

Loan Buyouts and Foreclosure: Evidence from Ginnie Mae*

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Abstract

Agency MBS issuers can repurchase loans from securitization pools at par. I develop a framework where issuers pursue loss mitigation only after they own the loan outright. I study how delays in buyout eligibility can deteriorate long-term loan quality by exploiting Ginnie Mae’s suspension of an early buyout option for FHA-insured mortgages. I find that earlier buyout eligibility reduces foreclosure by 40 percent and increases cures by 50 percent. I present evidence consistent with issuers curing loans through forbearance rather than modifications. For this margin, delaying eligibility postpones loss mitigation and leads to irreversible damage to long-term loan quality.

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1 Introduction

A broad literature on securitization studies how misaligned incentives limit the choice of loss mitigation offered to delinquent borrowers ([Keys et al., 2010](#); [Adelino et al., 2013](#); [Kruger, 2018](#); [Aiello, 2022](#)). The standard framing suggests that intermediaries, such as issuers and servicers, face certain frictions that limit their ability to help borrowers exit default. This paper argues that a first-order concern for issuers, prior to loss mitigation, is whether to acquire ownership of the loan outright. Upon a borrower’s delinquency, issuers may exercise an option to buy out the loan from the security vehicle by prepaying the loan’s outstanding balance at par. This option, known as a loan buyout, is embedded within approximately \$9 trillion of agency mortgage-backed securities, and to date there is little to no research on this margin.¹ This is the case even though buyouts were as prevalent as foreclosures throughout the financial crisis, as shown in [Figure 1](#).

Buying out a loan is a distinct decision from whether and how to provide loss mitigation. Upon the purchase of a loan out of its MBS pool, the issuer enjoys the benefits of ownership, most importantly capturing the spread between the loan’s original coupon and the prevailing market rate at the time of delinquency. Downstream from this choice of ownership, the issuer must then decide what form of loss mitigation to provide, which may include modifications or forbearance. However, the issuer may also choose to capture the spread indefinitely while leaving the borrower’s condition unchanged. This buyout framework is unique relative to standard models of renegotiation, which assume that intervention is tied to loss mitigation. Instead, issuers can be highly active in the secondary market by buying out delinquent loans from MBS pools, only to forgo loss mitigation entirely or proceed to foreclosure.

Under this framework, studying the role of securitization introduces novel empirical challenges. Agency MBS contracts bar issuers from executing buyouts until the borrower enters 90-day delinquency. Prior to this, the issuer cannot change the terms of the mortgage but

¹A notable exception is [Bandyopadhyay et al. \(2025\)](#), who document that issuers repurchase loans with relatively higher coupon spreads.

may offer forbearance as an in-pool alternative. Critically, for some loans, in-pool loss mitigation may not be privately worthwhile despite being optimal when the loan is owned outright. I explore this margin by measuring the effect of arbitrary delays in buyout eligibility on long-term loan quality. Critically, this margin is distinct from the legal and financial constraints traditionally emphasized in the securitization literature.

Empirically studying the effect of loan buyout on loan performance is difficult because of asymmetric information.² For the econometrician, a primary challenge is therefore disentangling the effect of buyout timing from the effect of underlying loan quality. If MBS issuers repurchase only low-quality loans for reputational reasons, the estimated relationship between buyout and loan performance will be biased downward. Alternatively, if issuers target loans with better long-run performance, 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 90-day delinquency). In November 2002, Ginnie Mae 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. Failing to securitize in time meant that late-originated loans lost eligibility for a rolling delinquency buyout while retaining the option at 90-day delinquency. I exploit this variation in eligibility by implementing a triple-difference strategy that compares outcomes for Ginnie Mae (treated) and GSE (control) loans across calendar month and calendar year of origination.

²A 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. Naturally, information frictions intensify when a borrower defaults.

Identification requires that the month of origination affect loan performance only through buyout eligibility timing. 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. The suspension was therefore 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 net 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.

The design aims to identify three distinct estimands: the effect of eligibility on early buyouts (the first stage), the effect of eligibility on loan outcomes (the reduced form), and their ratio (the instrumental-variables estimate), which captures the effect of an early buyout for loans whose buyout timing responded to the suspension. Figure [IA.13](#) in the Internet Appendix maps each onto the empirical framework.

I find that high-exposure loans experienced a 30 percentage point decline in early buyouts following an initial rolling delinquency. I identify buyouts in the data using a novel method that measures a change in investor status within two months of a qualifying delinquency. No public, market-wide, loan-level data directly identify Ginnie Mae buyouts before October 2009. To the best of my knowledge, this is the first market-wide loan-level measure of Ginnie Mae buyouts spanning the early 2000s through the financial crisis.³ Turning to long-run outcomes, I find that treated loans experienced a 4 percentage point increase in foreclosure starts within two years. 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. The result implies that a margin of loans exists for which earlier buyout eligibility

³Two senior executives at large residential real estate investment management companies who were active in the market during this period confirmed that no aggregated private measure existed.

was critical to preventing long-term deterioration in loan performance.

Exploring the mechanism, I find that the suspension of the buyout option reduces early cures by four percentage points. I define early cures as a loan becoming current within three months of a rolling delinquency, capturing whether loans exit delinquency in the window when buyout would counterfactually have been available. The decline in early cures is larger for later origination cohorts and broadly 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 capitalizing arrears into subordinate liens, effectively a reduction in forbearance.⁴ Absent the option to buy out loans, issuers may lack the financial incentive to pursue forbearance, producing a decline in loan cures. This is a striking result in that loss mitigation appears tied to the private benefits of ownership, with potentially lasting consequences for loan performance.

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-run differential should attenuate over this longer horizon as some loans regain buyout eligibility. Consistent with this prediction, the estimated effect on cures attenuates and is statistically indistinguishable from zero over the 15-month window. Together with the increase in foreclosure, this pattern suggests that longer-run loan performance deteriorates even as the estimated difference in cure rates diminishes.

I conclude by employing an instrumental-variables 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 per-

⁴FHA-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 will reimburse the issuer for any funds advanced, which will subsequently reinstate the loan as current. The arrears will then be capitalized into a zero-interest junior lien.

formance 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 timing, I estimate that an early buyout reduces the probability of foreclosure by at least 13.9 percentage points and increases early cures by at least 13.7 percentage points. Under the preferred specification, this implies that loan buyouts reduce foreclosure by about 40 percent and increase loan cures by about 50 percent.

Related Literature This paper bridges two broad research areas in corporate finance. First, I contribute to the literature on loss mitigation subject to securitization frictions. [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 to the financial crisis, there was little difference in renegotiation. The authors propose 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 \(2023\)](#), [Gabriel et al. \(2020\)](#), and [Collins and Urban \(2018\)](#) study the effect of foreclosure costs on foreclosures, renegotiations, and borrower repayment. This paper extends previous work by introducing a novel framework to understand loss mitigation as one of several downstream choices following the first-order decision of whether to take ownership. Instead of legal or financial constraints limiting creditor behavior, I find that issuers may choose to forgo in-pool loss mitigation absent the gains from ownership.

This paper thus connects the renegotiation literature to the literature on loan sales. Previous work has studied the role of asymmetric information in loan pricing and performance ([An et al., 2011](#); [Jiang et al., 2013](#); [Adelino et al., 2019](#)). Loan sales that involve issuer equity perform better ex post, precisely for those loans that are most opaque ([Begley and Purnanandam, 2016](#); [Ashcraft et al., 2019](#)). Finally, reputation effects ([Drucker and Puri,](#)

2008; Hartman-Glaser, 2017) and liquidity shortfalls (Irani and Meisenzahl, 2017) can also influence loan sale dynamics. While previous work has focused on sales at the time of origination, this paper instead focuses on loan purchase following delinquency. The framework in this paper further ties the choice of loan purchase to the gain from ownership.⁵

2 Institutional Background

2.1 The Government National Mortgage Association

Established in 1968 by the Department of Housing and Urban Development (HUD), the Government National Mortgage Association (GNMA or Ginnie Mae) facilitates a secondary market for securitization of loans issued to high-LTV borrowers. Ginnie Mae serves as a platform for Ginnie Mae-certified issuers to securitize loans insured by 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). Primarily, issuers are responsible for forwarding payments to investors, and Ginnie Mae guarantees the full and timely payment of principal and interest in case of issuer default. In case of borrower default, one of the four insuring agencies will compensate the issuer. The Ginnie Mae market is unique in that Ginnie Mae does not purchase loans from the issuers directly. Instead, it offers insurance and a securitization platform while issuers continue to act as custodians over timely payment to investors. In contrast, the Government-Sponsored Enterprises (GSEs), namely Fannie Mae and Freddie Mac, purchase the loans outright from lenders and issue their own MBS.

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

⁵Outside of the broad literature on securitization frictions, renegotiation, and loan sales, this paper relates to the growing literature on Ginnie Mae MBS and the FHA mortgage market. The Ginnie Mae market has evolved significantly over the past two decades (Kim et al., 2018; Adelino et al., 2026), enjoyed favorable regulatory status (Gete and Reher, 2020), and proven sensitive to policy changes (Bhutta and Ringo, 2021).

(serious delinquency). In exercising this option, the issuer buys the loan out of the pool at par, prepaying the outstanding principal balance to investors. Issuers choose to exercise this “buyout option” for a variety of reasons. First, an issuer cannot legally modify the terms of a mortgage unless the loan is outside of the MBS pool. Owning the loan outright therefore allows the issuer to provide loss mitigation in the form of changes to the interest rate or loan balance. Second, once a loan is bought out, the issuer is no longer liable to continue advancing high coupons to MBS investors on behalf of a delinquent borrower. Finally, an issuer may choose to purchase a loan out of the MBS pool to ease regulatory constraints. Ginnie Mae requires that an issuer’s delinquencies fall below defined thresholds, forcing issuers to buy out loans to maintain their standing in the Ginnie Mae market.

Table [IA.7](#) in the Internet Appendix summarizes these responsibilities and incentives across institutions. It distinguishes between issuers, servicers, and subservicers, and shows how their roles vary across types of MBS and stages in the loan’s life cycle. For Ginnie Mae MBS, the issuer retains responsibility for servicing the underlying mortgages, even when servicing tasks are delegated, and exercises the buyout option to place a loan on its own balance sheet. In contrast, the GSEs repurchase loans from MBS pools onto their own balance sheets rather than those of third-party servicers.

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 example, a borrower might initially miss a single month’s payment, but continue to make timely payments for the subsequent three months. If the initial missed payment is not repaid, then the loan enters rolling delinquency and becomes eligible for a loan buyout.

Buyouts operate like other forms of prepayment insofar as they accelerate payoffs at par and thus erode the MBS bond’s value. 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 four-month period, implying substantially lower risk of continued deterioration. Consequently, the market-wide benefits of permitting buyouts of rolling delinquencies are ambiguous.

2.3 Ginnie Mae’s Buyout Suspension

As mentioned in the previous subsection, prior to 2003, Ginnie Mae permitted issuers to buy out loans experiencing four periods of an uncured delinquency. However, given concerns about prepayment risk, investors urged Ginnie Mae to eliminate this 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. Usually, Ginnie Mae collaborates with a variety of stakeholders over the key features of a policy change. This may include the Mortgage Bankers Association (MBA), the broader issuer community, and MBS investors. A policy change of major importance would be anticipated by at least six months in order to provide all participants ample time to adjust to a new setting and prevent operational failure. The 2002 memorandum, however, was a stark departure from this practice.

A senior Ginnie Mae executive interviewed for this paper, who was centrally involved in developing the policy, described the memorandum as an announcement to “stop immediately.” In private conversation, the executive stated that, at most, Ginnie Mae inquired about the value of the buyout option with the MBA, with no revelation of any intention to suspend the policy. Ultimately, the policy announcement was intended to prevent anticipation on the part of the very issuers it was aimed at.

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, noting that early Ginnie Mae audits uncovered no abuse of the rolling delinquency provision.⁶ In addition, the Ginnie Mae executive confirmed that the policy change was unrelated to underlying loan performance, such as delinquency or foreclosure rates. 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.

3 Conceptual Framework

In the preceding section, I outlined the nature of Ginnie Mae’s buyout option and policy change. In this section, I present a stylized model of loan buyouts to better understand how a buyout suspension can help identify the effect of delays in intervention on loan performance. I treat the issuer and its servicing operation as a single decision maker, abstracting from incentive conflicts arising from delegated servicing.

3.1 A Simple Model of Loan Buyouts

The following builds on the intermediation framework of [Fuster et al. \(2024\)](#) and the self-cure framework of [Adelino et al. \(2013\)](#). Consider a simple model in which an issuer chooses whether to buy back a delinquent loan at par from MBS investors after the borrower misses a monthly payment. The subscripts B and W denote buyout and waiting, respectively. If the issuer chooses to buy out the loan upon default, the net payoff Π_B will be defined by the following components:

$$\Pi_B = -C^{funding} - K^{labor} + \beta \left[(1 - p) V_B^{cure} + p V_B^{miss} \right] \quad (1)$$

⁶Linda Lowell, “Ginnie Buyouts Rattle Investor Nerves,” HousingWire, October 28, 2009.

The issuer immediately incurs financing costs $C^{funding}$ and labor costs K^{labor} . The issuer finances the par repurchase externally, so the purchase price nets against the loan's face value. Therefore, all costs and values are expressed per dollar of outstanding principal.

Suppose that, following some form of loss mitigation, a loan faces a redefault risk of p . With probability p , buyout will generate V_B^{miss} , the continuation value after a missed payment, conditional on prior buyout. This continuation value reflects the failure of a post-buyout intervention. Loss mitigation is successful with probability $(1 - p)$, whereby the borrower resumes payment, generating V_B^{cure} , the continuation value after cure, conditional on prior buyout. If the issuer instead waits, the net payoff is:

$$\Pi_W = -C^{advance} + \beta \left[q V_W^{cure} + (1 - q) (V_W^{miss} - \theta D) \right] \quad (2)$$

Here $C^{advance}$ denotes the cost of continuing to pass through payments to MBS investors. This cost depends on the original terms of the mortgage, since higher-coupon loans require larger pass-through payments to MBS investors. If the issuer chooses to do nothing, a loan faces a self-cure probability of q . With probability q the borrower self-cures, generating V_W^{cure} , the continuation value after cure, conditional on waiting. With probability $(1 - q)$, the borrower fails to self-cure by missing another payment, generating V_W^{miss} , the continuation value after another missed payment, conditional on waiting. Finally, θ denotes the shadow price of the regulatory constraint requiring issuers to keep delinquencies below prescribed thresholds, and D is the issuer's delinquency rate. The product θD thus captures the regulatory cost of leaving a delinquent loan in the MBS pool.

Note that if the issuer forgoes loss mitigation after a buyout, the cure probability under buyout equals the self-cure probability under waiting. However, the continuation values need not converge, since the issuer still captures the gain from ownership in perpetuity. In this case, the issuer benefits from the coupon spread and regulatory relief without improving loan performance.

Let $r^{original}$ denote the loan's original interest rate and let r^{par} denote the prevailing market coupon at the time of delinquency. Suppose there exists a constant spread s such that

$$C^{advance} = r^{original} + s \quad \text{and} \quad C^{funding} = r^{par} + s$$

After I normalize and collect terms in Section [IA.2](#) and define δ as the spread between the loan's note rate and the prevailing market rate, the difference between buyout and waiting becomes:

$$\Delta\Pi = \beta\left(\tilde{V}_B - \tilde{V}_W + (1 - q)\theta D\right) + \delta - K^{labor} \quad (3)$$

Equation (3) yields several intuitive comparative statics. First, an issuer is more likely to choose a loan buyout instead of waiting if the coupon spread δ is large. Second, if a loan requires high labor costs to buy out and intervene, the issuer may choose to wait for the borrower to self-cure instead of spending effort ensuring successful loss mitigation. Third, buyout is more attractive when intervention raises the expected continuation value of the loan relative to waiting. This may occur, for example, when early intervention increases the probability of cure or reduces the likelihood of future missed payments relative to waiting. Finally, issuers may increase loan buyouts when their delinquency rates approach regulatory thresholds.

3.2 The Effect of Delayed Buyout on Loan Performance

In this subsection, I introduce an assumption about delinquency duration, alternative methods of intervention, and delays to loan buyout. First, I assume that post-intervention success $1 - p$ and self-cure probability q deteriorate with delinquency duration τ , such that $p'(\tau) > 0$ and $q'(\tau) < 0$. The longer the issuer waits, the lower the benefit to intervention even if it is eventually pursued. To isolate the cost of delayed intervention, I focus on loans for which timely loss mitigation improves borrower outcomes relative to waiting. Issuers nevertheless choose buyout only when $\Delta\Pi > 0$, namely when the private gain from buyout is positive.

Second, I introduce an alternative option to help borrowers exit delinquency. In particular, issuers can intervene for loss mitigation without a loan buyout. For example, an issuer may choose to capitalize the arrears of the loan into a second lien and nominally have the borrower exit delinquency. This form of forbearance is formalized in what is known as an FHA partial claim, whereby the FHA takes possession of title to the second lien in exchange for payment to the issuer. Such loss mitigation is viable even when the loan is still in the MBS pool because the issuer is not changing the terms of the mortgage. Here the net payoff from intervention without a buyout can be defined as:

$$\Pi_{NB} = -C^{advance} - K^{labor} + \beta \left[(1 - p) V_{NB}^{cure} + p (V_{NB}^{miss} - \theta D) \right] \quad (4)$$

where p is assumed to be equivalent to the redefault risk when an issuer buys out the loan, applying the same loss mitigation intervention under both scenarios. Since the loan remains in the pool, the issuer does not benefit from the lower funding costs.

With detailed exposition in Section [IA.2](#), the issuer should choose in-pool loss mitigation if the improved continuation value and reduced share of delinquency that result from intervention exceed labor costs. In contrast, issuers will choose a loan buyout over in-pool loss mitigation when they can gain from the coupon spread plus any increase in continuation value and reduced regulatory constraints. Therefore, there exists a subset of loans for which the issuer offers loss mitigation only when intervention is coupled with gains from ownership:

$$\Pi_B > \Pi_W \geq \Pi_{NB} \quad (5)$$

Finally, I introduce a restriction on loan buyouts that mirrors the effective exercise window for the buyout option in practice. A loan is eligible for a buyout only when the borrower fails to make three consecutive payments; namely, the borrower must enter 90-day delinquency. For loans for which $\Delta\Pi$ is negative, this restriction is inconsequential since a buyout was never profitable to the issuer. For loans for which Equation (5) holds, the policy

forces the loan onto an inferior path, despite the fact that the issuer would prefer to buy out the loan.

