Securitization and the Growth of Subprime Mortgage Lending *

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Abstract

Did private mortgage securitization contribute causally to the recent subprime lending boom, or was it simply a slightly more convenient way to finance lending that would have occurred anyway? We exploit a natural experiment in which the S&P rating agency increased costs of securitizing specific types of mortgages from certain states. We find significant reductions in total originations of affected mortgage types following the change, suggesting that securitization played a causal role in fueling mortgage expansion. We also, however, find suggestive evidence for an increase in mortgages held on books of financial institutions that partially offset the restriction of securitization.

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1 Introduction

The largest American financial collapse since the Great Depression has naturally led policy makers, academics, and the general public to search for answers to what factors could have helped cause such a catastrophe. Private residential mortgage backed securities were an early culprit that many narrative accounts of the crisis pointed to. The Financial Crisis Inquiry Commission describes what it terms "the runaway mortgage securitization train" and repeatedly lays blame on it, as well as other factors, in contributing to the buildup and then collapse of the US mortgage market (Financial Crisis Inquiry Commission, 2011). Accounts in the popular press likewise frequently discuss the ways in which private mortgage securitized "fueled" the housing market boom and bust.[1]

The stories told in these early narrative accounts of the crisis, linking private securitization with the growth of subprime mortgage lending in particular are by no means implausible. The practices of subprime mortgage lending and mortgage securitization by private, non-GSE entities both grew rapidly and in tandem in the early to mid 2000s during the lead up to the global financial crisis (Figure 1). In addition to their concurrent rise, between 50 and 75% of subprime mortgages were securitized, depending on year (Figure 2), and during some pre-crisis years, the majority of growth in total mortgage securitizations came from private securitization vehicles which focused their holdings on subprime mortgages (Figure 3). Facts such as these suggest some form of close linkage between subprime mortgage lending and private mortgage securitization.

Yet, heuristic descriptions of the lead up to the financial crisis and simple correlations between the growth of securitization and subprime lending fall short of rigorously establishing a causal link between these phenomena. In particular, they fail to address the crucial counterfactual question of how much of the housing market crisis would likely have occurred anyway, even in the absence of private mortgage securitization. If such securitization were simply a slightly more convenient means to finance mortgages that could also have been financed through more traditional funding channels, then accounts of the crisis that focus too heavily on "the runaway mortgage securitization train" could lead to misguided or at least incomplete

[1] See, e.g., *New York Times* "For Some Subprime Borrowers, Few Good Choices," 22 March 2007: "Securitization ... has provided much of the cash that fueled subprime lending" ; *Wall Street Journal* "New Era in Mortgage Bonds?," 15 October 2012: "Subprime and other private mortgage bonds ... fueled the real-estate boom" ; and *New York Times* "A Year Of Chaos In Finance," 19 December 2008: "The securitization machine that is so vilified today played a major role in greasing the boom that preceded it."

reform efforts that seek to prevent future crisis by altering the securitization market while failing to address the more fundamental economic forces that caused it to take on the damaging characteristics associated with it. Conversely, if securitization did play a causal role in driving the mortgage boom and bust, then an increased focus on understanding how and why it had the impacts that it did is warranted.

This paper elucidates the relationship between securitization and subprime mortgage lending by testing for a causal connection between the rise of private securitization and the dramatic increase in subprime lending during the lead up to the global financial crisis. We demonstrate strong evidence for such a causal connection. We do so by considering a change in policy by the S&P rating agency (which rated subprime mortgage backed securities) made in response to state predatory lending laws in certain states that had already been in effect for some time before the rating change. The rating change effectively increased the cost of securitizing certain types of mortgages in certain states, but was unrelated to any recent shifts in the risk or profitability of the affected mortgages nor to any other factors that would change the desirability of mortgages to borrowers or investors apart from the influence of the securitization channel. We show a sharp drop in origination volume of affected mortgage types in affected states, but no such impact for unaffected mortgage types or in unaffected states.

