogeneity is if there is unobserved heterogeneity in ω that is not captured by $c(v)$. If $\omega = c(v) + \varepsilon$, with $c(v) = E(\omega|v)$ such that ε is orthogonal to $c(v)$, then even a valid instrumentation strategy that solves the endogeneity of x will still need to address bias from unobserved heterogeneity ε. In Appendix B, I verify that my results are robust to the presence of independent unobserved heterogeneity by specifying $\varepsilon \sim \mathcal{N}(0, \sigma^2)$ so that the distribution $\tilde{F}$ of latent failure times τ is $\tilde{F}(\tau|X_{icgt}) = \int_{-\infty}^{\infty} F(\tau|X_{icgt}, \varepsilon_i)dR(\varepsilon_i)$, where $R(\varepsilon) = \Phi(\varepsilon/\sigma)$ and $\Phi(\cdot)$ is the standard normal cumulative density function.

4 Application to Subprime Mortgage Default

The surge in subprime mortgage defaults across cohorts during the Great Recession could have been exacerbated by the differential sensitivity of younger loans to housing price shocks and by changes in selection across cohorts (e.g., from changes to underwriting standards or fraud). Under the double-trigger model of mortgage default, looser underwriting standards predict higher default rates both because riskier borrowers are more likely to have income shocks and because riskier borrowers are less likely to be able to continue making mortgage payments after a given negative shock. Younger loans are also more sensitive to price declines because they have not yet accumulated as much equity and are thus more apt to be constrained in their ability to sell or refinance their mortgage. Even if cohort quality does not change over time such that an equal share of each cohort has an income shock that prevents its borrowers from making their mortgage payments, cohorts with positive

[27] Elbers and Ridder (1982) showed that the MPH model is identified provided there is at least minimal variation in the regressors.

equity are more likely to sell their homes or refinance.[28] Moreover, motivating the instrumental-variables strategy below, there will be feedback between price expectations and lending standards in the double-trigger model. Accordingly, both the selection channel and the house-price channel could generate variation across cohorts that could feature what credit analysts commonly refer to as vintage effects (Foote and Willen, 2018).

Figure 2 illustrates the differential effect that declining home prices had on origination cohorts by plotting the median mark-to-market combined loan-to-value ratio (CLTV) of each cohort of borrowers over time.[29] Each cohort's median current CLTV began rising in 2007 as prices declined nationwide. However, there are two main differences between early and late cohorts. First, origination CLTVs increased over time—the median 2007 CLTV in January 2007 was 10 percentage points higher than the median 2003 CLTV in January 2003, consistent with deteriorating underwriting standards. Second, earlier cohorts' median CLTVs declined from origination until 2007 as prices rose and as borrowers made their mortgage payments, reducing their indebtedness (with the former effect dominating because of the low amount of principal paid early in the mortgage amortization schedule). By contrast, later cohorts had not accumulated any appreciation or paid down any principal as prices fell almost immediately after their origination dates. By early 2008, more than one-half of borrowers in both the 2006 and 2007 cohorts were underwater, and by early 2009, more than one-half of the 2005 cohort was underwater.

To test the relative importance of each of these factors, I estimate how much of the heterogeneity in cohort default rates is explainable by observable composition differences, unobservable composition differences, and the differential effects of price declines across cohorts. Comparing observationally similar loans (i.e., by controlling for origination characteristics and loan age with a flexible baseline hazard specification within a geography) that were originated at different times allows me to exploit temporal variation in home prices within a geographic region. Likewise, comparing observationally similar loans originated at the same time but in different cities utilizes spatial variation in home prices. Cohort effects conditional on both observable origination quality and price changes capture potential unobservable differences in cohort quality, such as misrepresented origination characteristics (Ben-David, 2011; Piskorski et al., 2015; Griffin and Maturana, 2016). To account for the endogeneity of the home price series of each geographic area, I estimate counterfactual price series using a variety of plausible instruments for prices, as discussed in detail in section 6.2 below. This setup allows me to decompose observed cohort heterogeneity into its driving factors by successively introducing additional controls that explain differences in cohort default rates.

Combining a nonparametric baseline hazard function $\lambda_0(\cdot)$ with covariates entering through a parametric linear index function results in a semiparametric model of default. I specify the

[28] Note that labor-market shocks alone cannot generate the cohort pattern without differences in ex-ante risk (exposure to shocks) or ex-post sensitivity to shocks (e.g., through differences in accumulated equity).

[29] CLTV is the sum of all outstanding principal balances secured by a given property divided by the value of that property. The data used in Figure 2 estimate market values from CoreLogic's Automated Valuation Model—see section 5 for more details.

contribution of covariates as

$$X'_{icgt}\beta = \gamma_c + W'_i\theta + \mu \cdot \Delta Prices_{icgt} + \alpha_g \tag{14}$$

where γ_c and α_g are cohort and geographic fixed effects, respectively; W is a vector of borrower and loan attributes measured at mortgage origination; and $\Delta Prices_{icgt}$ is a measure of the change in prices faced by property i at time t. Borrower characteristics include FICO score (a credit score measuring the quality of the borrower's credit history), debt-to-income (DTI) ratio (calculated using all outstanding debt obligations), an indicator variable for whether the borrower provided full documentation of income during underwriting, and an indicator variable for whether the property was to be occupied as a primary residence. Attributes of the mortgage include the combined loan-to-value ratio at origination (using all open liens on the property for the numerator and the sale price for the denominator), the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages (when the new mortgage amount exceeds the outstanding principal due on the previous mortgage secured by the same house), mortgages with an interest-only period (when payments do not pay down any principal), balloon mortgages (non-fully amortizing mortgages that require a balloon payment at the end of the term), and mortgages accompanied by additional second liens.

As 2003 is the omitted cohort, the estimated baseline hazard function represents the conditional probability of default for a 2003 mortgage of each given age. The γ_c parameters scale this up or down depending on how cohort c mortgages default over their life-cycle, conditional on X and relative to 2003 mortgages of the same loan age. Successively conditioning on geographic fixed effects, borrower characteristics, loan characteristics, and price changes reveals the extent to which each factor explains the systematic variation in default risk across cohorts. The estimated $\hat{\gamma}_c$ without conditioning on any covariates are a measure of the average performance of each cohort. Conditioning on prices, $\hat{\gamma}_c$ estimates the quality of each cohort using default rates as an ex-post quality measure. Conditioning on observable loan and borrower characteristics and prices, $\hat{\gamma}_c$ represents the residual quality of each cohort. If cohort-level mortgage performance differences were driven by borrower unobservables, or if the explanatory power of the observables declined over time, then this would be captured by the cohort coefficients after controlling for all observables.

Cohort fixed effects present a challenge for many applications of loan-default models. For example, their interpretation affects the ability of loan-default models to be used to forecast cash flows and default events, including under counterfactual stress-test scenarios of interest to policymakers, financial intermediaries, and mortgage investors (Haughwout et al., 2017). If these parameters reflect unmeasured heterogeneity in underwriting stringency across loan vintages at the time of origination, then estimated vintage effects could reasonably be expected to persist over the life of the corresponding loans. However, if estimated cohort effects belie differential sensitivity to aggregate shocks by loans of different ages, then the extent to which they will capture persistent default-rate differences across cohorts is less clear. Similarly, if cohort fixed effects are simply nuisance parameters that reflect time-varying model misspecification, this obstructs inference about the relative quality of origination regimes. Accordingly, a useful feature of a loan default model is its specification's ability to attribute variation in default rates across cohorts to various default factors without large and unexplained cohort effects (Foote and Willen, 2018).

As discussed in section 3, I address two potential challenges inhibiting identification of the coefficients in (14): covariate measurement error and correlation between covariates and omitted factors ω. In the case of housing prices as a covariate in mortgage default models, the market value of a given home is unobserved in between sales and can only be estimated. Moreover, because individual-level price changes are endogenous to borrower behavior such as maintenance, analysts usually use local price indices to impute time-varying collateral values, leading to further measurement error in home-price proxies. As an example of a problematic omitted variable, credit supply shocks may directly affect both defaults and prices, potentially leading to a spurious estimated relationship between prices and defaults.[30] If a credit supply expansion leads to a decrease in the quality of the marginal borrower, prices will eventually fall as these riskier borrowers default (Baron and Xiong, 2017; Lopez-Salido et al., 2017). Their defaults will depress prices in at least three ways: from a positive shock to the supply of collateral for sale (Anenberg and Kung, 2014; Hartley, 2014), from negative foreclosure externalities (Campbell et al., 2011; Fisher et al., 2015), and by changing the beliefs of buyers and lenders (Brueckner et al., 2012; Glaeser and Nathanson, 2017; Armona et al., 2019; Liu and Palmer, 2021).

5 Data and Descriptive Statistics

In this section I briefly describe the data sources used in my analysis.

CoreLogic LoanPerformance (LP) Data. The main data source underlying this paper is the CoreLogic LoanPerformance (LP) Asset-Backed Securities database, a loan-level database providing detailed information on mortgages in private-label mortgage-backed securities including static borrower characteristics (DTI, FICO, owner-occupant, etc.), static loan characteristics (LTV, interest rate, purchase mortgage, etc.), and time-varying mortgage attributes updated monthly such as delinquency status and outstanding balance.[31] The LP data record monthly loan-level data on most private-label securitized mortgage balances, including an estimated 87% coverage of outstanding subprime securitized balances. Because about 75% of 2001-2007 subprime mortgages were securitized, this results in over 65% coverage of the subprime mortgage market.[32] My estimation sample is taken from a 1% random sample of first-lien subprime mortgages originated in 2003-2007 in the LP database followed through April 2013, resulting in a final dataset of over one million loan

[30]For evidence that credit expansions initially increase prices as demand increases, see Mayer and Sinai (2009), Mian and Sufi (2009), Favara and Imbs (2015), Landvoigt et al. (2015), Di Maggio and Kermani (2017), Argyle et al. (2021).

[31]Using LP data is standard in the economics literature for microdata-based analysis of subprime and near-prime loan performance. See Sherlund (2011), Mayer et al. (2009), Demyanyk and Van Hemert (2011), Krainer and Lederman (2011), and Fuster and Willen (2017) for examples. See GAO (2010) for a more complete discussion of the LP database and comparison with other loan-level data sources.

[32]See Mayer and Pence (2009) for a description of the relative representativeness of subprime data sources. Foote et al. (2008) and Elul (2015) suggest that non-securitized subprime mortgages are less risky than securitized ones.

× month observations.[33]

Table 1 reports descriptive statistics for static loan-level borrower and mortgage characteristics at origination by origination cohort. On these observable dimensions, subprime borrowers comprise a population with high ex-ante default risk. The average subprime borrower in my data has a credit score of 617, slightly above the then-national 25th percentile FICO score and substantially below the national median score of 720 (Board of Governors of the Federal Reserve System, 2007). Among borrowers who reported their income on their mortgage application, the average back-end debt-to-income ratio, which combines monthly debt payments made to service all open property liens, is almost 40%, well above standard affordable housing thresholds. More than half of the loans in my estimation sample are for cash-out refinances, where the borrower is obtaining the new mortgage for an amount higher than the outstanding balance of the prior mortgage. I define default as when a loan's delinquency status is in foreclosure or real-estate owned provided it ultimately terminated without being paid off in full (Mayer et al., 2009; Sherlund, 2011). As of April 2013, 24% of the mortgages in my sample have defaulted and 50% have been paid off, leaving 26% of the loans in the data still outstanding.

The distribution of many borrower characteristics is stable across cohorts. Average FICO scores, DTI ratios, combined loan-to-value ratios (measured using all concurrent mortgages and the sale price of the home, both at the time of origination), documentation status, and the fraction of loans that were owner-occupied or were taken out as part of a cash-out refinance are roughly constant across cohorts.[34] While there is substantial evidence that, pooling prime, near-prime, and subprime mortgages, borrower characteristics were deteriorating across cohorts (e.g., JEC, 2007), the lack of a noticeable decrease in borrower observables in my data is consistent with observations from Gerardi et al. (2008) and Demyanyk and Van Hemert (2011) who argue that the declines within the population of subprime borrowers were too small to fully account for the heterogeneity in performance across cohorts. Among mortgage product characteristics, however, there are important differences across cohorts, including a marked increase in prevalence of interest-only loans, mortgages with balloon payments, and mortgages accompanied by additional liens on the property. This finding of relatively stable borrower observables and large changes in certain mortgage characteristics is consistent with the findings of Rajan et al. (2015) and Mayer et al. (2009).

The trend in prepayment rates across cohorts in Table 1 is suggestive evidence that the differential availability of refinancing or reselling opportunities is an important aspect of the cohort

[33] There is no standardized definition of a subprime mortgage. Popular classification methods include mortgages originated to borrowers with a credit score below certain thresholds, mortgages with an interest rate that exceeds the comparable Treasury rate by three percentage points, certain mortgage product types, mortgages made by lenders who self-identify as making predominantly subprime mortgages, and mortgages serviced by firms that specialize in servicing subprime mortgages. For my purposes a subprime loan is one that is in a mortgage-backed security that was marketed at issuance as subprime, as in Mayer et al. (2009). Following Sherlund (2011), I additionally drop mortgages originated for less than $10,000, mortgages whose first payment date is listed as before the origination date or 90 days after the origination date, and non-standard property types such as manufactured housing.

[34] Note that the at-origination CLTVs reported here use the sale price of the home for its value, whereas the contemporaneous (mark-to-market) CLTVs in Figure 2 use estimated market values. If the divergence between these two measures over time is an important predictor of default, it will affect the magnitude of the estimated cohort main effects, which capture all unobserved factors changing across cohorts.

default pattern. Appendix Figure A1 shows the cumulative prepayment probability by cohort—the fraction of each cohort's mortgages that had been paid off within the given number of months since origination. The pattern across cohorts is exactly reversed from the cohort heterogeneity in default rates depicted in Figure 1—more recent borrowers prepaid their mortgages much less frequently and at slower rates than borrowers from 2003-2005. Given the evidence that later cohorts were more likely to face price declines early in their mortgage's life, the contrast between the cohort-level trends in defaults and prepayments is consistent with the notion that many underwater borrowers in distress default and many above-water borrowers in distress prepay.

Specifications which directly examine the effects of negative equity make use of a novel feature of the LP dataset: contemporaneous combined loan-to-value ratios (CLTVs), which are a measure of the total amount of debt secured against a property relative to its market value. To calculate the CLTV numerator, CoreLogic uses public records filings on additional liens on the property to estimate the total debt secured against the property at origination. For the denominator, CoreLogic's automated valuation model (popular in the mortgage lending industry) uses the characteristics of a property combined with recent sales of comparable properties in the area and monthly home price indices to impute a value for each property in each month.

Regional Data. For regional measures of home prices, I use the CoreLogic monthly Home Price Index (HPI) at the Core-Based Statistical Area (CBSA) level. These indices follow the Case-Shiller weighted repeat-sales methodology to construct a measure of quality-adjusted market prices from January 1976 to April 2013. They are available for several property categories—I use the single family combined index, which pools all single-family structure types (condominiums, detached houses, etc.) and sale types (i.e., does not exclude distressed sales). Each CBSA's time series is normalized to 100 in January 2000. I match loans to CBSAs using each loan's zip code and a 2008 crosswalk between zip codes and CBSAs from the U.S. Census Bureau.

I use publicly available Home Mortgage Disclosure Act (HMDA) data to calculate the subprime market share in a given CBSA $\times$ year by merging the lender IDs in the HMDA data with the Department of Housing and Urban Development subprime lender list as in Mian and Sufi (2009). HMDA data discloses the census tract of each loan, which I allocate proportionally to CBSAs using a crosswalk from tracts to zip codes and then from zip codes to CBSAs. I also use Metropolitan Statistical Area and Micropolitan Statistical Area unemployment rates from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics series.[35]

6 Loan Default Model Estimates

In this section, I first report loan default model estimates that take recent local home price changes as given. I then develop an instrument for home price changes based on projecting national price changes onto the historical cyclicality of each local housing market. After demonstrating the strength of the first stage and several tests consistent with the exclusion restriction being satisfied, I present

[35] Available at `http://www.bls.gov/lau/home.htm`.

control-function loan default model estimates accounting for the endogeneity and potential mismeasurement of of price changes. As an additional test of the identification strategy, I show that four other home-price instruments from the literature each pass relevance and exogeneity checks and that the control-function estimator returns similar results regardless of the instrument used. Finally, I test whether the misrepresentation of mortgage characteristics is a confounding force biasing my results.

6.1 Treating Price Changes as Exogenous

Table 2 reports estimates of equation (2) using the estimator described above, treating price changes as exogenous to offer initial estimates of the relationship between price changes, underwriting standards, and cohort-level differences in default rates. I cluster all standard errors at the CBSA level to account for area-specific shocks to the default rate in inference. All specifications include nonparametric controls for the baseline hazard function.[36] Column 1 includes only cohort fixed effects to quantify the pattern of declining cohort-level performance from Figure 1 in a hazard-model framework. These coefficients can be interpreted as the change in the log hazard rate and imply, for example, that subprime loans in the 2007 cohort had a default hazard 73 log points greater than the 2003 cohort (the omitted category). These unadjusted cohort coefficients are large and precisely estimated, implying that the probability of a 2005-2007 cohort mortgage defaulting in a given month conditional on the mortgage having survived to that month is more than twice as high as 2003 cohort mortgages. Column 2 adds fixed effects for each CBSA in the sample (568 fixed effects) to verify that cohort differences are not driven by the geographic composition of later cohorts. Conditioning on CBSA fixed effects does not materially affect the estimated differences in cohort default hazards.

Column 3 adds borrower characteristics and loan characteristics, as detailed in section 5. The coefficients on these credit risk factors (available upon request) all have intuitive signs. Borrowers had higher default rates if they lacked full income documentation, were not owner-occupants, or had lower FICO scores and higher DTI ratios. Mortgages defaulted more frequently if they were non-fixed-rate mortgages, had higher CLTVs or interest rates, or were accompanied by additional liens. Conditioning on origination characteristics in column 3 explains on average 29% of the unadjusted cohort effects estimated in column 1. This suggests that the loan product and borrower characteristics that were changing across cohorts (and the change in borrower unobservables that they represent) were an important driver of defaults.[37]

[36] The baseline hazard controls consist of an indicator variable for each possible value of loan age from 1-70 months, with the final indicator variable including all values of loan age exceeding 70 months. The estimated baseline hazard functions resemble the hump-shaped baseline hazards of Deng et al. (2000) and are available by request.

[37] Given prior work documenting that there are important nonlinearities and interactions in how origination characteristics affect mortgage risk (e.g., Davis et al., 2022), I test whether the ability of characteristics to explain the cohort pattern in default rates is limited by the linear and additively separable specification above. Appendix Table A1 replaces the origination characteristics controls above with fixed effects for 144 borrower cells, defined as unique combinations of 20-point FICO bins, 10-point DTI bins, 10-point CLTV bins, and most of the binary characteristics controlled for in Table 2. While the cells add explanatory power, as evidenced by their individual statistical significance and increase in the log likelihood, they do not significantly affect estimates of conditional cohort default rates.

Column 4 drops all borrower- and loan-level covariates and instead controls for the 12-month change in the log of the CoreLogic repeat-sales Home Price Index (HPI), defined at the CBSA-level as

$$\Delta \log(HPI_{icgt}) \equiv \log(HPI_{icgt}) - \log(HPI_{icgt-12}) \tag{15}$$

where HPI_{icgt} is the value of the CoreLogic repeat-sales price index for CBSA g in the calendar month corresponding to loan i having a duration of t.[38] Price declines are a strong predictor of default. The coefficient on the 12-month change in log HPI implies that properties experiencing the 25th percentile 12-month price change (-5%) would have a 45% higher default hazard than properties exposed to the 75th percentile 12-month change in prices (+5%), corresponding to a 3.9 percentage point increase in the annual default rate. The differences in default rates across cohorts in column 4 decrease by an average of 50% relative to the baseline cohort coefficients in column 1. Even without adjusting for observable compositional differences across cohorts, price declines alone can explain half of the cohort default pattern.

Controlling for both borrower and loan characteristics and price changes explains most of the cohort-level heterogeneity in default rates. The estimates in column 5 of each cohort's unobservable quality (i.e., the portion of cohort outcomes not attributable to price changes or individual-level controls) are small and, with the exception of the 2005 cohort, statistically insignificant (with 74% of the 2005 cohort default rate in column 1 explained in column 5).

As discussed above, the definition of default used in Table 2 is closely related to a foreclosure start. In Appendix Table A2, I test the explainability of the cohort pattern when using a more sensitive default measure defined as the first time a loan is at least 90 days past due.[39] In general, mortgage delinquencies seem more idiosyncratic and are harder to explain empirically than foreclosure starts. Price changes and characteristics are again important delinquency predictors but together explain only half of cohort delinquency rates. Again, the qualitative pattern holds that even without adjusting for changes in borrower and loan composition across cohorts, price declines alone are the most important predictor of mortgage delinquencies.[40]

Overall, these results illustrate that both observable loan characteristics and prices play important roles in explaining the rise in default rates across origination cohorts, together explaining on average 85% of the cohort disparities in column 1. Empirically, the patterns in Table 2 indicate that the incidence of price declines is disproportionately borne by later cohorts. To evaluate the counterfactual question of how much price declines alone would have increased default rates across cohorts even if cohort quality had not declined, I next apply the methods developed above to estimate causal effects.

[38] I index HPI by i as well to emphasize that in my notation t refers to event time (i.e., loan age). Even though HPI only varies by CBSA $\times$ calendar month, not all, for example, six-month old ($t = 6$) mortgages in CBSA g have the same HPI value.

[39] Potentially limiting the economic importance of delinquencies relative to foreclosure starts, I note that only 17% of loans in my sample that are ever 90 days past due eventually end up in some form of foreclosure.

[40] The attenuated importance of price changes for delinquency prediction relative to default is more consistent with double-trigger models than strategic default models. Moreover, in contrast to what I find for default, I fail to reject that price changes are exogenous in delinquency using the instrumentation strategy described in section 6.2.

6.2 Control Function Estimation

The interpretation of hazard-model estimates as causal—e.g., that cohort default rates would not have increased as much if a given factor hadn't changed—requires Assumption 2 above. The potential for changes in local home prices to be mismeasured or themselves be a function of contemporaneous shocks to the default hazard (e.g., through shocks to subprime lending, fraud, or employment) necessitates instrumenting for prices. I use several different instruments for price changes. First, I develop an novel instrument that isolates the long-run component of each Core Based Statistical Area's (CBSA) price cycle and is arguably independent of contemporaneous shocks to prices or default rates. Second, I confirm in section 6.2.4 that several alternative instruments for housing prices from the literature produce very similar default model estimates.