For the subset of loans in Equation (5), earlier loss mitigation may improve borrower outcomes relative to waiting, even though in-pool intervention is not privately worthwhile for the issuer. Despite this benefit, the borrower remains on the wait path to either self-cure or linger in worsening delinquency. Since $q'(\tau) < 0$ and $p'(\tau) > 0$, by the time buyout becomes feasible at 90-day delinquency, the borrower's self-cure probability has fallen and redefault risk has risen, implying a lower continuation value from intervention than had the issuer acted after initial delinquency. Therefore, preventing earlier loan buyouts may not only delay intervention, but also worsen loan outcomes.

Of course, allowing interventions at first delinquency introduces a countervailing concern, namely that borrowers may strategically default to obtain favorable loan modifications, lowering self-cure probability $q(\tau)$ endogenously. In addition, financially constrained servicers may instead pursue premature foreclosure or modification (Aiello, 2022). The optimal policy would balance the cost of delayed intervention against the moral hazard of making that intervention too accessible. This paper does not solve for the optimal policy rule, but the empirical results aim to shed light on the magnitude of the delay cost.

In particular, under the framework above, the 2002 memorandum suspended buyout for loans that entered 30-day delinquency while leaving underlying loan quality unchanged. As eligibility was suspended for loans originated closer to the policy change, differences in loan performance should primarily reflect the consequences of delaying intervention tied to buyout. Assessing the size and mechanism of this margin can shed light on the economic consequences of delaying loss mitigation for delinquent borrowers.

4 Data and Sample Construction

4.1 Data and Sample Construction

I use the CoreLogic (now Cotality) Loan-Level Market Analytics (LLMA) dataset to examine loan performance over the sample period. The dataset provides detailed origination characteristics and monthly performance outcomes for roughly 45% of mortgages originated in 2002. Data are collected from the 25 largest U.S. mortgage servicers.

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, as loans at the endpoints of the sale window lack variation across origination cohorts.⁷ 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 loans with 30-, 20-, or 15-year terms, loan-to-value ratios below 1.5, balances 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 (Kruger, 2018). This cutoff mitigates concerns that crisis dynamics drove delinquency onset.

⁷Restoring the asymmetrically trimmed observations yields a foreclosure estimate of 3.7 percentage points, compared with 4.2 percentage points in the baseline. The estimate remains statistically significant at the 5% level, indicating that the positive foreclosure effect is robust to restoring these observations.

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 borrower’s reported delinquency status remains at exactly 30 days past due in each of the final two months, 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 prevailing market rate at the time of delinquency, which I measure with the Freddie Mac 30-year fixed-rate mortgage average.⁸ 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.⁹

4.2 Measurement

I proxy for an issuer buyout using an indicator equal to one when a loan’s investor status removes its Ginnie Mae status within two months of a qualifying event.¹⁰ A qualifying event is either a rolling delinquency or a 90-day delinquency. Given the institutional background, I interpret a change in investor status around the time of a qualifying event as an issuer exercising a buyout.¹¹ For brevity, I refer to this proxy as a buyout throughout.

To assess the credibility of this proxy, I test whether it captures economic variation consistent with loan buyout activity. Section 4.4 examines whether measured buyouts vary

⁸Freddie Mac, 30-Year Fixed Rate Mortgage Average in the United States, retrieved from FRED.

⁹Before imposing this restriction, nearly 90% of otherwise-eligible Ginnie Mae loans have a positive coupon spread at rolling delinquency, reflecting relatively high original coupons in the sample of delinquent loans. Removing this restriction leaves the estimated effects similar in magnitude and statistically significant at the 5% level.

¹⁰In robustness checks, I vary the window from one to three months. The two-month and three-month windows produce nearly identical IV estimates (−13.9 and −14.2 percentage points, respectively). The one-month window yields a larger estimate (−18.2 percentage points), but one that is qualitatively equivalent.

¹¹Note that the GSEs repurchase loans directly, which means that loans do not change their investor status.

with coupon spreads and other issuer incentives. Section 5 examines whether the measure responds to the suspension of rolling-delinquency buyout eligibility.

Methodologically, my buyout measure is novel to the literature. No public, market-wide, loan-level data directly identify Ginnie Mae buyouts before October 2009. To the best of my knowledge, this is the first market-wide loan-level measure of Ginnie Mae buyouts spanning the early 2000s through the financial crisis. 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 approximately 30% of the roughly \$9 trillion agency MBS market, this measure should prove useful for future research on the Ginnie Mae market, the broader RMBS market, and the economics of loan buyouts.

Beyond the buyout proxy, I also infer two additional variables. First, the policy affected buyout eligibility based on the timing of securitization. While I do not directly observe securitization, I proxy for exposure to the policy change using the timing of origination. Second, I observe some loss-mitigation tools through changes in interest rates and principal balances, while other tools are not directly observed. These include forbearance through second liens that capitalize arrears and term extensions. Section 6 provides evidence on these unobserved channels. Figure IA.13 summarizes which elements of the empirical framework are observed directly, measured by proxy, or inferred from institutional details.

4.3 Summary Statistics

Table 1 reports summary statistics for origination characteristics and loan outcomes in my primary sample of Ginnie Mae loans. The primary sample consists of loans originated between July and November and sold to either Ginnie Mae or the GSEs, conditional on a rolling delinquency and being valued above par. The table compares loans originated in the treated year (2002) with those originated in the control year (2001). Corresponding summary statistics for the GSE sample are reported in Table IA.1 of the Internet Appendix.

Several differences between Ginnie Mae and GSE loans are notable. Ginnie Mae loans

have higher loan-to-value ratios, smaller balances, and fewer prime borrowers than GSE loans. 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. Despite these differences in observable loan characteristics, outcomes appear relatively similar when conditioned on rolling delinquency. Turning to buyouts, I find that over half of Ginnie Mae loans that experience a rolling delinquency are bought out. In contrast, GSE loans rarely change investor status in the data.

4.4 Buyout Behavior Over the Business Cycle

In this section, I present evidence that the novel buyout measure I introduce captures meaningful economic variation across a broad sample using the 90-day delinquency threshold. In particular, buyout activity should covary with model parameters introduced in Section 3 both in the cross-section and over time. The construction of the expanded sample and its summary statistics are discussed in Section IA.1.

From an issuer’s perspective, a loan buyout is particularly attractive when the loan is valued above par, namely when δ is high. This coupon spread should generate a clear threshold strategy, whereby even a slightly negative coupon spread in perpetuity will push the buyout wedge well below zero. To that end, I estimate ordinary least squares (OLS) regressions of the following form to test how a loan’s coupon spread covaries with buyout propensity:

$$Buyout_{itq} = \sum_{s \in \Theta} \beta_s Spread_i^s + X'_{itq} \gamma + Z_t + \alpha_{mq} + \varepsilon_{itq} \quad (6)$$

where Θ denotes the set of mutually exclusive bins for a loan’s interest rate spread. $Buyout_{itq}$ is an indicator variable for my buyout proxy, and $Spread_i^s$ is an indicator variable that equals one if the coupon spread is within interval s . Therefore, β_s represents the differential buyout propensity for loans in interval s relative to loans with coupon rates close to the prevailing market rate at the time of delinquency. Finally, I control for origination month and time-

varying MSA fixed effects, as well as loan characteristics. Standard errors are clustered at the MSA level.

Figure 3 reports estimates for the set of β_s in Equation (6). I estimate Equation (6) separately for loans delinquent in the pre-crisis period (2004–2007), crisis period (2008–2011), and post-crisis period (2012–2016). For the pre-crisis period, when delinquency levels were relatively low, estimates confirm that loans with relatively high coupon spreads experience greater buyout propensity. For spreads below zero, there appears to be little variation, which follows naturally from the model since these loans would generate negative gains from ownership in perpetuity. Above par, buyout propensity increases with the coupon spread. During the pre-crisis period, loans with a spread greater than 0.8 percentage points were 43.2 percentage points more likely to experience a buyout than loans with a coupon close to the prevailing market rate.

During the crisis period, the dramatic rise in delinquency made it costlier for issuers to leave delinquent loans in the MBS pool. High- δ loans already enjoyed elevated buyout rates, so rising delinquency did comparatively little to push them higher. By contrast, low- δ loans had a small or negative buyout wedge, giving issuers room to buy out the marginal loan. Consistent with this, while the coupon spread still meaningfully predicts buyout propensity in the crisis era, this effect attenuates as loan performance deteriorates. Strikingly, during the post-crisis period, the coupon spread loses virtually all of its predictive power as issuers face a rising interest rate environment where a majority of loans carry coupons below par.

In sum, the preceding analysis validates the novel buyout measure as capturing rich variation in issuer incentives over the business cycle. In Section IA.2, I explore how the dramatic rise in nonbank lending, aggregation, and lender concentration influenced buyout activity in ways that are predictable through the conceptual framework introduced in this paper.

5 Empirical Design and Main Results

5.1 Empirical Design

The conceptual framework introduced in Section 3 identifies a margin at which preventing early buyout may worsen loan performance. Estimating the role of buyout timing suffers from endogeneity concerns arising from asymmetric information in buyout. To approximate an ideal experiment, I exploit the timing of the 2002 Ginnie Mae memorandum that eliminated the buyout option for rolling delinquencies.

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.¹²

The intuition behind this is best illustrated in Figure 2. In Panel A, I plot the propensity for loans originated from 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, reflecting how much runway a loan has to securitize 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 in-

¹²APM 02-17 also revised loan-reporting standards, potentially delaying the pooling of new loans. Assuming that the delay affected new loans equally, loans originated earlier would still have more time to securitize before the January cutoff.

consequential. 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.

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. To account for origination-month seasonality unrelated to the memorandum, I implement a triple-difference (DDD) design. The design contrasts Ginnie Mae (treated) and GSE (control) loans across the policy year (2002) and the prior year (2001), and across high-exposure months (October–November) and low-exposure months (July–September). The primary reduced-form estimates are the triple-difference changes in the buyout proxy and subsequent loan outcomes associated with exposure to the Ginnie Mae policy change.

I condition on a loan entering rolling delinquency and holding a coupon above the prevailing market rate at the time of delinquency. Therefore, my estimates will apply to this selected margin of loans rather than the full population. Furthermore, a causal interpretation of my estimates would require that selection into my sample not vary with exposure. I assess this selection concern in Section 6 by testing how entry into rolling delinquency covaries with origination timing and loan outcomes.

I estimate ordinary least squares (OLS) regressions of the following form:

$$\begin{aligned}
Y_{itq} = & \beta_1 Y02_y + \beta_2 GNMA_i + \beta_3 (Y02_y \times GNMA_i) \\
& + \sum_t \zeta_t Z_t + \sum_t \phi_t (Z_t \times Y02_y) + \sum_t \eta_t (Z_t \times GNMA_i) \\
& + \delta (Z_{t \geq 10} \times Y02_y \times GNMA_i) + X'_{itq} \gamma + \alpha_{mq} + \varepsilon_{itq}
\end{aligned} \tag{7}$$

In the specification above, Y_{itq} is an indicator measuring loan performance for loan i originated in month t of year y 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. Here, I assume that, given the lag between origination and securitization, loans originated in October and November are precisely the loans that would be most exposed to the suspension in buyout eligibility. The indicator $Y02_y$ equals one for loans originated in the treatment year ($y = 2002$) 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, a fixed-rate indicator, the log of the original mortgage balance, the log of the current mortgage balance, and a term-length indicator. I proxy for underwriting stringency with a low-documentation indicator. 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 log of the ZIP3 house-price index and the logs of county GDP, population, and income, in addition to the county unemployment rate. I absorb MSA-by-quarter-of-delinquency fixed effects, α_{mq} , to capture time-varying local shocks. Standard errors are clustered at the MSA level to account for within-MSA residual correlation.¹³

The coefficients β_1 , β_2 , and β_3 capture variation unique to the treated year (2002), the

¹³In unreported tests, wild-cluster bootstrap inference at the MSA level preserves statistical significance for the preferred buyout, foreclosure, and early-cure estimates.

Ginnie Mae market segment, and the differential effect for 2002 Ginnie Mae loans, respectively. The coefficients ζ_t , ϕ_t , and η_t capture variation unique to calendar month- t , 2002 loans originated in calendar month- t , and Ginnie Mae loans originated in month t , respectively.

After I control for the systematic variation mentioned above, 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 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.

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, absent the November 2002 memorandum, the difference in loan outcomes between Ginnie Mae and GSE borrowers would have evolved similarly across origination months in 2002 as in 2001.

Because the memorandum was unanticipated and released after loan application and approval, the decision to originate a loan in any given month was independent of the policy-induced change in buyout eligibility. The memorandum was announced on November 6, 2002, and applied to MBS pools securitized after January 1, 2003. Given this narrow timeline, strategically sorting loans around the policy effective date would require changing the order of issuance within an already-constrained MBS pipeline. Moreover, origination timing reflects application and underwriting decisions made well before the announcement.¹⁴

Critical to my analysis is the assumption that lenders did not expect pre-January securitization to have a differential effect on loan outcomes relative to the prior year. Identification requires that any advance knowledge of the policy did not induce differential sorting across

¹⁴Applications and rate commitments can precede closing by several weeks, so some November originations may reflect decisions made before the policy announcement. Comparing October and November separately with July–September yields Wald estimates for subsequent loan outcomes that are similar in magnitude and statistically indistinguishable across the two months. This pattern is consistent with anticipation not driving the results.

origination cohorts. The timing of the announcement and interview evidence support the absence of anticipation. Under this assumption, differences in the likelihood of pre-January securitization across origination months provide a source of quasi-experimental variation in buyout propensity.

Nevertheless, unobservable loan quality may still correlate with origination timing. Figure [IA.1](#) plots baseline-adjusted means of loan characteristics for Ginnie Mae loans originated between July and November of 2001 and 2002.¹⁵ Observable characteristics are stable across months and years, varying by roughly 0.1 standard deviations relative to the July baseline, though interest rates exhibit a downward trend across months that I control for directly. To further assess balance, Figure [IA.3](#) reports triple-difference estimates using origination characteristics as dependent variables. The specifications retain all lower-order interactions but omit additional covariates and geographic fixed effects. I divide each estimate by the characteristic’s standard deviation in the pooled estimation sample to facilitate comparisons across characteristics. The largest differences are in initial interest rates and refinance status, at approximately -0.15 and 0.15 standard deviations, respectively. All five estimates are smaller than 0.25 standard deviations in absolute value. Although some differences are statistically significant, their magnitudes are small, and I control for these characteristics in the subsequent controlled specifications.

Formally, identification requires that loans originated later in 2002 would have exhibited outcomes similar to those of loans 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 as originations move from July toward the cutoff. To implement this test, I replace the single high-exposure interaction ($Z_{t \geq 10} \times Y02_y \times GNMA_i$) with the full set of month-specific

¹⁵Figure [IA.2](#) plots the corresponding means for GSE loans.

interactions and estimate month-level effects relative to July:

$$\begin{aligned}
Y_{itq} = & \beta_1 Y02_y + \beta_2 GNMA_i + \beta_3 (Y02_y \times GNMA_i) \\
& + \sum_t \zeta_t Z_t + \sum_t \phi_t (Z_t \times Y02_y) + \sum_t \eta_t (Z_t \times GNMA_i) \\
& + \sum_t \delta_t (Z_t \times Y02_y \times GNMA_i) + X'_{itq} \gamma + \alpha_{mq} + \varepsilon_{itq}
\end{aligned} \tag{8}$$

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 is a stronger assumption than necessary for Equation (7) because it governs differential variation within a given month relative to July, not across two discrete periods. A monotonic pattern across months would lend credibility to the assumption that outcomes are influenced by the limited window to securitize prior to the policy change.

Estimands and Interpretation The empirical design above is best represented by Figure [IA.13](#), which illustrates the chain from the suspension of early-buyout eligibility to subsequent loan outcomes. In addition, the figure outlines which estimands are identified at each stage of the empirical framework and helps clarify their interpretation and limitations.

In the first stage, I estimate the effect of exposure on early buyouts. In the reduced form, I estimate the effect of exposure on subsequent loan outcomes, including foreclosure, loan cures, modifications, and prepayments. In Section 7, the estimand I identify is the ratio of the reduced form to the first stage, namely the effect of an early buyout for loans whose buyout timing responded to the suspension. Note that these loans may nevertheless be bought out at 90-day delinquency, implying that the timing of buyout is the relevant dimension for these loans.

Finally, note that Figure [IA.13](#) includes two intermediate variables that I do not observe directly, namely issuer ownership and loss mitigation. By measuring a change in investor status at precisely the time of buyout eligibility, I am able to proxy for loan buyout using the institutional structure of the Ginnie Mae market.¹⁶ As for loss mitigation, I observe a limited set of tools through changes in interest rates and loan balances, while other tools are not directly observed. These include forbearance through second liens that capitalize arrears. As a result, the 2SLS estimate identifies the effect of an early buyout but does not separately identify which intermediate channel drives subsequent loan outcomes. Section 6 uses institutional details together with the effects on loan cures, modifications, and prepayments to assess these channels.

5.2 Effect on Buyout and Foreclosure

Effect on Buyout Table 2 Panel A presents estimates of Equation (7) 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 high-exposure period relative to the low-exposure period. This specification exploits within-calendar-month variation specific to the treated year and market segment.

In Column (1), I estimate that buyouts experienced a 30.6 percentage point decline, statistically different from zero at the 1% level. Here, my model includes exclusively fixed effects for origination month, year, and investor segment, alongside their pairwise interactions. Thus, the estimate reflects differential changes in buyout activity without adjusting for loan characteristics or local economic variables.

¹⁶Since GSE ownership status is unchanged in the data after a buyout, the GSE buyout measure reflects measurement error around zero. This concern applies only to buyout as an outcome. Foreclosure and cure are observed for both segments. The design assumes that the late-versus-early month gap in GSE buyouts does not change between 2001 and 2002, while allowing for level differences across years and months separately. Under this assumption, triple-difference estimates are consistent. Comparisons between 2SLS and the Wald ratio in Section 7 are consistent with this assumption.

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, fixed-rate status, refinance status, primary occupancy, single-family status, and loan term. Column (3) includes MSA fixed effects to control for slow-moving local conditions. Column (4) adds quarter-of-delinquency fixed effects to account for macroeconomic conditions common across all loans in a given quarter. Across Columns (2) through (4), point estimates range from 30.2 to 30.6 percentage points 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 30.3 percentage point reduction in buyouts, statistically significant at the 1% level.¹⁷

To further validate my research design, I estimate a dynamic triple-difference specification. Figure 4 Panel A plots coefficients from Equation (8) 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 2, which includes lower-order interactions, loan-level controls, and MSA-quarter-of-delinquency fixed effects.