Finding rigorous evidence for such a causal connection as this is a challenging econometric problem. Millions of subprime mortgages never were securitized. Furthermore, other types of debt that also began being privately securitized in the 1990s, such as consumer credit liabilities, never experienced the same rapid growth in volume as did subprime mortgage securitization, nor did the securitized portion of total liabilities in these other debt categories rise to such a large percentage of total liabilities (Figure 4).[2] Thus, private mortgage securitization and the subprime lending boom are clearly more than just two sides of the same coin. In fact, upon closer consideration, there seems no a priori reason to believe that a significant increase in private securitization should necessarily be seen as a causal driver in the growth of the underlying debt type.

Fundamentally, private mortgage securitizations offer an additional channel, but not a unique one, through which investors can take exposure to subprime mortgages. In the absence of securitization, for example, investors seeking exposure to subprime mortgages could buy mortgage servicing rights or invest in stock or bonds issued by mortgage originators, mortgage servicers,

[2]For an analysis of private securitization of corporate debt, which also exhibited very different patterns and characteristics than for subprime mortgages, see Benmelech et al. (2012)

or banks that hold mortgages on their books. Indeed, it was such an expansion of assets on books that characterized the Savings and Loan crisis: assets of S&Ls more than quadrupled between 1975 and 1988 as these institutions filled their balance sheets with a surge of new real estate loans at inflated values (Federal Deposit Insurance Corporation, 1997). By revealed choice, the fact that securitization rose rapidly in prominence during the 2000s suggests that it offered benefits to investors over other subprime mortgage investment options.[3] Yet, this fact alone demonstrates nothing about the size of the advantage investors saw securitization offering over other investment channels. If the advantage were quite small, it would be implausible to view securitization as a major causal driver of subprime lending.

Accordingly then, the inquiry into the causal role securitization played in the expansion of subprime lending can be seen in part as an inquiry into the size of the cost advantage that securitization offered over more traditional forms of mortgage investment.[4] Additionally, it is well known that during the financial crisis subprime mortgages began to default at astronomical rates. From an ex-post perspective at least, it seems clear that a great number of both homeowners and investors would have been better off if fewer subprime mortgages had been issued.[5] This paper therefore also aims to provide relevant information to policymakers seeking ways to prevent future financial calamities.

For a researcher to make a causal statement about the relationship between securitization and subprime lending, it is necessary to distinguish between the primary market for subprime loans (the initial transaction between borrower and lender), and the secondary market (in which the lender may then sell the loans to a third party, in particular for the purposes of this analysis, a securitization vehicle). In order to claim that securitization caused an increase in subprime lending (rather than just being correlated with it), it is generally necessary to identify one or more exogenous factors that influenced securitization but did not directly impact the primary market for subprime loan originations. Thus, for example, an increase in expected future house price values (or a drop in interest rates) might well make securitized mortgages more attractive to investors (the secondary market), but these influences would also likely make

[3]Whether these benefits represent a genuine social surplus or simply come from the ability to avoid regulations and externalize costs in the form of contributions to systemic risk and exploitation of implicit too-big-to-fail guarantees is of course another matter. Authors such as Acharya et al. (2013) take up this matter which we will not directly address here.

[4]Despite an extensive literature discussing the existence of such a cost advantage (see, e.g. Acharya et al., 2013) there has been up to this point little if any effort to directly assess its size. While achieving a direct estimate of the size of the cost benefit perceived by market participants of securitization over traditional mortgage investment opportunities still lies beyond the scope of this paper, our paper takes a step in that direction.

