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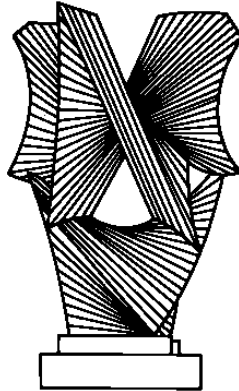
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## MORTGAGE MODIFICATION AND STRATEGIC BEHAVIOR: EVIDENCE FROM A LEGAL SETTLEMENT WITH COUNTRYWIDE

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# Mortgage Modification and Strategic Behavior: Evidence from a Legal Settlement with Countrywide\*

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## **Abstract**

We investigate whether homeowners respond strategically to news of mortgage modification programs by defaulting on their mortgages. We exploit plausibly exogenous variation in modification policy induced by U.S. state government lawsuits against Countrywide Financial Corporation, which agreed to offer modifications to seriously delinquent borrowers with mortgages throughout the country. Using a difference-in-difference framework, we find that Countrywide's relative delinquency rate increased more than ten percent per month immediately after the program's announcement. The borrowers whose estimated default rates increased the most in response to the program were those who appear to have been the least likely to default otherwise, including those with substantial liquidity available through credit cards and relatively low combined loan-to-value ratios. These results suggest that strategic behavior of borrowers should be an important consideration in designing mortgage modification programs.

# 1 Introduction

Debt relief programs have a long history and have attracted renewed interest during the recent financial crisis, which has seen more than five million U.S. homeowners lose their homes to foreclosure since 2007.<sup>1</sup> The potential benefits and costs of these programs are well-known. During a crisis, mortgage debt relief can prevent excessive foreclosures, which yield losses for both borrowers and lenders and may also generate negative externalities for surrounding communities.<sup>2</sup> Debt relief could also have macroeconomic benefits to the extent that high household leverage depresses aggregate consumption and employment, as Mian and Sufi (2012) show. On the other hand, debt relief can distort the incentives of homeowners, who may default on mortgages in order to qualify for relief even though they could continue making debt payments, at least in the near term. Strategic behavior like this not only increases the cost of debt relief programs to the lenders, but may also raise the long-run price of credit, if borrowers and lenders anticipate future debt relief programs.

Despite the economic importance of debt relief programs, there is little empirical evidence on their effects. This paper presents evidence on their costs. We study a recent mortgage modification program with a simple eligibility criterion—borrowers in default qualified—and estimate the extent to which the program affected borrower incentives to default.

Our approach is motivated by a key trade-off in the design of debt relief programs. In principle, a cost-effective program should apply eligibility criteria that efficiently identify homeowners who are highly likely to default unless they receive relief.

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<sup>1</sup>March 2011 CoreLogic Data Release.

<sup>2</sup>Several papers explore the potential benefits of debt relief to both borrowers and lenders during adverse economic conditions, including Bolton and Rosenthal (2002), Kroszner (2003), and Piskorski and Tchistyi (2011). Because foreclosures may exert significant negative externalities (see, for example, Campbell et al. 2009 and Mian et al. 2011), it might be socially optimal to modify mortgage contracts to a greater extent than lenders would select independently.

In practice, it is costly and difficult to identify these at-risk homeowners because homeowner default decisions can depend on hard-to-observe factors such as their financial ability to service debt, their private valuation of their homes, and their personal default costs. In 2011, for example, more than ten million U.S. homeowners—about one out of every four with a mortgage—were “underwater” on their mortgages because their mortgage debts exceed their estimated home values. An underwater homeowner is generally thought to be at high risk of default, yet a majority of these homeowners made timely mortgage payments throughout 2011, and may continue doing so without debt relief.<sup>3</sup>

One approach to the problem of identifying renegotiation recipients is to extend benefits only to homeowners who are delinquent. For example, a number of recent modification programs have made benefits available to homeowners who failed to make at least two monthly mortgage payments (such homeowners are at least “sixty days delinquent”).<sup>4</sup> This approach, however, could induce homeowners to default in order to obtain modification benefits even though they would not have defaulted otherwise. Although lenders and policymakers are aware of this “strategic behavior” problem, they often favor such an approach because the costs of delinquency are thought to be sufficiently high to deter strategic behavior by most homeowners. Seriously delinquent borrowers, for example, face higher costs of accessing liquidity through credit cards, auto loans, and any new mortgages or refinancings. Additionally, bounded rationality or moral considerations may decrease a borrower’s ability or willingness to behave strategically (see, for example, Guiso, et al. 2009).

An alternative way to target modification benefits—and reduce the risk of strategic behavior—is to offer benefits only to homeowners who complete a rigorous

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<sup>3</sup>See, e.g., March 2011 Written Testimony of David H. Stevens Assistant Secretary of Housing - Federal Housing Administration Commissioner U.S. Department of Housing and Urban Development.

<sup>4</sup>The primary example of this approach is the Bank of America/Countrywide modification program. Other programs, like the IndyMac/FDIC program, JP Chase Enhanced Program, Citi Homeownership Preservation Program, and GSE Streamlined Modification Program have also targeted seriously delinquent borrowers, though some include additional eligibility requirements. See Citigroup (2009).

audit that verifies that they are likely to default, or have defaulted, as a result of meaningful adverse conditions.<sup>5</sup> Such an audit, for example, would assess the home’s value and the homeowner’s current income and credit rating. Because this approach can be time-consuming, however, it may fail to extend benefits to homeowners before they enter foreclosure or decide to exit their homes, and could thereby lead to higher costs for borrowers, lenders, and surrounding communities.

These alternatives present a trade-off. One approach extends modification benefits quickly using a simple delinquency requirement, but generates potential strategic behavior. The other extends benefits more slowly using costly verification methods, minimizing strategic behavior, but potentially increases the number of foreclosures and results in higher costs for borrowers and lenders. A key factor affecting this trade-off, at least from the perspective of lenders (or mortgage investors), is the extent to which simple delinquency requirements generate strategic behavior.

In this paper, we provide evidence on the extent to which strategic behavior is induced by modification programs that use simple delinquency requirements to target benefits. We study a recent modification program—implemented by Countrywide Financial Corporation—that extended benefits to homeowners who were at least sixty days delinquent. The Countrywide program was the product of litigation commenced during Summer 2008 by U.S. state attorneys general, who alleged that Countrywide had engaged in deceptive lending practices. In October 2008, as part of a widely publicized Settlement, Countrywide agreed to modify all subprime mortgages that it serviced throughout the nation beginning in December 2008.<sup>6</sup>

A centerpiece of this Settlement was Countrywide’s commitment to offer expedited, unsolicited loan modifications to borrowers who were at least sixty days delin-

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<sup>5</sup>An example of this approach is Home Affordable Modification Program introduced in March 2009, which contains multiple eligibility requirements along with a trial period preceding any permanent modification.

<sup>6</sup>Although Countrywide had been acquired by Bank of America, the Settlement applied only to Countrywide mortgages.

quent. Three features of the Countrywide Settlement—its unexpected public announcement in advance of its implementation, nationwide coverage, and the requirement that a borrower be delinquent in order to receive benefits—make it a potentially useful setting for assessing borrower behavior in response to the offer of mortgage modification featuring a simple delinquency requirement.

We examine borrower responses to the public announcement of the Countrywide Settlement using an extensive dataset with information about all privately securitized mortgages, including the name of the servicer, origination mortgage amount and interest rate, origination FICO, and monthly payment history. We use a match of this mortgage-level dataset with the borrower-level data supplied by Equifax, one of the three major credit bureaus. The Equifax data include the borrowers’ updated credit scores as well as their payment histories and utilization rates for credit cards, mortgages, second liens, and other sources of credit. These data allow us to track homeowner credit behavior during the months before and after an initial default on a mortgage and examine the extent to which borrowers who default on mortgages pay their other debts.

Most of our analysis focuses on 2/28 hybrid adjustable-rate mortgages (2/28 ARMs), which were aimed primarily at subprime borrowers. These mortgages offer a relatively low introductory “teaser” rate for the first two years, after which the rate typically resets to a higher level (indexed to LIBOR or Treasury rates) for the remaining 28 years of the loan term. We show below that the loan characteristics of Countrywide’s 2/28 ARMs were comparable to those of other servicers prior to announcement of the Settlement.

We say that a borrower exhibits “strategic behavior” if he or she defaults in response to the public announcement of the Settlement and would not have defaulted otherwise, at least in the near term. In a difference-in-difference framework, we estimate the percentage increase in defaults among Countrywide borrowers during the



months immediately following the Settlement announcement *relative to* the percentage increase during the same period among comparable borrowers who were unaffected by the Settlement because their loans were not serviced by Countrywide (the “Control Group”). In regressions controlling for many borrower attributes, including current credit scores and indebtedness, we find a ten to eleven percent relative increase in the overall probability that previously-current Countrywide borrowers miss two payments in a row—“roll straight” from current to sixty days delinquent—during the three months immediately after the Settlement announcement.<sup>7</sup> The effect of the Settlement is even larger—sixteen to eighteen percent per month—when we subset on borrowers with (i) substantial available credit through credit cards and (ii) lower current combined loan-to-value (CLTV) ratios. These borrowers were arguably less likely to default in the near term because they had significant untapped liquidity through credit cards or some positive equity in their homes.

We confirm that these results are not driven by idiosyncratic features of Countrywide loans or borrowers. Although we observe an increase in relative default rates among Countrywide loans targeted by the Settlement (subprime first lien mortgages), we do not observe an increase in relative default rates among loans not targeted by the Settlement. Default rates on credit cards and second mortgages held by Countrywide borrowers did not increase relative to default rates among Control Group borrowers. Nor do we observe an increase in relative default rates among non-subprime fixed-rate mortgages (FRMs) held by Countrywide borrowers.

Together, these results inform ongoing discussions on the trade-off between (i) quickly-implemented programs with simple but possibly manipulable eligibility criteria and (ii) slowly-implemented programs with more rigorous verification of homeowner distress. The Countrywide Settlement was a quickly-implemented program with a

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<sup>7</sup>By “roll straight” we mean that loans migrate from a “current” status to a “sixty-days delinquent” status during a two month period, implying that previously-current (paid up) borrowers missed two payments in a row.

simple eligibility criterion. We find non-trivial effects on strategic behavior—a sixteen to eighteen percent monthly relative increase in default rates among the borrowers least likely to default otherwise—during the months immediately after its announcement. Further research is needed to determine whether the costs of such strategic behavior are large relative to the potential benefits of a simple modification program that quickly extends benefits to a large number of homeowners.

Previous studies of incentives and strategic behavior in the context of the recent crisis have examined a number of questions, including the impact of bailouts and regulatory design on banks’ incentives to take risk,<sup>8</sup> the likelihood that some lenders originated mortgages with greater risk due to their ability to sell the loans in the securitized market,<sup>9</sup> and the impact of securitization on servicer decisions to foreclose or renegotiate delinquent loans<sup>10</sup>. Little attention has been given so far to strategic behavior among homeowners.

Our analysis is also broadly connected to the household finance literature, surveyed by Campbell (2006) and Tufano (2009), especially the recent empirical literature examining household motives behind mortgage defaults. Most of this recent literature aims to assesses the relative importance of two key drivers of mortgage default: negative equity and illiquidity.<sup>11</sup> Guiso et al. (2009) also explore how moral and social considerations affect the decision to default on a mortgage. To the best of our knowledge, our paper is the first to assess the effect of mortgage modification programs on incentives to default on a mortgage.

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<sup>8</sup>See Farhi and Tirole (2009) and Poole (2009), for example, for the analysis of bailouts. See Agarwal et al. (2012) that examine differences between federal and state regulators and their impact on banks’ decisions.

<sup>9</sup>Mian and Sufi (2009), Keys, et al. (2010, 2011), Rajan et al. (2010), Berndt and Gupta (2009), and Purnanandam (2010) provide evidence suggesting that originators might have made riskier loans when they were able to securitize these loans.

<sup>10</sup>Piskorski et al. (2010) show that bank-held delinquent loans were foreclosed at a lower rate relative to comparable mortgages that were securitized. Agarwal et al. (2011) corroborate their findings and provide further evidence that bank-held loans were much more likely to be renegotiated than comparable securitized mortgages.

<sup>11</sup>See, among others, Foote et al. (2008), Cohen-Cole and Morse (2010), and Elul et al. (2010). See also Mian and Sufi (2011) who examine the role of the home equity-based borrowing channel in the recent crisis using a data set consisting of individual credit files.

Our paper is also related to the empirical literature examining the effects of various policies on household behavior, such as the impact of unemployment insurance on workers' incentives to work.<sup>12</sup> We contribute to this literature by examining the effects of mortgage modification policy on borrowers' incentives to repay their loans. Finally, our paper helps inform the empirical literature on contract renegotiation.<sup>13</sup>

Our paper is organized as follows. In Section 2 we describe the Countrywide Settlement and our hypotheses regarding its effects on homeowner behavior. Sections 3 and 4 describe our data and empirical methodology. We present our results in Section 5 and discuss their implications for the design of mortgage modification policies in Section 6.

## 2 Countrywide Settlement and Hypotheses

### 2.1 The Settlement

In June 2008, attorneys general in California and Illinois brought suit against Countrywide, alleging deceptive lending practices. The California complaint, for example, alleged that Countrywide, purchased by Bank of America, had “implemented [a] deceptive scheme through misleading marketing practices designed to sell risky and costly loans to homeowners, the terms and dangers of which they did not understand.”<sup>14</sup> Over the next three months, similar suits were brought by attorneys general in over thirty other states.

On October 6, 2008, Countrywide entered a multi-state Settlement, pursuant to which it agreed to extend offers of loan modification to all seriously delinquent or near-

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<sup>12</sup>See, for example, Meyer (1990) and Krueger and Meyer (2002).

<sup>13</sup>See, among others, recent research by Benmelech and Bergman (2008) and Roberts and Sufi (2009) in the context of corporate default, and Matvos (2009) for renegotiation in NFL football contracts.

<sup>14</sup>State of California (2008a, p. 5). See also State of Illinois (2008).

delinquent subprime first-mortgage loans<sup>15</sup> that it services throughout the nation.<sup>16</sup> It is irrelevant whether the loan was originated by Countrywide, whether it is securitized or held in Countrywide’s portfolio,<sup>17</sup> whether it previously received a modification, or whether the borrower’s home is encumbered by a second mortgage or junior lien.

The Settlement targets subprime first mortgages currently serviced by Countrywide, including hybrid ARMs and Option ARMs. To qualify for modification, the mortgage and borrower must satisfy four criteria: The loan must have been originated before 2008 and have been within Countrywide’s servicing portfolio on June 30, 2008; the borrower’s loan-to-value ratio (LTV) must be at least seventy-five percent; payments of principal or interest must be sixty or more days delinquent (or likely to become delinquent as a result of an interest rate reset or negative amortization trigger); and the borrower’s post-modification mortgage payments must not exceed certain thresholds.<sup>18</sup> The program was scheduled to last till June 30, 2012.

With respect to subprime hybrid ARMs, which are the primary focus of this paper, seriously delinquent borrowers should be considered for *unsolicited* restoration of the introductory interest rate for five years. Additionally, all seriously delinquent Hybrid ARM borrowers must be considered for some type of fully-amortizing loan modification. One type would reduce the interest rate for five years (to as low as 3.5 percent), after which the loan would be converted to an FRM at a low rate.