6.2.1 Isolating Exogenous Variation in House-Price Cyclicality

Sinai (2013) notes that a similar set of metropolitan areas had large or small 1980s and 2000s price cycles. There could be several drivers of this persistent cyclicality, including local demographics, housing supply elasticity, migration elasticity, and industry composition, and one virtue of this omnibus approach is that it does not require explicitly specifying the aspect of housing market structure that leads to this predictability. For a measure of historical cyclicality to be a valid instrument for subsequent price changes, whatever leads a given area to have a consistently cyclical housing market must be unrelated to the subprime credit expansion (Assumption 5), as I describe and test in section 6.2.2 below.

To measure this persistence, I determine the portion of a CBSA's price cycle that is predictable using only the historical cyclicality of that city. First, I form a summary measure σ_g^P quantifying the long-run cyclicality of CBSA g defined as the standard deviation of monthly changes in the CoreLogic repeat sales home price index from 1980-1995

$$\sigma_g^P \equiv \left(\frac{1}{T-1} \sum_{t \in \mathcal{T}} (\Delta HPI_{gt} - \overline{\Delta HPI}_g)^2 \right)^{1/2} \tag{16}$$

where $T = 180$ is the number of months over which the standard deviation is calculated; $\mathcal{T}$ is the set of months from January 1980 to December 1995, inclusive; $\Delta HPI_{gt} = HPI_{gt} - HPI_{gt-1}$; and $\overline{\Delta HPI}_g$ is the average value of ΔHPI_{gt} for CBSA g and $t \in \mathcal{T}$.[41] Plotting the 1980-1995 HPI paths shows that high-σ^P areas had more pronounced boom-bust cycles and that the actual timing of each CBSA's house price cycle was unrelated to σ^P, consistent with the asynchronicity of regional price fluctuations during that time period. Panel I of Figure 3 shows the average value of the CoreLogic repeat sales home price index by quartile of σ^P. The persistence in price volatility isolated by the first stage is apparent: the average price cycle in the late 2000s was more pronounced for CBSAs that had stronger price cycles in the 1980s, that is, higher quartiles of σ^P have stronger

[41] I calculate the standard deviation of the first differences in the HPI variable to emphasize the importance of the (low-frequency) price cycle. CBSAs with high variance of HPI in levels (as opposed to high ΔHPI) could simply be areas that had sustained price growth or strong seasonality.

price cycles. As discussed below, a key virtue of this instrument is that many of the places with the strongest subprime expansions did not have large price cycles in the 1980s such that instrumenting using historical variation deemphasizes these areas' price declines that were arguably the most confounded with the subprime credit expansion.

The first instrument set for the price change variable is the long-run cyclicality measure σ_g^P interacted with calendar-month indicator variables. The first stage for the 12-month price change is then

$$\Delta \log(HPI_{icgt}) = \sum_s \pi_s \sigma_g^P \cdot 1(s = t_0(i) + t) + Z'_{2,icgt}\pi_2 + v_{icgt} \tag{17}$$

where $Z_{2,icgt}$ contains the same covariates as equation (14) above—cohort effects, geographic fixed effects, loan and borrower characteristics, and the nonparametric baseline hazard function to ensure that predicted values from equation (17) are orthogonal to the other controls in equation (2). The function $t_0(i)$ evaluates to the calendar time of loan i's origination date, and the π_s coefficients are turned on when the observation on loan i at t months after origination corresponds to calendar month s.

Table 3 tests Assumption 4 and reports the results from estimating first-stage equation (17) by OLS with standard errors clustered at the CBSA level. Column 1 includes just the instrument set and no other controls. The statistical relationship between actual price changes and the interactions between the cyclicality measure and calendar time is strong—the instruments explain 50% of the variation in twelve-month CBSA-level home price changes. Adding controls for the baseline hazard and CBSA fixed effects in column 2 improves the overall fit slightly (R^2 increases to 0.56). Including loan and borrower characteristics in column 3 does not affect the partial F-statistic, which tests the joint hypothesis that all of the coefficients on the instrument set are zero, suggesting that weak instruments are not a problem in this setting and Assumption 4 holds.[42]

To provide intuition for how this instrument operates, I compute counterfactual price indices by regressing log home-price indices on geographic fixed effects and an interaction of σ_g^P with calendar-month indicators

$$\log(HPI_{gt}) = \alpha_g + \sum_s \pi_s \sigma_g^P \cdot 1(s = t) + u_{gt} \tag{18}$$

where HPI_{gt} is the value of the CoreLogic home price index in CBSA g in calendar month t. The estimated $\hat{\pi}_s$ shift the baseline log HPI of each CBSA (α_g) according to the cross-sectional relationship each calendar month between prices and 1980s price volatility.[43] Predicted values $\widehat{\log HPI}_{gt}$ from this regression provide an alternative time series of home prices in geography g based on the quasi-fixed tendency of home prices in geography g to cycle up and down.

[42]The cohort coefficients in columns 2 and 3 illustrate that later cohorts were exposed to stronger price declines than earlier cohorts, in part by virtue of selection—younger loans have had less time to prepay and are thus more likely to be extant and exposed to recent price declines.

[43]Note that equation (18) does not control for main effects for each date. While this loads the national month-to-month variation in home prices onto the π_t, date effects—the embodiment of the rise in subprime defaults—are the very object the hazard model seeks to explain. As they are not instruments and they do not belong in the second-stage, I purposefully omit them here.

Figure 4 shows the actual log home price series for 2003-2013 (left-hand panel) along with predicted values from equation (18) (right-hand panel). The left-hand panel shows that the actual HPI series are characterized by idiosyncratic deviations from the national trend, i.e., price shocks that potentially arise from factors such as local credit expansions and local labor market fluctuations that may also independently affect default rates. It is precisely the effects of these types of shocks from which the instrument is designed to abstract. Because nothing in equation (18) allows for differential price trends across CBSAs, the predicted time series in the right-hand panel all change in the same direction each month, differing only in the magnitude of the price change depending on their historical price volatility. To the extent that the actual price paths reflect time-varying local housing market changes, each line on the right is an estimate of the counterfactual price path that might occur absent local price shocks that are potentially driven by factors that also affect local default risk. Intuitively, my empirical strategy instruments for the actual price series on the left with the predicted price series on the right.

6.2.2 Exclusion Restriction

The necessary exclusion restriction (Assumption 5) for control-function results to be unbiased estimates of the causal effect of price changes is the independence of an instrument (e.g., the size of a CBSA's 1980s price cycle σ_g^P) from any omitted factors ω that affect default. Note that with CBSA fixed effects, it is not a threat to identification if cyclical areas are fundamentally different from less cyclical areas in some time-invariant way (e.g., an inherently risky area may always have both higher defaults and larger price swings). However, this exclusion restriction would be violated if pro-cyclical housing-market areas (high σ_g^P) also have pro-cyclical trends in the credit risk of borrowers. For example, if high-σ_g^P areas had more rapid subprime growth, then σ_g^P may proxy for changes in unobserved borrower quality in CBSA g. Similarly, if high-σ_g^P areas have greater unemployment rate fluctuations, these adverse shocks to local aggregate demand could increase defaults (through income shocks) and decrease prices (through demand shocks).[44]

Figure 5 offers graphical evidence that subprime shares and unemployment rates—residualizing both for CBSA fixed effects—did not vary systematically with σ_g^P. The relevant period is different for each endogeneity concern. Panel I of Figure 5 plots the annual adjusted subprime share of HMDA-covered mortgages originated in 2003-2007 by quartiles of σ_g^P. There is no apparent relationship between σ_g^P and subprime originations—places with historically large price cycles do not seem to have been any more prone to subprime credit expansion.[45] Results using the share of purchase originations that were packaged into subprime mortgage-backed securities are similar. Importantly, although many areas with a strong boom-bust in housing markets in the 2000s also had strong subprime credit expansions, using historical cyclicality as an instrument isolates house-price fluctu-

[44]Note that Mayer (2010), Mian (2010), and Mian and Sufi (2014) argue that price declines first caused unemployment in the recent recession.

[45]Note that the same fact is not true about the relationship between subprime originations and the size of the 2000s price cycle—areas that originated the highest share of subprime mortgages indeed had stronger (contemporaneous) price cycles, consistent with Di Maggio and Kermani (2017).

ations unrelated to the credit expansion, as required by the identifying exclusion restriction. Panel II of Figure 5 plots average smoothed unemployment rates from 1990-2013 by σ_g^P quartile. Areas that had cyclical housing markets from 1980-1995 had contemporaneously cyclical labor markets, as evidenced by the top quartile having the strongest unemployment cycle in the early 1990s. However, panel II shows that the top quartile of σ_g^P had around a 1 percentage point *lower* unemployment rate in the Great Recession than the bottom quartile, showing distressed employment conditions to be unrelated to historical cyclicality.

As an additional test of the exclusion restriction, I examine whether σ_g^P predicts trends in observable mortgage and borrower characteristics. Intuitively, if an instrument is not predictive of observable characteristics, it is more plausibly unrelated to unobservables as well. I reestimate the first-stage equation (17), replacing $\Delta\log(HPI_{icgt})$ with x_{icgt} where x is one of the borrower or loan characteristics summarized in Table 1 and the controls corresponding to each regression consist of the remaining controls. I also normalize σ_g^P by its standard deviation so that each month's coefficient is interpretable as the predicted change in a given characteristic from a CBSA having a one standard deviation higher historical cyclicality relative to the omitted months (January and February 2003). Figure 6 plots the result of this exercise for four example origination characteristics: credit scores, debt-to-income ratios, combined loan-to-value ratios, and the owner-occupied indicator. Across all four panels, nearly every month is a statistically and economically insignificant predictor of the indicated characteristic with no overall pattern or trend. I interpret this as σ_g^P not being predictive of any particular pattern in lending standards over time, consistent with the instrument independence assumption required by Assumption 5.

6.2.3 Control Function Results

Table 4 employs a nonlinear control function approach, which accounts for the endogeneity of price changes to the credit expansion by controlling for the first-stage residuals $\hat{v}_{icgt}$ in the default hazard index

$$X'_{icgt}\beta = \gamma_c + W'_i\theta + \mu \cdot \Delta Prices_{icgt} + \rho\hat{v}_{icgt} + \alpha_g \tag{19}$$

where $\hat{v} = \Delta\log(HPI) - \Delta\widehat{\log HPI}$ and $\Delta\widehat{\log HPI}$ is fitted from equation (17).

Column 1 of Table 4 repeats column 6 of Table 2 for reference, controlling for the 12-month change in prices and not conditioning on origination characteristics W. Column 2 additionally controls for the residuals $\hat{v}_{icgt}$, estimated from OLS on equation (17) (omitting loan and borrower characteristics in the construction of the residuals). The coefficient on the price change variable is still large and significant—borrowers experiencing a 1% price decline over the previous year have a 4.4% higher conditional probability of default. The adjusted cohort differences are smaller in column 2 than column 1, meaning that after accounting for endogeneity, the share of the cohort pattern explainable by price declines is larger. Comparing column 2 to the benchmark differences in cohort performance measured in column 1 of Table 2, controlling and instrumenting for prices without controlling for borrower or loan characteristics explains 58% of the difference in unadjusted cohort outcomes.[46]

The next two columns additionally control for borrower and loan characteristics. The estimated cohort effects in these specifications capture the latent quality of each cohort, i.e., the heterogeneity in cohort-level default rates not explained by ex-ante observable quality or price changes. Column 3 repeats column 5 of Table 2 for reference, controlling for price changes in addition to all of the other controls. Column 4 reports control function estimates of this specification. The coefficient on the price change variable increases in magnitude from -3.9 to -4.6 (see below discussion). The coefficient on the endogenous portion of the 12-month change in home prices is again positive and significant. As before, I cannot reject that each of the cohort latent quality measures is statistically indistinguishable from zero with the exception of the 2005 cohort. Moreover, the estimated cohort effects in column 4 are each smaller than those in column 3 which treat prices as exogenous. The specification in column 4 explains 93% of the unadjusted differences in cohort default rates in column 1 of Table 2. The similarity of the price coefficient with and without origination characteristics controls suggests price changes and origination characteristics are not strongly related and supports the interpretation that columns 1 and 2 reveal the share of the cohort pattern explained by price declines alone.

In both columns 2 and 4, instrumenting increases the magnitude of the estimated effect of price changes relative to not instrumenting (columns 1 and 3). A default elasticity of -4.5 implies that for a 10 log-point decrease in housing prices, the probability of default increases by 45 log points (or a 54% increase in default for a 10% decrease in prices). As discussed below, similar patterns emerge when using alternative instruments for house price declines. There are two candidate explanations for the increase in the estimated price elasticity of default after instrumenting.[47] First, as discussed above, instrumenting may solve an errors-in-variables problem with $\Delta Prices$—CBSA-level HPI estimates may be an imperfect proxy for the relevant property value shock faced by an individual borrower in a given CBSA. Because the literature has generally found that such covariate measurement error causes attenuation bias, larger control-function estimates are consistent with baseline results being downward biased from measurement error. Second, this pattern is also potentially consistent with some degree of treatment effect heterogeneity—locales that are compliers to the instrument that have experienced large price declines historically may be more sensitive (i.e., their default behavior more elastic) to current price declines.

Besides the relevance and exogeneity conditions (Assumptions 4 and 5 tested in sections 6.2.1 and 6.2.2), the control function results of Table 4 also rely on the control function being correctly specified as linear. Appendix C addresses the possibility that the conditional distribution of the endogeneity is misspecified. Results in Appendix Table A4 allowing for flexible specifications of $c(v)$ are consistent with the results of Tables 2 and 4, providing evidence that there is a large causal

[46] A t-test on the coefficient ρ on the residuals is equivalent to a Rivers and Vuong (1988) test for endogeneity in a probit model. However, although the statistical significance of $\hat{\rho}$ in columns 2 and 4 confirms that price changes are endogenous, the overall cohort effect decomposition in Table 4 is similar to Table 2.

[47] Instrumenting for the underwater indicator in Appendix Table A3 decreases its magnitude, consistent with measurement-error attenuation bias dominating CBSA-level prices and endogeneity bias dominating individual equity positions.

effect of price declines on defaults that explains much of the cohort default pattern.

6.2.4 Robustness to Alternative Instruments

In this section, I introduce four alternative instruments for house prices from the literature and demonstrate that they each result in similar control function estimates of the subprime loan default model above. First, Guren et al. (2021) build on the persistence in cyclicality developed here but estimate a slightly different specification, estimating a MSA-specific loading γ_g on regional house prices using data from 1976-2017. They then use $\hat{\gamma}_g \Delta P_{r(g)t}$ as an instrument for $\Delta Prices_{gt}$, where $\Delta P_{r(g)t}$ is the change in a regional house price index for the region $r(g)$ corresponding to CBSA g. Intuitively, their strategy isolates variation in local house prices driven by regional price fluctuations and a static factor loading free from contemporaneous CBSA-specific shocks in a similar spirit to σ_g^P above—see Guren et al. (2021) for further details. To implement their instrument in my framework, I replace σ_g^P with $\hat{\gamma}_g$ in the first-stage equation (17) and use interactions of date fixed effects and $\hat{\gamma}_g$ as the instrument set, projecting house prices onto the sensitivity of each CBSA to its regional housing cycle.

Second, I incorporate the social network house price instrument of Bailey et al. (2018a), who find that beliefs about house prices propagate through social networks. Using shocks to out-of-state housing markets weighted by the share of each housing market to a given county's social network, they find that the housing markets of socially connected counties comove in a way that cannot be explained by their shared economics or common shocks. To implement this beliefs-based instrument, I construct a predicted change in the county-level log home price index for each county j in month t as

$$\Delta \widehat{\log HPI}_{jt} = \frac{\sum_{k \notin s(j)} \alpha_{jk} pop_k \Delta \log HPI_{kt}}{\sum_{k \notin s(j)} \alpha_{jk} pop_k}, \tag{20}$$

where the instrument averages the change in the log housing price index of each county k not in the same state $s(j)$ as county k weighted by how socially connected counties j and k are using a county-level measure α_{jk} aggregated from Facebook networks by Bailey et al. (2018b) and the 2000 population pop_k of county k. To aggregate from counties to CBSAs, I use a HUD county-CBSA crosswalk and average county-level $\Delta \widehat{\log HPI}_{jt}$ for each county j in a given CBSA weighted by the population of county j in 2000. The coefficient on $\Delta \widehat{\log HPI}_{gt}$ in the first stage is 1.3 with a t-statistic of 10.

Third, Saiz (2010) finds that housing markets with a high share of land unavailable for building (because of steep terrain, water, etc.) have higher housing supply elasticities and larger housing price swings. Despite widespread use of the various predictors of housing supply elasticities in Saiz (2010) as instruments for house prices, Davidoff (2016) questions their validity given their correlation with demand factors. For comparability with other work using these measures as instruments, I present results here using the share of unavailable land as a potential instrument for house prices. To extend the coverage of the MSA-based Saiz (2010) measure to all CBSAs in my sample, I aggregate county-level land unavailability measures from Lutz and Sand (2023) to the CBSA-level weighting by 2000 population.[48] CBSAs with higher land unavailability had more pronounced booms and busts in the CoreLogic HPI data while not having differentially worse credit scores, LTV ratios, or DTI ratios. Given the correlation documented by Davidoff (2016) between land unavailability and demand factors, it is notable that the other instruments used here are uncorrelated with land unavailability—for example, the R^2 of a regression of land unavailability on σ_g^P is 0.01.

Finally, Choi et al. (2016) argue that when there are many publicly traded companies headquartered nearby, local bias by investors can divert capital away from housing markets, leading to more muted fluctuations in home prices. I use the log of the ratio of the total book value of all publicly traded companies headquartered in a given MSA to the total income of that MSA from Choi et al. (2016) as a candidate instrument for house price changes plausibly unrelated to contemporaneous shocks to lending standards, etc. Consistent with Choi et al. (2016), I find that MSAs with a larger share of income attributable to public companies headquartered therein have a smaller 2000s boom-bust cycle.

Figure 3 demonstrates the degree to which the instruments predict price changes by plotting the average HPI over time by quartile of each instrument. I focus on the four static instruments for this exercise to be able to assign each CBSA to a fixed quartile of the instrument; because the social network predicted HPI of equation (20) is time-varying, a given CBSA is in different quartiles in different months. Consistent with the large first-stage partial F-statistic for each instrument mentioned below, the instruments are broadly predictive of the boom-bust cycle in prices, with quartiles more exposed to each instrument having larger price cycles. Next, I reexamine whether each instrument is related to local economic and individual origination characteristics, similar to Figures 5 and 6 discussed above. Appendix Figures A2-A7 examine whether each instrument is predictive of subprime shares, unemployment rates, credit scores, DTI ratios, CLTV ratios, and owner occupancy rates. Each of the four instruments is almost always a statistically (and economically) insignificant predictor of origination credit scores, debt-to-income ratios, leverage ratios, and the owner occupied share.[49] Moreover, the lack of trends across origination months suggests that any spurious relationship between an instrument and unobservable lending standards is unlikely to explain the predictive power of instrumented prices on the cohort default pattern. Subprime shares are broadly similar across quartiles of each instrument. Appendix Figure A3 shows that there are notable differences in unemployment rate trends across quartiles of the regional sensitivity instrument. This helps illustrate the conceptual difference between the cyclicality and sensitivity instruments; while regional sensitivity measures how a housing market comoves with its broader regional economy, cyclicality is measured idiosyncratically. Appendix Figure A3 demonstrates that this results in the sensitivity instrument weighting areas whose labor markets are also more sensitive to regional or aggregate shocks.

Table 5 estimates equations (2) and (19) in the text with the first stage given by (17). Column 1 repeats column 4 of Table 4 for ease of comparison. Each instrument is a sufficiently strong

[48] Where they overlap, the Saiz (2010) and Lutz and Sand (2023) measures have a correlation of 0.99.

[49] The most notable exception is that areas with higher land unavailability have lower average CLTV ratios, which is the opposite prediction of the expected endogeneity of the price boom being caused by looser lending standards.

predictor of house price changes, with first-stage partial F-statistics ranging from 20 for the public headquarters instrument in column 5 to 309 for the regional sensitivity instrument in column 2. Across all five instruments, the instrumented elasticity of default with respect to house prices is large, statistically significant, and greater in magnitude than the results in Table 2 treating CBSA price changes as exogenous and measured without error. With the exception of results using the land unavailability measure in column 4 that may be indicative of some of the problems highlighted by Davidoff (2016), the magnitude of the default elasticity is quite consistent across columns, ranging from -4.5 to -4.7. Moreover, across all five instruments, the share of the cohort default pattern that is explainable by price declines is consistently around 60% with the entire model of observable loan product and borrower characteristics and price declines explaining around 95% of cohort-level default rates.

In summary, I use the control-function estimator of section 3 with a variety of instruments to decompose the default pattern across origination cohorts into differences in cohort characteristics, differences in their exposure to price declines, and unobservable cohort quality. The results confirm that prices and observable mortgage characteristics are both important, with price changes alone causally explaining at least 60% of the increase in cohort default rates. Even after accounting for the simultaneous relationship between collateral values and default risk, there is only weak evidence that a decline in unobserved borrower quality is a substantial explanation for the increase in defaults across cohorts.

6.3 Accounting for Misrepresented Mortgage Characteristics

The results above take origination characteristics recorded by servicers as given. However, there is evidence that the securitization chain involved a significant amount of misrepresentation of mortgage characteristics. For example, Griffin and Maturana (2016) document the prevalence properties seemingly overvalued by more than 20% and false claims that borrowers were owner occupants or did not have second mortgages. Piskorski et al. (2015) find that default rates were 70% higher for loans with misrepresentations, and Griffin et al. (2021) find that house prices had a stronger boom-bust cycle in areas with more misbehaving originators. Such misreported data poses a problem for the cohort-effects decomposition exercise; perhaps observed origination characteristics predict some of the cohort pattern, but actual origination characteristics would predict more.

I assess the magnitude of this concern in several ways. First, using data on the prevalence of three types of potential mortgage fraud at the CBSA-level from Griffin and Maturana (2016), I estimate additional specifications controlling for measures of fraud directly and interacted with the characteristics most likely to have been misrepresented. Second, I allow the effects of CBSA-level fraud measures to vary across cohorts to directly test the extent to which the cohort pattern can be explainable by increasing fraud. Finally, I use estimates from the literature about the increase in fraud across origination cohorts and the effect of fraud on default to bound how much of the increase in defaults across cohorts could potentially be explained by misrepresentations.

