Loan Buyouts and Foreclosure: Evidence from Ginnie Mae

Abstract

This paper studies the effect of loan buyouts on foreclosures. I exploit a 2002 suspension of Ginnie Mae's buyout option to isolate buyout effects from loan-quality differences. The unanticipated memorandum barred issuers from buying out loans that were 30 days delinquent while preserving the buyout option for loans that reached 90-day delinquency. I find that high-exposure loans experienced about a 30 percentage point decline in buyouts and roughly a 4 percentage point increase in foreclosure starts. Treated loans also exhibit a 4 percentage point decline in short-run cures, with no differential effect at longer horizons once loans become eligible for a 90-day buyout. Taken together, the results suggest that delaying buyouts impedes timely loan cures and thereby raises foreclosure risk. Finally, instrumental-variables estimates imply buyouts causally reduce foreclosure by 40% and raise loan cures by about 45%.

1 Introduction

The financial crisis of 2008 revealed how misaligned incentives shaped intermediary behavior, both in extending mortgage credit and in foreclosing on the collateral securing that debt (Keys et al., 2010; Kruger, 2018; Aiello, 2022). While prevailing accounts have emphasized the paucity of loan modifications for securitized mortgages, far less attention has focused on trustees' option to repurchase mortgages from investors—commonly termed a loan buyout. Figure 1 shows that buyouts were active throughout the crisis, yet their contribution to the surge in foreclosures remains poorly understood.

Empirically studying the effect of loan buyout on foreclosure and renegotiation is difficult because of asymmetric information. A large literature shows that the sale of newly originated loans is subject to a lemons problem (Adelino et al., 2019; Begley and Purnanandam, 2016). To signal quality, sellers provide costly guarantees—such as an equity stake or longer hold times—and buyers demand similar assurances. Information frictions intensify when a borrower defaults. For the econometrician, a primary challenge is therefore disentangling the effect of buying out a delinquent loan from the effect of underlying loan quality. If trustees repurchase only low-quality loans, the estimated relationship between buyout and loan performance will be biased downward. Alternatively, if trustees instead target loans with stronger prospects for reputational reasons, the bias will be upward.

To overcome this challenge, I use a triple-difference identification strategy that exploits the timing of a Ginnie Mae buyout suspension and cross-sectional variation in exposure to the policy change. When a Ginnie Mae issuer securitizes a pool of loans, the issuer retains the right to repurchase any mortgage from Ginnie Mae investors in one of two cases: (i) the borrower fails to remain current for four consecutive months (a rolling delinquency); or (ii) the borrower misses three consecutive payments (a serious delinquency). In November 2002, however, Ginnie Mae abruptly issued a memorandum that eliminated the buyout option for rolling delinquencies for loans securitized on or after January 1, 2003.

The policy change introduced a narrow runway for late-originated loans to securitize before the January 1, 2003 deadline. Loans issued immediately prior to the memorandum therefore had insufficient time to securitize and thus lost the buyout option for rolling delinquencies. I exploit this variation by comparing outcomes for Ginnie Mae (treated) and GSE (control) loans across the treated year (2002) and the prior year (2001), and across high-exposure months (October–November) versus low-exposure months (July–September).

Identification requires that the month of origination affect loan performance only through buyout eligibility. Using month of origination satisfies this requirement in several respects. First, the memorandum was announced in a way designed to prevent market anticipation.

Second, the policy change was not motivated by fraud or material differences in loan quality. Therefore, mechanically, the suspension was independent of contemporaneous expectations about buyout eligibility and loan outcomes. Third, I include baseline indicators and all pairwise interactions for origination month, year, and investor segment, which nets out seasonal and segment-specific patterns tied to origination month. Thus, the remaining variation in outcomes across month, year, and Ginnie Mae status is plausibly driven by the policy shock.

I find that high-exposure loans experienced a 30 percentage point decline in buyout propensity. I identify buyouts in the data using a novel method that measures a change in investor status within two months of a qualifying delinquency. To the best of my knowledge, this is the first use of a proprietary dataset to identify a buyout event in agency mortgage-backed securities. Turning to long-run outcomes, in two years, these treated loans experienced a 4 percentage point increase in foreclosure starts. These effects are robust to controls for seasonality in the Ginnie Mae market segment and to specifications that absorb variation specific to the treatment year. In event-study estimates, the effects increase monotonically as origination dates approach the memorandum, consistent with progressively narrower windows to securitize before January 1, 2003.

Exploring the mechanism, I find that the suspension of the buyout option reduces short-run reperformance by four percentage points. Short-run delinquency exits, also known as cures, are defined as a loan becoming current within three months of a rolling delinquency. The decline in short-run cures grows monotonically across origination months and mirrors the rise in foreclosures. Moreover, loans do not experience differential rates of modification or prepayment that could explain the decline in cures.

Given the absence of observable changes in loan terms, I argue that the decline in cures likely reflects a reduction in issuers' use of capitalizing arrears into subordinate liens. FHA-insured loans permit servicers to file a "partial claim," under which the borrower's arrears are capitalized into a junior lien held by HUD. The partial claim immediately reinstates the mortgage as current, reimburses the servicer for advanced funds, and leaves the subordinate lien in place until the first lien is prepaid or the property is sold. Absent the option to buy out loans, issuers may lack the financial incentive or capacity to pursue partial claims, producing a decline in delinquency exits.

Exploring medium-run outcomes, I estimate the effect of the buyout suspension on cures using a 15-month window following a rolling delinquency. If loans that later reach 90-day delinquency remain eligible for buyout, the short-horizon differential should attenuate over this longer horizon as some loans regain eligibility and cure. Consistent with this prediction, the differential effect on cures falls to zero over the 15-month window. This result implies that long-run loan performance depends critically on the timing of reinstatement to current

status.

I conclude by employing an instrumental-variables (IV) strategy that exploits the quasi-experimental variation in buyout propensity created by the Ginnie Mae memorandum. This approach provides (i) policy-relevant causal estimates of the effect of buyout on loan performance and (ii) a framework to assess external validity. I estimate two-stage least squares (2SLS) regressions using vintage-month indicators as excluded instruments. Under the assumption that vintage month affects loan outcomes only through quasi-random variation in buyout propensity, I estimate that buyout reduces the probability of foreclosure by at least 14.2 percentage points and increases early cures by at least 14.0 percentage points. Interpreting these estimates as average treatment effects on the treated (given one-sided noncompliance), they imply that loan buyouts reduce foreclosure by 40 percent and increase reperformance by 45 percent for treated loans. Finally, in heterogeneous-effects tests, financially constrained borrowers and borrowers with relatively unencumbered collateral benefit most from buyout. Overall, creditors appear to respond to financial incentives: high-quality loans that offer the largest coupon spread reperform most effectively following an initial delinquency.

Literature Review

This paper bridges two broad research areas in corporate finance. First, I contribute to the literature on creditor behavior under debtor delinquency. Work by Piskorski et al. (2010), Agarwal et al. (2011), and Kruger (2018) find that securitization leads to fewer modifications and more foreclosures, whereas Adelino et al. (2013) argue that, prior the financial crisis, there was little difference in renegotiation. The paper proposes self-cure risk and moral hazard as alternative explanations for the lack of renegotiation. Aiello (2022) demonstrates that financial constraints on the part of servicers lead to foreclosures and modifications that reduce value to investors and replace alternative borrower actions. Finally, Ahsin (2021), Gabriel et al. (2020), and Collins and Urban (2018) study the effect of foreclosure costs on foreclosures, renegotiations, and borrower repayment. My paper complements preceding work by demonstrating the efficacy of buyout as an alternative to foreclosure and renegotiation.

This paper also contributes to the literature on loan sales. Previous work has studied the role of asymmetric information in selling debt. An et al. (2011) find that Conduit CMBS sales feature a lower lemons discount that portfolio sales even after controlling for various economic conditions and loan characteristics. The authors argue that this is likely due to the informational advantage enjoyed by portfolio lenders, which subjects sales to a problem of adverse selection. Jiang et al. (2013) further find that balance sheet loans perform worse

than loans that are ultimately sold, likely due to the time buyers have to observe ex-post loan performance.

Adelino et al. (2019) operationalize this insight and find that loans sold earlier perform worse than loans sold later in the private label securities market. The authors show that signaling is most important for those loans with limited hard information to identify quality. Begley and Purnanandam (2016) further demonstrate that RMBS deals with higher levels of equity perform better ex-post, precisely for those loans that are most opaque. In complimentary work, Ashcraft et al. (2019) find that retention by B-piece buyers of CMBS is associated with high default probabilities of senior tranches. The authors demonstrate that this result is due to moral hazard as opposed to adverse selection on the part of selling underperforming securities. In addition to adverse selection and moral hazard, Drucker and Puri (2008) and Hartman-Glaser (2017) study reputation effects in the market for loan sales. Finally, beyond loan quality, work by Irani and Meisenzahl (2017) find that banks with liquidity shortfalls increase loan sales. While previous work has focused on sale at the time origination, my work instead focuses on loan sale around delinquency.

Outside of loan sales and creditor behavior, this paper relates to the literature on Ginnie Mae bonds and FHA loan issuance. The Ginnie Mae market has evolved significantly over the past two decades. Adelino et al. (2020) note that borrowers served by the Ginnie Mae market switched into the private securitization market prior to the financial crisis. Since the crisis, Gete and Reher (2020) find that the favorable regulatory status held by Ginnie Mae bonds led to an increase in non-bank market share in the post-crisis period. Similarly, Bhutta and Ringo (2021) find that the credit supply expanded in 2015 following reductions in insurance premiums for FHA loans. In recent years, Kim et al. (2018) document significant liquidity concerns related to issuers and servicers within the broader Ginnie Mae market. They document growth in issuance, concerns over financial stability, and significant costs borne by Ginnie Mae issuers. As of March 2022, Ginnie Mae made up about 25% of the $8.5 trillion in agency MBS outstanding. This is over two times the share of Ginnie Mae issuance outstanding as of June 2007.

This paper contributes to previous work by studying the buyout option, a particular feature of the Ginnie Mae mortgage market. Importantly, this option exists for all agency debt when a loan experiences serious delinquency. While $8 trillion worth of outstanding agency debt retain this buyout option, there remains a dearth of research on its consequences. Relevant to the current paper, Bandyopadhyay et al. (2021) find that creditors buy back loans in Ginnie Mae securitization pools when the loan has a relatively higher interest rate spread over the ten-year treasury rate. The paper provides suggestive evidence that issuers remove loans in order to re-securitize them under declining interest rate environments. The

authors posit that buyout creates a friction between servicers and borrowers due to a loan's change of ownership from origination to securitization to buyout and then to re-securitization again. The current paper aims to extend this research by asking how buyouts affects loan performance directly.

The remainder of this paper proceeds as follows. Section 2 describes the institutional background of the Ginnie Mae loan market and the delinquency buyout option. Section 3 details the data, sample construction, and summary statistics. Section 4 estimates the baseline relationship between buyout and loan performance. Section 5 estimates the effect of the Ginnie Mae policy change on buyout and foreclosure. Section 6 explores the mechanism underlying the increase in foreclosures. Section 7 implements an IV approach that estimates the causal relationship between buyout and loan performance. Section 8 concludes.

2 Institutional Background

2.1 The Government National Mortgage Association

Established in 1968 within the Department of Housing and Urban Development (HUD), the Government National Mortgage Association (GNMA, commonly known as Ginnie Mae) was created to facilitate the securitization of mortgages extended to high loan-to-value borrowers. Unlike the government-sponsored enterprises (GSEs), which purchase loans outright from originators, Ginnie Mae operates as a guarantor rather than a direct purchaser. Approved issuers securitize mortgages insured or guaranteed by one of four federal agencies—the Federal Housing Administration (FHA), the Department of Veterans Affairs (VA), the Rural Housing Service (RHS), or the Office of Public and Indian Housing (PIH)—and retain responsibility for remitting principal and interest payments to investors. Ginnie Mae, in turn, guarantees the timely payment of principal and interest in the event of issuer default, while borrower defaults are covered by the respective insuring agency. This institutional structure makes the Ginnie Mae market unique in that the agency provides the insurance and securitization platform, while issuers remain custodians of loan performance and cash flow to investors. By contrast, the GSEs purchase loans and issue MBS, retaining custodianship of the loans in the pools they securitize.

2.2 The Delinquency Buyout Option

Issuers of both Ginnie Mae and GSE mortgage-backed securities (MBS) retain the option to repurchase loans from a securitized pool once a mortgage becomes 90 days delinquent—a threshold commonly referred to as serious delinquency. In exercising this option, the issuer

buys the loan out of the pool at par, remitting the outstanding principal balance to investors. Repurchased loans that subsequently reperform (cure) may be re-securitized, allowing issuers to return them to the secondary market.

In addition to the 90-day delinquency trigger, Ginnie Mae permitted issuers to buy out loans under a "rolling delinquency" rule prior to 2003. A rolling delinquency was defined as a borrower failing to remain current for four consecutive payment periods. For instance, a borrower might miss the first payment but make timely payments in the following three months. If the missed installment was not cured by the fourth period, the loan qualified as a rolling delinquency. In such cases issuers had the right to repurchase the loan from the pool at par—before it reached the standard 90-day delinquency threshold.

Ginnie Mae designed the buyout option to address severe deterioration in MBS pool quality. The option benefits investors by ensuring timely repayment, helps issuers preserve pool performance, and reduces Ginnie Mae's exposure to distressed loans. Although improving pool quality tends to raise MBS prices, buyout options may also lower prices by increasing prepayment risk. Buyouts operate like other forms of prepayment: they accelerate payoffs and thus amplify the prepayment discount. While this mechanism is clearly justified for mortgages that are 90 days delinquent, the case is less compelling for rolling delinquencies. By definition, borrowers classified as rolling delinquent remain current except for one or two intermittent missed payments over a three-month period, implying substantially lower risk of continued deterioration. Consequently, the market-wide benefits of permitting buyouts of rolling delinquencies are ambiguous.

From the issuer's perspective, a loan buyout can deliver benefits beyond preserving MBS pool quality. An issuer may prefer to repurchase a high-coupon loan at par following a brief delinquency if it has private information suggesting the borrower will cure. In that case, waiting for the loan to become current and then resecuritizing it can generate substantial profits because of the spread between the loan's coupon and the prevailing par mortgage coupon.

2.3 Ginnie Mae's Buyout Suspension

Given concerns about prepayment risk, investors urged Ginnie Mae to eliminate the buyout option on rolling delinquencies to support higher MBS prices. Ginnie Mae found the argument persuasive, particularly because lower bond valuations undermined its mission to facilitate housing finance. On November 6, 2002, Executive Vice President George Anderson issued "All Participants Memorandum 02-24," which limited buyouts to loans at least 90 days delinquent. The policy applied to all new Ginnie Mae issuances beginning January 1, 2003.

