Yellow Light Foreclosures: Collateral Enforcement Under Delayed Debt Sale

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Abstract

This paper studies how delayed debt sale affects foreclosure and renegotiation. To disentangle the effect of loan sales from that of loan quality, I exploit an unanticipated suspension in loan buyouts for Ginnie Mae-securitized mortgages early delinquency. The ban suspended buyouts for loans experiencing a rolling 30-day delinquency but still allowed buyouts for 90-day delinquencies. I find that treated loans experienced an 18.6 percentage point decline in buyouts relative to untreated loans. These same loans subsequently experienced a 2.7 percentage point increase in foreclosures and a 2.8 percentage point reduction in cures. The effect on cures breaks down at longer horizons, suggesting that the delay in debt sale prevents curing the loan fast enough to avoid foreclosure. Finally, placebo tests and pre-trends reinforce the exclusion restriction.

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1 Introduction

The enforceability of creditor rights is fundamental to determining the credit supply (Pence (2006), Rampini and Viswanathan (2010)). While lending decisions are made ex-ante, a creditor's security interest is enforced ex-post following a debtor's default. At that point, creditors face a choice of either renegotiating the terms of their debt contract or liquidating the associated collateral. However, recent work demonstrates that creditors may choose a third option—selling their debt when other options become costly. This paper aims to extend this insight by studying how the sale of debt following borrower delinquency affects loan outcomes. In particular, this paper will focus on a friction in lending markets, namely delays in debt sale.

Empirically studying the effect of loan sales on foreclosure and renegotiation is challenging due to asymmetric information. A rich body of work explores how the selling of newly originated loans is riddled by a lemons problem (Adelino et al. (2019), Begley and Purnanandam (2016) To signal loan quality, buyers demand costly signals, such as a seller's equity stake or longer hold times. Naturally, informational asymmetries are amplified when a borrower defaults. For the econometrician, therefore, a primary challenge is disentangling the effect delinquent loan sale from that of loan quality. If creditors only sell low quality loans, then the relationship between loan sale and loan performance will be biased down. Alternatively, if creditors sell the best performing loans due to reputational concerns, then the opposite will be true.

To overcome this challenge, I exploit a policy change in the Ginnie Mae mortgage market related precisely to delinquent loan purchase. When a Ginnie Mae bond issuer securitizes a pool of loans, the issuer retains the right to buy out any individual mortgage from Ginnie Mae bond investors under one of the following scenarios: (i) when a loan holds the status of at least one missed payment for a consecutive four-month period (rolling delinquency) or (ii) when a loan enters a status of three missed payments (serious delinquency). In November 2002, however, Ginnie Mae abruptly released a memorandum that eliminated the buy out option under scenario (i) for all loans securitized as of January 1, 2003.

Exploiting this policy change, I compare outcomes for loans originated closer to the memorandum and those originated earlier in the year. Identification requires that the vintage month not directly correlate with loan performance except through buyout. Using month of origination satisfies this conditions in several ways. First, based on conversations with a former Ginnie Mae executive, the memorandum was intentionally announced in a manner that would prevent market anticipation. Hence, loans originated shortly before the announcement were similar to those originated earlier in the year but for the probability of buyout. Second, the former executive confirmed that the policy change was not precipitated by any fraudulent behavior or material loan quality. By definition, the buyout suspension was assigned independent of loan outcomes. To further corroborate this, I provide graphical evidence that ex-ante loan characteristics and ex-post delinquency does not experience any stark change across vintage months. Third, I restrict identification to within-delinquency month and within-MSA variation. Theoretically, outcomes should not differ significantly for loans that experience equivalent macroeconomic conditions at the time of a rolling delinquency and differ in vintage by only a short span of time. Finally, I rely upon placebo tests and pre-trends to demonstrate that vintage month does not correlate with loan outcomes except when loans are treated with exposure to the policy shock.

In what effectively serves as the first stage of my analysis, I find that buyout probability precipitously falls for loans originated in the months immediately preceding the policy shock relative to loans originated months earlier. Exploiting within-MSA and within-delinquency time variation, I estimate the differential effect of vintage month on buyout rates, conditional on observables. Given a baseline buyout rate of 52%, I find that the buyout rate for loans issued in the two-month period immediately preceding the policy announcement experienced a 18.6 percentage point reduction. The estimate for the last vintage month alone represents close to a 60% reduction relative to the baseline. In sharp contrast, the buyout rate for 90-day delinquent loans experienced no break in trend for months before and after the policy announcement, as shown in Figure 1. Based on estimates of buyout activity earlier in the calendar year, my first-stage estimates strongly suggest that the buyout rate would have evolved in parallel with seriously delinquent loans, if not for the Ginnie Mae policy change.

Given the stark drop in buyout activity preceding the policy, I next estimate the differential effect of vintage month on foreclosure 18 months following a rolling delinquency. Exploiting variation within-delinquency time, I find that Ginnie Mae loans experience a monotonic increase in foreclosures across vintage month, peaking to a 5.7 percentage point increase relative to the earliest month in my sample. Widening the control group to include the first four months of my sample, I find that the last two months experience a differentially 2.6 percentage point higher foreclosure rate. Taken together, the drop in the buyout rate induced by the policy change coincided with an increase in the foreclosure rate in the months preceding the announcement.

Exploring the mechanism, I find that within three months of a rolling delinquency, loans exit delinquency differentially less in the months immediately prior to the policy announcement. I measure cures using a three month window immediately following a rolling delinquency in order to differentiate these early cures from those that follow a buyout due to serious delinquency. I find that point estimates of the differential effect decline monotonically to -7.4 percentage points. Under the null that the last two-month period should experience no significant deviation from the first four vintage months in my sample, I find a statistically significant differential decline of 4.6 percentage points.

Next, I confirm that the reduction in early cures is related to the suspension of buyouts. To this end, I estimate the differential effect of vintage month on late cures using a longer 12-month window following rolling delinquency. If issuers still retained the buyout option for loans experiencing a serious delinquency, then I should see the differential effect from my previous exercise attenuate using a longer horizon, capturing the timing of two more missed payments. Along these lines, I find that most vintage months exhibit no statistically significant differential effect. The two-month estimate is 40% smaller than the estimate for early cures. Essentially, the effect of the buyout suspensions appears to affect foreclosure through early cures, not cures in general.

Given that the reduction in early cures, as opposed to late cures, appears to drive the increase in foreclosures, I investigate how early intervention prevents loan quality from deteriorating. I find that the reduction in early cures is uncorrelated with changes to a borrower's interest rate, principal balance, and required payment. Instead, point estimates for the differential effect of vintage month on modifications and payment changes are close to zero and generally statistically insignificant. This implies that the reduction in early cures must be related to a reduction in either term extensions or payment deferrals, two alternatives not measured in the data. Of course, there remains the possibility that modifications occur in a manner undetected by my sample.

My analysis rests upon the key identifying assumption that an omitted variable, such as loan quality, does not correlate with both vintage month and loan performance. This may be violated if, for example, less credit-worthy borrowers happen to obtain Ginnie Mae loans right before the policy announcement. Naturally, this would bias my estimates, spuriously indicating that loans experience higher foreclosures precisely in vintage months preceding the 2002 memorandum. While evidence using ex-ante loan characteristics, ex-post loan quality, and the nature of the shock suggest that this is unlikely, I provide several additional robustness checks to further validate my research design.

First, I re-estimate my main specifications across several placebo samples. In an initial test, I construct a sample of loans originated prior to a 2003 placebo shock, one year after the true policy announcement. Since the buyout suspension was in full effect by 2003, loans issued around the 2003 placebo shock should experience no variation in rolling delinquency buyout. In a second set of placebo tests, I construct a sample of loans for which there exists no buyout incentive. These loans have a value below par at the time of delinquency, whereby the initial interest rate is below the average market rate at the time of delinquency.

Across both samples, I test whether vintage month differentially affects loan outcomes when either buyout is entirely suspended or the incentive to buyout does not exist. Indeed, I find no systematic pattern in variation across origination time, validating my key identifying assumption that vintage month does not correlate with long-term loan outcomes, except through treatment. The point estimates for the final two-month period in both samples are statistically and economically indistinguishable from zero across all placebo tests.

In order to further validate my research design, I exploit a wider sample of vintage months to test for the presence of pre-trends. I expand my sample to the entire calendar year of origination and provide graphical evidence on differential effects in loan outcomes prior to the start of my primary sample. If loan outcomes differed across origination time in later vintage months, then outcomes should also vary earlier in the calendar year. I find that there exists no meaningful pre-trends across both 2002 and 2003 issued Ginnie Mae loans. Instead I find a monotonic pattern in loan outcomes only in the last months of the 2002 vintage sample, consisting of precisely the loans exposed to treatment. The limited variation in early vintage months combined with a systematic pattern in later 2002 vintages makes the possibility of another confounding channel less likely.

Finally, I estimate my main specification using an analogous sample of GSE loans issued in 2002. If my main results were due to unobserved correlation between loan performance and vintage month unrelated to the Ginnie Mae announcement, then I should identify this differential effect using a sample of non-Ginnie Mae loans, as well. Instead, using the GSE sample, I find point estimates statistically insignificant and close to zero for the two-month period preceding the policy shock across all tests. In my most demanding specification, I use a pooled sample of 2002 issued Ginnie Mae and GSE loans, estimating a specification that interacts vintage month with Ginnie Mae status. I identify the treatment effect of the Ginnie Mae policy across vintage month using vintage month fixed effects. This specification takes advantage of the pooled nature of the sample and accounts for common shocks to vintage month directly. Here, I find symmetric point estimates, whereby the differential increase in foreclosure mirrors the differential decline in early cures. In the two-month period preceding the policy announcement, Ginnie Mae loans experience a 2.7 percentage point increase in foreclosures and 2.8 percentage point decline in early cures. The equivalency across point estimates strongly suggests that buyouts reduce foreclosures due to early cures.

I conclude by employing an instrumental variables (IV) approach that exploits the quasi-experimental variation in buyout propensity preceding the Ginnie Mae shock. Using an IV approach is useful as it provides (i) policy-relevant causal estimates of the elasticity of buyout and loan performance and (ii) a framework to interpret the external validity of my results. I estimate two-stage least squares (2SLS) regressions using vintage month as the set of excluded instruments. Under the assumption that vintage month affects loan outcomes only through quasi-random variation in buyout propensity, I estimate that buyout reduces the probability of foreclosure by at least 11.6 percentage points and increases early cures by at least 13 percentage points. Given that my research design exploits the suspension of buyout, this one-sided non-compliance facilitates an interpretation of average treatment effect on the treated. In words, my estimates represent the degree to which buyout reduces foreclosure for those loans that ultimately experience buyout. Finally, exploring the heterogeneity, I find that financially constrained borrowers and borrowers with unencumbered collateral benefit most from buyout. Ultimately, creditors appear to respond to financial incentives, whereby loans providing the largest interest rate spread reperform most effectively after initial delinquency.

Literature Review

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

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

Adelino et al. (2019) operationalize this insight and find that loans sold earlier perform worse than loans sold later in the private label securities market. The authors show that signaling is most important for those loans with limited hard information to identify quality.

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

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

This paper contributes to previous work by studying the buyout option, a particular feature of the Ginnie Mae mortgage market. Importantly, this option exists for all agency debt when a loan experiences serious delinquency. While $8 trillion worth of outstanding agency debt retain this buyout option, there remains a dearth of research on its consequences. Relevant to the current paper, Bandyopadhyay et al. (2021) find that creditors buy back loans in Ginnie Mae securitization pools when the loan has a relatively higher interest rate spread over the ten-year treasury rate. The paper provides suggestive evidence that issuers remove loans in order to re-securitize them under declining interest rate environments. The authors posit that buyout creates a friction between servicers and borrowers due to a loan's change of ownership from origination to securitization to buyout and then to re-securitization again. The current paper aims to extend this research by asking how buyouts affects loan performance directly.

The remainder of this paper proceeds as follows. Section 2 describes the institutional background of the Ginnie Mae loan market and the delinquency buyout option. Section 3 details the data, sample construction, and summary statistics. Section 4 estimates the baseline relationship between buyout and loan performance. Section 5 estimates the effect of the Ginnie Mae policy change on buyout and foreclosure. Section 6 explores the mechanism underlying the increase in foreclosures. Section 7 tests the robustness of my main results. Section 8 implements an IV approach that estimates the causal relationship between buyout and loan performance. Section 9 concludes.

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 Authority (FHA), Department of Veteran 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 platform while issuers continue to act as custodians over timely payment to investors. In contrast, the Government-Sponsored Enterprises (GSE's) purchase the loans outright from lenders.

2.2 The Delinquency Buyout Option

Issuers of Ginnie Mae and GSE MBS retain a right to buy out loans from the securitized mortgage pool when a loan experiences a 90-day delinquency, also know as serious delinquency. This means that the issuer may return the remaining principal balance to investors at par in order to purchase the loan. Furthermore, within the Ginnie Mae market, an issuer may re-securitize a buyout loan when it begins to reperform.

In addition to the 90-day delinquency condition, prior to 2003, Ginnie Mae allowed loan buyouts for a rolling delinquency. This type of delinquency occurred when a borrower remained non-current for four periods. As an extreme example of this, a borrower might miss a payment in period one while servicing payments in periods two, three, and four. As long as the borrower failed to make investors whole by the fourth period, the issuer had the right to buy the loan out of the pool prior to the traditional 90-day delinquency measure.

Ginnie Mae intended that the buyout option on delinquent mortgages target severe cases of deterioration in loan quality. Providing this option benefits investors through timely repayment, helps issuers maintain pool quality, and uplifts the broader market by lowering Ginnie Mae's exposure to bad loans. However, allowing issuers to retain a buyout option naturally lowers MBS prices. In general, prepayment lowers the price of a bond since investors demand lower prices to compensate for faster pay-off speeds. Buyouts function in the same manner and therefore exacerbate this problem. While this discount is worthwhile for 90-day delinquent mortgages, rolling delinquencies do not necessarily fulfill the spirit of the policy. Rolling delinquency borrowers by definition pay every period except for a single missed payment at least four months earlier. Therefore, these loans contribute far less risk to the broader market than seriously delinquent loans both due to the level of delinquency and the probability of further deterioration. Hence, the systematic benefit of buying out rolling delinquencies is unclear.

With this context in mind, investors petitioned Ginnie Mae to remove the buyout option in order to raise MBS prices. Ginnie Mae found the argument to suspend the buyout option compelling, especially given that lower bond prices would hurt its mission to facilitate housing finance. On November 6, 2002, Ginnie Mae's Executive Vice President, George Anderson, released All Participants Memorandum 02-24, which restricted buyout to exclusively loans that were at least 90 days delinquent. This applied to all new Ginnie Mae bond issuance as of January 1, 2003.

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 6 months in order to provide all participants ample time to adjust to a new setting and prevent operational failure. In stark contrast, one Ginnie Mae executive close to the matter described the 2002 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 exploiting the letter of the law in the first place.

The Ginnie Mae executive confirmed that the policy change was not precipitated by fraudulent exercise of the option. Ginnie Mae would regularly conduct field reviews to ensure that issuers were fulfilling all responsibilities associated with general Ginnie Mae policy. Of course, these reviews would include auditng for compliance with the buyout policy. An article in HousingWire corroborates this, confirming that early Ginnie Mae audits found no abuse of the rolling delinquency buyout option[1]. Ultimately, while issuers failed to fulfill the spirit of the original policy, the Ginnie Mae announcement was not precipitated by fraudulent behavior.