After accounting for systematic variation across months, years, and investor segments, any remaining differential trends in buyout propensity should be approximately random relative to July. Instead, the estimates in Panel A present a monotonic decline in buyout activity across month of origination, consistent with a sharp securitization cutoff. Estimates for loans originated in August 2002 are statistically indistinguishable from those for July. In September 2002, Ginnie Mae loans begin to experience a statistically significant decline in buyout propensity of 7.0 percentage points. This magnitude rises to 24.0 percentage points

¹⁷Table IA.5 and Table IA.6 of the Internet Appendix report robustness checks using issuer fixed effects and a logit specification, respectively. Results for all outcomes are qualitatively similar.

in October 2002 and to 40.5 percentage points 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 2 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.

Effect on Foreclosure Central to my identifying assumption is that the drop in buyout activity estimated above 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 timing of buyout eligibility affects long-term loan outcomes. As described in Section 3, there may be a subset of affected loans for which loss mitigation is privately optimal only when the issuer captures the gains from ownership. For these loans, absent ownership, issuers may prefer to wait rather than provide in-pool loss mitigation. This should be particularly consequential if longer spells of delinquency lead to long-run deterioration in loan quality, as posited in Section 3. Here, I test whether restricting early buyout eligibility increases subsequent foreclosure risk, as the mechanism predicts.

The regressions in Table 2 Panel B test the effect of the Ginnie Mae policy change on loan quality by estimating Equation (7) 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 therefore need to miss additional payments after having resumed regular payments despite the outstanding arrearage.

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 additional loan-level controls or granular fixed effects, I find that loans originated in the high-exposure period experienced a 3.5 percentage point increase in foreclosures, statistically significant at the 5% level. The 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 that loans originated in low-exposure months receive some treatment.

Column (2) includes controls to account for variation correlated with origination characteristics and local economic 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 4.2 to 4.4 percentage points, 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 4.2 percentage point increase in foreclosures relative to the control group, statistically significant at the 5% level.

Taken together, the evidence in Table 2 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, absent the memorandum, the difference in loan outcomes between Ginnie Mae and GSE borrowers would have evolved similarly across origination months in 2002 as in 2001. To evaluate the validity of this assumption, I estimate a dynamic triple-difference specification.

Figure 4 Panel B plots coefficients from Equation (8) 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 2. 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 those for July. In September 2002, Ginnie Mae loans begin to experience a statistically significant increase in foreclosure rates of 6.3 percentage points. This magnitude increases to 6.6 percentage points in October 2002 and rises to 8.2 percentage points by November 2002.

Ultimately, the results in Table 2 Panel B and Figure 4 Panel B provide credible evidence that the increase in foreclosures is due to the suspension of the buyout option for rolling-delinquent loans and not to unobserved factors correlated with origination month. These findings are consistent with the model introduced in Section 3, in which ownership gains can make timely loss mitigation privately worthwhile. The increase in foreclosure indicates that restricting early buyout eligibility has lasting consequences for loan performance.

6 Mechanism: Short- and Medium-Run Loan Outcomes

6.1 Short-Run Loan Outcomes

The previous section showed that restricting early buyout eligibility increases subsequent foreclosure. I next examine short-run outcomes, namely early cures, to assess whether the policy affected loss mitigation around the time when buyout would counterfactually have been available. A reduction in early cures would support delayed or forgone intervention as a channel through which the policy worsens loan performance.

Table 3 Panel A presents estimates of Equation (7) using loan cures (i.e., exiting delinquency) as the dependent variable. Here the outcome of interest, Y_i , equals one if loan i experiences a cure 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. In order to account for this, I restrict the window for measuring cures to three months after the initial rolling delinquency. 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 percentage point decline in early cures, statistically significant at the 5% level. This implies that, absent the buyout suspension, treated loans would have been 4.2 percentage points 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-level and local economic 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 4.8 to 5.2 percentage points 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 percentage point 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, local economic controls, and time-varying geographic fixed effects.

To examine how the estimates vary with exposure, I estimate a dynamic triple-difference specification. Figure 5 Panel A plots coefficients from Equation (8) 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 3, including lower-order interactions, loan-level controls, and MSA-quarter-of-delinquency fixed effects.

I find that the estimates for δ_t decline 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 those for July. In September 2002, Ginnie Mae loans begin to experience a statistically significant decline in early cures of 7.3 percentage points. The corresponding declines are 6.6 percentage points in October 2002 and 7.7 percentage points in November 2002.

The reduction in early cures documented in Table 3 Panel A and Figure 5 Panel A is consistent with a decline in timely issuer intervention. The similar magnitudes of the decline in early cures and the increase in foreclosure further support delayed resolution as an important channel through which the suspension worsens loan performance. However,

these estimates capture average effects and do not establish that the suspension reduced early cures and increased foreclosure for the same loans.¹⁸

To pin down the mechanism further, I explore what tools issuers deploy to help borrowers exit delinquency. If issuers rely on loan modifications to facilitate cures, then losing the buyout option would legally restrict their ability to modify loan terms. Suspending buyouts should therefore reduce modifications and, in turn, cures, consistent with the estimates in Table 3 Panel A. In Panel B, I test whether a reduction in modifications explains the decline in early cures.

I estimate Equation (7) 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.¹⁹ In Columns (1) to (5), I sequentially add borrower- and loan-level controls, local economic controls, 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 cures following a buyout, despite having greater legal flexibility to do so.

To further rule out alternative tools, in Table 3 Panel C, I test for changes in prepayment activity. I present estimates of Equation (7) using a prepayment indicator within three

¹⁸To assess the joint significance of the cure and foreclosure results, in Figure IA.9, I present evidence from several placebo experiments following Young (2019). I exclude selected crisis-period origination windows. I find that only 1 out of 112 alternative five-month windows over 2000–2013 produce joint dynamics as extreme as those identified in this sample. To assess selection into rolling delinquency, I estimate Equation (7) without additional covariates using the sample of initially delinquent loans. The dependent variable is subsequent entry into rolling delinquency. Panel A of Figure IA.10 compares the estimate for the July–November 2001–2002 window with the distribution of estimates from 112 alternative five-month origination windows. Estimates are statistically indistinguishable from zero. In Panels B and C, the rolling-entry estimates exhibit little association with the corresponding foreclosure and early-cure estimates. Taken together, these patterns assuage concerns that differential entry into rolling delinquency explains the main results.

¹⁹Due to data limits, I cannot identify term extensions.

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 characteristics and local economic conditions, 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 -1.0 percentage points 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 Panel B and Figure IA.8 report differential trends in modification propensity and prepayment activity, respectively. The point estimates appear invariant to exposure across all calendar months.

Taken together, while the results earlier in this 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 same loan’s 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 2001.²⁰ Furthermore, HUD found that, of the 25 largest servicers, half employed partial claims more often than modifications or forbearance.

In sum, the decline in early cures and the increase in foreclosure support the interpretation that delaying access to the private benefits of ownership discourages timely loss mitigation and worsens loan performance. Issuers have the tools available to deploy loss mitigation independently of buyout eligibility. Interpreted through the model introduced in Section 3, a reasonable conclusion is that labor costs and the option value of waiting deter issuers from intervening while the loan remains in the MBS pool. Finally, the results highlight the

²⁰HUD OIG, *Loss Mitigation Program*, Audit Report 2002-DE-0001.

potential long-run benefits of resolving even a single missed payment promptly. In Section [IA.3.3](#), I explore the implications of this argument by studying how the effect of buyouts varies with financial slack.

6.2 Medium-Run Loan Outcomes

The previous section suggests that delaying buyout eligibility increased foreclosure rates by reducing loss mitigation, likely in the form of forbearance rather than loan modifications. In this section, I investigate whether the reduction in early cures attenuates at longer horizons.

Here, I examine whether the decline in cures reflects a permanent failure to exit delinquency or a delay in cure timing. Under the first scenario, the suspension causes loans to miss a critical window for loss mitigation, leaving them irreparably trapped in delinquency. Under the second scenario, loans may regain buyout eligibility upon entering 90-day delinquency, but the delay nevertheless damages long-run loan performance. The first channel would imply that loss mitigation is exclusively critical at the time of rolling delinquency. The second implies that delays in intervention can permanently damage a borrower's redefault risk.

In Table [4](#), I explore this channel using a longer observation window. I estimate Equation [\(7\)](#) using outcomes within a 15-month window following a rolling delinquency, providing a one-year extension to the original three-month window of observation to allow loans time to progress in delinquency. In Column (1), I estimate that high-exposure loans enter 90-day delinquency 5.5 percentage points 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 4.0 percentage points. At 15-month horizons, treated loans experience a 26.3 percentage point reduction in buyouts, statistically significant at the 1% level and representing a 13 percent decline in magnitude relative to Column (5) in Table [2](#). These results imply that, despite the suspension of early buyouts, issuers exercise their option to

purchase loans when those loans 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 three-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 0.7 percentage points, 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 statistically significant increase in modifications or prepayments in Columns (4) and (5) that would explain alternative loss mitigation tools. Therefore, neither the reduction in cures following early buyouts nor the increase in cures following late buyouts covaries with modifications or prepayment.

Taken together, my results suggest that the short-run reduction in cures attenuates at longer horizons. The buyout suspension reduces early cures by 4.2 percentage points (Table 3 Column 5), increases foreclosures by 4.2 percentage points (Table 2 Column 5), and has no effect on modifications or prepayments. The close alignment of the cure and foreclosure magnitudes, together with little movement in observable alternative exits, supports delayed resolution as an important channel through which the policy worsens loan outcomes. The estimated change in cures is statistically indistinguishable from zero over the 15-month window, while foreclosure is nevertheless differentially higher. This pattern is consistent with lasting consequences of delayed intervention despite attenuation of the estimated difference in cure rates.

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 $Z_{t \geq 10} \times Y02_y \times GNMA_i$ 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:

$$\begin{aligned} Buyout_{itq} = & \beta_1^{(1)} Y02_y + \beta_2^{(1)} GNMA_i + \beta_3^{(1)} (Y02_y \times GNMA_i) \\ & + \sum_t \zeta_t^{(1)} Z_t + \sum_t \phi_t^{(1)} (Z_t \times Y02_y) + \sum_t \eta_t^{(1)} (Z_t \times GNMA_i) \\ & + \delta (Z_{t \geq 10} \times Y02_y \times GNMA_i) + X'_{itq} \gamma^{(1)} + \alpha_{mq}^{(1)} + \varepsilon_{itq}^{(1)}, \end{aligned} \quad (9)$$

where $(Z_{t \geq 10} \times Y02_y \times GNMA_i)$ 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 , $Y02_y$, 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:

$$\begin{aligned} Y_{itq} = & \beta_1^{(2)} Y02_y + \beta_2^{(2)} GNMA_i + \beta_3^{(2)} (Y02_y \times GNMA_i) \\ & + \sum_t \zeta_t^{(2)} Z_t + \sum_t \phi_t^{(2)} (Z_t \times Y02_y) + \sum_t \eta_t^{(2)} (Z_t \times GNMA_i) \\ & + \rho \widehat{Buyout}_{itq} + X'_{itq} \gamma^{(2)} + \alpha_{mq}^{(2)} + \varepsilon_{itq}^{(2)}, \end{aligned} \quad (10)$$

where $\widehat{Buyout}_{itq}$ denotes the predicted values from Equation (9). Under the assumption that the triple interaction is a valid instrument, ρ identifies the causal effect of buyout on loan performance.

7.1 Identification

To identify a causal effect, the instrument must (i) vary with buyout activity, (ii) be as-good-as-randomly assigned, and (iii) affect loan performance only through buyout. The triple-difference interaction $Z_{t \geq 10} \times Y02_y \times GNMA_i$ inherits these properties from the research design discussed in Section 5.1. In particular, relevance is established by the first-stage estimates in Table 2 Panel A and Figure 4 Panel A, independence follows from the unanticipated nature of the memorandum, and the exclusion restriction is supported by a fixed effect strategy that absorbs systematic variation described in Section 5.1. I provide a formal discussion of each condition in Section IA.3.1 of the Internet Appendix.

In addition to these standard conditions, the research design must accommodate heterogeneity in the instrument’s response. If only a subset of loans is affected by origination month, the instrument must satisfy a monotonicity condition, namely that 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 buyout suspension. 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 with (i) a short time-to-securitization (always-takers) or (ii) an excessively long time-to-securitization (never-takers) because their buyout eligibility (treatment) is unaffected by the timing of origination (instrument).

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 the same is not true for the group of always-takers. Here, this population is reduced since treatment fundamentally depends on the timing of securitization. Namely, 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. This reduction in always-takers allows the LATE to be relevant for a complier group that closely approximates the treated population.

7.2 Results

Before turning to my second-stage estimates, I report standard diagnostic tests. First-stage Cragg-Donald and Kleibergen-Paap statistics exceed conventional critical values in all specifications, ruling out weak-instrument concerns. In the overidentified model, the Sargan–Hansen test fails to reject the null that additional instruments are exogenous at the 10% level for both foreclosure and cure outcomes. Details on these diagnostics are provided in Section [IA.3.2](#) of the Internet Appendix.

Table 5 presents second-stage estimates from the instrumental-variables estimation using foreclosure and cure as the dependent variables. Columns (1) to (6) 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 being valued above par. The estimated coefficients measure the causal effect of buyout on foreclosure and cure. In Columns (1)

and (4), I estimate an overidentified model, replacing $Z_{t \geq 10}$ with Z_t . In Columns (2) and (5), 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 5.1 and 6.

Column (1) of Table 5 presents estimates of the second-stage regression given by Equation (10) using the full set of triple interactions $Z_t \times Y02_y \times GNMA_i$. I find that buyout reduces foreclosure by 14.5 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 a local average treatment effect for compliers. Among late-originated 2002 Ginnie Mae loans that were bought out, the observed foreclosure rate is 20 percent. Relative to a counterfactual rate of approximately 33 percent absent buyout, the estimate in Column (2) implies that buyout reduces foreclosure by about 40 percent.

Column (2) of Table 5 presents just-identified 2SLS estimates using first-stage regressions given by Equation (9) with the triple interaction $Z_{t \geq 10} \times Y02_y \times GNMA_i$. 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 those of 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 5.1, where I present first-stage and reduced-form estimates reported in Column (5) of Table 2.

In the second stage, the estimate in Column (2) indicates that early buyout reduces foreclosure by 13.9 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 and 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 (4) of Table 5 presents estimates of the second-stage regression given by Equation (10) using early cure as the outcome of interest. I find that early buyout increases early cure by 14.9 percentage points, statistically different from zero at the 5% level. Among late-originated 2002 Ginnie Mae loans that were bought out, the observed three-month cure rate is 41 percent. Relative to a counterfactual cure rate of approximately 27 percent absent buyout, the estimate in Column (5) implies that early buyout increases early cures by about 50 percent.

Column (5) of Table 5 presents just-identified 2SLS estimates using first-stage regressions given by Equation (9). Here, the excluded instrument is the triple interaction $Z_{t \geq 10} \times Y02_y \times GNMA_i$, 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 2. The estimate in Column (5) indicates that buyout increases early cures by 13.7 percentage points, statistically different from zero at the 5% level. This estimate represents a 1.1 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 0.2 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.

For GSE loans, the proxy is rarely equal to one and should not be interpreted as a buyout transaction. Nevertheless, the full-sample 2SLS requires that seasonal variation in buyout not change between 2001 and 2002 for the GSE segment. This assumption cannot be assessed directly because actual GSE buyout activity is not observed. The same concern

does not arise for Ginnie Mae loans because buyout is correctly measured in that segment. As a robustness check, I report a Wald ratio estimator that removes the GSE segment from the first stage by estimating the denominator on the Ginnie Mae sample alone. The Wald ratio is computed as the reduced-form triple-difference coefficient (full sample) divided by the first-stage difference-in-differences coefficient (Ginnie Mae sample). Standard errors are obtained using an MSA-clustered block bootstrap with 1,000 replications. The close agreement between the full-sample 2SLS and the Wald ratio confirms that including the GSE sample in the pooled regression does not materially affect the estimates. Columns (3) and (6) of Table 5 report the Wald ratio estimates for foreclosure and cure, respectively. The Wald estimates of -15.8 and 15.6 percentage points are closely aligned with the corresponding 2SLS estimates, reinforcing the robustness of the main results.²¹

In Section [IA.3.3](#), I explore how financial constraints, collateral scarcity, and loan quality intermediate the effect of loan buyouts through the instrumental-variables framework introduced in this section.

8 Conclusion

This paper examines the effect of buyout timing on loan performance. I exploit quasi-experimental variation in the timing of buyout eligibility across vintage months preceding a suspension imposed on 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.

²¹The just-identified model is preferred because it does not require proportionality across origination months. The non-proportionality observed in the event study partly reflects treatment-effect heterogeneity and sensitivity to sample construction. The current specification balances proportionality against alternative samples and definitions that introduce bias. For example, expanding the sample beyond July 2007 risks contamination from the MBS market collapse, and narrowing the buyout window to one month increases measurement error.

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, early buyout reduces foreclosure probability by 13.9 percentage points for loans whose buyout timing responds to the policy. Together with the increase in early cures, this finding supports loss mitigation tied to ownership as a channel through which timely buyouts improve loan performance. 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 re-securitizing buyout loans. This paper highlights potential benefits of preserving a broad agency buyout option, effects that are consequential beyond Ginnie Mae given that approximately \$9 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 this setting focuses on agency debt, the dynamics identified here have broader implications for creditors in other lending markets.