In Appendix Table A5, I first demonstrate in column 1 that the Griffin and Maturana (2016)

CBSA-level fraud variables are predictive of default and jointly highly significant. Columns 2-3 repeat columns 2-3 of Table 2 for comparison. Column 4 reestimates the specification of column 3, but interacts the four origination characteristics most affected by misreporting with the Griffin and Maturana (2016) variables: owner occupancy, CLTV ratio, and the indicator for a second mortgage. Both the main effects and the interaction terms in column 4 show that the origination characteristics are generally stronger predictors of default when fraud is lower. However, comparing the cohort effects in column 4 to column 3 shows that the extra explanatory power provided by the fraud interactions does not improve the ability of origination characteristics to explain the cohort pattern above what is already captured by CBSA fixed effects and the vector of observed origination characteristics.

To directly test for the ability of misrepresentations over time to explain the cohort pattern, in Appendix Table A6, I interact the cohort indicators with each measure of fraud. Because the Griffin and Maturana (2016) fraud measures are averages across 2002-2007 origination cohorts, I use interaction terms to allow for the incidence of any misrepresentation to load differentially across cohorts. The cohort main effects controlling for the cohort by fraud interaction terms shows what the pattern would be when each fraud measure is normalized to zero.[50] The overall pattern of the cohort and fraud interaction terms is that later cohorts seem to be more sensitive to fraud measures than earlier cohorts. However, consistent with Appendix Table A5, the cohort decomposition pattern itself is mostly unaffected by allowing for later cohorts to have greater exposure to fraud.

Finally, I conduct a back-of-the-envelope exercise using estimates from the literature of the effect of misrepresentations on defaults and prices to bound the potential contribution of misrepresentations to the cohort pattern. For misrepresented origination characteristics to directly affect the cohort pattern, misrepresentations need to change across cohorts. Griffin and Maturana (2016) show that most types of misrepresentations were relatively constant across origination cohorts, limiting their scope to directly impact cohort-level default rates. However, they show that the prevalence of unreported second liens increased by around 10 pp from 2003-2005 before declining again. Conservatively using this as the maximal amount of unreported second liens across cohorts, I apply to it the estimates of the association between unreported second liens and defaults. Griffin and Maturana (2016) estimate that mortgages with an unreported second lien had 1.97 times higher delinquency odds, and Piskorski et al. (2015) estimate that delinquency rates were 10.15 pp higher for mortgages with unreported second liens. Using these estimates and the increase in default rates across cohorts in my data using their respective delinquency definitions, I estimate that a 10 pp increase in unreported second liens would explain 3-4% of the observed increase in delinquencies across cohorts. I also bound the indirect effect of misrepresentations on defaults through price declines given the result in Griffin et al. (2021) that average prices would have fallen by 3.8 pp less from 2007-2010 if the market share of mortgage originators with a high share of misrepresentations

[50]Because a zero fraud share is outside the range of the data for each of these measures, I choose the 25th percentile of each measure for the normalization. Accordingly, the cohort main effects controlling for the interactions capture the expected increase in the cohort default rate relative to the 2003 cohort in a CBSA at the 25th percentile of the distribution of each fraud measure.

were zero. Annualizing this effect of misrepresentations on prices, I multiply it by the price elasticity of default estimated in column 4 of Table 4. I conservatively estimate that the default hazard would have increased by 5.8% less across cohorts without fraud-induced stronger price declines, which potentially explains another 3.1% of the increase in defaults across cohorts. This bounding exercise helps explain why measures of the the prevalence of misrepresentations had limited additional power to explain the increase in defaults across cohorts in Appendix Tables A5 and A6.

Overall, the main conclusions from this analysis are that a) CBSA-level fraud measures are individually predictive of default, consistent with the literature, b) the observable individual-level origination characteristics and the CBSA-level fixed effects already seem to capture the bulk of the effect of CBSA-level fraud measures on default such that including CBSA-level fraud measures does not change the hazard model's ability to decompose cohort effects, and c) given the estimates in the literature on the increase in misreported characteristics across origination cohorts and the associated increase in the default rate, the ability of fraud to explain the cohort pattern is likely modest.

7 Estimating Counterfactual Default Rates

A useful way to characterize the relative importance of selection and circumstances in cohort default rates is to estimate default rates under alternative scenarios wherein credit standards did not decline across cohorts or all cohorts faced similar price declines at similar loan ages.[51] To account for the possibility that price changes were larger because of the subprime expansion, I also consider counterfactual scenarios with attenuated price changes. Because the coefficients in the control-function loan default model account for endogeneity and potential mismeasurement of local house prices, they can be used for such counterfactual estimation. I calculate expected cumulative default rates for each cohort using counterfactual explanatory variables as an estimate of the impact of the price and lending standards channels.

For the cumulative default rate, I estimate the cumulative incidence function for default, which takes into account that for default to be observed, prepayment must not yet have occurred. For counterfactual analysis, this is particularly crucial given that a key explanation for the cohort pattern in default rates is that prepayment risk varied significantly across cohorts such that counterfactual covariates will change the prepayment hazard, too. Using the control function specification estimated in column 4 of Table 4 as my preferred specification, I predict the cumulative default rate within t months of origination given covariates X. Extending the notation from section 3.3 to allow for failure-type-specific discrete hazard functions $h^d(\cdot|X)$ and $h^p(\cdot|X)$ for default and prepayment, respectively, and now denoting $S(\cdot|X)$ as the overall survival function instead of the default-time

[51] See Davis et al. (2022) for a related counterfactual exercise testing how prime mortgage default would have evolved under alternative scenarios.

survival function, I estimate the cumulative incidence function for default $CIF^d(t|X)$ as

$$\widehat{CIF}^d(t|X) = \sum_{s=1}^{t} h^d(s|X)S(s-1|X). \tag{21}$$

The overall survival function $S(t|X)$ can be estimated as

$$S(t|X) = \prod_{s=1}^{t}(1 - h^d(s|X))(1 - h^p(s|X)).$$

For default, the estimated $h^d(t|X_{icgt}) = \exp(X'_{icgt}\hat{\beta} + \hat{\rho}\hat{v}_{icgt} + \hat{\psi}_t)$ where $\hat{\psi}_t$ are nonparametric estimates of the log average baseline default hazard function between time $t-1$ and t as in (11). The prepayment hazard coefficients reported in Appendix Table A7 are estimated analogously using the specification of column 4 of Table 4. For each counterfactual scenario $\tilde{X}$, I calculate $\widehat{CIF}^d(60|\tilde{X})$ as in (21) for all loans in the sample, even ones that defaulted before $t = 60$ in the data. The expected share of loans in cohort c that will have defaulted within 60 months of origination is then

$$\widehat{\text{Cumulative Default Rate}}_c(60) = \frac{1}{N_c}\sum_i \widehat{CIF}^d(60, \tilde{X}_{icg60}) \tag{22}$$

where N_c is the number of cohort-c loans in the sample.

Table 6 shows the counterfactual five-year default rates for 12 scenarios, each representing a different combination of counterfactual price paths and loan characteristics.[52] The first row uses the estimates of column 4 of Table 4 and column 4 of Appendix Table A7 to estimate the cumulative incidence function for default and then average it across individuals within a cohort as in (22) to estimate the cumulative five-year default rate for each cohort —the share of each cohorts loans predicted to default within five years of origination. The spread between the predicted five-year default rates of 2003 and 2006 mortgages is 26 pp.

Rows 2 and 3 estimate the cumulative default rates that would have prevailed if all mortgages had the characteristics reported in Table 1 of the average 2003 (row 2) or 2006 (row 3) mortgage but faced actually realized price declines. Default rates would have been lower if the characteristics of mortgages had not deteriorated over time, especially for later borrower cohorts. If all borrowers had taken out the average 2006 mortgage, row 3 shows that default rates would have been roughly 2.5 pp higher for 2003-2005 cohorts than predicted in row 1. The spread between the 2003 and 2006 cohorts is cut by 13% to 30% (4-8 pp) in rows 3 and 2, respectively, by holding origination characteristics constant. However, even if the composition of mortgage products did not change from 2003 to 2006—a conceptual upper bound on the effect of tighter subprime mortgage regulation—the 2006 cohort default rate would still have been three times higher than the 2003 cohort default rate (row 2).

The remaining rows experiment with counterfactual price paths. Rows 4-5 use actual individual loan characteristics and two alternative price scenarios. Row 4 assigns each loan to have the average price change that 2003-cohort loans faced at the same number of months since origination. Row

[52] Appendix Table A8 reaches similar qualitative conclusions analyzing predicted two-year default rates.

5 repeats this exercise using the prices to which 2006-cohort loans were exposed. As expected, mortgages from every cohort would have defaulted much less if they had initially experienced several years of rapid price appreciation, as did 2003-cohort mortgages. Ceteris paribus, if 2006-cohort mortgages had faced the same prices that the average 2003 mortgage did (row 4), their default rate is predicted to have been 17% instead of 36% as in row 1. Similarly, if 2003-cohort mortgages had faced the prices that the average 2006 mortgage faced (row 5), their default rate would have been 21% instead of 10%. The spread between the 2003 and 2006 cohorts seen in row 1 is significantly smaller in rows 4-5, showing that if they had faced the same prices, the 2006 cohort default rate would have been 8-12 pp higher than the 2003 default rate instead of 26 pp higher in row 1.

In rows 6-7, I fix both characteristics and price changes at 2003 (row 6) or 2006 (row 7) levels. The combination of common prices and common mortgage characteristics explains most of the differences in the unadjusted cohort default rates of row 1. As a measure of the latent quality of each cohort, rows 6 and 7 suggest that declines in unobserved borrower quality not captured by loan and borrower observables are not particularly important in explaining the subprime cohort pattern.

Weren't prices themselves driven by the subprime credit expansion? For rows 8-12, I model an attenuated counterfactual trajectory of house prices using results from Griffin et al. (2021). While the control-function strategy above purges the estimated price elasticity of default of bias from potential correlation between local price changes and local credit conditions, the level of prices changes may have been different but for the nationwide subprime expansion. Griffin et al. (2021) estimate that a one standard deviation increase in the subprime share increased house prices by an additional 2.6 pp from 2003-2006 and decreased prices by an additional 3.7 pp from 2007-2010. This implies that nationwide, average price increases were 1.4 log points per year higher in 2003-2006 and 1.9 log points lower per year from 2007-2010 than they would have been if the subprime share had been zero nationwide. Rows 8-10 of Table 6 use these conservative counterfactual price changes with the estimated default and prepayment coefficients from Table 4 and Appendix Table A7. Row 8 looks at the effect of the attenuated prices with actual origination characteristics. The counterfactual default rates for the scenario in which there is attenuated price growth are in between the 2003 and 2006 price scenarios. Rows 9-10 report default rates under the counterfactual of an attenuated boom-bust price cycle and fixed mortgage characteristics at either the 2003- or 2006-cohort averages. Even with fixed mortgage characteristics and the average trajectory of prices estimated to have prevailed if the subprime expansion hadn't happened, there would still be a sizable spread between 2003 and 2006 cohort default rates.

Finally, rows 11 and 12, respectively, consider scenarios that hold mortgage characteristics fixed and fix prices at an attenuated version of 2003 and 2006 average price changes. Scenarios such as rows 6-7 that hold both prices and characteristics fixed facilitate fair comparisons of cohorts holding both circumstances and selection fixed. However, the *levels* of cohort default rates in these scenarios may be too low or high given that the average price changes faced by the 2003 and 2006 cohorts were likely higher and lower, respectively, than they would have been absent the mid-2000s subprime credit expansion. Nevertheless, the effect of this adjustment is minor. Overall default rates in row 11 are 0.4 pp higher than row 6, and overall default rates in row 12 are 2 pp lower than row 7, consistent with the relatively modest effect of the subprime expansion on the boom and bust in house prices estimated by Griffin et al. (2021).

Using as a benchmark any of the scenarios holding price changes and characteristics fixed across cohorts, it seems that the low and high actual default rates experienced by the 2003 and 2006 cohorts, respectively, were not particularly representative of the relative quality of these cohorts' unobservables. Notably, this demonstrates that raw comparisons of mortgage vintage performance are not a particularly useful barometer of lending standards given the historically abnormal price paths faced by each vintage.

8 Conclusion

This paper presents a methodology to account for endogeneity in duration models via a control-function approach. When covariates in a hazard model are endogenous or mismeasured, the ability of the model to evaluate claims about how the likelihood of failure would be different given alternative covariate values is limited. I demonstrate how instrumental variables that satisfy the usual relevance and exogeneity conditions can be used to consistently estimate causal effects in a proportional hazards model. The estimator readily accommodates nonparametric baseline hazard specifications and semi-parametric control-function specifications, while facilitating testing for covariate misspecification.

Using a novel identification strategy to address the joint endogeneity of defaults and house-price declines, I estimate a model of mortgage default that combines observable loan and borrower characteristics with data on price changes and explains 93% of the heterogeneity in the performance of subprime borrower cohorts. Mortgage default is particularly sensitive to changes in local house prices. Across a variety of instruments, I find an elasticity of default with respect to house prices changes of about -4.5, implying that for every 10% decrease in local house prices, the subprime mortgage default rate increases by 54%. I find that deteriorating underwriting standards can explain at most 30% of the default rate differences across cohorts. These results imply that while tighter subprime lending standards would have muted the increase in defaults somewhat, even the well-performing 2003-2004 subprime cohorts would have been sensitive to significant property value declines, especially early on in their life cycle.

Given their central role in precipitating the Great Recession, there has been sustained, active debate about the surge in the subprime-mortgage defaults in the late 2000s, resulting in new legislation, regulation, at least $150 billion in fines for mortgage-originating banks (Scannell, 2017), and many subprime MBS-related lawsuits. Much of the post-mortem analysis has focused on contrasting the relative performance of late and early borrower cohorts given the disproportionate share of subprime defaults by late-cohort borrowers. Diverse views of the cause of this deterioration in cohort-level mortgage outcomes have motivated strong opinions about the appropriate regulatory response to the subprime crisis. Advocates of stricter primary and secondary mortgage market regulation argue that the cohort pattern stems from a deterioration in underwriting standards over time, i.e., the lending of riskier mortgage products to riskier borrowers, and that these looser standards were the main cause of the crash. This view has motivated a regulatory response that aims to avert future crises and ensure that mortgages are robust to economic shocks, for example, by restricting the mortgage contract space.

Did the relatively more stringent underwriting of the 2003 cohort lead to that cohort's borrowers being relatively immune to the stress of negative price shocks? No, 2003 borrowers would have also been quite sensitive to price declines if they had faced them earlier in their mortgage life-cycle. I estimate that if 2003 borrowers had faced the prices that the average 2006 borrower did, 2003 borrowers would have had a five-year cumulative default rate of 21% instead of 10%. Similarly, only 17% of subprime loans in the maligned 2006 cohort would have defaulted within five years instead of 36% if they had faced the initial appreciation the 2003 mortgages did. Finally, I consider what cohort default rates would have been if the subprime credit expansion hadn't amplified the housing price cycle, again finding that cohorts would have defaulted at similar rates.

The results of this paper suggest a scope for both underwriting standards to ex-ante affect mortgage outcomes and for ex-post loan modification programs to reduce the frictions associated with negative equity and housing-market illiquidity. More broadly, the methodology in this paper is a useful input into designing and evaluating stress-testing and risk-management procedures. Attributing the ex-post performance of vintages of debt or insurance contracts to fixed differences in environmental conditions at issuance—a common analytical technique in the finance industry—is potentially misleading. This study highlights the reality that cohort outcomes are driven by both vintage effects (i.e., characteristics bottled into the contracts at origination) and path dependency, i.e., that exposure to economic conditions affect cohorts differently depending on their history. Finally, to the extent that macroprudential policy can be used to compress the amplitude and frequency of house-price cycles—a promising open question for research—policymakers could employ such tools to both avoid and cope with similar situations in the future.

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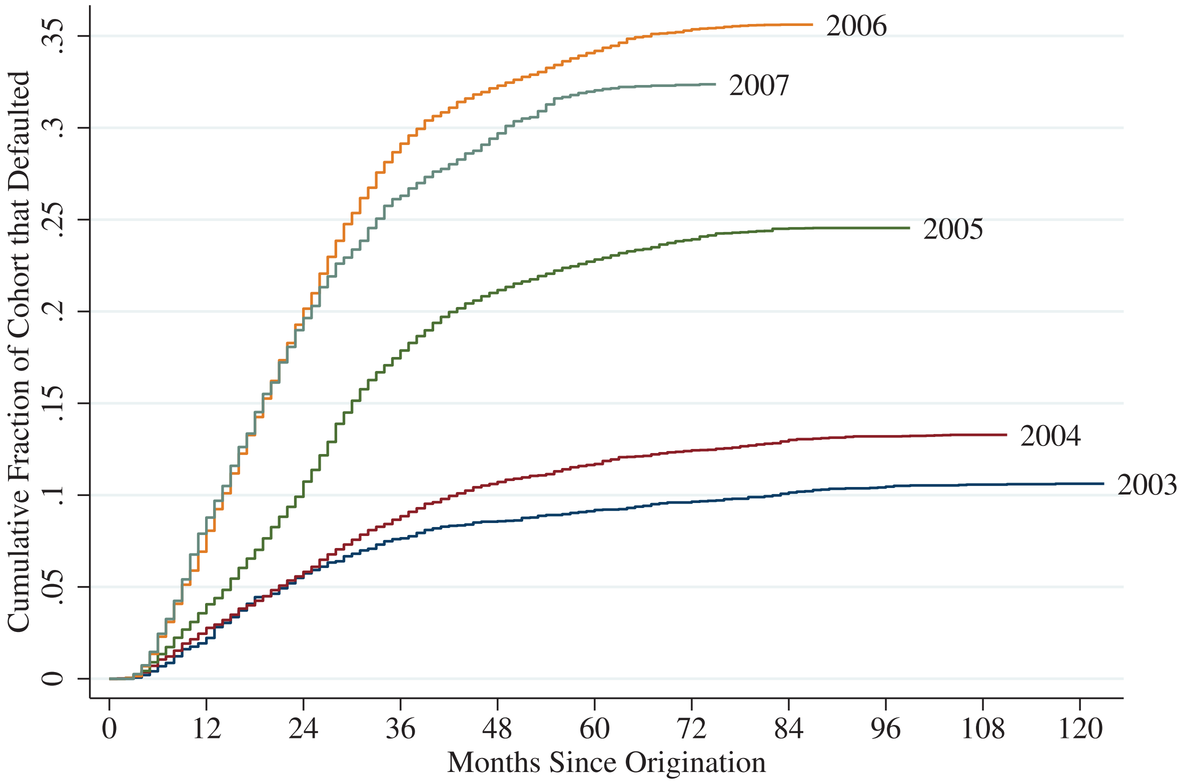
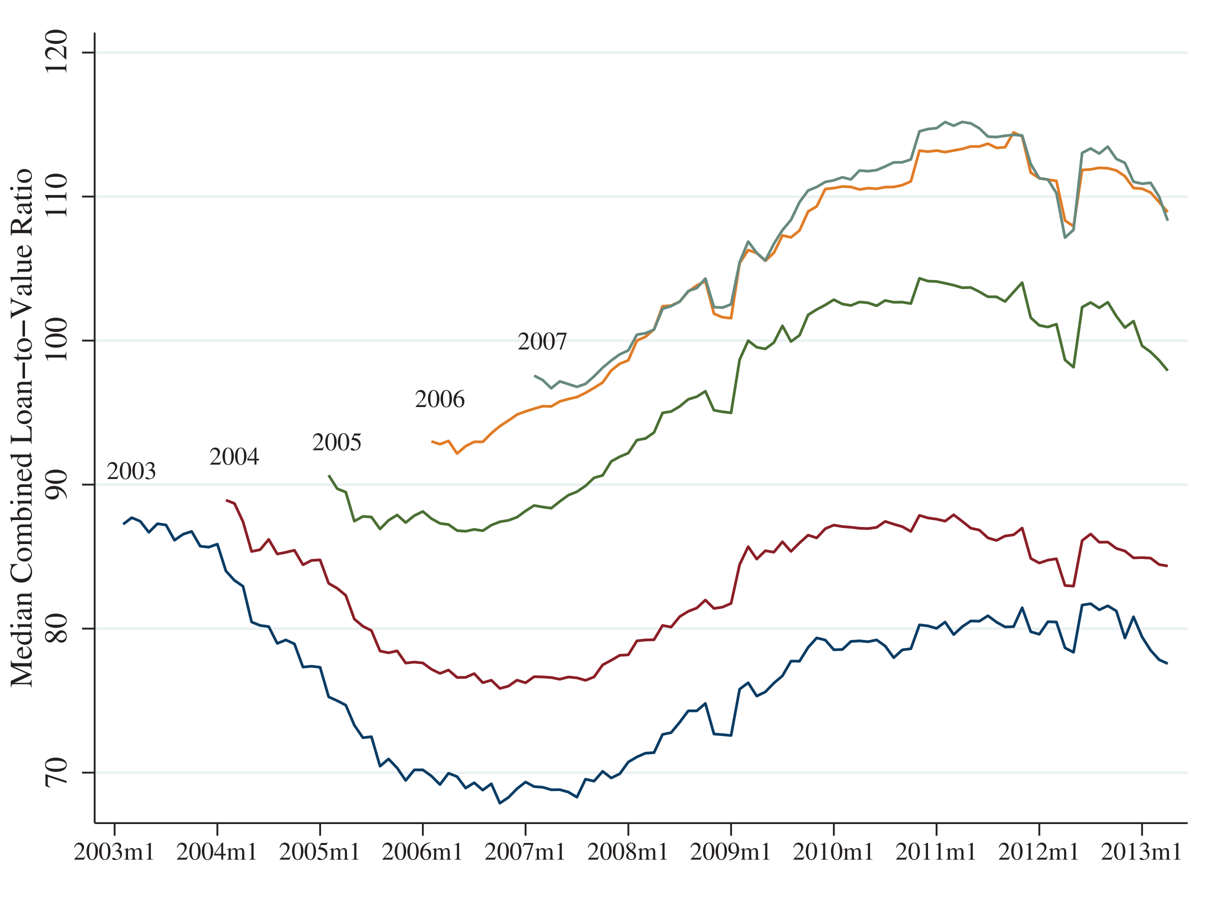
Figure 1: Cumulative Default Rate by Origination Cohort