Importantly, the Ginnie Mae executive also confirmed that the policy change was unrelated to loan performance, such as delinquencies or foreclosures. In fact, Ginnie Mae at the time was not monitoring delinquencies and payoffs in a manner similar to today. Since issuers would continue to advance payments to investors after a mortgage default, Ginnie Mae simply ensured that issuers were current on the pass through to investors. Ultimately, the policy change was implemented independent of loan outcomes.

For mortgages originated early in 2002, the policy change had no material effect. These loans likely securitized well before the January 1, 2003 deadline. Therefore, issuers of these loans still retained an option to buy out the mortgage following a rolling delinquency. For loans experiencing a 90-day delinquency, there was no change to the option to buy out the mortgage, both preceding and following the policy announcement. This was due to the fact that the policy applied to rolling delinquencies, in particular. Ultimately, the loans most affected by this policy were precisely those mortgages originated closer to the timing of the memorandum. For these loans, there was an increasingly small chance of securitizing before the January deadline.

The intuition underlying this change is best captures by Figure 1. Here, I plot the buyout rate for loans across vintage month and delinquency type, both rolling delinquency and serious delinquency. Up until July 2002, buyout rates appear indistinguishable across either delinquency type for each month of origination. However, starting August 2002, the buyout rate on rolling delinquencies experiences a stark break, precipitously dropping towards zero as origination month approaches the timing of the policy announcement. In contrast, buyout rates on serious delinquencies experience no change across vintage month. The break in trend presented in Figure 1 is a direct consequence of the suspension of buyouts, whereby loans originated later are unable to securitize in time to avoid the suspension.

3 Data

3.1 CoreLogic Loan-Level Market Analytics Data

I use the CoreLogic Loan-Level Market Analytics (LLMA) data to study loan performance over my sample period. The data is obtained through the 25 largest mortgage servicers in the US and represents about 45% of all mortgages originated in the US over the sample period

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

of 2002. The dataset includes origination characteristics such as FICO score, origination balance, initial interest rate, original LTV, original term, origination month, origination year, and zip code. The dataset also includes monthly performance data such as unpaid mortgage balance and delinquency status.

3.2 Dataset Construction and Summary Statistics

In order to construct my primary sample, I identify all loans that were originated between June 2002 and November 2002. I retain loans that identify Ginnie Mae as an investor within 6 months of origination. While the data does not provide a measure of securitization date, identifying the investor in this manner allows me to associate a loan with the appropriate securitizing agency. In order to eliminate outliers, I retain fixed-rate loans with 30-, 20-, and 15-year terms, loan-to-value ratios below 1.5, and an associated Metropolitan Statistical Area (MSA) located in the contiguous United States. Finally, since Corelogic LLMA data has incomplete information on credit score, I retain loans with an inferred collateral type, which measures a loan's prime or subprime status. Along these lines, I also drop loans that are censored due to sale of servicing rights.

My sample is further restricted by two conditions. I first restrict my sample to loans that enter a rolling delinquency by July 2007. I use this cutoff date since it was the last month prior to the freeze on asset backed commercial paper. Doing so, I abstract from concerns related to the broader financial crisis influencing the onset of delinquencies. I define a rolling delinquency as four consecutive months of an uncured 30-day delinquency. This identifies the moment that a loan enters the fourth consecutive month that the borrower failed to make up one single missed payment. I condition on rolling delinquency because this represents the relevant population affected by the memorandum.

I next restrict my sample to loans valued at above par upon experiencing a rolling delinquency. A loan is valued above par when its initial interest rate is above the average market rate at the time of delinquency. Hence, issuers receive a profit from buying out and resecuritizing the mortgage, thereby paying new investors less for the same loan because of the lower market rate. This sampling restriction is necessary to study loan outcomes when buyout is a worthwhile option. In some specifications, I exploit this incentive compatibility constraint by conducting placebo tests using the sample of loans that experience a rolling delinquency when the buyout incentive is not present, namely when the loan is valued below par.

In order to identify buyout status, I measure a change in investor status from Ginnie Mae to non-Ginnie Mae within one month of a qualifying credit event. A qualifying event will be either a rolling delinquency or a 90-day delinquency. Note that, this measure does not identify buyout activity for GSE securitized loans because these loans do not change their status following delinquency. Instead, the GSE's repurchase the loans directly and retain them on balance sheet. In contrast, Ginnie Mae does not purchase loans nor issue securities directly, but rather functions as a platform for other issuers. Issuers will purchase a loan from the Ginnie Mae security pool directly upon a qualifying credit event, hence removing the loan's Ginnie Mae status in the data.

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

Panel A of Table 1 reports summary statistics for origination characteristics of loans in my primary sample. My primary sample consists of loans issued between June and November of 2002, conditioning on a rolling delinquency and a value above par. There are several noticeable differences across buyout status. First, buyout loans have an interest rate 14 basis points higher than non-buyout loans. This is consistent with issuer incentives to buy loans with a higher interest rate in order to profit from resecuritizing a loan when valued above par (Bandyopadhyay et al. (2021)). Second, buyout loans hold better observable characteristics along some dimensions, such as loan size, prime status, and documentation status. This is unsurprising given the weight that investors place on observable loan quality when purchasing Ginnie Mae bonds. Naturally, an issuer will choose to buyout those loans that will facilitate bond sale following resecuritization.

Panel B of Table 1 displays estimates for medium- and long-term loan outcomes across buyout status. Here I use the sample of loans issued in the pre-period, namely between January and May of 2002. Doing so, I measure baseline loan outcomes unrelated to the Ginnie Mae policy. I find that loans across buyout status experience near identical foreclosure rates 18 months following a rolling delinquency. Furthermore, cure rates are equivalent both within a 3-month window around a rolling delinquency, as well as a 12-month window. Finally, modification and payment changes are relatively rare following a rolling delinquency across buyout status.

4 Baseline Results

In my baseline model, I compare differences in loan outcomes across mortgages that are bought out of Ginnie Mae pools and those that remain. Conditional on rolling delinquency and a value above par, I estimate ordinary least squares (OLS) regressions of the following form:

$$Foreclosure_i = X_i'\gamma + \beta Buyout_i + \alpha_t + \alpha_m + \varepsilon_i \tag{1}$$

$Foreclosure_i$ measures whether a loan experiences a foreclosure start within 18 months following a rolling delinquency. $Buyout_i$ is an indicator for a mortgage being bought out of the Ginnie Mae pool. X_i is a vector of loan, borrower, and regional characteristics. Following Kruger (2018), I control for loan terms such as the origination interest rate, LTV, an indicator for inferred prime loan status, log of the original mortgage balance, and an indicator for term length. I also account for underwriting quality by including indicators for low-income documentation or no income documentation. In addition, I control for loan purpose by including indicators for refinancing, primary residence, and single-family homes. Finally, I include measures of local economic activity at the time of delinquency, such as the monthly county-level unemployment rate, the log of the monthly county-level labor force, and the log of the annual county-level house price index, GDP, population, and income. In various specifications, I include fixed effects for month of delinquency α_t and the borrower's MSA α_m. Standard errors are clustered at the MSA level to account for within-MSA residual correlation.

Table 2 reports estimates of Equation (1) using the sample of Ginnie Mae loans issued in 2002 that experienced a rolling delinquency while valued above par. For this exercise, I derive my estimates using loans issued between January and May of 2002, prior to the start of my primary sample. Doing so, I abstract from any effect of the Ginnie Mae policy and measure a stable benchmark relationship between buyout and foreclosure. Column (1) shows results from a univariate regression of foreclosure on buyout. The estimated effect is economically and statistically indistinguishable from zero. In Column (2), after including control variables, the estimate increases slightly and remains statistically insignificant. In Columns (3) and (4), separately including MSA and month of delinquency fixed effects, respectively, changes the size and significance little. Even after the inclusion of both fixed effects simultaneously, the point estimate remains below one percentage point and statistically insignificant. In summary, the baseline results presented in Table 2 suggest that there exists no meaningful relationship between buying a loan out and foreclosure initiation.

5 Main Results

The analysis in the previous section naturally suffers from estimation bias due to asymmetric information in loan sales. For example, an issuer with private information may choose to buyout a low-quality loan in order to prevent high delinquency in its Ginnie Mae portfolio.

Alternatively, an issuer may buyout precisely those loans most likely to re-perform after a rolling delinquency. This would provide an issuer with the opportunity to profit from resecuritizing the mortgage after it becomes current. Hence, the relationship between foreclosure and buyout may be biased up or down depending on what incentives dominate.

In order to differentiate the effect of loan sale from that of loan quality, an ideal experiment would treat one of two identical delinquent loans with a sale and, thereafter, measure loan performance. Such an experiment would account for both loan quality as well as local economic conditions faced by the borrowers. In order to approximate this, I exploit the timing of the 2002 Ginnie Mae memorandum that eliminated the buyout option on rolling delinquencies. My strategy relies upon the fact that loans originated closer in time to the memorandum were less likely to experience a buyout relative to those originated earlier in the year. This is a mechanical consequence of the memorandum, which declared that any loan securitized after January 1, 2003 would not retain a buyout option for rolling delinquencies. Given the lag between origination and securitization, loans issued closer in time to the memorandum were less likely to be securitized prior to January in order to retain the buyout option.

In theory, the month of origination should correlate with buyout propensity but should not vary with loan quality within a short span of time. The policy was intentionally delivered so as to be unanticipated my market participants. If the memorandum was unanticipated and loan quality did not differ over a short horizon, then origination month (relative to the timing of the policy) should have no effect on future loan performance except through quasi-experimental variation in buyout propensity. I discuss the validity of this assumption in Section 5.1 below.

Conditional on rolling delinquency and a value above par, I estimate OLS regressions of the following form:

$$Y_i = X_i'\gamma + \sum \pi_\tau Z_\tau + \alpha_t + \alpha_m + \xi_i \tag{2}$$

In the above specification, Y_i is an indicator variable measuring loan performance. Z_τ is equal to one if a loan is originated τ months prior to policy announcement. As before, X_i is a vector of loan, borrower, and regional characteristics. Furthermore, I include fixed effects for month of delinquency and the borrower's MSA. Finally, standard errors are clustered at the MSA level to account for within-MSA residual correlation.

5.1 Identification

The coefficient of interest, π_τ, measures the differential change in loan outcomes over month of origination due to the policy change. I identify π_τ by exploiting the timing of the Ginnie

Mae memorandum, holding fixed variation within a MSA and within a month of delinquency. The key identifying assumption requires that the month of origination is as good as randomly assigned. Under this assumption, loan outcomes should have evolved smoothly across vintage month, if not for the Ginnie Mae policy announcement. Z_τ satisfies this condition in that the memorandum was unanticipated and released after loan origination. Hence, the decision to originate a loan in a particular month was done independently relative to the effect of the policy change.

However, my assumption may be difficult to support if origination month is correlated with time series variation in loan characteristics, such as interest rates and house prices. In order to assuage these concerns, I employ two weaker assumptions. I assume that origination month is quasi-randomly assigned after (i) conditioning on observables and (ii) restricting my sample to a short span of time. Conceptually, I am comparing two identical loans differing in origination date by only a few months. Since economic conditions should vary little in the immediate short run, origination month assignment should be as good as random.

Nevertheless, unobservable loan quality may still correlate with the timing of loan origination even after the battery of controls outlined above. To further rule this possibility out, I refer to the time series variation in ex-ante observable loan characteristics across Ginnie Mae status. A stark change in observable characteristics prior to the announcement would indicate some change in unobservable quality. In Figure 2, I estimate the average interest rate, loan-to-value ratio, credit score, and loan balance across origination time and loan type. For this exercise, my sample consists of all loans originated between January 2002 and January 2003. There seems to be little variation in loan characteristics within and across Ginnie Mae status in the months immediately preceding the announcement, except for the average interest rate. While the average interest rate falls by 50 basis points from July 2002 to November 2002, this trend is shared across Ginnie Mae and GSE loans. Hence, the evidence in Figure 2 suggests that, conditional on observables, including the initial interest rate and the timing of delinquency, loan quality should vary little over origination month.

As a final piece of suggestive evidence, I refer to institutional details related to loan buyouts. If the month of origination only affects loan performance through the policy change, then loan outcomes unrelated to the policy should be independent of origination month. Since the original Ginnie Mae policy affected a loan's performance *after* experiencing a rolling delinquency, loan performance *before* a rolling delinquency should be constant across vintage. Figure 3 plots the delinquency rate across origination time and Ginnie Mae status using the sample of all loans originated between January 2002 and January 2003. In the months preceding the policy announcement, loans across Ginnie Mae and GSE loans vary little in their likelihood of experiencing any delinquency. Given that delinquency correlates with unobservable loan quality, this supports the assumption that origination month should be uncorrelated with loan outcomes except through the Ginnie Mae policy change.

5.2 Effect on Buyout

Table 3 presents estimates of regression Equation (2) using buyout as the dependent variable. This specification is equivalent to a first stage in so far as I am interested in the effect of buyout on secondary loan outcomes. Column (1) reports estimates for the sample of Ginnie Mae loans issued between June and November of 2002, conditional on rolling delinquency and a value at above par. The estimated coefficients measure the differential change in buyout activity across month of origination relative to loans issued in June 2002. This specification exploits variation within-MSA and within-delinquency month. I am effectively comparing buyout rates across vintage month for loans experiencing a rolling delinquency in the same period, conditioning on average local buyout rates. Hence, this controls for both the time-series and cross-sectional variation that would generally affect loan outcomes based on economic conditions.

In Column (1), the estimate for the earliest month is 1 percentage point, statistically indistinguishable from zero. The size of the estimate increases monotonically for months closer to the timing of the policy. In three months prior to the policy announcement, the estimate is -4.2 percentage points, statistically different from zero at the 5% level. In the last row, I find that, on average, the Ginnie Mae policy caused loans issued in November to experience a 30.6 percentage point decline in buyouts relative to loans issued in June of that year, statistically different than zero at the 1% level.

In Column (2), I relax the assumption that month of origination is as good as randomly assigned. Instead of using a full set of vintage indicator variables, I estimate Equation (2) using a single indicator variable $Z_{\tau>-3}$ for loans issued in the two-month period immediately prior to the policy announcement. In particular, I estimate regressions of the following form:

$$Y_i = X_i'\gamma + \pi_{t>-3} Z_{\tau>-3} + \alpha_t + \alpha_m + \xi_i \tag{3}$$

Here, the coefficient of interest, $\pi_{t>-3}$, identifies the differential change in buyout activity in the last two-month period relative to the previous four months. The key identifying assumption is that that loans issued in the two-month period before the policy would have reflected loan outcomes similar to the previous four vintage months, if not for the Ginnie Mae policy shock. This assumption seems more defensible in that loan quality within a short six-month period should not deviate significantly in the last two-month period relative to the first four months of the sample. However, if the Ginnie Mae policy change should affect loan outcomes, then this would be most stark precisely in the last two-month period before the announcement. Indeed, Column (2) presents an 18.6 percentage point drop in buyout for the two-month period preceding the policy relative to the first four months. This estimate is statistically different from zero at the 1% level.