[5]Nevertheless, a full social welfare analysis of risky loans goes well beyond the scope of this paper and would depend upon many complexities we do not directly consider here.

subprime mortgages themselves more attractive to borrowers and direct lenders (even if they didn't plan to securitize those mortgages). Thus, while a house price increase might spark an increase in both total subprime mortgage securitizations (a secondary market outcome) and subprime mortgage originations (a primary market outcome), observing the concurrent rise in securitization and loan origination would not support a conclusion that the securitization *caused* the loan origination, since they would both be responding to a common third factor (the house price increase). If, by contrast, one can identify an exogenous factor that impacts *only* the securitization market, but does nothing to impact how desirable a mortgage is to a consumer, and how desirable that mortgage is to a lender who doesn't plan on securitizing it, then it can be possible to draw conclusions about the causal impact of securitization on subprime lending. For example, if such an exogenous factor makes it more expensive to securitize subprime mortgages, but does nothing (directly) to impact the demand that consumers have for those mortgages, or how desirable those mortgages are to lenders/investors who don't plan to securitize them, and that factor corresponds with a decrease in the total volume of subprime lending, then it could be reasonable to conclude that securitization played a causal role in driving total subprime lending volume.

In order to identify such an exogenous factor that impacted securitization but not primary market lending, we exploit a natural experiment that increased the costs of securitizing certain types of loans in some US states but not in others. This allows us to test whether an increase in the cost of securitization caused fewer loans to be made than otherwise would have. Between 2001 and 2003, a number of states passed anti-predatory lending (APL) laws.[6] These laws placed restrictions on mortgage terms and marketing. Furthermore, many of the laws provided that if a loan violated the provisions of the law and was subsequently sold to a third party (such as a securitization vehicle), the loan purchaser could be held liable for damages under the law.

A substantial amount of time after the state APL laws were passed, in May of 2004, the S&P rating agency adopted policies to address the impact of the laws on private mortgage securitization vehicles whose bonds S&P rated.[7] Specifically, S&P began requiring that loan originators provide additional credit enhancement for any loans affected by the APL laws that they sold to securitization vehicles. This credit enhancement sought to address the liability risk associated with the affected loans and effectively increased the cost of securitizing them. The

[6] For background on these APL laws, see for example Ding et al. (2011).

[7] At this time S&P was the dominant player in this rating sector, rating between 92% and 96% of all residential mortgage backed securities (Raiter, 2008, 2010).

rating change did nothing, however, to affect the primary market for loans - the supply and demand for mortgages determined by how desirable it was for a consumer to take out a loan and how valuable that loan was to its originator. Instead, it affected only how expensive it was to securitize a loan that had already been originated. As such, it provides an opportunity to study what happened to patterns of total lending when securitization became more expensive but nothing else changed about the primary market for loans.[8]

Furthermore, the S&P rating change had no impact on loans in states without APL laws or on loans in affected states in the event the loan terms made them exempt from the laws. Based on this, we are able to identify "treatment" and "control" groups of loans (based both on loan terms and where they were originated) that either did or did not experience an increase in the cost of securitization. We use negative binomial count regressions to model numbers of loans made in geographic areas that were and were not impacted by the rating change. We then use a difference in differences methodology to compare how much lending changed pre/post rating change in the "treatment" vs. "control" states. In this way we are able to isolate the impact of an increase in the cost of securitization on certain types of loans in certain states while controlling for broader patterns in lending activity that impacted all states equally.

Our initial results, when looking at the total pool of subprime loans, show a statistically significant reduction in the number of subprime mortgages that were securitized in affected states as compared to the number securitized from unaffected control states. This suggests that the S&P rating change was indeed an influential event, at least for the subprime securitization market. But, when we test for levels of overall subprime lending (i.e. looking both at subprime loans that were securitized as well as those that were held on books of financial institutions) we find almost no difference between states affected by the rating change and those unaffected. At first blush then, this might suggest that a significant increase in the cost of securitization had essentially no impact on the total levels of subprime lending and thus that securitization played little role in driving the expansion of subprime lending. One could naturally presume then that there was a near total substitution of other funding channels for the securitization channel when the latter became more expensive.