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<sup>15</sup>The Settlement defined a subprime first mortgage as one that “is identified as such in connection with a securitization in which it is part of the pool of securitized assets or, in the case of a [Countrywide] Residential Mortgage Loan that is not included in a securitization, was classified as being ‘subprime’ on the systems of [Countrywide] and its subsidiaries on June 30, 2008. ‘Subprime Mortgage Loans’ do not include first-lien residential mortgage loans that are Federal Eligible.” Countrywide (2008, p. 5).

<sup>16</sup>A summary of the settlement is provided by a “Multistate Settlement Term Sheet” (see Countrywide, 2008). More detailed terms are provided by State of California (2008b), among other sources.

<sup>17</sup>Although securitization agreements often limit the servicer’s authority to modify mortgages (see Mayer et al. 2009 and Piskorski et al. 2010), Countrywide stated, “it currently has, or reasonably expects to obtain, discretion to pursue the foreclosure avoidance measures outlined in this agreement for the substantial majority of Qualifying Mortgages. Where [Countrywide] does not enjoy discretion to pursue these foreclosure avoidance measures, [Countrywide] will use its best effort to seek appropriate authorization from investors.” Countrywide (2008, p. 4).

<sup>18</sup>The threshold is forty-two percent of income if taxes and insurance are escrowed and thirty-four percent of income otherwise.

Countrywide agreed to be proactive in contacting borrowers eligible for modifications under the Settlement. Although it made this commitment on October 6, 2008, it announced that it would not be ready to proactively contact borrowers during first few months of the program.<sup>19</sup> Countrywide also agreed to temporarily suspend the foreclosure process for any borrower who might be eligible for a modification.

## 2.2 Public Awareness of the Settlement

The Countrywide Settlement was widely reported in early October 2008, prior to its nationwide rollout in December 2008. Figure 1 documents the sudden interest in the Settlement during this period: As reported by Google Trends, internet searches for the term “Countrywide Modification” spiked in October, as newspapers around the country announced the Settlement. Search activity dramatically increased just after this date.

Internet discussion forums also show that at least some Countrywide borrowers were aware that the Settlement targeted borrowers who were at least sixty days delinquent. In one forum, borrowers report that they were in touch with Countrywide as early as October 2008 regarding their eligibility and were told that benefits were available to borrowers who were sixty days delinquent. Some forum participants also indicate that they responded to the Settlement by missing mortgage payments in order to qualify for benefits.<sup>20</sup>

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<sup>19</sup>In a press release, for example, the Attorney General for Washington State explained that Countrywide would not be ready to contact proactively borrowers prior to December 2008. See <http://www.atg.wa.gov/countrywidePR100608.aspx>.

<sup>20</sup>The information reported in this paragraph is drawn from comments posted at <http://loanworkout.org/2009/02/countrywide-idiots/>. This site includes statements such as: “We started the process back in Oct of 2008. We have an ARM with a 8.75% rate currently. We have applied for a rate reductions but were told we would have to be delinquent on our account to qualify.”; “We received a loan modification agreement in December, but this was after we were told not to make a mortgage payment, because if we made a payment and we were current we would not qualify.”; “In order to get the help we were requesting, we had to go from having an excellent pay history to completely tarnishing our record by missing 2 months of payments...so we skipped our payments for 2 months.”; “We would not even be behind if they did not advise us to enter into the loan modification and not send any payments in until it was approved or denied!”

Countrywide was aware of the potential for strategic behavior. Its Settlement included a provision stating that, if it “detects material levels of intentional nonperformance by borrowers that appears to be attributable to the introduction of the loan modification program, it reserves the right to require objective prequalification of borrowers for loan modifications under the program and to take other reasonable steps.”<sup>21</sup> It appears that this provision was not widely reported and may not have deterred some homeowners from strategically defaulting on their mortgages in order to qualify for modifications.

## 2.3 Hypotheses

We view the Settlement as an opportunity to assess homeowner response to the sudden announcement of a modification policy using simple but manipulable qualification criteria. Most of our analysis focuses on 2/28 ARMs, a type of loan primarily targeted by the Settlement and common among subprime borrowers.<sup>22</sup> We recall that these mortgages offer a relatively low introductory “teaser” rate for the first two years, after which the rate typically resets to a higher level for the remaining 28 years of the loan term.

Assuming the announcement was an exogenous shock—an assumption we justify in the next section—we propose the following differences-in-differences (DD) estimation strategy: Relative to the same type of mortgages held by comparable borrowers and serviced by other servicers, were Countrywide 2/28 ARMs more likely to “roll” from current to sixty days delinquent—i.e., abruptly stop payment for two months—during the period immediately after public announcement of the Settlement? By abruptly stopping payment, homeowners could make themselves eligible for the benefits of the Settlement.

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<sup>21</sup>Multi-State Settlement, p. 9.

<sup>22</sup>Mayer et al. (2009) report that among subprime borrowers, over 75 percent of mortgages originated over the 2003–2007 period were of the hybrid type.

We test for this DD effect beginning in October 2008, the month of the Settlement announcement. There is, however, a potential confound beginning in early 2009. In February of that year the federal government announced plans to implement a widespread modification program, the Home Affordable Mortgage Plan (HAMP), which went on-line in March 2009.<sup>23</sup> It is a potential confound because its effect on Countrywide borrowers, who may have already applied for or obtained modifications pursuant to the Countrywide Settlement, may differ from its effect on non-Countrywide borrowers. Additionally, Countrywide borrowers may have suspended their response to the Settlement because they expected the forthcoming federal program to be more generous.<sup>24</sup>

To avoid this potential confound, we focus our analysis on the behavior of borrowers during the first few months after Settlement announcement (October 2008 to February 2009), paying particular attention to their behavior during the first quarter of the program (October 2008 to December 2009).

To be sure, an increase in the delinquency rate among Countrywide borrowers (relative to those in the Control Group) does not necessarily show that they were acting strategically to become eligible for modification. They may have been struggling to make monthly mortgage payments and been likely to default in the near future. The Settlement announcement may only have convinced them to default slightly earlier than they would have otherwise. Such defaults are not strategic because the borrowers were already distressed and likely to default.

To assess whether economic distress—rather than strategic behavior—is driving excess post-Settlement defaults among Countrywide borrowers (relative to the Control Group), we examine the behavior of homeowners who were *least* likely to default when

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<sup>23</sup>Bob Willis, “HUD’s Donovan Says U.S. to ‘Accelerate’ Mortgage Modification,” Bloomberg (Feb. 7, 2009).

<sup>24</sup>The HAMP guidelines do not have any specific requirement that a loan must be delinquent to be eligible. In fact, the program provides additional financial incentives to servicers to modify loans that are currently making payments (but are at risk of default in the future).

the Settlement was announced: (i) homeowners with substantial available credit on their credit cards (equal to at least five times their monthly mortgage payment) and (ii) homeowners with relatively low current CLTV ratios. Because these homeowners had access to significant amounts of additional liquidity, or might have had some positive equity left in their houses, they were less likely to default in the absence of a modification program, at least in the near future. If we observe a rise in delinquency rates among these homeowners, we think it is suggestive of strategic behavior by those impacted by the Settlement, rather than changes in other economic factors that might be coincident with announcement of the Settlement.

As an additional test of strategic behavior, we examine the behavior of homeowners with respect to debts that were not targeted by the Settlement, including second mortgages and credit cards. If strategic behavior—not economic distress—induced excess defaults on Countrywide subprime first mortgages, we do not expect to observe excess defaults (relative to the Control Group) with respect to such non-targeted debts during the period immediately after Settlement announcement.

Finally, we consider the behavior of borrowers with FRMs. While hybrid ARMs are a risky mortgage product usually targeted at subprime borrowers, FRMs are a more conventional mortgage product that are often taken out by more creditworthy (non-subprime) borrowers who would not have qualified for modification under the Settlement. Hence, we do not expect to observe a response among non-subprime FRMs. This comparison is useful because it tests whether the Settlement announcement altered the behavior of some but not all Countrywide borrowers. We can therefore assess whether the post-Settlement increase in Countrywide defaults (relative to the Control Group) reflects strategic behavior among targeted borrowers (those with hybrid ARMs) or just a generalized rise in default rates across all Countrywide borrowers, including non-targeted homeowners (those with non-subprime FRMs).



### 3 Data

We use a match between two databases: (i) loan-level mortgage data collected by BlackBox Logic and (ii) borrower-level credit report information collected by Equifax. BlackBox is a private company that provides a comprehensive, dynamic dataset with information about twenty-one million privately securitized Subprime, Alt-A, and Prime loans originated after 1999. These loans account for about ninety percent of all privately securitized mortgages from that period. The BlackBox data, which are obtained from mortgage servicers and securitization trustees, include static information taken at the time of origination, such as mortgage date and amount, FICO credit score, servicer name, interest rate, term, and interest rate type. The BlackBox data also include dynamic data on monthly payments, mortgage balances, and delinquency status.

Equifax is a credit reporting agency that provides monthly data on borrowers' current credit scores, payments and balances on mortgage and installment debt, and balances and credit utilization for revolving debt (such as credit cards and HELOCs). Equifax reports the "Vantage" credit score, which is comparable to FICO and ranges from 501 to 990.

The match between BlackBox and Equifax data was performed by 1010Data, a provider of data warehousing and processing, using a proprietary match algorithm. We impose four restrictions on the merged BlackBox-Equifax data in order to create a "Base Sample." First, we restrict the data to the types of loans that might have been eligible for the Countrywide Settlement, namely first-lien mortgages on residential properties that were the owners' primary residences (we excluded mortgages on properties that were purchased as second homes or by investors). First-liens were identified as loans with the following characteristics in the BlackBox dataset: (i) a lien type of "first" and (ii) a current or origination mortgage balance that was within five percent of the current or origination balance reported for the largest two first mortgages in the Equifax dataset. Second, we retain only loans that were originated during

2005, 2006, and the first half of 2007 because we have access to Equifax data covering these originations. Third, we exclude mortgages with an origination LTV less than seventy. Borrowers with lower LTVs are unlikely to have been subprime borrowers at the time of origination. Finally, we exclude mortgages serviced by Citibank, IndyMac, and J.P. Morgan, all of which implemented modification programs around the time that the Settlement was announced. We are interested in comparing the behavior of Countrywide borrowers to that of similar borrowers who were not offered modification benefits around the time of the announcement. After imposing these restrictions, we obtain a Base Sample that includes more than 500,000 2/28 ARMs and more than 700,000 FRMs.

Although 1010Data was able to link nearly all BlackBox mortgages to Equifax credit reports, we took steps to reduce the likelihood of poor-quality linkages by creating a “Matched Sample” on which we perform all analysis involving Equifax covariates. We exclude from the Matched Sample any observation for which the borrower zip code reported in Equifax does not match the property zip code in the BlackBox dataset. This exclusion omits mismatched loans at the level of zip code and provides additional verification that owner-occupants held the loans in our sample.<sup>25</sup> Due to these restrictions, the Matched Sample is smaller than the Base Sample and includes more than 300,000 2/28 ARMs and more than 450,000 FRMs.

Because the Equifax data include information about current balances on other junior mortgages held by the borrower, we are able to compute an initial combined loan-to-value (CLTV) ratio for each property. We can then calculate an estimate of current CLTV of properties at any point of time using zip-level home prices indices

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<sup>25</sup>We have conducted an extensive comparison of merge quality between datasets in the Matched Sample, checking fields such as dynamic payment history, originations balance, and origination dates. We find that these fields match very closely across the two databases, providing additional verification of merge quality.

provided by Zillow for locations for which these indices are available.<sup>26</sup>

In the analysis below, we report results both for the full Base Sample as well as the smaller Matched Sample. Variables provided by Equifax are used as covariates only in the Matched Sample.

## 4 Methodology

Our objective is to measure the effect of the Countrywide Settlement on borrower behavior immediately after it was announced in October 2008. To do this, we implement a differences-in-differences (DD) approach that compares transition rates of Countrywide mortgages (treatment group) and comparable mortgages (control group) before and after the Settlement announcement. The transition of interest is the “rollover rate,” defined as the rate at which loans migrate from current status to sixty-days past due during a two-month period (i.e., missing two payments in a row).

Our analysis period runs from July 2007 through February 2009. Mid-2007 roughly marks the beginning of the subprime crisis and the end of originations of subprime mortgages. Thus, by July 2007, all subprime mortgages in our study have been originated and are present in our data (a majority of mortgages enter our data during 2006 and the first half of 2007). Moreover, until early 2007, there was an active refinancing market for subprime loans. As a result, a significant number of loans exit our data due to refinancing (or prepayment) prior to 2007. By commencing our analysis in July 2007, we focus on a period when the vast majority of mortgages exit our data for only one reason—default. This allows us to focus on a simple transition of mortgages

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<sup>26</sup>For both the Base and Matched Samples, we use the MAPLE/Geocorr2k engine provided by the Missouri Census to link property zip code to Metropolitan Statistical Areas. We compute the current CLTV of a loan as a ratio of combined outstanding loan balances to the current estimated house value (a house price at loan origination indexed by cumulative change of zipcode house price index).

from current status to default.<sup>27</sup> We end our analysis period in February 2009, which is when the federal government announced plans to implement a widespread modification program, which went on-line in March 2009. This presents a potential confound, as discussed above.

For our analysis of a particular class of mortgages (2/28 ARMs and non-subprime FRMs), we select as a Control Group the same class of loans serviced by institutions other than Countrywide, excluding Citibank, IndyMac, and J.P. Morgan for the reasons given above. As we show below, within a given class of mortgages, Countrywide and Control Group loans exhibit small differences in observable attributes both at origination and at the time the Settlement was announced. Additionally, there is little change in these observable attributes around the time of the announcement.

Although we show that the Countrywide and Control Group loans are similar along observable dimensions, one might be concerned that unobservable differences are potentially important because Countrywide was sued while other mortgage lenders and servicers were not (an omitted variable bias). Alternatively, one might be concerned that the lawsuit itself was triggered by already-mounting delinquencies among Countrywide loans (a reverse causality problem). While potentially troubling, we do not believe that these issues generate an appreciable bias in our results. First, state attorneys general appear to have selected Countrywide as a defendant because it was the largest originator and servicer of subprime mortgages and was still solvent at the time of the lawsuits (its financial distress was resolved through its acquisition by “deep-pocketed” Bank of America). Other major subprime originators, such as New Century and IndyMac, had already collapsed and either filed for bankruptcy or been placed into receivership by the federal government. Second, although Countrywide allegedly failed to disclose all features of its mortgage products, its lending practices may not

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<sup>27</sup>A competing hazard model would be needed for data prior to July 2007. Such a model would be more complex to implement because distressed borrowers could use the refinancing option as an alternative to default. Moreover, such a model might also have to account for the possible structural shift in the parameters’ values resulting from the collapse of the subprime refinancing market in 2007.

have differed substantially from those of other institutions, who appear to have limited their disclosures to borrowers as well.<sup>28</sup>

For these reasons, we view the Countrywide Settlement as a plausibly exogenous shock to Countrywide mortgages, which are closely similar to mortgages serviced by other institutions. Nonetheless, our analysis below includes detailed controls for time-varying mortgage terms and borrower characteristics, as well as Countrywide-specific fixed effects interacted with time dummies (which allow us to observe any pre-Settlement changes in delinquency rates among Countrywide or Control Group mortgages). Even if similar borrowers were offered different terms by Countrywide than by other services, our controls should capture this heterogeneity.