The suspension represented a clear break in lenders' expectations for mortgages originated before the announcement. Ordinarily, Ginnie Mae consults a broad set of stakeholders when considering major policy changes, including the Mortgage Bankers Association (MBA), issuers, and MBS investors. Significant changes are typically announced at least six months in advance to give market participants sufficient time to adjust and to avoid operational disruptions. The 2002 memorandum, however, departed sharply from this practice. One Ginnie Mae executive described the announcement as a directive to "stop immediately." In private conversations, the executive noted that, at most, Ginnie Mae had sought the MBA's views on the value of the buyout option, without disclosing any intent to suspend the policy. The abrupt release was designed precisely to prevent anticipation by issuers who had been exploiting the letter of the law.

The Ginnie Mae executive emphasized that the policy change was not prompted by fraudulent use of the buyout option. Ginnie Mae routinely conducted field reviews to ensure issuers' compliance with program requirements, including adherence to buyout rules. Contemporary reporting supports this account: HousingWire noted that early Ginnie Mae audits uncovered no abuse of the rolling delinquency provision.[1] While, in practice, issuers may have circumvented the spirit of the policy, the 2002 announcement was not a response to fraud.

I further investigate whether the buyout suspension was a response to poor loan performance for mortgages pooled into Ginnie Mae MBS. If past loan performance influenced the decision to suspend the buyout option, then any autoregressive variation in future loan outcomes would also correlate with the timing of the suspension. To that end, the Ginnie Mae executive confirmed that the policy change was unrelated to underlying loan performance, such as delinquency or foreclosure rates. At the time, Ginnie Mae did not track delinquencies and payoffs with the same rigor as it does today. Because issuers advanced payments to investors even after borrower default, Ginnie Mae's oversight focused primarily on whether issuers remained current on pass-through obligations. Taken together, the policy change was implemented independently of loan outcomes.

Key to my analysis is that the Ginnie Mae policy suspension generated quasi-experimental variation in loan outcomes across origination months. For loans originated early in 2002, the policy change had little effect because these loans were typically securitized well before the January 2003 deadline. Issuers of such loans therefore retained the option to buy out loans following a rolling delinquency. However, as loans were originated closer in time to the policy change, they faced a diminishing likelihood of being securitized prior to the January 2003 deadline.

[1] Linda Lowell, "Ginnie Buyouts Rattle Investor Nerves," HousingWire, October 28, 2009.

Ultimately, the Ginnie Mae buyout suspension presents a uniquely clean setting for identifying the effect of buyouts on loan outcomes. Unlike policies that respond to contemporaneous loan performance and that result from lengthy policy deliberations, the Ginnie Mae suspension was plausibly exogenous to both loan outcomes and market expectations at the time of origination. As a result, the variation in buyout eligibility across origination months provides a relatively clean shock for studying how buyouts affect loan outcomes.

The intuition behind this is best illustrated in Figure 2. In Panel A, I plot the propensity for loans originated in July to November to change their investor status to Ginnie Mae by December of the same year. This plot presents average pre-January status change, with July as the baseline month. Mechanically, nearly all loans intended for Ginnie Mae and originated in July and August change their investor status well before January. In September, this estimate falls by 10 percent, then by over 20 percent in October. By November, loans are 60 percent less likely to change investor status by January 1 of the following year. This pattern is mechanical and stable over time: it reflects only how much runway a loan has to be securitized before an arbitrary future date——here, January 1 of the following year.

For loans originated in 2002, securitization before January 1 was arbitrary but not inconsequential. Failing to securitize before January 1 of the following year meant that a loan would not retain the buyout option for rolling delinquencies. This key difference is apparent in Panel B of Figure 2, which plots average buyout propensity by month of origination, adjusted to July as the baseline. For Ginnie Mae loans originated in 2001, there is no clear difference in buyout propensity across origination months. In stark contrast, loans originated in 2002 exhibit a drop-off in buyout propensity relative to July that is nearly parallel to the decline in pre-January investor status changes. The break in trend shown in Figure 2 directly reflects the suspension of rolling-delinquency buyouts, since later-originated loans were unable to securitize in time to avoid the new restriction.

Critical to my analysis is that lenders did not expect pre-January securitization to have a differential effect on loan outcomes relative to the prior year. Therefore, the Ginnie Mae policy change mechanically generates quasi-experimental variation in buyout propensity via variation in pre-January securitization across month of origination.

3 Data

3.1 CoreLogic Loan-Level Market Analytics Data

I use the CoreLogic Loan-Level Market Analytics (LLMA) dataset to examine loan performance over the sample period. The data, collected from the 25 largest U.S. mortgage servicers, cover roughly 45% of mortgages originated in 2002. The dataset provides detailed

origination characteristics, including borrower FICO score, loan balance, initial interest rate, loan-to-value ratio, term, and origination date (month and year), as well as geographic identifiers at the ZIP code level. It also tracks monthly performance outcomes such as unpaid principal balance and delinquency status.

3.2 Dataset Construction and Summary Statistics

To construct my sample, I identify all loans originated between July and November in 2001 and 2002. I retain loans that indicate being sold to Ginnie Mae, Fannie Mae, or Freddie Mac within six months of origination. I drop November-originated loans in which the indicator occurs exactly six months after origination, and July-originated loans in which the indicator occurs in the month of origination, because these observations lack variation across origination months. Because the dataset does not report securitization dates directly, using the sale indicator lets me assign each loan to a securitizing agency. Along these lines, I further restrict the analysis to FHA loans in the Ginnie Mae sample and conventional loans in the GSE sample.

To eliminate outliers, I retain fixed-rate loans with 30-, 20-, or 15-year terms, loan-to-value ratios below 1.5, balance less than $1 million, and an associated Metropolitan Statistical Area (MSA) located in the contiguous United States. Because CoreLogic LLMA data have incomplete information on credit scores for this period, I retain loans with an inferred collateral type, which measures a loan's prime or subprime status. I also exclude loans that are censored due to the sale of servicing rights.

I further impose three sample restrictions. First, I retain loans that enter rolling delinquency by July 2007, the last month before the asset-backed commercial paper market freeze. This cutoff mitigates concerns that crisis dynamics drove delinquency onset. Second, I define rolling delinquency as four consecutive months initiated by a single missed payment. As an exception, I allow a second missed payment only if the final two months each show exactly one missed payment, so that the borrower is only one payment past due at the end of the rolling-delinquency period. This requirement preserves loans with sufficient remaining value to be credible buyout candidates and excludes loans on the cusp of 90-day delinquency.

Finally, I restrict the sample to loans valued above par at the time they experience a rolling delinquency. A loan is valued above par when its current interest rate exceeds the par mortgage coupon rate at the time of delinquency. Hence, issuers receive a spread above their cost of funds to execute a buyout and may benefit from re-securitizing the above-par loan in the future. This sampling restriction is necessary to study loan outcomes for which a buyout is a worthwhile option.

To identify buyout status, I measure a change in investor status from Ginnie Mae to

a non-Ginnie Mae investor within two months of a qualifying event. A qualifying event is either a rolling delinquency or a 90-day delinquency. Note that this measure does not capture buyout activity for GSE-securitized loans because those loans do not change investor status following delinquency. Instead, the GSEs repurchase the loans directly and retain them on their balance sheets. In contrast to the GSEs, Ginnie Mae does not purchase loans or issue securities directly, but rather functions as a platform for issuers. Issuers purchase a loan from the Ginnie Mae securities pool upon a qualifying event, thereby removing the loan's Ginnie Mae status in the data.

Methodologically, my buyout measure is novel to the literature. To the best of my knowledge, this is the first use of a proprietary dataset to identify buyout status in agency mortgage-backed securities. It is important to reiterate that every agency loan retains a buyout option for its issuer upon serious delinquency. Given that Ginnie Mae loans represent 25% of an $8.5 trillion market, this measure should prove useful for future research on the Ginnie Mae market, the broader RMBS market, and the economics of distressed-debt sales.

Table 1 reports summary statistics for origination characteristics and loan outcomes in my primary sample. The primary sample consists of loans sold to either Ginnie Mae or the GSEs between July and November, conditioning on a rolling delinquency and being valued above par. Panel A reports loans originated in the treated year 2002. Panel B reports loans originated in the control year 2001.

Several differences across Ginnie Mae and GSE loans are notable. First, despite nearly equivalent initial interest rates, Ginnie Mae loans have higher loan-to-value ratios and fewer prime borrowers than GSE loans. GSE loans have average LTVs close to 80% and about 70% prime borrowers. In contrast, the average LTV for Ginnie Mae loans is close to 95% and roughly 40% of borrowers are prime. This pattern is consistent with Ginnie Mae serving as a platform to facilitate mortgage access for low-income, high-LTV borrowers.That role is also reflected in the lower refinancing share among Ginnie Mae loans—nearly two-thirds of GSE loans are refinances, compared with about one-quarter of Ginnie Mae loans. Finally, Ginnie Mae loan balances are on average about 10% smaller than GSE loans.

Despite these differences in observable loan characteristics, loan outcomes appear relatively similar when conditional on rolling delinquency. Ginnie Mae loans tend to reperform after rolling delinquency and experience foreclosure at similar frequencies as GSE loans. Turning to buyouts, two-thirds of Ginnie Mae loans that experience a rolling delinquency are bought out; by contrast, this share is nearly zero in the GSE sample. This is not because the GSEs do not repurchase delinquent loans but because such loans do not appear to change investor status in the data.

4 Baseline Results

In my baseline model, I compare differences in loan outcomes between mortgages bought out of Ginnie Mae pools and those that remain. I use the sample of loans originated in the year before treatment (2001) and sold to Ginnie Mae (the treated market segment). Conditional on rolling delinquency and being valued above par, I estimate ordinary least squares (OLS) regressions of the following form:

$$Foreclosure_{itq} = \sum_{t} \zeta_t \, Z_t + \beta Buyout_{itq} + X'_{itq}\gamma + \alpha_{iq} + \varepsilon_{itq} \tag{1}$$

$Foreclosure_{itq}$ measures whether loan i, originated in month t and delinquent in quarter q, experiences a foreclosure start within 24 months following a rolling delinquency. $Buyout_{itq}$ is an indicator for a mortgage being bought out of the Ginnie Mae pool within two months of a rolling delinquency. X_{itq} is a vector of loan, borrower, and regional characteristics. Following Kruger (2018), I control for loan terms such as the origination interest rate, loan-to-value ratio, an indicator for inferred prime status, the log of the original mortgage balance, and an indicator for term length. I also account for underwriting quality by including indicators for income-verification status. In addition, I control for loan purpose with indicators for refinance status, primary-residence, and single-family properties. Finally, I include measures of local economic activity at the time of delinquency, such as the monthly county-level unemployment rate and the log of the annual house-price index, GDP, population, and income. I also include MSA–quarter fixed effects, denoted α_{iq}. All regressions also include month-of-origination fixed effects Z_t, effectively demeaning outcomes by their respective monthly averages. Standard errors are clustered at the MSA level to account for within-MSA residual correlation.

Table 2 reports estimates of Equation (1) using the sample of Ginnie Mae loans originated in 2001 that experienced a rolling delinquency while valued above par. I derive estimates using loans originated between July and November 2001, one year prior to the policy announcement. This approach abstracts from any effect of the Ginnie Mae policy and provides a stable benchmark relationship between buyout and foreclosure. Column (1) reports a univariate regression of foreclosure on buyout, demeaned relative to the month of origination. The estimated effect is -1.5% and is statistically significant at the 5% level. In Column (2), after including controls, the estimate falls slightly in magnitude and becomes statistically insignificant. In Column (3), including MSA fixed effects has little effect on the magnitude or significance. After including quarter-of-delinquency fixed effects, however, the estimated effect is 2.4% and is statistically significant at the 1% level. Finally, after including

MSA–quarter-of-delinquency fixed effects, the estimated effect remains positive at 2.1% and is statistically significant at the 5% level.

The baseline results in Table 2 indicate that there is no clear relationship between buyouts and foreclosure initiation. There may be a selection effect: buyout appears to correlate with loan quality, as evidenced by the attenuating point estimates from Column (1) to Column (2) after introducing loan-level controls. Moreover, the estimated effect of buyout on loan outcomes is highly sensitive to the timing of delinquency, as shown by the positive effect in Column (4) after including quarter-of-delinquency fixed effects. Even so, despite controlling for a rich set of origination characteristics and time-varying local economic conditions, it remains unclear how loan outcomes differ by buyout status independent of unobserved heterogeneity.

5 Main Results

The analysis in the previous section naturally suffers from endogeneity concerns arising from asymmetric information in buyout. For example, an issuer with private information may choose to buy out a low-quality loan to prevent high delinquency in its Ginnie Mae portfolio. Alternatively, an issuer may buy out precisely those loans most likely to reperform after a rolling delinquency, creating an opportunity to profit from re-securitizing the mortgage once it becomes current. Hence, the relationship between foreclosure and buyout may be biased upward or downward depending on which channel dominates.

To separate the effect of buyout from loan quality, an ideal experiment would randomly assign a sale to one of two identical delinquent loans and then measure subsequent loan performance. Such an experiment would account for both loan quality and local economic conditions faced by borrowers. To approximate this, I exploit the timing of the 2002 Ginnie Mae memorandum that eliminated the buyout option for rolling delinquencies. My strategy relies on the fact that loans originated closer in time to the memorandum were less likely to be bought out than loans originated earlier in the year. This is a mechanical consequence of the memorandum, which declared that any loan securitized after January 1, 2003 would not retain a buyout option for rolling delinquencies. Given the lag between origination and securitization, loans issued closer to the memorandum were less likely to be securitized prior to January and therefore less likely to retain the buyout option.

In theory, the month of origination should correlate with buyout propensity but should not systematically vary with loan quality over a short time horizon. The policy was intentionally delivered so as to be unanticipated by market participants and was unrelated to past loan performance. If the memorandum was unanticipated and loan quality did not differ

significantly over that short horizon, then originating a loan closer in time to the policy—versus originating it earlier—should have no effect on future loan performance except through quasi-experimental variation in buyout propensity.

To rule out origination-month seasonality unrelated to the memorandum, I implement a difference-in-differences-in-differences (DDD) design. The design contrasts Ginnie Mae (treated) and GSE (control) loans across the treated year (2002) and the prior year (2001), and across high-exposure months (October–November) and low-exposure months (July–September). Conditional on rolling delinquency and being valued above par, I estimate ordinary least squares (OLS) regressions of the following form:

$$\begin{aligned} Y_{itq} = {} & \beta_1 Y02_t + \beta_2 GNMA_i + \beta_3 (Y02_t \times GNMA_i) \\ & + \sum_t \zeta_t Z_t + \sum_t \phi_t (Z_t \times Y02_t) + \sum_t \varphi_t (Z_t \times GNMA_i) \\ & + \delta (Z_{t\geq 10} \times Y02_t \times GNMA_i) + X'_{itq}\gamma + \alpha_{iq} + \varepsilon_{itq} \end{aligned} \tag{2}$$

In the specification above, Y_{itq} is an indicator measuring loan performance for loan i originated in month t and delinquent in quarter q. The indicator variable Z_t equals one if a loan was originated in calendar month t. The indicator variable $Z_{t\geq 10}$ equals one if a loan was originated in October or November. I choose October–November as the cutoff date for high exposure because the average lag between origination and securitization is one to two months. The indicator $Y02_t$ equals one for loans originated in the treatment year (2002) and zero otherwise. The indicator $GNMA_i$ equals one for loans originated in the treated market segment (Ginnie Mae) and zero otherwise. X_{itq} is a vector of loan, borrower, and regional characteristics.