We refine our analysis, however, by focusing more precisely on the subset of loans whose characteristics made them specifically impacted by the state APL laws and thus by the S&P

[8]The original APL laws did affect the primary market for loans, which is why it is important that the S&P rating change occurred significantly after those were passed.

rating change as well.[9] In order to identify loans most impacted by the S&P rating change we analyze in detail nearly two dozen unique state anti-predatory lending statutes. Following this, we proceed by analyzing the complex network of federal legislation and regulations that interact with these state laws and at times dramatically alter the impact they would otherwise have. It is only by combining these multiple layers of legal analysis that we are able to pinpoint the loans most affected by the S&P rating change and then focus on them in our analysis.

As expected, when focusing on this more narrowly defined pool of subprime loans, we see an even larger decrease in the number of subprime mortgages that were securitized. But now, when we look at the total number of subprime mortgages, we see a smaller but still statistically significant reduction in total lending, including both securitized and non-securitized loans. Furthermore, we see some suggestive indications of a corresponding increase in the number of subprime loans from affected states held on the books of financial institutions.

Together, these results provide evidence that an increase in the costs of securitization resulted in a partial, rather than complete, substitution of other funding channels over the securitization channel. To the extent that these results can be generalized, they suggest that the rapid rise in subprime mortgage lending may have been partially fueled by the proliferation of private mortgage securitization but that a significant portion of the boom in subprime lending would have occurred even in the absence of the securitization channel.

To the best of our knowledge, this paper is the first to successfully offer causal evidence of the link between private securitization and subprime mortgage lending.[10] Mian and Sufi (2009) look directly at subprime securitization, and find correlational but not causal evidence of a link between increases in securitization and a greater likelihood of subprime mortgage applications being granted. Gabriel and Rosenthal (2007) present evidence that in the area of prime mortgage lending, securitization by GSEs had a causal impact on the likelihood that mortgage applications would be granted. Keys et al. (2010) investigate the causal impact of securitization on subprime lending, but focus on loan risk, rather than total volume of lending as we do here.

[9] See Section 3.3 for a detailed discussion of which types of loans were affected and how by the rating changes.

[10] One previous paper, Nadauld and Sherlund (2013), also looks for causal evidence of a relationship between securitization and subprime lending volume. Nadauld and Sherlund (2013) build an empirical methodology based on the assumption that changes to broker-dealer regulations uniquely impacted the securitization activity of investment banks as compared to commercial banks and other non-bank securitizers. Yet, while investment banks have historically been associated with broker-dealers, ever since the effective repeal of the Glass-Steagall Act in 1999, Commercial banks have also been allowed to own broker-dealers. Because the empirical strategy in Nadauld and Sherlund (2013) does not mention or make provision for the fact that nearly every large private securitizer owned a broker-dealer, not just the investment banks, it is unclear what conclusions can be drawn from the analysis presented in that paper.

This paper also enhances the empirical understanding of the impacts of anti-predatory lending efforts. Agarwal et al. (2014) provides an in-depth analysis of an anti-predatory lending program implemented in certain neighborhoods in Chicago. Our work expands upon this by analyzing a much more far reaching set of anti-predatory efforts across the nation. Other work has studied the impacts of some or all of the state anti-predatory lending laws that we consider - see generally, for example Keys et al. (2010); Nadauld and Sherlund (2013); Bostic et al. (2008); Ho and Pennington-Cross (2006). Yet, these previous studies have generally not distinguished in their data samples between loans affected and unaffected by the predatory lending laws. Perhaps unsurprisingly then, these previous efforts have frequently come to mixed or inconclusive results about the various impacts of state anti-predatory lending laws. By conducting an extensive legal analysis of state and federal predatory lending laws and demonstrating techniques for building a sample of loans specifically impacted by these laws, we provide an important resource for future researchers aiming either to study the impact of the laws themselves, or, as in our case, to use those laws (and the response of actors like the S&P agency to those laws) to study other important financial topics.