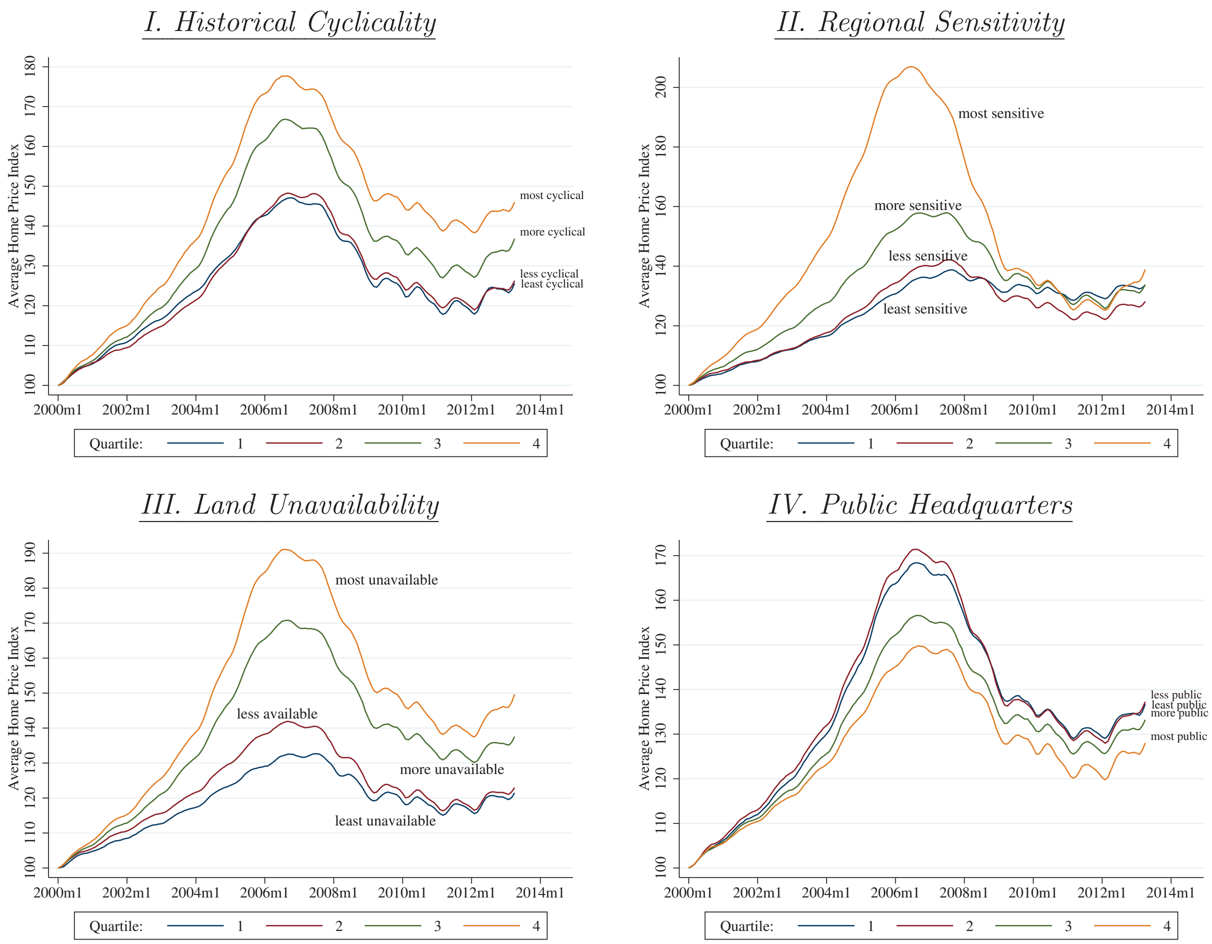
Notes: Figure plots the Kaplan-Meier estimate of the fraction of each cohort that has terminated by default within a given number of months since origination. Default is measured as the first time that a loan's delinquency status is marked as in foreclosure or real-estate owned provided it ultimately terminated without being paid off in full.

Figure 2: Median Current Combined Loan-to-Value Ratio Over Time by Origination Cohort



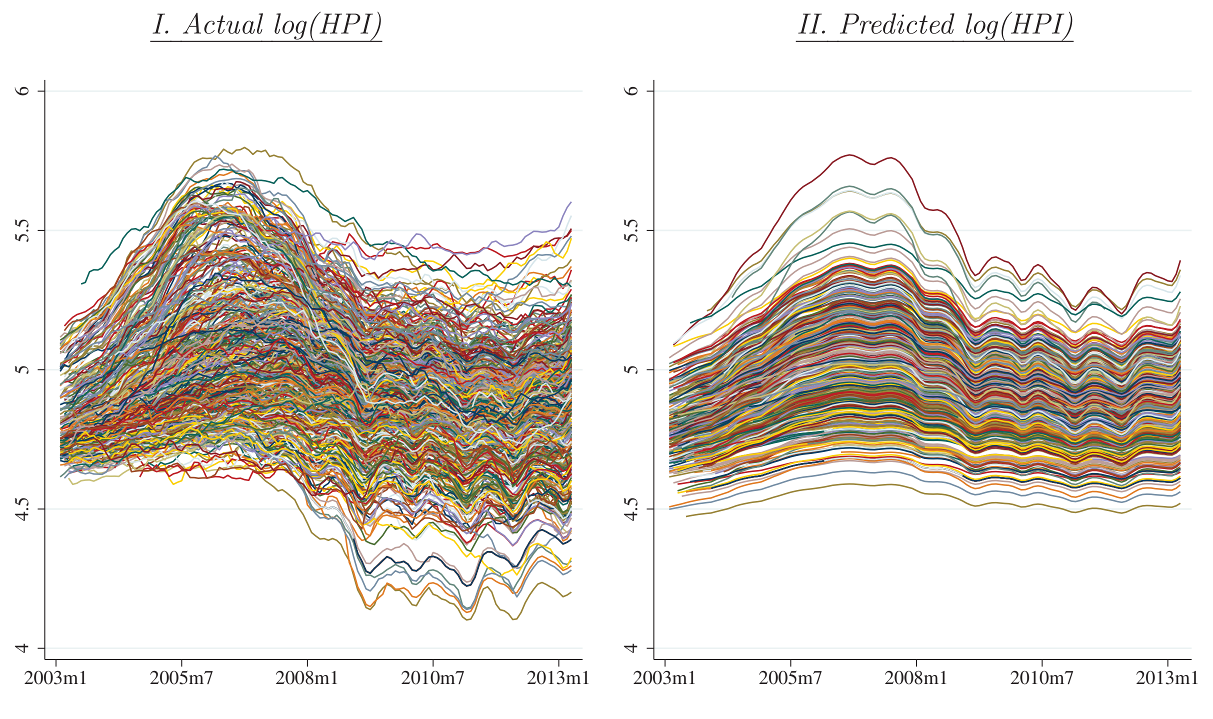
Notes: Figure shows the median current combined loan-to-value ratio (CLTV) of subprime borrowers for existing subprime mortgages in each cohort in each calendar month in percentage points. Current CLTV is calculated by LoanPerformance as the total outstanding principal on a loan divided by CoreLogic's automated assessing model's estimate of the market value of each home.

Figure 3: Average Home Price Index by Instrument Quartile



Notes: Figures plot monthly average HPI values by quartile of the indicated instrument. Cyclicality is measured as the standard deviation of one month changes to the log home price index from 1980-1995, as defined in equation (16) in the text. Regional sensitivity from Guren et al. (2021) is the sensitivity of each CBSA's price index to a regional price index. Land unavailability from Saiz (2010) is the share of land unavailable for development. Public headquarters from Choi et al. (2016) is the ratio of the total book value of publicly traded companies headquartered in each MSA to that MSA's income. See section 6.2.4 for more details. Each series has been normalized to 100 in January 2000.

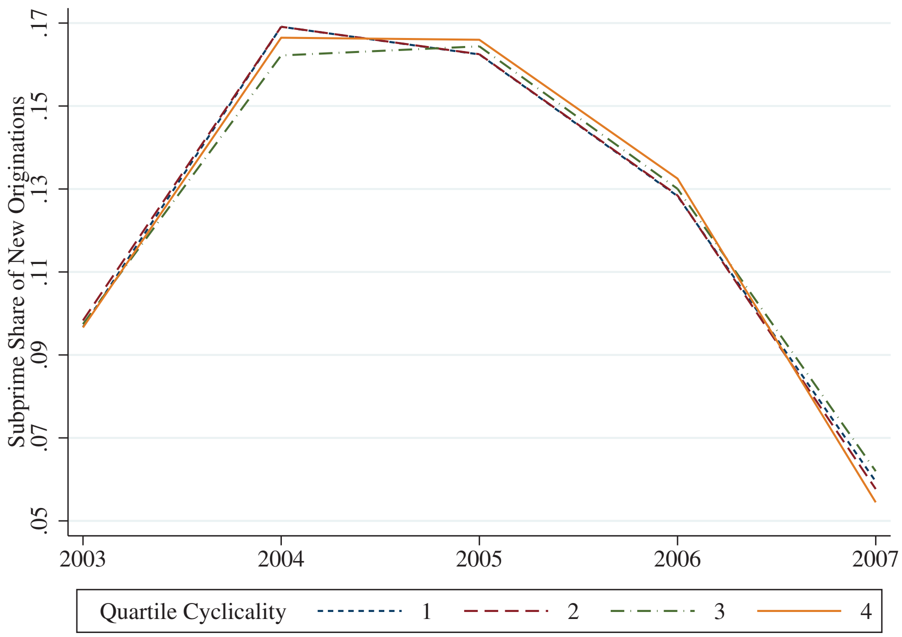
Figure 4: Observed and Predicted Home Price Indices



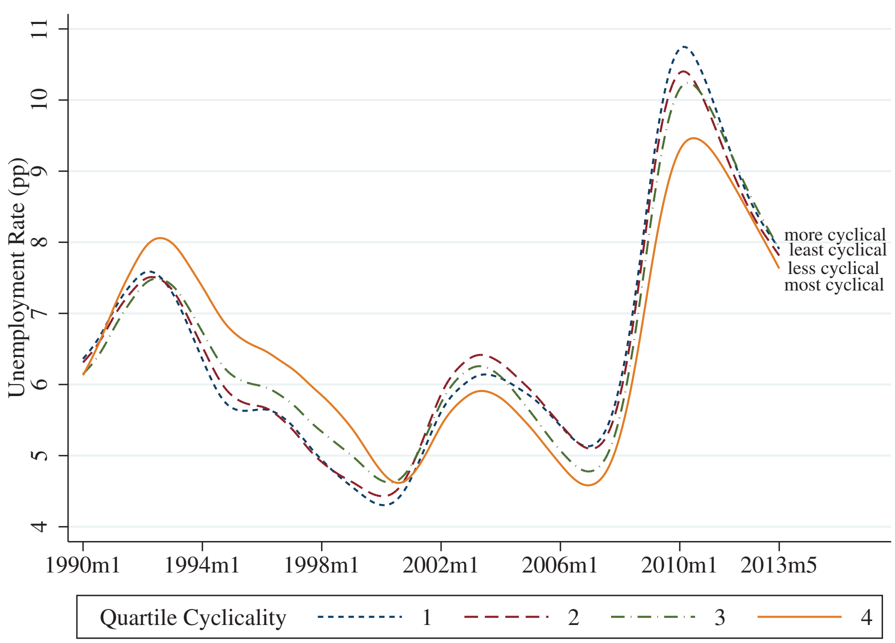
Notes: Figure plots observed log home price indices (left panel) and predicted indices (right panel) using long-run variation in the price cycle. The right-hand panel lines show the predicted values from a first stage regression of log(HPI) on CBSA fixed effects and the instrument set, as specified in equation (18) in the text.

Figure 5: Subprime Share and Unemployment Rate by Historical Cyclicality

I. Subprime Market Share by Historical Cyclicality Quartile

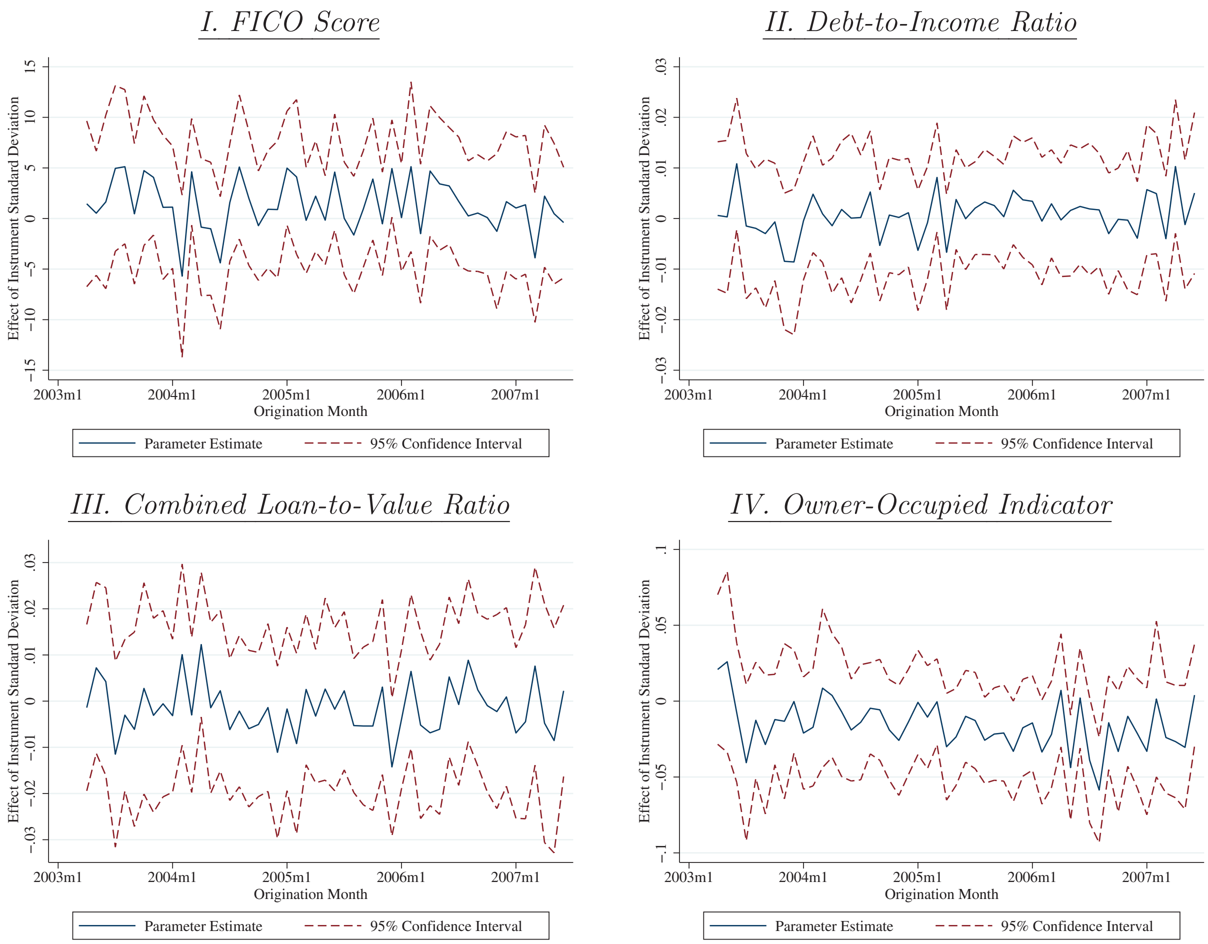


II. Unemployment Rates by Historical Cyclicality Quartile



Notes: Figures plot average subprime market shares (panel I) and average unemployment rates in percentage points (panel II) by quartile of the historical cyclicality measure defined by equation (16). Subprime market shares are calculated using HMDA data as the fraction of mortgages originated in a given year that were made by a lender on the HUD subprime lender's list in any year after residualizing for CBSA fixed effects. Unemployment rates are obtained from the Bureau of Labor Statistics Local Area Unemployment Series after residualizing for CBSA fixed effects and then filtering with a HP filter with $\lambda = 1{,}600$.

Figure 6: Testing Whether Historical Cyclicality Predicts Mortgage Characteristics



Notes: Figures plot regression coefficients of the indicated individual mortgage or borrower characteristic measured at origination on origination-month indicators interacted with the historical cyclicality instrument defined by equation (16). The instrument has been standardized such that plotted coefficients represent the relationship between a one standard deviation increase in σ_g^P in the given month and the average value of the indicated origination characteristic. All regressions control for CBSA fixed effects, cohort main effects, baseline hazard dummies, and the remaining mortgage or borrower characteristics used as controls in Table 2. Dashed lines plot 95% confidence intervals clustered at the CBSA level.

Table 1: Summary Statistics by Cohort

	Cohort					
	2003	2004	2005	2006	2007	Pooled
Default	0.11	0.13	0.25	0.36	0.32	0.24
	(0.31)	(0.34)	(0.43)	(0.48)	(0.47)	(0.42)
Prepaid	0.76	0.71	0.52	0.28	0.18	0.50
	(0.43)	(0.45)	(0.50)	(0.45)	(0.38)	(0.50)
FICO Score	617.0	618.2	618.6	616.1	614.3	617.3
	(61.9)	(61.2)	(59.7)	(56.5)	(54.7)	(59.1)
Debt-to-Income x DTI non-missing	0.29	0.30	0.27	0.32	0.31	0.30
	(0.19)	(0.19)	(0.20)	(0.19)	(0.19)	(0.19)
DTI missing	0.26	0.23	0.32	0.21	0.24	0.26
	(0.44)	(0.42)	(0.47)	(0.41)	(0.43)	(0.44)
Combined LTV	0.83	0.84	0.86	0.86	0.84	0.85
	(0.13)	(0.13)	(0.14)	(0.14)	(0.15)	(0.14)
Interest Rate	7.23	6.78	6.89	7.89	8.07	7.27
	(1.29)	(1.21)	(1.18)	(1.25)	(1.39)	(1.33)
Full Documentation	0.71	0.70	0.68	0.67	0.68	0.68
	(0.45)	(0.46)	(0.47)	(0.47)	(0.46)	(0.46)
Owner Occupied	0.91	0.91	0.92	0.92	0.91	0.92
	(0.28)	(0.28)	(0.28)	(0.27)	(0.29)	(0.28)
Cash-out Refi	0.57	0.57	0.53	0.51	0.58	0.54
	(0.50)	(0.49)	(0.50)	(0.50)	(0.49)	(0.50)
Adjustable Rate	0.61	0.63	0.57	0.45	0.34	0.54
	(0.49)	(0.48)	(0.50)	(0.50)	(0.47)	(0.50)
Interest-only	0.03	0.11	0.21	0.13	0.09	0.13
	(0.16)	(0.31)	(0.41)	(0.33)	(0.29)	(0.34)
Balloon	0.01	0.00	0.02	0.21	0.28	0.09
	(0.10)	(0.04)	(0.15)	(0.41)	(0.45)	(0.28)
Has 2nd Lien	0.07	0.15	0.24	0.28	0.16	0.20
	(0.25)	(0.36)	(0.42)	(0.45)	(0.37)	(0.40)
Observations	4,407	7,251	9,444	8,336	2,734	32,172

Notes: Table reports means and standard deviations in parentheses of individual loan characteristics by borrower cohort. Default, prepaid, and censored are indicator variables for a mortgage's termination type. The remaining characteristics are measured at time of origination. Full documentation, owner occupied, cash-out refinance, adjustable rate, interest-only, balloon mortgage, and has second lien are all indicator variables for the given characteristic. See section 5 in the text for more details.

Table 2: Default Hazard Model Estimates

	(1)	(2)	(3)	(4)	(5)
2004 Cohort	0.217***	0.223***	0.188***	0.137**	0.094
	(0.072)	(0.071)	(0.069)	(0.068)	(0.066)
2005 Cohort	0.717***	0.709***	0.519***	0.407***	0.190***
	(0.100)	(0.097)	(0.087)	(0.075)	(0.068)
2006 Cohort	0.954***	0.984***	0.579***	0.470***	0.045
	(0.130)	(0.129)	(0.121)	(0.093)	(0.086)
2007 Cohort	0.734***	0.800***	0.466***	0.235***	-0.107
	(0.120)	(0.116)	(0.116)	(0.083)	(0.084)
Δlog(HPI)				-3.685***	-3.857***
				(0.131)	(0.152)
CBSA Fixed Effects		✓	✓	✓	✓
Origination Characteristics			✓		✓
Observations	1,224,716	1,224,716	1,224,716	1,224,716	1,224,716
Log likelihood	-44,335	-43,574	-42,498	-43,142	-42,033

Notes: Table reports maximum-likelihood estimates of the default hazard model given in equations (2) and (14) in the text. The change in log(HPI) is the 12-month difference in the log of the CBSA-level CoreLogic HPI. Origination characteristics include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, an owner-occupied indicator, the combined loan-to-value ratio, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. All specifications include indicator variables for each value of loan age as a nonparametric baseline hazard function estimate. Standard errors in parentheses are clustered at the CBSA level.

Table 3: First-Stage Effect of Long-Run Cyclicality on Price Changes

	(1)	(2)	(3)
2004 Cohort		-0.009***	-0.009***
		(0.003)	(0.003)
2005 Cohort		-0.026***	-0.026***
		(0.006)	(0.006)
2006 Cohort		-0.047***	-0.045***
		(0.010)	(0.009)
2007 Cohort		-0.057***	-0.053***
		(0.011)	(0.011)
Baseline Hazard		✓	✓
CBSA Fixed Effects		✓	✓
Origination Characteristics			✓
Partial F-statistic	49.0	31.4	31.2
Observations	1,224,716	1,224,716	1,224,716
R-squared	0.50	0.56	0.56

Notes: Table estimates first stage specifications detailed by equation (17) by OLS. Dependent variable is the 12-month change in the log house price index. The 124 instruments are calendar month indicator variables interacted with the historical cyclicality measure defined by equation (16) in the text (coefficients not reported). Baseline hazard controls include indicator variables for each value of loan age. Origination characteristics include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, an owner-occupied indicator, the combined loan-to-value ratio, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. Standard errors in parentheses are clustered by CBSA.