Following Kruger (2018), I control for loan terms such as the origination interest rate, current interest rate, loan-to-value ratio, an indicator for inferred prime status, the log of the original mortgage balance, the log of the current mortgage balance, and a term-length indicator. I proxy underwriting stringency with indicators for income verification. I also control for loan purpose with indicators for refinance loans, primary residences, and single-family homes. To capture local economic conditions at the time of delinquency, I include the county unemployment rate and the log of the house-price index, GDP, population, and income. I absorb MSA-by-quarter of delinquency fixed effects, α_{iq}, to capture time-varying local shocks. Because delinquency timing covaries with origination month, these local controls mitigate spurious correlations in outcomes. Standard errors are clustered at the MSA level to account for within-MSA residual correlation.

The coefficient β_1 captures variation unique to the treated year (2002) and is common across market segments. The coefficient β_2 captures variation unique to the Ginnie Mae

market segment and is common across years. The coefficient β_3 captures the differential effect for Ginnie Mae loans in 2002 after netting out the separate year and segment effects.

I also include calendar-month indicators and their interactions to absorb month-specific variation. The coefficients ζ_t capture month-t fixed effects that are common across all segments and years. The coefficients ϕ_t capture variation specific to month t in the treated year, common across all segments. The coefficients η_t capture differential variation specific to Ginnie Mae loans originated in month t, common across all years.

The triple-difference coefficient of interest, δ, identifies the causal effect of the buyout suspension on loan outcomes for high exposure loans relative to low exposure loans. The key identifying assumption is that loan outcomes would have evolved similarly between high-exposure and low-exposure origination months absent the Ginnie Mae buyout suspension. The coefficient δ contrasts October–November originations with July–September originations within 2002 for Ginnie Mae loans, after differencing out within-calendar-month variation for each market segment and year separately.

As an aside, the causal effect identified in Equation 2 is a lower bound on the policy's true effect. Because exposure varies continuously over time, loans originated in low-exposure months receive some treatment. Although most July–September originations would have securitized into Ginnie Mae MBS before the announcement, a subset likely failed to do so due to capacity constraints or quality control. These lagging low-exposure loans are therefore treated if they miss the deadline. Consequently, the coefficient δ differences out part of the policy-induced variation that is common across origination months.

5.1 Identification

I identify δ from the timing of the Ginnie Mae memorandum while holding fixed within-MSA–quarter–of–delinquency variation. The key identifying assumption is that loans originated in the last two months before the policy (October–November) would have exhibited outcomes similar to those of the first three months (July–September), if not for the Ginnie Mae policy shock.

Because the memorandum was unanticipated and released after loan application and approval, the timing of origination is, by definition, independent of the policy-induced buyout eligibility. Namely, the decision to approve a loan in any given month was made independently of the policy change. However, this assumption may be difficult to support if calendar-month correlates with time-series variation in loan characteristics, such as interest rates and house prices. To assuage these concerns, I employ a weaker set of assumptions. I assume that origination in high-exposure versus low-exposure months is quasi-randomly assigned after (i) conditioning on observables and (ii) restricting the sample to a short window

of time. Conceptually, I am comparing two identical loans differing in origination date by only a few months. Since economic conditions should vary little in the immediate short run, origination month assignment should be as good as random.

Nevertheless, unobservable loan quality may still correlate with the timing of origination even after the battery of controls described above. To further rule out this possibility, I examine time-series variation in ex ante observable loan characteristics by month and year of origination. A stark change in observable characteristics prior to the announcement would indicate a change in unobservable quality. In Figure 3, I plot baseline-adjusted means of loan characteristics for Ginnie Mae loans originated between July and November of 2001 and 2002. Specifically, I plot average loan balance, loan-to-value ratio, interest rate, and prime-borrower status. Loan balance, LTV, and prime-borrower status each vary across months and years by roughly 0.1 standard deviations, and they show little change in the months immediately preceding the announcement. Interest rates, however, exhibit systematic differences across years and a downward trend across months.

Taken together, the foregoing provides suggestive evidence that the treated- and control-year samples are approximately comparable. Variation across years and months in pre-policy characteristics appears stable relative to the July baseline. While I can control for the observed systematic variation in interest rates over time, unobserved heterogeneity endogenous to both origination timing and loan outcomes may remain. To address this, I include a comprehensive set of fixed effects to mitigate endogeneity from unobserved loan quality correlated with origination timing. Specifically, I include fixed effects for calendar month, treatment year, and investor market segment. These absorb common seasonal variation, year-specific macroeconomic variation, and systematic differences between the Ginnie Mae and GSE samples.

Despite these controls, one may still be concerned about seasonality unique to the treated year. To address this, I interact calendar-month indicators with the treatment-year indicator. Doing so controls for unobserved heterogeneity associated with loans originated in calendar month t in 2002. This is a highly robust control because it accounts for variation across loan vintages that could be highly correlated with the timing of loan outcomes, such as delinquency, foreclosure, and prepayment.

Furthermore, certain market segments may experience seasonally distinct loan outcomes. I address this by interacting calendar-month indicators with the treated market-segment indicator. This controls for variation specific to loans originated in calendar month t and sold to Ginnie Mae investors. The interaction therefore absorbs identifying variation that might otherwise explain why Ginnie Mae loans exhibit higher foreclosure rates in the fall than in the summer.

After controlling for this rich set of interactions, origination timing in high-exposure months relative to low-exposure months during the treated year within the Ginnie Mae market is plausibly as-good-as-randomly assigned, except for the policy change. I therefore identify δ from quasi-experimental variation in origination timing that shifts buyout eligibility—and hence buyout propensity—around the January securitization cutoff.

Formally, identification requires that loans originated later in 2002 would have exhibited outcomes similar to those originated earlier in the year, were it not for the Ginnie Mae policy change. Although the counterfactual is not directly testable, I assess its plausibility with an event-study design that tracks outcomes by calendar month. If exposure increases as the window to securitize before January narrows, the policy effect should strengthen monotonically as originations move from July toward the cutoff. To implement this test, I replace the single high-exposure interaction ($Z_{t\geq 10} \times Y02_t \times GNMA_i$) with the full set of month-specific interactions and estimate month-level effects relative to July:

$$\begin{aligned} Y_{itq} = {} & \beta_1 Y02_t + \beta_2 GNMA_i + \beta_3 (Y02_t \times GNMA_i) \\ & + \sum_t \zeta_t Z_t + \sum_t \phi_t (Z_t \times Y02_t) + \sum_t \varphi_t (Z_t \times GNMA_i) \\ & + \sum_t \delta_t (Z_t \times Y02_t \times GNMA_i) + X'_{itq}\gamma + \alpha_{iq} + \varepsilon_{itq} \end{aligned} \tag{3}$$

The coefficients δ_t trace the month-specific triple-difference effects relative to July. Each δ_t measures the differential trend in outcomes for loans originated in month t, comparing Ginnie Mae to GSE loans after accounting for seasonal variation by market segment and year. The identifying assumption is that loans originated in months after July would have exhibited outcomes similar to those in adjacent months in the absence of the memorandum. This assumption is stronger than that in Equation 2 because it requires that month-to-month loan quality not deviate materially from that in July within the short window studied. If the policy operates through the January securitization cutoff, the set of δ_t coefficients should display a monotonic pattern as originations move from July toward November, reflecting the shrinking window to securitize before the cutoff.

5.2 Effect on Buyout

Table 3 Panel A presents estimates of Equation (2) using buyout as the dependent variable. Columns (1) to (5) report results from the CoreLogic LLMA sample of Ginnie Mae and GSE loans originated between July and November in 2001 and 2002, conditional on rolling delinquency and being valued above par. The estimated coefficients measure the differential change in buyout activity for loans originated in the treated period (October–November)

relative to the control period (July–September). This specification exploits within-calendar-month variation that is specific to the treatment year and to Ginnie Mae loans.

Column (1) reports a 30.5% decline in buyout activity, significant at the 1% level, using a specification without fixed effects or additional covariates. The model includes only baseline indicators and their pairwise interactions for origination month, year, and investor segment. Thus, the estimate reflects the average difference in buyout activity between high- and low-exposure originations, before adjusting for loan characteristics or delinquency timing.

Column (2) adds loan- and borrower-level controls including current and initial interest rates, loan-to-value ratio, the logs of current and initial loan balance, and indicators for credit quality, refinance status, primary occupancy, single-family status, and loan term. Column (3) absorbs MSA fixed effects so identification comes from within-MSA comparisons, removing slow-moving local conditions. Column (4) adds quarter-of-delinquency fixed effects, capturing macroeconomic conditions common to loans delinquent in the same quarter. Across Columns (2) through (4), point estimates range from 29.9 to 30.4 percent and remain significant at the 1% level.

Column (5) includes MSA and quarter-of-delinquency fixed effects jointly, restricting identification to within-MSA–quarter comparisons. In the most saturated model, Ginnie Mae loans originated in the last two months of 2002 exhibit a 30.1 percent reduction in buyouts, statistically significant at the 1% level.

To further validate my research design, I estimate a dynamic triple-difference specification. Figure 4 Panel A plots coefficients from Equation (3) with 95% confidence intervals. Panel A displays differential trends in buyout propensity across origination months for Ginnie Mae versus GSE loans, after accounting for seasonal variation by market segment and year. The coefficients δ_t are normalized to July, and the specification mirrors Column (5) of Table 3, which includes lower-order interactions, loan-level controls, and MSA–quarter–of–delinquency fixed effects.

Absent the Ginnie Mae policy, month-to-month differences in differential trends should be negligible after absorbing calendar-month seasonality and month-specific variation by segment and year. The event-study comparison removes, for each month, variation common to non-Ginnie loans and variation common to other years. Any remaining variation associated with 2002 Ginnie Mae loans would therefore be expected to be as good as random relative to the July baseline. Instead, the estimates decline monotonically as originations move from July toward November, indicating a sharp reduction in buyout rates for cohorts increasingly constrained by the January 1 securitization cutoff. Estimates for loans originated in August 2002 are statistically indistinguishable from July. In September 2002, Ginnie Mae loans begin to experience a statistically significant decline in buyout propensity of 10 percent.

This magnitude rises to 20 percent in October 2002 and to 40 percent by November 2002. The sharp, monotonic decline in buyout propensity in the months after July and before the policy reinforces the validity of the research design and supports a causal interpretation of the results.

Taken together, the estimates reported in Table 3 Panel A and Figure 4 Panel A suggest that the Ginnie Mae policy severely restricted buyout activity for loans issued in the months preceding the policy announcement relative to earlier originated loans.

5.3 Effect on Foreclosure

Central to my identifying assumption is that the drop in buyout activity identified in the previous section reflects the narrow runway for late-originated loans to securitize before the January 2003 deadline. Loans issued immediately prior to the policy shock could not securitize in time to retain the buyout option for rolling delinquencies. With this in mind, the Ginnie Mae policy shock provides an opportunity to test how the buyout option affects long-term loan outcomes. An issuer can expect that, after buying out a loan, the mortgage can be re-securitized into a new Ginnie Mae pool if the borrower becomes current following a rolling or a serious delinquency. Thus, the buyout option generates an incentive for issuers to maintain high loan quality among re-pooled loans to increase investor appetite. Eliminating the buyout option would therefore unwind these incentives and could cause loan quality to deteriorate.

The regressions in Table 3 Panel B test the effect of the Ginnie Mae policy change on loan quality by estimating Equation (2) with foreclosure as the outcome of interest. Here, Y_i equals one if loan i experiences a foreclosure start within 24 months of a rolling delinquency. I use a 24-month window rather than a shorter window to accommodate the long transition from a 30-day rolling delinquency into foreclosure. This differs from much of the literature, which typically measures transitions from 60- or 90-day delinquency into foreclosure. Moreover, loans that experience a rolling delinquency should take longer to enter foreclosure because the borrower is current on three of the last four payments; to reach foreclosure, the borrower would need to miss additional payments after having already cured earlier misses.

Columns (1) to (5) report estimates for the sample of loans sold to Ginnie Mae (treated group) and the GSEs (control group) that were originated in 2001 (control year) or 2002 (treated year) and were valued above par at the time of rolling delinquency. The coefficient of interest, δ, measures the differential effect of the Ginnie Mae policy shock on foreclosures for loans originated in the high-exposure period relative to loans originated in the low-exposure period. As before, the specification controls for loan characteristics, time-varying local economic conditions, and lower-order interactions of calendar month, calendar year,

and market segment.

In Column (1), absent any controls or fixed effects, I find that loans originated in the high-exposure period experienced a 3.5 percent increase in foreclosures, statistically significant at the 5% level. This model includes baseline indicators and their pairwise interactions for origination month, year, and investor segment; thus, identification is net of seasonal variation attributable to origination month–year and origination month–segment. Furthermore this estimate represents a lower bound on the policy's true effect, given loans originated in low-exposure months receive some treatment.

Column (2) includes controls to account for variation correlated with origination characteristics and county-level conditions at the time of delinquency. Column (3) introduces MSA fixed effects to account for common shocks within a given geography, and Column (4) controls for time-varying macroeconomic shocks using quarter-of-delinquency fixed effects. Across Columns (2) to (4), the point estimate ranges from 4.0 percent to 4.5 percent and is statistically significant at the 5% level.

In my most restrictive specification, Column (5) controls for time-varying local economic conditions at the time of delinquency using MSA–quarter-of-delinquency fixed effects. In this specification, loans originated in high-exposure months experienced a 4.3 percent increase in foreclosures relative to the control group, statistically significant at the 5% level.

Taken together, the evidence in Table 3 Panel B suggests that loans originated in high-exposure months experienced higher rates of foreclosure relative to loans originated in low-exposure months, plausibly due to the suspension of buyouts for rolling-delinquent loans. The credibility of this conclusion depends on the key identifying assumption that high-exposure loans would have experienced outcomes similar to those of low-exposure loans absent the Ginnie Mae policy change. To evaluate the validity of this assumption, I estimate a dynamic triple-difference specification.

Figure 4 Panel B plots coefficients from Equation (3) with 95% confidence intervals. The estimates represent differential trends in foreclosure propensity across origination months for Ginnie Mae versus GSE loans, after accounting for seasonal variation by market segment and year. The coefficients δ_t are normalized to July, and the specification mirrors Column (5) of Table 3. It includes lower-order interactions, loan-level controls, and MSA–quarter–of–delinquency fixed effects.

I find that the estimates for δ_t increase monotonically as origination moves from July toward November, indicating a gradual rise in foreclosure starts for cohorts increasingly constrained by the January securitization cutoff. Estimates for loans originated in August 2002 are statistically indistinguishable from July. In September 2002, Ginnie Mae loans begin to experience a statistically significant increase in foreclosure rates of 6 percent. This magni-

tude increases to near 7 percent in October 2002 and rises to over 8 percent by November 2002.