In sum, our identification assumption is that, in the absence of the Settlement, comparable Countrywide and Control Group loans would display similar payment patterns (up to a constant difference) during the period of study.

## 4.1 Comparability of Countrywide and Control Group Loans

Table 1 helps justify our identifying assumption that Countrywide and Control Group servicers had similar borrower bases with comparable loan terms around the time of the Settlement announcement. The table presents summary statistics for the stock of 2/28 ARMs in our data as of September 2008, the month just before public announcement of the Settlement on October 6, 2008. The summary statistics include characteristics of the loans at origination and as of September 2008.

As Table 1 shows, measured at means, Countrywide and Control Group loans had similar CLTVs, interest rates, and credit scores: Origination and current CLTV

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<sup>28</sup>See, e.g., Lacko and Pappalardo (2007). Moreover, it is not clear whether any differences in disclosure policies would have affected borrowers' choices. Using data from the Survey of Consumer Finances, Bucks and Pence (2008) report that although most borrowers seem to know basic mortgage terms, borrowers with adjustable-rate mortgages appear likely to underestimate or not know how much their interest rates could change.

differ by at most two percentage points, origination and current interest rates differ by at most eleven basis points, and origination FICO and current Vantage differ by at most one point. Origination balances differ by about \$10,000, less than ten percent of the standard deviation. Available utilization on credit cards is measured as a fraction of the total credit limit available on all credit cards that have been used by the borrower. Comparing Countrywide and Control Group borrowers, Table 1 shows comparable levels of credit card utilization.

We obtain similar inferences for non-subprime FRMs (see Table A.1 in the Appendix). We define a loan as “non-subprime” if the borrower’s origination FICO was more than 620.<sup>29</sup> Non-subprime FRMs are comparable across most dimensions, including origination and current CLTV, origination FICO and current Vantage, origination balance, and credit card utilization. Some difference exist, however, with respect to interest rates, with average interest rate being thirty basis points lower for Countrywide loans.

We explore our identifying assumption further for 2/28 ARMs by tracking the evolution of current interest rates, Vantage scores, and CLTV among 2/28 ARMs over time. Panel (a) of Figure 2 shows these patterns for the 2/28 ARMs Matched Sample. We find that the current average interest rates for Countrywide and Control Group loans generally track each other closely during the months preceding the Settlement announcement with the average difference of about ten basis points. Panel (b) of Figure 2 shows that Vantage scores evolved in nearly identical patterns for Countrywide and Control Group loans. Finally, Panel (c) of Figure 2 shows that current average CLTV display similar patterns for Countrywide and Control Group loans with the maximum difference of less than two percentage points.

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<sup>29</sup>Subprime status is difficult to define because there is no single agreed-upon definition. In order to be conservative, we define an FRM as “non-subprime” if the borrower’s origination FICO was greater than 620, a common threshold for subprime status. Most lenders define a borrower as subprime if the borrower’s FICO credit score is below 620 on a scale that ranges from 300 to 850. This is also how the Office of the Comptroller of the Currency defines subprime status in their mortgage metrics reports (see also Keys et al. 2010).

We have also investigated these patterns grouping loans by their quarter of origination and have obtained very similar inference. This comparison also reveals that Countrywide loans tend to reset on average to somewhat higher interest rates than the Control Group loans. This suggests that on average Countrywide loans may have been somewhat riskier than those in the Control Group. We account for this difference by controlling for variation in interest rates over time in the regressions reported below.

We also verify that default rates among Countrywide and Control Group loans follow similar trends prior to our estimation period (July 2007 through February 2009). As Figure A.1 in the Appendix shows, the stock of loans in default evolved similarly for the two groups between mid-2006 and mid-2007, by which point about 7.2% of Countrywide and 7.6% of Control Group loans had entered default during this period. Similarly, about 22% of Countrywide and 23% of Control Group of loans refinanced or prepaid by July 2007.

Along the same dimensions, we observe similar patterns among non-subprime FRMs. Overall, these patterns point to close comparability across Countrywide and Control Group loans before the Settlement was announced in October 2008. As some variation between these two groups could be due to differences in the timing and mix of mortgages originated, we include a wide range of controls (for loan, pool, and individual borrower characteristics) in the regressions reported below.

## 4.2 Empirical Specification

We estimate a probit specification of the following form:

$$\Pr(Y_{it} = 1 | \text{Current}_{it-2}) = \Phi(\alpha + \beta \cdot CW_{it} + \mu \cdot \text{Oct-Dec}_{it} + \delta \cdot CW_{it} \cdot \text{Oct-Dec}_{it} + \gamma \cdot X_{it}) \quad (1)$$

The dependent variable is the probability that a mortgage becomes sixty days past due in month  $t$  ( $Y_{it} = 1$ ), conditional upon being current sixty days (two months) earlier

( $\text{Current}_{it-2}$ ). We call this the “rollover rate” from current to sixty days delinquent. It is our primary dependent variable because the Settlement targeted borrowers who were at least sixty-days delinquent on their subprime first mortgages.  $CW_{it}$  is a dummy variable that equals 1 if the loan is serviced by Countrywide.  $Oct-Dec_{it}$  is another dummy, equaling 1 if month  $t$  occurs during the period October through December 2008. October 2008 is the first month during which we could observe a borrower response to announcement of the Settlement on October 6, 2008.<sup>30</sup>  $X_{it}$  is a vector of loan and borrower characteristics that includes variables such as initial Vantage score and the change in Vantage score from origination to the current period, initial and current CLTV, origination quarter, initial and current interest rate, loan balance, the controls for date of reset, dummies for each quarter before and after the Settlement announcement, and interactions between these time dummies and the Countrywide indicator ( $CW_{it}$ ). Standard errors are clustered by mortgage.<sup>31</sup> We estimate the above specification on monthly data spanning from July 2007 through February 2009.

The coefficient of interest is  $\delta$ , which measures the “difference in difference”—the estimated change in the difference between Countrywide and Control Group rollover rates during the quarter immediately after Settlement announcement (October-December 2008) relative to the first quarter in our analysis period (July-September 2007). As we discussed above, we focus primarily on the three months (ending in December) after the Settlement announcement because of the potential confound arising from the HAMP program, which was announced in early 2009. We choose July-September 2007 as the omitted quarter in our analysis because our regressions include interactions between the Countrywide indicator and quarterly time dummies for every quarter before and after

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<sup>30</sup>Our data record the payment status of the borrower as of the end of a given month. For example, a borrower who is thirty-days delinquent in September will be recorded as being sixty-days delinquent in October if no new payments were received by the end of October. Thus, a 60-day past on payments delinquency status in October 2008 is the first record that could reflect a borrower’s decisions made after the Settlement announcement.

<sup>31</sup>The results presented in the paper are very similar when we cluster the standard errors by the location (MSA) of the property backing the loan (See Tables A.3 and A.4 in the Appendix).



the Settlement announcement. By omitting the first quarter of our analysis period, we make it easier for the reader to detect any systematic differences between Countrywide and the Control Group during the quarters preceding the announcement. In order to facilitate a direct assessment of the change in rollover rates between the quarters before (July-September 2008) and after (October-November 2008) the Settlement announcement, we separately compute the difference between the estimated interactions for these two quarters and report the statistical significance of the difference.

## 5 Results

### 5.1 Evolution of Default Rates

We begin our analysis by plotting rollover rates for Countrywide and Control Group loans. Figure 3 plots the average monthly rollover rate for each of the five quarters preceding the Settlement announcement, the quarter just after the announcement (Oct-Dec 2008), and the Jan-Feb 2009 period. Panel (a) examines all loans. Panel (b) subsets on “low utilization” borrowers, defined as those who had sufficiently large liquidity available to them through credit cards that they could charge the equivalent of five or more months of mortgage payments when they become delinquent on their mortgages. Panel (c) subsets on borrowers whose mortgages had a CLTV less than 100 percent at the time of their delinquency. These low credit utilization and low CLTV borrowers were arguably less likely to default in the near term because they had significant untapped liquidity through their credit cards or some positive equity in their homes.

Panel (a) of Figure 3 shows a significant increase in the sixty-day rollover rate of Countrywide loans relative to the Control Group (top panel) during the October-December 2008 period, the first quarter during which we could observe an effect of the

Settlement announcement. However, we also observe an increase in the delinquency rate of Countrywide loans relative to the Control Group during the quarter immediately preceding the Settlement announcement, suggesting the possibility of a pre-Settlement increase in the delinquency rate of Countrywide loans. This pre-Settlement increase is less evident when we subset on “low utilization” borrowers in Panel (b). Here we continue to see a significant increase in the rollover rate among Countrywide loans, relative to the Control group, after Settlement announcement. But for these borrowers, we observe only a slight pre-Settlement increase in Countrywide rollover rates relative to the Control Group. When we subset on low CLTV borrowers in Panel (c), we again observe a substantial post-Settlement increase in Countrywide delinquencies relative to the Control Group, while observing no pre-Settlement difference between Countrywide and the Control Group.

Overall, Figure 3 suggests that a number of Countrywide borrowers might have missed payments in response to the Settlement announcement. Although we observe a pre-Settlement increase in delinquencies among Countrywide borrowers, relative to the Control Group, the increase appears to be driven primarily by the lowest quality loans (borrowers with relatively high utilization of credit cards and high CLTVs). Among more creditworthy borrowers we observe little relative change in delinquencies prior to the Settlement announcement, suggesting that our identification assumptions are reasonable at least with respect to these borrowers.

## 5.2 Baseline Model of Settlement Effects

Table 2 implements equation (1) for 2/28 ARMs. Column (1) estimates the model using the full Base Sample, but includes only a minimal set of controls—time dummies, a Countrywide dummy, and interactions between the Countrywide and time dummies. The time dummies identify the quarters before and after Settlement announcement. The excluded category is July-September 2007, the first quarter of our

analysis period. The final time dummy—Jan-Feb 2009—includes only two months because we stop our analysis in February 2009, the month before HAMP was announced. These time dummies, along with the Countrywide  $\times$  Time interactions, control for time-varying differences between Countrywide and Control Group loans. The coefficients are marginal effects and can be compared to the mean monthly rollover rate among Countrywide loans during the July to September 2008 period, as reported at the bottom of the table (“Avg. Delinquency”).<sup>32</sup> In order to facilitate a direct assessment of the change in rollover rates between the quarters before (July-September 2008) and after (October-November 2008) the announcement, the bottom of the table reports the magnitude and statistical significance of the difference between the estimated interactions, Countrywide  $\times$  Jul-Sep 2008 and Countrywide  $\times$  Oct-Dec 2008.

Column (2) adds additional controls from the BlackBox database, listed in Table 1. These controls include a wide range of loan- and borrower-level characteristics, such as origination FICO, initial LTV and CLTV (when available), current LTV, initial interest rate and any change in rate over time. Column (2) also includes MSA fixed effects,<sup>33</sup> dummies that identify loans that had reset within the preceding three or six months, and interactions between these reset variables and the Countrywide dummy. These variables account for heterogeneity across loans and systematic differences between Countrywide and the Control Group, including the possibility that Countrywide mortgages experienced higher default rates at rate resets or during other time periods. Together, the variables allow us to test whether post-Settlement differences between Countrywide and the Control Group are significantly different from pre-Settlement differences.

Columns (3) and (4) analyze the Matched Sample: Column (3) includes the same controls as in Column (2); Column (4) includes the set of Equifax controls,

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<sup>32</sup>The estimated treatment effect in nonlinear “difference-in-differences” probit models such as ours is given by the incremental effect of the coefficient of the interaction term (see Kremer and Snyder 2010; Puhani 2012).

<sup>33</sup>In unreported regressions, we obtained virtually identical results when we included both State dummies and State  $\times$  Time interactions.

including information about second liens, credit card utilization, and current credit scores. Column (4) also uses current and origination CLTV, whereas prior columns use current and origination LTV.

Across all columns in Table 2, the Countrywide  $\times$  Oct-Dec 2008 interaction is positive, statistically significant, and economically meaningful. Controlling for borrower and loan characteristics, the estimates imply a 0.48 to 0.54 percentage point increase in the monthly delinquency rate of Countrywide loans, relative to the Control Group, during the quarter following the Settlement announcement. This represents a ten to eleven percent increase in Countrywide’s monthly rollover rate relative to the average rate among Countrywide loans during the quarter immediately prior to the announcement (4.8%, as reported at the bottom of the table).<sup>34</sup> Because the magnitude of the effect does not vary meaningfully between Columns (2) and (4), we conclude that restricting our attention to the Matched Sample with a full set of controls does not bias our inference.

The estimates in Table 2 point to a potential pre-Settlement differential trend in Countrywide delinquency rates. In Column (1), the coefficient for Countrywide  $\times$  Jul-Sep 2008 is substantial and statistically significant. The size and significance of this coefficient largely disappears, however, when we add controls that account for borrower and loan characteristics heterogeneity. The controls that substantially reduce this coefficient are those that identify the current interest rate charged on the loan and the date when a loan’s “teaser rate” expires (the interest rate will reset to a potentially higher date) (See Table A.2 in the Appendix). This is unsurprising as the mortgage interest rate can be viewed as a composite variable, capturing the overall riskiness of the borrower. Likewise, it is well known that hybrid ARMs tend to experience higher

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<sup>34</sup>These estimates compare the quarter immediately after the announcement (the fourth quarter of 2008) to the first quarter of our analysis period (the third quarter of 2007). At the bottom of the table, we present estimates comparing the quarter immediately after to the quarter immediately before (third quarter of 2008). The results are similar: Estimates range from 0.48 to 0.55 percent, again representing a ten to eleven percent increase relative to the rollover rate during the third quarter of 2008.

default rates immediately after interest rate resets. Some weak evidence of a potential pre-Settlement trend remains in Columns (2) through (4): Countrywide rollover rates declined relative to Controls during April-June 2008, reversed that decline in July-September 2008, and then increased substantially after the Settlement announcement. However, the decline in April-June 2008 (and reversal in July-September 2008) is marginally significant and small in magnitude (three percent of the average delinquency rate among Countrywide loans during July-September 2008), especially after we include a full battery of controls in Column (4). Moreover, as we show below, we find no evidence of a pre-Settlement differential trend in default rates of Countrywide loans when we subset on borrowers who were most likely to be responding strategically to the Settlement announcement.

Columns (2) through (4) report a statistically significant and economically meaningful effect (in relative terms) of the Settlement during the first quarter following the announcement. However, we observe no effect in January and February of 2009.<sup>35</sup> This finding is difficult to interpret for several reasons. First, it may reflect the fact that the Countrywide Settlement was announced through media channels in early October 2008. If only a subset of Countrywide borrowers received the news, and responded quickly, we might not observe an effect in early 2009. Alternatively, the absence of an effect in early 2009 might reflect anticipation among borrowers that the federal government would implement its own mortgage modification program. Media reports during that time suggested that a federal program would be forthcoming with the official announcement being made in February 2009. For these reasons, we believe that our empirical strategy can identify the effects of the Settlement only in the first few months following the announcement.