The estimates in Columns (1) and (2) confirm that buyout activity fell significantly following the Ginnie Mae policy change for loans issued immediately prior to the announcement. While the drop in buyout activity is stark, there still remains the possibility that these estimates reflect some trend in buyout activity unrelated to the treatment. In order to falsify my results, I perform a placebo test using a sample of Ginnie Mae loans issued in 2003. Here, τ equals month of origination relative to a placebo shock one year following the announcement. Hence, I assess whether calendar month correlates with buyout activity using a year that had no policy change. I report estimates in Columns (3) and (4) using the sample of Ginnie Mae loans issued six months prior to a 2003 placebo shock. I find no meaningful pattern in the differential effect of origination month on buyout activity. Point estimates are smaller than 0.5 percentage points and generally statistically indistinguishable from zero.

In Columns (5) and (6), I report analogous estimates using the sample of Ginnie Mae loans issued in 2002 that experienced a rolling delinquency while valued below par. If vintage month should relate to buyout activity only due to the Ginnie Mae policy change, then vintage month is irrelevant to loan outcomes when the issuer does not have an incentive to buy the loan out in the first place. When a loan is trading below par, its initial interest rate at the time of origination is below the average market rate at the time of delinquency. Therefore, after buyout and resecuritization, issuers would owe new investors more than what was previously paid. This means that the issuer has no incentive to buy the mortgage if its value falls below par at the time of delinquency. The estimates in Columns (5) and (6) are close to 1 percentage point and generally statistically indistinguishable from zero. For loans valued below par, I find no meaningful pattern in the differential effect of origination month on buyout activity.

Figure 4 plots the coefficients in Columns (1) and (3) using 95% confidence intervals. The plot shows the degree to which 2002 vintage and 2003 vintage Ginnie Mae loans experienced differential trends in buyout activity across origination month. Previously, in order to restrict variation in loan quality, I estimated Equation (2) using a limited six-month window of origination. The key identifying assumption hinged upon constant loan quality within a short window of origination. Now, I expand the set of vintage months to January of the sample's origination year. Doing so provides confidence in the internal validity of my estimates. Normalizing to June 2002, I plot estimates from five months prior to June 2002 and five months preceding the policy announcement. Under the key identifying assumption that Ginnie Mae loans would have experienced similar buyout activity relative to June 2002 if not for the Ginnie Mae policy shock, there should be no significant difference in outcomes across vintage months. Indeed, buyout activity remained relatively stable for months earlier in the calendar year. However, starting in the months immediately prior to the 2002 shock, there is a stark drop in buyout for 2002 Ginnie Mae loans. In contrast, Ginnie Mae loans issued in 2003 experience no change in buyout activity for an entire calendar year preceding the 2003 placebo shock.

Taken together, the estimates reported in Table 3 and Figure 4 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, loans issued in 2003, and loans valued below par at delinquency.

5.3 Effect on Foreclosure

The drop in buyout activity identified in the previous section was likely due to the lag between origination and securitization. Loans issued immediately prior to the policy shock could not securitize in time to bypass the suspension of buyouts on rolling delinquencies. With this in mind, the Ginnie Mae policy shock provides an opportunity to test how the buyout option affects long term loan outcomes. An issuer can expect that after buying out a loan, the mortgage can be resecuritized into a new Ginnie Mae pool if the borrower becomes current following a rolling or serious delinquency. Therefore, the buyout option naturally generates an incentive for issuers to maintain high loan quality among repooled loans in order to raise investor appetite. Eliminating the buyout option would therefore unwind these incentives and possibly cause loan quality to deteriorate.

The regressions in Table 4 test the effect of the Ginnie Mae policy change on loan quality by estimating regression Equation (2) using foreclosure as the the outcome of interest. Here, Y_i takes a value of one if loan i experiences a foreclosure start within 18 months of a rolling delinquency. I choose an 18-month window as opposed to a 12-month window in order to accommodate the long transition from a 30-day rolling delinquency into foreclosure. This contrasts with the literature, which usually measures transition from a 60- or 90-day delinquency into foreclosure. Moreover, loans experiencing a rolling delinquency should take longer to enter foreclosure simply because the borrower is current on all payments but a single payment from earlier in the year. In order to fall into worse delinquency and prompt foreclosure proceedings, a borrower would need to begin missing payments again after three periods of current payment.

Column (1) reports estimates for the sample of Ginnie Mae loans issued in 2002 and valued above par at the time of rolling delinquency. The coefficient of interest, π_τ, measures the differential effect of the Ginnie Mae policy shock on foreclosures for late originated loans relative to loans issued earlier in June 2002. As before, this specification controls for time-series and cross-sectional variation in economic conditions using delinquency month and MSA fixed effects. I estimate that the differential effect of vintage month on foreclosure grew monotonically from 1.8 percentage points, statistically significant at the 10% level, to 5.7 percentage points, statistically significant at the 1% level. This represents a three-fold increase in the foreclosure rate up to the month preceding the policy announcement. In Column (2), I find that, on average, the Ginnie Mae policy change caused loans issued in the two-month period preceding the announcement to experience a 2.6 percentage point increase in foreclosures relative to the first four months in my sample. This estimate is statistically different from zero at the 1% level.

As before, the estimates in Columns (1) and (2) may suffer from systematic shocks to foreclosure due to some unobserved factor correlated with vintage month but unrelated to the Ginnie Mae policy. I once again conduct placebo tests that address this concern. As an aside, the placebo tests in the previous section were not entirely informative given the nature of the samples. For example, loans issued after the January 1, 2003 cutoff had no buyout activity following a rolling delinquency precisely because the policy change entirely eliminated this option. For loans issued before the cutoff but valued below par at delinquency, the incentive to buyout never existed in the first place, independent of the policy. Hence, there is mechanically limited variation in buyout across vintage month for both samples. These placebo tests gain traction when studying loan outcomes other than buyout. Specifically, if the tests in the previous section confirmed that a subset of loans were unaffected by the policy shock, then these are precisely the loans that should similarly experience no differential change in secondary loan outcomes, such as foreclosure.

To this end, I report estimates in Columns (3) and (4) using the sample of Ginnie Mae loans issued six months prior to a 2003 placebo shock. Once again, I find no meaningful pattern in the differential effect of origination month on foreclosure. Point estimates are all economically and statistically indistinguishable from zero. I report estimates in Columns (5) and (6) using the sample of Ginnie Mae loans issued in 2002 and valued below par at the time of rolling delinquency. Here too, point estimates are statistically indistinguishable from zero across all vintage months. Overall, these results parallel those presented in Table 3 and provide confidence in the validity of the key identifying assumption.

In Figure 5, I plot the coefficients in Columns (1) and (3) along with their 95% confidence intervals. As before, I expand the sample from a six-month window to the full calendar year preceding the policy announcement. For Ginnie Mae loans originated in both 2002 and 2003, I find no differential change in foreclosure across vintage months preceding June of each respective sample's calendar year. This stability is persistent in the control group, whereby Ginnie Mae loans experience no significant differential change in foreclosure relative to the baseline month of June across the entire calendar year preceding the 2003 placebo shock.

In contrast, treated loans experience a monotonic increase in foreclosure across vintage months immediately preceding the 2002 policy shock. Given the stability in foreclosure rates for 2003 originated loans, Figure 5 reenforces the key identifying assumption that loans issued immediately prior to the 2002 memorandum would have experienced outcomes similar to loans originated earlier in the year and loans originated in 2003, but for the effect of the Ginnie Mae policy change. The results in Table 4 and Figure 5 seem to suggest that the increase in foreclosure is likely due to the suspension of the buyout option and not unobserved factors correlated with vintage month.

6 Mechanism

6.1 Effect on Early Cure

As an immediate consequence of the Ginnie Mae policy shock, issuers could no longer execute the option to buyout a loan upon suffering a rolling delinquency. In the long run, loans originated immediately prior to the policy shock appear to experience differentially higher foreclosure rates following a rolling delinquency relative to loans originated earlier in the year. The underlying mechanism is straightforward in that the lag between origination and securitization likely prevented loans originated later in the year from securitizing before the January 1, 2003 deadline, thus failing to retain the buyout option. Hence, buyout can help prevent foreclosure.

In this section, I further examine this relationship by studying the medium run impact of the policy shock on loan outcomes. After a loan experiences a buyout, the borrower must be current before the issuer can resecuritize the mortgage. Hence, the issuer will have a material incentive to make the loan reperform after buyout, likely through renegotiation of the terms of the mortgage or forbearance. However, if the Ginnie Mae shock eliminated the buyout option, I expect to see a reduction in cures immediately following a rolling delinquency.

The regressions in Table 5 test the effect of the Ginnie Mae policy shock on early cures. I estimate regression Equation (2) using early cure as the dependent variable. Here, the outcome of interest Y_i takes a value of one if loan i experiences a cure within 3 months of a rolling delinquency. I use a 3-month window in order to differentiate between a cure associated with a rolling delinquency and a cure associated with a serious delinquency. While the Ginnie Mae policy shock eliminated the buyout option on rolling delinquencies, issuers still had the right to buyout a mortgage following a serious delinquency. Hence if a borrower missed two more payments following a rolling-delinquency and entered 90-day delinquency, then the issuer could execute their buyout option even if the loan securitized after the January 1, 2003 deadline. By restricting the window to cures within 3 months, I identify the effect of the Ginnie Mae policy shock on early cures as opposed to cures in general. The coefficient of interest, π_τ, measures the differential effect of the Ginnie Mae policy shock on immediate cures for late originated loans relative to loans issued earlier in June 2002.

Column (1) reports estimates for the sample of Ginnie Mae loans issued in 2002 and valued above par at the time of rolling delinquency. I estimate that the differential effect of vintage month on early cure decreased monotonically from 1.1 percentage points, statistically insignificant, to -7.4 percentage points, statistically significant at the 1% level. In Column (2), I find that, on average, the Ginnie Mae policy change caused loans issued in the two-month period preceding the announcement to experience a 4.6 percentage point drop in early cure relative to the first four months in my sample. This estimate is statistically different from zero at the 1% level.

In Columns (3) and (4), using the sample of Ginnie Mae loans issued in 2003 and valued above par at the time of rolling delinquency, I report estimates from specifications equivalent to the first two columns. As before, I present results from a falsification test around a placebo shock in 2003 to determine the validity of my key identifying assumption. If vintage month correlates with the early cure rate in a manner unrelated to the true Ginnie Mae policy shock, then I should identify a reduction in early cures across origination time. In Column (3), most point estimates are statistically insignificant and overall present no systematic pattern as shown in Column (1). In fact, Column (4) reports a statistically and economically insignificant coefficient for the two-month period preceding the placebo shock, equal to a differential effect of -0.1 percentage points relative to the first four months.

In Columns (5) and (6), I estimate Equations (2) and (3), respectively, using the sample of Ginnie Mae loans issued in 2002 and valued below par at the time of rolling delinquency. Here, I estimate the effect of vintage month on a sample of loans that should be unaffected by the elimination of the buyout option due to the lack of a buyout incentive in the first place. I hypothesize that, in contrast to Columns (1) and (2), there should be no differential effect of vintage month on early cures using this sample. Indeed, Column (5) reports a statistically insignificant coefficient for each vintage month in 2002. Here, I fail to reject the null that loans valued below par experienced no differential change in early cures across vintage months. In Column (6), this is reenforced by testing for any differential effect in the two-month period preceding the policy shock. I find that the estimated coefficient is small and not statistically different from zero.

Figure 6 plots the estimates from Columns (1) and (3) using 95% confidence intervals. I now expand the window of vintage months to the entire calendar year preceding the shock associated with each respective sample. In the case of loans originated in 2002, this shock is represented by the true policy shock. In the case of loans issued in 2003, the shock is represented by a placebo shock, one year from the date of the Ginnie Mae memorandum. I find that most estimates across vintage months preceding June of the calendar year are statistically not different from zero across both samples. Furthermore, for loans issued in 2003, most estimates following June are also statistically insignificant, presenting no systematic pattern across vintage months.

In contrast, loans issued in 2002 experience a monotonic decrease in early cures across vintage months following June 2002 and immediately preceding the policy shock. Taken together, Figure 6 validates the key identifying assumption that the reduction in early cures is not driven by some unobserved factors correlated with vintage month. Rather, as evidenced in Table 5, this reduction is likely a direct consequence of the Ginnie Mae policy change. These results suggest that when issuers lose the option to buyout a mortgage, those loans experience reduced cures immediately following a rolling delinquency. In contrast, if a loan either held no option for buyout (2003 Ginnie Mae loans) or no incentive for buyout (2002 Ginnie Mae loans valued below par), then there appears to be no systematic reduction in early cures across vintage months.

6.2 Effect on Late Cures and Modifications

The previous section suggests that the the suspension of buyout likely increased the foreclosure rate for loans experiencing a rolling delinquency due to a reduction in early cures. In this section I investigate this channel further by trying to pin down how early cures increase loan quality sufficiently to prevent foreclosure among loans unaffected by the buyout suspension.

As a first pass, I focus on the difference between early and late cures. If an issuer fails to cure a loan following a rolling delinquency due to Ginnie Mae's policy change, then the same issuer can still exercise the buyout option by waiting only two more periods of missed payments. The suspension of buyouts reduced cures that would otherwise occur due to a rolling delinquency loan buyout. Hence, measuring cures using a longer horizon should attenuate the estimated effect due to buyouts associated with a serious delinquency. If the effect identified in the previous section relates to the Ginnie Mae policy shock, then there should be no differential effect across vintage months for late cures due to the absence of any treatment.

In Column (1) of Table 6, I explore this channel using a longer window of observation.

Here, I estimate regression Equation (2), where the outcome of interest Y_i takes a value of one if loan i experiences a cure within 12 months of a rolling delinquency. The goal of estimating around a 3-month window earlier was to identify an immediate change in cure rates following a rolling delinquency. Using a 12-month window, I measure changes in cures following both a a rolling delinquency and a serious delinquency, hereafter termed late cures.

In Column (1), I present estimates using the sample of Ginnie Mae loans issued in 2002, valued above par at the time of rolling delinquency. Estimates for all vintage months are statistically insignificant, with the exception of the month immediately preceding the policy change. Even then, this estimate is close to half the analogous estimate in Column (1) of Table 5. The same is true in Column (2), where the coefficient estimate for the two-month period immediately prior to the policy shock is close to 40% of the analogous estimate in Column (2) of Table 5. Overall, the results in the first two columns of Table 6 suggest that the reduction in cures is only present under a short horizon. Hence, the increase in foreclosures identified in the previous section can be attributed to the reduction in early cures as opposed to cures in general.

The difference in estimated effects between early and late cures has policy implications. Assuming that foreclosures increased for late originated loans due to the reduction in immediate cures, then these results imply that the timing of a cure matters. To see this, note that there is no systematic reduction in late cures prior to the policy announcement. If timing for loan cure was immaterial, then the foreclosure rate would have remained constant across vintage month since the late cure rate is relatively constant. And yet I find that the foreclosure rate increased in line with the reduction in *early* cures. Therefore, the results above imply that for a loan that may eventually cure, curing earlier increases its long term loan performance.