We organize the remainder of this paper as follows. Section 2 presents a detailed discussion of the rating change by the S&P agency and how it created a natural experiment we are able to exploit. Section 3 describes our data. Section 4 explains our empirical methodology, including a more detailed discussion of how our legal analysis critically informs that empirical methodology. Section 5 presents our results. We also conduct a number of robustness checks and discuss them in Section 6. Section 7 offers offers conclusions and policy implications.

2 The S&P Natural Experiment and its Legal Background

2.1 The S&P Rating Change

The analysis in this paper centers around changes in the rating policy of the S&P rating agency, which assigns credit ratings to bonds issued by securitization vehicles that own subprime mortgages. Around the period of this study, S&P was the dominant player in the business of rating residential mortgage backed securities, rating between 92% and 96% of all residential mortgage backed securities (Raiter, 2008, 2010). S&P's rating changes pertained to the treatment of certain loans affected by a number of state-level anti-predatory lending (APL)

laws which had been previously passed. These laws not only prohibited practices deemed as "predatory," they also established a system of "assignee liability" in which a securitization vehicle (or any other purchaser of mortgages) could be held liable for violations of the APL laws committed by loan originators. This created a new potential source of losses for securitization vehicles. S&P responded, over a period of several years and many announcements and updates to those announcements, by implementing policies for rating securities backed by loans affected by the various state APL laws.[11] The most significant of these rating changes, for the purposes of this study, occurred on May 13, 2004. These changes increased the costs of securitizing loans affected by the given APL laws, generally by requiring additional credit enhancement in order to cover the possible liability that could flow from them.

There are three key aspects of the APL laws and subsequent rating changes that make our analysis possible. First of all, the rating changes affected the costs of selling loans that had already been originated, but they did nothing to directly change the cost of originating loans nor the desirability of those loans to borrowers. Thus, any shifts in total subprime lending observed in response to the rating changes can be seen as direct consequences of the securitization channel being restricted while holding constant all other aspects of the primary market equilibrium matching borrower demand for loans with supply from loan originators.

Secondly, the S&P rating changes occurred some time after the APL laws were enacted. This allows for the possibility that it took time for a new equilibrium level of subprime lending to arise in the aftermath of the APL laws.[12] Because of the gap between the enactment of the laws and the S&P rating change, we are able to interpret changes in subprime lending levels following the rating change as being a direct result of the rating change rather than as lingering effects of the enactment of the laws.[13]

Finally, the S&P rating change affected certain states but not others and the selection of which states it impacted was independent of any existing trends in subprime lending levels within different states. When making its decision to enact its rating change, the S&P agency considered only the legal details of previously enacted state APL laws. It did not, therefore,

[11] In all, S&P has issued several dozen such announcements. Several of the most important for the purposes of this paper include Abrams (2003d,a,b); Coleman and Abrams (2003); Abrams (2003c, 2004b,d,c,a)

[12] An extensive literature already exists seeking to measure the equilibrium impact of the APL laws themselves. See, for example, Harvey and Nigro (2004); Ho and Pennington-Cross (2007); Bostic et al. (2008); Ding et al. (2011, 2012).

[13] Other literature that has looked directly at the impact of the APL laws themselves shows that both lenders and securitizers were very quick to respond to changes in the laws (Keys et al., 2012), further supporting our argument that the time between enactment of the laws in question and the S&P rating change was sufficient to allow a new "baseline" lending and securitization level to emerge prior to the new impact from the rating change.

consider factors such as its assessments of housing market trends, borrower quality, etc. in any of the affected states (Abrams, 2004d), factors which if present in the S&P's consideration would invalidate the analysis we wish to perform.[14]

The rating change therefore provides a good opportunity for a natural experiment to exploit via a difference in differences technique. Simply comparing levels of subprime lending in the affected states before and after the S&P rating change could not convincingly disentangle effects of the rating change from overall trends in the trajectory of subprime lending.[15] But, by making this before and after comparison for both affected and unaffected states and then by comparing these two differences to each other, we are able to isolate the causal effects of the S&P rating change.[16] Thus, throughout this paper, when we speak of changes in response to or following the S&P rating change, we are referring to changes in levels of subprime lending in affected states *relative to* changes in subprime lending in unaffected states.