Table 4: Control-Function Estimates of Default Hazard

	(1)	(2)	(3)	(4)
2004 Cohort	0.137**	0.127*	0.094	0.083
	(0.068)	(0.068)	(0.066)	(0.066)
2005 Cohort	0.407***	0.362***	0.190***	0.142**
	(0.076)	(0.075)	(0.068)	(0.067)
2006 Cohort	0.470***	0.392***	0.045	-0.034
	(0.093)	(0.095)	(0.086)	(0.088)
2007 Cohort	0.235***	0.146*	-0.107	-0.196**
	(0.084)	(0.087)	(0.084)	(0.088)
Δlog(HPI)	-3.685***	-4.381***	-3.857***	-4.597***
	(0.131)	(0.348)	(0.152)	(0.360)
Δlog(HPI) Fitted Residuals		0.977**		1.033**
		(0.409)		(0.419)
CBSA Fixed Effects	✓	✓	✓	✓
Origination Characteristics			✓	✓
Observations	1,224,716	1,224,716	1,224,716	1,224,716
Log likelihood	-43,142	-43,137	-42,033	-42,028

Notes: Table reports maximum-likelihood control-function estimates of the default hazard model given in equations (2) and (19) in the text with the first stage given by (17). The change in log(HPI) is the 12-month difference in the log of the CBSA-level CoreLogic HPI. Fitted residuals are estimated from a linear first stage regression of the 12-month change in the log price index on the instruments and remaining controls. Origination characteristics include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, an owner-occupied indicator, the combined loan-to-value ratio, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. All specifications include indicator variables for each value of loan age as a nonparametric baseline hazard function estimate. Standard errors in parentheses are adjusted for the first-stage estimation of the fitted residuals by GMM and clustered at the CBSA level.

Table 5: Control-Function Estimates of Default Hazard Using Alternative Instruments

Instruments	(1) Historical Cyclicality	(2) Regional Sensitivity	(3) Social Network	(4) Land Unavailability	(5) Public Headquarters
2004 Cohort	0.083	0.078	0.077	0.079	0.076
	(0.066)	(0.058)	(0.058)	(0.058)	(0.059)
2005 Cohort	0.142**	0.140**	0.122**	0.113**	0.160***
	(0.067)	(0.055)	(0.056)	(0.055)	(0.056)
2006 Cohort	-0.034	-0.030	-0.061	-0.088	-0.020
	(0.088)	(0.057)	(0.062)	(0.058)	(0.061)
2007 Cohort	-0.196**	-0.191***	-0.227***	-0.257***	-0.188***
	(0.088)	(0.066)	(0.071)	(0.067)	(0.069)
Δlog(HPI)	-4.597***	-4.536***	-4.716***	-5.178***	-4.512***
	(0.360)	(0.172)	(0.240)	(0.219)	(0.265)
Fitted Residuals	1.033**	1.779***	1.291***	2.161***	0.856***
	(0.419)	(0.288)	(0.303)	(0.287)	(0.319)
CBSA Fixed Effects	✓	✓	✓	✓	✓
Origination Char.	✓	✓	✓	✓	✓
First-stage F-statistic	31.2	308.7	102.4	40.7	19.6
Observations	1,224,716	1,195,600	1,195,600	1,213,592	1,127,673
Log likelihood	-42,028	-41,228	-41,239	-41,798	-38,831

Notes: Table reports maximum-likelihood control-function estimates of the default hazard model given in equations (2) and (19) in the text with the first stage given by (17), replacing the instrument set with the instrument indicated in each column. The change in log(HPI) is the 12-month difference in the log of the CBSA-level CoreLogic HPI. Fitted residuals are estimated from a linear first stage regression of the 12-month change in the log price index on the indicated instruments and remaining controls. Origination characteristics include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, an owner-occupied indicator, the combined loan-to-value ratio, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. All specifications include indicator variables for each value of loan age as a nonparametric baseline hazard function estimate. Standard errors in parentheses are adjusted for the first-stage estimation of the fitted residuals by GMM and clustered at the CBSA level.

Table 6: Counterfactual 5-Year Default Rates by Cohort

	Counterfactual Scenario		Predicted 5-Year Cumulative Default Rate					
	Prices	Loan Composition	2003	2004	2005	2006	2007	Overall
(1)	Actual	Actual	9.8%	13.1%	23.7%	35.8%	33.1%	23.3%
(2)	Actual	2003	9.0%	11.8%	20.3%	27.1%	25.9%	19.1%
(3)	Actual	2006	12.2%	15.7%	26.3%	34.7%	33.6%	24.8%
(4)	2003	Actual	9.3%	10.4%	13.7%	17.4%	16.3%	13.5%
(5)	2006	Actual	21.3%	23.4%	28.6%	33.1%	29.8%	27.7%
(6)	2003	2003	8.6%	9.3%	11.0%	11.6%	11.4%	10.5%
(7)	2006	2006	26.7%	28.3%	31.7%	31.3%	29.6%	30.0%
(8)	Attenuated	Actual	10.3%	13.3%	23.0%	33.8%	31.3%	22.6%
(9)	Attenuated	2003	9.5%	12.0%	19.6%	25.4%	24.3%	18.4%
(10)	Attenuated	2006	12.9%	16.1%	25.5%	32.7%	31.6%	24.0%
(11)	2003 Attenuated	2003	9.1%	9.8%	11.5%	12.0%	11.8%	10.9%
(12)	2006 Attenuated	2006	24.8%	26.3%	29.6%	29.3%	27.7%	28.0%
		Number of Loans	4,407	7,251	9,444	8,336	2,734	32,172

Notes: Table reports estimated cumulative default rates under the indicated counterfactual scenarios of prices and loan characteristics. Cumulative default rates are defined as the predicted share of loans that would have defaulted within 60 months of origination. Scenarios using actual characteristics retain observed covariates. Scenarios using a given year's prices replace all price changes with the average price changes faced by the given year's borrowers at each value of loan age. Scenarios using a given year's loan characteristics assign all loans the average characteristics from the indicated cohort. Attenuated price scenarios in rows 8-10 replace actual 2003-2010 price changes with smaller magnitude price changes that Griffin et al. (2021) estimate would have prevailed if the subprime market share were zero. Attenuated scenarios in rows 11 and 12 replace all price changes with the average price changes faced by the 2003 or 2006 cohorts, respectively, attenuated using Griffin et al. (2021) estimates. See section 7 for more details.

Internet Appendix

A Proof of Theorem 1

Given that in a survival model, the likelihood of failure depends on whether failure has already occurred in a prior observation, classical GMM theoretical results that assume stationary, independent, and identically distributed processes do not apply. Instead, the additional regularity conditions below allow me to cast the MLE estimator defined in section 3.3 as a non-classical GMM estimator and use the Lindeberg-Feller Central Limit Theorem to invoke the consistency and asymptotic normality theorem of Huber (1967) to prove Theorem 1. Because the control-function survival estimator has the same sandwich form for its asymptotic variance as classical GMM, the sequential GMM results from Newey (1984) and Newey and McFadden (1994) apply. As such, the asymptotic variance of the control-function survival estimator can be estimated using the standard GMM variance formula after stacking the first- and second-stage moment conditions, as described in section 3.3.1.[53]

In order to prove Theorem 1, I first require the following additional regularity conditions. For expositional clarity in what follows, I let i index observations (previously indexed by $icgt$).

Assumption A1 (Regularity Conditions)**.** *The following conditions hold on the the parameter space $(\beta, \psi, \rho, \pi) \equiv \theta \in \Theta$, the covariates X and instruments Z, and the log likelihood function $\ell = \sum_i \ell_i$, with ℓ_i defined by (10).*

1. *The true parameter θ_0 lies in the interior of a compact parameter space Θ.*
2. *The matrix of covariates X and the matrix of instruments Z are each full rank.*
3. *The Lindeberg Condition holds for the individual-specific second-stage quasi-score contributions. Formally, this means that for each observation i, each element j of the MLE first-order conditions $g_\ell(\theta)$ defined by (12) has finite mean and variance and satisfies for all $\epsilon > 0$:*

$$\lim_{n\to\infty} \frac{1}{s_n^2} E\left[\mathrm{Var}(g_{\ell i,j}(\theta)) \cdot 1\left(|g_{\ell i,j}(\theta) - E\left[g_{\ell i,j}(\theta)\right]| > \epsilon s_n\right)\right] = 0,$$

where $s_n^2 \equiv \sum_{i=1}^{n} \mathrm{Var}(g_{\ell i,j}(\theta))$.

Parameter space compactness will help establish uniform convergence. The true parameter lying in the interior of Θ is required for asymptotic normality but is mild given that Θ is not fixed. A sufficient condition is that θ_0 is real valued and there are no constraints on the parameters, in which case there always exists a sufficiently large compact parameter space that includes θ_0 in its interior (Bahadur, 1971). Assumption A1.2 is the mild MLE equivalent of no collinearity among the covariates or instruments and implies that the expected Hessian of the log likelihood function ℓ exists and is non-singular when evaluated at the true parameter θ_0. Note that Assumption 4 is a necessary condition for Assumption A1.2. Finally, the Lindeberg Condition in Assumption A1.3 is weaker than the finite variance and identically and independently distributed assumptions needed to invoke the Lindeberg-Levy Central Limit Theorem. Given the non-stationarity of duration models, I follow Prentice and Gloeckler (1978) and instead use the Lindeberg-Feller Central Limit Theorem. Intuitively, the Lindeberg Condition means that no single observation can affect the MLE first-order condition for any variable too much. Sufficient conditions for the Lindeberg Condition to hold are that the covariates are bounded and the hazard rate is bounded away from zero.

More formally, Theorem 1 can be restated as follows.

[53] See Liu (2023) for a more detailed discussion of the proof.

Restatement of Theorem 1. *Under Assumptions 1, 3-5, and A1, the maximum-likelihood estimator $\hat{\theta}$ obtained by estimating the first-stage (6) by OLS and then maximizing the sum of the log likelihoods defined by (10) is consistent and asymptotically normal, with*

$$\sqrt{n}(\hat{\theta} - \theta_0) \xrightarrow{d} \mathcal{N}(0, G^{-1}\Omega(G^{-1})'),$$

where θ_0 is the true parameter, $\xrightarrow{d}$ signifies convergence in distribution, $\mathcal{N}(\cdot,\cdot)$ is the normal distribution, G is the derivative of the GMM moment conditions $g(\cdot)$ defined in (12), and Ω is the asymptotic variance of $g(\cdot)$.

Proof. To prove this theorem, it suffices to show that five key conditions are met, in which case Huber's (1967) Z-estimation consistency and asymptotic normality theorem holds for $\hat{\theta}$. Z-estimation is extremum estimation with a differentiable objective function and can be considered a non-classical generalization of GMM. Restated for my setting, the necessary conditions are as follows.

Assumption A2 (Conditions for Huber (1967) Z-estimation Theorem)**.** *The following conditions hold on the score $g(\theta) = E(g_i(\theta))$ and its empirical analog $\hat{g}(\hat{\theta})) \equiv \sum_{i=1}^{n} g_i(\hat{\theta})/n$, where $g_i(\cdot)$ is the vector of moment conditions for observation i of the GMM estimator as in (12), and the summation is taken over all observations.*

1. *The estimator $\hat{\theta}$ uniquely solves the quasi-maximum likelihood objective function $\hat{g}(\hat{\theta})) = 0$ and the true parameter value θ_0 is the unique solution of $g(\theta_0) = 0$.*

2. *$\sqrt{n}\,(\hat{g}(\theta_0) - g(\theta_0)) \xrightarrow{d} \Gamma_0$, where Γ_0 is some random variable that has a known distribution.*

3. *The score $g(\cdot)$ is Frechet-differentiable at θ_0 with nonsingular derivative $G(\theta_0)$.*

4. *For every sequence $\delta_n \to 0$ as $n \to \infty$,*
$$\sup_{\|\theta-\theta_0\|\leq\delta_n} \frac{\|\sqrt{n}\,(\hat{g}(\theta) - g(\theta)) - \sqrt{n}\,(\hat{g}(\theta_0) - g(\theta_0))\|}{1 + \sqrt{n}\,\|\theta - \theta_0\|} \xrightarrow{p} 0.$$

5. *The true finite-dimensional parameter θ_0 is in the interior of the parameter space Θ.*

To see that each of the conditions in A2 holds given the assumptions in A1 above, first consider the quasi-score of the proportional hazards model:

$$\hat{g}_\ell(\theta) = \frac{1}{n}\sum_{i=1}^{n} \frac{\partial \ell_i}{\partial \theta}.$$

To augment this estimator to account for the estimates of the control function, I follow Newey (1984) and Newey and McFadden (1994) to stack the moment conditions for the first-stage estimation. I define

$$\hat{g}_\pi(\theta) = \frac{1}{n}\sum_{i=1}^{n} Z_i'(x_i - Z_i'\pi).$$

Then the combined Z-estimator criterion defining the estimator $\hat{\theta}$ is $\hat{g}(\hat{\theta}) \equiv \begin{pmatrix} \hat{g}_\ell(\hat{\theta})' & \hat{g}_\pi'(\hat{\theta})' \end{pmatrix}' = 0$.

To establish Condition A2.1, the uniqueness of both $\hat{\theta}$ and θ_0 follows from the monotonicity of $\hat{g}_\ell(\cdot)$ and $g_\ell(\cdot)$ (see Prentice and Gloeckler, 1978) and of $\hat{g}_\pi(\cdot)$ and $g_\pi(\cdot)$. The Leibniz integral rule holds here at θ_0 (Liu, 2023), implying that the score has expectation zero at θ_0 whenever the MLE first-order conditions (FOCs) hold in expectation at θ_0, in which case $g(\theta_0) = 0$. Assumptions 1 and 3-5 imply the MLE FOCs will be expectation zero at θ_0. If, for example, the exclusion restriction in Assumption 5 fails, then the MLE FOC will hold at some $\theta \neq \theta_0$.

Condition A2.2 is satisfied because the Lindeberg Condition holding (Assumption A1.3) is sufficient for the Lindeberg-Feller Central Limit Theorem to hold, meaning A2.2 holds with Γ_0 being a mean-zero normal distribution. Assumption A1.2 means that the score's derivative will be invertible (i.e., the information matrix equality holds) so Condition A2.3 will hold.

To satisfy Condition A2.4, it is sufficient to demonstrate uniform convergence of $\hat{g}(\cdot)$. Given the compact parameter space and the Lindeberg Condition, the absolute value of the components of $\hat{g}(\cdot)$ is bounded over the parameter space such that the conditions of the Uniform Weak Law of Large Numbers stated in Theorem 12.1 of Wooldridge (2002) are each met. Accordingly, $\hat{g}(\cdot)$ will converge uniformly, satisfying Condition A2.4.

Finally, Condition A2.5 is directly provided by regularity Assumption A1.1. As mentioned above, a sufficient condition for θ_0 being in the interior of Θ is that θ_0 is a real-valued vector and there are no shape restrictions on Θ.

Thus, sufficient conditions for the Z-estimator consistency and asymptotic normality theorem of Huber (1967) are met, and the control-function survival estimator is consistent and asymptotically normal with the usual GMM variance. □

B Unobserved Heterogeneity

This appendix examines the robustness of the results in Table 2 to misspecification from ignoring independent unobserved heterogeneity ε by allowing the true hazard model to be specified as in (13). This approach is referred to in the duration literature as the Mixed Proportional Hazards Model. There is a large literature on the relative merits of parametric assumptions on the baseline hazard function and the unobserved heterogeneity distribution. See Lancaster (1979), Heckman and Singer (1984), Han and Hausman (1990), Meyer (1990), Horowitz (1999), and Hausman and Woutersen (2014).

Equation (13) pins down the conditional distribution F of latent failure times τ to be

$$F(\tau|X_{icgt},\, \varepsilon_i) = 1 - \exp\left(-\Lambda((t|X_{icgt}, \varepsilon_i)\right)$$

where $\Lambda(\cdot|X, \varepsilon)$ is the integrated hazard. Specifying the distribution of ε to have cumulative distribution function $R(\cdot)$, the distribution $\tilde{F}(\tau|X_{icgt})$ of $\tau|X$ is then obtained by integrating out ε.

$$\tilde{F}(\tau|X_{icgt}) = \int_{-\infty}^{\infty} F(\tau|X_{icgt}, \varepsilon_i) dR(\varepsilon_i) \tag{B.1}$$

The modified likelihood $\tilde{h}(t|X)$ of observing failure at time $\tau \in (t-1,\, t]$ is then

$$\tilde{h}(t|X) = 1 - \tilde{S}(t|X)/\tilde{S}(t-1|X) \tag{B.2}$$

where the new survivor function is denoted $\tilde{S}(\cdot|X) = 1 - \tilde{F}(\cdot|X)$. Estimation then proceeds by replacing $h(\cdot|X)$ with $\tilde{h}(\cdot|X)$ in the log-likelihood expression of equation (10).

The results of maximizing the sample log-likelihood function described by (10), replacing $h(t|X)$ with $\tilde{h}(t|X)$ defined in equation (B.2) and modeling $\varepsilon \sim \mathcal{N}(0, \sigma^2)$, are presented in Appendix Table A9. Consistent with the survivorship-bias intuition discussed in section 3.3.2 above, column 1 shows that the unadjusted differences in default rates across cohorts are even more pronounced when accounting for independent unobserved heterogeneity than in the baseline results of column 1 of Table 2. To account for this in the exercise of decomposing the strongest factors in the cohort default pattern, I compare the adjusted cohort coefficients in columns 2-4 to the unadjusted cohort coefficients in column 1. Including CBSA fixed effects, borrower and loan characteristics in column 2 explains 30% of cohort heterogeneity—the average decrease in the estimated cohort dummies. Controlling instead for CBSA fixed effects and 12-month price changes in column 3 reduces the residual difference in the default hazard across cohorts by an average of 61%. Conditioning on price changes and loan and borrower characteristics in column 4 explains 81% of the cohort differentials in column 1.

As before, the 2005 cohort is the only borrower cohort to have a default hazard that is statistically distinguishable from the 2003 cohort hazard after adjusting for prices and loan and borrower observables, although these covariates explain 73% of the 2005 cohort coefficient in column 1. I conclude that the qualitative pattern of Table 2 is robust to allowing for independent unobserved heterogeneity: prices explain over 60% of cohort heterogeneity in default risk and combined with borrower and loan characteristics explain most of the increase in defaults across cohorts. This has important implications for the robustness of these results to employing correlated competing risks specifications such as the estimators developed by Han and Hausman (1990) and McCall (1996), which allow for correlated unobserved heterogeneity in both the default and prepayment hazards. Given the insensitivity of the cohort pattern decomposition to allowing for unobserved heterogeneity, the default hazard can safely be estimated separately, as discussed above.

C Robustness to Control Function Specification

In general, the control function $c(v)$ need not be linear. I approximate an arbitrary form of $c(\cdot)$ in Appendix Table A4 using third- and fifth-order polynomials in the fitted residuals, i.e., $c(v_{icgt}) = \sum_{k=0}^{5} \rho_k \hat{v}_{icgt}^k$. Columns 1-3 of Appendix Table A4 do not control for borrower or loan characteristics. Column 1 is repeated from column 2 of Table 4 for convenience. Column 2 adds squared and cubed residuals. These coefficients are strongly significant, and a likelihood ratio test for the hypothesis that $\rho_2 = \rho_3 = 0$ rejects, pointing to likely non-normality of the unobserved heterogeneity that is correlated with price shocks. However, the slope coefficients are relatively unaffected from the additional flexibility in the form of $c(\cdot)$. Column 3 adds fourth- and fifth-order terms, which again do not noticeably affect the estimated effect of prices or differences in the latent quality of cohorts. The estimated coefficients $\hat{\rho}$ on the powers of the residuals in column 3 are very imprecise, and a likelihood ratio test fails to reject that $\rho_4 = \rho_5 = 0$. Columns 4-6 repeat the specifications in columns 1-3, additionally controlling for borrower and loan characteristics. The same findings are apparent: higher-order polynomial terms of the residuals are jointly significant, rejecting the exogeneity of price changes, and the estimated effects of the covariates are relatively unchanged. Consistent with results only allowing for a linear control function, price declines alone explain over 60% of the deterioration in subprime mortgage cohort performance over time. The final model in either columns 5 or 6 explains 96% of cohort heterogeneity in default rates, with the cohort effects each insignificant at the 95% confidence level.

D Mechanisms

Through what mechanisms do price changes affect default rates? Are price declines important for defaults only insofar as they push borrowers into negative equity positions? Below, I estimate the causal effect of negative equity on defaults and to what extent indebtedness itself explains the increase in defaults across subprime borrower cohorts.

The intuition above focuses on the differential effect of price declines on later cohorts in pushing them underwater, as seen in Figure 2. In contrast to underwater homeowners, positive-equity but distressed borrowers (i.e., borrowers unable to make their monthly mortgage payments) have two main alternatives to default. First, homeowners with positive equity can sell their home and use the proceeds to pay off their outstanding mortgage debt. Second, positive-equity borrowers can sometimes refinance into a lower payment and/or extract equity via a second lien to temporarily help make their first-mortgage payments. These options are not readily available to distressed borrowers who are underwater. Lenders are normally unwilling to originate a refinance mortgage to someone who does not have sufficient equity, let alone negative equity—see DeFusco and Mondragon (2020) for a discussion. Furthermore, selling a house secured by a mortgage in a negative equity position (known as a short sale) requires either coming up with sufficient cash to pay the shortfall between the sale price and the outstanding debt or working with the lender (and any junior lienholders) to secure forgiveness of the remaining debt. By definition, distressed borrowers are unlikely to have ample savings, making the former unlikely. Lenders are also wary of agreeing to short sales, partly because of asymmetric information about the borrower's current and future finances (Zhang, 2019). An additional source of elevated default risk for underwater borrowers comes from the possibility of strategic default (Gerardi et al., 2018), although recent work questions the empirical relevance of strategic default in explaining mortgage default (Ganong and Noel, 2023).

Empirically testing that the differential prevalence of negative equity is the channel through which price declines explain so much cohort heterogeneity presents several practical challenges. First, a borrower's current mortgage balance is determined by initial balance and principal paid down, which in turn are endogenous to a borrower's unobserved credit and income risk. I instrument for the actual balance of the mortgage with the scheduled balance calculated using the origination interest rate as if the borrower had paid down a 30-year fixed-rate mortgage on schedule. Second, constructing a measure of negative equity status requires knowing the current market value of the home, an unknown (and endogenous) quantity that must be estimated by the borrower as well as the econometrician. CoreLogic provides such a measure using their Automated Valuation Model that imputes property values in each month for each subprime mortgage in the data. As this estimated value is measured with error and partly a function of nearby market prices and therefore affected by CBSA-level shocks, I instrument for this valuation using the origination loan amount and counterfactual price indices computed using the historical cyclicality instruments, similar to above. Third, because the prepayment obstacles faced by borrowers depend on the total debt of all loans secured against their home, measuring current negative equity necessitates knowing updated information about additional liens. Not observing updated information on the outstanding balance of additional liens, I assume that all observed second mortgages have not been paid down. Although this introduces additional measurement error into the estimated balances, instrumenting for outstanding balances using scheduled balances addresses this concern. This approach also addresses concerns that CLTVs are misstated because of unreported second lines (Griffin and Maturana, 2016).