In Columns (3) and (4), I explore whether a reduction in modifications can explain the decline in early cures following the suspension of buyouts. Here, I test whether loans cure following a buyout due to issuers modifying the terms of the mortgage to incentivize repayment. Naturally, if the buyout option is suspended for rolling delinquencies, issuers will no longer offer borrowers these modifications, thus reducing the cure rate for mortgages most affected by the policy change. To this end, I estimate regression Equation (2) and Equation (3), where the outcome of interest Y_i takes a value of one if loan i experiences a modification within 3 months of a rolling delinquency. Once again, a 3-month window associates an identified modification with a rolling delinquency instead of a serious delinquency. Modifications are identified by measuring any changes to the principal balance or reduction in interest rates. Due to data limitations, I cannot identify term extensions. The results in Columns (3) and (4) suggest that there is no differential effect of vintage month on modifications.

Point estimates are close to zero and statistically insignificant.

In Columns (5) and (6), I test for any changes in required loan payments. If the mortgage contract was modified along dimensions other than the interest rate and principal balance, then this may be reflected in the required monthly payments owed by the borrower. Namely, the issuer may negotiate with the borrower to make the loan current by adjusting the per period payment. Hence, a reduction in buyout activity should reduce the rate at which the issuer offers a payment change. I estimate regression Equation (2) and Equation (3), where the outcome of interest Y_i takes a value of one if loan i experiences a change in loan payments of at least \$50 within 3 months of a rolling delinquency. The results in Columns (3) and (4) are generally statistically insignificant and close to zero, suggesting that there is no differential effect of vintage month on payment changes.

Taken together, while the results in this section suggest that the timing of a loan cure affects long term loan performance, the precise mechanism is unclear. Estimates of the differential effect on renegotiation are statistically and economically indistinguishable from zero. Thus, there appears to be no observable change in loan terms that would explain the relationship between vintage month and early cure. Issuers, therefore, likely induce the early cures that I identify through at least one of the following remaining forbearance channels: (i) extending a subordinate lien on the property to compensate the payments owed to the issuer, (ii) extending the length of the mortgage to make up for the missed payment, or (iii) negotiating terms in a manner unobserved in the data.

Of these three possibilities, given the specialized nature of a rolling delinquency, I posit that issuer incentives are best aligned with the use of a subordinate lien. To understand this argument, consider that when a borrower enters serious delinquency due to a heavy debt burden, then reducing the required payment through a modification can plausibly induce reperformance. In contrast, a rolling delinquency, by definition, occurs when a borrower is *already* paying all recent periods except for a single missed earlier payment. Therefore, the issuer may assume that the borrower will continue to extend future payments under the current terms of the mortgage. Reducing the interest or principal under this assumption would be redundant and amount to a relative loss. Alternatively, if the issuer were to capitalize the arrears into a principal balance increase, the borrower would face higher per period payments. For the marginally constrained borrower, increasing the debt burden will also increase the risk of default due to the borrower's limited capacity to service the debt. In contrast to a modification, placing arrears into a subordinate lien can make an issuer whole, cure a loan, and maintain the present value of future payments without further burdening the borrower.

In sum, I find suggestive evidence that intervening early through buyout can reduce foreclosure rates with no obvious change in payments and loan terms. I argue that issuers instead rely upon extending a second lien on the property to reinstate the delinquent loan. In Section 8.3.2, I explore the implications of this argument by studying how the effect of buyout varies with financial slack.

7 Robustness

The results from the previous section hinged upon the assumption that the vintage month only affects the outcome through treatment. This assumption would be violated if loan quality fundamentally differed across month of origination in a manner not accounted for in loan characteristics, even after conditioning on delinquency. While I provide evidence that this is unlikely given trends in loan characteristics over time, there still may be some unaccounted variation in the lending environment from one month to the next. In order to capture this variation, my empirical strategy requires a control group that can account for common trends in loan quality within a given month of origination. GSE loans serve as a natural candidate to control for such common variation. Hence, I augment Equation (2) using regressions of the following form:

$$Y_i = X_i'\gamma + \sum \pi_\tau Z_\tau \times \text{GNMA}_i + \theta\ \text{GNMA}_i + \alpha_\tau + \alpha_t + \alpha_m + \xi_i \tag{4}$$

In the above, Y_i measures loan performance and Z_τ is equal to one if a loan is originated in month τ, as before. Now, I include an interaction term, GNMA_i, which equals one if loan i is associated with Ginnie Mae. I retain the control variables and fixed effects used in previous regressions. I now account for shocks common to loans issued in month τ by including vintage fixed effects α_τ. Note that these fixed effects absorb Z_τ. Finally, I include an indicator variable GNMA_i to measure Ginnie Mae status in order to account for heterogeneity in loan outcomes across loan type. Standard errors are clustered at the MSA level to correct for within-MSA residual correlation.

7.1 Identification

The coefficient of interest, π_τ, measures the differential change in loan outcomes among Ginnie Mae loans relative to both loans issued earlier in the year as well as relative to non-Ginnie loans. I identify π_τ by restricting variation to within a given vintage month. Hence, after using α_t and α_m to account for time-series and cross-sectional variation, I can further control for common shocks within a given month of origination through α_τ. Furthermore, the Ginnie Mae memorandum generates the quasi-experimental variation necessary for identification by virtually eliminating the buyout option on loans issued immediately prior to the policy announcement. I assume that loan performance across vintage month and Ginnie Mae status would have remained relatively constant in the absence of the Ginnie Mae policy change.

The key identifying assumption is relaxed relative to Section 5. Here, vintage month must be as good as randomly assigned after accounting for common shocks to loan outcomes across Ginnie Mae and GSE loans within a given month of origination. Practically, this means that if some unobserved quality correlated with both the timing of origination as well as loan outcomes, then this factor must be accounted for in both Ginnie Mae and GSE loans. This assumption would be violated if some unobserved factor significantly predicted both vintage and loan outcomes for Ginnie Mae loans uniquely. This seems unlikely given the evidence in Figure 2 and Figure 3, where both ex-ante and ex-post loan quality seems to move in parallel prior to the memorandum across Ginnie Mae and GSE loans.

7.2 Results

Table 7 presents estimates using the sample of all agency loans and foreclosure as the outcome of interest. Column (1) first reports estimates of regression Equation (2) for the sample of exclusively GSE loans issued in 2002, conditional on a rolling delinquency and a value at above par. The vintage month coefficients are generally statistically insignificant, indicating that there is no systematic differential effect of month of origination on foreclosure. This is mirrored in Column (2), where loans issued in the two-month period immediately prior to the policy shock experience no differential effect in foreclosures relative to loans issued in the first four months. These results validate the notion that GSE loans do not experience any meaningful changes in foreclosure rates across vintage months. Therefore, GSE loans function as a suitable control to Ginnie Mae loans issued in 2002.

In Column (3), I estimate Equation (4) using the sample of GSE and Ginnie Mae loans issued in 2002, conditional on a rolling delinquency and a value above par. As before, I include origination, borrower, and time-varying controls, in addition to MSA level and month of delinquency fixed effects. Given that my sample now includes both loans securitized through Ginnie Mae (treatment group) and loans securitized through the GSE's (control group), I can also include vintage month fixed effects while still identifying the treatment effect of interest. Here, I restrict identifying variation to the differential effect of vintage month on loan outcomes for Ginnie Mae loans in particular, independent of common shocks across all mortgages originated in a particular month. This specification allows me to isolate the treatment effect from the effect of unobserved factors correlated with both vintage month and foreclosure.

Column (3) reports coefficient estimates for the interaction term $Z_\tau \times$ GNMA. The coefficient estimate for the first month in my sample is close to zero and statistically insignificant. Three months prior to the policy shock, I estimate that the foreclosure rate increased by 2.6 percentage points for Ginnie Mae loans relative to both Ginnie Mae loans issued in June 2002 and all agency debt issued in the same month. This point estimate is statistically different from zero at the 5% level. One month immediately preceding the policy change, the coefficient estimate on the interaction term increases 60% to 4.2 percentage points, statistically different from zero at the 1% level. The point estimates in Column (3) indicate that the 2002 policy change led to a monotonic increase in foreclosure rates for Ginnie Mae loans across vintage months.

In Column (4), I find that, on average, the Ginnie Mae policy change caused loans issued in the two-month period preceding the announcement to experience a 2.7 percentage point increase in foreclosures relative to the first four months in my sample and relative to all agency debt issued in the same period. This estimate is statistically different from zero at the 1% level. This specification relaxes the assumption that Ginnie Mae loans experienced a differential effect across vintage months preceding the policy. Instead I assume that loans issued in the two-month period preceding the announcement would have experienced foreclosure rates relatively similar to loans issued in the first four months and relative to other GSE loans within the same period. The estimate in Column (4) is virtually identical to the corresponding estimate in Column (2) of Table 4 using the sample of Ginnie Mae loans, further bolstering confidence in my underlying assumptions.

Table 8 presents results from estimating regression Equation (4) using early cures as the outcome of interest. The columns in this table are analogous to those presented in Table 7. As before, Column (1) reports estimates using regression Equation (2) and the sample of exclusively GSE loans. Across all vintage months, the estimates are close to zero and statistically insignificant. Column (2) corroborates the results in Column (1), whereby the coefficient estimate for the two-month period immediately prior to the policy shock is close to zero and statistically insignificant. Taken together, the results in Columns (1) and (2) indicate that GSE loans experience no differential effect in early cures across vintage month. Hence, they serve as a plausibly valid control group to test the effect of the 2002 policy shock on early cures among Ginnie Mae loans relative to agency loans in general.

Columns (3) reports coefficient estimates of regression Equation (4). While the coefficients are individually statistically insignificant, they present a clear monotonic decline in early cure rates. In contrast, the point estimates in Column (1) are small and present no systematic pattern across vintage months. The differential decline in early cure rates among Ginnie Mae loans is most stark using a pooled specification. In Column (4), I estimate that loans issued in the two-month period prior to the policy experienced a 2.8 percentage point decline in early cures relative to the first four months and relative to all agency loans issued in the same period. This estimate is significantly different from zero at the 5% level. The point estimate measuring the reduction in early cures for Ginnie Mae loans in Column (4) of Table 8 is nearly equivalent to the point estimate measuring the increase in foreclosures for Ginnie Mae loans in column (4) of Table 7. These results provide confidence that the change in foreclosure and early cure rates are tightly linked to the Ginnie Mae policy change.

8 Instrumental Variables

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

Using vintage month as an 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 month of origination as an instrument for buyout in Section 8.1 below. Conditional on rolling delinquency and value at above par, the first-stage is represented by regressions of the the following form:

$$Buyout_i = X_i'\gamma_1 + \sum_{\tau} \pi_\tau Z_\tau + \alpha_t + \alpha_m + \xi_i \tag{5}$$

where Z_τ is the instrumental variable, equal to one if a loan is originated in month τ. In the second-stage, conditional on rolling delinquency and value at above par, I regress loan performance on fitted values for buyout:

$$Y_i = X_i'\gamma_2 + \rho \widehat{Buyout}_i + \alpha_t + \alpha_m + \eta_i \tag{6}$$

Here, $\widehat{Buyout}_i$ represents the predicted values from Equation (5). Under the assumption that Z_τ is a valid instrument, ρ identifies the causal effect of buyout on loan performance.

8.1 Identification

8.1.1 LATE

In order to identify the causal effect of interest, the instrument I employ must satisfy certain conditions. Namely, month of origination must vary with buyout activity, must be as good as randomly assigned, and must affect loan performance only through buyout. In addition, my research design must accommodate heterogeneity in instrument response. In particular, if there existed only a subset of loans for which month of origination affects buyout, my instrument must satisfy an additional condition, whereby month of origination must affect all loans in the same direction. In this section, I discuss the validity of these assumptions and their implications for external validity.

Relevance To satisfy the relevance condition, an instrument must affect the probability of treatment assignment. Therefore, the month of origination satisfies the relevance condition if a loan's origination month differentially affects the probability that a loan is bought out. As shown in Section 2, Figure 1 demonstrates that the buyout rate for loans experiencing a rolling delinquency drops precipitously across vintage months starting in August 2002. In contrast, the buyout rate for serious delinquent loans experiences no change across vintage month. Finally, regression estimates from Table 4 and Figure 4 further validate the argument that vintage month differentially affects buyout propensity even after accounting for ex-ante loan characteristics.

Independence A valid instrument must be as good as randomly assigned. Given that the the memorandum was unanticipated and released after loan origination, vintage month is a function of conditions exogenous to the policy shock itself. Hence, the timing of origination is not driven by the suspension of the buyout option. As done in Section 5.1, I adopt an assumption more flexible than pure random assignment. I assume that origination month is as good as randomly assigned after (i) conditioning on observables and (ii) restricting my sample to a short span of time. Within a short span of time and controlling for factors observable at origination, the precise timing of origination should matter little.

Exclusion Restriction In order to satisfy the exclusion restriction, an instrument must not affect the outcome variable except through treatment. In the context of my research design, this means that, conditional on a rolling delinquency, a loan's month of origination should not affect its long-term performance except through buyout.

Immediately, the nature of the instrument helps support this assumption. Loan quality and, by extension, loan outcomes should be unrelated to the timing of the policy since the announcement was unanticipated. Furthermore, conditional on observables and within a short period of time, whether a loan is originated in one month or the next should not affect long term loan performance. My analysis further conditions on rolling delinquency, month of delinquency, and MSA of origination. Therefore, I restrict identification to two observationally identical loans, both experiencing a rolling delinquency and the same economic conditions at the time of delinquency.

Turning to graphical evidence, as presented in Section 5.1, Figure 2 and Figure 3 present ex-ante loan characteristics and ex-post loan quality, respectively. If loan quality varied across vintage month in a manner that would reflect significant changes in long-term loan outcomes, then this would be apparent here. Figure 2 demonstrates that Ginnie Mae loans experience no discrete break in ex-ante loan characteristics across vintage months immediately preceding the announcement. Similarly, Figure 3 presents little variation in ex-post delinquency rates across Ginnie Mae status and origination months.

Turning to the analysis in the previous section, Figures 5 and 6 present evidence that loan outcomes varied little in the early months of the calendar year for loans issued in both 2002 and 2003. For loans issued in 2003, origination timing remained uncorrelated with loan outcomes even in later months of the calendar year. In contrast, loans issued immediately prior to the 2002 policy shock uniquely experienced a stark change in long-term performance. If vintage month correlated with loan outcomes in a manner unrelated to treatment, then this would be apparent in outcomes for loans issued during unexposed periods. Instead, I find that outcomes differ exclusively for loans issued in the most exposed months.

Monotonicity In order to identify a local average treatment effect, I require an additional assumption of monotonicity, whereby units do not select out of treatment due to assignment to the instrument (defiers). In the context of my research design, this requires that if a loan (i) received a late origination date and (ii) experienced a buyout, then it would be equally or more likely to experience a buyout if it received an early origination date. This is satisfied mechanically given the nature of the instrument. The instrument exploits the fact that loans originated closer in time to the policy announcement would be less likely to securitize prior to the deadline—thus subject to the ban on buyouts. If a late originated loan happened to securitize prior to the deadline, then having more time to securitize would make the loan more or equally likely to securitize prior to the deadline, by definition.