In order to successfully exploit this natural experiment, however, we must dig deeper into understanding the S&P rating change, the state APL laws it responded to, and thus which loans the rating change affected and how.

2.2 Anti-predatory Lending Legislation in America

2.2.1 HOEPA and Federal Predatory Lending Legislation

During the late 1980s and early 1990s consumer advocates and legislators grew concerned about certain lending practices viewed as "predatory." Specifically, attention focused on what was termed "equity stripping," or the practice of repeatedly refinancing home mortgage loans, each time adding new fees, closing costs, and credit insurance premiums while offering cash advances to homeowners to entice them into accepting these ultimately detrimental transactions (Ding et al., 2011, p. 250). Advocates suggested that lenders would often obfuscate large fees, high rates, and other unfavorable terms so that homeowners would be unaware they were also becoming obligated to pay these when they refinanced their home mortgages and got the up-front cash payments.

In response to this, in 1994 the US Congress passed the Homeownership Equity Protection

[14] As discussed in more detail in Section 4, we also include a separate term in our regression analyses to capture and control for any linear time trend unique to the affected states. This provides an additional level of assurance that the assumptions of our model are met, most critically that there was no pre-existing downward trend in lending behavior in affected states, after controlling for all relevant observable factors, that would improperly be identified as reflecting the causal impact of the S&P rating change.

[15] For example, the housing market overall might have simply entered a dip during that time.

[16] See Section 4 for more details on this point.

Act (HOEPA).[17] Because the concern that led to the Act focused on equity-stripping refinance loans, HOEPA only applied to refinance and home equity loans and not to home purchase loans. Furthermore, HOEPA only applied to loans with interest rates that were 8% or 10% above prime lending rates (for first and junior lien mortgages, respectively) or with points and fees exceeding certain thresholds. For those loans that satisfied these requirements, HOEPA mandated clearer disclosures of rates and terms, banned some practices outright, such as certain types of balloon payments, and in some situations required a showing that a loan was in the economic interest of the homeowner. If these requirements were violated, borrowers could sue not just the original lender for damages, but also any party the originator had assigned (i.e. sold) the loan to. Thus was established the system of assignee liability within the context of anti-predatory lending law.

2.2.2 State Predatory Lending Legislation

Starting in 1999, states began enacting their own APL laws and regulations. Most, though not all, of these were modeled off of HOEPA. Some state laws exactly mirrored HOEPA. Their intended effect appears to be to prevent cities and counties within those states from passing their own, more restrictive APL ordinances, since the state laws would preempt local ordinances (Ding et al., 2011). Thus, paradoxically, the purpose of at least some state APL laws was to *decrease* regulation of predatory lending, rather than to *increase* it.

Apart from those states that mirrored HOEPA, state APL laws generally sought to expand upon the coverage of HOEPA. They did this in up to four primary ways. First, some states included more types of loans, notably purchase loans, in the coverage of their laws. Secondly, many states lowered the interest rate and fee thresholds for loans that their laws covered. Thirdly, many states increased the requirements or added to the prohibitions for covered loans. Finally, some states increased the penalties (including the potential liability of lenders and assignees) for violations of the laws.[18]

[17] HOPEA was part of the Riegle Community Development And Regulatory Improvement Act Of 1994, PL 103–325, September 23, 1994, 108 Stat 2160.