Ultimately, the results in Table 3 Panel B and Figure 4 Panel B provide credible evidence that the increase in foreclosures is likely due to the suspension of the buyout option for rolling-delinquent loans and not to unobserved factors correlated with origination month. Relative to the counterfactual in which buyouts remained available, these results imply that long-term loan performance improves when issuers purchase loans in rolling delinquency out of the MBS pool.

6 Mechanism

6.1 Effect on Cures

As an immediate consequence of the Ginnie Mae policy shock, issuers could no longer exercise the option to buy out a loan when it experienced a rolling delinquency. In the long run, loans originated immediately prior to the policy shock appear to experience differentially higher foreclosure rates following a rolling delinquency relative to loans originated earlier in the year. The direct mechanism is straightforward: the lag between origination and securitization likely prevented loans originated later in the year from securitizing before the January 1, 2003 deadline, and thus from retaining the buyout option. Hence, buyouts can help prevent foreclosure.

In this section, I further examine this relationship by studying the short-run impact of the policy shock on loan outcomes. After a loan is bought out, the borrower must be current before the issuer can re-securitize the mortgage. Hence, the issuer has a strong incentive to make the loan reperform after a buyout, likely through renegotiation of mortgage terms or forbearance. However, if the Ginnie Mae shock eliminated the buyout option, I expect to see a reduction in delinquency exits immediately following a rolling delinquency.

Table 4 Panel A presents estimates of Equation (2) using delinquency exits as the dependent variable. Here the outcome of interest, Y_i, equals one if loan i experiences a delinquency exit within three months of a rolling delinquency, which I refer to as an early cure. I use a three-month window to distinguish a cure associated with a rolling delinquency from a cure associated with a 90-day (serious) delinquency. Although the Ginnie Mae policy shock eliminated the buyout option for rolling delinquencies, issuers retained the right to buy out a mortgage following a serious delinquency. Thus, if a borrower missed two additional payments after a rolling delinquency and entered a 90-day delinquency, the issuer could still execute a buyout even if the loan was securitized after the January 1, 2003 deadline. By restricting the window to cures within three months, I identify the effect of the policy shock

on early cures as distinct from variation in cures over longer horizons. The coefficient of interest, δ, measures the differential effect of the policy shock on early cures for Ginnie Mae loans originated in 2002 during high-exposure months relative to loans originated in low-exposure months.

In Column (1), absent controls and fixed effects, high-exposure loans experience a 4.2 percent decline in early cures, statistically significant at the 5% level. This implies that, absent the buyout suspension, treated loans would have been 4.2 percent more likely to exit delinquency and reperform. In Column (2), I account for variation correlated with loan terms and local economic conditions by including loan- and county-level controls. In Column (3), I add MSA fixed effects to account for slow-moving local economic conditions. Column (4) includes quarter-of-delinquency fixed effects, absorbing common macroeconomic conditions at the time of a loan's delinquency. Across Columns (2) to (4), the decline in early cures ranges from 4.6 percent to 4.9 percent and remains statistically significant at the 5% level.

In Column (5), I account for time-varying local economic conditions by absorbing MSA–quarter–of–delinquency fixed effects. Here I estimate that treated loans experienced a 4.2 percent decline in reperformance, statistically significant at the 5% level. Ultimately, the point estimate appears invariant to model specification across a battery of loan-level controls, county-level controls, and time-varying geographic fixed effects.

To further validate that the estimates represent monotonic variation in exposure, I estimate a dynamic difference-in-differences specification. Figure 5 Panel A plots coefficients from Equation (3) with 95% confidence intervals. The estimates represent differential trends in early-cure likelihood across origination months for Ginnie Mae versus GSE loans, after accounting for seasonal variation by market segment and year. The coefficients δ_t are normalized to July, and the specification mirrors Column (5) of Table 4, including lower-order interactions, loan-level controls, and MSA–quarter–of–delinquency fixed effects.

I find that the estimates for δ_t decline monotonically as origination moves from July toward November, indicating a gradual reduction in early cures for loans originated closer to the January securitization cutoff. Estimates for loans originated in August 2002 are statistically indistinguishable from July. In September 2002, Ginnie Mae loans begin to experience a statistically significant decline in early cures of 7 percent. This estimate is roughly equal to the estimate for October 2002 and rises to over 8 percent by November 2002.

The results in Table 4 Panel A and Figure 5 Panel A imply that the decline in loan reperformance is likely due to issuer inability to pull delinquent loans out of their associated MBS pools. Issuers may fail to facilitate cures for these loans, perhaps because of financial or legal constraints that bind when buyouts are infeasible. I conclude that the increase in

foreclosures is likely driven more directly by the decline in early loan reperformance. This assumption is supported by the close symmetry in point estimates for Column (5) in Table 3 Panel B and Table 4 Panel A: high-exposure loans experienced a 4.3 percent increase in foreclosures, which almost matches the 4.2 percent decline in early cures. These estimates lend credibility to the hypothesis that the long-run increase in foreclosures is driven, at least in part, by the decline in early cures resulting from restricted buyout activity. Taken together, these results suggest that, relative to the counterfactual in which buyouts remained available, issuers help loans reperform quickly following a buyout, thereby preventing severe deterioration in loan quality.

6.2 Effect on Modifications and Prepayment

In Table 4 Panel B, I examine whether a reduction in modifications explains the decline in early cures following the suspension of buyouts. I test whether loans cure after a buyout because issuers modify mortgage terms. Once a loan is securitized into a mortgage-backed securities pool, issuers face legal constraints on modifying loan terms. Buyouts allow issuers to remove loans from the MBS pool and thereby circumvent these constraints. Suspending buyouts should therefore reduce modifications and, in turn, loan reperformance, consistent with the estimates in Table 4 Panel A.

To this end, I estimate Equation (2) where the outcome Y_i equals one if loan i experiences a modification within three months of a rolling delinquency. The three-month window links an identified modification to a rolling delinquency rather than to a 90-day (serious) delinquency. Modifications are identified as changes to the principal balance or reductions in interest rates. Due to data limits, I cannot identify term extensions. In Columns (1) to (5), I sequentially add borrower- and loan-level controls, county fixed effects, MSA fixed effects, and MSA–quarter fixed effects. Across all specifications, the estimate for δ is statistically and economically indistinguishable from zero. The near absence of variation across origination months indicates that the buyout suspension did not reduce early cures via changes in loan terms. Relative to the counterfactual in which buyouts remained available, these results imply that issuers do not rely on modifications to facilitate loan reperformance following a buyout, despite having greater legal flexibility to do so.

In Table 4 Panel C, I test for changes in prepayment activity. If loans increasingly experience quit-claims, short sales, or refinancing as alternatives to modification, then, mechanically, observed cures will decline due to censoring. I present estimates of Equation (2) using a prepayment indicator within three months of a rolling delinquency as the dependent variable. I model the effect of high exposure on prepayment likelihood by sequentially adding controls for loan- and county-level characteristics, time-varying macroeconomic conditions,

and time-varying local economic conditions. Across Columns (1) to (5), the estimate for δ is statistically indistinguishable from zero and equals -1 percent in my most restrictive specification. Thus, the decline in loan cures cannot be explained by borrower prepayment. There is no evidence that borrowers substituted refinancing for buyouts, nor that lower-quality loans inflate the share of loans lingering in delinquency. Relative to the counterfactual in which buyouts remained available, these results imply that issuers do not facilitate prepayments following a buyout.

As a final falsification test, I estimate a dynamic triple-difference specification. Figure 5 Panels B and C plot coefficients from Equation (3) with 95% confidence intervals. Panel B reports differential trends in modification propensity across origination months for Ginnie Mae versus GSE loans, after accounting for seasonal variation by market segment and year. Panel C reports differential trends when the dependent variable is a prepayment indicator. The point estimates for both outcomes appear invariant to exposure across all calendar months. Therefore, the point estimates in Table 4 do not reflect any systematic variation across calendar months that is masked by the single interaction term. Instead, Figure 5 supports the interpretation that the suspension of buyouts did not reduce modification or prepayment propensity.

Taken together, while the results in the previous subsection suggest that a decline in loan curing affects long-run loan performance, the precise mechanism remains unclear. Estimates of the differential effect on renegotiation are statistically and economically indistinguishable from zero. Prepayment activity, which might indicate refinancing, also fails to vary with exposure. Thus, there is no observable change in loan terms that would explain the relationship between vintage month and early cure. Issuers therefore likely induce the early cures I identify through at least one of the following forbearance channels: (i) extending a subordinate lien on the property to compensate the issuer for missed payments, (ii) extending the mortgage term to make up for the missed payment, or (iii) negotiating terms in ways unobserved in the data.

Of these three possibilities, given the specialized nature of a rolling delinquency, I posit that issuers' incentives are best aligned with the use of a subordinate lien. To see why, consider that when a borrower enters serious delinquency because of a heavy debt burden, reducing the required payment through a modification can plausibly induce reperformance. In contrast, a rolling delinquency, by definition, occurs when a borrower has made recent payments except for a single earlier missed payment. Therefore, the issuer may reasonably assume the borrower will continue to make payments under the current mortgage terms. Reducing interest or principal under that assumption would be redundant and would entail a realized loss in loan value.

Alternatively, if the issuer capitalizes the arrears into the principal balance, the borrower faces higher per-period payments. For a marginally constrained borrower, increasing the debt burden also increases the risk of default because the borrower has limited capacity to service the debt. In contrast to a modification, placing arrears into a subordinate lien can make the issuer whole, cure the loan, and preserve the present value of future payments without further burdening the borrower.

Indeed, the FHA has offered second liens as a loss-mitigation tool for decades. When a borrower enters their fourth month of delinquency, an issuer may file a "partial claim" with the FHA for the amount of any advanced funds. The partial claim will fully reinstate the loan to current status while capitalizing the arrears into a junior lien on the property. The partial claim charges no interest and requires no payment until the first lien is prepaid or the home is sold. In a report on FHA loss-mitigation policies, HUD found that partial claims rose from 4,000 claims per year in 1999 to 10,000 per year in 2001.[2] Furthermore, HUD found that, of the 25 largest servicers, half employed partial claims more often than modifications or forbearance.

In sum, I find suggestive evidence that early intervention through buyouts can reduce foreclosure rates without obvious changes in payments or loan terms. I argue that issuers instead rely on extending a subordinate lien on the property to reinstate delinquent loans. In Section 7.3, I explore the implications of this argument by studying how the effect of buyouts varies with financial slack.

6.3 Effect on Medium Run Loan Outcomes

The previous section suggests that the suspension of buyouts likely increased foreclosure rates for loans experiencing a rolling delinquency by reducing early cures. In this section, I investigate this channel further by examining how early cures improve loan quality sufficiently to prevent foreclosure among loans that retained buyout eligibility.

Here I focus on the difference between early and late cures. If an issuer fails to cure a loan following a rolling delinquency because of Ginnie Mae's policy change, the same issuer can still exercise its buyout option by waiting two additional missed payments. The suspension of buyouts reduced cures that would otherwise have occurred following a buyout for a rolling delinquency. Hence, measuring cures over a longer horizon should attenuate the estimated effect attributable to buyouts associated with a serious delinquency *after* rolling delinquency. If the effect identified in the previous section is due to the Ginnie Mae policy shock, there should be no differential effect across origination months for late cures, since, in the medium run, loans become eligible for buyout upon worsening delinquency.

[2]HUD OIG, *Loss Mitigation Program*, Audit Report 2002-DE-0001.

In Table 5, I explore this channel using a longer observation window. I estimate Equation (2) using outcomes within a 15-month window following a rolling delinquency, providing a one-year extension to the original 3-month window to allow loans time to progress in delinquency. In Column (1), I estimate that high-exposure loans enter 90-day delinquency 5.5 percent more often than less-exposed loans, statistically significant at the 1% level. The increase in severe delinquencies should subsequently open opportunities for issuers to buy out loans. In Column (2), the magnitude of the estimated effect on buyout activity attenuates by 4 percentage points. At 15-month horizons, treated loans experience a 26.3 percent reduction in buyouts, statistically significant at the 1% level and representing a 12 percent decline in magnitude relative to Column (5) in Table 3. These results imply that, despite the suspension of early buyouts, issuers exercise their option to purchase loans when they become eligible upon entering 90-day delinquency.

I next estimate the effect of the Ginnie Mae policy on loan cures within a 15-month window. In the previous subsection, I estimated the effect on cures within a 3-month window to identify immediate reperformance following a rolling delinquency. Using a longer 15-month window, I measure changes in cures following both a rolling delinquency and a subsequent 90-day delinquency, hereafter termed late cures. In Column (3), I estimate a coefficient of 0.4 percent, which is statistically insignificant. This finding is consistent with the hypothesis that issuers cure loans when they subsequently enter 90-day delinquency and thereby regain the right to execute a buyout. Reinforcing the results from Section 6, I find no increase in modifications or prepayments in Columns (4) and (5) that would explain alternative issuer behavior after regaining the ability to exercise buyouts. Therefore, neither the reduction in cures following early buyouts nor the increase in cures following late buyouts covary with modifications and prepapyment.

Taken together, my results suggest that the reduction in cures documented in the previous subsection is confined to a short horizon. Thus, the increase in foreclosures estimated in Section 5 can be attributed to the reduction in early cures specifically rather than a reduction in loans ever exiting delinquency in general. This distinction between early and late cures has important policy implications. Namely, long-term loan performance is fundamentally altered simply because a loan exits delinquency later than it counterfactually would have otherwise. Indeed, treated loans eventually reperform, so there is no medium-run difference in delinquency exits relative to the control group. If cure timing were immaterial, however, such eventual reperformance would render the foreclosure rate constant across vintage months. Instead, I find that the foreclosure rate increased in line with the decline in early cures. Ultimately, for loans that may eventually cure, failing to cure early can materially worsen long-term performance.

7 Instrumental Variables

The previous sections established that the Ginnie Mae policy change caused a stark reduction in buyout activity following rolling delinquency. In the absence of buyouts, loans experienced an increase in foreclosures and a decrease in early cures. In the following section, I formalize these results using an instrumental-variables approach. This approach yields policy-relevant causal estimates of the elasticity of buyout activity and loan performance.