I define the variable $Underwater_{it}$ for whether the current CLTV of a loan, estimated by CoreLogic based on the outstanding debt owed to all outstanding liens and contemporaneous market conditions, is greater than 100%. I first present estimates that do not account for the endogeneity of CLTV. Appendix Table A3 contains default hazard specifications as above, replacing $\Delta Prices$ with functions $q(\cdot)$ of CLTV

$$X_{icgt}'\beta = \gamma_c + W_i'\theta + \eta' q(CLTV_{it}) + \alpha_g. \tag{D.1}$$

Controlling for $q(CLTV) = 1(CLTV > 1) = Underwater$ in addition to loan and borrower characteristics and CBSA fixed effects in column 1 of Appendix Table A3 shows that underwater mortgages have more than double the conditional default probability of mortgages that are not underwater. However there is substantial unexplained heterogeneity across cohorts in column 1. Compared with column 5 of Table 2, the underwater indicator variable explains much less cohort heterogeneity than the 12-month change in prices. Even after adjusting for location, mortgage age, borrower and loan characteristics, and the estimated negative equity status, differences in cohort default rates relative to the 2003 cohort are all positive and significant, with the exception of the 2007 cohort. Column

2 of Appendix Table A3 tests whether this is driven by the functional form restriction on $q(\cdot)$ by controlling for a linear spline in the current CLTV that allows for a location and scale shift in the effect of $CLTV$ in several bins:

$$q(CLTV_{it}) = \sum_{j=1}^{J} 1(CLTV_{it} \in \mathcal{C}_j) \times (a_j + b_j CLTV_{it}) \tag{D.2}$$

where j indexes the set $\mathcal{C}$ consisting of J CLTV intervals $\{[0,80),[80,85),[85,90),...,[150,\infty)\}$. Adding flexibility in the specification of the leverage function $q(\cdot)$ further decreases the adjusted differences across cohorts but only explains on average an additional 8% of the differences in the latent quality of cohorts. The specification in column 2 explains 62% of the cohort-level differences in default.

To test whether the effect of prices is driven entirely by negative equity or whether price changes still explain cohort heterogeneity even conditional on underwater status, column 3 additionally controls for the twelve-month change in log HPI. Adding in the price change variable to the linear spline controls significantly affects the estimated cohort heterogeneity relative to column 2 but also relative to column 5 of Table 2, which is identical to column 3 except for the inclusion of $q(CLTV)$. This suggests that CLTVs and prices interact in explaining defaults, intuitively due to borrowers with less equity being more sensitive to price declines. Controlling for both price changes and current CLTVs reduces the 2006 and 2007 cohort differences to be strongly negative. In other words, 2006-2007 cohort borrowers defaulted less than would be expected controlling for a flexible function of their relative equity, the price changes they faced, and loan and borrower characteristics. The estimated latent quality of the 2004-2005 cohorts in column 3 is significantly worse than the 2003 cohort, with the 2005 estimate smaller and the 2004 estimate larger than the results in Table 2 that do not control for current CLTVs. The coefficient on the price change variable is large and significant.

There is a significant relationship between defaults and negative equity but also evidence that prices affect defaults in other ways than through negative equity. Still, as discussed above, caution is required interpreting these results because mark-to-market leverage (CLTV) could be correlated with unobserved borrower quality.

D.1 Instrumenting for Loan-to-Value Ratios

The main obstacle in interpreting the results in columns 1-3 of Appendix Table A3 is the endogeneity of CLTVs, which are the ratio of loan principal balances and property values. To the extent that borrowers whose unobserved quality is high pay back their mortgages more rapidly, loan balances (and hence CLTVs) will be determined in part by unobserved borrower quality.[54] Similarly, borrowers with lower unobserved quality may take out mortgages with higher initial balances or slower amortization schedules that leave them more likely to be underwater. To address the endogeneity of the CLTV numerator, I calculate the scheduled loan principal amount at each month since origination if borrower i had taken out a 30-year fixed interest rate loan with the same origination interest rate and purchase price and was always current on his or her payments.[55] Using the amortization formula,

$$Scheduled\ Principal_{it} = Origination\ Amount_i \left(\frac{(1+r_i)^{360} - (1+r_i)^t}{(1+r_i)^{360} - 1} \right) \tag{D.3}$$

[54] For example, statistically, low FICO-score borrowers are less responsive to in-the-money refinance opportunities.

[55] Cunningham and Reed (2013) refer to a similar approach as a synthetic-mortgage IV strategy.

where $Origination\ Amount_i$ is the originated loan size of mortgage i, t is the loan age in months, and the monthly interest rate r_i is the origination interest rate divided by 12.

To account for the endogeneity in purchase prices, I compute what the purchase price of the home would have been if the borrower had taken out only a first-mortgage for the same dollar amount at the GSE conforming loan leverage ratio (80% of purchase price). In logs, using this predicted purchase price is equivalent to using the log of the origination amount as an instrument. Finally, with the predicted home price indices, I can calculate an alternative measure of the change in a property's value since origination using the predicted HPI series

$$\widehat{Appreciation}_{igt} = \widehat{\log HPI}_{igt} - \widehat{\log HPI}_{ig1} \tag{D.4}$$

where $\widehat{\log HPI}$ are the predicted values from estimating equation (18).

Before presenting first stage results, Appendix Figure A8 illustrates the statistical relationship between each of the three instruments and the corresponding component of CLTVs. Diagonal lines depict the fitted bivariate linear regression line. Panel I plots actual log principal balances versus scheduled log principal balances. The fit is strong and the slope of the bivariate regression line is close to 1, showing the tight relationship between traditional amortization schedules and loan balances. The most noticeable deviation is the presence of many outliers well below the regression line, representing a minority of people that paid their mortgages back faster than scheduled. Instrumenting will address the possibility that their faster payback is a signal of these borrowers' unobserved (high) quality.

Panel II plots actual log sale prices against log origination amounts. The average relationship between origination balances and actual sale prices is not far off from a setting where all borrowers took out mortgages at 80% of the sale price of the home, in which case there would be a perfect fit between log origination amount and log sale price with an intercept of log(1.25) and a slope of 1. The most obvious outliers are those well above the regression line—borrowers who took out mortgages with much lower leverage (i.e., from accumulated equity in the case of refinances or through a larger downpayment in the case of sales). Using log origination amounts as an instrument to explain CLTVs will account for any correlation between actual sale prices, initial leverage, and unobserved borrower quality.

Panel III plots assessed property values against counterfactual property values, defined as

$$\widehat{Value} = 1.25 \times Origination\ Amount \times \exp(\widehat{Appreciation}) \tag{D.5}$$

to show the predictive power of the generated instrument $\widehat{Appreciation}$. The workhorse behind this relationship is the long-run price cyclicality instrument σ_g^P used to predict HPI values and subsequently impute appreciation-since-origination and corresponding counterfactual property values. There is a clear positive relationship between counterfactual property values and assessed values. Positive deviations from the regression line represent homes in areas and months with much higher prices than would be predicted based on the 1980s price cycle of that CBSA. Negative deviations represent homes where price declines have been more acute than expected a priori. Instrumenting for actual assessed values will address the potential for these local price changes to be correlated with unobserved borrower default risk.

The first stage for CLTV is a linear regression of CLTV on the scheduled loan balance, the loan origination amount, predicted appreciation using the counterfactual price series, and the usual controls Z_2

$$CLTV_{icgt} = Z'_{1,igt}\Upsilon_1 + Z'_{2,icgt}\Upsilon_2 + \nu_{icgt} \tag{D.6}$$

where the instrument set consists of

$$Z_{1,igt} = \Big(\; \log(Scheduled\, Principal_{it}), \quad \log(Origination\, Amount_i), \quad \widehat{Appreciation}_{igt} \; \Big).$$

Appendix Table A10 reports the results of estimating the first-stage equation (D.6) by OLS with clustered standard errors. Note that missing data—loans for which CoreLogic has not estimated a contemporaneous CLTV in a given month—reduces the sample size of specifications involving CLTV from 1.2 to 1.0 million monthly loan observations. Column 1 reports results of regressing CLTV on Z_1 without controlling for Z_2. The relationship between each of the instruments and CLTV values is large and precisely estimated. Mortgages with higher origination amounts (positive predictors of sale prices) have lower CLTVs. Mortgages with higher scheduled principal balances have higher CLTVs. Mortgages with higher predicted appreciation have lower CLTVs. Adding cohort indicator variables, baseline hazard controls, and CBSA fixed effects in column 2 strengthens the estimated effect of origination amounts and scheduled principal and attenuates the effect of predicted appreciation on the CoreLogic contemporaneous CLTVs. The cohort pattern confirms the trends in median CLTVs plotted in Figure 2, showing that later cohorts have much higher CLTVs. Successively controlling for borrower and loan characteristics in column 3 and price change instruments in column 4 continues the trend. The CLTV instruments are still powerful predictors of contemporaneous leverage. Column 5 additionally controls for the monthly CBSA unemployment rate. Local labor market conditions are correlated with CLTVs: the coefficient on the unemployment rate suggests that the equity share of property values in areas with high unemployment rate is lower. Controlling for the unemployment rate, the predicted appreciation instrument is no longer significant. Still, the partial F-statistic for the joint significance of the instruments is above 200 in every column.

Columns 4-6 of Appendix Table A3 report the results of estimating the default hazard function after incorporating $\hat{\nu}_{icgt}$ from equation (D.6) into the linear index $X\beta$ in equation (D.1).[56] Column 4 includes the underwater indicator variable as a parsimonious summary of the causal influence of negative equity on default conditional on the CLTV residuals, price changes, and price change residuals. The effect of being underwater on default in column 4 is still significant but greatly attenuated from column 1, suggesting that holding prices fixed, a mortgage being underwater causes the default hazard to be 33% higher (28 log points) than that of above-water mortgages. This suggests that some of the performance differences across cohorts that columns 1-3 attributed to negative equity were actually unobserved differences in borrower quality across cohorts that affected both defaults and equity. Furthermore, the CLTV residuals are significant in columns 4-6, rejecting the null hypothesis that CLTVs are exogenous. Columns 5 and 6 instead control for a linear spline in q. The estimated effect of prices is large and significant across all specifications, showing an elasticity of default with respect to price declines of -3 to -5, meaning that even holding a property's CLTV fixed, a 1% price decline increases the default hazard by 3-5%.

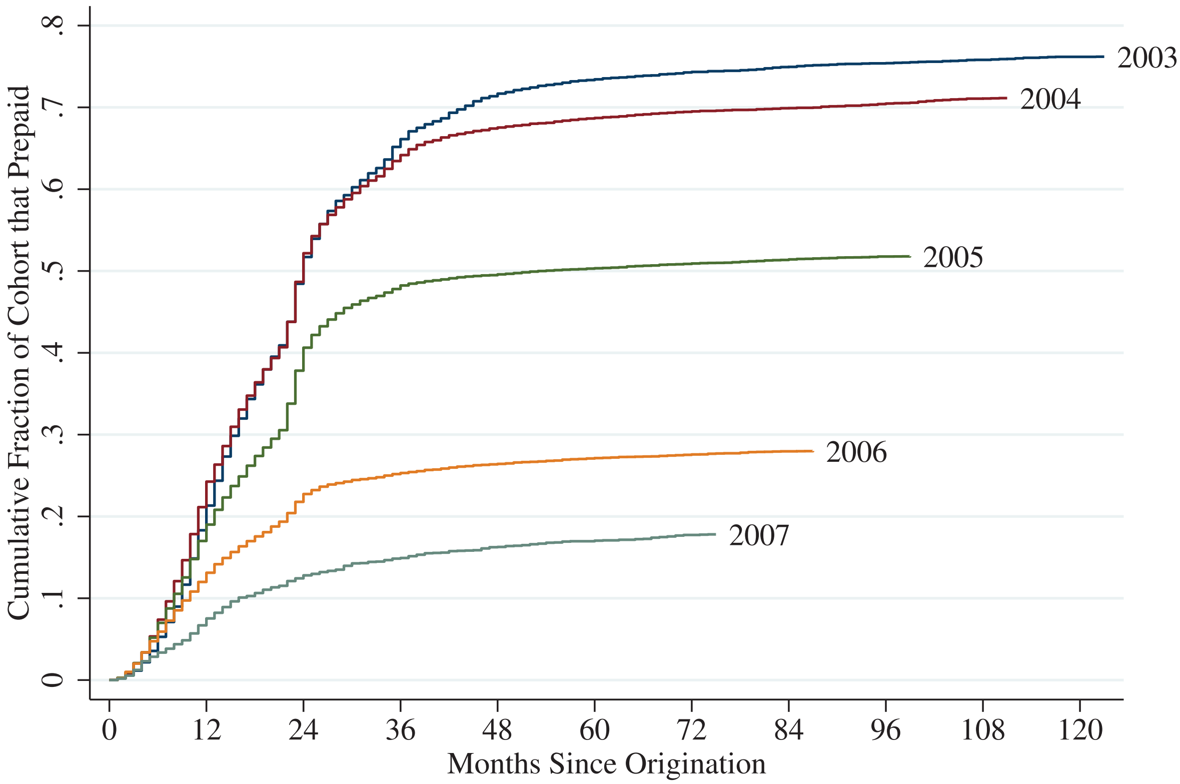
Comparing columns 3 and 5, the estimated cohort differences after controlling and instrumenting for price changes and mark-to-market leverage are smaller than the corresponding estimates column 3 that do not account for the endogeneity of prices or CLTVs. Consistent with the patterns above, the control-function specification in column 5 is more successful at explaining the default rates of later cohorts than earlier cohorts, suggesting that negative equity was a more important factor in late-cohort defaults than early cohort defaults. While highly predictive of individual defaults, the smaller effect of CLTV controls on earlier cohort default rates is consistent with earlier cohorts' CLTVs not having increased as much (see Figure 2).

[56] Imbens and Wooldridge (2007) discuss the control function approach when the estimating equation contains several nonlinear functions of the right-hand side endogenous variable. Under the assumption that the unobserved component of default risk is independent of the instruments (the control function exclusion restriction), controlling for the fitted residuals of CLTV is sufficient to instrument for any function of CLTV.

To verify that these findings are robust to controlling for local labor market fluctuations, I control directly for the unemployment rate in column 6 of Appendix Table A3. Conditional on all of the other controls, mortgages in cities with increased unemployment rates are slightly less likely to default. Accounting for local labor market fluctuations does not materially affect the estimated coefficients on prices or CLTV residuals. However, including the unemployment rate decreases the measure of the difference in latent quality between the 2003 and 2004-2005 cohorts enough to be statistically insignificant.

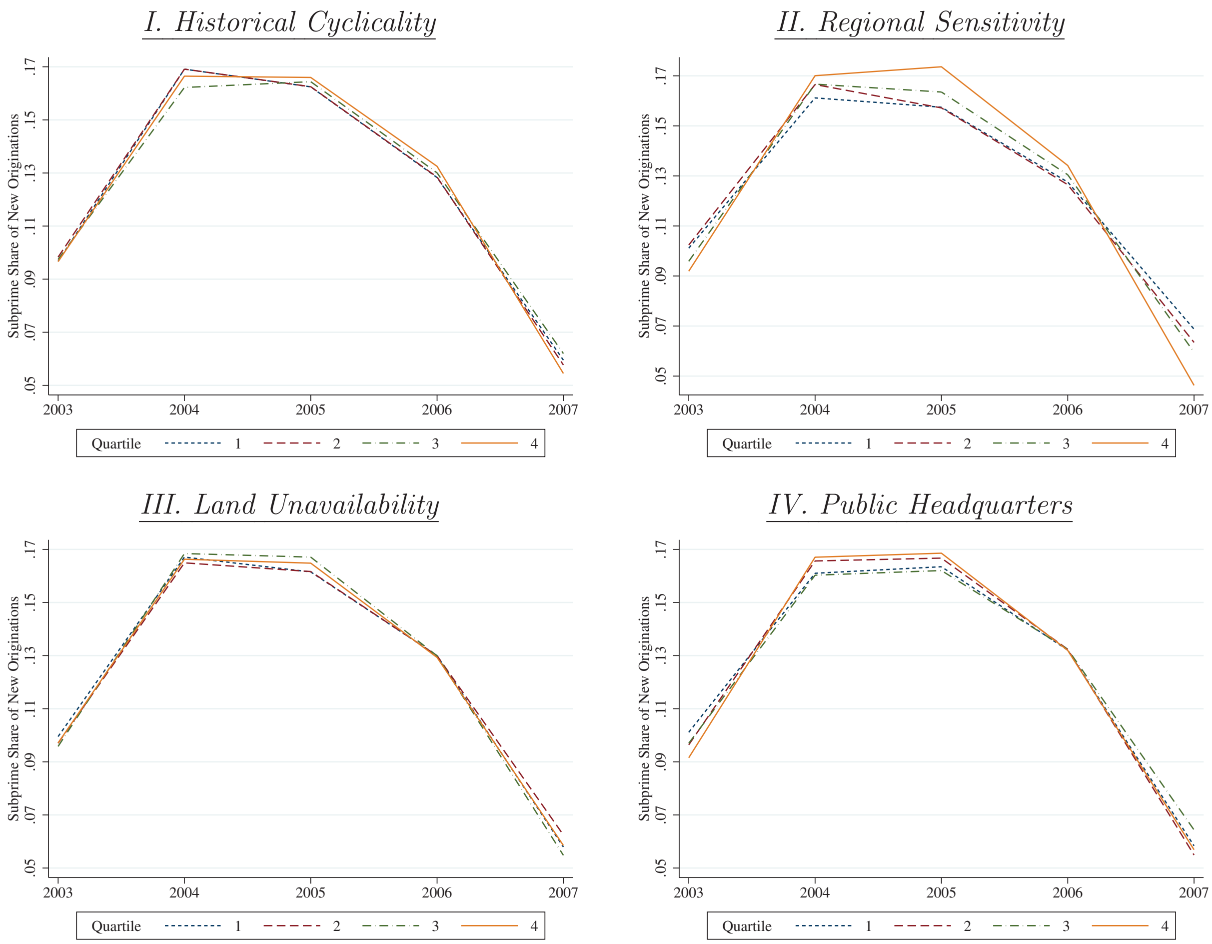
Taken together, the results of Appendix Table A3 point to multiple mechanisms through which price declines cause defaults. As expected, negative equity explains most of the relationship between cohort default rates and price changes, especially among later cohorts. Nevertheless, prices affect default risk in other ways besides their effect on default through negative equity and their correlation with local unemployment rates. This highlights the additional salience of recent price changes to price expectations and market liquidity even conditional on negative equity. For any given equity position, borrowers in areas with strongly declining prices may be less motivated to make mortgage payments after adverse shocks. Moreover, given that buyers' expectations of future prices are strongly influenced by recent price changes (Glaeser and Nathanson, 2017; Gennaioli and Shleifer, 2018; Guren, 2018; Armona et al., 2019; Bailey et al., 2019; Liu and Palmer, 2021), falling prices can have affect default independent of the frictions associated with negative equity. Lazear (2012) documents the comovement of volume and price move together in housing markets, with the consequence being that housing-market illiquidity will be particularly acute when prices are falling. Even above-water owners in areas experiencing recent price declines may be unable to sell in the face of a thin market of spooked buyers. Still, there is strong evidence that negative equity is responsible for much of the effect of prices on defaults and that the differential prevalence of negative equity across cohorts explains most of the observed increase in cohort-level default rates.

Figure A1: Cumulative Prepayment Rate by Origination Cohort



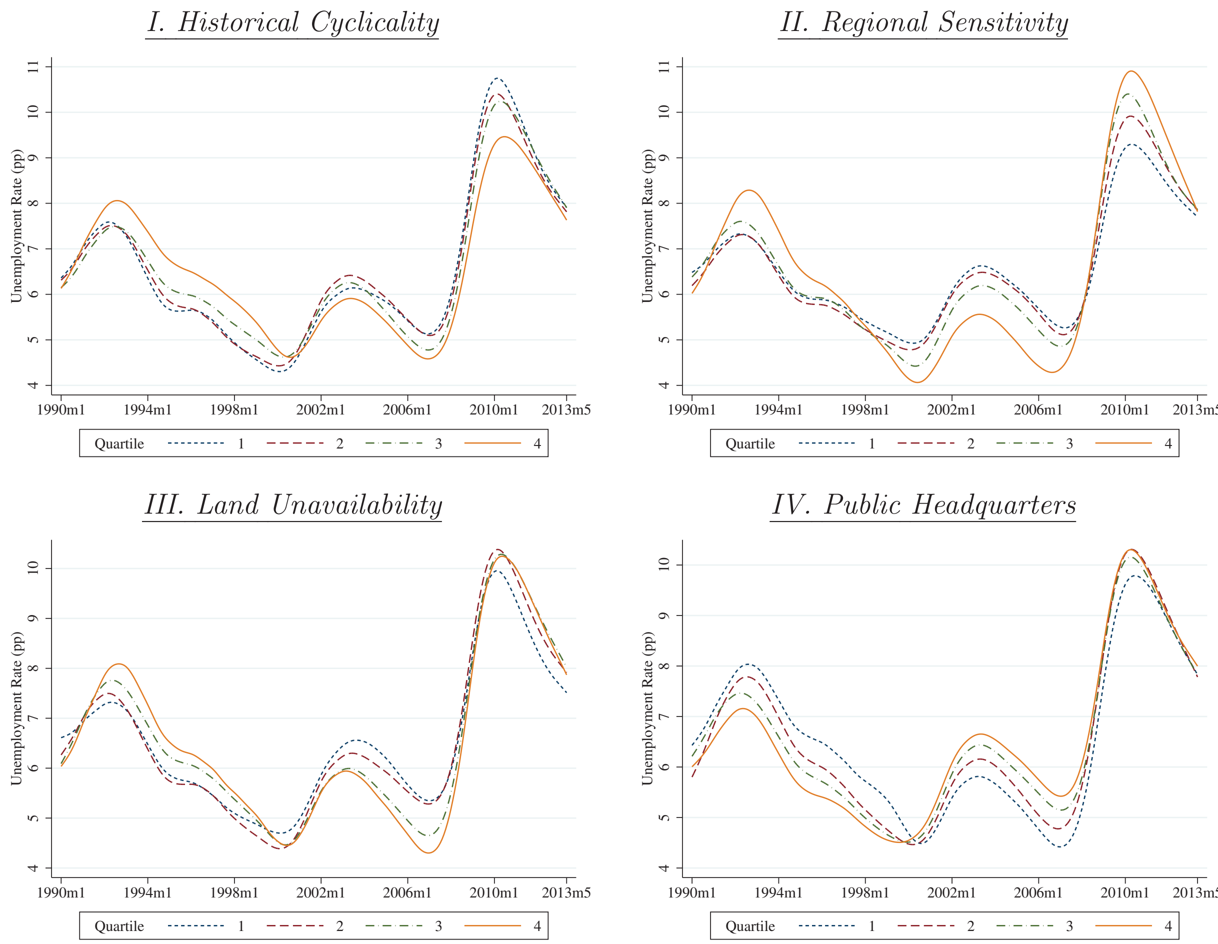
Notes: Figure plots the Kaplan-Meier estimate of the fraction of each cohort that has terminated by prepayment within a given number of months since origination. Prepayment means repayment in full such as through refinancing or selling.