[18] One further complication is that even within states that passed new APL laws, not all lenders were equally impacted. Approximately 20% of loans made during this time were originated by lenders regulated by either the Office of Thrift Supervision (OTS) or the Office of the Comptroller of the Currency (OCC). These two federal regulators, but not the federal regulators for other lenders, used their preemptive power to exempt entities they regulated from the state anti-predatory lending legislation. Nevertheless, the preempting actions of both of these regulators came well before the S&P rating change and we see no evidence in our data to suggest movement of lending activity towards lenders covered by these preemptions in response to the S&P change. Thus, since the rating changes were relevant for the vast majority of loans originated in the affected states, they still constitute a meaningful impact on the securitization market for loans from those states. Section 5 begins with empirical tests that specifically validate this assertion.

2.2.3 The S&P Response: Additional Credit Enhancement

Starting in January of 2003, S&P began announcing changes to its rating policies for securities containing loans that state APL laws impacted. These initial announcements affected only a handful of states. In May of 2004, in response to the growing number of state APL laws and the growing attention paid to them, S&P issued a set of three simultaneous announcements laying out criteria for dealing with loans affected by APL laws in more than twenty different states (Abrams, 2004d,c,e). In many of these states, this was the first time S&P had enforced specific criteria for their laws, while for others, the new criteria superseded old. For about 15 states, S&P began requiring some form of credit enhancement from sellers who sold loans affected by the APL laws to securitization vehicles (Abrams, 2004d, p. 2). This credit enhancement required sellers to commit additional resources in order to cover potential losses to the securitization vehicle if it were held liable for APL violations that loan originators committed.[19]

Of all of the S&P APL announcements, this May 2004 set is by far the most significant. This is both because of the large number of states that it covered, many of which for the first time, and because in many of those cases, the initiation of the additional credit enhancement (ACE) came some time after the implementation of the state law. Following the May 2004 announcement, S&P continued to issue new announcements and to update its criteria for loans affected by state APL laws. Most of these later announcements were in response to further states passing or amending their APL laws. After May 2004, however, S&P began to revise its rating criteria as soon as state law changed, ending the lags between legal change and S&P policy change that provided such a useful tool in developing the methodology of this paper.

Of the states mentioned in the S&P announcements, six are directly useful for our analysis in that the S&P announcements represented a unique increase in the cost of securitizing loans from these states as compared to other states. These "treatment" states are Arkansas, Connecticut, Georgia, Illinois, New Mexico and New York. We exclude several of the states mentioned in the announcements from our study completely for reasons such as that they passed additional anti predatory lending laws or modifications to their laws after the May 2004 S&P announcement. Other states are mentioned in the announcements but do not extend their coverage beyond HOEPA. We use these states as controls since the S&P announcement would not have impacted them in any way different from its impact on the rest of the states in the country.

[19] Many of the S&P announcements prior to May 2004 simply required lenders to verify compliance with the state APL laws and to offer to repurchase non-complying loans. Thus, by adding a requirement of additional credit enhancement in May of 2004, S&P increased the strictness of its requirements, even for some of the states it had addressed in earlier announcements.

Table 1 and Section 3.3 give more details on our classification of states mentioned in the S&P announcements.[20]

3 Data

3.1 CoreLogic Mortgage Data

Our analyses in this study primarily use mortgage data licensed from CoreLogic. We estimate that this database covers approximately 70% of total mortgage originations in the United States during the period of our study.[21] After restricting the data to the specific time and geographic reach of our study, we are left with 5.9 million loans in total (3.1 million purchase loans and 2.8 million refinance loans). The CoreLogic data contains information available upon the origination of a mortgage such as the FICO score of the borrower, the initial interest rate, the type of loan (purchase, refinance, etc.) and many other characteristics. Furthermore, the data contains monthly updates on a loan's performance, indicating whether a loan is current on its payments, in delinquency, in foreclosure, etc. We present summary statistics on the CoreLogic data we use in this study in Table 2.