Local Average Treatment Effect Under the assumption that vintage month satisfies the conditions outlined above, I identify the local average treatment effect (LATE) for those loans whose treatment varies due to variation in the instrument. Namely, this subset represents those loans that experienced a buyout due to early origination and would not have experienced a buyout if originated later (compliers). Note that this research design will not identify the effect of buyouts for those loans with (i) a short time-to-securitization (always-takers) and (ii) an excessively long time-to-securitization (never-takers), since the timing of origination would be unaffected by the policy.

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

8.1.2 Weak Instrument Test

Asymptotic properties of the 2SLS estimator depend on the instrument's correlation with the endogenous variable. The bias of the estimated treatment effect will be inversely proportional to how well the instrument predicts treatment. Therefore, small violations of the exclusion restriction will be exacerbated by a weak first-stage. 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 obtain the multivariate F-statistic, namely the Cragg-Donald Wald statistic, and its robust counterpart, the Kleibergen-Paap Wald test statistic (Bazzi and Clemens (2013)). Intuitively, these statistics are used to test the null hypothesis that the first-stage coefficients are zero. Stock and Yogo (2005) provide critical values defined by a 10% rejection region under the null that 2SLS bias relative to OLS exceeds 10%, in addition to critical values for the null that I falsely reject $\rho = 0$ in a two-tailed 5% t-test.

8.1.3 Overidentification Test

When an instrumental variables model has more instruments than endogenous variables, then the model is overidentified. In such a case, there is no longer a unique estimator for the coefficient of interest. This is apparent from estimates plotted in Figure 4 and Figure 5. Taking the ratio of estimates for each vintage month across the two plots, the estimated treatment effect for November is close to half the treatment effect estimated for

October. Accounting for this heterogeneity, 2SLS obtains a consistent estimator of the weighted average of individual treatment effects, whereby the weights represent the influence of a particular instrument on the treatment. In the context of my research design, this would imply 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 that at least one instrument is valid and holds a null hypothesis that additional instruments are exogenous. Failing to reject the null, along with confidence in the validity of at least one instrument, would provide strong evidence that all instruments are valid.

Importantly, rejecting the null hypothesis would still leave room for at least one instrument to be valid, given the baseline assumption of the test. In the context of my research design, this would most likely hold for the last two-month period since buyout activity broke most sharply in this period relative to other loan vintages. For this reason, I present estimates for both the overidentified model and the just identified model.

8.2 Results

8.2.1 Effect on Foreclosure

Panel A of Table 9 presents second-stage estimates from the instrumental variable estimation using foreclosure as the dependent variable. Column (1) reports estimates for the sample of Ginnie Mae loans issued in 2002, conditional on a rolling delinquency and a value at above par. Here, I use the full set of vintage months as instruments. Note that first-stage and reduced-form estimates are effectively reported in Column (1) of Table 3 and Table 4, respectively, from Section 5. In Column (1) of Table 9, I estimate Cragg-Donald and Kleibergen-Paap Statistics of over 100, a strong rejection of the null hypothesis that the set of instruments is weak. This is unsurprising given the results from Section 5, indicating that vintage month strongly correlates with buyout activity, dropping starkly in the last months prior to the policy announcement. Given that these diagnostic statistics far exceed the critical values provided by Stock and Yogo (2005), the worst-case bias of 2SLS should be limited and extreme outlier 2SLS estimates are highly unlikely.

In Column (1) of Panel A, I also report the p-value for the Sargan–Hansen test of overidentifying restrictions using foreclosure as the outcome of interest. Importantly, this test assumes that at least one instrument is exogenous. While inherently untestable, the evidence presented in Section 5 strongly suggests that, at a minimum, the two-month period preceding the policy announcement should satisfy the exclusion restriction. Hence, assuming that this two-month period provides exogenous variation in buyout activity, I fail to reject the null hypothesis that additional instruments are exogenous at the 5% level. This provides confidence in the validity of the full set of instruments.

Column (1) of Panel A in Table 9 presents estimates of the second-stage regression given by Equation (6). I find that buyout reduces foreclosure by 12.4 percentage points, statistically different from zero at the 1% level. This specification accounts for the same set of loan, borrower, and regional characteristics as earlier. Here, I restrict identification to within-delinquency month and within-MSA variation in foreclosure across vintage months. Under the assumptions outlined in Section 8.1, I identify the average treatment effect on the treated. Therefore, the estimate in Column (1) implies that loans experiencing a buyout would have counter-factually experienced an 80% increase in foreclosure.

Column (2) of Panel A in Table 9 presents just-identified 2SLS estimates using first-stage regressions given by Equation (3). Here, the excluded instrument is $Z_{\tau>-3}$, an indicator variable equal to one if a loan experiences a rolling delinquency in the two-month period immediately prior to the Ginnie Mae policy announcement. As before, the coefficient from the first-stage regression, $\pi_{t>-3}$, identifies the differential change in buyout activity in the last two-month period relative to the previous four months. The choice of $Z_{\tau>-3}$ as the strongest candidate for the just-identified 2SLS estimate assumes that loans issued in the two-month period immediately preceding the policy announcement would have experienced virtually equivalent outcomes to those loans issued in the first four months, if not for the Ginnie Mae policy shock.

Indeed, the first stage seems to suggest this to be the case. As before, I borrow from Section 5, where I present first-stage and reduced-form estimates, reported in Column (2) of Table 3 and Table 4, respectively. In Column (2) of Table 9, I estimate Cragg-Donald and Kleibergen-Paap Statistics of over 200, strongly rejecting the the null hypothesis that the set of instruments is weak.

Turning to the second-stage, the estimate in Column (2) indicates that buyout reduces foreclosure by 14 percentage points, statistically different from zero at the 1% level. This estimate is 1.6 percentage points larger than the estimate in Column (1). The just-identified 2SLS estimator has the desirable property that its distribution is centered at the population parameter value, hence it is approximately median unbiased (Angrist and Krueger (1999)). The similarity in estimates across Columns (1) and (2) suggests that the bias of the overidentified 2SLS estimate should be limited relative to the population parameter.

In Column (3), I present second-stage estimates for the sample of all agency loans issued in 2002, conditional on a rolling delinquency and a value at above par. Here, I augment the first-stage regression Equation (5) to include interaction terms with an indicator for Ginnie

Mae status and vintage month fixed effects. The effective first-stage regression is equivalent to Equation (4). In theory, I am comparing foreclosure rates between Ginnie Mae and GSE loans after controlling for variation attributable to vintage, delinquency time, and MSA.

In Column (3) of Panel A, I report the p-value for the Sargan–Hansen test of overidentifying restrictions using foreclosure as the outcome of interest. Assuming at least one exogenous instrument, I fail to reject the null hypothesis that additional instruments are exogenous at the 10% level. The assumption of at least one exogenous instrument is relaxed relative to the specification in Column (1) since I interact vintage month with Ginnie Mae status. Hence the p-value in Column (3) provides strong reassurance regarding the validity of the set of instruments.

The estimate in Column (3) indicates that buyout reduces foreclosure by 11.6 percentage points, statistically different from zero at the 1% level. This estimate is 0.8 percentage points smaller than the estimate in Column (1) and 2.4 percentage points smaller than the estimate in Column (2). Assuming that including GSE loans corrects for any omitted variable bias associated with vintage month, estimates using Ginnie Mae loans alone are relatively close to the more precise estimate given by Column (3). In fact, the coefficient in Column (3) still indicates that loans experiencing a buyout would have counter-factually experienced a foreclosure rate close to 80% higher.

Finally, Column (4) of Panel A in Table 9 presents just-identified 2SLS estimates using a first-stage regression similar to Equation (4). Here, the excluded instrument is $Z_{\tau>-3} \times$ GNMA, an indicator variable equal to one if a Ginnie Mae loan experiences a rolling delinquency in the two-month period immediately prior to the Ginnie Mae policy announcement. The estimate in Column (4) indicates that buyout reduces foreclosure by 12.5 percentage points, statistically different from zero at the 1% level. This final specification holds the most desirable properties, as it is approximately median unbiased and accounts for within-vintage variation. Importantly, this estimate is within almost 10% of the just-identified estimate in Column (2) using the sample of only Ginnie Mae loans.

8.2.2 Effect on Early Cures

Panel B of Table 9 presents second-stage estimates from the instrumental variable estimation using early cure as the dependent variable. Column (1) reports estimates using the full set of vintage month instruments Z_τ for the sample of Ginnie Mae loans issued in 2002, conditional on a rolling delinquency and a value at above par. The first stage estimates are the same as in Panel A and the reduced-form estimates are reported in Column (1) of Table 5 from Section 5.

Column (1) of Panel B presents estimates of the second-stage regression given by Equation

(6) using early cure as the outcome of interest. I find that buyout increases early cure by 21.5 percentage points, statistically different from zero at the 1% level. Column (1) of Panel B also reports the p-value for the Sargan–Hansen test of overidentifying restrictions using early cure as the dependent variable. Assuming at least one instrument is exogenous, I reject the null hypothesis that additional instruments are exogenous at the 1% level. In the context of measuring early cures, vintage month fails to identify the same population parameter. This is likely due to limited variation in the first few months of my primary sample, as show in Figure 6. Importantly, this test does not reject the validity of at least one instrument, which I argue is best represented by the two-month period immediately preceding the policy announcement.

With this in mind, Column (2) of Panel B in Table 9 presents just-identified 2SLS estimates using first-stage regressions given by Equation (3). Here, the excluded instrument is $Z_{\tau>-3}$, an indicator variable equal to one if a loan experiences a rolling delinquency in the two-month period immediately prior to the Ginnie Mae policy announcement. Once again, first-stage estimates are the same as in Panel A and I report reduced-form estimates in Column (2) of Table 5. The estimate in Column (2) of Panel B in Table 9 indicates that buyout increases early cures by 24.8 percentage points, statistically different from zero at the 1% level. Given that the 2SLS estimator is a weighted average of individual treatment effects based on the influence of each instrument, the overidentified estimate is naturally close to the just-identified estimate.

In Column (3), I present second-stage estimates for the sample of all agency loans issued in 2002, conditional on a rolling delinquency and a value at above par. I report the p-value for the Sargan–Hansen test of overidentifying restrictions using early cure as the outcome of interest. Assuming at least one exogenous instrument, I fail to reject the null hypothesis that additional instruments are exogenous at the 10% level. The p-value in Column (3) reenforces the the validity of the set of instruments in estimating the treatment effect when including all agency loans. Given the failure of this test in Column (1), the result in Column (3) implies that accounting for vintage month fixed effects absorbs significant variation necessary to ensure that the set of instruments is exogenous.

The size of the estimate in Column (3) further supports this conclusion. Here I estimate that buyout increases early cures by 13.2 percentage points, statistically different from zero at the 5% level. This estimate is almost half of those presented in Columns (1) and (2). The estimate in Column (4) of Panel B does not vary much from Column (3), where I report a just-identified 2SLS estimate of 13.0 percentage points, statistically different from zero at the 5% level. Given that these estimates account for vintage month fixed effects, the results in Panel B suggest that unobserved factors may correlate with both vintage month and incidence of early cure. Hence, accounting for this variation is important in identifying the treatment effect for early cures in particular. Note that these estimates are virtually equivalent to the analogous specification in Panel A, reenforcing the argument that buyouts reduce foreclosure due to an increase in early cures.

8.3 Heterogeneous Treatment Effects

In the previous section, I estimated the causal elasticity of buyout and loan performance. Under the assumptions of one-sided non-compliance, my results indicate that buyouts reduce foreclosure for loans normally selected into buyout. Given that foreclosure rates are equivalent across buyout status, I argue that issuers likely buyout loans with worse counter-factual performance relative to non-buyout loans. In other words, the evidence in the previous section suggests that issuers select low-quality loans for buyout. While the interpretation above explains selection into treatment, in this section, I explore heterogeneity in the treatment effect to better characterize the channel through which loans are affected by buyout.

8.3.1 Debtor Financial Constraints

In this subsection, I examine the degree to which financial constraints can explain the results in the previous section. Borrowers with relatively more liquid savings or stable lines of credit may find it easier to prevent worsening delinquency after an initial missed payment. Therefore, unconstrained borrowers may prevent foreclosure independent of selection into buyout. In contrast, I hypothesize that early intervention should benefit those borrowers that are most at risk to worsen in delinquency due to limited access to finance. If an issuer provides forbearance that reduces the burden of repayment upon a constrained debtor, then perhaps that borrower will 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 incentive to maintain loan quality. Here, I directly test whether early cures can prevent worsening delinquency for precisely these most at-risk borrowers.

In order to explore this hypothesis, I reestimate my main IV specification on various subsamples derived using proxy variables for financial constraints. I construct these restricted samples by first identifying key demographic characteristics that may correlate with financial constraints due to economic or historical reasons. The variables I measure include the percent of adults that (i) hold less than a high school education, (ii) are below the poverty line, (iii) are unemployed, and (iv) identify as non-white. These factors proxy for difficulty in accessing financing due to issues related to employment, income, wealth, and discrimination. For example, borrowers in localities with above median unemployment may find it difficult to refinance when interest rates decline (Defusco and Mondragon (2020)). Similarly, borrowers in primarily non-white counties may fall into worse delinquency following an income shock due to limited liquid savings (Ganong et al. (2020)). I obtain county-level values for each threshold variable using the 2000 decennial census. Finally, I split my primary sample into loans from counties with above and below median values of each threshold variable.

In Panel A of Figure 7, I report just-identified 2SLS estimates using subsamples derived from each measure of financial constraints. I follow the specification outlined in Column (2) of Table 4. I include all controls, as well as fixed effects for MSA and delinquency month. Here, I plot IV estimates that measure the causal effect of buyout on foreclosure for constrained and unconstrained localities, separately. Across all measures, I find that borrowers in financially constrained localities experience a larger treatment effect relative to unconstrained counties. Counties with less educational attainment experience a 25% larger reduction in foreclosures relative to counties with more graduates. Localities with a higher rate of poverty experience a 70% larger treatment effect relative to more affluent counties. High unemployment counties experience a treatment effect that is three times larger than the effect estimated for low unemployment counties. Finally, the effect of buyout appears exclusively salient for counties with an above-median non-white population.

Assuming that these variables proxy for financial constraints, these results suggest that limited access to finance likely increases the impact of the reduction in foreclosure risk following a loan buyout. For each proxy variable, loans in constrained counties appear to benefit most from selection into buyout. Taken together, these results reenforce the argument that buyout reduces foreclosure by incentivizing issuers to cure loans.

8.3.2 Creditor Financial Slack

While financially constrained borrowers benefit most from buyout, issuers may fail to provide effective foreclosure relief when financial slack is limited. For example, Aiello (2022) finds that financially constrained servicers perform foreclosures and modifications that are value reducing for both investors and borrowers. In the context of buyout, the most relevant constraint should correlate with the availability of collateral. This is due to the use of subordinate debt to capitalize arrears and cure loans, as suggested by evidence presented in Section 6.2. I hypothesize that when the credit supply contracts or the underlying asset (the borrower's home) is over-levered, then an issuer has less financial slack to extend a second lien to reinstate the loan. Thus, buyout should benefit borrowers residing in localities with greater slack in the mortgage credit supply. If the credit supply is less constrained, then an issuer has more flexibility to capitalize arrears into a subordinate loan to cure a delinquent loan. Since the subordinate loan is inherently tied to collateral, whether issuers can cure a loan through subordination depends on the availability and quality of the underlying collateral.