Using the month–year–market interaction $Z_{t\geq 10} \times Y02_t \times GNMA_i$ as an excluded instrument for buyout activity, I estimate two-stage least squares (2SLS) regressions to identify the effect of loan buyout on loan performance. I discuss the validity of using the triple-difference as an instrument for buyout in Section 7.1 below. Conditional on rolling delinquency and being valued above par, the first stage is represented by regressions of the following form:

$$\begin{aligned} Buyout_{itq} = {} & \beta_1^{(1)} Y02_t + \beta_2^{(1)} GNMA_i + \beta_3^{(1)} (Y02_t \times GNMA_i) \\ & + \sum_\tau \zeta_\tau^{(1)} Z_\tau + \sum_\tau \phi_\tau^{(1)} (Z_\tau \times Y02_t) + \sum_\tau \varphi_\tau^{(1)} (Z_\tau \times GNMA_i) \\ & + \delta (Z_{t\geq 10} \times Y02_t \times GNMA_i) + X_{itq}' \gamma^{(1)} + \alpha_{iq}^{(1)} + \varepsilon_{itq}^{(1)}, \end{aligned} \tag{4}$$

where $(Z_{t\geq 10} \times Y02_t \times GNMA_i)$ is the instrumental variable, equal to one if a loan was originated in October or November 2002 and sold to Ginnie Mae. All lower-order interactions of Z_t, $Y02_t$, and $GNMA_i$ enter both stages as controls, so identification relies solely on variation in the triple difference. In the second stage, conditional on rolling delinquency and being valued above par, I regress loan performance on the fitted values for buyout:

$$\begin{aligned} Y_{itq} = {} & \beta_1^{(2)} Y02_t + \beta_2^{(2)} GNMA_i + \beta_3^{(2)} (Y02_t \times GNMA_i) \\ & + \sum_\tau \zeta_\tau^{(2)} Z_\tau + \sum_\tau \phi_\tau^{(2)} (Z_\tau \times Y02_t) + \sum_\tau \varphi_\tau^{(2)} (Z_\tau \times GNMA_i) \\ & + \rho \widehat{Buyout}_{itq} + X_{itq}' \gamma^{(2)} + \alpha_{iq}^{(2)} + \varepsilon_{itq}^{(2)}, \end{aligned} \tag{5}$$

Where $\widehat{Buyout}_{itq}$ denotes the predicted values from Equation (4). Under the assumption that the triple interaction is a valid instrument, ρ identifies the causal effect of buyout on loan performance.

7.1 Identification

7.1.1 LATE

To identify a causal effect, the instrument I employ must satisfy several conditions. Conditional on year and market segment, the timing of origination must (i) vary with buyout activity, (ii) be as-good-as-randomly assigned, and (iii) affect loan performance only through buyout. In addition, the research design must accommodate heterogeneity in the instrument's response. In particular, if only a subset of loans is affected by origination month, the instrument must satisfy an additional monotonicity condition. Namely, origination month must affect buyout for all loans in the same direction. In this section, I discuss the validity of these assumptions and their implications for external validity.

Relevance To satisfy the relevance condition, an instrument must affect the probability of treatment assignment. In practice, I exploit variation in the three-way interaction $Z_{t\geq 10} \times Y02_t \times GNMA_i$, which shifts buyout propensity. Conditional on year and market segment, month of origination satisfies the relevance condition if a loan's origination month differentially affects the probability that the loan is bought out. As shown in Section 2 and Figure 2, the buyout rate for loans experiencing a rolling delinquency drops precipitously across vintage months beginning in August 2002. In contrast, the buyout rate for loans originated in 2001 experiences no change across vintage months. Regression estimates in Table 3 and coefficients plotted in Figure 4 validate this argument, whereby vintage month differentially affects buyout propensity even after accounting for lower-order interactions and loan-level controls.

Independence A valid instrument must be as-good-as-randomly assigned. Given that the memorandum was unanticipated and released after loan origination, vintage month is a function of conditions exogenous to the policy shock itself. Hence, the timing of origination is not driven by the suspension of the buyout option. As in Section 5.1, I adopt an assumption weaker than pure random assignment. I assume that origination month is as-good-as-randomly assigned after (i) conditioning on observables and (ii) restricting the sample to a short span of time. Within a short window and after controlling for factors observable at origination, the precise timing of origination should matter little. Furthermore, absorbing a comprehensive set of fixed effects helps rule out endogeneity from unobserved loan quality correlated with origination timing. Namely, by including lower-order interactions of origination month, year, and Ginnie Mae status, I absorb any variation attributable to seasonality in Ginnie Mae loans or to timing of origination. The remaining identifying

variation—variation attributable to loans originated in a given month of 2002 and sold to Ginnie Mae—is plausibly as-good-as-randomly assigned.

Exclusion Restriction To satisfy the exclusion restriction, an instrument must not affect the outcome variable except through the treatment. In this context, conditional on year and market segment, month of origination should not affect long-term loan performance except through buyout.

The nature of the instrument supports this assumption. Loan quality and, by extension, loan outcomes should be unrelated to the timing of the policy because the announcement was unanticipated. Moreover, conditional on observables and within a short window, whether a loan is originated in one month or the next should not affect long-term performance. My analysis further conditions on MSA–quarter fixed effects and on lower-order interactions of origination month, year, and Ginnie Mae status. Concretely, the instrument is the three-way interaction $Z_{t\geq 10} \times Y02_t \times GNMA_i$, which is excluded from the second stage. All pairwise interactions and main effects enter both stages as controls. Therefore, only the highest-order interaction is used as the excluded instrument. This restricts identification to pairs of observationally identical loans: originated in the same year, delinquent in the same MSA–quarter, and sold to the same securitizing agency.

Monotonicity To identify a local average treatment effect, I require the additional monotonicity assumption, whereby units do not select out of treatment because of instrument assignment (defiers). In the context of my research design, this requires that if a loan (i) is originated later in time and (ii) experiences a buyout, then it would be at least as likely to experience a buyout if it were originated earlier. This condition is satisfied mechanically, given the nature of the instrument. The instrument exploits the fact that loans originated closer in time to the policy announcement are less likely to securitize prior to the deadline—thus subject to the ban on buyouts. If a late-originated loan happens to securitize prior to the deadline, then having more time to securitize would, by definition, make the loan at least as likely to securitize prior to the deadline.

Local Average Treatment Effect Under the assumption that vintage month satisfies the conditions outlined above, I identify the local average treatment effect (LATE) for loans whose treatment varies with the instrument. That is, the LATE is identified for the subset of loans that experienced a buyout because they were originated early and that would not have been bought out if originated later (compliers). Note that this research design does not identify the effect of buyouts for loans that are (i) always-takers—those with a

short time-to-securitization—or (ii) never-takers—those with an excessively long time-to-securitization—because the timing of origination would be unaffected by the policy.

External Validity Importantly, time is not the only dimension that determines which loans are affected by the instrument. In particular, the treatment effect is unidentified for loans that would never be bought out following a rolling delinquency, independent of securitization timing and, hence, instrument intensity. Note that there is no equivalent group of always-takers, since treatment fundamentally depends on the timing of securitization—if some always-takers fail to securitize in time, they no longer function as always-takers in this research design. Therefore, the instrument identifies an effect for a relevant population that includes loans that would otherwise contribute no variation but for the timing of the policy. Under the assumption that time-to-securitization is random within a short window, this one-sided noncompliance allows the LATE to be interpreted as the average treatment effect on the treated (ATT).

7.1.2 Weak Instrument Test

Asymptotic properties of the two-stage least-squares (2SLS) estimator depend critically on the instrument's correlation with the endogenous variable. The bias of the estimated treatment effect is inversely related to the instrument's predictive power: a weaker first stage amplifies bias from even small violations of the exclusion restriction. Furthermore, the asymptotic variance of the 2SLS estimator is also inversely proportional to the correlation between the instrument and treatment. Hence, a weak relationship between the treatment and instrument will produce an inefficient estimate of the treatment effect, even if the estimate is consistent.

To test for weak instruments, I compute multivariate F-statistics, namely the Cragg-Donald Wald statistic, and its robust counterpart, the Kleibergen-Paap Wald test statistic (Bazzi and Clemens, 2013). Intuitively, these statistics test the null hypothesis that the first-stage coefficients are zero. Stock and Yogo (2005) provide critical values corresponding to a 10% rejection region for the null that 2SLS bias relative to OLS exceeds 10 percent, as well as critical values for the null that a two-tailed 5% t-test falsely rejects $\rho = 0$.

7.1.3 Overidentification Test

When an instrumental-variables model has more instruments than endogenous variables, the model is overidentified. In that case, there is no longer a unique estimator for the coefficient of interest. This is apparent in Figure 4 Panel A and Panel B. Taking the ratio of

estimates for each vintage month across the two plots shows that the estimated treatment effect for November is close to half that for October. Accounting for this heterogeneity, 2SLS yields a consistent estimator of a weighted average of individual treatment effects, where the weights represent each instrument's influence on the treatment. In the context of my research design, this implies that estimates for the two-month period prior to the policy should have the greatest weight, since loans originated during this period were most severely affected.

In addition to increasing precision, an overidentified model provides a means to test the exclusion restriction under certain assumptions. The Sargan–Hansen overidentification test assumes at least one instrument is valid and tests the null hypothesis that the remaining instruments are exogenous. Failure to reject the null, together with confidence that at least one instrument is valid, provides evidence that all instruments are valid.

Importantly, rejecting the null still permits at least one instrument to be valid, given the test's baseline assumption. In the context of my research design, that would most plausibly hold for the two-month period immediately preceding the policy, since buyout activity declined most sharply for loans originated in that window relative to other vintages. For this reason, I present estimates for both the overidentified model and the just-identified model.

7.2 Results

Table 6 presents second-stage estimates from the instrumental-variables estimation using foreclosure and cure as the dependent variables. Columns (1) to (4) report estimates using the sample of Ginnie Mae and GSE loans originated between July and November of 2001 and 2002, conditional on rolling delinquency and valued above par. The estimated coefficients measure the causal effect of buyout on foreclosure and cure. In Columns (1) and (3), I estimate an overidentified model, replacing $Z_{t\geq 10}$ with Z_t. In Columns (2) and (4), I estimate the just-identified model using $Z_{t\geq 10}$. These specifications exploit within-year–segment variation across months of origination. Note that first-stage and reduced-form estimates are reported in Sections 5 and 6.

In Column (1) of Table 6, I estimate Cragg–Donald and Kleibergen–Paap statistics exceeding 99, strongly rejecting the null that the set of instruments is weak. This is unsurprising given the results from Section 5, which show that vintage month strongly correlates with buyout activity, dropping sharply in the last months prior to the policy announcement. Given that these diagnostic statistics far exceed the critical values in Stock and Yogo (2005), the worst-case bias of 2SLS should be limited, making extreme outlier 2SLS estimates unlikely. I also report the p-value for the Sargan–Hansen test of overidentifying restrictions using foreclosure as the outcome of interest. Importantly, this test assumes that at least one

instrument is exogenous. While inherently untestable, the evidence presented in Section 5 strongly suggests that, at minimum, the two-month period preceding the policy announcement satisfies the exclusion restriction. Hence, assuming that this two-month period provides exogenous variation in buyout activity, I fail to reject the null that the additional instruments are exogenous at the 10% level. This finding supports the validity of the full set of instruments.

Column (1) of Table 6 presents estimates of the second-stage regression given by Equation (5) using the full set of triple interactions $Z_t \times Y02_t \times GNMA_i$. I find that buyout reduces foreclosure by 14.7 percentage points, statistically different from zero at the 5% level. This specification accounts for lower-order interactions, loan-level controls, and MSA–quarter–of–delinquency fixed effects. Here, I restrict identification to within-year–segment variation in foreclosure across vintage months. Under the assumptions outlined in Section 7.1, I identify the average treatment effect on the treated. Given that 2001 Ginnie Mae loans entered foreclosure at a rate of 19.5 percent after buyout, the estimate in Column (1) implies that loans experiencing a buyout would have, counterfactually, experienced a 75% increase in foreclosure.

Column (2) of Table 6 presents just-identified 2SLS estimates using first-stage regressions given by Equation (4) with the triple interaction $Z_{t\geq 10} \times Y02_t \times GNMA_i$. Rather than measuring variation across calendar months, the first-stage coefficient identifies the differential change in buyout activity in the last two months relative to the previous three months. Choosing $Z_{t\geq 10}$ for the just-identified 2SLS estimate rests on the assumption that loans issued in the two-month period immediately preceding the policy announcement would have experienced essentially equivalent outcomes to loans issued in the first three months, absent the Ginnie Mae policy shock. Indeed, the first stage suggests this is the case. As before, I refer to Section 5, where I present first-stage and reduced-form estimates reported in Column (5) of Table 3. In Column (2) of Table 6, I estimate Cragg–Donald and Kleibergen–Paap statistics exceeding 300, strongly rejecting the null hypothesis that the instrument is weak.

Turning to the second stage, the estimate in Column (2) indicates that buyout reduces foreclosure by 14.2 percentage points, statistically different from zero at the 5% level. This estimate is 0.5 percentage points smaller than the estimate in Column (1). The just-identified 2SLS estimator has the desirable property that its sampling distribution is centered at the population parameter; hence it is approximately median unbiased (Angrist and Krueger, 1999). The similarity in estimates across Columns (1) and (2) suggests that the bias of the overidentified 2SLS estimate is likely small relative to the population parameter.

Column (3) of Table 6 presents estimates of the second-stage regression given by Equation (5) using early cure as the outcome of interest. I find that buyout increases early cure by 15.5

percentage points, statistically different from zero at the 1% level. Given that, after buyout, 2001 Ginnie Mae loans exited delinquency at a rate of 45 percent, the estimate in Column (3) implies that loans experiencing a buyout would have, counterfactually, experienced a 30% reduction in delinquency exits. Column (3) also reports the p-value for the Sargan–Hansen test of overidentifying restrictions using early cure as the dependent variable. As before, assuming that at least one instrument is exogenous, I fail to reject the null that the additional instruments are exogenous at the 10% level. Once again, this finding supports the validity of the full set of instruments.

Column (4) of Table 6 presents just-identified 2SLS estimates using first-stage regressions given by Equation (4). Here, the excluded instrument is the triple interaction $Z_{t\geq 10} \times Y02_t \times GNMA_i$, an indicator equal to one if a loan sold to Ginnie Mae originates in October or November of 2002. As before, the first-stage estimates match those reported in Column (5) of Table 3. The estimate in Column (2) of Panel B in Table 6 indicates that buyout increases early cures by 14 percentage points, statistically different from zero at the 5% level. This estimate represents a 1.5 percentage point decline in magnitude relative to the overidentified model. Because the 2SLS estimator is a weighted average of individual treatment effects based on each instrument's influence, the overidentified estimate is naturally close to the just-identified estimate. Importantly, the estimated effect of buyout on cures is within 0.5 percentage points of the estimated effect of buyout on foreclosure. The similarity in magnitudes further reinforces the argument that buyouts reduce foreclosure by increasing early cures.

7.3 Heterogeneous Treatment Effects

In the previous section, I estimated the causal elasticity of buyout activity and loan performance. Under the assumption of one-sided noncompliance, the results indicate that buyouts reduce foreclosure for loans that would normally be selected into buyout. Given that endogenous regressions of buyout and foreclosure reveal a near-zero relationship in Table 2, I argue that issuers likely target loans with worse counterfactual performance for buyout. In other words, the evidence in the previous section suggests that issuers selectively buy out lower-quality loans. Indeed, a positive selection effect is consistent with the near-zero estimates in Table 2 given the negative treatment effect estimated in Table 6. While this interpretation explains selection into treatment, in this section, I explore heterogeneity in the treatment effect to better characterize the channel through which buyouts affect loan outcomes.