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<sup>35</sup>Similarly, when we extend our analysis period through the end of 2009, we do not observe an effect in the months after February.

### 5.3 Settlement Effects by Credit Card Utilization and CLTV

The baseline results displayed in Table 2 present two inferential problems. First, although they report a marked post-Settlement increase in the rollover rate of Countrywide loans relative to the Control Group, it is arguable whether this increase does necessarily reflect strategic behavior. It could instead reflect an increase in defaults by economically distressed borrowers who were already highly likely to default in the near term. Second, the coefficients in Column (1) of Table 2 point to a potential pre-Settlement increase in Countrywide delinquency rates relative to the Control Group. Although we do not find much evidence of this pre-Settlement increase once we control for borrower and loan characteristics (see Columns (2) to (4) of Table 2), it could be argued that our results are confounded by pre-existing trends.

We address these concerns by identifying subsets of borrowers who were unlikely to default in the absence of the Settlement. In particular, we hypothesize that borrowers with access to substantial liquidity (through credit cards) or with some positive equity in their houses would be much less likely to default otherwise. Additionally, by focusing on these borrowers, we potentially increase the comparability of treatment and control groups, allowing us to better estimate program effects.

We stratify our sample by levels of credit card utilization (utilization is measured monthly) and CLTV. With respect to utilization, we identify three groups: borrowers with access to credit equal to more than five months of mortgage payments (“> 5 Months”), those with available credit equal to one to five months of payments (“1-5 Months”), and those with available credit equal to no more the one payment (“0-1 Months”). We hypothesize that borrowers with high levels of available credit (e.g., “> 5 Months”) are likely to be less liquidity constrained and therefore less vulnerable to economic shocks than borrowers with lower levels of available credit. We have more than 60,000 loans associated with such borrowers in our data.

We similarly separate borrowers into three groups based on their current CLTV: borrowers with CLTV less than 100 (“above water”), those with CLTV between 100 and 120, and those with CLTV greater than 120 (“underwater”). Again, we hypothesize that borrowers with CLTV under 100 are less likely to default because they have some positive housing equity. We have about 100,000 of such loans in our data. Finally, we identify a group of borrowers who had high available credit (“> 5 Months”), but were underwater on their homes (“CLTV > 100”). By subsisting on these homeowners, then, we can test the effect of the Settlement on default behavior by homeowners who are often thought to be most likely to engage in strategic behavior.

Table 3 reruns our main specification with the full set of controls (Column (4) in Table 2) for each group of borrowers. Columns (1) through (3) separate borrowers by credit card utilization, (4) through (6) separate them by CLTV, and (7) subsets on underwater borrowers with high available utilization. Pre-Settlement delinquency rates differ substantially across these groups, as the bottom of the table shows (“Avg. Countrywide 2008Q3 Delinquency”). The average monthly delinquency rate was 0.03 among borrowers with over five months of available credit, but 0.052 among those with no more than one month of available credit. Similarly, Countrywide borrowers with estimated CLTV less than 100 had rollover rates (0.031) that were over 60 percent lower than the rollover rates among substantially underwater homeowners (0.078). These statistics are consistent with our hypothesis that, prior to the Settlement, borrowers with substantial credit available and those with some positive estimated equity left in their homes were substantially less likely to default than more credit-constrained or “underwater” borrowers.

Across Columns (1) through (7), we observe a substantial post-Settlement increase in Countrywide’s relative rollover rate. Stratifying borrowers by credit card utilization, Columns (1) through (3) show that the effect of the Settlement was largest among borrowers with the most available credit. Among borrowers with “> 5 Months”

of available credit, Countrywide’s relative rollover rate increased by 0.54 percentage points, an eighteen percent increase relative to the pre-Settlement mean monthly rollover rate among Countrywide loans (0.03). The effect is substantially smaller among borrowers in the “1-5 Months” category (0.52 percentage points, a thirteen percent increase) and the “0-1 Months” category (0.63 percentage points, a twelve percent increase).<sup>36</sup> Equally important, Columns (1) through (3) show no evidence of a significant pre-Settlement relative increase in rollover rates among Countrywide borrowers.

These results are inconsistent with the possibility that idiosyncratic economic shocks to Countrywide borrowers explain observed differences between Countrywide and Control Group borrowers immediately after the Settlement announcement. Additionally, the absence of a pre-Settlement increase in Countrywide delinquencies supports the hypothesis that the Settlement induced defaults among borrowers who were unlikely to default otherwise, at least in the near future.<sup>37</sup>

We observe largely similar patterns when we stratify loans by CLTV in Columns (4) through (6). The largest effects of the Settlement are concentrated among “above-water” borrowers: Countrywide’s relative rollover rate increased sixteen percent compared to its pre-Settlement mean among borrowers with  $CLTV < 100$ , but only ten percent among underwater borrowers (and the effect is only marginally significant for borrowers with  $CLTV > 120$ ).<sup>38</sup> We also observe a relative increase in defaults during

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<sup>36</sup>The magnitude of the post-Settlement increase is slightly smaller when we compute the estimated change between the quarters immediately before and after the settlement. As reported at the bottom of Table 3, the increase is 0.43 percentage points among borrowers with “> 5 Months” of available credit, a fourteen percent increase relative to the pre-Settlement mean. Among borrowers in the other categories, the increase is smaller—an eight percent increase among borrowers in the “1-5 Months” category and a nine percent increase among borrowers in the “0-1 Months” category.

<sup>37</sup>In unreported regressions we verify that our results are robust to different definitions of credit utilization. We reran our regressions separately on borrowers with zero to one months, one to two months, two to four months, four to six months, and six to twelve months of available credit. Consistent with Table 3, we find that post-Settlement relative increase in rollover rates of Countrywide loans is larger—among borrowers with greater available credit.

<sup>38</sup>When we compute the difference between the estimated coefficients for Countrywide  $\times$  Oct-Dec 2008 and Countrywide  $\times$  Jul-Sep 2008 (the quarters before and after the settlement), we obtain



January-February 2009 among borrowers with  $CLTV < 100$ .<sup>39</sup> There is no evidence of a pre-Settlement relative increase in Countrywide defaults except among loans with  $CLTV$  between 100 and 120. These results offer further support for the hypothesis that the Settlement induced defaults among borrowers who were less likely to default otherwise, at least in the near future.

Column (7) of Table 3 provides evidence that the Settlement induced an increase in Countrywide’s relative rollover rate among underwater borrowers with substantial available credit. The effect is economically large (over twelve percent) relative to the pre-Settlement delinquency rate among Countrywide loans.<sup>40</sup> This is consistent with the hypothesis that the Settlement induced defaults among homeowners who are thought to be most likely to act strategically.

## 5.4 Additional Robustness Checks

Our empirical model treats Countrywide loans as a treatment group and all non-Countrywide loans as a control. This strategy could mask important heterogeneity across loans in the control group. We would worry about a confound, for example, if some control group loans experienced an increase in their post-Settlement rollover rate, even though they were not targeted by the Settlement. To address this possibility, we reran our preferred specification—Column (4) of Table 2—but added dummies identifying loans serviced by the top five non-Countrywide servicers and interacted these dummies with the pre- and post-Settlement time dummies. The remaining loans

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comparable results, as shown at the bottom of Table 3. Among above-water borrowers, we observe a thirteen percent increase in Countrywide’s relative rollover rate compared to the pre-Settlement mean. Borrowers with  $CLTV$  above 120 exhibit an increase of only nine percent.

<sup>39</sup>The program was covered by the news media mostly in early October 2008. It is possible that only a subset of Countrywide borrowers heard about it and these informed borrowers responded immediately. Accordingly, we do not see a response in January-February 2009 among most borrowers because informed borrowers have already responded. Borrowers with lower  $CLTV$  levels may have taken more time to respond, possibly as their perceived cost of strategic behavior was higher.

<sup>40</sup>The effect is even larger—fifteen percent relative to the pre-Settlement mean—when we compare the estimated coefficients for  $\text{Countrywide} \times \text{Oct-Dec 2008}$  and  $\text{Countrywide} \times \text{Jul-Sep 2008}$ , as the bottom of Table 3 shows.

were left in the Control Group. Thus, for both Countrywide and the top five servicers, we estimated the rollover rate relative to the Control Group. Although we continue to find a strong effect of the Settlement among Countrywide loans, we observe no impact among loans serviced by the top five non-Countrywide Servicers. We also find that the coefficient for Countrywide  $\times$  Oct-Dec 2008 is significantly different from the coefficients for the corresponding interactions with dummies for non-Countrywide servicers. As an additional placebo test, we reran our preferred specification but dropped Countrywide loans and replaced the Countrywide indicator with an indicator for loans serviced by Wells Fargo, the second largest servicer in our dataset. Results from that exercise showed no meaningful post-Settlement increase in the relative rollover rate among Wells Fargo loans. This was also true when we subsetted on borrowers with high available credit ( $> 5$  months) and low CLTV (CLTV below 100). Together, these results are inconsistent with the hypothesis that the results in Tables 2 and 3 are confounded by differential changes in default patterns among particular non-Countrywide loans.

An additional potential limitation of the estimates reported in Table 2 is that they are derived from a model that implicitly constrains the effect of the Settlement announcement to be the same for all loan vintages. The Settlement, however, could have impacted some loan vintages differently than others. For example, its terms suggest that it may have been most beneficial to borrowers experiencing interest rate resets before or around the time the Settlement was announced. We therefore separately examined loans by vintage to determine whether the effect is larger for some vintages than others. In particular, we reran our main specification from Table 2 for each quarterly origination cohort in our data. Despite a relatively small sample size for each cohort, we observe a statistically significant post-Settlement increase in the Countrywide rollover rate, relative to Control loans, both in cohorts that were resetting around the time of the Settlement (2006Q2 and 2006Q3) and in origination cohorts

that reset more than a year before the Settlement announcement (2005Q1, 2005Q2, and 2006Q1). The effects range from more than 10% to more than 20% in relative terms. These results help alleviate concerns that our results are driven by unobservable differences between Countrywide and the Control Group around the reset date (all of our specifications control for the magnitude and timing of any interest rate reset).

Another potential limitation of our empirical model is that it considers only one possible response to the Settlement announcement: rolling from current to sixty days past due by missing two payments in a row. We have chosen the specification in our paper because it allows us to model the behavior of a relatively homogeneous group of borrowers. Additionally, we believe that evidence of strategic behavior is more compelling if we observe a substantial rise in defaults among borrowers who were current on payments prior to the Settlement announcement. The announcement could have induced other responses, however, such as rolling from thirty to sixty days past due or remaining in the status of sixty days past due instead of making an additional payment.

To account for more complex responses to the Settlement announcement, we re-estimated our preferred specification—Column (4) of Table 2—but changed the dependent variable to indicate whether a loan is current in a given month (a dependent variable equals 1 if the loan is current and zero otherwise). We also include a loan in our analysis regardless of its status in previous months (in our baseline specification, a loan is included in our analysis of a particular month only if it was current two months prior). Thus, a loan will be counted as non-current in a given month if it is past due on payments, regardless of prior delinquency history. This specification allows us to study the difference in the rate at which Countrywide and Control Group mortgages stay in the “current” payment status.<sup>41</sup> We obtain results comparable to those in Table 2: Countrywide loans exhibit a relative decline in the probability of being current

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<sup>41</sup>This alternative specification differs from our baseline specification in two important respects: First, it accounts for loans transitioning from current to delinquent as well as loans migrating from delinquent to current. Second, it includes loans with more heterogeneous payments histories. Because of

during the quarter immediately after Settlement announcement. An additional 1.5 to 2 percent more Countrywide loans exit current status per month during this quarter (Figure A.2 in the Appendix presents these estimates). This implies that by the end of 2008 the program resulted in a more than 10 percent increase in the number of delinquent Countrywide loans relative to their number as of September 2008. Again, we observe larger program effects when we subset on borrowers with relatively high credit utilization ( $> 5$  months) or low CLTV (CLTV below 100).

Finally, our empirical model generally estimates the effect of the Settlement during the quarters—not the months—before and after the announcement (the exception being Jan-Feb 2009). We use quarterly time effects because they are more parsimonious, allow us to estimate the Settlement effect with greater power (which is important because the fraction of loans that default per month is relatively small), and allow us to avoid some of the issues associated with noisy variation in monthly default rates (in a monthly specification, the excluded category would be just one specific month, not an entire quarter). Nonetheless, we verified that our results are robust to the way we define our time interval. We re-estimated our main specification, but replaced the time dummies with monthly dummies, which were interacted with the Countrywide indicator. We observe a substantial increase in Countrywide’s relative rollover rate during November and December 2008 (Figure A.3 in the Appendix presents these estimates). This is consistent with the hypothesis that Countrywide borrowers heard news of the Settlement in early October 2008 and quickly responded to it by missing payments.

## 5.5 Effects of Settlement on Non-Targeted Debts

The Settlement targeted subprime first lien mortgages. We do not expect to observe an increase in defaults among non-targeted debts—such as second lien mort-

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these differences, it is more challenging to control for differences in risk characteristics across borrowers when we estimate this alternative specification. We have tried to account for these differences by including a control for the payment status of the loan in the prior month (current, 30 days past due, etc.) in addition to the full battery of controls included in our baseline specification.

gages and credit card debt—in response to the Settlement. To the contrary, we expect to observe Countrywide borrowers remaining current on these debts while defaulting on first mortgages in order to obtain the benefits of the Settlement.

Similarly, we do not expect the Settlement announcement to affect the default behavior of borrowers who were not eligible for benefits, such as borrowers with non-subprime mortgages. Although the Settlement offered relief to subprime fixed-rate mortgage (FRM) borrowers, a vast majority of securitized FRMs in our data are non-subprime loans offered to borrowers with relatively high credit ratings. Because these “non-subprime FRMs” were not targeted by the Settlement, they provide a useful placebo test: we can test the effect of the Settlement on Countrywide borrowers who were ineligible for benefits.

Table 4 tests these hypotheses—that rollover rates for non-targeted debts should not exhibit a post-Settlement increase among Countrywide borrowers, relative to the Control Group. Columns (1) and (2) re-estimate our preferred specification, but change the dependent variable to measure the probability of being sixty-days past due on a second lien (Column 1) or missing a payment on credit card debt (Column 2), conditional upon being current two months earlier.<sup>42</sup> Borrowers are included in these regressions only if they have a second lien or credit card. Across both columns, we observe no effect of the Settlement on the relative delinquency rate of Countrywide borrowers, consistent with the hypothesis that the Settlement did not induce defaults on non-targeted debts.<sup>43</sup>

Columns (3) and (4) rerun these regressions on subsets of borrowers with high available credit ( $> 5$  Months) and borrowers with above-water mortgages ( $CLTV < 100$ ). Although these borrowers had the lowest default rates on first mortgages prior to

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<sup>42</sup>We obtain similar inferences when we eliminate this condition and allow loans to have any payment status during the preceding months.