Figure A2: Subprime Share by Instrument Quartile



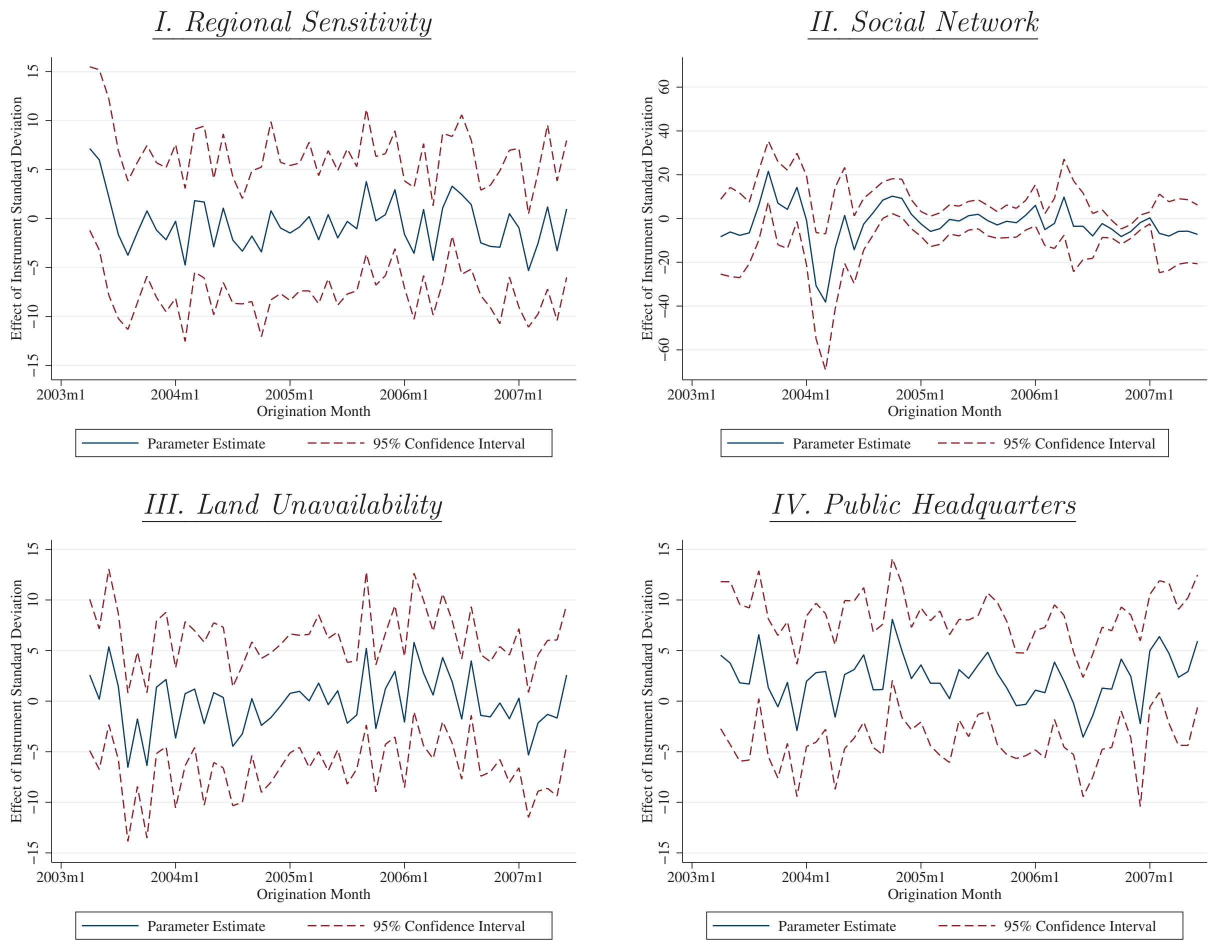
Notes: Figures plot average subprime market share by quartile of the indicated instrument. Subprime market shares are calculated using HMDA data as the fraction of mortgages originated in a given year that were made by a lender on the HUD subprime lender's list in any year after residualizing for CBSA fixed effects. Cyclicality is measured as the standard deviation of one month changes to the log home price index from 1980-1995, as defined in equation (16) in the text. Regional sensitivity from Guren et al. (2021) is the sensitivity of each CBSA's price index to a regional price index. Land unavailability from Saiz (2010) is the share of land unavailable for development. Public headquarters from Choi et al. (2016) is the ratio of the total book value of publicly traded companies headquartered in each MSA to that MSA's income. See section 6.2.4 for more details.

Figure A3: Unemployment Rate by Instrument Quartile



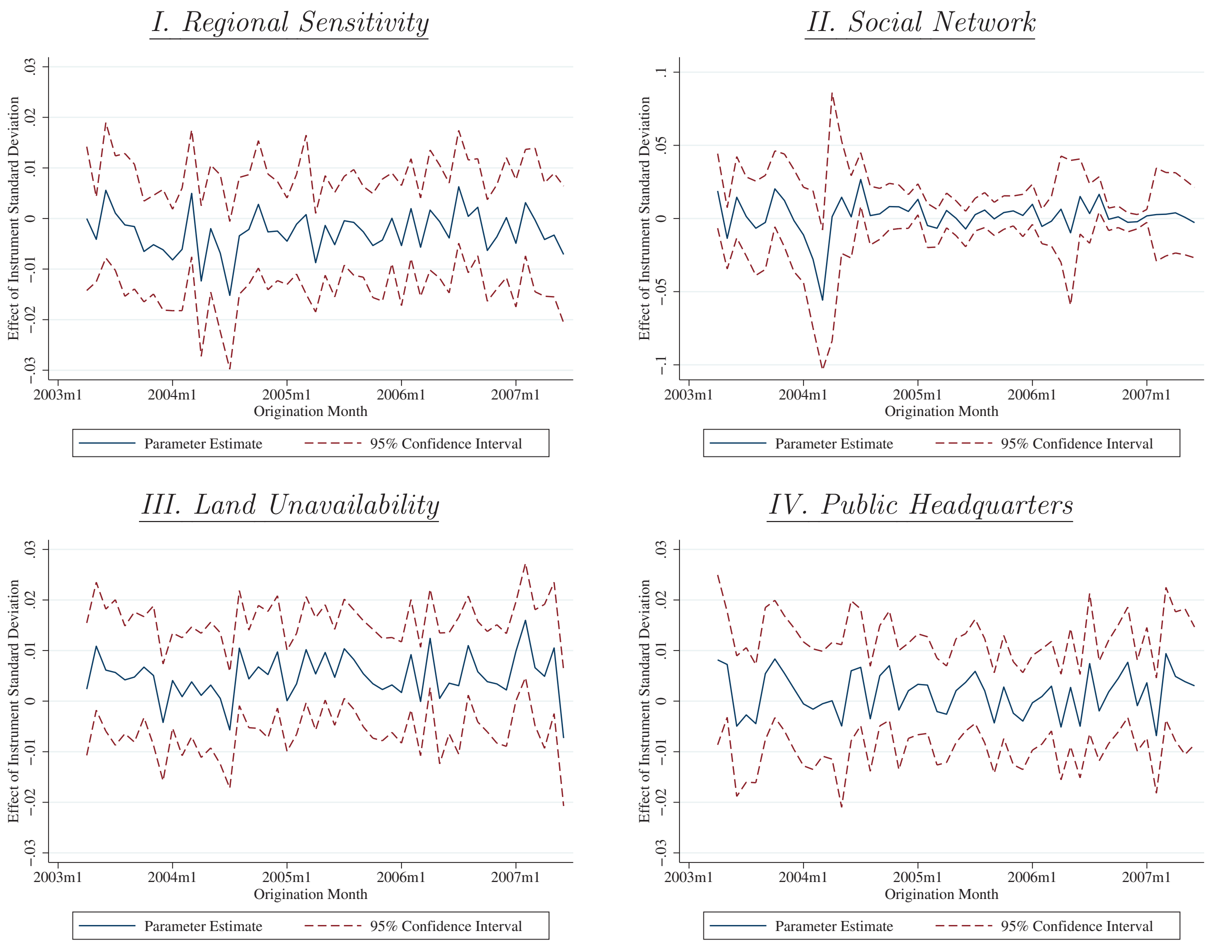
Notes: Figures plot average subprime market share by quartile of the indicated instrument. Subprime market shares are calculated using HMDA data as the fraction of mortgages originated in a given year that were made by a lender on the HUD subprime lender's list in any year after residualizing for CBSA fixed effects. Cyclicality is measured as the standard deviation of one month changes to the log home price index from 1980-1995, as defined in equation (16) in the text. Regional sensitivity from Guren et al. (2021) is the sensitivity of each CBSA's price index to a regional price index. Land unavailability from Saiz (2010) is the share of land unavailable for development. Public headquarters from Choi et al. (2016) is the ratio of the total book value of publicly traded companies headquartered in each MSA to that MSA's income. See section 6.2.4 for more details.

Figure A4: Testing Whether Alternative Instruments Predict FICO Scores



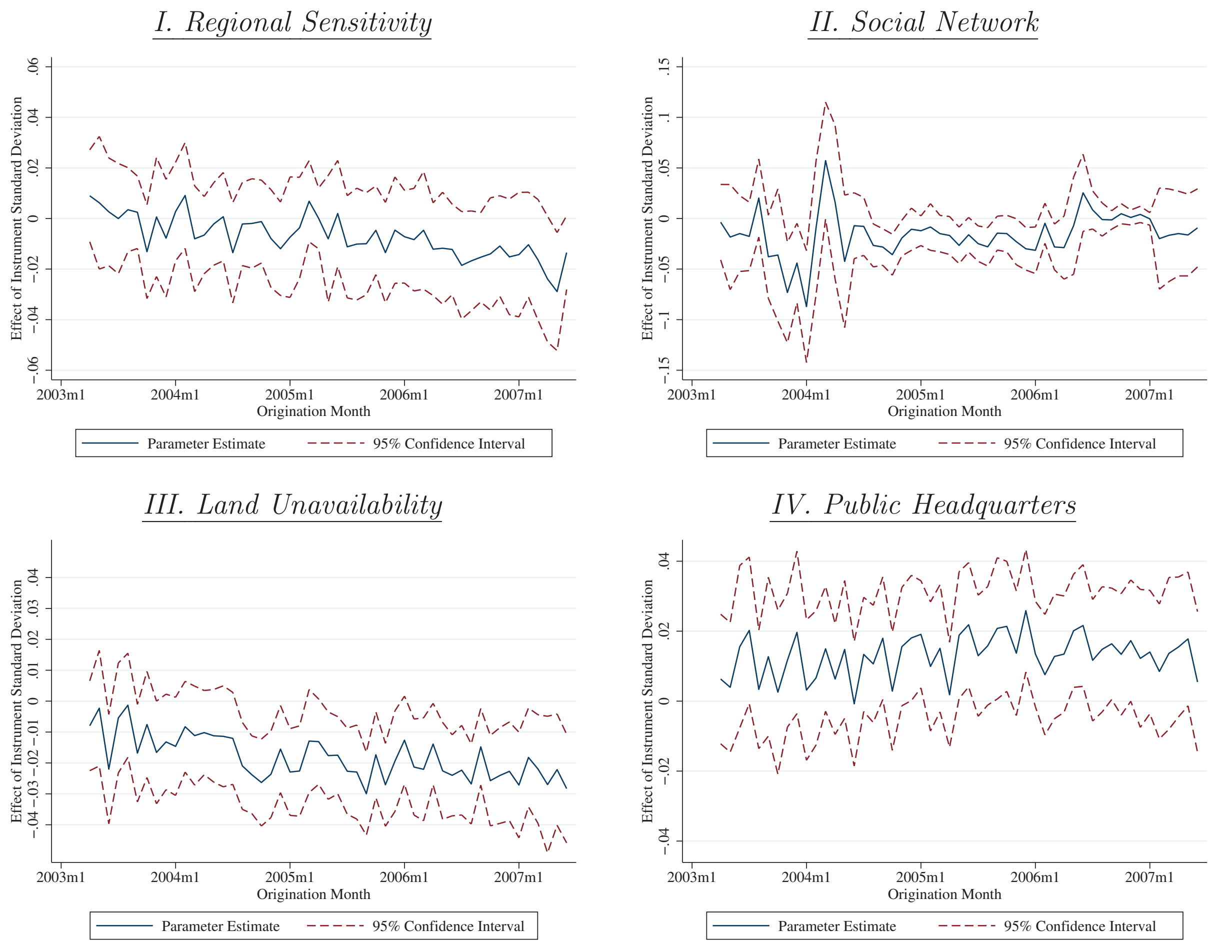
Notes: Figures plot regression coefficients of origination FICO scores regressed on origination-month indicators interacted with the indicated instrument in each panel as defined in 6.2.4. Each instrument has been standardized such that plotted coefficients represent the relationship between a one standard deviation increase in the indicated instrument in the given month and the average value of origination FICO scores. All regressions control for CBSA fixed effects, cohort main effects, baseline hazard dummies, and the remaining mortgage and borrower characteristics used as controls in Table 2. Dashed lines plot 95% confidence intervals clustered at the CBSA level.

Figure A5: Testing Whether Alternative Instruments Predict DTI Ratios



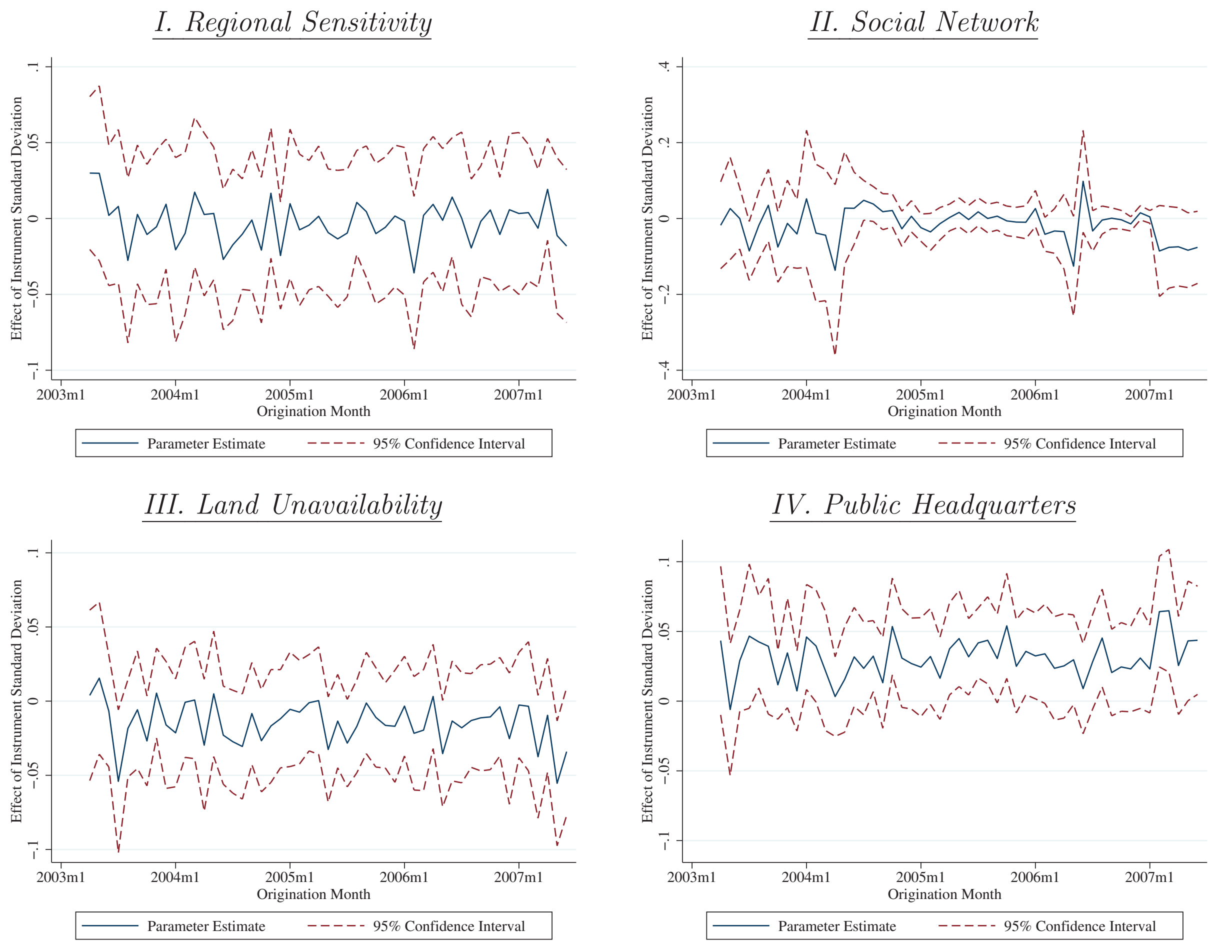
Notes: Figures plot regression coefficients of origination back-end debt-to-income ratios regressed on origination-month indicators interacted with the indicated instrument in each panel as defined in 6.2.4. Each instrument has been standardized such that plotted coefficients represent the relationship between a one standard deviation increase in the indicated instrument in the given month and the average value of origination DTI ratios. All regressions control for CBSA fixed effects, cohort main effects, baseline hazard dummies, and the remaining mortgage and borrower characteristics used as controls in Table 2. Dashed lines plot 95% confidence intervals clustered at the CBSA level.

Figure A6: Testing Whether Alternative Instruments Predict CLTV Ratios



Notes: Figures plot regression coefficients of origination combined loan-to-value ratios regressed on origination-month indicators interacted with the indicated instrument in each panel as defined in 6.2.4. Each instrument has been standardized such that plotted coefficients represent the relationship between a one standard deviation increase in the indicated instrument in the given month and the average value of origination CLTV ratios. All regressions control for CBSA fixed effects, cohort main effects, baseline hazard dummies, and the remaining mortgage and borrower characteristics used as controls in Table 2. Dashed lines plot 95% confidence intervals clustered at the CBSA level.

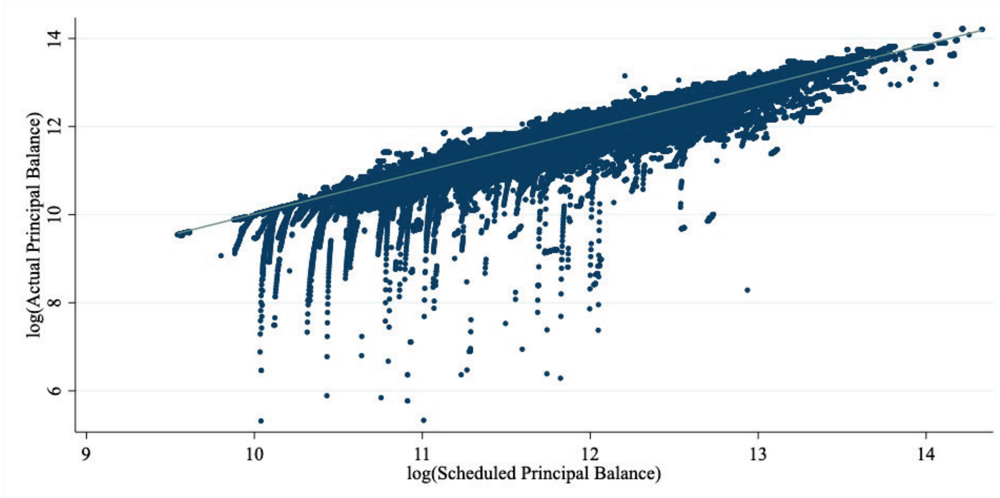
Figure A7: Testing Whether Alternative Instruments Predict Owner Occupied Status



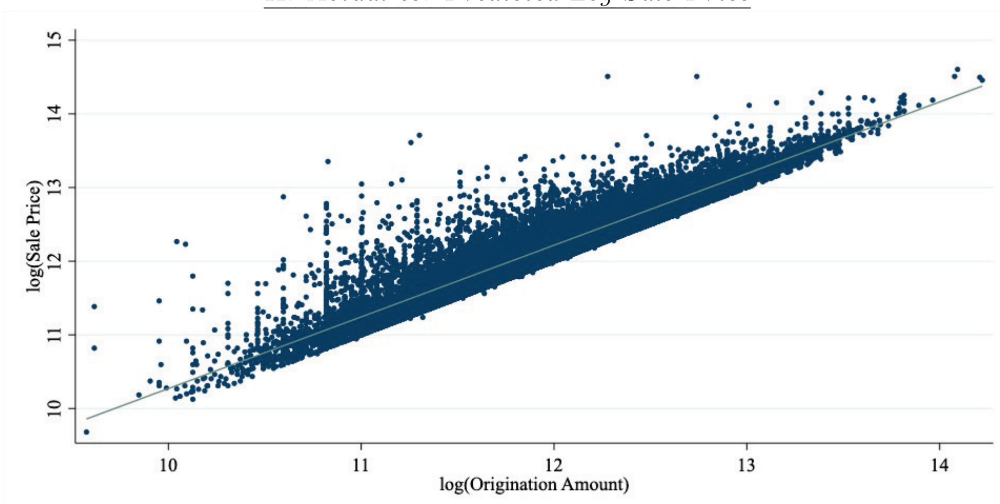
Notes: Figures plot regression coefficients of origination stated owner-occupied status regressed on origination-month indicators interacted with the indicated instrument in each panel as defined in 6.2.4. Each instrument has been standardized such that plotted coefficients represent the relationship between a one standard deviation increase in the indicated instrument in the given month and the average owner occupancy rate. All regressions control for CBSA fixed effects, cohort main effects, baseline hazard dummies, and the remaining mortgage and borrower characteristics used as controls in Table 2. Dashed lines plot 95% confidence intervals clustered at the CBSA level.