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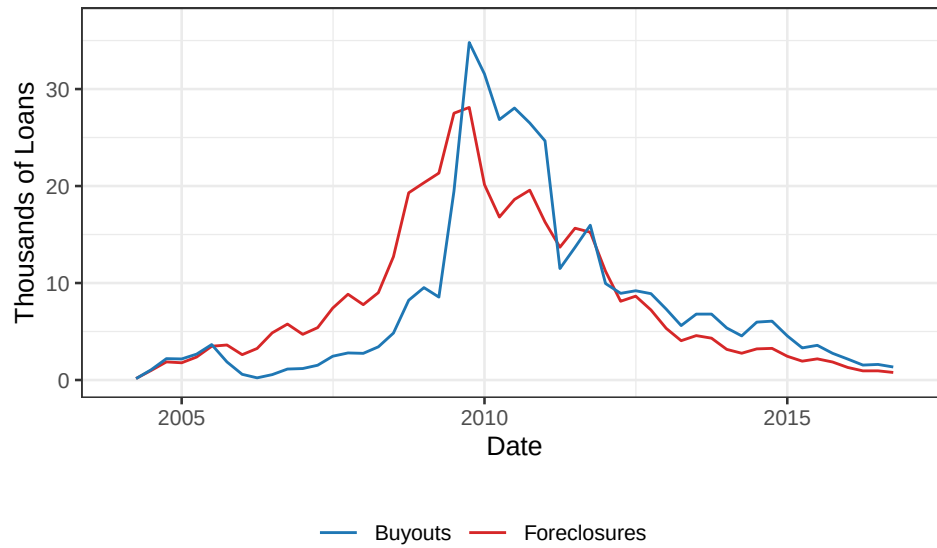
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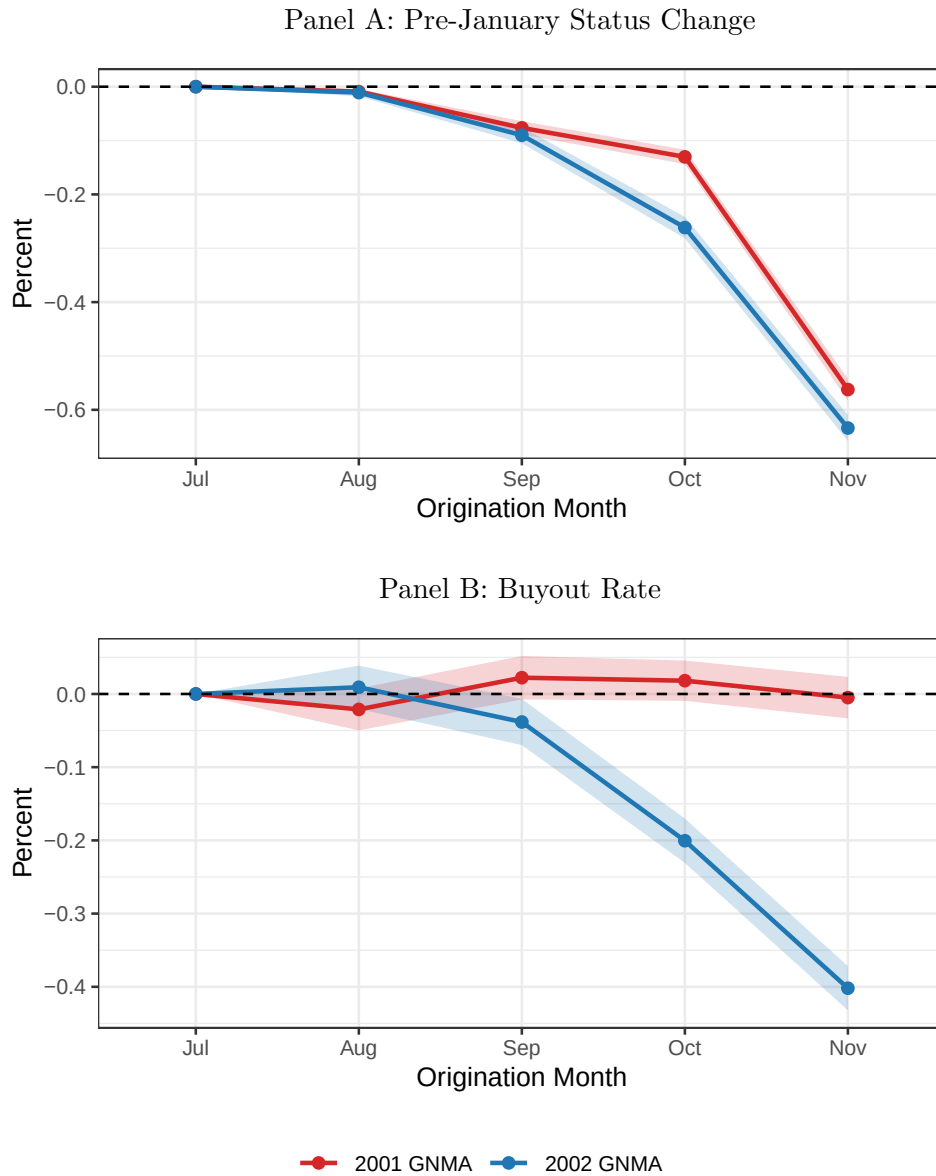
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Figure 1: Buyouts and Foreclosures



Note: This figure plots buyout and foreclosure counts, in thousands, 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 cover loans experiencing 90-day delinquency between 2004 and 2016, with origination dates spanning 2004 to 2014. The sample is restricted to loans entering 90-day delinquency within the first three years after origination.

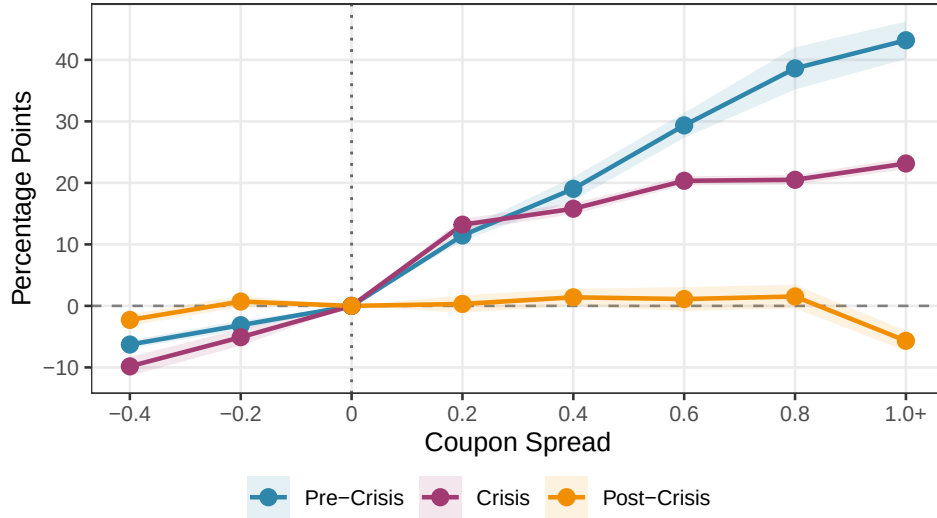
Figure 2: Investor Status Change and Buyout Across Origination Month



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 status 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 that 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 are collected from CoreLogic.

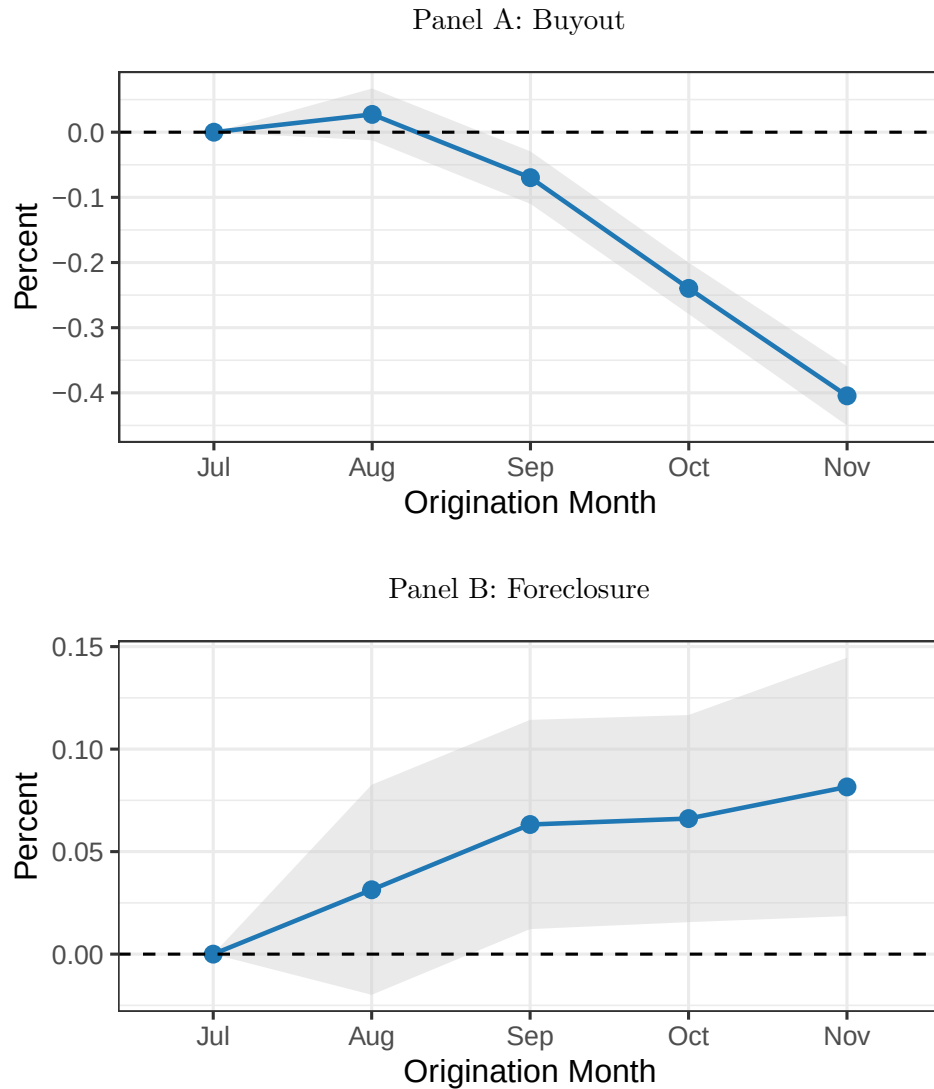
Figure 3: Coupon Spread and Loan Outcomes

Panel A: Buyout



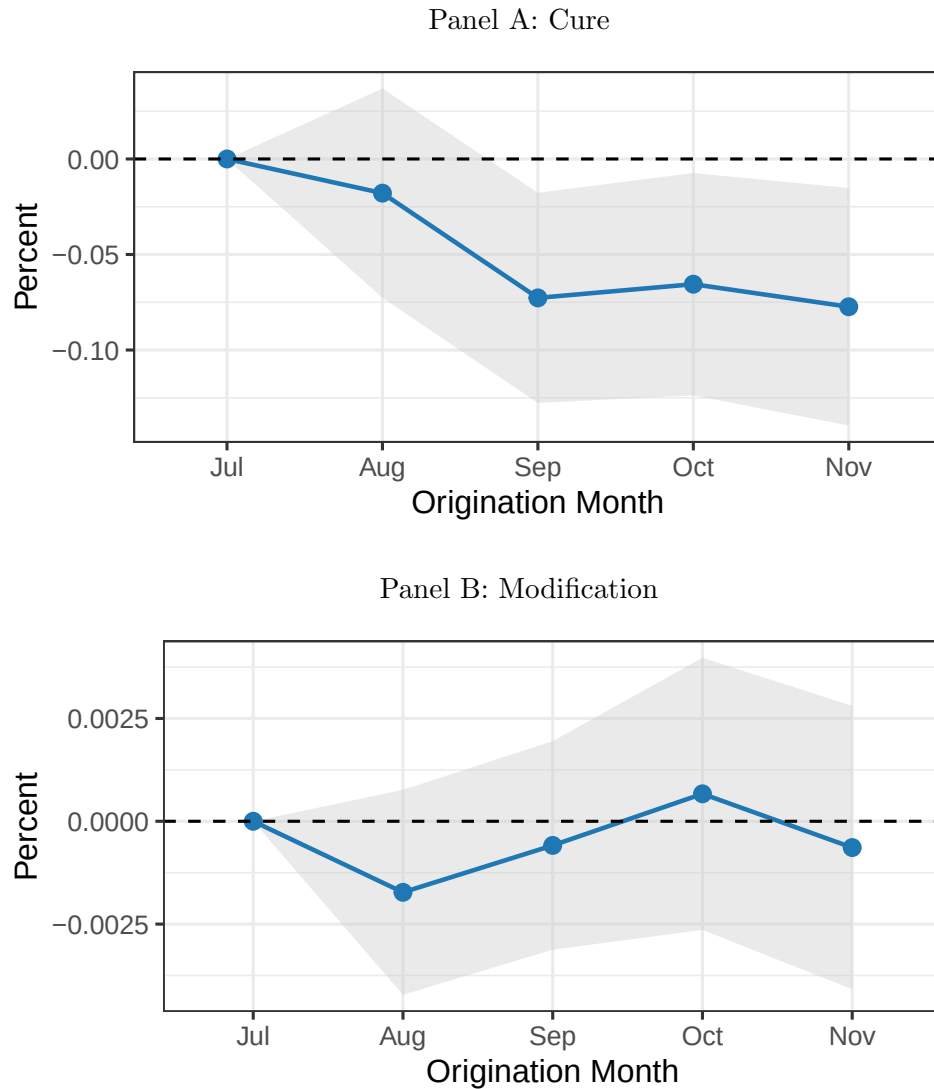
Note: This figure plots estimates from regressions of buyout on coupon spread bins. Observations are at the loan level. The coupon spread is defined as the difference between a loan's initial interest rate and the prevailing market rate at the time of 90-day delinquency. The outcome equals one if a loan is bought out of a Ginnie Mae MBS pool within two months of a 90-day delinquency. Estimates are shown separately for three periods: Pre-Crisis (2004 to 2007), Crisis (2008 to 2011), and Post-Crisis (2012 to 2016). Loan-level controls include initial LTV, log initial balance, log current balance, and indicators for primary residence, single-family status, fixed-rate status, refinance status, loan term, low-documentation status, and inferred prime status. All specifications include MSA-by-quarter-of-delinquency and month-of-origination fixed effects. Shaded bands denote 95% confidence intervals. Standard errors are clustered at the MSA level. Data cover FHA loans originated between 2004 and 2014 and sold to Ginnie Mae, with delinquency observed through December 2016.

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



Note: This figure plots dynamic triple-difference estimates from the fully specified model in Column (5) of Table 2. Panel A reports estimates with the dependent variable equal to buyout. Panel B reports estimates with the dependent variable equal to foreclosure. Each dot is the estimated coefficient on $Z_t \times Y02 \times \text{GNMA}$, 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 cover GNMA and GSE loans originated between July and November of 2001 and 2002, with delinquency observed between the time of origination and July 2007.

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



Note: This figure plots dynamic triple-difference estimates from the fully specified model in Column (5) of Table 3. Panel A reports estimates with the dependent variable equal to cure. Panel B reports estimates with the dependent variable equal to modification. Each dot is the estimated coefficient on $Z_t \times Y02 \times \text{GNMA}$, 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 cover GNMA and GSE loans originated between July and November of 2001 and 2002, with delinquency observed between the time of origination and July 2007.

Table 1: Summary Statistics: Ginnie Mae Loans

	2002		2001	
	Mean (1)	SD (2)	Mean (3)	SD (4)
Initial Rate	6.68	0.43	7.19	0.43
Original LTV	95.77	7.05	95.52	6.80
Balance (000s)	111.90	41.61	108.37	40.84
15-Year Term	0.02	0.13	0.02	0.12
20-Year Term	0.03	0.16	0.02	0.15
Prime Borrower	0.43	0.49	0.59	0.49
Refinance Loan	0.25	0.44	0.24	0.43
Primary Occupancy	0.83	0.38	0.93	0.26
Single-Family	0.83	0.38	0.87	0.34
Low/No Doc	0.28	0.45	0.15	0.36
Buyout	0.55	0.50	0.60	0.49
Foreclosure	0.20	0.40	0.20	0.40
Cure	0.46	0.50	0.47	0.50
Modification	0.00	0.03	0.00	0.03
Prepayment	0.04	0.20	0.05	0.21
Observations	9,018		11,352	

Note: This table reports mean and standard deviation values of loan characteristics and outcomes for Ginnie Mae loans, conditional on rolling delinquency and above-par status. Columns (1) and (2) present statistics for loans originated in 2002. Columns (3) and (4) present statistics for loans originated in 2001. Foreclosure equals one if a loan experiences a foreclosure start within 24 months after rolling delinquency.

Table 2: Effect of the GNMA Policy on Buyout and Foreclosure: Triple Difference

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Buyout</i>					
$Z_{t \geq 10} \times Y02 \times \text{GNMA}$	-0.306*** (0.015)	-0.303*** (0.015)	-0.302*** (0.015)	-0.306*** (0.015)	-0.303*** (0.015)
Observations	43,266	43,266	43,266	43,266	41,826
<i>Panel B: Foreclosure</i>					
$Z_{t \geq 10} \times Y02 \times \text{GNMA}$	0.035** (0.018)	0.042** (0.017)	0.042** (0.017)	0.044** (0.017)	0.042** (0.019)
Observations	43,266	43,266	43,266	43,266	41,826
Interactions	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes
MSA FE			Yes		
Quarter FE				Yes	
MSA $\times$ 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. In Columns (2) to (5), loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, prime status, fixed-rate status, documentation type, loan purpose, and property type. Local economic controls at the time of delinquency include the log of the ZIP3 house-price index and the logs of county GDP, population, and income, in addition to the county unemployment rate. 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 cover all loans originated between July and November of 2001 and 2002 sold to Ginnie Mae or the GSEs, with delinquency observed between the time of origination and July 2007.

Table 3: Effect of the GNMA Policy on Short-Run Loan Outcomes: Triple Difference

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Cure</i>					
$Z_{t \geq 10} \times Y02 \times \text{GNMA}$	-0.042** (0.019)	-0.048** (0.019)	-0.052*** (0.019)	-0.048*** (0.018)	-0.042** (0.020)
Observations	43,266	43,266	43,266	43,266	41,826
<i>Panel B: Modification</i>					
$Z_{t \geq 10} \times Y02 \times \text{GNMA}$	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Observations	43,266	43,266	43,266	43,266	41,826
<i>Panel C: Prepayment</i>					
$Z_{t \geq 10} \times Y02 \times \text{GNMA}$	-0.004 (0.008)	-0.004 (0.008)	-0.006 (0.008)	-0.005 (0.008)	-0.010 (0.008)
Observations	43,266	43,266	43,266	43,266	41,826
Interactions	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes
MSA FE			Yes		
Quarter FE				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 three 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. In Columns (2) to (5), loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, prime status, fixed-rate status, documentation type, loan purpose, and property type. Local economic controls at the time of delinquency include the log of the ZIP3 house-price index and the logs of county GDP, population, and income, in addition to the county unemployment rate. 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 cover all loans originated between July and November of 2001 and 2002 sold to Ginnie Mae or the GSEs, with delinquency observed between the time of origination and July 2007.