<sup>43</sup>This evidence also suggests that the delinquency induced by the Countrywide program did not shift the “moral compass” of borrowers by encouraging them to default on other types of debt.

the Settlement announcement, as Table 3 showed, their rollover rates exhibited the strongest response to the announcement. Here too we find no evidence that the Settlement increased delinquency rates on non-targeted debts among Countrywide borrowers, relative to Control Group. To the contrary, Column (4) shows that credit card delinquency rates *decreased* among low utilization Countrywide borrowers, relative to the Control Group, after the Settlement announcement. This pattern suggests that some borrowers may have strategically defaulted on first mortgages and then used the additional available cash flow to service credit card debts. With respect to second liens, these results suggest that the Settlement may have resulted in a behavior of borrowers that effectively reverses the priorities of first and second liens: in response to the Settlement announcement, Countrywide borrowers continued making payments on lower-priority second liens loans while defaulting on more senior loans.<sup>44</sup>

Finally, Column (7) estimates our main specification using data on non-subprime fixed-rate mortgages (FRMs). Again, we observe no increase in rollover rates among Countrywide borrowers, relative to the Control Group, during the months following the Settlement announcement. Indeed, relative rollover rates appear to have declined among these Countrywide borrowers during those months.<sup>45</sup>

To summarize, we provide evidence that Announcement of the Settlement induced a substantial increase in relative rollover rates and strategic defaults among (i)

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<sup>44</sup>We also estimated a version of our main specification in which the dependent variable equals 1 if the borrower becomes sixty days delinquent, conditional upon being current two months earlier, while remaining current on credit cards for at least six months following the month in which he or she becomes sixty days delinquent. This dependent variable measures a type of default behavior—defaulting on first mortgages while remaining current on other debts—that others have described as “strategic behavior.” See Experian-Oliver Wyman Market Intelligence Reports. We observe a substantial increase in this type of default among Countrywide borrowers, relative to the Control Group during the quarter following the Settlement announcement. The effect is larger when we subset on borrowers with more available credit or those with above-water mortgages.

<sup>45</sup>We have also investigated the program response among Countrywide subprime FRMs that were targeted by the Settlement. For that purpose, we define an FRM loan as “subprime” if the borrower’s origination FICO was less than 620. We find some evidence that Countrywide subprime FRMs experienced a relative increase in delinquency rates after announcement, but our estimates are imprecise because we have only about 10,000 Countrywide subprime FRMs in our data (compared to more than 130,000 non-subprime FRMs).

Countrywide mortgages that were eligible for relief, but not (ii) among non-mortgage debt held by eligible Countrywide borrowers or (iii) Countrywide mortgages that were ineligible for relief.

## 6 Conclusion

We investigate whether homeowners respond strategically to news of mortgage modification programs by defaulting on their mortgages. We analyzed a program—resulting from a settlement of litigation against Countrywide—that used a simple eligibility criterion: A borrower becomes eligible upon default. We find that this program induced a substantial increase in strategic delinquency, defined as the rate at which previously current borrowers miss two payments in a row. Our estimates range from a ten percent increase in delinquency among all borrowers in our sample to an eighteen percent increase among borrowers who were least likely to default otherwise because they had access to liquidity or relatively low mortgage debt.

Our empirical setting only allows us to study homeowner responses during the first few months following the program announcement. We cannot study longer-term effects of the Countrywide program. However, there are reasons to think that our estimates may constitute a lower bound for the effects of similar programs implemented on a national scale. In particular, the Countrywide Settlement received relatively brief media coverage. A better-advertised, national program with persistent media coverage could yield effects larger than those we observe here.<sup>46</sup>

Simple "back-of-the-envelope" calculations suggest that the effects of strategic behavior could be substantial. Over 45 million first-lien mortgages were outstanding

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<sup>46</sup>It could be argued that our results over-estimate the effects of programs with simple eligibility rules because the Settlement targeted a lender (Countrywide) that had been accused of deceiving borrowers. The effects we observe in this paper could reflect, in part, the angry response of outraged homeowners. On the other hand, debt relief programs are often implemented during financial crises, during which borrowers frequently protest the behavior of lenders. Alston (1984), for example, discusses the violence that preceded some state laws offering debt relief during the Great Depression.

and current in early 2007, when housing prices began to fall.<sup>47</sup> Suppose lenders considered at that time whether to implement a national mortgage modification program with simple eligibility criteria similar to the Countrywide Settlement. The estimates in Table 2 imply that the Countrywide Settlement resulted in a 0.54 percentage point absolute increase in the monthly delinquency rate during the quarter immediately after its announcement. This means that 1.62 percent of current loans became delinquent during this quarter as a result of the Settlement Announcement. Applying that estimate (1.62 percent) to the stock of outstanding, current loans (45 million), a national program would immediately induce over 700,000 additional strategic defaults.<sup>48</sup>

If the typical loan modification offered debt relief equivalent to about thirty percent of a borrower’s outstanding loan balance (with an average balance of about \$200K), these strategic defaults would impose losses of over \$43 billion on mortgage lenders and investors (in terms of foregone payments from borrowers).<sup>49</sup> These losses would be incurred immediately after announcement of the national program. If programs with simple eligibility criteria have longer-run impacts on strategic default rates—which we cannot assess due to the limits of our empirical setting—the losses would be larger.

With this rough cost estimate in hand, we can explore the potential trade-off facing mortgage lenders (and investors). Simple modification programs can result in strategic behavior leading to unnecessary modifications. On the other hand, if lenders try to avoid these losses by implementing slower programs with more complex eligibility criteria, they may fail to prevent some foreclosures that reduce payoffs to both lenders and homeowners. To illustrate this trade-off, assume that a foreclosure

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<sup>47</sup>Based on LPS and BlackBox databases.

<sup>48</sup>A similar increase in strategic defaults is implied by our estimates in Table 3 that subset on less-risky borrowers with relatively high remaining credit card utilization or low CLTV. This implies that our back-of-the envelope calculations are unlikely to be biased by the relatively low creditworthiness of Countrywide borrowers.

<sup>49</sup>We assume a loan modification equal to thirty percent of a borrower’s outstanding loan balance because home prices fell over thirty percent after mid-2007, according to the Case-Shiller 10-city composite index (Sinai 2012). A modification of the magnitude we contemplate here would allow most homeowners to become above-water on their mortgages.



results in deadweight losses to lenders equal to about fifty percent of the borrower’s outstanding loan balance.<sup>50</sup> Assume as well that foreclosures can be prevented by offering homeowners debt relief equivalent to about thirty percent of their balances. Unnecessary foreclosures, then, expose lenders to losses equal to twenty percent of the borrower’s outstanding balance. Unnecessary modifications, by contrast, expose them to losses equal to thirty percent of the borrower’s outstanding balance. If lenders were choosing between a quick modification program with simple eligibility criteria and a slow program with more careful verification of eligibility, the lenders would be indifferent between a quick program generating over 700,000 unnecessary modifications and a slow program generating over 1 million unnecessary foreclosures. Both programs generate the same costs to lenders. This simple example may help explain relatively slow pace of mortgage renegotiations during the recent crisis.<sup>51</sup>

It might be thought that this trade-off facing lenders—quick programs that engender strategic behavior versus slow programs that lead to unnecessary foreclosures—could be resolved by using simple proxies to identify and deny benefits to borrowers who are likely to default strategically in response to a simple program. In this paper, for example, we rely on two proxies— high available credit utilization and low current CLTV—to identify borrowers who are likely to be acting strategically when we observe a post-program increase in delinquency rates. It may, however, be difficult for lenders to apply the same proxies in designing effective mortgage modification programs. First, available utilization is a manipulable proxy: Borrowers can strategically reduce their available utilization in order to qualify for benefits. Second, although CLTV is less manipulable than utilization, it is also harder to verify and often measured with noise, particularly for homes that haven’t experienced recent transactions. Hence, imposing

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<sup>50</sup>This loss arises from (i) the house price decline that has already occurred and (ii) the deadweight costs of the foreclosure process.

<sup>51</sup>There are other factors that could adversely affect mortgage renegotiation such as institutional frictions implied by the high rate of securitization of loans at risk of foreclosure (see Piskorski et al 2010 and Agrawal et al. 2011).

high estimated CLTV eligibility restrictions could result in some eligible borrowers not qualifying for a help. On the other hand, an accurate measurement of CLTV would likely entail a potentially costly and time-consuming investigation, which could transform a quick program with simple eligibility criteria into a slower and potentially less effective program.<sup>52</sup>

The above rough calculations show that the costs associated with strategic default—even if relatively small compared to the amount of mortgage debt outstanding—may be large enough to induce lenders to favor slower, more cautious debt relief programs. Although these programs can be rational for lenders, they may result in welfare costs by allowing more foreclosures to occur. Such unnecessary foreclosures can yield negative externalities for surrounding communities, as illustrated by Campbell et al. (2010) and Mian et al. (2011). Additionally, debt-relief programs may mitigate the distorting effects of high household debt levels on aggregate demand, investment decisions, and employment (Mian and Sufi 2012).

Our analysis does not necessarily imply that it would be socially optimal to incentivize or require lenders to implement quick modification programs with simple eligibility criteria. We have focused only on one direct cost of strategic behavior to lenders from these programs but other costs may be important too. Such programs can potentially generate undesirable redistributive effects. Additionally, generous debt relief programs can have adverse impacts on the future availability and cost of credit. Therefore, we cannot say whether the total economic costs of simple debt relief policies that generate strategic behavior are large relative to the potential gains to borrowers, lenders, and society from such policies.

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<sup>52</sup>A lender could rely instead on regional price indices to appraise home values, but the resulting CLTV calculations would be imprecise and could result in some eligible borrowers not qualifying for help if a modification program featured high CLTV restriction. The difficulty in measuring CLTV may help explain why recent modification programs have been fairly generous on this dimension—for example, requiring a loan-to-value greater than seventy-five percent—that effectively ensure that the vast majority of underwater borrowers will satisfy such requirements (see Citigroup 2009).

Our results instead highlight a trade-off that merits further investigation: mortgage modification policies that use simple but potentially manipulable eligibility criteria (i) do appear to generate economically meaningful strategic behavior, but (ii) may also offer benefits more quickly and to a larger group of homeowners at risk of default. More work must be done to assess the overall costs and benefits of such modification policies both in the near term and in the long run.

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Table 1: Summary Statistics for 2/28 ARMs

This table presents summary statistics for 2/28 adjustable rate mortgages (ARMs) serviced by Countrywide and the Control Group in the Matched Sample as of September 2008. The summary statistics include characteristics of these loans at origination and as of September 2008, the month before public announcement of the Settlement on October 6, 2008. CLTV and interest rates are reported in percentage terms; loan balances are in thousands of dollars.

Countrywide		Control		
Number of Loans	43,418	203,960		
BlackBox Variables				
	mean	sd	mean	sd
Initial CLTV	92	8.8	90	8.9
Initial Interest Rate	8.18	1.28	8.07	1.24
Current Interest Rate	8.63	1.35	8.56	1.37
Origination FICO	618	54	617	56.3
Origination Balance	196	114	206	133
Low/No Doc	0.42	0.49	0.6	0.49
Refi	0.055	0.23	0.1	0.3
Cash Out Refi	0.39	0.49	0.37	0.48
Prior 60 DPD	0.15	0.36	0.14	0.35
60 DPD in Prior Year	0.12	0.32	0.11	0.31
Equifax Variables				
	mean	sd	mean	sd
Current CLTV	121	28	119	28
Current Vantage	667	82	666	84
2 <sup>nd</sup> Lien Balance	47.9	31.4	52.4	38.4
Has Junior Lien	0.44	0.5	0.38	0.48
Credit Utilization	0.47	0.37	0.47	0.37

Table 2: 2/28 ARMs Default Specification

This table reports estimates of probit specification (1) using data on Hybrid 2/28 ARMs. The dependent variable takes the value of one when a loan becomes sixty days past due in a given month, conditional upon being current sixty days earlier, and is equal to zero otherwise. Column (1) estimates the model using the Base Sample and includes only time dummies, a Countrywide dummy that equals one if the loan is serviced by Countrywide, and interactions between the Countrywide and time dummies. The excluded category is July-September 2007, the first quarter of our analysis period. Column (2) adds additional controls from the BlackBox data, including a wide range of loan and borrower-level characteristics, such as origination LTV, origination FICO and CLTV (when available) and their interactions with time dummies, initial interest rate, current interest rate, reset controls capturing the timing of reset, and MSA fixed effects for the location of the property backing the loan. Columns (3) and (4) use the Matched Sample instead of the Base Sample. Column (3) includes the same controls as in Column (2); Column (4) includes additional Equifax controls, such as current CLTV, credit card utilization, and current credit scores (Vantage) and their change over time. Coefficients reported are marginal effects from a probit regression; t-statistics are in parentheses; standard errors clustered at the loan ID; \*  $p < 0.05$ , \*\*  $p < 0.01$ .

	(1) Base Sample	(2) Base Sample	(3) Matched Sample	(4) Matched Sample
Countrywide $\times$ Oct-Dec 2007	-0.0012* (-2.26)	-0.0005 (-1.17)	0.0002 (0.46)	0.0004 (0.88)
Countrywide $\times$ Jan-Mar 2008	-0.0003 (-0.67)	-0.0008 (-1.59)	-0.0002 (-0.46)	0.0002 (0.36)
Countrywide $\times$ Apr-Jun 2008	-0.0004 (-0.77)	-0.0022** (-4.14)	-0.0018** (-3.10)	-0.0014* (-2.50)
Countrywide $\times$ Jul-Sep 2008	0.0056** (8.33)	-0.0003 (-0.58)	0.0001 (0.30)	0.0006 (1.15)
<b>Countrywide <math>\times</math> Oct-Dec 2008</b>	<b>0.016** (20.06)</b>	<b>0.0052** (8.09)</b>	<b>0.0048** (6.56)</b>	<b>0.0054** (7.83)</b>
Countrywide $\times$ Jan-Feb 2009	0.0092** (10.47)	0.0001 (0.16)	-0.0005 (-0.70)	0.0005 (0.75)
Countrywide	-0.0002 (-0.68)	0.0016** (4.25)	0.0017** (3.68)	0.0018** (4.25)
Origination Quarter	No	Yes	Yes	Yes
BlackBox Control	No	Yes	Yes	Yes
MSA Control	No	Yes	Yes	Yes
Reset Control	No	Yes	Yes	Yes
Equifax Control	No	No	No	Yes
N. of Observations	9448457	9448457	6261055	6261055
Avg. Share Countrywide	0.15	0.15	0.18	0.18
Avg. Countrywide 2008Q3 Delinquency	0.049	0.049	0.048	0.048
Countrywide $\times$ (Q4 2008 - Q3 2008)	0.0104	0.0055	0.0047	0.0048
Wald Test (p-value)	0.0000	0.0000	0.0000	0.0000

Table 3: 2/28 ARMs Default Specification by Current Credit Utilization and CLTV

This table reports estimates of probit specification (1) for Hybrid 2/28 ARMs, but stratifies the Matched Sample by borrowers current credit utilization and CLTV. The models are estimated using the full set of Blackbox and Equifax controls used in Column (4) of Table 2. The dependent variable takes the value of one if the loan becomes sixty days past due in a given month, conditional upon being current sixty days earlier, and is equal to zero otherwise. The excluded category is July-September 2007, the first quarter of our analysis period. Columns (1) through (3) separate borrowers by available credit card utilization, (4) through (6) separate them by CLTV, and (7) subsets on underwater borrowers with high available credit utilization. Coefficients reported are marginal effects from a probit regression; t-statistics are in parentheses; standard errors clustered at the loan ID; \*  $p < 0.05$ , \*\*  $p < 0.01$ .