I begin by examining the extent to which financial constraints can explain the results in the previous section. Borrowers with relatively more liquid savings or stable lines of credit

may find it easier to prevent worsening delinquency after an initial missed payment. Thus, unconstrained borrowers may avoid foreclosure independent of selection into buyout. By contrast, I hypothesize that early intervention should benefit borrowers who are most at risk of worsening delinquency because of limited access to finance. If an issuer provides forbearance that reduces the repayment burden on a constrained borrower, that borrower may be incentivized to continue making future payments. For example, Melzer (2017) finds that homeowners at risk of default cut back mortgage principal payments due to limited incentives to maintain loan quality. Here, I directly test whether early cures can prevent worsening delinquency for these most at-risk borrowers.

To explore this hypothesis, I re-estimate my main IV specification on subsamples defined by proxy variables for financial constraints. The variables I use to construct these categorizations include the the percent of adults who (i) have less than a high-school education, (ii) are unemployed, and (iii) live below the poverty line. These factors proxy for difficulty accessing financing due to limited employment, income, or educational attainment. For example, borrowers in counties with above-median unemployment may find it difficult to refinance when interest rates decline (Defusco and Mondragon, 2020). I obtain county-level values for each threshold variable from the 2000 decennial census. Finally, I split the primary sample into loans from counties with above-median and below-median values of each threshold variable.

In Panel A of Figure 6, I report just-identified 2SLS estimates from subsamples defined by each measure of financial constraint. I follow the specification in Column (2) of Table 6, including all lower-order interactions, loan-level controls, and MSA–quarter–of–delinquency fixed effects. The plotted IV estimates measure the causal effect of buyout on foreclosure separately for constrained and unconstrained localities. Across the unemployment and education measures, borrowers in financially constrained localities experience larger treatment effects than those in unconstrained counties. Less-educated counties exhibit a treatment effect roughly three times larger than better-educated counties. Counties with higher unemployment exhibit a treatment effect about twice as large as counties with lower unemployment. Higher-poverty counties experience a 20 percent larger treatment effect than counties with lower poverty rates, although the estimate is statistically insignificant at the 5% level. Assuming these variables proxy for financial constraints, these results suggest that limited access to finance increases the benefit of buyout for long-term loan performance.

While financially constrained borrowers benefit most from buyout, issuers may still fail to provide effective foreclosure relief when financial slack is limited. For example, Aiello (2022) finds that financially constrained servicers undertake foreclosures and modifications that reduce value for both investors and borrowers. In the context of buyout, the most relevant constraint is the availability of collateral. This follows from the use of subordinate debt to

capitalize arrears and cure loans, as suggested by evidence in Section 6.3. I hypothesize that when the credit supply contracts or the underlying asset (the borrower's home) is over-levered, issuers have less financial slack to extend a second lien to reinstate the loan. Thus, buyout should benefit borrowers residing in localities with greater slack in the mortgage credit supply. If the credit supply is less constrained, issuers have greater flexibility to capitalize arrears into a subordinate loan to cure a delinquent mortgage. Because the subordinate loan is tied to collateral, an issuer's ability to cure via subordination depends on the availability and quality of the underlying collateral.

I test this hypothesis by re-estimating my main IV specification on subsamples defined by proxies for slack in the mortgage credit supply. As before, I construct restricted samples by first generating measures of financial slack. The variables I use are the county-level loan-denial rate and the county-level loan-to-income ratio of originated loans. The average denial rate measures the degree to which lenders restrict credit supply in a county relative to others. The average loan-to-income ratio measures the availability of collateral to secure a subordinate lien. I obtain these threshold variables from 2000 HMDA loan-application data. Finally, I split my primary sample into loans from counties with above-median and below-median values of each threshold variable.

In Panel B of Figure 6, I report just-identified 2SLS estimates from subsamples defined by each measure of financial slack. As before, I follow the specification in Column (2) of Table 6, exploiting within-MSA and within-delinquency-month variation to identify the effect of buyout on foreclosure for each subsample. In the first set of tests, I plot IV estimates that measure the causal effect of buyout on foreclosure across counties with above- and below-median slack in the mortgage credit supply. Across both threshold variables, the treatment effect is larger in counties where the mortgage credit supply is slack. In counties with below-median denial rates, loans experience a treatment effect of 20 percent, statistically significant at the 5% level. For above-median denial rates, the point estimate is 5 percent and statistically insignificant. Loans issued in counties with an average loan-to-income ratio below the median experience a treatment effect of 23 percent, statistically significant at the 1% level. In contrast, above-median loan-to-income counties exhibit a treatment effect of 5 percent, statistically insignificant. Ultimately, the close relationship between the salience of the buyout effect and collateral quality supports the hypothesis that issuers capitalize arrears into subordinate debt. Counties with slack in credit supply or in underlying collateral appear to benefit most from buyouts because issuers can deploy the loss-mitigation tools enabled by buyouts.

In order to distinguish between financial slack in the mortgage credit supply and borrower financial constraints, I repeat the exercise above using a measure of financial slack

that abstracts from mortgage credit in particular. I obtain the aggregate debt-to-income ratio for each county in my primary sample from the Enhanced Financial Accounts (EFA) data provided by the Federal Reserve. The EFA combines household debt data from the Equifax/Federal Reserve Bank of New York Consumer Credit Panel with income data from the Bureau of Labor Statistics (Ahn et al., 2018). I split my primary sample into loans from counties with above- and below-median values of this debt-to-income measure. Whereas aggregate loan-to-income signals slack available for mortgage credit in particular, aggregate debt-to-income is broader because it includes all consumer debt, such as auto and credit-card debt. Hence, debt-to-income functions as a signal of borrower financial constraints rather than slack for any single credit type. Given this context, the direction of the treatment effect is ambiguous.

In Panel B of Figure 6, I report just-identified 2SLS estimates from subsamples defined by aggregate debt-to-income. The plotted IV estimates measure the causal effect of buyout on foreclosure separately for loans issued in counties with above- and below-median debt-to-income. In contrast to the first two sample splits, borrowers in counties with below-median debt-to-income experience only a 4 percentage point larger treatment effect than loans issued in high debt-to-income counties. Neither estimate is statistically significant at the 5% level. These results support the earlier interpretation that financially constrained borrowers experience the largest reduction in foreclosure following buyout. They also differ from the first two sample splits in Panel B because aggregate debt-to-income captures broader financial constraints and not just mortgage-credit slack.

As a final test for heterogeneity, I explore the role of loan quality and payoff to the issuer. In particular, I test how past loan performance and the size of the interest-rate differential affect the quality of loan cure. I first decompose my primary sample into loans with above- and below-median interest-rate spreads. I calculate this differential by subtracting a loan's current interest rate from the par mortgage coupon at the time of delinquency. I then classify a loan as above median if, relative to other loans in its market segment and year of origination, the loan's differential is above the median. This classification indicates whether the issuer's payoff provides a strong or weak incentive to re-securitize. I hypothesize that loans with a strong incentive to re-securitize will perform better than loans with a weaker incentive. Prior literature shows that sellers of debt often signal loan quality to facilitate sale (Begley and Purnanandam, 2016). Creditors may increase effort to cure loans to signal loan quality ex post, building a reputation for high-quality cures in newer mortgages. Work by Hartman-Glaser (2017) and Adelino et al. (2017) argues that reputation is fundamental to MBS markets in particular.

Naturally, loans with the largest spreads will also tend to have the highest interest rates.

Therefore, my results may fail to distinguish issuer incentives to buy out a high-payoff loan from incentives to buy out a low-quality loan, despite the high payoff. To that end, I further split the sample by loan performance. I classify high-quality loans as those that were never delinquent before experiencing a rolling delinquency and that missed only one payment during the rolling-delinquency spell. I classify a loan as low-quality if the preceding criterion fails to hold.

In Panel C of Figure 6, I report just-identified 2SLS estimates using subsamples defined by loan quality and interest-rate spreads. I plot IV estimates that measure the causal effect of buyout on foreclosure for loans with strong and weak buyout incentives, separately. I find that low-quality loans, across both low and high interest-rate differentials, experience no treatment effect from buyout. The point estimate for low-quality loans with an above-median payoff is 2 percent and is statistically insignificant at the 5% level. In contrast, estimates for the sample of high-quality loans are statistically significant at the 5% level. High-quality loans with below-median payoff experience a treatment effect of 21 percent. High-quality loans with an above-median payoff experience a treatment effect twice as large, estimated to be 42 percent.

Overall, these estimates measure the causal effect of buyout on foreclosure across loans with above- and below-median interest-rate premiums and across high- and low-risk loans. Among loans with a high interest-rate premium, high-quality loans experience a twofold larger reduction in foreclosures relative to low-payoff loans. The estimate for low-quality, high-interest-rate loans is statistically insignificant and close to zero. When the interest-rate premium is below the median, the point estimates are similar across both loan types. These results imply that when issuer incentives are low, issuers weakly rely on quality. In contrast, when issuer incentives are strong, the best-performing loans ex-ante experience the largest reduction in foreclosures.

These results are consistent with the hypothesis that issuers expend greater effort to cure a loan when the payoff is high and risk is low. Such loans are the best candidates for re-securitization into a new MBS pool, given issuer reputation concerns and investor demand. When issuers' incentives do not align with re-securitization—for example, because the interest-rate spread is low—concerns about future reperformance are correspondingly less salient. This is reflected in the attenuation of quality differentials among low-spread loans.

8 Conclusion

This paper examines the effect of loan buyouts on loan performance. I exploit quasi-experimental variation in buyout propensity across vintage months preceding a suspension of rolling-delinquency buyouts. The policy announcement was unanticipated and differentially affected loans that could not securitize within an increasingly narrow window before the suspension took effect. Absorbing seasonal variation by market segment and year renders month of origination plausibly as-good-as randomly assigned, except through the policy change.

I estimate that high-exposure loans were 30 percentage points less likely to be bought out than low-exposure loans. In response to this decline in buyout activity, treated loans experienced a 4 percentage point increase in foreclosure starts and a commensurate decline in early cures. I find no other observable changes in loan terms, consistent with issuers capitalizing arrears into a subordinate lien. Using an instrumental-variables framework, I recover policy-relevant causal estimates of the effect of buyout on loan performance.

The results have implications for research and policy. First, preventing worsening delinquency and foreclosure remains a central policy concern since the financial crisis. My main result speaks directly to this issue: buyouts can incentivize reperformance, and timely intervention appears critical for reducing long-run foreclosure risk. Second, the efficacy of the buyout option has been debated in the wake of the COVID pandemic. For example, Ginnie Mae recently introduced stricter criteria for resecuritizing buyout loans. This paper highlights potential benefits of an agency buyout option: improved loan quality for investors and, more directly, foreclosure prevention for borrowers. These effects are consequential beyond Ginnie Mae loans given that $8.5 trillion of agency debt carries buyout options on seriously delinquent mortgages. Third, distressed-debt financing outside the mortgage market—with a face value on the order of $1 trillion—is an important component of asset markets. Although my setting focuses on agency debt, the dynamics I identify have broader implications for creditors in other lending markets.

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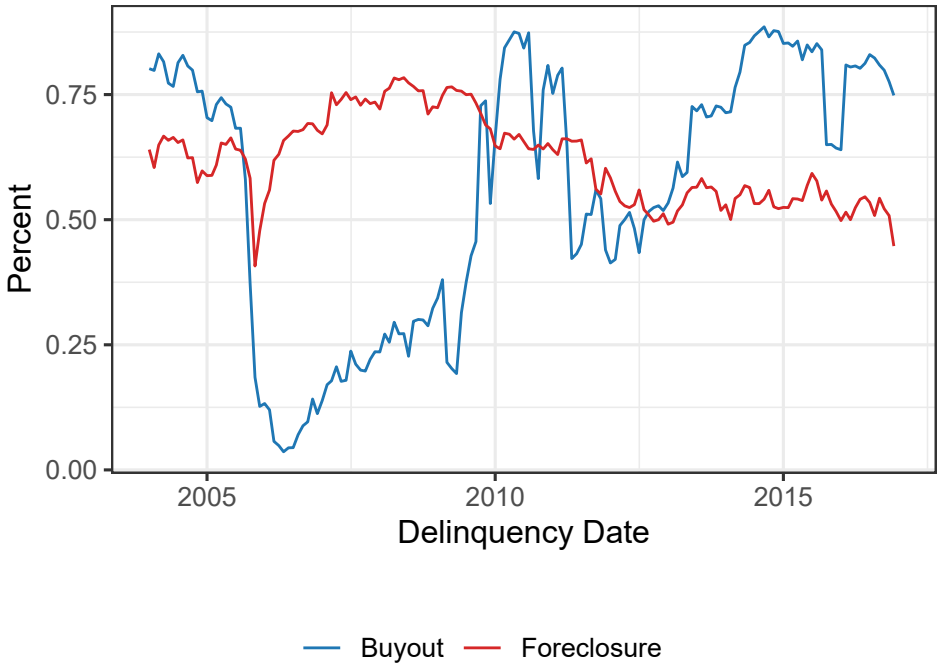
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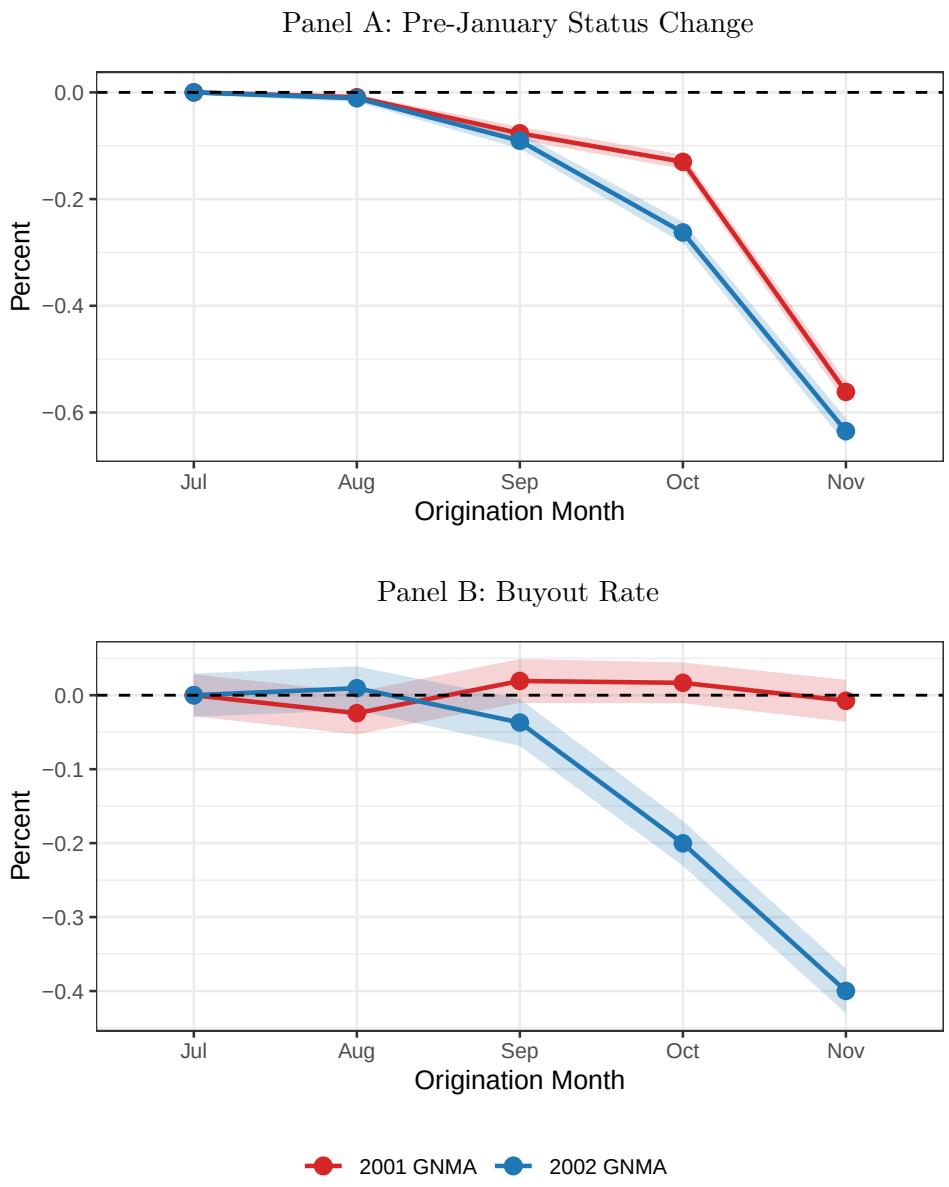
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Figure 1. Buyout and Foreclosure Propensity