Table 4: Effect of the GNMA Policy on Medium-Run Outcomes: Triple Difference

	90d DLQ (1)	Buyout (2)	Cure (3)	Modification (4)	Prepayment (5)
$Z_{t \geq 10} \times Y02 \times \text{GNMA}$	0.055*** (0.016)	-0.263*** (0.013)	0.007 (0.015)	0.000 (0.003)	0.020 (0.017)
Interactions	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
MSA $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	41,826	41,826	41,826	41,826	41,826

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, fixed-rate status, documentation type, loan purpose, and property type. Local economic controls at the time of delinquency include the log of the ZIP3 house-price index and the logs of county GDP, population, and income, in addition to the county unemployment rate. 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 cover all loans originated between July and November of 2001 and 2002 sold to Ginnie Mae or the GSEs, with delinquency observed between the time of origination and July 2007.

Table 5: Effect of the GNMA Policy on Foreclosure and Cure: Instrumental Variables

	Foreclosure			Cure		
	(1)	(2)	(3)	(4)	(5)	(6)
Buyout	-0.145** (0.058)	-0.139** (0.062)	-0.158** (0.074)	0.149*** (0.056)	0.137** (0.067)	0.156* (0.082)
Interactions	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
MSA×Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	2SLS	2SLS	Wald	2SLS	2SLS	Wald
Excluded	$Z_t \cdot Y02 \cdot GNMA$	$Z_{t \geq 10} \cdot Y02 \cdot GNMA$	—	$Z_t \cdot Y02 \cdot GNMA$	$Z_{t \geq 10} \cdot Y02 \cdot GNMA$	—
Observations	41,826	41,826	41,826	41,826	41,826	41,826
Cragg-Donald	156.1	532.3	—	156.1	532.3	—
Kleibergen-Paap	108.3	386.7	—	108.3	386.7	—
Sargan-Hansen p	0.183	—	—	0.258	—	—

Note: This table reports instrumental variables (two-stage least squares and Wald ratio) estimates of the effect of the 2002 Ginnie Mae memorandum on loan outcomes. Observations are at the loan level. In Columns (1) to (3), the outcome equals one if a loan enters foreclosure within 24 months after a rolling delinquency. In Columns (4) to (6), 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 (4) is the triple interaction $Z_t \times Y02 \times GNMA$. The excluded instrument in Columns (2) and (5) is $Z_{t \geq 10} \times Y02 \times GNMA$. In the 2SLS specifications, 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. Columns (3) and (6) report Wald ratio estimates, computed as the reduced-form DDD coefficient (full sample) divided by the first-stage DD coefficient (GNMA loans only). The GNMA-only first stage includes origination-month and year effects, the listed controls, and MSA-by-quarter-of-delinquency fixed effects. Loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, prime status, fixed-rate status, documentation type, loan purpose, and property type. Local economic controls at the time of delinquency include the log of the ZIP3 house-price index and the logs of county GDP, population, and income, in addition to the county unemployment rate. First-stage strength and overidentification are summarized by the Cragg–Donald and Kleibergen–Paap statistics and the Sargan–Hansen p -values. 2SLS standard errors (in parentheses) are clustered at the MSA level. Standard errors for Wald ratios are obtained via MSA-clustered block bootstrap (1,000 replications). 2SLS significance tests use the model’s t reference distribution; Wald-ratio significance tests use a standard-normal reference with the bootstrapped standard errors. 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 entry into rolling delinquency observed through July 2007.

Internet Appendix for “Loan Buyouts and Foreclosure: Evidence from Ginnie Mae”

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IA.1 Supplementary Data Construction and Summary Statistics

IA.1.1 Comparative Statics: Dataset Description

The comparative statics analysis in Section 4.4 uses a broader sample than the primary causal analysis. The primary causal analysis focuses on loans originated in the period immediately prior to the memorandum and the year prior. Further, the analysis is restricted to loans that enter rolling delinquency. The business-cycle analysis, in contrast, studies the broad population of Ginnie Mae loans that experience a 90-day delinquency.

This broader sample consists of FHA loans from the CoreLogic LLMA dataset originated between 2004 and 2014 that enter 90-day delinquency within 36 months of origination. Production sample filters are applied: loan balances at or below \$1 million, standard terms (15, 20, or 30 years), loan-to-value ratios at or below 150%, valid MSA codes, and locations in the contiguous United States. Delinquency is observed through 2016. As before, the buyout proxy is measured as a change in investor status within two months of a 90-day delinquency.

Tables IA.2, IA.3, and IA.4 report summary statistics for the sample of Ginnie Mae loans experiencing 90-day delinquency, split by business-cycle period: pre-crisis (2004–2007), crisis (2008–2011), and post-crisis (2012–2016).

IA.1.2 HMDA-CoreLogic

To identify issuers, I impute lender identifiers using Home Mortgage Disclosure Act (HMDA) data. HMDA reports loan characteristics for the near universe of loans originated in the United States. In addition, HMDA reports the lender's name, the lender's nonbank status, and whether the loan was ultimately sold. CoreLogic does not directly report lender identifiers. To classify a lender as a bank or nonbank, I use the Avery file, which links HMDA respondent identifiers to various lender characteristics.

I impute lenders using a match on ZIP code using a census-tract-to-ZIP crosswalk, rounded loan amount, loan type, loan purpose, occupancy status, and investor channel. I merge loans across four steps: (i) originations sold directly to the GSEs or Ginnie Mae, (ii) purchased loans that I bridge back to their original loan record in HMDA within the same year, (iii) purchased loans bridged within a bandwidth of years, and (iv) purchased loans across all years. Within each step, I employ six passes that relax the criteria for geographic and loan amount matching. The overall match rate is approximately 75%.

IA.2 Model Appendix

IA.2.1 Buyout Wedge

Define $\tilde{V}_B \equiv (1-p)V_B^{cure} + pV_B^{miss}$ and $\tilde{V}_W \equiv qV_W^{cure} + (1-q)V_W^{miss}$ as the expected continuation values under buyout and waiting, respectively. The buyout wedge $\Delta\Pi = \Pi_B - \Pi_W$ then decomposes as:

$$\Delta\Pi = \beta(\tilde{V}_B - \tilde{V}_W + (1-q)\theta D) + \Delta C - K^{labor} \quad (\text{IA.1})$$

where $\Delta C = C^{advance} - C^{funding}$. The buyout wedge has two components. The first is the difference in continuation values conditional on buyout versus waiting. If buying out the loan and pursuing loss mitigation generates higher expected value than leaving the loan in the pool, the issuer has an incentive to intervene. Note that buying out the loan comes with the additional benefit of reducing regulatory constraints. The second component is the net

cost savings from stopping advance payments to MBS investors versus incurring the funding and labor costs of a buyout.

IA.2.2 Buyouts and In-Pool Loss Mitigation

Comparing in-pool intervention to waiting:

$$\Pi_{NB} - \Pi_W = \beta(\tilde{V}_{NB} - \tilde{V}_W + (1 - q - p)\theta D) - K^{labor} \quad (\text{IA.2})$$

where $\tilde{V}_{NB}$ is the expected continuation value of following in-pool loss mitigation. Since the cost of advancing funds continues to exist across both in-pool loss mitigation and waiting, the issuer chooses in-pool loss mitigation only if the improved continuation value and reduced share of delinquency that result from intervention exceed labor costs. I assume throughout that, absent the ownership gains from buyout, the servicer's margin on a performing loan is insufficient to justify the labor cost of intervention. This reflects the institutional reality that servicers lack a direct financial incentive to pursue costly loss mitigation when they hold no ownership stake in the loan. Note that this assumption is most plausible at early stages of delinquency, when the gap between intervention and waiting is small. In contrast, comparing buyout to in-pool intervention:

$$\Pi_B - \Pi_{NB} = \beta(\tilde{V}_B - \tilde{V}_{NB} + p\theta D) + \delta \quad (\text{IA.3})$$

Since labor costs exist for both options, the issuer will choose a loan buyout over in-pool loss mitigation when it can gain from the coupon spread plus any value premium associated with ownership and reduced regulatory constraints. This further validates the assumption that $\Pi_W \geq \Pi_{NB}$, since Π_W embeds the option for the issuer to access ownership gains in the future, whereas Π_{NB} eliminates that possibility by curing the borrower early. Hence, a borrower's access to loss mitigation is conditional on an issuer's private benefit of ownership. This formalizes why an issuer will at times fail to offer forbearance despite its potential

benefit to the borrower. Namely, there exists a subset of loans for which the issuer offers loss mitigation only when intervention is coupled with gains from ownership:

$$\Pi_B > \Pi_W \geq \Pi_{NB} \tag{IA.4}$$

IA.2.3 Banks vs. Nonbanks

This subsection documents heterogeneity in buyout behavior between bank and nonbank issuers using the production sample of Ginnie Mae loans originated between 2004 and 2014. The analysis exploits issuer identities obtained from HMDA-CoreLogic matched data.

Figure [IA.4](#) documents heterogeneity in buyout behavior, coupon spreads, and delinquency rates between bank and nonbank issuers. Panel A shows that, during the crisis, banks bought out at lower rates than nonbanks, but in the post-crisis period the gap reversed sharply, reaching nearly 30 percentage points by 2016.

Turning to the model in Section [3](#), I note that this pattern may reflect several channels, including coupon spreads, regulatory constraints, and labor costs. Panel B shows that the excess-coupon difference is close to zero throughout, except for a spike in delinquency year 2013, indicating that the post-crisis buyout gap is not driven by differential coupon incentives. In contrast, Panel C shows that bank-securitized loans exhibited lower delinquency rates during the crisis and higher delinquency rates in its aftermath. This is consistent with a model in which higher delinquency rates force issuers to buy out more loans as they approach delinquency-related regulatory constraints.

Panel D documents a labor-cost channel that may explain the higher buyout rate for banks. Among matched loans that enter 90-day delinquency, the share originated by nonbanks rises from under 10% for the 2009 cohort to nearly 40% for the 2014 cohort. Under higher capacity constraints due to limited labor to meet loan volume, the buyout rate should decline as market share increases.

Finally, I estimate ordinary least squares (OLS) regressions of the following form:

$$Buyout_{itq} = \gamma Nonbank_i + \sum_{p \in \Omega} \delta_p Nonbank_i \times Period_t^p + X'_{itq} \lambda + Z_t + \alpha_{mq} + \varepsilon_{itq} \quad (IA.5)$$

where $\Omega = \{crisis, post\}$ denotes the set of period indicators with the pre-crisis period omitted. $Nonbank_i$ is an indicator equal to one if loan i is originated by a nonbank issuer, and $Period_t^p$ is an indicator equal to one if the loan reaches delinquency in period p . The loan and borrower controls follow Section 4.4, with the addition of initial and current interest rates. I include MSA-by-quarter-of-delinquency and month of origination fixed effects and cluster standard errors at the MSA level. Therefore, γ captures the baseline nonbank differential in the pre-crisis period, and δ_p represents the shift in the nonbank differential in period p . I use the variance-covariance matrix of the jointly estimated coefficients to construct confidence intervals around the total implied nonbank differential within each period.

Figure IA.5 reports estimates for the set of $\delta_p + \gamma$ in Equation (IA.5). I estimate Equation (IA.5) jointly for loans delinquent in the pre-crisis period (2004–2007), crisis period (2008–2011), and post-crisis period (2012–2016). Blue markers represent the total implied differential in the pre-crisis period, maroon markers represent the crisis period, and yellow markers represent the post-crisis period. Vertical bars denote 95 percent confidence intervals clustered at the MSA level. As documented in Figure IA.4, the nonbank buyout rate is differentially higher in the crisis period, likely due to lower delinquency rates. During the post-crisis period, this differential is significantly lower, once again likely due to the increased labor costs following a rapid increase in Ginnie Mae origination share.

IA.2.4 Loan Source and Local Exposure

There may be several dimensions of cross-sectional heterogeneity worth exploring. For example, suppose issuers face a shadow cost λ that amplifies the cost of continuing to advance payments to MBS investors or funding a buyout. Furthermore, issuers may assign a lower

continuation value to loans they did not originate themselves, but instead purchased from an arm’s-length originator, because of uncertainty about unobserved loan quality. To that end, $\tilde{V}_B^{own} \geq \tilde{V}_B^{purchased}$, or equivalently, issuers may suffer a continuation-value penalty χ when the loan was aggregated rather than originated by the issuer. Let A_{agg} denote an indicator for whether the loan was aggregated.

In addition, issuers with high MBS issuance volume tied to a particular locality may be more sensitive to local spillovers than issuers with lower exposure. In that case, issuers may suffer a continuation-value penalty tied to exposure ϕ and the level of local delinquencies D_{local} when a loan remains or becomes delinquent. Let T denote an indicator that the issuer is the top MBS issuer or servicer in the locality.

Note that the spillover penalty applies on both sides of the buyout decision. On the wait side, if the borrower fails to self-cure with probability $(1 - q)$, the loan remains delinquent in the pool and the issuer bears $\phi D_{local} T_{top}$. On the buyout side, if the loan redefaults with probability p following intervention, the issuer again bears $\phi D_{local} T_{top}$. In contrast, the regulatory cost θD applies only on the wait side, since a bought-out loan is removed from the MBS pool regardless of subsequent performance. Incorporating these sources of heterogeneity, Equation (3) becomes:

$$\Delta\Pi = \beta \left(\tilde{V}_B - \tilde{V}_W - \chi A_{agg} + [(1 - q) - p](\phi D_{local} T_{top}) + (1 - q)\theta D \right) + \lambda \delta - K^{labor} \quad (\text{IA.6})$$

Equation (IA.6) extends the baseline buyout wedge $\Delta\Pi$ to incorporate a continuation-value penalty χ for aggregated loans and a local-spillover term ϕ for issuers with concentrated local exposure. This section presents descriptive evidence on both channels using the sample of Ginnie Mae loans originated between 2004 and 2014.

Aggregated vs. Direct Originations Issuers that purchase loans from arm’s-length originators—rather than originating them directly—may assign a lower continuation value to those loans because of residual uncertainty about unobserved borrower quality. If opacity

increases the effective cost of buying out, aggregated loans should exhibit a lower buyout propensity conditional on observables. Figure [IA.6](#) reports the estimated buyout differential for aggregated loans across pre-crisis, crisis, and post-crisis periods using Equation [IA.5](#). The within-lender specification isolates the source-type channel from unobserved issuer heterogeneity. Indeed, aggregated loans exhibit lower buyout propensities relative to other loans delinquent in the same MSA-quarter and relative to a lender’s portfolio of originated loans. The aggregation discount is smaller during the crisis. One interpretation is that aggregated loans have weaker underlying credit quality and experience a larger decline in self-cure probabilities during crises. This would make waiting relatively less attractive, strengthening buyout incentives for aggregated loans relative to directly originated loans. Nevertheless, the buyout differential remains negative, consistent with a persistent informational disadvantage for aggregated loans.

Local Exposure Issuers that hold a concentration position within a particular locality may be particularly sensitive to the downstream effects of their loss mitigation for a particular loan on other local assets. Therefore, issuers should face a shadow cost for allowing additional delinquencies to linger in precisely their most active localities. The model captures this through the term $\phi D_{local} T_{top}$ in Equation ([IA.6](#)), where D_{local} is the level of local delinquencies and T_{top} indicates whether the issuer is a top servicer. Figure [IA.7](#) reports the estimated buyout differential for top local issuers, defined as issuers in the top decile of Ginnie Mae FHA origination volume within a lagged county-quarter, across business-cycle periods using Equation [IA.5](#). The within-lender specification isolates the role of local issuer concentration relative to unobserved issuer heterogeneity. The figure documents higher buyout propensities in localities where an issuer is a top lender during the crisis and post-crisis periods, relative to localities where the same issuer is not a top lender. The pre-crisis differential is statistically indistinguishable from zero.

IA.3 Supplemental Instrumental-Variables Tests

IA.3.1 Instrumental-Variables Identification Conditions

This subsection provides a formal discussion of the relevance, independence, and exclusion restriction conditions underlying the instrumental-variables strategy in Section 7. The monotonicity condition is discussed in Section 7.1 of the main text.

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 $Z_{t \geq 10} \times Y02_y \times GNMA_i$, 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, loans originated in 2001 experience no change in the buyout rate across vintage months. Regression estimates in Table 2 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 application and approval, 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. 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. 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. Furthermore, including MSA-quarter-of-delinquency fixed effects accounts for time-varying local economic conditions.

IA.3.2 Instrumental-Variables Diagnostic Tests

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 $\rho = 0$.

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 implied treatment effect varies across origination months. 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.

IA.3.3 Heterogeneous Treatment Effects

In Section 7, I estimated the causal effect of buyout activity on loan performance. The results indicate that buyouts reduce foreclosure for loans whose buyout eligibility was delayed by the policy. In this section, I explore heterogeneity in the treatment effect to better characterize the channel through which buyouts affect loan outcomes. In unreported tests,

all subgroup first-stage F -statistics exceed 100 except for two that exceed 19. None of the tested differences in IV estimates is statistically significant at the 10% level. I therefore interpret the cross-group patterns as suggestive evidence.

First, I examine the extent to which financial constraints can explain the results in Section 7. 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 independently 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 test whether early buyouts reduce foreclosure more strongly 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 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.

Figure [IA.11](#) examines heterogeneity along measures of financial constraints, including education, unemployment, and poverty. I find that, 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 4.3

times as large as that in better-educated counties. Counties with higher unemployment exhibit a treatment effect 2.4 times as large as that in counties with lower unemployment. Higher-poverty counties experience a treatment effect 1.2 times as large as that in 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.

Second, I investigate the role of financial slack in generating effective loss mitigation. As motivation, [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 6.2](#). I hypothesize that when the underlying collateral is over-levered, issuers have less capacity to extend a subordinate lien, limiting the feasibility of arrears capitalization even when the issuer would otherwise benefit from the buyout. 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 A of Figure [IA.12](#), 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 [5](#), exploiting within-MSA and within-delinquency-quarter 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 19.7 percentage points, statistically significant at the 5% level. For above-median denial rates, the point estimate is 6.5 percentage points and statistically insignificant. Loans issued in counties with an average loan-to-income ratio below the median experience a treatment effect of 23.3 percentage points, statistically significant at the 1% level. In contrast, above-median loan-to-income counties exhibit a treatment effect of 5.1 percentage points, statistically insignificant. Ultimately, the close relationship between the salience of the buyout effect and mortgage credit slack supports the hypothesis that issuers capitalize arrears into subordinate debt. Counties with slack in the credit supply appear to benefit most from buyouts because issuers have greater capacity to extend a subordinate lien when the underlying collateral is not over-levered.