	(1) > 5 Months	(2) 1-5 Months	(3) 0-1 Months	(4) CLTV < 100	(5) 100 ≤ CLTV < 120	(6) 120 ≤ CLTV	(7) > 5 Months & CLTV > 100
Countrywide × Oct-Dec 2007	-0.0004 (-0.50)	0.0008 (0.84)	-0.0001 (-0.18)	0.0011 (1.77)	-0.0011 (-1.28)	-0.0059 (-1.66)	-0.0026 (-1.58)
Countrywide × Jan-Mar 2008	0.0012 (1.29)	0.0008 (0.86)	-0.0001 (-0.17)	0.0013 (1.86)	-0.0008 (-0.96)	-0.0054 (-1.58)	-0.0006 (-0.36)
Countrywide × Apr-Jun 2008	0.0003 (0.39)	-0.0014 (-1.51)	-0.0014 (-1.30)	-0.0009 (-1.22)	-0.0032** (-3.62)	-0.0055 (-1.64)	-0.0024 (-1.51)
Countrywide × Jul-Sep 2008	0.0011 (1.23)	0.0018 (1.77)	0.0015 (1.32)	0.0009 (1.01)	-0.0011 (-1.15)	0.0007 (0.22)	-0.0011 (-0.69)
<b>Countrywide × Oct-Dec 2008</b>	<b>0.0054** (4.59)</b>	<b>0.0052** (4.31)</b>	<b>0.0063** (4.74)</b>	<b>0.0050** (4.51)</b>	<b>0.0038** (3.12)</b>	<b>0.0076 (1.95)</b>	<b>0.0049* (2.30)</b>
Countrywide × Jan-Feb 2009	0.0002 (0.22)	0.0009 (0.75)	0.0015 (1.05)	0.0049** (3.38)	-0.0004 (-0.34)	-0.0029 (-0.82)	-0.0028 (-1.58)
Countrywide	-0.0001 (-0.17)	0.0011 (1.48)	0.0025** (3.12)	0.0005 (1.19)	0.0040** (5.28)	0.0078* (2.18)	0.0025 (1.55)
Origination Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
BlackBox Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reset Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Equifax Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of Observations	1210922	1639789	1748890	1994158	1715891	1067641	611797
Avg. Share Countrywide	0.18	0.19	0.18	0.16	0.18	0.19	0.20
Avg. Countrywide 2008Q3 Delinquency	0.030	0.041	0.052	0.031	0.039	0.078	0.040
Countrywide × (Q4 2008 - Q3 2008)	0.0043	0.0034	0.0048	0.0041	0.0049	0.0069	0.006
Wald Test (p-value)	0.0000	0.0019	0.0001	0.0010	0.0000	0.0001	0.0000



Table 4: Default Specifications for Other Debt Types

This table reports estimates from models similar to probit specification (1), but with the dependent variable measuring delinquency on loans that were not targeted by the Settlement. The models are estimated using the Matched Sample and the full set of Blackbox and Equifax controls used in Column (4) of Table 2. In Column (1) below, the dependent variable equals one if the borrowers becomes sixty-days past due on a second lien, conditional upon being current two months earlier, and equals zero otherwise. In Column (2), the dependent variable equals one if the borrower becomes delinquent on credit card debt, conditional upon being current two months earlier, and equals zero otherwise. Columns (3)-(6) show the corresponding results after subsetting on borrowers with low credit utilization (Columns 3 and 4) and CLTV < 100 (Columns 5 and 6). Column (7) uses data on borrowers with fixed-rate mortgages (FRMs). The dependent variable equals one if the borrower becomes sixty-days past due on a FRM, conditional upon being current two months earlier, and equals zero otherwise. Across all columns, the excluded category is July-September 2007, the first quarter of our analysis period. Coefficients reported are marginal effects from a probit regression; t-statistics are in parentheses; standard errors clustered at the loan ID \*  $p < 0.05$ , \*\*  $p < 0.01$ .

	5+ Months Utilization				CLTV < 100		
	(1) 2nd Lien	(2) Credit Card	(3) 2nd Lien	(4) Credit Card	(5) 2nd Lien	(6) Credit Card	(7) Non-Subprime FRM
Countrywide × Oct-Dec 2007	0.0002 (0.23)	-0.0016 (-1.25)	-0.0011 (-0.90)	-0.0026 (-1.49)	0.0001 (0.15)	-0.0009 (-0.43)	0.0000 (0.91)
Countrywide × Jan-Mar 2008	0.0012 (1.26)	0.0009 (0.63)	-0.0009 (-0.77)	-0.0030 (-1.59)	0.0011 (0.96)	0.0039 (1.52)	0.0003** (3.64)
Countrywide × Apr-Jun 2008	-0.0000 (-0.04)	0.0004 (0.26)	-0.0018 (-1.66)	-0.0022 (-1.10)	-0.0002 (-0.20)	0.0013 (0.43)	0.0003** (3.02)
Countrywide × Jul-Sep 2008	0.0004 (0.49)	-0.0001 (-0.13)	-0.0016 (-1.42)	-0.0016 (-0.77)	0.0013 (0.88)	-0.0000 (-0.02)	0.0000 (0.78)
Countrywide × Oct-Dec 2008	0.0004 (0.42)	-0.0018 (-1.11)	-0.0018 (-1.58)	-0.0047* (-2.23)	-0.0002 (-0.16)	0.0011 (0.29)	-0.0002** (-3.27)
Countrywide × Jan-Feb 2009	-0.0003 (-0.37)	-0.0016 (-0.82)	-0.0031** (-3.08)	-0.0033 (-1.23)	0.0017 (0.87)	0.0071 (1.38)	-0.0002** (-2.75)
Countrywide	0.0018* (2.11)	-0.0014 (-1.17)	0.0033* (2.32)	0.0033 (1.90)	0.0001 (0.17)	-0.0026 (-1.41)	0.0004** (6.42)
Origination Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
BlackBox Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reset Control	Yes	Yes	Yes	Yes	Yes	Yes	No
Equifax Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of Observations	1906151	3230307	525191	903902	265800	870999	8920874
Avg. Share Countrywide	0.20	0.18	0.21	0.18	0.18	0.16	0.30
Avg. Countrywide 2008Q3 Delinquency	0.028	0.073	0.021	0.045	0.010	0.082	0.0090
Countrywide × (Q4 2008 - Q3 2008)	0.0000	-0.0017	-0.0002	-0.0031	-0.0015	0.0011	-0.0002
Wald Test (p-value)	0.94	0.28	0.83	0.12	0.31	0.77	0.0000

Figure 1: Internet Searches for “Countrywide Modification”

This figure plots an index of the weekly volume of internet searches for the term Countrywide Modification, as reported by Google Trends. Searches for this term spiked in October 6, the day the Countrywide Settlement was announced and reported by newspapers around the country.

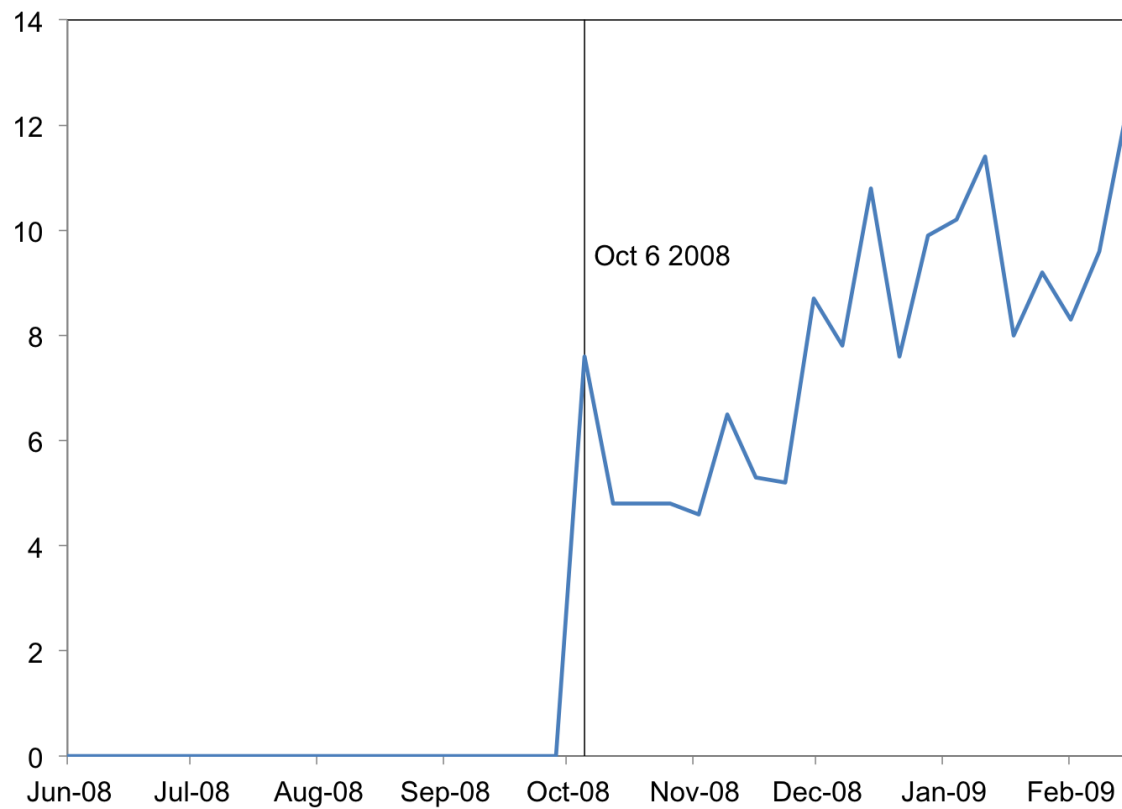


Figure 2: Comparability of Countrywide and Control Group - Evolution of Observables.

This figure shows the monthly evolution of current mortgage interest rate (panel (a)), current Vantage credit score of borrowers (panel (b)), and current CLTV (panel (c)) for 2/28 ARMs in the Matched Sample prior to the Settlement Announcement. The straight line shows monthly averages for Countrywide loans while the dashed line shows the corresponding values for mortgages in the Control Group.

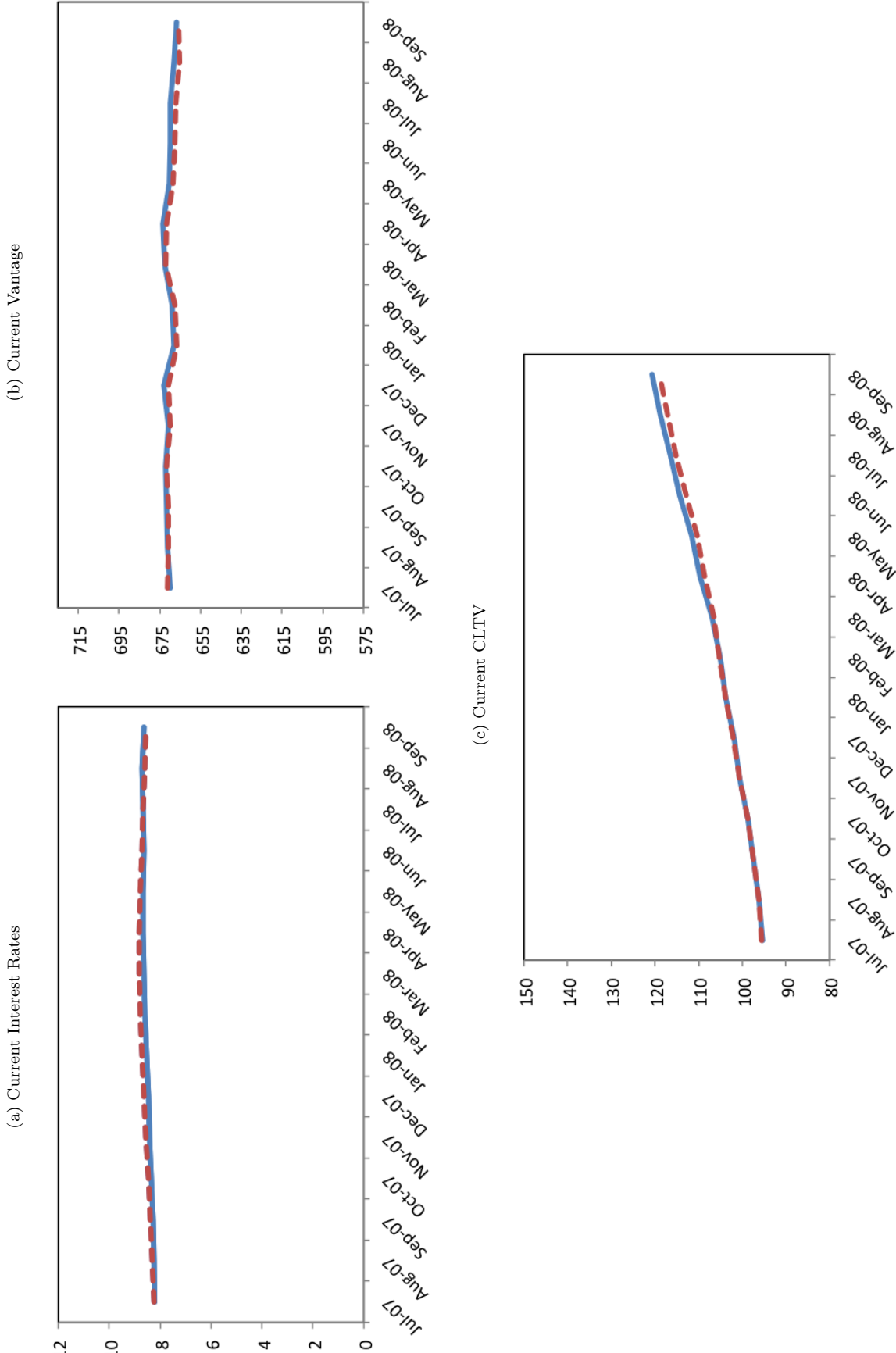
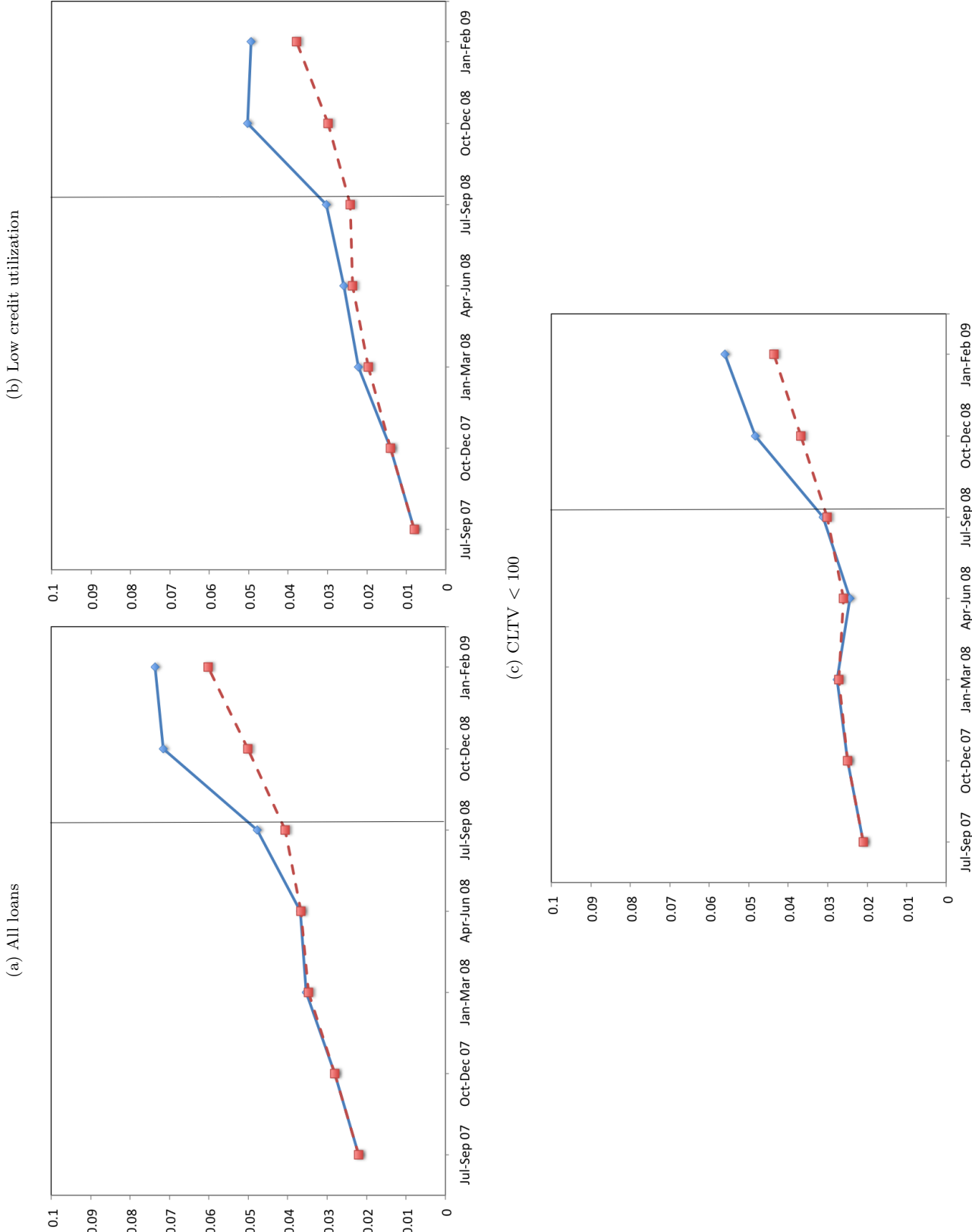