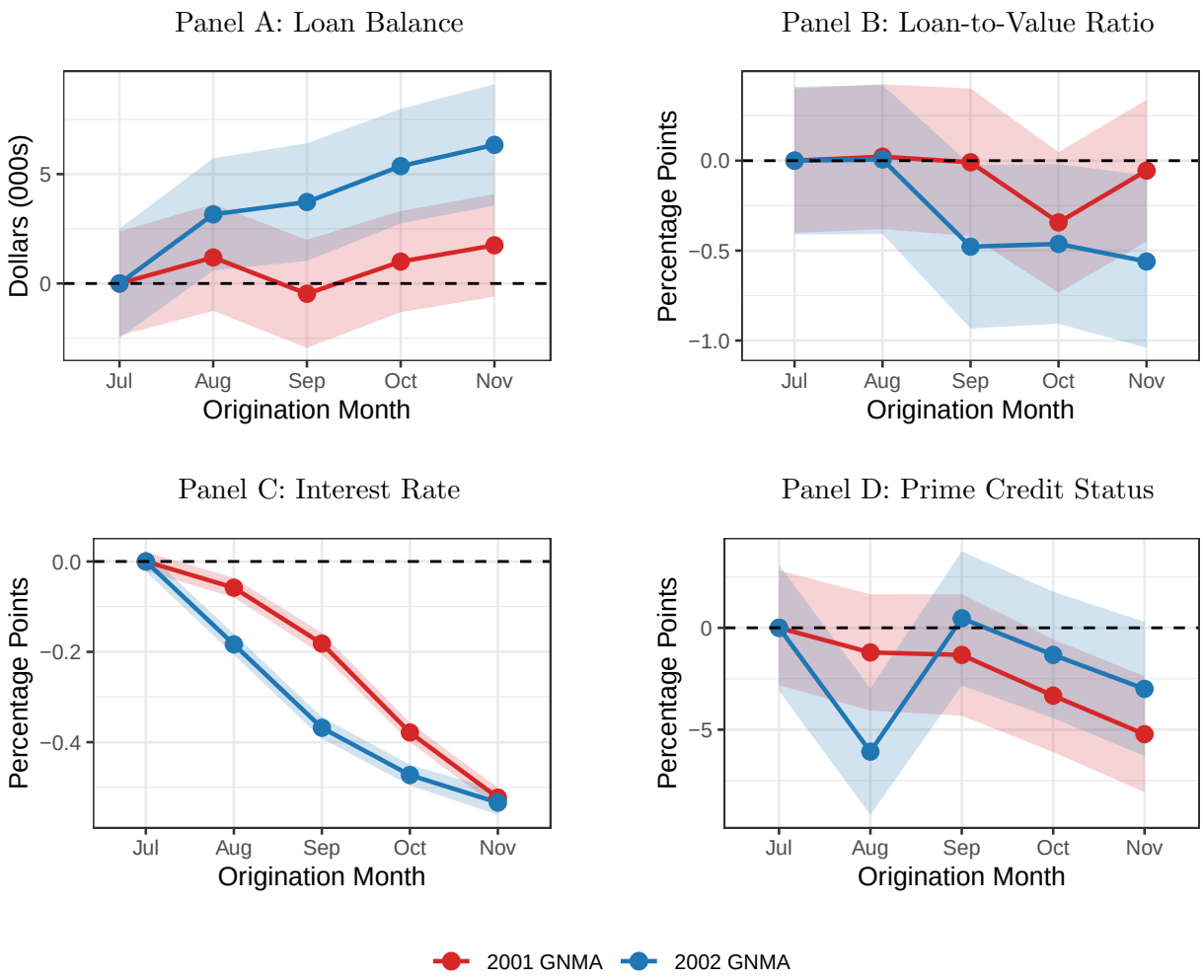
Note: This figure plots the buyout and foreclosure rate for loans experiencing a 90-day delinquency from the Ginnie Mae MBS pool. The buyout measure is constructed from CoreLogic data as a change in investor status within two months of a 90-day delinquency. Data covers loans experiencing 90-day delinquency between 2004 and 2016, with origination dates spanning 2004 to 2014. The sample is restricted to loan outcomes observed within the first three years after origination.

Figure 2. Investor Status Change and Buyout Across Origination Month



Note: This figure plots baseline-adjusted means for Ginnie Mae loans by origination month and year. Panel A shows the propensity for a loan to change its investor code before January. Panel B shows the propensity for a loan to be bought out of the Ginnie Mae MBS pool. For each year the value plotted for a given month equals the given month's mean minus the July average. Shaded bands show 95% confidence intervals. Standard errors are computed as the standard error of the difference of means. Data is collected from CoreLogic.

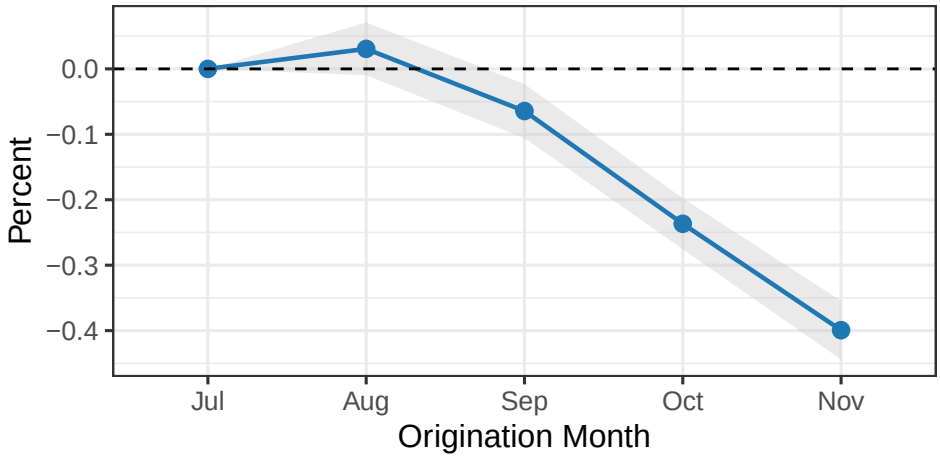
Figure 3. Loan Characteristics Across Origination Month and Year



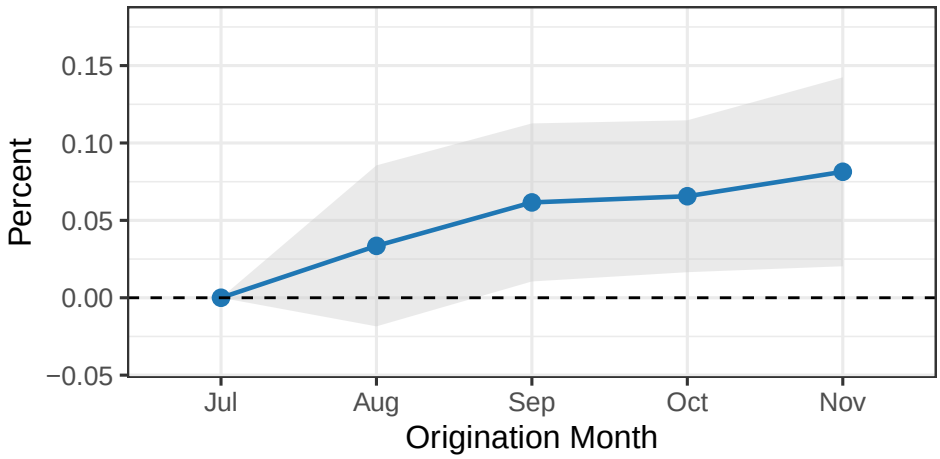
Note: This figure plots baseline-adjusted mean of loan characteristics for Ginnie Mae loans originated in July to November of 2001 and 2002. Panel A presents loan balance, Panel B presents loan-to-value at origination, Panel C presents initial interest rate, and Panel D presents prime borrower share. For each year the value plotted for a given month equals the given month's mean minus the July average. Shaded bands show 95% confidence intervals. Standard errors are computed as the standard error of the difference of means. Data is collected from CoreLogic.