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 A of Figure [IA.12](#), 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 a 5.4 percentage point larger treatment effect than those in high debt-to-income counties. Neither estimate is statistically significant at the 5% level. The attenuation across DTI subsamples is consistent with the buyout channel operating through slack in the mortgage credit supply rather than through general borrower leverage. Aggregate debt-to-income captures broader financial constraints and not just mortgage-credit slack, which may explain the weaker separation relative to the denial-rate and loan-to-income splits.

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 the prevailing market rate from the loan's current interest rate 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 buy out. I hypothesize that loans with a strong incentive to buy out through the loan's coupon 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.

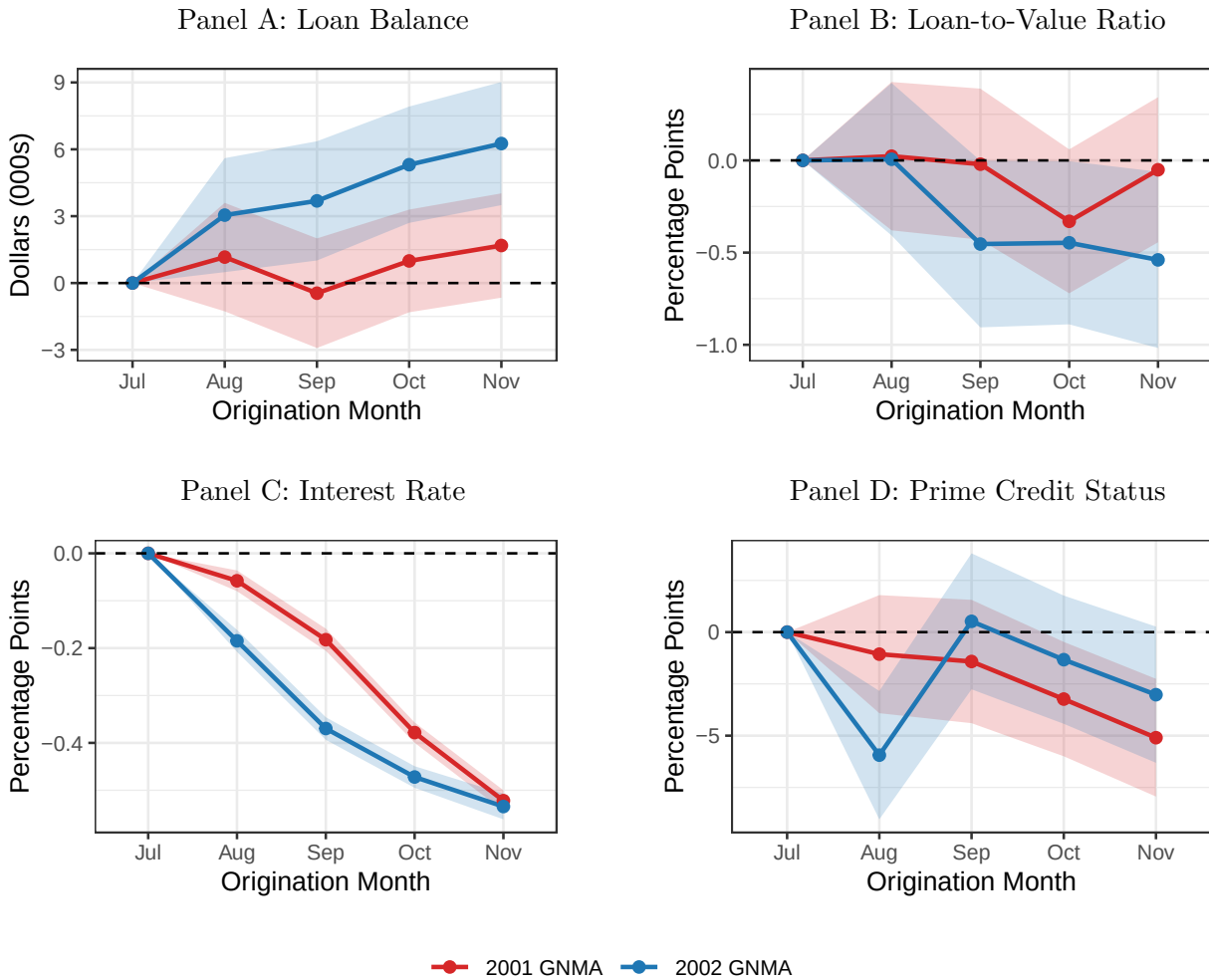
In Panel B of Figure [IA.12](#), I report just-identified 2SLS estimates using subsamples defined by loan quality and interest-rate spreads. I plot IV estimates that measure the causal effect of buyout on foreclosure for loans with strong and weak buyout incentives, separately. I find that low-quality 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 4.7 percentage points and is statistically insignificant at the 5% level. In contrast, high-quality loans with below-median payoff experience a treatment effect of 21.3 percentage points, statistically significant at the 5% level. High-quality loans with an above-median payoff experience a treatment effect roughly twice as large, estimated to be 38.5 percentage points and statistically significant at the 10% level but not at the 5% level.

Overall, among high-quality loans, those with an above-median interest-rate spread experience a treatment effect roughly twice as large as that for those with a below-median spread (38.5 versus 21.3 percentage points). Low-quality loans show no significant treatment effect regardless of spread. These results imply that when issuer incentives are strong and loan quality is high, the potential for durable reperformance is greatest, producing the largest reduction in foreclosures.

These results are consistent with the hypothesis that issuers expend greater effort to

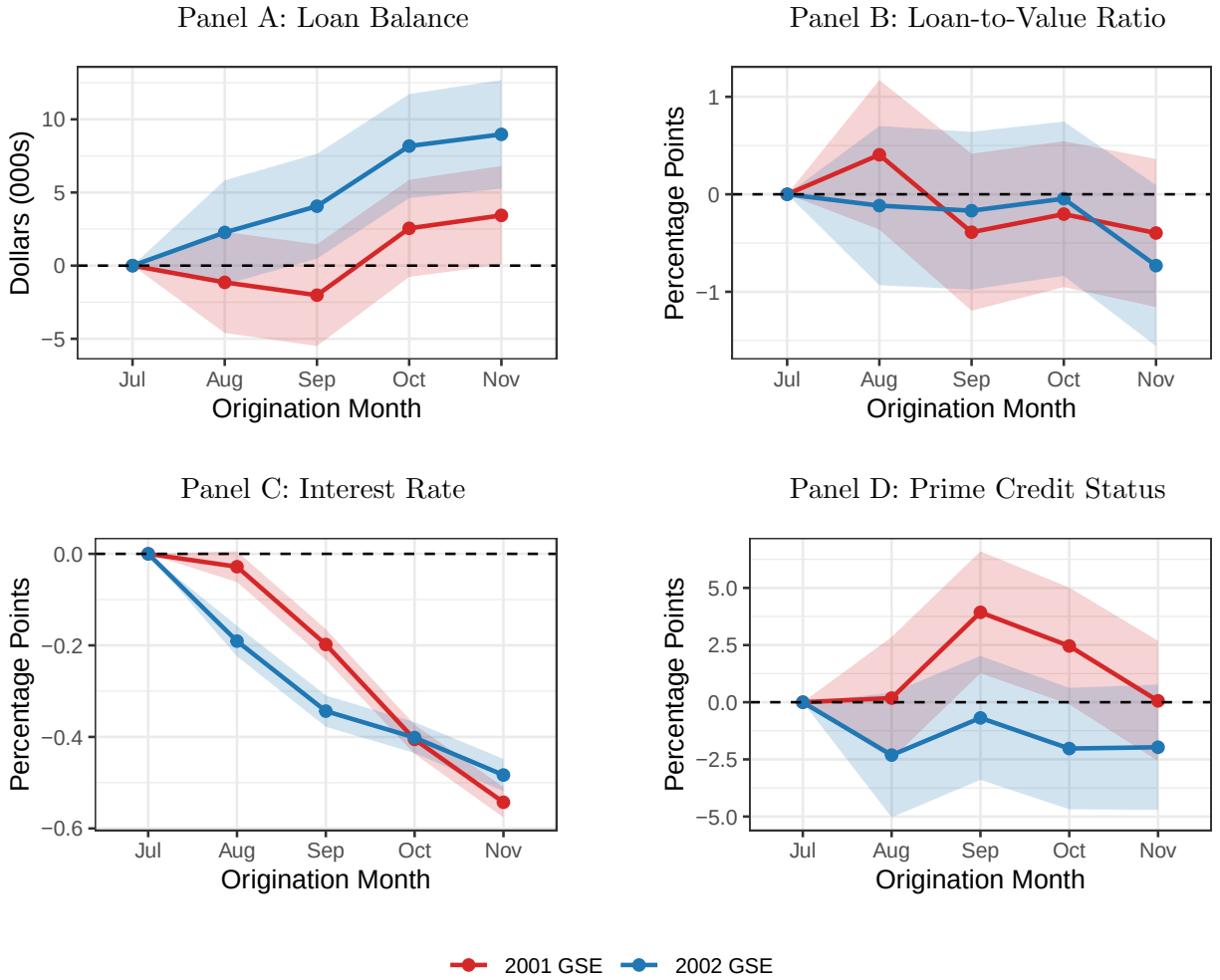
cure a loan when the payoff is high and risk is low. Such loans are the best candidates for re-securitization into a new MBS pool, given issuer reputation concerns and investor demand. When issuers' incentives do not align with re-securitization—for example, because the interest-rate spread is low—concerns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

Figure IA.1: Ginnie Mae Loan Characteristics Across Origination Month and Year



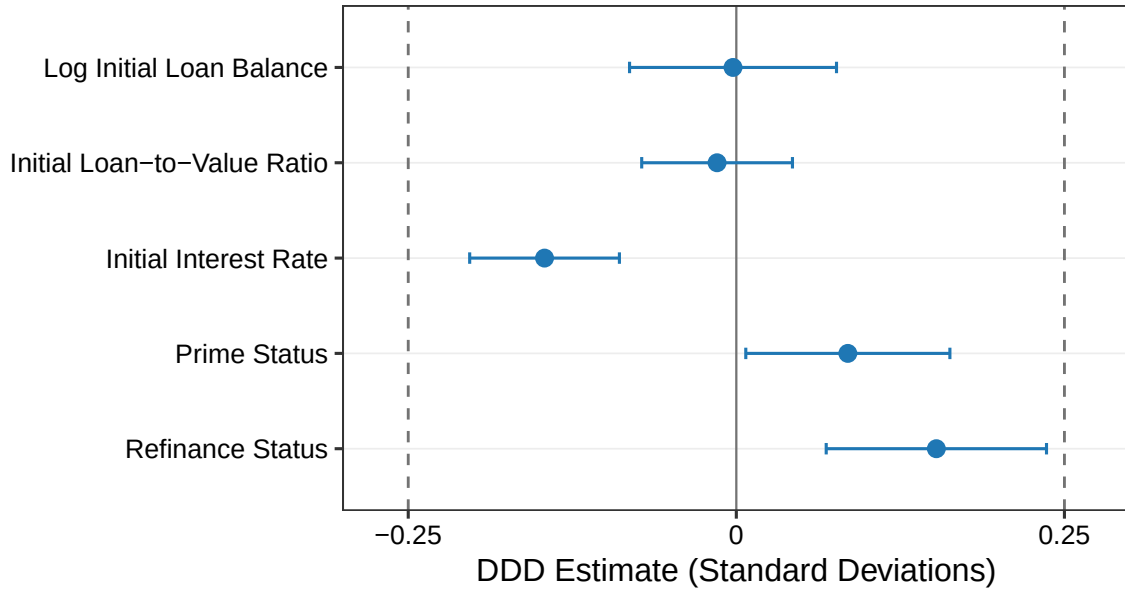
Note: This figure plots baseline-adjusted means of loan characteristics for Ginnie Mae loans originated from July to November of 2001 and 2002. Panel A presents loan balance, Panel B presents loan-to-value at origination, Panel C presents the initial interest rate, and Panel D presents the prime borrower share. For each year, the value plotted for a given month equals that 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 are collected from CoreLogic.

Figure IA.2: GSE Loan Characteristics Across Origination Month and Year



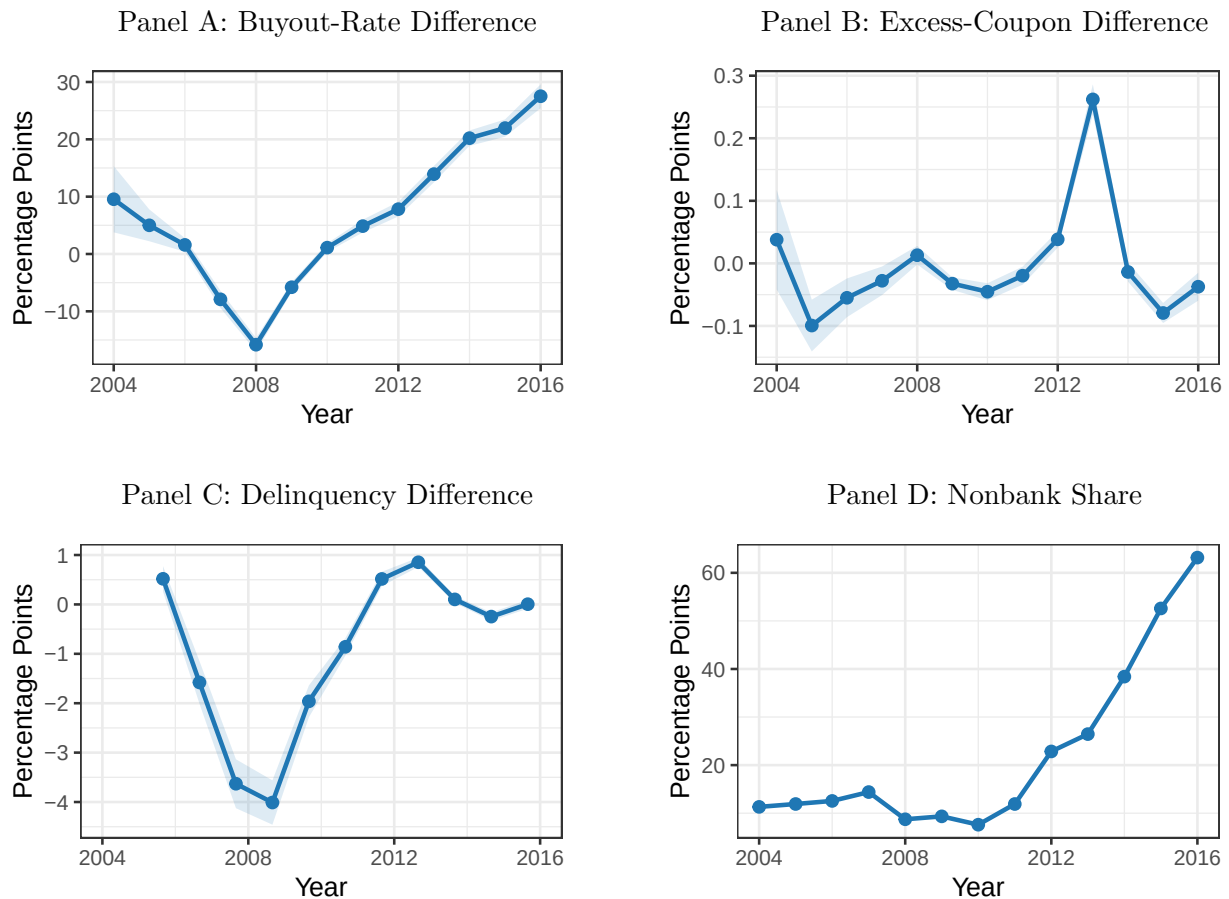
Note: This figure plots baseline-adjusted means of loan characteristics for GSE loans originated from July to November of 2001 and 2002. Panel A presents loan balance, Panel B presents loan-to-value at origination, Panel C presents the initial interest rate, and Panel D presents the prime borrower share. For each year, the value plotted for a given month equals that 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 cover CoreLogic GSE loans originated between July and November of 2001 and 2002.

Figure IA.3: Loan Characteristics in the Triple-Difference Comparison



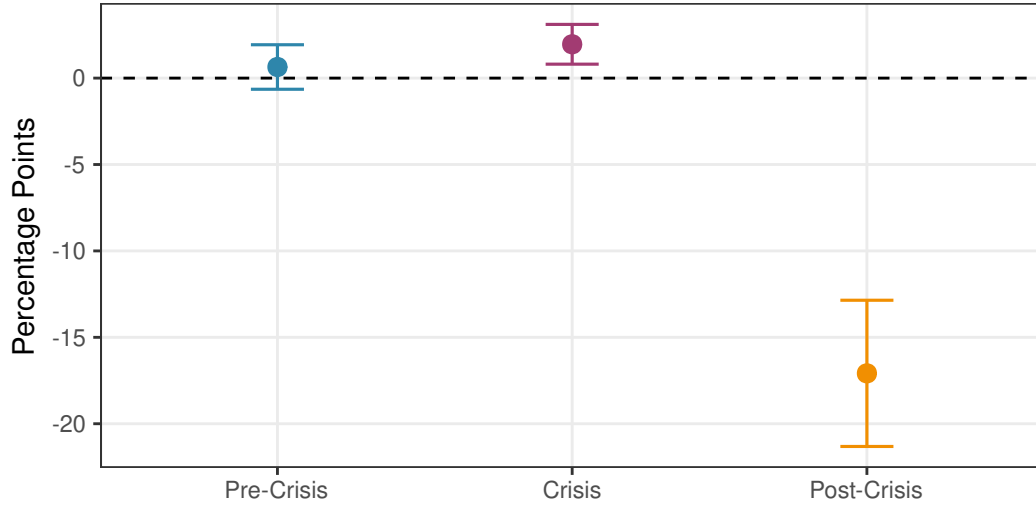
Note: This figure plots triple-difference estimates using log initial loan balance, initial loan-to-value ratio, initial interest rate, prime status, and refinance status as dependent variables. Specifications include all lower-order interactions of origination month, year, and investor segment, without additional covariates or geographic fixed effects. Estimates and 95% confidence intervals, clustered by MSA, are divided by each characteristic's standard deviation in the pooled estimation sample. Dashed lines mark ± 0.25 standard deviations as a reference magnitude for assessing covariate differences (Baker et al., 2026). Data cover Ginnie Mae and GSE loans originated in July–November 2001 and 2002, conditional on qualifying rolling delinquency by July 2007 and above-par status at delinquency.