Figure 3: Evolution of Default Rates for 2/28 ARMs - Countrywide and Control Group

This figure plots the average monthly rollover rate (the 60-day delinquency rate among borrowers who were current two months before) during each of the five quarters preceding the Settlement announcement, the quarter just after the announcement (Oct-Dec 2008), and the Jan-Feb 2009 period. Panel (a) plots the rate for all the loans, panel (b) subsets on borrowers with low credit utilization, and panel (c) subsets on borrowers whose mortgages had a CLTV less than 100 percent at the time of delinquency. The straight line shows the rollover rate for Countrywide loans while the dashed line shows the corresponding rate for mortgages in the Control Group.



For Online Publication:

Appendix: Tables and Figures

Table A1: Summary Statistics for Non-Subprime FRMs.

This table presents summary statistics for non-subprime fixed-rate mortgages (FRMs) serviced by Countrywide and the Control Group in the Matched Sample as of September 2008. The summary statistics include characteristics of these loans at origination and as of September 2008, the month before public announcement of the Settlement on October 6, 2008. CLTV and interest rates are reported in percentage terms; loan balances are in thousands of dollars.

Countrywide		Control		
Number of Loans	130,033	301,568		
BlackBox Variables				
	mean	sd	mean	sd
Initial CLTV	88.7	9.36	87.3	9.67
Initial Interest Rate	6.73	0.95	6.40	0.52
Origination FICO	709.3	51.7	719.4	48.3
Origination Balance	323	216	313	200
Low/No Doc	0.66	0.47	0.67	0.47
Refi	0.21	0.41	0.17	0.37
Cash out Refoi	0.23	0.42	0.30	0.46
Prior 60 DPD	0.029	0.17	0.042	0.20
60 DPD in Prior Year	0.012	0.11	0.0091	0.095
Equifax Variables				
	mean	sd	mean	sd
Current CLTV	106.9	25.0	108.0	24.2
Current Vantage	798.8	122.1	809.5	109.1
2 <sup>nd</sup> Lien Balance	53.9	33.0	53.5	30.7
Has Junior Lien	0.40	0.49	0.46	0.50
Credit Utilization	0.31	0.30	0.33	0.31

Table A2: 2/28 ARMs Default Specification - The Role of Control Variables

This table reports estimates of probit specification (1) using data on Hybrid 2/28 ARMs. The dependent variable takes the value of one when a loan becomes sixty days past due in a given month, conditional upon being current sixty days earlier, and is equal to zero otherwise. Column (1) estimates the model using the Matched Sample and includes only time dummies, a Countrywide dummy that equals one if the loan is serviced by Countrywide, and interactions between the Countrywide and time dummies. The excluded category is July-September 2007, the first quarter of our analysis period. Columns (2) through (12) progressively add a set of controls from the BlackBox and Equifax databases. Coefficients reported are marginal effects from a probit regression; t-statistics are in parentheses; standard errors clustered at the loan ID; \*  $p < 0.05$ , \*\*  $p < 0.01$ .

	(1)	(2)	(3)	(4)	(5)	(6)
	Matched Sample	Matched Sample	Matched Sample	Matched Sample	Matched Sample	Matched Sample
Countrywide × Oct-Dec 2007	0.0000 (0.13)	-0.0003 (-0.49)	-0.0003 (-0.62)	-0.0004 (-0.67)	-0.0000 (-0.12)	0.0002 (0.40)
Countrywide × Jan-Mar 2008	0.0009 (1.38)	0.0005 (0.77)	0.0003 (0.46)	0.0001 (0.20)	0.0001 (0.25)	-0.0000 (-0.02)
Countrywide × Apr-Jun 2008	0.0004 (0.72)	0.0003 (0.52)	-0.0000 (-0.08)	-0.0004 (-0.76)	0.0001 (0.19)	-0.0013* (-2.08)
Countrywide × Jul-Sep 2008	0.0065** (8.43)	0.0064** (8.34)	0.0056** (7.52)	0.0046** (6.46)	0.0039** (5.25)	0.0014* (2.00)
<b>Countrywide × Oct-Dec 2008</b>	<b>0.016** (17.88)</b>	<b>0.016** (17.91)</b>	<b>0.015** (17.07)</b>	<b>0.014** (15.97)</b>	<b>0.012** (13.67)</b>	<b>0.0071** (8.78)</b>
Countrywide × Jan-Feb 2009	0.0090** (9.24)	0.0093** (9.45)	0.0084** (8.74)	0.0071** (7.75)	0.0049** (5.22)	-0.0007 (-0.86)
Countrywide	-0.0002 (-0.61)	0.0009 (1.89)	0.0019** (3.96)	0.0028** (5.86)	0.0031** (6.06)	0.0027** (5.41)
Origination Quarter	Yes	Yes	Yes	Yes	Yes	Yes
Blackbox FICO Controls	No	Yes	Yes	Yes	Yes	Yes
Blackbox CLTV Controls and Balance	No	No	Yes	Yes	Yes	Yes
Blackbox FICO and CLTV Time Interactions	No	No	No	Yes	Yes	Yes
Blackbox Interest Rate Controls	No	No	No	No	Yes	Yes
Reset Controls	No	No	No	No	No	Yes
Blackbox Documentation and Refi Controls	No	No	No	No	No	No
Blackbox Payment Controls	No	No	No	No	No	No
MSA Controls	No	No	No	No	No	No
Vantage Controls	No	No	No	No	No	No
Equifax CLTV Controls	No	No	No	No	No	No
Other Equifax Controls	No	No	No	No	No	No
N. of Observations	6261055	6261055	6261055	6261055	6261055	6261055
Avg. Share Countrywide	0.18	0.18	0.18	0.18	0.18	0.18
Avg. Countrywide 2008Q3 Delinquency	0.048	0.048	0.048	0.048	0.048	0.048
Countrywide × (Q3 2008 - Q4 2008)	0.0095	0.0096	0.0094	0.0094	0.0081	0.0057
Wald Test (p-value)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Table A2 (cont'd): 2/28 ARMs Default Specification - The Role of Control Variables

	(7)	(8)	(9)	(10)	(11)	(12)
	Matched Sample	Matched Sample	Matched Sample	Matched Sample	Matched Sample	Matched Sample
Countrywide $\times$ Oct-Dec 2007	0.0000 (0.12)	0.0002 (0.43)	0.0002 (0.46)	0.0005 (0.96)	0.0005 (0.98)	0.0004 (0.88)
Countrywide $\times$ Jan-Mar 2008	-0.0003 (-0.49)	-0.0003 (-0.54)	-0.0002 (-0.46)	0.0002 (0.38)	0.0001 (0.34)	0.0002 (0.36)
Countrywide $\times$ Apr-Jun 2008	-0.0016* (-2.56)	-0.0018** (-3.01)	-0.0018** (-3.10)	-0.0011* (-2.02)	-0.0014* (-2.50)	-0.0014* (-2.50)
Countrywide $\times$ Jul-Sep 2008	0.0010 (1.45)	0.0002 (0.39)	0.0001 (0.30)	0.0011 (1.85)	0.0007 (1.30)	0.0006 (1.15)
<b>Countrywide <math>\times</math> Oct-Dec 2008</b>	<b>0.0066**</b> <b>(8.31)</b>	<b>0.0049**</b> <b>(6.54)</b>	<b>0.0048**</b> <b>(6.56)</b>	<b>0.0058**</b> <b>(8.19)</b>	<b>0.0055**</b> <b>(7.90)</b>	<b>0.0054**</b> <b>(7.83)</b>
Countrywide $\times$ Jan-Feb 2009	-0.0009 (-1.15)	-0.0005 (-0.68)	-0.0005 (-0.70)	0.0007 (1.00)	0.0004 (0.63)	0.0005 (0.75)
Countrywide	0.0026** (5.26)	0.0017** (3.75)	0.0017** (3.68)	0.0018** (4.27)	0.0018** (4.32)	0.0018** (4.25)
Origination Quarter	Yes	Yes	Yes	Yes	Yes	Yes
Blackbox FICO Controls	Yes	Yes	Yes	Yes	Yes	Yes
Blackbox CLTV Controls and Balance	Yes	Yes	Yes	Yes	Yes	Yes
Blackbox FICO and CLTV Time Interactions	Yes	Yes	Yes	Yes	Yes	Yes
Blackbox Interest Rate Controls	Yes	Yes	Yes	Yes	Yes	Yes
Reset Controls	Yes	Yes	Yes	Yes	Yes	Yes
Blackbox Documentation and Refi Controls	Yes	Yes	Yes	Yes	Yes	Yes
Blackbox Payment Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Controls	No	No	Yes	Yes	Yes	Yes
Vantage Controls	No	No	Yes	Yes	Yes	Yes
Equifax CLTV Controls	No	No	No	Yes	Yes	Yes
Other Equifax Controls	No	No	No	No	Yes	Yes
N. of Observations	6261055	6261055	6261055	6261055	6261055	6261055
Avg. Share Countrywide	0.18	0.18	0.18	0.18	0.18	0.18
Avg. Countrywide 2008Q3 Delinquency	0.048	0.048	0.048	0.048	0.048	0.048
Countrywide $\times$ (Q4 2008 - Q3 2008)	0.0056	0.0047	0.0047	0.0047	0.0048	0.0048
Wald Test (p-value)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000



Table A3: 2/28 ARMs Default Specification - Standard Errors Clustered at MSA Level

This table is analogous to Table 2 except that standard errors are clustered at the Metropolitan Statistical Area (MSA) corresponding to the location of the property backing the loan (in Table 2, they were clustered at the loan ID). Coefficients reported are marginal effects from a probit regression; t-statistics are in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ .

	(1)	(2)	(3)	(4)
	Base Sample	Base Sample	Matched Sample	Matched Sample
Countrywide $\times$ Oct-Dec 2007	-0.0012* (-2.04)	-0.0005 (-1.15)	0.0002 (0.51)	0.0004 (0.99)
Countrywide $\times$ Jan-Mar 2008	-0.0003 (-0.60)	-0.0008 (-1.63)	-0.0002 (-0.47)	0.00020 (0.37)
Countrywide $\times$ Apr-Jun 2008	-0.0004 (-0.66)	-0.0022** (-3.96)	-0.0018** (-2.90)	-0.0014* (-2.40)
Countrywide $\times$ Jul-Sep 2008	0.0056** (6.82)	-0.0003 (-0.58)	0.0001 (0.31)	0.0006 (1.21)
<b>Countrywide <math>\times</math> Oct-Dec 2008</b>	<b>0.016** (15.11)</b>	<b>0.0052** (7.96)</b>	<b>0.0048** (6.12)</b>	<b>0.0054** (7.49)</b>
Countrywide $\times$ Jan-Feb 2009	0.0092** (8.87)	0.0001 (0.15)	-0.0005 (-0.56)	0.0005 (0.61)
Countrywide	-0.0002 (-0.53)	0.0016** (3.72)	0.0017** (3.34)	0.0018** (3.80)
Origination Quarter	No	Yes	Yes	Yes
BlackBox Control	No	Yes	Yes	Yes
MSA Control	No	Yes	Yes	Yes
Reset Control	No	Yes	Yes	Yes
Equifax Control	No	No	No	Yes
N. of Observations	9448457	9448457	6261055	6261055
Avg. Share Countrywide	0.15	0.15	0.18	0.18
Avg. Countrywide Delinquency 2008Q3	0.049	0.049	0.048	0.048
Countrywide $\times$ (Q4 2008 - Q3 2008)	0.0104	0.0055	0.0047	0.0048
Wald Test (p-value)	0.0000	0.0000	0.0000	0.0000

Table A4: 2/28 ARMS Default Specification by Current Credit Utilization and CLTV - Standard Errors Clustered at MSA Level

This table is analogous to Table 3 except that standard errors are clustered at the Metropolitan Statistical Area (MSA) corresponding to the location of the property backing the loan (in Table 3, they were clustered at the loan ID). Coefficients reported are marginal effects from a probit regression; t-statistics are in parentheses; \*  $p < 0.05$ , \*\*  $p < 0.01$ .