Figure 4. Effect of the GNMA Policy on Buyout and Foreclosure Across Origination

Panel A: Buyout

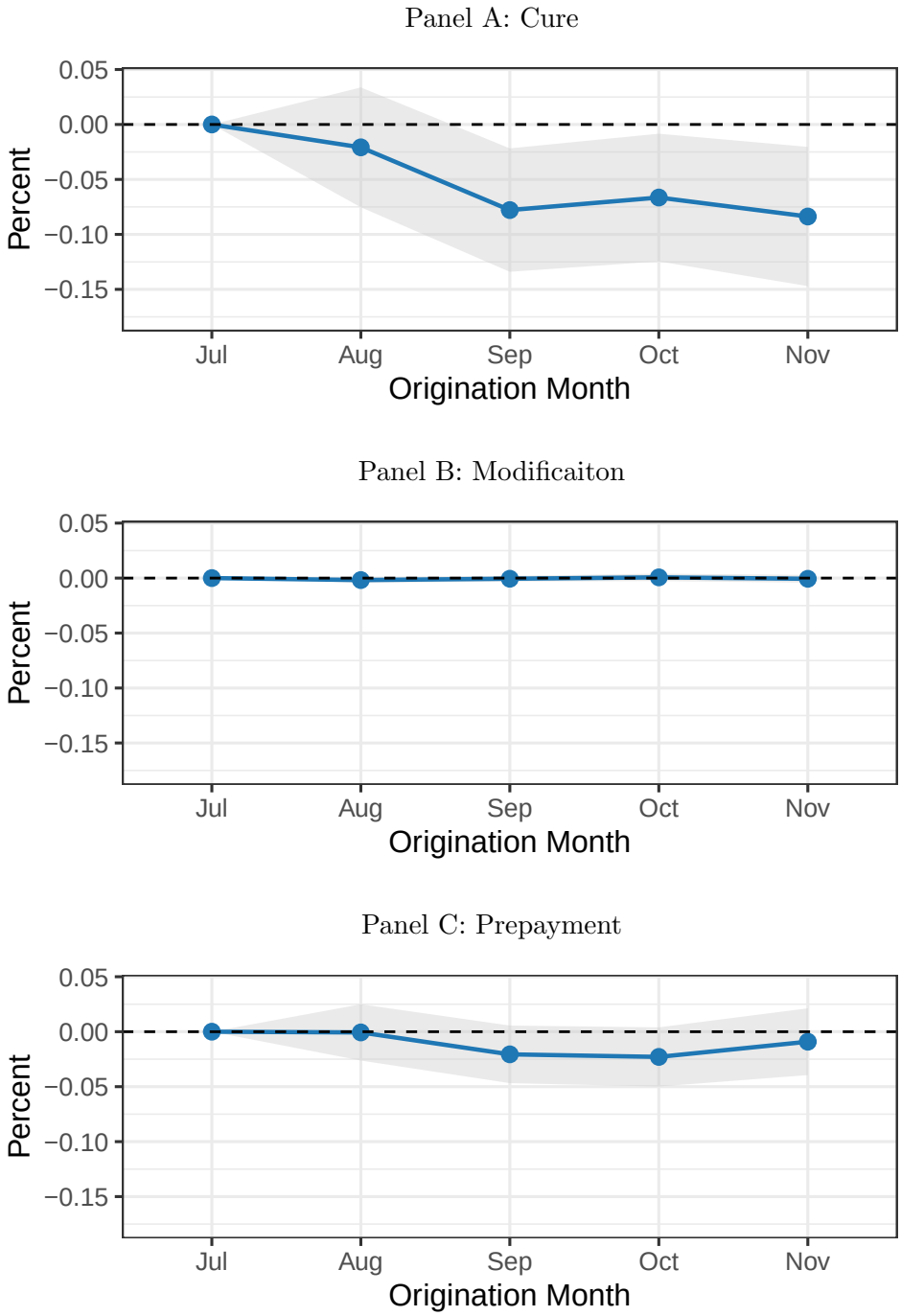


Panel B: Foreclosure



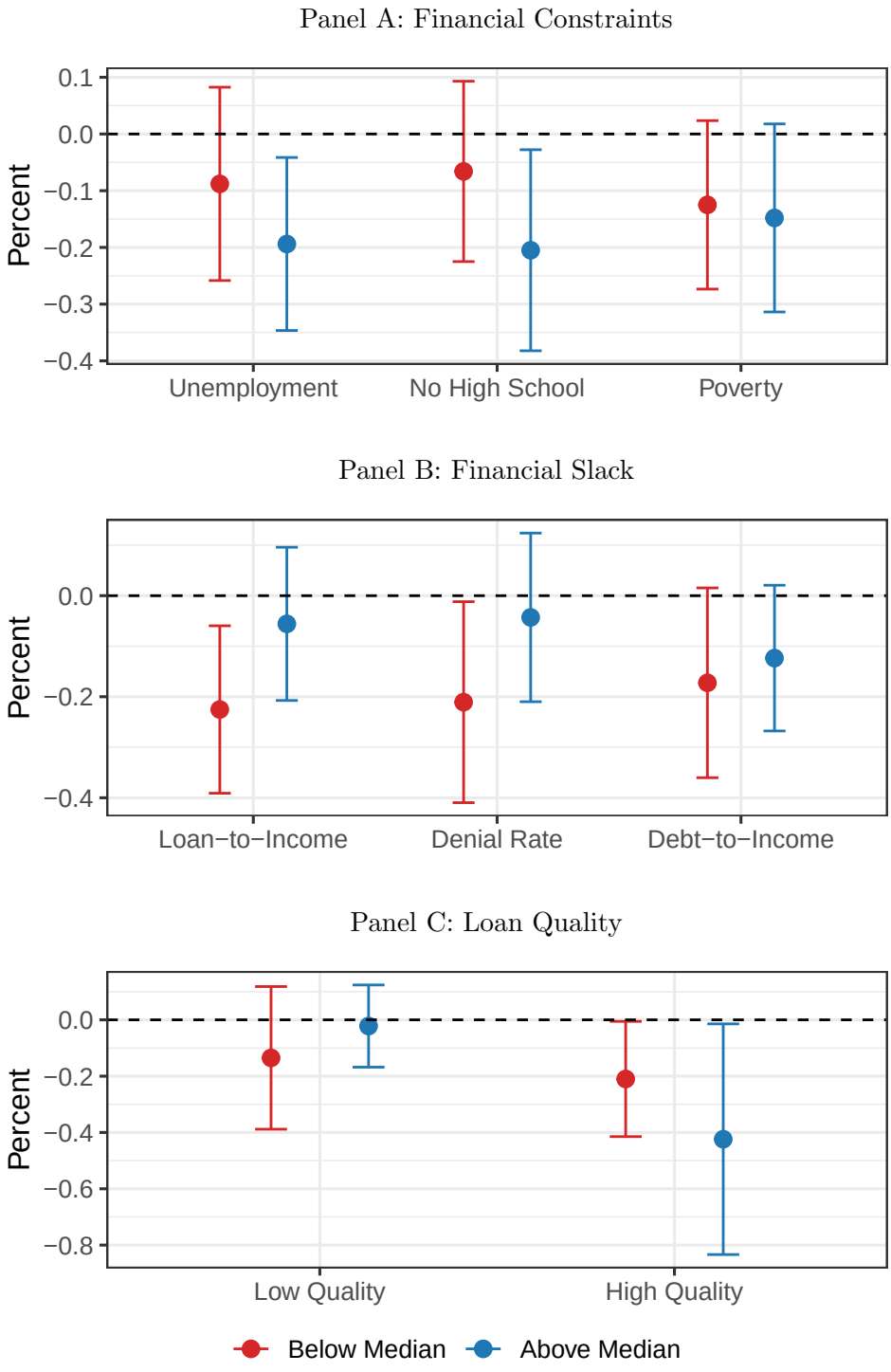
Note: This figure plots dynamic triple-difference estimates from the fully specified model in Column (5) of Table 4. Panel A reports estimates with the dependent variable equal to cure, Panel B to modification, and Panel C to prepayment. Each dot is the estimated coefficient on $Z_t \times \text{Y02} \times \text{GNMA}$, with July omitted. The x–axis lists origination months July–November. Shaded bands denote 95% confidence intervals (clustered by MSA). The specification includes the full set of loan–level controls and fixed effects, and absorbs all lower–order interactions of origination month, year, and GNMA status. Data is collected from CoreLogic and covers GNMA and GSE loans originated between July and November of 2001 and 2002.

Figure 5. Effect of the GNMA Policy on Short-Run Outcomes Across Origination



Note: This figure plots dynamic triple-difference estimates from the fully specified model in Column (5) of Table 4. Panel A reports estimates with the dependent variable equal to cure, Panel B to modification, and Panel C to prepayment. Each dot is the estimated coefficient on $Z_t \times \text{Y02} \times \text{GNMA}$, with July omitted. The x–axis lists origination months July–November. Shaded bands denote 95% confidence intervals (clustered by MSA). The specification includes the full set of loan–level controls and fixed effects, and absorbs all lower–order interactions of origination month, year, and GNMA status.

Figure 6. Effect of Buyout on Foreclosure: Heterogeneity



Note: This figure plots estimates from a 2SLS specification corresponding to Column (5) in Table 6. Panel A measures the effect across measures of financial constraint. Panel B measures the effect across measures of financial slack. Panel C measures the effect across measures of loan quality. Each red dot corresponds to the effect estimated over a subsample of loans with above-median characteristics, each blue dot corresponds to the below-median subsample, and vertical error bars denoting 95% confidence intervals (clustered by MSA).

Table 1. Summary Statistics

	All Loans		GNMA		GSE	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
Panel A: Treatment Year 2002						
Initial Rate	6.68	0.53	6.68	0.43	6.68	0.60
Original LTV	87.53	13.44	95.76	7.08	81.08	13.72
Balance (000s)	120.63	54.35	111.94	41.74	127.44	61.61
15-Year Term	0.06	0.24	0.02	0.13	0.10	0.29
20-Year Term	0.04	0.18	0.03	0.16	0.04	0.20
Prime Borrower	0.58	0.49	0.43	0.49	0.69	0.46
Refinance Loan	0.42	0.49	0.25	0.44	0.55	0.50
Primary Occupancy	0.87	0.34	0.83	0.38	0.90	0.30
Single-Family	0.83	0.37	0.83	0.38	0.84	0.37
Low/No Doc	0.32	0.46	0.28	0.45	0.34	0.47
Buyout	0.25	0.43	0.55	0.50	0.01	0.12
Foreclosure	0.24	0.42	0.25	0.43	0.23	0.42
Cure	0.50	0.50	0.46	0.50	0.53	0.50
Modification	0.00	0.02	0.00	0.03	0.00	0.02
Prepayment	0.05	0.22	0.04	0.20	0.06	0.24
Observations	20,338		8,929		11,409	
Panel B: Control Year 2001						
Initial Rate	7.18	0.52	7.19	0.44	7.16	0.59
Original LTV	87.96	12.74	95.51	6.81	80.54	12.85
Balance (000s)	115.63	50.68	108.41	40.92	122.72	57.84
15-Year Term	0.07	0.25	0.02	0.12	0.12	0.32
20-Year Term	0.03	0.18	0.02	0.15	0.04	0.20
Prime Borrower	0.67	0.47	0.59	0.49	0.74	0.44
Refinance Loan	0.41	0.49	0.24	0.43	0.59	0.49
Primary Occupancy	0.91	0.28	0.93	0.26	0.90	0.31
Single-Family	0.86	0.34	0.87	0.34	0.86	0.35
Low/No Doc	0.21	0.41	0.15	0.36	0.27	0.44
Buyout	0.30	0.46	0.60	0.49	0.00	0.04
Foreclosure	0.22	0.42	0.23	0.42	0.22	0.41
Cure	0.50	0.50	0.47	0.50	0.53	0.50
Modification	0.00	0.02	0.00	0.03	0.00	0.01
Prepayment	0.06	0.23	0.05	0.21	0.06	0.24
Observations	22,703		11,249		11,454	

Note: This table reports mean and standard deviation values of loan characteristics and outcomes for GNMA and GSE loans, conditional on rolling delinquency and above-par status. Panel A presents estimates for loans originated in 2002 and Panel B presents estimates for loans originated in 2001.

Table 2. Effect of Buyout on Foreclosure: Baseline OLS

	Foreclosure				
	(1)	(2)	(3)	(4)	(5)
Buyout	-0.015**	-0.012	-0.011	0.024***	0.021**
	(0.007)	(0.007)	(0.008)	(0.008)	(0.011)
Origination FE	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes
MSA FE			Yes		Yes
Quarter FE				Yes	Yes
MSA-Quarter FE					Yes
Observations	11,298	10,997	10,997	10,997	10,997

Note: This table reports OLS estimates of the effect of foreclosure on buyout. Observations are at the loan level. For Columns 1 to 5, the outcome equals one if a loan enters foreclosure within 24 months after a rolling delinquency. The dependent variable Buyout equals one if a loan is bought out within two months after a rolling delinquency. Loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, prime status, documentation type, loan purpose, and property type. County-level controls at the time of delinquency include the unemployment rate, log house price index, log GDP, log population, and log income. All specifications include origination-month fixed effects. Standard errors (in parentheses) are clustered at the MSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers GNMA loans originated between July and November of 2001 with outcomes observed through July 2007.

Table 3. Effect of the GNMA Policy on Buyout and Foreclosure: Triple Difference

	(1)	(2)	(3)	(4)	(5)
Panel A: Buyout					
$Z_{t\geq 10} \times$ Y02 $\times$ GNMA	-0.305***	-0.300***	-0.299***	-0.304***	-0.301***
	(0.015)	(0.016)	(0.016)	(0.015)	(0.016)
Observations	43,219	42,203	42,203	42,203	42,203
Panel B: Foreclosure					
$Z_{t\geq 10} \times$ Y02 $\times$ GNMA	0.035**	0.042**	0.040**	0.045***	0.043**
	(0.018)	(0.017)	(0.017)	(0.017)	(0.018)
Observations	43,219	42,203	42,203	42,203	42,203
Interactions	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes
MSA FE			Yes		Yes
Quarter FE				Yes	Yes
MSA-Quarter FE					Yes

Note: This table reports OLS estimates of the effect of the 2002 Ginnie Mae memorandum on loan outcomes. Observations are at the loan level. In Panel A, the outcome equals one if a loan is bought out within two months after a rolling delinquency. In Panel B, the outcome equals one if a loan enters foreclosure within 24 months after a rolling delinquency. The indicator GNMA equals one for loans sold to Ginnie Mae within six months of origination. The indicator Y02 equals one for loans originated in 2002. The indicator $Z_{t\geq 10}$ equals one for loans originated in the calendar months October or November. Loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, prime status, documentation type, loan purpose, and property type. County-level controls at the time of delinquency include the unemployment rate, log house price index, log GDP, log population, and log income. All specifications absorb lower-order interactions of origination month, year, and GNMA status. Standard errors (in parentheses) are clustered at the MSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between July and November of 2001 and 2002 sold to Ginnie Mae or the GSEs and delinquency observed between the time of origination and July 2007.

Table 4. Effect of the GNMA Policy on Short-Run Loan Outcomes: Triple Difference

	(1)	(2)	(3)	(4)	(5)
Panel A: Cure					
$Z_{t\geq 10} \times$ Y02 $\times$ GNMA	-0.044**	-0.047**	-0.050**	-0.047**	-0.042**
	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)
Observations	43,219	42,203	42,203	42,203	42,203
Panel B: Modification					
$Z_{t\geq 10} \times$ Y02 $\times$ GNMA	0.001	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	43,219	42,203	42,203	42,203	42,203
Panel C: Prepayment					
$Z_{t\geq 10} \times$ Y02 $\times$ GNMA	-0.003	-0.005	-0.006	-0.006	-0.011
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Observations	43,219	42,203	42,203	42,203	42,203
Interactions	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes
MSA FE			Yes		Yes
Quarter FE				Yes	Yes
MSA-Quarter FE					Yes

Note: This table reports OLS estimates of the effect of the 2002 Ginnie Mae memorandum on loan outcomes. Observations are at the loan level. In Panel A, the outcome equals one if a loan exits delinquency within three months after a rolling delinquency. In Panel B, the outcome equals one if a loan experiences a modification within three months after a rolling delinquency. In Panel C, the outcome equals one if a loan experiences a prepayment within 24 months after a rolling delinquency. The indicator GNMA equals one for loans sold to Ginnie Mae within six months of origination. The indicator Y02 equals one for loans originated in 2002. The indicator $Z_{t\geq 10}$ equals one for loans originated in the calendar months October or November. Loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, prime status, documentation type, loan purpose, and property type. County-level controls at the time of delinquency include the unemployment rate, log house price index, log GDP, log population, and log income. All specifications absorb lower-order interactions of origination month, year, and GNMA status. Standard errors (in parentheses) are clustered at the MSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between July and November of 2001 and 2002 sold to Ginnie Mae or the GSEs and delinquency observed between the time of origination and July 2007.

Table 5. Effect of the GNMA Policy on Medium-Run Outcomes: Triple Difference

	90d DLQ	Buyout	Cure	Modification	Prepayment
	(1)	(2)	(3)	(4)	(5)
$Z_{t\geq 10} \times$ Y02 $\times$ GNMA	0.056***	-0.263***	0.004	0.016	0.000
	(0.016)	(0.013)	(0.015)	(0.017)	(0.003)
Interactions	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
MSA-Quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	42,203	42,203	42,203	42,203	42,203

Note: This table reports OLS estimates of the effect of the 2002 Ginnie Mae memorandum on loan outcomes. Observations are at the loan level. For Columns 1 to 5, the outcome takes a value of one if, within a 15-month window following a rolling delinquency, a loan experiences a 90-day delinquency, buyout, cure, modification, or prepayment, respectively. The indicator GNMA equals one for loans sold to Ginnie Mae within six months of origination. The indicator Y02 equals one for loans originated in 2002. The indicator $Z_{t\geq 10}$ equals one for loans originated in the calendar months October or November. Loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, prime status, documentation type, loan purpose, and property type. County-level controls at the time of delinquency include the unemployment rate, log house price index, log GDP, log population, and log income. All specifications absorb lower-order interactions of origination month, year, and GNMA status. Standard errors (in parentheses) are clustered at the MSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between July and November of 2001 and 2002 sold to Ginnie Mae or the GSEs and delinquency observed between the time of origination and July 2007.

Table 6. Effect of the GNMA Policy on Foreclosure and Cure: Instrumental Variables

	Foreclosure		Cure	
	(1)	(2)	(3)	(4)
Buyout (2SLS)	-0.147**	-0.142**	0.155***	0.140**
	(0.058)	(0.061)	(0.058)	(0.069)
Interactions	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
MSA-Quarter FE	Yes	Yes	Yes	Yes
Observations	42,203	42,203	42,203	42,203
Excluded	$Z_t \cdot$Y02$\cdot$GNMA	$Z_{t\geq 10} \cdot$Y02$\cdot$GNMA	$Z_t \cdot$Y02$\cdot$GNMA	$Z_{t\geq 10} \cdot$Y02$\cdot$GNMA
Cragg-Donald	154.54	528.38	154.54	528.38
Kleibergen-Paap	99.223	355.02	99.223	355.02
Sargan-Hansen	0.16680		0.20331	

Note: This table reports instrumental variables (two-stage least squares) estimates of the effect of the 2002 Ginnie Mae memorandum on loan outcomes. Observations are at the loan level. In Columns 1 to 2, the outcome equals one if a loan enters foreclosure within 24 months after a rolling delinquency. In Columns 3 to 4, the outcome equals one if a loan exits delinquency (cure) within three months after a rolling delinquency. The endogenous regressor is Buyout. The excluded instrument in Columns 1 and 3 is the triple interaction $Z_t \times$ Y02 $\times$ GNMA; in Columns 2 and 4 it is $Z_{t\geq 10} \times$ Y02 $\times$ GNMA. All lower-order interactions of origination month, year, and GNMA status enter both stages as controls; only the triple interaction is excluded from the second stage. Loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, prime status, documentation type, loan purpose, and property type. County-level controls at the time of delinquency include the unemployment rate, log house price index, log GDP, log population, and log income. First-stage strength and overidentification are summarized by the Cragg–Donald and Kleibergen–Paap statistics and the Sargan–Hansen p-values. Standard errors (in parentheses) are clustered at the MSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data cover loans originated between July and November of 2001 and 2002 sold to Ginnie Mae or the GSEs, with outcomes observed through July 2007.