Figure IA.4: Bank–Nonbank Differences and Nonbank Share of Delinquent Loans



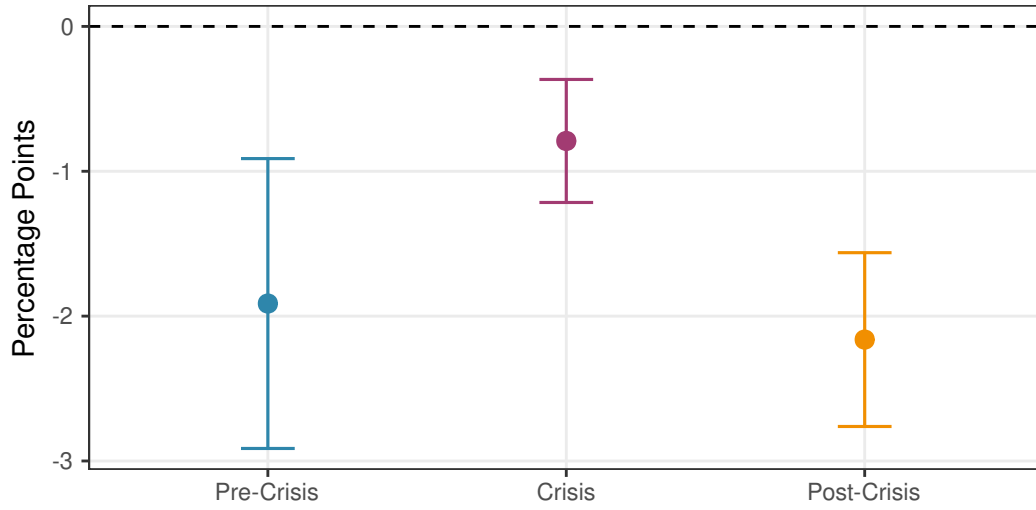
Note: This figure plots bank-minus-nonbank differences by delinquency year. Panel A reports the buyout-rate difference. Panel B reports the excess-coupon difference, defined as the note rate minus the 30-year fixed mortgage rate at the time of delinquency. Panel C reports the 90-day delinquency rate difference by approximate delinquency year, where the origination year is shifted forward by the average time to 90-day delinquency (20 months). Panel D reports the share of matched loans in the 90-day-delinquency sample originated by nonbanks, by origination year. Shaded bands denote 95% confidence intervals computed from difference-in-means standard errors. Panels A to C cover Ginnie Mae FHA loans originated between 2004 and 2014 with delinquency observed within 36 months of origination and through December 2016. Panel D includes origination cohorts through 2016. Bank and nonbank classifications are imputed from HMDA-identified issuer identities.

Figure IA.5: Nonbank Servicer Heterogeneity



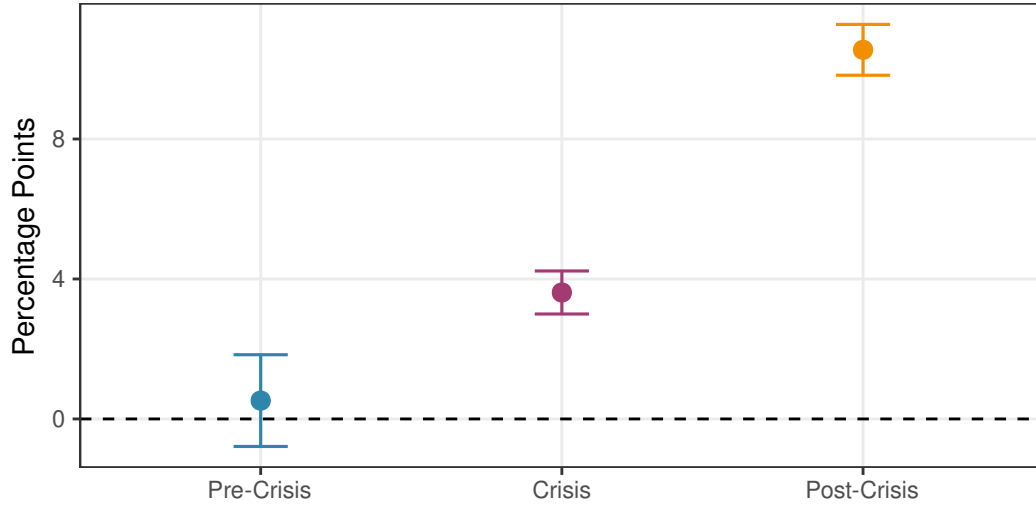
Note: This figure plots estimates from regressions of buyout on nonbank status interacted with period indicators. Observations are at the loan level. The outcome equals one if a loan is bought out of a Ginnie Mae MBS pool within two months of a 90-day delinquency. Estimates are shown separately for three periods: Pre-Crisis (2004 to 2007), Crisis (2008 to 2011), and Post-Crisis (2012 to 2016). Loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, and indicators for primary residence, single-family status, fixed-rate status, refinance status, loan term, low-documentation status, and inferred prime status. All specifications include MSA-by-quarter-of-delinquency and month of origination fixed effects. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the MSA level. Data cover FHA loans originated between 2004 and 2014 and sold to Ginnie Mae, with delinquency observed through December 2016.

Figure IA.6: Buyout Heterogeneity: Aggregated vs. Direct Originations



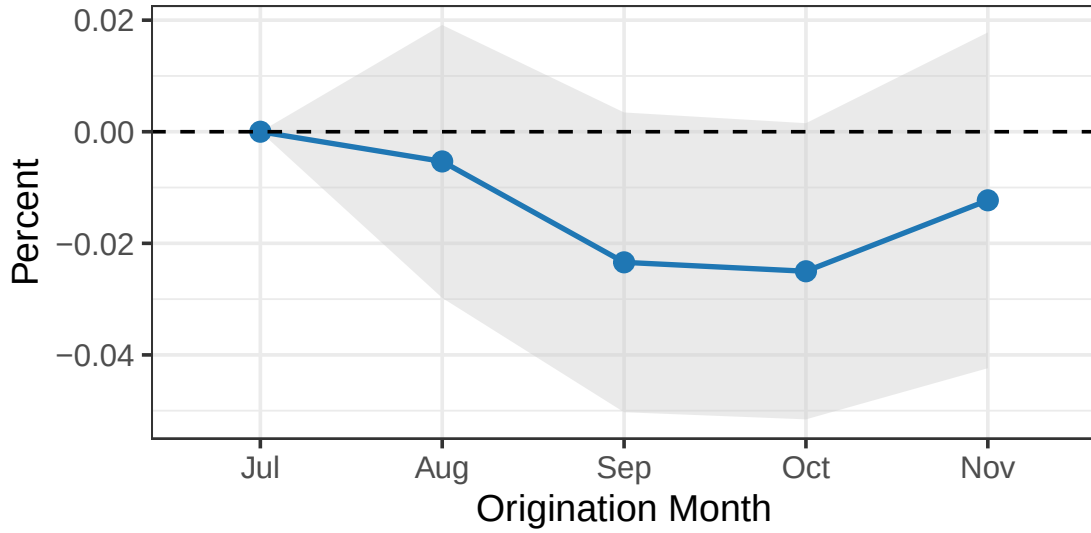
Note: This figure plots estimates from regressions of buyout on aggregated status interacted with period indicators. Observations are at the loan level. The outcome equals one if a loan is bought out of a Ginnie Mae MBS pool within two months of a 90-day delinquency. Estimates are shown separately for three periods: Pre-Crisis (2004 to 2007), Crisis (2008 to 2011), and Post-Crisis (2012 to 2016). Loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, and indicators for primary residence, single-family status, fixed-rate status, refinance status, loan term, low-documentation status, and inferred prime status. All specifications include MSA-by-quarter-of-delinquency, month of origination, and issuer fixed effects. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the MSA level. Data cover FHA loans originated between 2004 and 2014 and sold to Ginnie Mae, with delinquency observed through December 2016.

Figure IA.7: Buyout Heterogeneity: Local Servicer Exposure



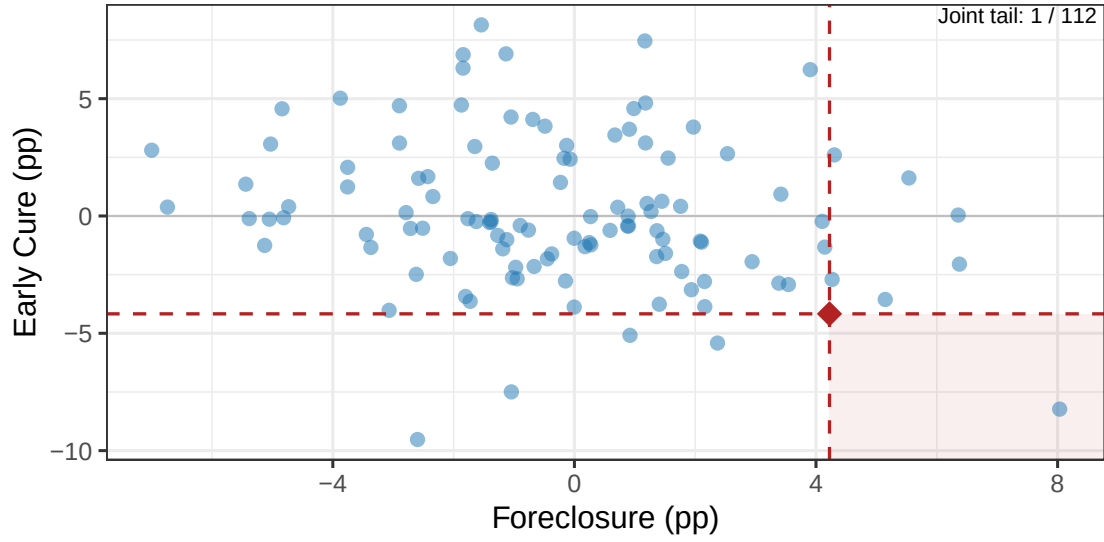
Note: This figure plots estimates from regressions of buyout on top MBS issuer status interacted with period indicators. Observations are at the loan level. The outcome equals one if a loan is bought out of a Ginnie Mae MBS pool within two months of a 90-day delinquency. Estimates are shown separately for three periods: Pre-Crisis (2004 to 2007), Crisis (2008 to 2011), and Post-Crisis (2012 to 2016). Loan-level controls include initial and current interest rates, initial LTV, log initial balance, log current balance, and indicators for primary residence, single-family status, fixed-rate status, refinance status, loan term, low-documentation status, and inferred prime status. All specifications include MSA-by-quarter-of-delinquency, month of origination, and issuer fixed effects. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the MSA level. Data cover FHA loans originated between 2004 and 2014 and sold to Ginnie Mae, with delinquency observed through December 2016.

Figure IA.8: Dynamic Prepayment Effects



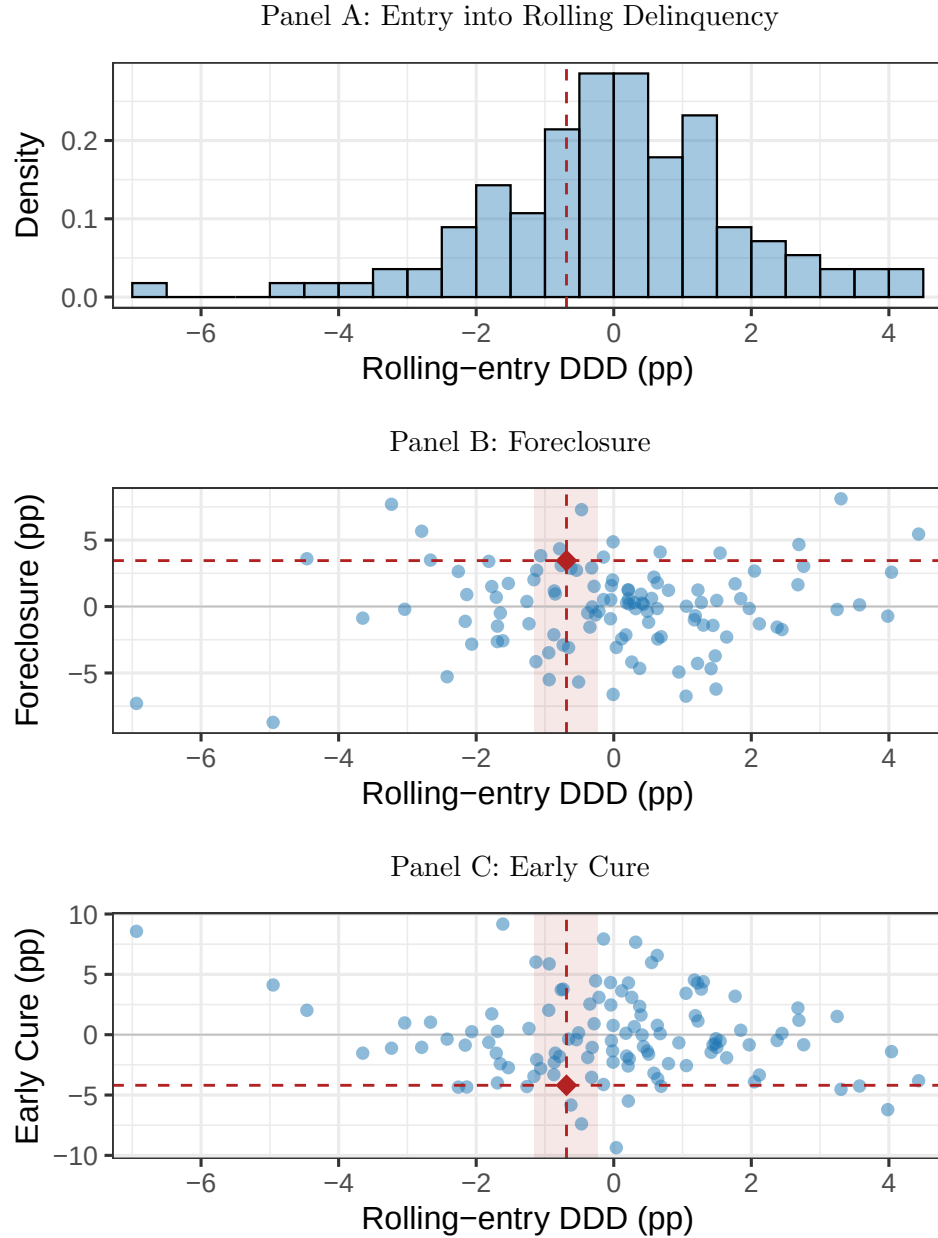
Note: This figure plots dynamic triple-difference estimates from the fully specified model in Column (5) of Table 3. The dependent variable equals one if the loan balance went to zero within three months of a rolling delinquency. Each dot is the estimated coefficient on $Z_t \times Y02 \times GNMA$, 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 cover GNMA and GSE loans originated between July and November of 2001 and 2002, with delinquency observed between the time of origination and July 2007.

Figure IA.9: Placebo Experiments: Joint Scatter Plot



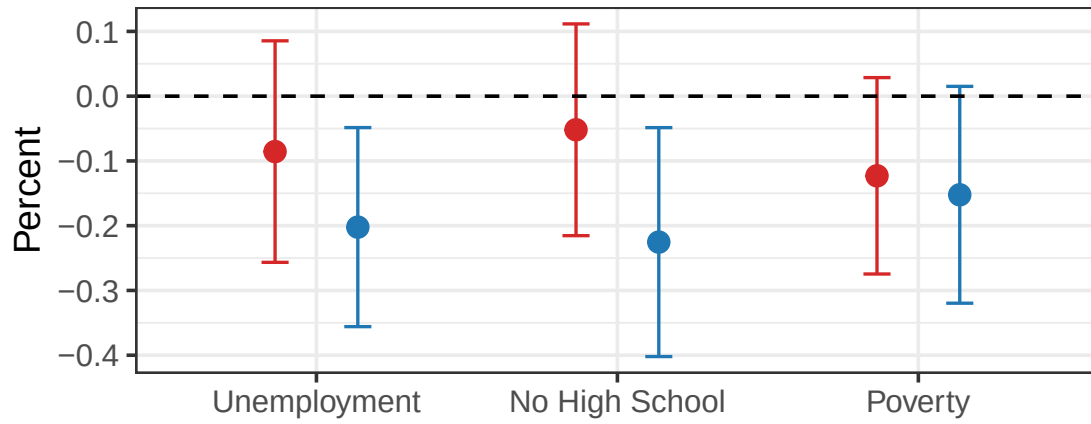
Note: This figure plots the placebo diagnostics following [Young \(2019\)](#), using the controls and fixed effects of Column (5) of Table 2. Each point represents one of 112 alternative five-month origination windows. The horizontal axis plots the placebo triple-difference coefficient for foreclosure, and the vertical axis plots the corresponding coefficient for cure. The identified sample (July–November, 2001–2002) is marked in red. Only 1 out of 112 alternative windows produce joint dynamics as extreme as those identified in the primary sample.