	(1) >5 Months	(2) 1-5 Months	(3) 0-1 Months	(4) CLTV < 100	(5) 100 ≤ CLTV < 120	(6) 120 ≤ CLTV < 150	(7) >150 Months & CLTV > 100
Countrywide × Oct-Dec 2007	-0.0004 (-0.47)	0.0008 (0.77)	-0.0001 (-0.18)	0.0011* (1.97)	-0.0011 (-1.09)	-0.0059 (-1.68)	-0.0026 (-1.37)
Countrywide × Jan-Mar 2008	0.0012 (1.36)	0.0008 (0.88)	-0.0001 (-0.17)	0.0013* (1.96)	-0.0008 (-0.85)	-0.0054 (-1.77)	-0.0006 (-0.40)
Countrywide × Apr-Jun 2008	0.0003 (0.34)	-0.0014 (-1.43)	-0.0014 (-1.40)	-0.0009 (-1.13)	-0.0032** (-3.49)	-0.0055 (-1.58)	-0.0024 (-1.43)
Countrywide × Jul-Sep 2008	0.0011 (1.10)	0.0018 (1.59)	0.0015 (1.13)	0.0009 (1.03)	-0.0011 (-1.00)	0.0007 (0.21)	-0.0011 (-0.64)
<b>Countrywide × Oct-Dec 2008</b>	<b>0.0054**</b> (4.53)	<b>0.0052**</b> (3.45)	<b>0.0063**</b> (5.44)	<b>0.0050**</b> (4.00)	<b>0.0038**</b> (3.25)	<b>0.0076</b> (1.74)	<b>0.0049*</b> (2.26)
Countrywide × Jan-Feb 2009	0.0002 (0.23)	0.0009 (0.72)	0.0015 (0.93)	0.0049** (4.29)	-0.0004 (-0.27)	-0.0029 (-0.77)	-0.0028 (-1.70)
Countrywide	-0.0001 (-0.15)	0.0011 (1.48)	0.0025** (2.70)	0.0005 (1.08)	0.0040** (3.85)	0.0078* (2.15)	0.0025 (1.47)
Origination Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
BlackBox Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reset Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Equifax Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of Observations	1210922	1639789	1748890	1994158	1715891	1067641	611797
Avg. Share Countrywide	0.18	0.19	0.18	0.16	0.18	0.19	0.20
Avg. Countrywide Delinquency 2008Q3	0.030	0.041	0.052	0.031	0.039	0.078	0.040
Countrywide × (Q4 2008 - Q3 2008)	0.0043	.0034	0.0048	0.0041	0.0049	0.0069	0.006
Wald Test (p-value)	0.0000	0.0034	0.0000	0.0012	0.0000	0.0006	0.0004

Figure A1: 2/28 ARMs Pre-estimation Default Patterns - Countrywide and Control Group

This figure presents the quarterly cumulative default rates for Countrywide and Control Group 2/28 ARM loans from mid 2006 through mid 2007 (just before our analysis period begins). The denominator equals the total stock of loans that had been originated up to that quarter; the numerator is the total number of loans that have ever defaulted from mid 2006 through that quarter. A loan is counted as in default if its status is 60 days past due on payments or worse. Foreclosed loans are counted as having defaulted while loans that have prepaid are counted as being current. The straight line shows the default rate for Countrywide loans; the dashed line shows the corresponding rate for mortgages in the Control Group.

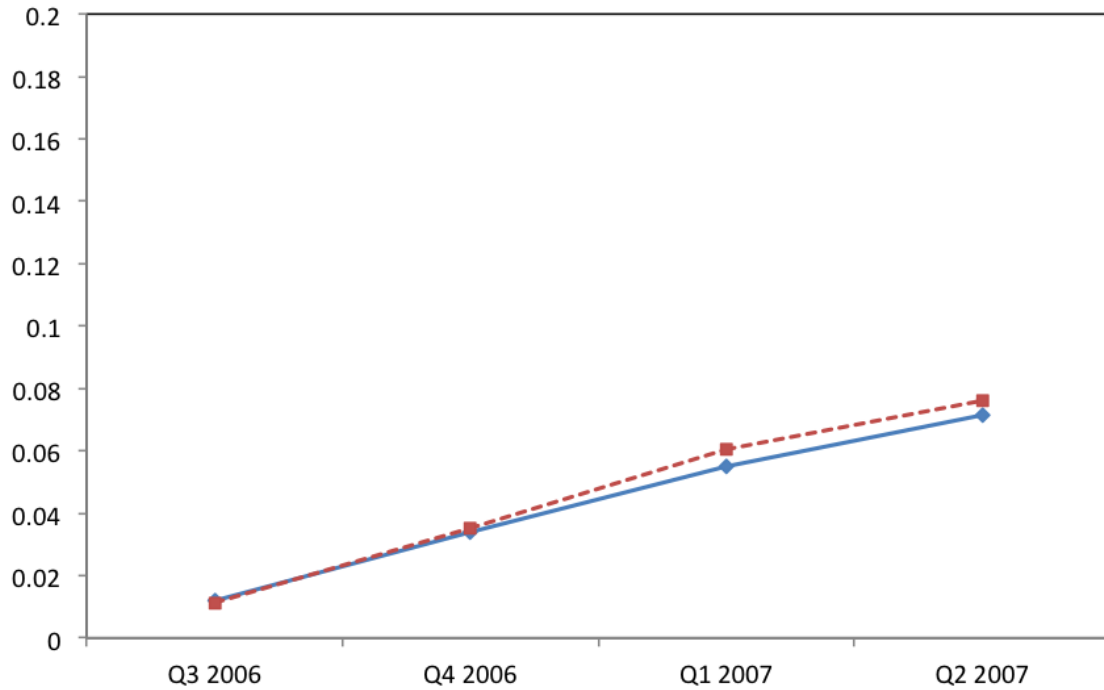


Figure A2: 2/28 ARMs Specification for Survival in Current Status

This figure presents estimates of a similar specification to the baseline specification in Column (4) of Table 2 but with a changed dependent variable. Now the dependent variable equals one if the outstanding loan is current in a given month and is zero otherwise. This specification allows us to study the difference in the rate at which Countrywide and Control Group mortgages stay in the "current" payment status. We include loans in our analysis regardless of their status in previous months (we include controls for the payment status of a loan in the prior month in addition to the full battery of controls included in our baseline specification). Foreclosed loans leave the estimation sample. Panel (a) shows the results for all loans, while panel (b) and panel (c) show the respective results for borrowers with low credit utilization and for loans with CLTV < 100. The displayed coefficients are marginal probit estimates of the interactions between Countrywide and the time dummies, where the excluded category is July-September 2007. The dashed lines show 99% confidence intervals around these estimates. Coefficients reported are marginal effects from a probit regression (a change of dummy variable from 0 to 1); standard errors clustered at the loan ID.

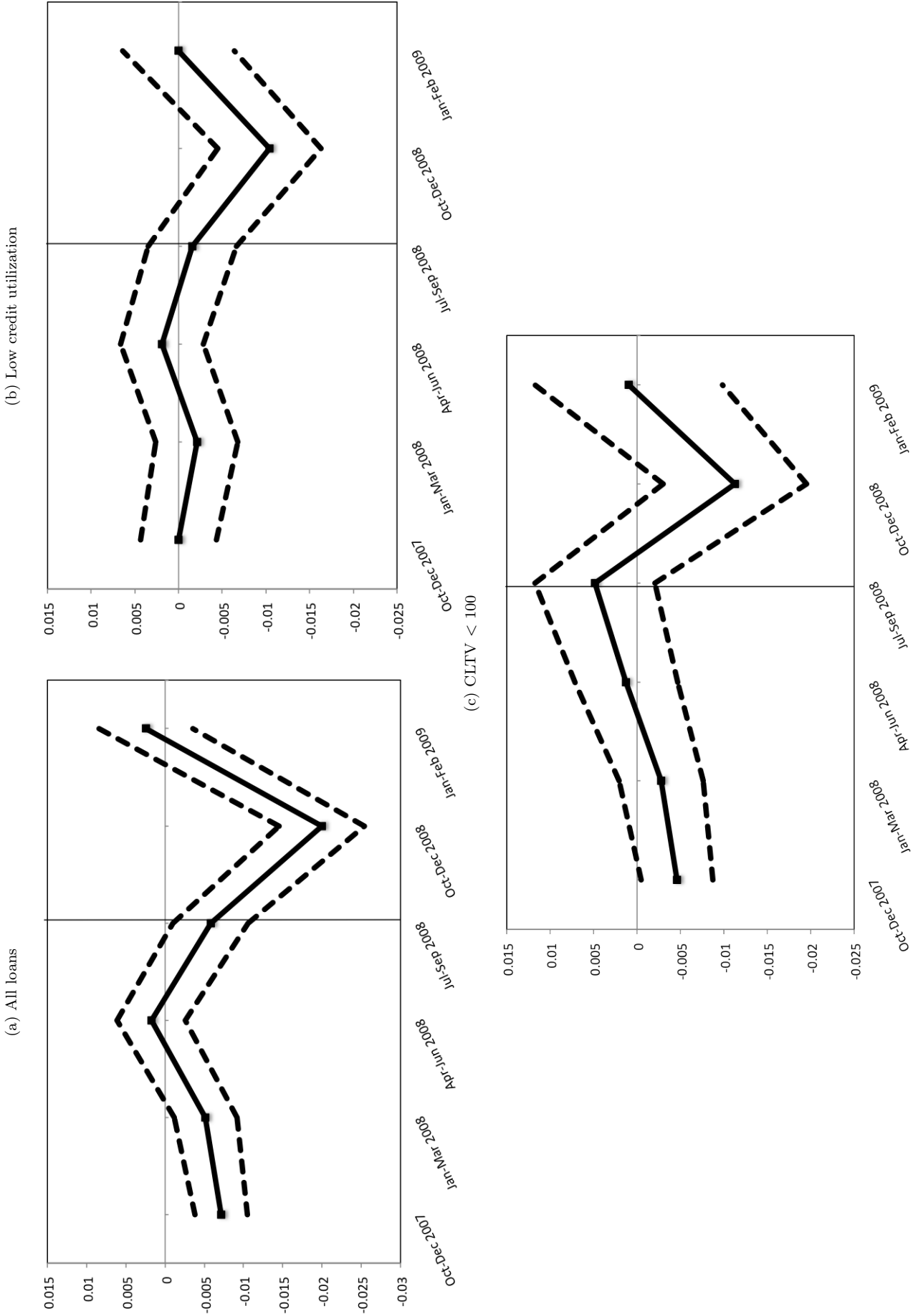
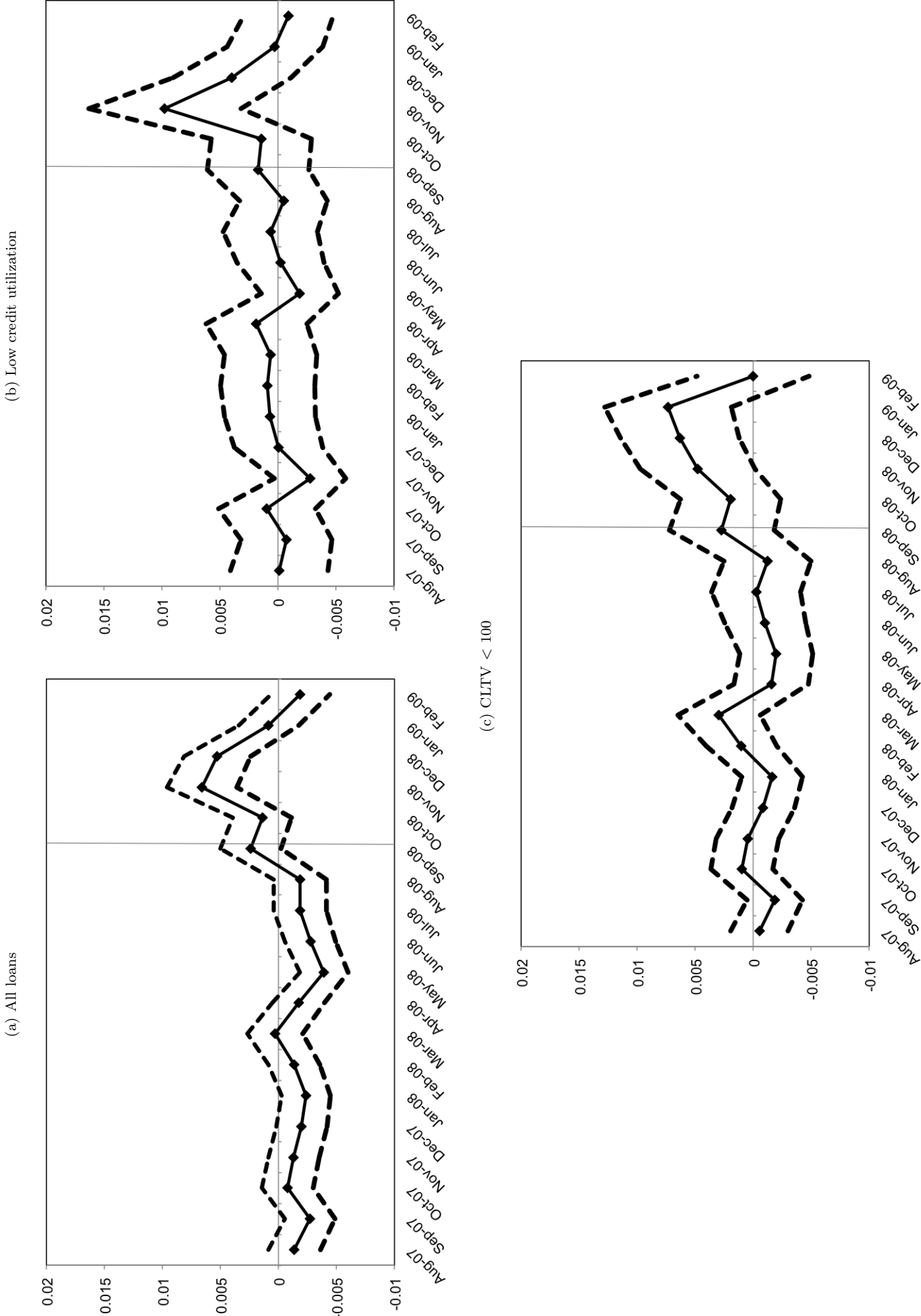


Figure A3: 2/28 ARMs default Specification - Monthly Effects

This figure plots estimates of probit specification (1), but with monthly time dummies instead of quarterly time dummies. WE use the Matched Sample and the full set of Blackbox and Equifax controls used in Column (4) of Table 2. The displayed coefficients are marginal probit estimates of the interactions between the Countrywide and time dummies. The excluded category is the July-September 2007 period. The dashed lines show 99% confidence intervals around these estimates. Standard errors clustered at the loan ID.



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1. Lee Anne Fennell and Eduardo M. Peñalver, Exactions Creep, December 2013
2. Lee Anne Fennell, Forcings, November 2013
3. Neil Bhutta and Benjamin J. Keys, Interest Rates and Equity Extraction during the Housing Boom, January 2014
4. Christopher Mayer, Edward Morrison, Tomasz Piskorski, and Arpit Gupta, Mortgage Modification and Strategic Behavior: Evidence from a Legal Settlement with Countrywide, January 2014
5. Edward R. Morrison, Coasean Bargaining in Consumer Bankruptcy, January 2014
6. Atif Mian, Amir Sufi, and Francesco Trebbi, Foreclosures, House Prices, and the Real Economy, January 2014