Figure A8: First-Stage Plots for Combined Loan-to-Value Ratio Components

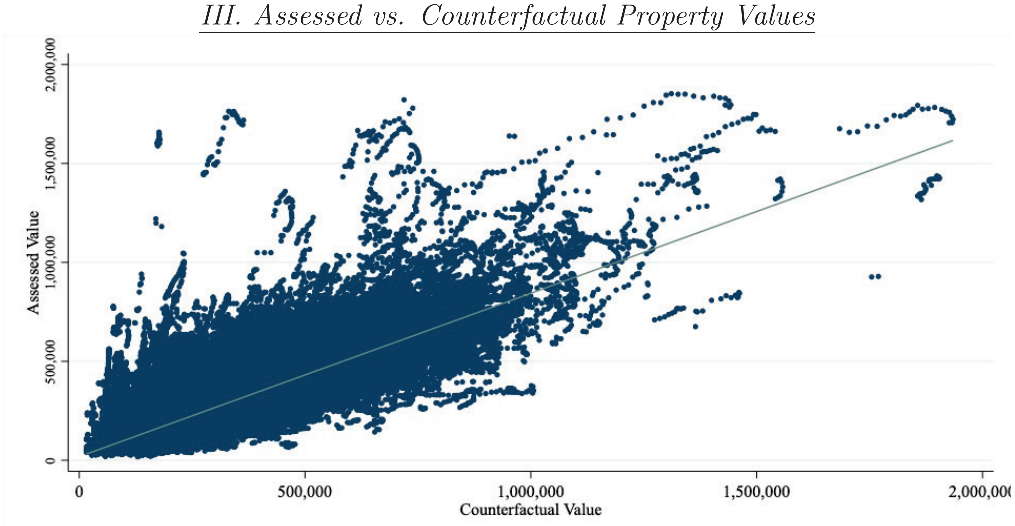
I. Actual vs. Scheduled Log Principal Balance



II. Actual vs. Predicted Log Sale Price



III. Assessed vs. Counterfactual Property Values



Notes: Panel I plots actual log principal balances versus log balances corresponding to the 30-year fixed-rate mortgage amortization schedule. Panel II plots log sale prices against log origination amounts. Panel III plots property values against counterfactual values, imputed using home price indices predicted using long-run local variation in home-price cyclicality. Solid lines show the fitted bivariate linear regression line.

Table A1: Default Hazard Model Estimates Controlling for Borrower Cells

	(1)	(2)	(3)	(4)
2004 Cohort	0.188***	0.197***	0.094	0.101
	(0.069)	(0.069)	(0.066)	(0.067)
2005 Cohort	0.519***	0.525***	0.190***	0.196***
	(0.087)	(0.086)	(0.068)	(0.066)
2006 Cohort	0.579***	0.574***	0.045	0.038
	(0.121)	(0.118)	(0.086)	(0.084)
2007 Cohort	0.466***	0.464***	-0.107	-0.108
	(0.116)	(0.117)	(0.084)	(0.085)
Δlog(HPI)			-3.857***	-3.856***
			(0.152)	(0.153)
CBSA Fixed Effects	✓	✓	✓	✓
Origination Characteristics	✓		✓	
Borrower Cell Fixed Effects		✓		✓
Observations	1,224,716	1,201,349	1,224,716	1,201,349
Log likelihood	-42,498	-41,998	-42,033	-41,543

Notes: Table reports maximum-likelihood estimates of the default hazard model given in equations (2) and (14) in the text. Origination characteristics controls in columns 1 and 3 include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, an owner-occupied indicator, the combined loan-to-value ratio, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. Columns 2 and 4 instead control for indicators of each borrower type, with 144 types defined as unique combinations of 20-point FICO bins, 10-point DTI bins, 10-point CLTV bins, and indicators for full-documentation status, adjustable-rate mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. The change in log(HPI) is the 12-month difference in the log of the CBSA-level CoreLogic HPI. All specifications include indicator variables for each value of loan age as a nonparametric baseline hazard function estimate. Standard errors in parentheses are clustered at the CBSA level.

Table A2: Delinquency Hazard Model Estimates

	(1)	(2)	(3)	(4)	(5)
2004 Cohort	0.23***	0.24***	0.19***	0.18***	0.18***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)
2005 Cohort	0.70***	0.55***	0.51***	0.33***	0.34***
	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)
2006 Cohort	1.11***	0.80***	0.79***	0.43***	0.45***
	(0.09)	(0.08)	(0.06)	(0.06)	(0.05)
2007 Cohort	1.17***	0.92***	0.80***	0.50***	0.52***
	(0.09)	(0.09)	(0.07)	(0.06)	(0.05)
Δlog(HPI)			-2.89***	-3.09***	-2.93***
			(0.09)	(0.11)	(0.19)
Δlog(HPI) Fitted Residuals					-0.23
					(0.23)
CBSA Fixed Effects		✓	✓	✓	✓
Origination Characteristics		✓		✓	✓
Observations	941,275	941,275	941,275	941,275	941,275
Log likelihood	-67,867	-65,516	-66,799	-65,009	-65,008

Notes: Table reports maximum-likelihood estimates of a delinquency hazard model replacing the default dependent variable in equation (2) with an indicator equal to one the first month a loan is marked as at least 90 days past due. The first-stage partial F-statistic in column (5) is 26. All specifications include indicator variables for each value of loan age as a nonparametric baseline hazard. Standard errors in parentheses are clustered at the CBSA level. See notes to Table 2.

Table A3: Effect of Current Combined Loan-to-Value Ratio on Control-Function Default Hazard

	(1)	(2)	(3)	(4)	(5)	(6)
2004 Cohort	0.202***	0.188***	0.127**	0.111*	0.107*	0.092
	(0.066)	(0.064)	(0.063)	(0.060)	(0.061)	(0.063)
2005 Cohort	0.424***	0.348***	0.138**	0.099*	0.035	0.095
	(0.075)	(0.068)	(0.063)	(0.059)	(0.059)	(0.064)
2006 Cohort	0.317***	0.159*	-0.185**	-0.213***	-0.338***	-0.146*
	(0.096)	(0.083)	(0.076)	(0.065)	(0.065)	(0.084)
2007 Cohort	0.143	-0.055	-0.417***	-0.433***	-0.588***	-0.279***
	(0.096)	(0.084)	(0.078)	(0.077)	(0.076)	(0.093)
Underwater	0.683***			0.296***		
	(0.060)			(0.035)		
12-month Δlog(HPI)			-3.221***	-5.080***	-5.509***	-4.814***
			(0.237)	(0.281)	(1.071)	(0.411)
CLTV Fitted Residuals				0.007***	0.025***	0.011***
				(0.000)	(0.002)	(0.001)
Δlog(HPI) Fitted Residuals				2.199***	2.514***	2.170***
				(0.344)	(0.352)	(0.443)
Unemployment Rate						-0.050***
						(0.012)
CBSA Fixed Effects	✓	✓	✓	✓	✓	✓
Origination Characteristics	✓	✓	✓	✓	✓	✓
CLTV Linear Spline		✓	✓		✓	✓
Observations	1,037,634	1,037,634	1,037,634	1,037,581	1,037,581	1,036,611
Log likelihood	-35,935	-35,723	-35,477	-35,479	-35,366	-35,329

Notes: Table reports maximum-likelihood estimates of the default hazard model given in equations (2) and (D.1) in the text. Current combined loan-to-value ratios (CLTVs) are calculated by LoanPerformance as the total outstanding principal on a loan divided by an automated assessing model's estimate of the market value of each home. Underwater is an indicator for CLTV>1. The linear spline is defined by equation (D.2) in the text. All specifications include individual-level controls for origination characteristics, CBSA fixed effects, and indicator variables for each value of loan age as a nonparametric baseline hazard function estimate. Sample size decreases in column 6 because not all sample CBSAs have BLS unemployment rates available. Standard errors in parentheses are clustered at the CBSA level.

Table A4: Nonparametric Control-Function Estimates of Default Hazard

	(1)	(2)	(3)	(4)	(5)	(6)
2004 Cohort	0.127**	0.121**	0.121**	0.083	0.077	0.078
	(0.057)	(0.057)	(0.057)	(0.058)	(0.058)	(0.058)
2005 Cohort	0.362***	0.336***	0.339***	0.142***	0.117**	0.120**
	(0.053)	(0.054)	(0.054)	(0.055)	(0.056)	(0.056)
2006 Cohort	0.392***	0.358***	0.359***	-0.034	-0.066	-0.064
	(0.057)	(0.058)	(0.058)	(0.060)	(0.061)	(0.061)
2007 Cohort	0.146**	0.121*	0.120*	-0.196***	-0.219***	-0.219***
	(0.066)	(0.067)	(0.068)	(0.070)	(0.070)	(0.071)
12-month $\Delta\log(\text{HPI})$	-4.381***	-4.753***	-4.672***	-4.597***	-4.957***	-4.889***
	(0.255)	(0.262)	(0.280)	(0.260)	(0.267)	(0.275)
$\Delta\log(\text{HPI})$ Fitted Residuals	0.977***	0.310	0.671	1.033***	0.453	0.746*
	(0.315)	(0.353)	(0.432)	(0.319)	(0.356)	(0.423)
$(\Delta\log(\text{HPI})$ Fitted Residuals$)^2$		-4.516***	-1.198		-4.855***	-1.799
		(1.552)	(3.542)		(1.601)	(3.474)
$(\Delta\log(\text{HPI})$ Fitted Residuals$)^3$		22.778***	-8.895		18.849**	-6.003
		(7.342)	(26.892)		(7.485)	(25.219)
$(\Delta\log(\text{HPI})$ Fitted Residuals$)^4$			-75.179			-70.659
			(67.389)			(65.476)
$(\Delta\log(\text{HPI})$ Fitted Residuals$)^5$			291.839			204.388
			(355.441)			(335.377)
CBSA FE	✓	✓	✓	✓	✓	✓
Origination Characteristics				✓	✓	✓
Observations	1,224,716	1,224,716	1,224,716	1,224,716	1,224,716	1,224,716
Log likelihood	-43,137	-43,114	-43,112	-42,028	-42,008	-42,006

Notes: Table reports maximum-likelihood control-function estimates of the default hazard model given in equations (2) and (19) in the text with the first stage given by (17). The change in log(HPI) is the 12-month difference in the log of the CBSA-level CoreLogic HPI. Fitted residuals are estimated from a linear first stage regression of the 12-month change in the log price index on the instruments and remaining controls. Origination characteristics include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, an owner-occupied indicator, the combined loan-to-value ratio, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. All specifications include indicator variables for each value of loan age as a nonparametric baseline hazard. Standard errors in parentheses are adjusted for two-stage estimation by GMM and clustered at the CBSA level.

Table A5: Default Hazard Model Estimates Accounting for Misrepresented Mortgage Characteristics

	(1)	(2)	(3)	(4)
2004 Cohort		0.223***	0.188***	0.167**
		(0.071)	(0.069)	(0.076)
2005 Cohort		0.709***	0.519***	0.541***
		(0.097)	(0.087)	(0.097)
2006 Cohort		0.984***	0.579***	0.619***
		(0.129)	(0.121)	(0.134)
2007 Cohort		0.800***	0.466***	0.494***
		(0.116)	(0.116)	(0.129)
Share Misreporting Owner Occupancy	0.054			
	(0.712)			
Share Overstating Value by 20%+	-0.948***			
	(0.349)			
Share Unreported Second Lien	2.1689**			
	(1.032)			
Owner Occupied			-0.509***	-0.486***
			(0.053)	(0.084)
Owner Occupied × Share Misreporting Occupancy				0.186
				(1.012)
CLTV Ratio			2.203***	3.673***
			(0.180)	(0.646)
CLTV × Share Overvalued by 20%+				-5.375***
				(1.314)
Has Second Lien			0.153***	0.195
			(0.037)	(0.136)
Has Second Lien × Share Misreporting Second Lien				-0.605
				(1.267)
CLTV × Share Misreporting Second Lien				-2.251
				(4.617)
CBSA Fixed Effects		✓	✓	✓
Other Origination Characteristics			✓	✓
Observations	1,025,174	1,224,716	1,224,716	1,025,174
p-value for fraud effects jointly equal zero	0.008			0.002

Notes: Table reports maximum-likelihood estimates of the default hazard model given in equations (2) and (14) in the text. Share misreporting owner occupancy, share overstating value by at least 20%, and share with an unreported second lien are measured at the CBSA level by Griffin and Maturana (2016). Reported p-values for fraud effects test the null hypothesis that all of the fraud effects in the given column are jointly equal to zero. Other origination characteristics include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, and balloon mortgages. All specifications include indicator variables for each value of loan age as a nonparametric baseline hazard. Standard errors in parentheses are clustered at the CBSA level.

Table A6: Default Hazard Model Estimates Allowing Cohort-Specific Fraud Effects

	(1)	(2)	(3)	(4)
2004 Cohort	0.188***	0.153*	0.198**	0.332***
	(0.069)	(0.085)	(0.081)	(0.084)
2005 Cohort	0.519***	0.544***	0.496***	0.849***
	(0.087)	(0.103)	(0.095)	(0.115)
2006 Cohort	0.579***	0.591***	0.531***	0.984***
	(0.121)	(0.140)	(0.115)	(0.166)
2007 Cohort	0.466***	0.462***	0.400***	0.839***
	(0.116)	(0.133)	(0.116)	(0.153)
			Fraud Measure	
		Owner Occ	2nd Lien	Overvalued
2004 Cohort × Fraud Measure		0.668	-0.782	-1.461***
		(1.504)	(2.072)	(0.548)
2005 Cohort × Fraud Measure		-0.371	2.326	-3.226***
		(1.505)	(2.610)	(0.723)
2006 Cohort × Fraud Measure		0.800	3.882	-4.188***
		(1.877)	(3.679)	(1.057)
2007 Cohort × Fraud Measure		0.994	3.811	-3.940***
		(1.840)	(3.416)	(0.940)
CBSA Fixed Effects	✓	✓	✓	✓
Origination Characteristics	✓	✓	✓	✓
Observations	1,224,716	1,028,846	1,112,903	1,109,112
p-value for fraud effects jointly equal zero		0.40	0.26	0.001

Notes: Table reports maximum-likelihood estimates of the default hazard model given in equations (2) and (14) in the text. Fraud measures in interaction terms in columns 2-4 indicated by headers are share misreporting owner occupancy, share overstating value by at least 20%, and share with an unreported second lien, respectively, and each measured at the CBSA level by Griffin and Maturana (2016). Reported p-values for fraud effects test the null hypothesis that all of the cohort × fraud measure effects in the given column are jointly equal to zero. Origination characteristics include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, an owner-occupied indicator, the combined loan-to-value ratio, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. All specifications include indicator variables for each value of loan age as a nonparametric baseline hazard. Standard errors in parentheses are clustered at the CBSA level.

Table A7: Control-Function Estimates of Prepayment Hazard

	(1)	(2)	(3)	(4)
2004 Cohort	0.002	-0.001	0.010	0.007
	(0.029)	(0.021)	(0.030)	(0.021)
2005 Cohort	-0.190***	-0.203***	-0.195***	-0.209***
	(0.047)	(0.026)	(0.046)	(0.027)
2006 Cohort	-0.669***	-0.693***	-0.701***	-0.727***
	(0.049)	(0.039)	(0.050)	(0.040)
2007 Cohort	-1.186***	-1.213***	-1.190***	-1.219***
	(0.066)	(0.057)	(0.068)	(0.059)
Δlog(HPI)	2.970***	2.778***	2.873***	2.662***
	(0.204)	(0.188)	(0.190)	(0.190)
Δlog(HPI) Fitted Residuals		0.247		0.271
		(0.214)		(0.216)
CBSA Fixed Effects	✓	✓	✓	✓
Origination Characteristics			✓	✓
Observations	1,210,445	1,210,445	1,210,445	1,210,445
Log likelihood	-76,764	-76,763	-76,022	-76,021

Notes: Table reports maximum-likelihood control-function estimates of the prepayment hazard model following the default hazard model estimated in Table 4, analogous to equations (2) and (19) with the first stage given by (17). The change in log(HPI) is the 12-month difference in the log of the CBSA-level CoreLogic HPI. Fitted residuals are estimated from a linear first-stage regression of the 12-month change in the log price index on the instruments and remaining controls. Origination characteristics include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, an owner-occupied indicator, the combined loan-to-value ratio, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. All specifications include indicator variables for each value of loan age as a nonparametric baseline hazard function estimate. Standard errors in parentheses are adjusted for the first-stage estimation of the fitted residuals by GMM and clustered at the CBSA level.

Table A8: Counterfactual 2-Year Default Rates by Cohort

	Counterfactual Scenario		Predicted 2-Year Cumulative Default Rate					
	Prices	Loan Composition	2003	2004	2005	2006	2007	Overall
(1)	Actual	Actual	6.0%	6.7%	11.4%	20.2%	21.3%	12.7%
(2)	Actual	2003	5.3%	5.7%	9.0%	13.8%	16.0%	9.6%
(3)	Actual	2006	7.3%	7.8%	12.3%	18.6%	21.3%	13.0%
(4)	2003	Actual	5.7%	6.4%	7.9%	8.7%	7.2%	7.4%
(5)	2006	Actual	12.7%	14.1%	16.9%	18.3%	15.3%	15.9%
(6)	2003	2003	5.1%	5.5%	6.1%	5.5%	4.8%	5.6%
(7)	2006	2006	15.7%	16.9%	18.3%	16.4%	14.5%	16.8%
(8)	Attenuated	Actual	6.5%	7.1%	11.6%	19.0%	19.9%	12.5%
(9)	Attenuated	2003	5.7%	6.1%	9.1%	12.9%	14.8%	9.4%
(10)	Attenuated	2006	7.9%	8.4%	12.4%	17.4%	19.8%	12.8%
(11)	2003 Attenuated	2003	5.5%	6.0%	6.5%	5.9%	5.2%	6.0%
(12)	2006 Attenuated	2006	14.7%	15.8%	17.1%	15.4%	13.6%	15.7%
		Number of Loans	4,407	7,251	9,444	8,336	2,734	32,172

Notes: Table reports estimated cumulative default rates under the indicated counterfactual scenarios of prices and loan characteristics. Cumulative default rates are defined as the predicted share of loans that would have defaulted within 24 months of origination. Scenarios using actual characteristics retain observed covariates. Scenarios using a given year's prices replace all price changes with the average price changes faced by the given cohort's borrowers at each value of loan age. Scenarios using a given year's loan characteristics assign all loans the average characteristics from the indicated cohort. Attenuated price scenarios in rows 8-10 replace actual 2003-2010 price changes with smaller magnitude price changes that Griffin et al. (2021) estimate would have prevailed if the subprime market share were zero. Attenuated scenarios in rows 11 and 12 replace all price changes with the average price changes faced by the 2003 or 2006 cohorts, respectively, attenuated using Griffin et al. (2021) estimates. See section 7 for more details.

Table A9: Default Hazard Model Estimates Allowing for Unobserved Heterogeneity

	(1)	(2)	(3)	(4)
2004 Cohort	0.254***	0.227***	0.137**	0.108
	(0.089)	(0.085)	(0.069)	(0.072)
2005 Cohort	0.924***	0.659***	0.407***	0.246***
	(0.123)	(0.112)	(0.086)	(0.074)
2006 Cohort	1.361***	0.782***	0.470***	0.120
	(0.175)	(0.155)	(0.132)	(0.096)
2007 Cohort	1.079***	0.670***	0.235**	-0.032
	(0.162)	(0.153)	(0.114)	(0.095)
Δlog(HPI)			-3.685***	-4.025***
			(0.140)	(0.169)
CBSA FE		✓	✓	✓
Origination Characteristics		✓		✓
Observations	1,224,716	1,224,716	1,224,716	1,224,716
Log likelihood	-44,295	-42,424	-43,142	-41,997

Notes: Table reports maximum-likelihood estimates of the default hazard model given in equations (13), (B.1), and (B.2) in the text. The change in log(HPI) is the 12-month difference in the log of the CBSA-level CoreLogic HPI. Origination characteristics include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, an owner-occupied indicator, the combined loan-to-value ratio, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. All specifications include indicator variables for each value of loan age as a nonparametric baseline hazard. Standard errors in parentheses are clustered by CBSA.

Table A10: First-Stage Results for Combined Loan-to-Value Ratios

	(1)	(2)	(3)	(4)	(5)
log(Origination Amount)	-0.644***	-0.725***	-0.952***	-0.720***	-0.972***
	(0.047)	(0.042)	(0.046)	(0.042)	(0.046)
log(Principal Balance)	0.791***	0.918***	1.065***	0.914***	1.079***
	(0.044)	(0.042)	(0.047)	(0.042)	(0.046)
Predicted Appreciation	-1.253***	-0.623***	-0.606***	-0.706***	-0.468***
	(0.160)	(0.136)	(0.137)	(0.122)	(0.111)
2004 Cohort		-0.001	0.003	0.029**	0.013
		(0.008)	(0.007)	(0.012)	(0.010)
2005 Cohort		0.030**	0.038***	0.080***	0.047**
		(0.013)	(0.013)	(0.022)	(0.019)
2006 Cohort		0.091***	0.110***	0.153***	0.100***
		(0.021)	(0.022)	(0.035)	(0.030)
2007 Cohort		0.156***	0.173***	0.227***	0.136***
		(0.028)	(0.028)	(0.044)	(0.036)
Unemployment Rate					0.071***
					(0.008)
Baseline hazard		✓	✓	✓	✓
CBSA Fixed Effects		✓	✓	✓	✓
Origination characteristics			✓	✓	✓
Price instruments				✓	✓
Observations	1,045,146	1,045,146	1,045,146	1,045,146	1,036,611
R-squared	0.24	0.36	0.42	0.36	0.46
Partial F-stat	243.6	334.2	234.8	48.0	221.2

Notes: Table estimates first stage specifications detailed by equation (D.6) by OLS. Dependent variable is current combined loan-to-value ratio, calculated by CoreLogic as the total outstanding principal on a loan divided by an automated assessing model's estimate of the market value of each home. Origination characteristics include FICO score, debt-to-income (DTI) ratio, a full documentation of income indicator, an owner-occupied indicator, the combined loan-to-value ratio, the mortgage interest rate, and indicator variables for adjustable-rate mortgages, cash-out refinance mortgages, mortgages with an interest-only period, balloon mortgages, and mortgages accompanied by additional second liens. Sample size decreases in column 5 because not all sample CBSAs have BLS unemployment rates available. Standard errors are clustered by CBSA.