Figure IA.10: Rolling Delinquency and Subsequent Loan Outcomes



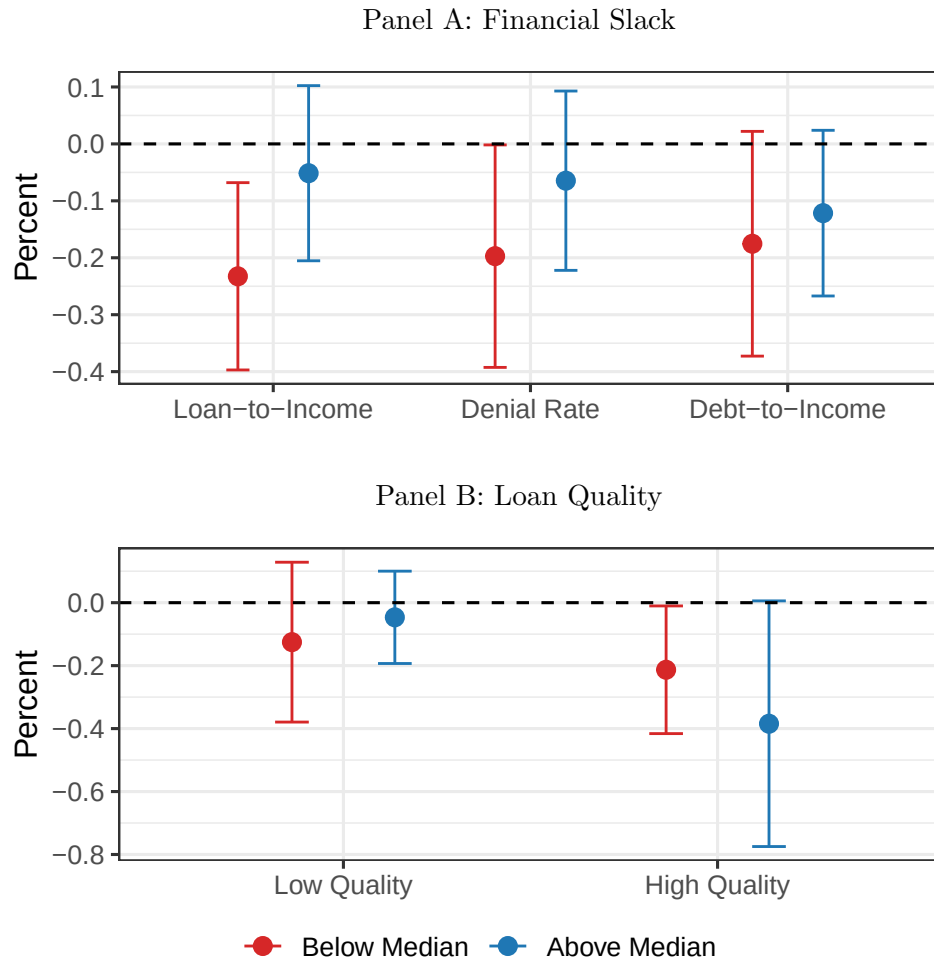
Note: This figure reports triple-difference estimates across 112 alternative five-month origination windows. Panel A plots the distribution of estimates for entry into rolling delinquency, conditional on having entered delinquency. Panels B and C plot these estimates on the horizontal axis against the corresponding foreclosure and early-cure estimates, respectively, on the vertical axis. Red lines and diamonds identify the July–November 2001–2002 comparison. Shading spans ± 0.25 standard deviations.

Figure IA.11: Financial Constraints Heterogeneity



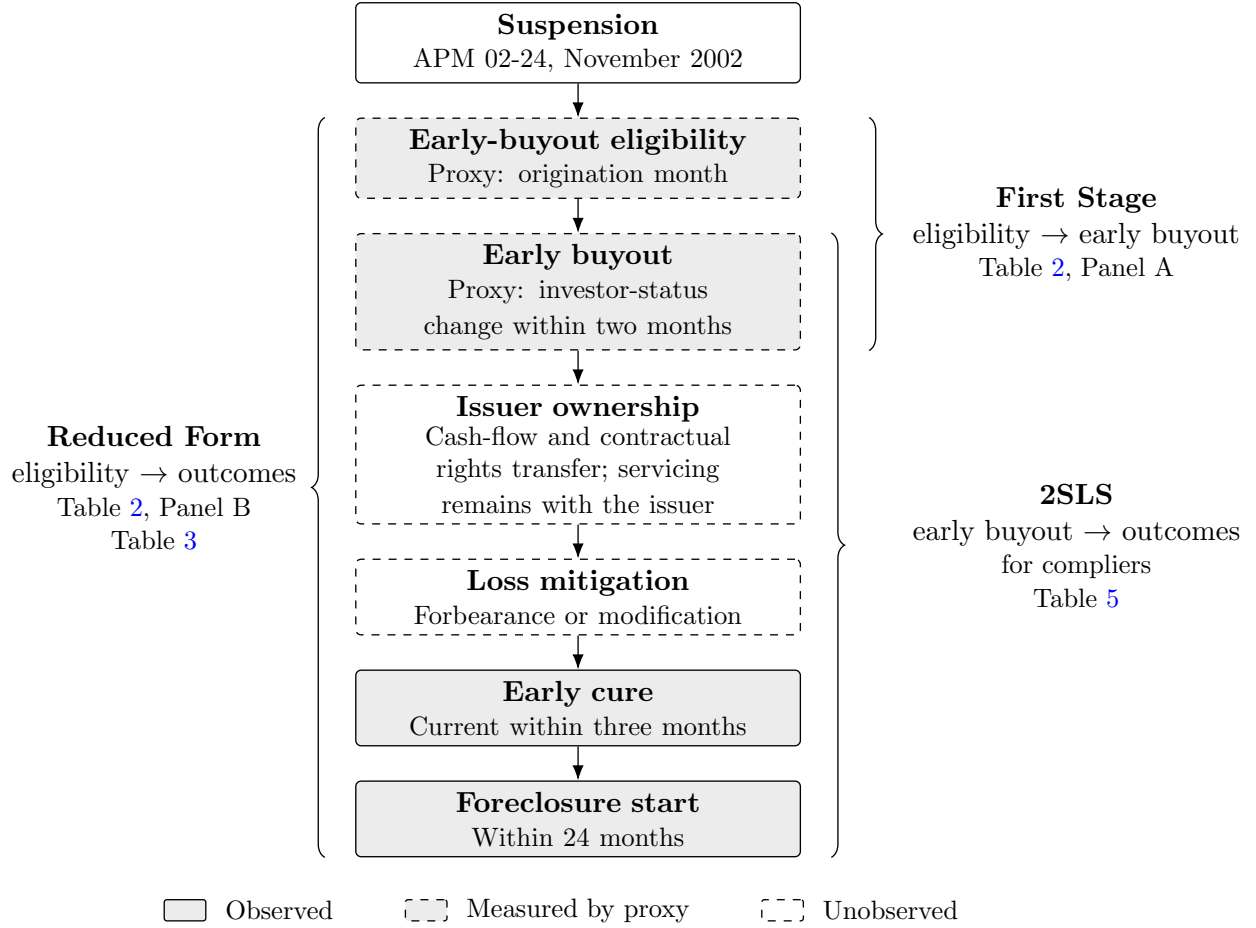
Note: This figure plots estimates from a just-identified 2SLS specification for subsamples defined by borrower financial constraints. The three pairs correspond to county-level measures of education, unemployment, and poverty from the 2000 decennial census. Each red dot corresponds to the effect estimated over a subsample of loans with below-median characteristics, each blue dot corresponds to the above-median subsample, with vertical error bars denoting 95% confidence intervals (clustered by MSA). The specification follows Column (2) of the main IV results, including all lower-order interactions, loan-level controls, and MSA $\times$ Quarter fixed effects. Data cover GNMA and GSE loans originated between July and November of 2001 and 2002, with delinquency observed between the time of origination and July 2007.

Figure IA.12: Effect of Buyout on Foreclosure: Heterogeneity



Note: This figure plots estimates from a 2SLS specification corresponding to Column (2) in Table 5. In Panel A, each pair of estimates corresponds to a subsample split by the indicated measure of financial slack, with red and blue dots denoting below- and above-median values, respectively. In Panel B, each pair corresponds to a subsample split by the interaction of loan quality and coupon spread, with red and blue dots denoting below- and above-median interest-rate spreads. Vertical error bars denote 95% confidence intervals (clustered by MSA). Data cover GNMA and GSE loans originated between July and November of 2001 and 2002, with delinquency observed between the time of origination and July 2007.

Figure IA.13: Empirical Framework and Estimands



Note: This figure illustrates the empirical framework linking the November 2002 suspension of early-buyout eligibility to subsequent loan outcomes following a rolling delinquency. Braces indicate first-stage, reduced-form, and 2SLS estimands. Shaded solid boxes denote variables observed directly in CoreLogic. Shaded dashed boxes denote variables measured using the proxies shown. Unshaded dashed boxes indicate outcomes not directly observed.

Table IA.1: Summary Statistics: GSE Loans

	2002		2001	
	Mean (1)	SD (2)	Mean (3)	SD (4)
Initial Rate	6.68	0.61	7.16	0.59
Original LTV	81.11	13.71	80.55	12.86
Balance (000s)	127.27	61.53	122.57	57.77
15-Year Term	0.10	0.29	0.12	0.32
20-Year Term	0.04	0.20	0.04	0.20
Prime Borrower	0.69	0.46	0.74	0.44
Refinance Loan	0.55	0.50	0.59	0.49
Primary Occupancy	0.90	0.30	0.90	0.31
Single-Family	0.84	0.37	0.86	0.35
Low/No Doc	0.34	0.47	0.27	0.44
Buyout	0.01	0.12	0.00	0.04
Foreclosure	0.19	0.39	0.18	0.39
Cure	0.53	0.50	0.53	0.50
Modification	0.00	0.02	0.00	0.01
Prepayment	0.06	0.24	0.06	0.24
Observations	11,547		11,594	

Note: This table reports mean and standard deviation values of loan characteristics and outcomes for GSE loans, conditional on rolling delinquency and above-par status. Columns (1) and (2) present statistics for loans originated in 2002. Columns (3) and (4) present statistics for loans originated in 2001. Foreclosure equals one if a loan experiences a foreclosure start within 24 months after rolling delinquency.

Table IA.2: Summary Statistics: Pre-Crisis Sample (2004–2007)

	All Loans		Buyout		No Buyout	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
Initial Rate	6.11	0.66	6.37	0.66	6.02	0.64
Original LTV	96.05	6.95	96.15	6.83	96.02	6.99
Balance (000s)	124.92	54.40	120.68	54.38	126.43	54.33
15-Year Term	0.02	0.14	0.01	0.10	0.02	0.15
20-Year Term	0.03	0.16	0.04	0.20	0.02	0.14
Prime Borrower	0.31	0.46	0.32	0.47	0.30	0.46
Refinance Loan	0.26	0.44	0.25	0.43	0.27	0.44
Primary Occupancy	0.71	0.45	0.61	0.49	0.75	0.43
Single-Family	0.79	0.41	0.80	0.40	0.79	0.41
Low/No Doc	0.25	0.43	0.15	0.35	0.28	0.45
Observations	91,801		24,188		67,613	

Note: This table reports mean and standard deviation values of loan characteristics for FHA loans sold to Ginnie Mae, conditional on 90-day delinquency. The sample covers loans with 90-day delinquency through December 2007. Buyout equals one if a loan is bought out of a Ginnie Mae MBS pool within two months of 90-day delinquency. Data cover FHA loans originated between 2004 and 2014 sold to Ginnie Mae with delinquency observed through December 2016.

Table IA.3: Summary Statistics: Crisis Sample (2008–2011)

	All Loans		Buyout		No Buyout	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
Initial Rate	5.98	0.72	5.99	0.70	5.98	0.75
Original LTV	94.89	7.77	95.03	7.43	94.69	8.21
Balance (000s)	176.14	84.96	176.67	84.69	175.43	85.31
15-Year Term	0.01	0.12	0.01	0.11	0.02	0.14
20-Year Term	0.00	0.05	0.00	0.05	0.00	0.05
Prime Borrower	0.47	0.50	0.50	0.50	0.42	0.49
Refinance Loan	0.47	0.50	0.46	0.50	0.47	0.50
Primary Occupancy	0.98	0.13	0.99	0.11	0.97	0.16
Single-Family	0.79	0.41	0.80	0.40	0.77	0.42
Low/No Doc	0.27	0.44	0.25	0.43	0.29	0.46
Observations	471,944		270,299		201,645	

Note: This table reports mean and standard deviation values of loan characteristics for FHA loans sold to Ginnie Mae, conditional on 90-day delinquency. The sample covers loans with 90-day delinquency between January 2008 and December 2011. Buyout equals one if a loan is bought out of a Ginnie Mae MBS pool within two months of 90-day delinquency. Data cover FHA loans originated between 2004 and 2014 sold to Ginnie Mae with delinquency observed through December 2016.

Table IA.4: Summary Statistics: Post-Crisis Sample (2012–2016)

	All Loans		Buyout		No Buyout	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
Initial Rate	4.51	0.66	4.42	0.64	4.66	0.67
Original LTV	95.27	6.93	95.43	6.59	95.00	7.45
Balance (000s)	176.61	89.34	172.35	86.49	183.59	93.40
15-Year Term	0.02	0.14	0.02	0.13	0.03	0.17
20-Year Term	0.00	0.05	0.00	0.05	0.00	0.05
Prime Borrower	0.82	0.39	0.84	0.37	0.78	0.41
Refinance Loan	0.37	0.48	0.33	0.47	0.44	0.50
Primary Occupancy	1.00	0.06	1.00	0.06	1.00	0.05
Single-Family	0.80	0.40	0.83	0.38	0.76	0.43
Low/No Doc	0.24	0.43	0.21	0.41	0.29	0.45
Observations	171,279		106,256		65,023	

Note: This table reports mean and standard deviation values of loan characteristics for FHA loans sold to Ginnie Mae, conditional on 90-day delinquency. The sample covers loans with 90-day delinquency between January 2012 and December 2016. Buyout equals one if a loan is bought out of a Ginnie Mae MBS pool within two months of 90-day delinquency. Data cover FHA loans originated between 2004 and 2014 sold to Ginnie Mae with delinquency observed through December 2016.

Table IA.5: Effect of the GNMA Policy on Loan Outcomes: Lender Fixed Effects

	Buyout (1)	Foreclosure (2)	Cure (3)	Modification (4)	Prepayment (5)
$Z_{t \geq 10} \times Y02 \times \text{GNMA}$	-0.305*** (0.017)	0.036* (0.021)	-0.046* (0.025)	0.001 (0.001)	-0.008 (0.010)
Interactions	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
MSA $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes
Observations	31,505	31,505	31,505	31,505	31,505

Note: This table reports OLS estimates of the effect of the 2002 Ginnie Mae memorandum on loan outcomes using the HMDA-identified issuer identities. Observations are at the loan level. In Column (1), the outcome equals one if a loan is bought out within two months after a rolling delinquency. In Column (2), the outcome equals one if a loan enters foreclosure within 24 months after a rolling delinquency. In Column (3), the outcome equals one if a loan exits delinquency within three months after a rolling delinquency. In Column (4), the outcome equals one if a loan experiences a modification within three months after a rolling delinquency. In Column (5), the outcome equals one if a loan experiences a prepayment within three 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, fixed-rate status, documentation type, loan purpose, and property type. Local economic controls at the time of delinquency include the log of the ZIP3 house-price index and the logs of county GDP, population, and income, in addition to the county unemployment rate. All specifications absorb lower-order interactions of origination month, year, and GNMA status. Lender fixed effects absorb the HMDA holding-company identity of the loan servicer. Standard errors (in parentheses) are clustered at the MSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data cover the CoreLogic sample with imputed lender indicators of GNMA and GSE loans originated between July and November of 2001 and 2002, with delinquency observed between the time of origination and July 2007.

Table IA.6: Effect of the GNMA Policy on Loan Outcomes: Logit Triple Difference

	Buyout (1)	Foreclosure (2)	Cure (3)
$Z_{t \geq 10} \times Y02 \times \text{GNMA}$	-3.917*** (0.826)	0.307** (0.132)	-0.182** (0.087)
<i>Average Discrete Effects</i>			
Policy effect	-0.5558*** (0.0376)	0.0461** (0.0186)	-0.0426** (0.0205)
Interactions	Yes	Yes	Yes
Controls	Yes	Yes	Yes
MSA $\times$ Quarter FE	Yes	Yes	Yes
Observations	37,676	36,323	40,087

Note: This table reports logit estimates of the effect of the 2002 Ginnie Mae memorandum on loan outcomes with MSA $\times$ Quarter fixed effects. Observations are at the loan level. The top panel reports log-odds coefficients on the triple interaction $Z_{t \geq 10} \times Y02 \times \text{GNMA}$. The bottom panel reports average discrete effects on predicted probabilities for Ginnie Mae loans originated in October–November 2002 and retained in each regression. Each effect compares the fitted probability with the estimated triple-interaction contribution to the counterfactual probability without that contribution, holding all other model components fixed. Standard errors for these average effects are computed using the delta method with the MSA-clustered covariance matrix. In Column (1), the outcome equals one if a loan is bought out within two months after a rolling delinquency. In Column (2), the outcome equals one if a loan enters foreclosure within 24 months after a rolling delinquency. In Column (3), the outcome equals one if a loan exits delinquency within three 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, fixed-rate status, documentation type, loan purpose, and property type. Local economic controls at the time of delinquency include the log of the ZIP3 house-price index and the logs of county GDP, population, and income, in addition to the county unemployment rate. 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 cover all loans originated between July and November of 2001 and 2002 sold to Ginnie Mae or the GSEs, with delinquency observed between the time of origination and July 2007.

Table IA.7: Institutional Roles and Buyout Incentives

Actor	Role	Decisions	Costs and benefits
Ginnie Mae issuer, loan in pool	Pools loans, issues MBS, and retains responsibility for servicing (may delegate to subservicer)	Can buy out a loan after a qualifying delinquency; can offer forbearance after filing a partial claim; cannot modify loan terms	Must advance principal and interest to investors; must maintain delinquency below regulatory thresholds; must pay financing and labor costs to buy out a loan
Ginnie Mae issuer, after buyout	Owns the loan outright and still retains responsibility for servicing (may delegate to subservicer)	Can offer forbearance after filing a partial claim; can modify loan terms; can resecuritize loan after cure	Buys out the loan at par; relieved of advancing payments to investors; must service any external financing used for buyout
Subservicer	Hired by issuer to engage in day-to-day servicing operations	Performs servicing and loss mitigation; does not independently hold the buyout option	Receives servicing fees, may advance funds; issuer is still directly responsible to Ginnie Mae
Ginnie Mae MBS investors	Hold pass-through MBS securities backed by loan pools	No direct decision making	Receive principal and interest payments, guaranteed by Ginnie Mae; suffer prepayment risk; paid out at par for loan buyout
Ginnie Mae	Guarantees timely payment of principal and interest to investors	Sets pooling and buyout eligibility; regulates issuers	Exposed to risk that issuers fail to pass through payments to investors
FHA/HUD	Insures loans in case of default	Sets loss mitigation eligibility; pays out insurance claims on delinquent loans	Exposed to credit risk of individual loans; receives a junior lien after paying out a partial claim (i.e., insurance claim)
GSEs	Pool loans, issue MBS, and delegate servicing to subservicer	Set servicing requirements and authorize or execute loan repurchases	Exposed to credit risk of individual loans; must advance principal and interest to investors; must pay financing and labor costs to buy out a loan
GSE servicers	Hired by GSEs to engage in day-to-day servicing operations	Performs servicing and loss mitigation; does not independently hold the buyout option	Receives servicing fees, may advance funds

Note: This table reports the roles, decisions, costs, and benefits of the institutions involved in issuing and servicing mortgage-backed securities guaranteed by Ginnie Mae, Fannie Mae, and Freddie Mac. The allocation of servicing tasks and costs may vary across contracts and over time. An issuer that delegates servicing tasks remains responsible to Ginnie Mae. Buyouts are described in Section 2.