I test this hypothesis by reestimating my main IV specification on subsamples derived using proxy variables for slack in the mortgage credit supply. As before, I construct restricted samples by first generating measures of financial slack. The variables I use include the county-level loan denial rate and the county-level loan-to-income ratio for originated loans. The average denial rate measures the degree to which lenders restrict the credit supply in one county relative to another. The average loan-to-income ratio measures the availability of collateral to secure a second lien. I obtain measures for each threshold variable using 2002 HMDA loan application data. Finally, as before, I split my primary sample into loans from counties with above and below median values for each threshold variable.

In Panel B of Figure 7, I report just-identified 2SLS estimates using subsamples derived from each measure of financial slack. As before, I follow the specification in Column (2) of Table 5, exploiting within-MSA and within-delinquency month variation to identify the effect of buyout on foreclosure for each subsample. In the first two rows, I plot IV estimates that measure the casual effect of buyout on foreclosure across counties with above and below median slack in the mortgage credit supply. I find that 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 that is 30% larger than in counties with less slack. Loans issued in counties with an average loan-to-income ratio below median experience a treatment effect that is four times as large as counties with above median ratios. In both instances, estimates obtained using counties with less slack appear statistically insignificant. Ultimately, the close relationship between the salience of the buyout effect and quality of collateral seems to support the hypothesis that issuers rely upon capitalizing arrears into subordinate debt.

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 using the Enhanced Financial Accounts (EFA) data provided by the Federal Reserve. The EFA data represents a combination of household debt data from the Equifax/Federal Reserve Bank of New York Consumer Credit Panel and income data from Bureau of Labor Statistics (Ahn et al. (2018)). I split my primary sample into loans from counties with above and below median values for this debt-to-income measure.

Whereas aggregate loan-to-income signals the degree of slack available for mortgage credit in particular, aggregate debt-to-income is more broad since it includes all consumer debt, such as auto and credit card debt. Hence, debt-to-income functions as a signal for borrower financial constraints more than slack for any individual credit type. Given this context, I hypothesize that loans issued in high debt-to-income counties experience larger treatment effects relative to loans originated in low debt-to-income counties. If consumer debt-to-income, as opposed to mortgage loan-to-income, is representative of borrower financial constraints, then this test should resemble the tests in the Section 8.3.1.

In the final row of Panel B in Figure 7, I report just-identified 2SLS estimates using subsamples derived from aggregate debt-to-income. I plot IV estimates that measure the causal effect of buyout on foreclosure for loans issued in counties with above and below median debt-to-income, separately. In contrast to the first two rows of Panel B in Figure 7, I find that borrowers in counties with above median debt-to-income experience a 70% larger treatment effect relative to loans issued in low debt-to-income counties. The estimate for below median debt-to-income counties is statistically insignificant. These results support the interpretation reached in Section 8.3.1 that financially constrained borrowers experience the largest reduction in foreclosure following buyout. These results also contrast with the first two rows of Panel B in Figure 7 because, instead of measuring borrower financial constraints, the first two rows measure financial slack as it relates to collateral quality and availability.

8.3.3 Profit Motive

My main results condition on a positive difference between the initial interest rate and the average mortgage rate at the time of delinquency. This positive interest rate premium implies that I identify the effect of buyout on loan performance when the buyout incentive exists. Naturally, if the issuer resecuritizes the buyout loan for a lower rate but receives the same per period amount from the borrower, then the issuer profits from passing through less per period to investors.

In this section, I explore how this profit motive affects the quality of loan cure. I first decompose my primary sample into loans with above and below interest rate premiums. Doing so, I classify loans based on whether issuer profit motive provides a strong or weak incentive to resecuritize. I hypothesize that loans with a strong incentive to resecuritize will likely perform better than loans with a weaker incentive. Prior literature has demonstrated that the sellers of debt often signal loan quality in order to facilitate sale (Begley and Purnanandam (2016)). Creditors may increase effort into curing loans in order to signal loan quality ex-post, building reputation of high-quality cures in newer mortgages. Work by Hartman-Glaser (2017) and Adelino et al. (2017) argue that reputation is fundamental to MBS markets in particular.

In Figure 8, I report just-identified 2SLS estimates using subsamples derived from above and below median interest rate premiums. In the first row, I plot IV estimates that measure the causal effect of buyout on foreclosure for loans with a strong and weak buyout incentive, separately. I find that loans with a strong incentive to buyout and resecuritize experience an 80% larger reduction in foreclosures relative to loans with a weak profit motive. This result suggests that issuers plausibly put greater effort to cure precisely the loans that have the greatest payoff.

However, an alternative interpretation hinges upon the results in Section 8.3.1. The loans with the highest interest rate premium are also the loans with the highest interest rate. In that case, one interpretation of my result is that loans with high interest rates, and therefore borrowers that are financially constrained, benefit most from loan buyout. To test this channel, I repeat the previous exercise and decompose my primary sample into loans with no delinquency and loans with some delinquency over the past year. Doing so, I exploit ex-post loan quality to identify the effect of loan buyout on foreclosure for high and low risk loans separately. In Figure 8, I report just-identified 2SLS estimates using these. I find that ex-post riskier loans experience an 60% larger reduction in foreclosures relative to ex-post less risky loans. The point estimate for low risk loans is statistically insignificant at the 5% level. This result fails to reject the null hypothesis that high premium loans experience a larger reduction in foreclosure due to loan risk instead of issuer incentives.

However, this last result may mask treatment effect heterogeneity across both interest rate premium and loan risk. A sharp rejection of my null hypothesis would therefore condition on both dimensions simultaneously. If high risk, high premium loans in particular experienced a larger treatment effect, then I would definitively fail to reject the null hypothesis that my results are driven by borrower financial constraints instead of profit motives. To test this, I once again repeat the previous exercise by decomposing my primary sample into loans with no delinquency and loans with some delinquency over the past year. I then decompose each subsample into loans with above and below median interest rate premiums.

Table 10 presents just-identified 2SLS estimates that are equivalent to those in in Column (2) of Table 5. These estimates measure the casual effect of buyout on foreclosure across loans with above and below median interest rate premium and across high and low risk. I find that among loans with a high interest rate premium, low risk loans experience a two-fold larger reduction in foreclosures relative to high risk loans. The estimate for high risk loans is statistically insignificant. When the interest rate premium is below median, I find that the effect is exclusively salient for high risk loans. Taken together, these results imply that when issuer incentives are low, then borrower financial constraints determine the size of the treatment effect. When issuer incentives are strong, then the best performing loans experience the largest reduction in foreclosures.

9 Conclusion

This paper examines the role of delinquent loan buyout on loan performance. I exploit quasi-experimental variation in buyout propensity across the vintage months preceding a suspension on rolling delinquency buyout. The policy announcement was unanticipated and affected those loans securitized following a January 1, 2003 deadline. Within a short span of time preceding the shock, loan quality and economic conditions should vary little across vintage months. However, due to the lag between origination and securitization, loans issued in the months immediately prior to the announcement were far less likely to securitize in time to avoid the suspension. I estimate that in the last month alone, a mortgage experiencing a rolling delinquency was 30.6 percentage points less likely to experience a buyout compared to only five months earlier. In response to this drop in buyout activity, I find that the foreclosure rate for loans issued preceding the announcement monotonically increased by 5.7 percentage points and early cures monotonically fell by 7.4 percentage points. I demonstrate that this reduction in cures is not due to interest rate, principal balance, or payments modifications, but likely due to a reduction in term extensions or payment deferrals. Finally, using an instrumental variables framework, I identify policy-relevant causal estimates of the elasticity of buyout and loan performance.

The results in this paper have implications for several areas of future research and policy discussion. First, preventing worsening delinquency and foreclosure in mortgage markets has been of primary importance since the financial crisis. The main result of this paper speaks directly to this concern, whereby I find that delinquent debt sale creates incentives for better loan quality. In order for issuers to resell delinquent debt, they renegotiate with borrowers in a manner that reduces long-term foreclosure risk. Importantly, I find that early cures in particular are effective at reducing foreclosure, as opposed to late cures. Second, the efficacy of the buyout option has recently come into question again in the wake of the recent COVID pandemic. Ginnie Mae for example recently introduced stricter criteria by which an issuer could resecuritize a buyout loan. This paper, in contrast, points to the benefit of the agency buyout option on delinquent debt. I find that this benefit would accrue to investors through better loan quality, and, more directly, benefit borrowers through foreclosure prevention. This finding is of great consequence even beyond Ginnie Mae loans given that \$8.5 trillion worth of agency debt outstanding holds a buyout option on seriously delinquent mortgages. Third, with a face value close to \$1 trillion, financing of distressed debt outside of the mortgage market represents an essential component of asset markets. While my setting is based on agency debt in particular, the dynamics that I identify have implications for creditors in other lending markets as well.

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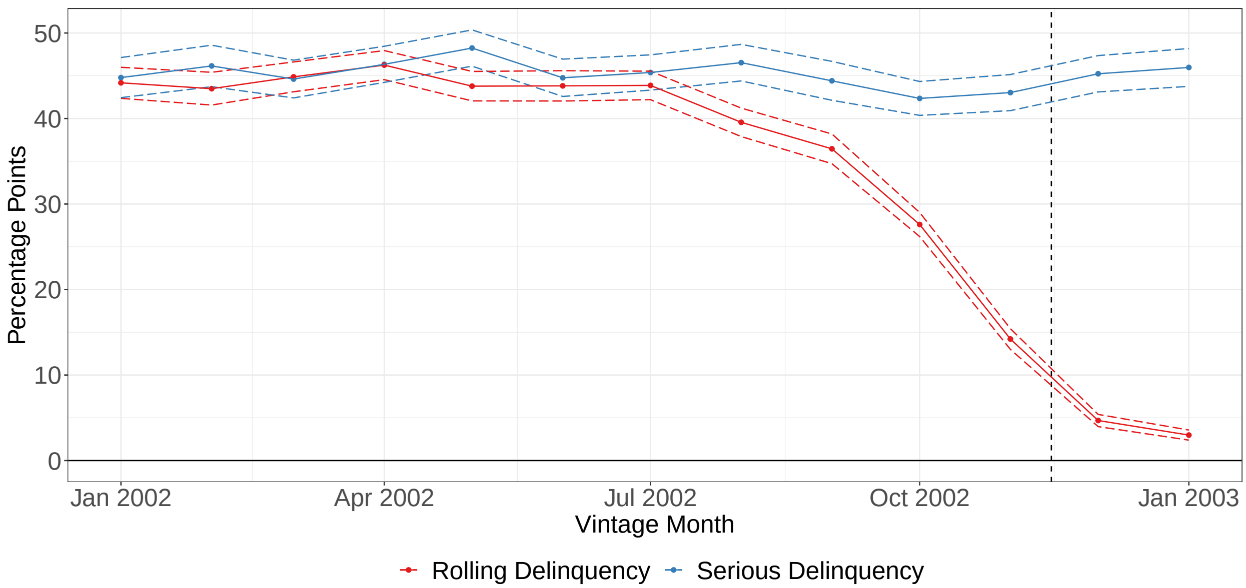
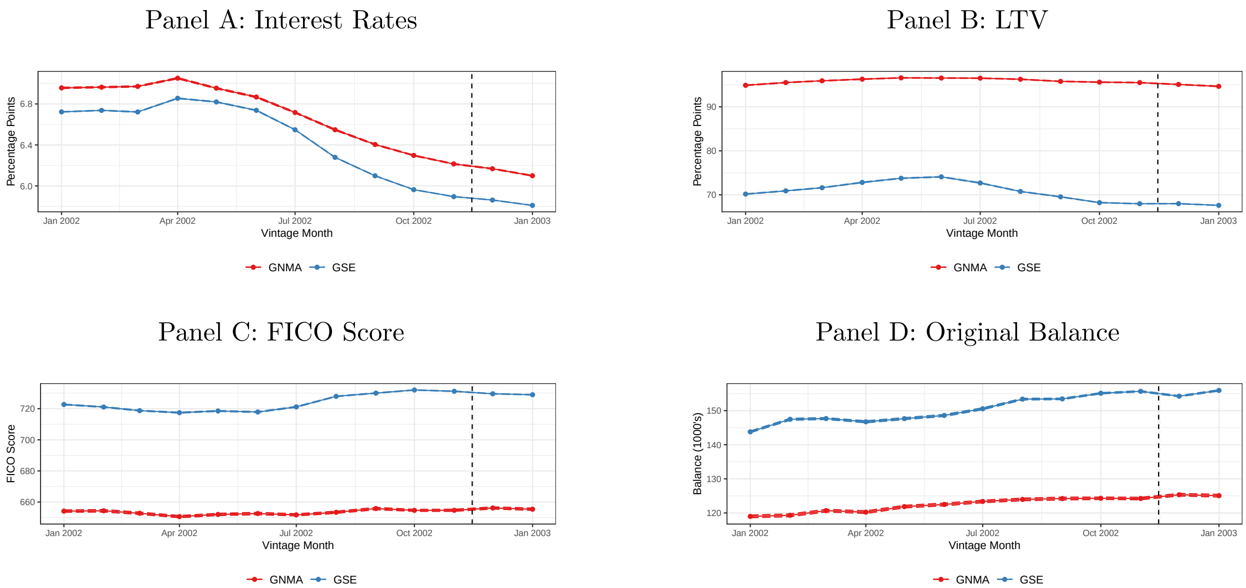
Figure 1. Buyout Rate of Rolling Delinquencies



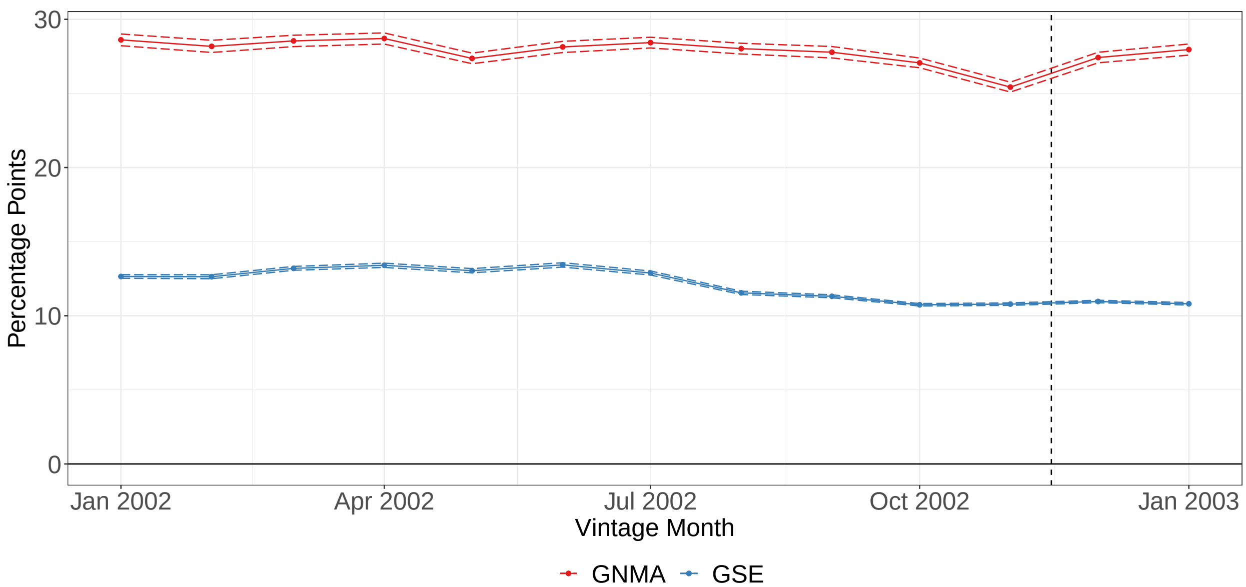
Note: This figure plots the buyout rate of GNMA loans across month of origination, conditional on delinquency. Loans are originated between January 2002 and January 2003. Delinquency is observed between the time of origination and July 2007. Rolling delinquency occurs when a loan holds the status of at least one missed payment for a consecutive four periods. Serious delinquency occurs when a loan enters a status of three missed payments. Each dot represents an estimated average for a given month. The colored dotted lines represent 95% confidence intervals calculated using standard errors. The black dashed vertical line indicates the date of the policy change. Data is collected from Corelogic.

Figure 2. Loan Characteristics Across Origination Month and Investor



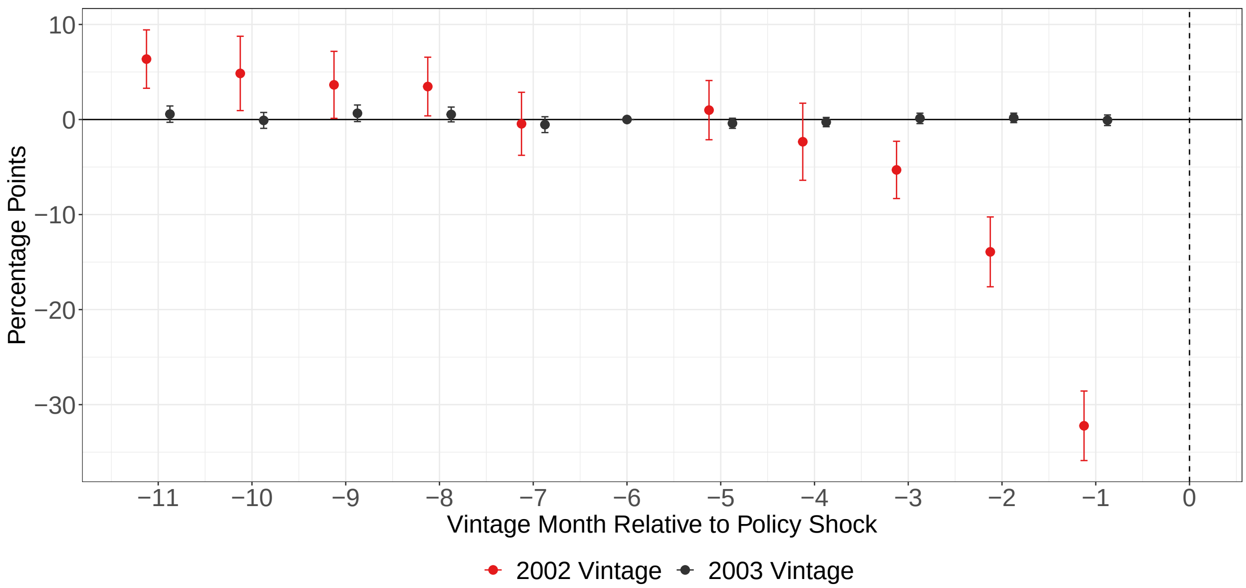
Note: This figure plots loan characteristics of GNMA and GSE loans across month of origination. Loans are originated between January 2002 and January 2003. Panel A plots the interest rate. Panel B plots the loan-to-value ratio. Panel C plots the FICO credit score. Panel D plots the loan balance. Each dot represents an estimated average for a given month. The colored dotted lines represent 95% confidence intervals calculated using standard errors. The black dashed vertical line indicates the date of the policy change. Data is collected from Corelogic.

Figure 3. Delinquency Rate Across Origination Month and Investor



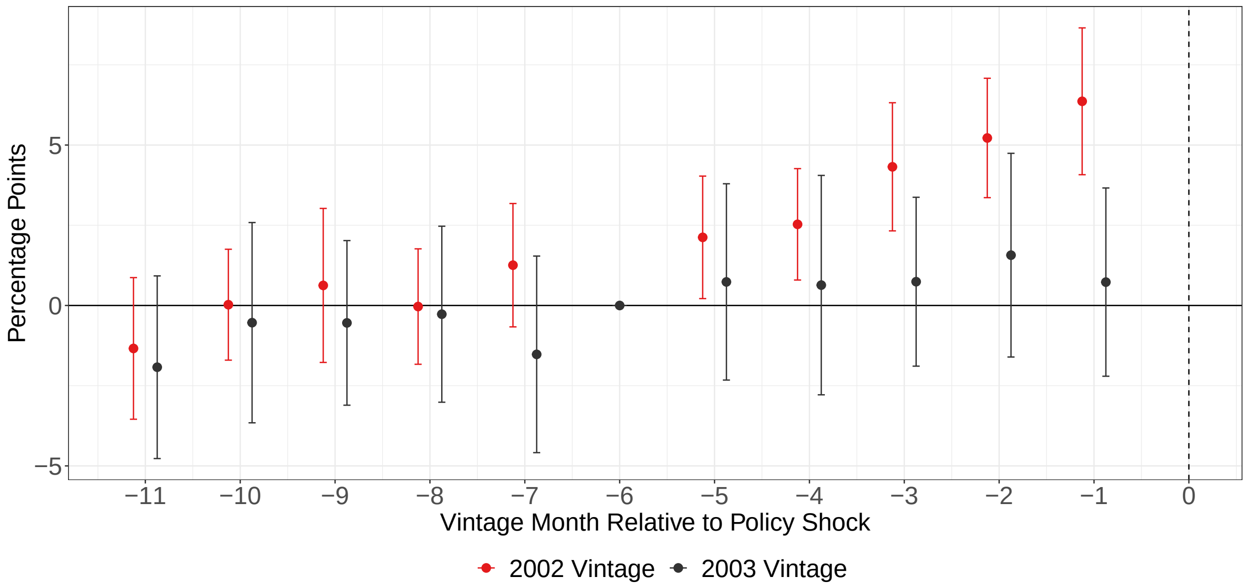
Note: This figure plots the delinquency rate of GNMA and GSE loans across month of origination. Loans are originated between January 2002 and January 2003. Delinquency is observed between the time of origination and July 2007. Delinquency is defined as missing at least one payment since the time of origination. Each dot represents an estimated average for a given month. The colored dotted lines represent 95% confidence intervals calculated using standard errors. The black dashed vertical line indicates the date of the policy change. Data is collected from Corelogic.

Figure 4. Effect of GNMA Policy Change on Buyout



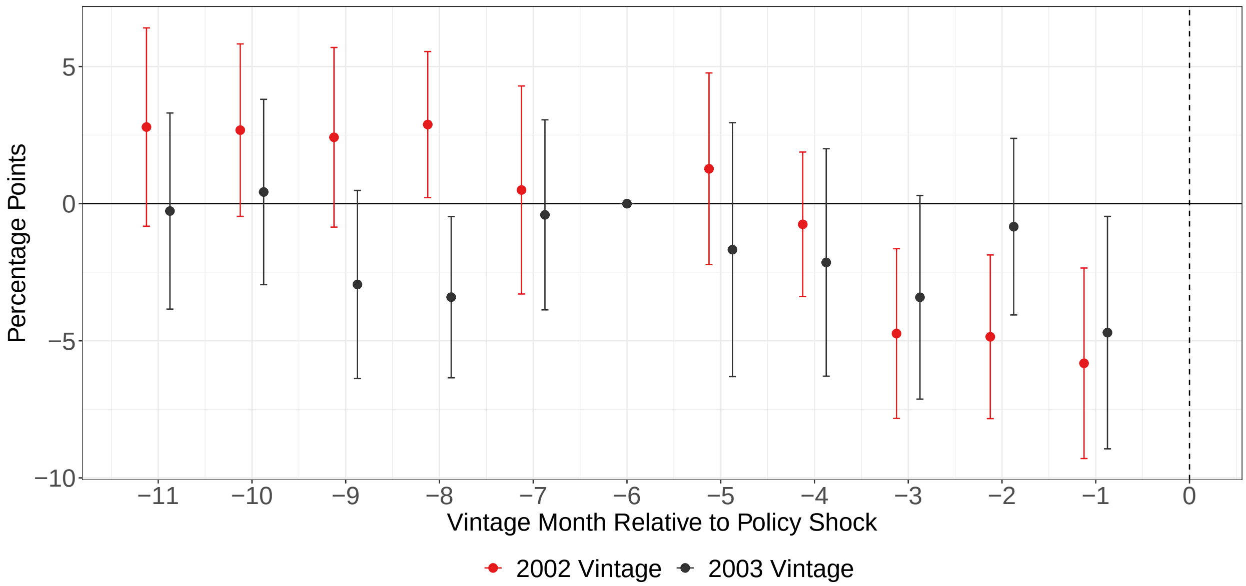
Note: This figure reports OLS regression estimates of the effect of the GNMA policy change on the probability of a buyout, conditional on rolling delinquency and value at above par. The outcome variable takes a value of one if a loan experiences a buyout within 1 month after a rolling delinquency. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Vintage month measures the month of origination relative to the policy or placebo shock, which occurs in November of the sample's calendar year. The coefficient for relative month -6 is normalized to zero. The series in red plots the coefficient estimates using the sample of loans originated in 2002. The series in black plots the coefficient estimates using the sample of loans originated in 2003. The black dashed vertical line indicates the date of the policy change. The error bar represents 95% confidence intervals calculated using MSA-clustered standard errors. Loans are originated between January and November of the sample's calendar year. Outcomes are observed between the time of origination and July 2007. Data is collected from Corelogic.

Figure 5. Effect of GNMA Policy Change on Foreclosure



Note: This figure reports OLS regression estimates of the effect of the GNMA policy change on the probability of a foreclosure, conditional on rolling delinquency and value at above par. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Vintage month measures the month of origination relative to the policy or placebo shock, which occurs in November of the sample's calendar year. The coefficient for relative month -6 is normalized to zero. The series in red plots the coefficient estimates using the sample of loans originated in 2002. The series in black plots the coefficient estimates using the sample of loans originated in 2003. The black dashed vertical line indicates the date of the policy change. The error bar represents 95% confidence intervals calculated using MSA-clustered standard errors. Loans are originated between January and November of the sample's calendar year. Outcomes are observed between the time of origination and July 2007. Data is collected from Corelogic.

Figure 6. Effect of GNMA Policy Change on Early Cure

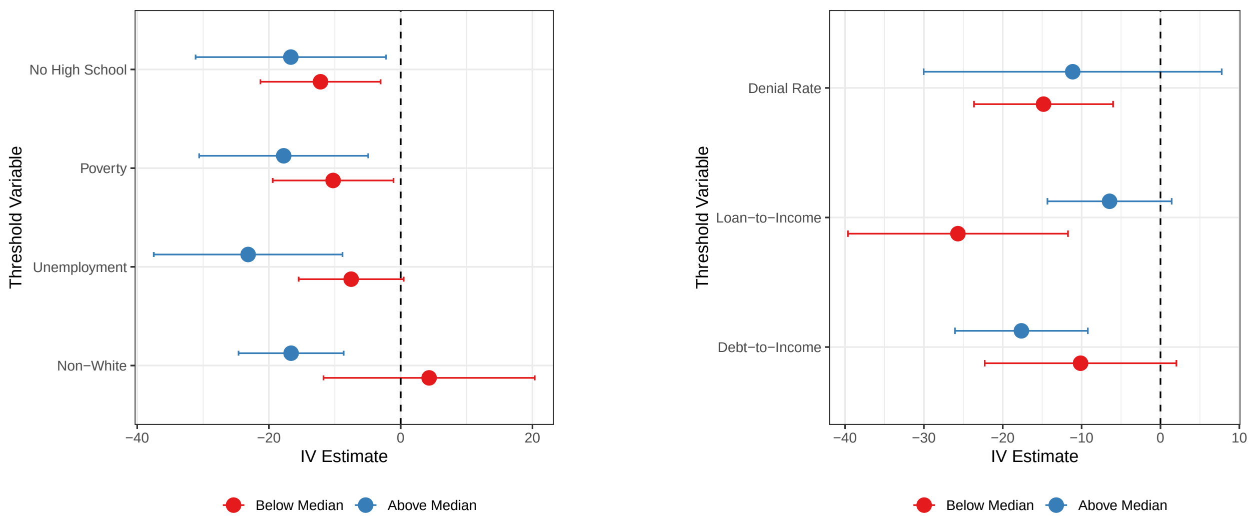


Note: This figure reports OLS regression estimates of the effect of the GNMA policy change on the probability of a cure, conditional on rolling delinquency and value at above par. The outcome variable takes a value of one if a loan experiences a cure within 3 months after a rolling delinquency. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Vintage month measures the month of origination relative to the policy or placebo shock, which occurs in November of the sample's calendar year. The coefficient for relative month -6 is normalized to zero. The series in red plots the coefficient estimates using the sample of loans originated in 2002. The series in black plots the coefficient estimates using the sample of loans originated in 2003. The black dashed vertical line indicates the date of the policy change. The error bar represents 95% confidence intervals calculated using MSA-clustered standard errors. Loans are originated between January and November of the sample's calendar year. Outcomes are observed between the time of origination and July 2007. Data is collected from Corelogic.

Figure 7. Effect of GNMA Policy Change on Foreclosure: Heterogeneity

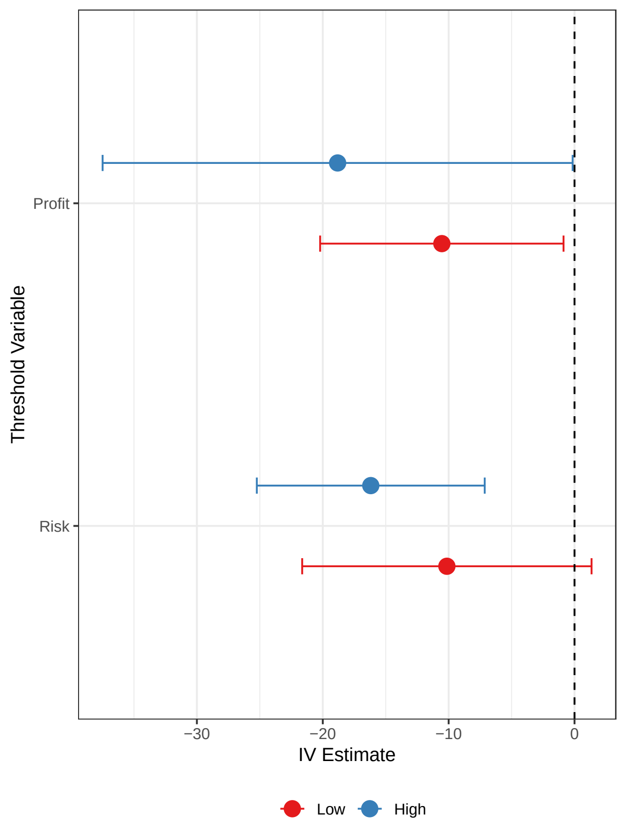
Panel A: Financial Constraints

Panel B: Financial Slack



Note: This figure reports heterogeneous treatment effect estimates of buyout on foreclosure. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. Buyout is instrumented with $Z_{\tau>-3}$ from Column 2 in Table 4. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Point estimates are obtained by adding the baseline treatment effect and the corresponding interaction. The error bar represents 95% confidence intervals calculated using MSA-clustered standard errors. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007. Data is collected from Corelogic.

Figure 8. Effect of GNMA Policy Change on Foreclosure: Profit Motive



Note: This figure reports heterogeneous treatment effect estimates of buyout on foreclosure. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. Buyout is instrumented with $Z_{\tau>-3}$ from Column 2 in Table 4. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. High and low profit is defined by the rate spread on the loan. High and low risk is defined by any prior delinquency in the past year. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Point estimates are obtained by adding the baseline treatment effect and the corresponding interaction. The error bar represents 95% confidence intervals calculated using MSA-clustered standard errors. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007. Data is collected from Corelogic.

Table 1. Summary Statistics

	All Loans		Buyout		No Buyout	
	Mean	SD	Mean	SD	Mean	SD
Panel A: Primary Sample						
Initial Rate	6.76	0.44	6.84	0.40	6.70	0.46
Original LTV	96.13	7.07	96.10	6.87	96.15	7.21
Balance (000's)	112.74	43.12	109.57	42.09	115.13	43.73
15 Month Term	0.02	0.14	0.02	0.14	0.02	0.14
20 Month Term	0.03	0.16	0.03	0.16	0.03	0.16
Prime Borrower	0.45	0.50	0.46	0.50	0.44	0.50
Refinance Loan	0.24	0.43	0.21	0.41	0.27	0.44
Primary Occupancy	0.83	0.37	0.82	0.38	0.84	0.37
Single-Family	0.84	0.37	0.85	0.35	0.82	0.38
Low Doc/No Doc	0.26	0.44	0.21	0.40	0.31	0.46
Number of Obs	13,432		5,758		7,674	
Panel B: Pre-Period						
Buyout	0.52	0.50	1.00	0.00	0.00	0.00
Foreclosure	0.15	0.36	0.15	0.36	0.15	0.36
Early Cure	0.47	0.50	0.47	0.50	0.47	0.50
Late Cure	0.74	0.44	0.75	0.43	0.74	0.44
Modification	0.00	0.02	0.00	0.02	0.00	0.01
Payment Change	0.03	0.17	0.02	0.13	0.04	0.19
Number of Obs	11,439		5,905		5,534	

Note: This table reports mean and standard deviation values of loan characteristics and outcomes for GNMA loans, conditional on rolling delinquency and value at above par. Panel A includes loans originated between June 2002 and November 2002. Panel B includes loans originated between January 2002 and May 2002. Outcomes are observed between the time of origination and July 2007. Buyout is measured within 1 month of rolling delinquency. Foreclosure is measured within 18 months of rolling delinquency. Early cure, modification, and payment change are measured within 3 months of rolling delinquency. Late cure is measured within 12 months of rolling delinquency. Data is collected from Corelogic.

Table 2. Effect of Buyout on Foreclosure

	Foreclosure				
	(1)	(2)	(3)	(4)	(5)
Buyout	-0.000	-0.003	-0.003	0.006	0.005
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Controls		X	X	X	X
MSA FE			X		X
Month FE				X	X
Obs	11,439	11,236	11,236	11,236	11,236

Note: This table reports OLS regression estimates of the effect of the buyout on foreclosure, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. Buyout takes a value of one if a loan experiences a buyout within 1 month after a rolling delinquency. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. Columns 2 includes controls. Column 3 includes fixed effects for MSA. Column 4 includes fixed effects for month of delinquency. Column 5 includes fixed effects for MSA and month of delinquency. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between January 2002 and May 2002 and outcomes observed between the time of origination and July 2007.

Table 3. Effect of the GNMA Policy Change on Buyout

	Buyout					
	(1)	(2)	(3)	(4)	(5)	(6)
Z_{-5}	0.010		-0.003**		0.014	
	(0.016)		(0.002)		(0.009)	
Z_{-4}	-0.014		-0.003**		0.011	
	(0.022)		(0.001)		(0.007)	
Z_{-3}	-0.042**		-0.001		0.011	
	(0.017)		(0.002)		(0.007)	
Z_{-2}	-0.126***		-0.003**		0.013**	
	(0.023)		(0.001)		(0.007)	
Z_{-1}	-0.306***		-0.002		0.011*	
	(0.022)		(0.002)		(0.006)	
$Z_{\tau>-3}$		-0.186***		-0.001		0.001
		(0.012)		(0.001)		(0.003)
MSA FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Obs	13,204	13,204	7,109	7,109	3,153	3,153

Note: This table reports OLS regression estimates of the effect of the vintage month on buyout, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a buyout within 1 month after a rolling delinquency. The Z_τ dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. The sample in Columns 1 and 2 consists of loans originated in 2002. The sample in Columns 3 and 4 consists of loans originated in 2003. The sample in Columns 5 and 6 consists of loans originated in 2002, but valued at below par. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between January and May of the sample's calendar year and outcomes observed between the time of origination and July 2007.

Table 4. Effect of the GNMA Policy Change on Foreclsoure

	Foreclosure					
	(1)	(2)	(3)	(4)	(5)	(6)
Z_{-5}	0.018*		0.004		-0.051	
	(0.010)		(0.016)		(0.048)	
Z_{-4}	0.022**		0.009		-0.031	
	(0.010)		(0.019)		(0.044)	
Z_{-3}	0.037***		0.016		0.011	
	(0.011)		(0.015)		(0.038)	
Z_{-2}	0.045***		0.023		-0.029	
	(0.010)		(0.018)		(0.041)	
Z_{-1}	0.057***		0.010		-0.015	
	(0.012)		(0.016)		(0.042)	
$Z_{\tau>-3}$		0.026***		0.008		-0.016
		(0.007)		(0.009)		(0.012)
MSA FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Obs	13,204	13,204	7,109	7,109	3,153	3,153

Note: This table reports OLS regression estimates of the effect of the vintage month on foreclosure, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. The Z_τ dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. The sample in Columns 1 and 2 consists of loans originated in 2002. The sample in Columns 3 and 4 consists of loans originated in 2003. The sample in Columns 5 and 6 consists of loans originated in 2002, but valued at below par. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between January and May of the sample's calendar year and outcomes observed between the time of origination and July 2007.

Table 5. Effect of the GNMA Policy Change on Early Cure

	Early Cure					
	(1)	(2)	(3)	(4)	(5)	(6)
Z_{-5}	0.011		-0.013		-0.011	
	(0.018)		(0.024)		(0.106)	
Z_{-4}	-0.013		-0.023		-0.035	
	(0.015)		(0.022)		(0.086)	
Z_{-3}	-0.058***		-0.041**		-0.036	
	(0.017)		(0.020)		(0.078)	
Z_{-2}	-0.064***		-0.009		-0.034	
	(0.017)		(0.018)		(0.086)	
Z_{-1}	-0.074***		-0.046**		-0.041	
	(0.019)		(0.022)		(0.084)	
$Z_{\tau>-3}$		-0.046***		-0.001		-0.005
		(0.010)		(0.011)		(0.019)
MSA FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Obs	13,204	13,204	7,109	7,109	3,153	3,153

Note: This table reports OLS regression estimates of the effect of the vintage month on cure, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a cure within 3 months after a rolling delinquency. The Z_τ dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. The sample in Columns 1 and 2 consists of loans originated in 2002. The sample in Columns 3 and 4 consists of loans originated in 2003. The sample in Columns 5 and 6 consists of loans originated in 2002, but valued at below par. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between January and May of the sample's calendar year and outcomes observed between the time of origination and July 2007.

Table 6. Effect of the GNMA Policy Change on Loan Performance: Mechanism

	Late Cure		Modification		Payment Change	
	(1)	(2)	(3)	(4)	(5)	(6)
Z_{-5}	0.001		-0.001		-0.003	
	(0.014)		(0.001)		(0.005)	
Z_{-4}	-0.008		-0.001		-0.014***	
	(0.013)		(0.001)		(0.005)	
Z_{-3}	-0.020		-0.001		-0.010**	
	(0.014)		(0.001)		(0.004)	
Z_{-2}	-0.018		-0.000		-0.006	
	(0.015)		(0.001)		(0.005)	
Z_{-1}	-0.041**		-0.001		-0.006	
	(0.017)		(0.001)		(0.005)	
$Z_{\tau>-3}$		-0.018**		0.000		0.002
		(0.008)		(0.000)		(0.003)
MSA FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Obs	13,204	13,204	13,204	13,204	13,204	13,204

Note: This table reports OLS regression estimates of the effect of the vintage month on loan outcomes, conditional on rolling delinquency and value at above par. Observations are at the loan level. In Columns 1 and 2, the outcome variable takes a value of one if a loan experiences a cure within 12 months after a rolling delinquency. In Columns 3 and 4, the outcome variable takes a value of one if a loan experiences an interest rate reduction, principal increase, or principal reduction within 3 months after a rolling delinquency. In Columns 4 and 5, the outcome variable takes a value of one if a loan experiences a payment change within 3 months after a rolling delinquency. The Z_τ dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007.

Table 7. Effect of the GNMA Policy Change on Foreclsoures: All Agency Loans

	Foreclosure			
	(1)	(2)	(3)	(4)
Z_{-5}	0.020*			
	(0.011)			
Z_{-4}	0.003			
	(0.011)			
Z_{-3}	0.016			
	(0.011)			
Z_{-2}	0.016			
	(0.012)			
Z_{-1}	0.022**			
	(0.010)			
$Z_{-5}\times$GNMA			0.001	
			(0.015)	
$Z_{-4}\times$GNMA			0.025*	
			(0.014)	
$Z_{-3}\times$GNMA			0.026**	
			(0.013)	
$Z_{-2}\times$GNMA			0.037***	
			(0.014)	
$Z_{-1}\times$GNMA			0.042***	
			(0.013)	
$Z_{\tau>-3}$		0.008		
		(0.007)		
$Z_{\tau>-3}\times$ GNMA				0.027***
				(0.008)
MSA FE	X	X	X	X
Month FE	X	X	X	X
Vintage FE			X	X
GNMA FE			X	X
Obs	14,463	14,463	27,667	27,667

Note: This table reports OLS regression estimates of the effect of the vintage month on foreclosure, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. The Z_τ dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. The GNMA dummy takes a value of one if a loan is associated with GNMA. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Columns 3 and 4 include fixed effects for vintage month and GNMA status. The sample in Columns 1 and 2 consists of loans associated with the GSE's. The sample in Columns 3 and 4 consists of all agency loans. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007.

Table 8. Effect of the GNMA Policy Change on Early Cure: All Agency Loans

	Early Cure			
	(1)	(2)	(3)	(4)
Z_{-5}	-0.001			
	(0.015)			
Z_{-4}	-0.012			
	(0.014)			
Z_{-3}	-0.007			
	(0.014)			
Z_{-2}	-0.014			
	(0.014)			
Z_{-1}	-0.008			
	(0.017)			
$Z_{-5}\times$GNMA			0.017	
			(0.021)	
$Z_{-4}\times$GNMA			0.013	
			(0.017)	
$Z_{-3}\times$GNMA			-0.027	
			(0.020)	
$Z_{-2}\times$GNMA			-0.021	
			(0.020)	
$Z_{-1}\times$GNMA			-0.033	
			(0.021)	
$Z_{\tau>-3}$		-0.005		
		(0.010)		
$Z_{\tau>-3}\times$ GNMA				-0.028**
				(0.013)
MSA FE	X	X	X	X
Month FE	X	X	X	X
Vintage FE			X	X
GNMA FE			X	X
Obs	14,463	14,463	27,667	27,667

Note: This table reports OLS regression estimates of the effect of the vintage month on cure, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a cure within 3 months after a rolling delinquency. The Z_τ dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. The GNMA dummy takes a value of one if a loan is associated with GNMA. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Columns 3 and 4 include fixed effects for vintage month and GNMA status. The sample in Columns 1 and 2 consists of loans associated with the GSE's. The sample in Columns 3 and 4 consists of all agency loans. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007.

Table 9. Effect of the GNMA Policy Change on Loan Performance: Instrumental Variables

	Loan Performance			
	(1)	(2)	(3)	(4)
Panel A: Foreclosure				
Buyout	-0.124***	-0.140***	-0.116***	-0.125***
	(0.034)	(0.040)	(0.035)	(0.041)
Sargan-Hansen Test (p-value)	0.075	-	0.331	-
Panel B: Early Cure				
Buyout	0.215***	0.248***	0.132**	0.130**
	(0.046)	(0.057)	(0.053)	(0.062)
Sargan-Hansen Test (p-value)	0.001	-	0.404	-
Excluded Instrument	Z_τ	$Z_{\tau>-3}$	$Z_\tau \times$ GNMA	$Z_{\tau>-3} \times$ GNMA
Cragg-Donald Statistic	106	355	168	633
Kleibergen-Paap Statistic	116	232	167	406
MSA FE	X	X	X	X
Month FE	X	X	X	X
Vintage FE			X	X
GNMA FE			X	X
Obs	13,204	13,204	27,667	27,667

Note: This table reports instrumental variables (two-stage least-squares) estimates of regressions of foreclosure and cure preceding the GNMA policy announcement. Observations are at the loan level. The outcome variable in Panel A takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. The outcome variable in Panel B takes a value of one if a loan experiences a cure within 3 months after a rolling delinquency. Buyout is instrumented with Z_τ, $Z_{\tau>-3}$,$Z_\tau \times$ GNMA, and $Z_{\tau>-3} \times$ GNMA in Columns 1 to 4, respectively. The Z_τ dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. The GNMA dummy takes a value of one if a loan is associated with GNMA. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Columns 3 and 4 include fixed effects for vintage month and GNMA status. Standard errors are reported in parentheses and are clustered by MSA. The sample in Columns 1 and 2 consists of loans associated with GNMA. The sample in Columns 3 and 4 consists of all agency loans. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007.

Table 10. Effect of the GNMA Policy Change on Loan Performance: Sample Splits

	Foreclosure			
	Low Profit		High Profit	
	Low Risk	High Risk	Low Risk	High Risk
	(1)	(2)	(3)	(4)
Buyout	-0.018	-0.190***	-0.275*	-0.122
	(0.070)	(0.068)	(0.159)	(0.099)
MSA FE	X	X	X	X
Month FE	X	X	X	X
Obs	3,647	4,703	2,460	2,394

Note: This table reports instrumental variables (two-stage least-squares) estimates of regressions of foreclosure and cure preceding the GNMA policy announcement. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. Buyout is instrumented with $Z_{\tau>-3}$ from Column 2 in Table 4. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. High and low profit is defined by the rate spread on the loan. High and low risk is defined by any prior delinquency in the past year. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Standard errors are reported in parentheses and are clustered by MSA. The sample in Columns 1 and 2 consists of loans associated with GNMA. The sample in Columns 3 and 4 consists of all agency loans. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007.