While there are a relatively large number of subprime mortgage databases that give monthly loan performance data similar to that found in CoreLogic, the CoreLogic database is relatively unique in covering both subprime mortgages that are securitized as well as those that are held on the books of financial institutions.[22] This is crucial to our research question since we are investigating the impact that changes in the costs of securitizing subprime loans had on the total number of subprime loans (both securitized and non-securitized) that are originated. Another important advantage that the CoreLogic data we use has is that it identifies the month that each loan is originated, rather than just the year, as in some other mortgage databases. This allows us to precisely compare mortgage lending conditions just before and just after the

[20] S&P also required additional credit enhancement for loans affected by HOEPA, which impacted loans regardless of their state. Thus, it is not the case that the S&P rating change had no impact on lending in our "control" states. Rather, the S&P rating change equally impacted all 50 states with regards to the HOEPA eligible loans made in those states. But, the S&P rating change had a unique impact on our treatment states because it pertained to broader swaths of loans in those states than in the rest of the country. It is this unique impact of the S&P rating change on the treatment states that we measure and use in our analyses.

[21] Estimating this coverage percentage for the CoreLogic data is challenging. As a rough approximation, we note that prior work has estimated that another mortgage database, the Home Mortgage Disclosure Act (HMDA) data, covers approximately 90% of all US mortgages (Williams, 2015). Using this estimate as a starting point, we divide the number of loans in the CoreLogic data in a given year by the number of loans in the HMDA data in a given year and then multiply by 0.9. Doing this gives approximate coverage percentages of 70%, 73% and 69% for years 2004, 2005 and 2006.

[22] Much of CoreLogic's data was formerly marketed by LoanPerformance, which CoreLogic has now purchased.

rating change and gives us a significant sample size of observations following the rating change with which to measure its continuing effects.

Starting from the raw loan-level data, we aggregate numbers of subprime mortgage originations by month and geographic location. The specific geographic division that we use is known as a "Metropolitan Statistical Area" or MSA. MSAs are defined by the federal government's Office of Management and Budget. Each MSA has at its center an urban area of at least 50,000 people. MSAs also include adjacent areas that are socioeconomically tied to the urban center. There are 388 MSAs defined in the United States. Because we exclude certain states from our analysis (as described in detail below and in Table 1) our analysis includes a total of 332 MSAs.[23] Our data set thus consists of a balanced panel with each observation representing the number of a particular type of loan made in a particular MSA in a particular month. There are roughly 13,000 such MSA-month combinations within our data.

One challenge of the CoreLogic data is that while it contains substantial numbers of mortgages that are privately securitized, held on books, and owned or securitized by GSEs, only 20% or so of the loans in the data contain information identifying which of these types of institutions hold the loan. Thus, when we perform analyses looking just at privately securitized loans, or just at loans held on books, it is from a reduced pool of total mortgages that we are drawing.

While somewhat disappointing, we do not consider this to be a significant problem for several reasons. First of all, our most essential analyses involve looking at the mortgage market as a whole - we are investigating the impact on the *total* mortgage market of changes in the securitization market. Thus, for these analyses, the issue of only 20% of the loans being classified by holder is irrelevant because we are looking at all loans regardless of holder. Secondly, because the data set is so large, even when we restrict it to the 20% of loans for which the holder is identified, we are still left with hundreds of thousands of loans and thus generally plenty for our analyses (Table 2 gives details on numbers of loans identified). Finally, provided that there is no systematic bias in determining which loans CoreLogic has holder information for, or, as a backup, provided that any biases that do exist are uncorrelated with the dynamics we are studying, the fact that our analyses draw from a subset of the total loans should not be a problem.

[23] A number of MSAs, such as Kansas City, span state boundaries. We treat the portions in each separate state as separate units of observation since we are able to count how many loans are originated on either side of the state line and because separate demographic statistics are available for the portions of the MSAs on either side of the state lines.

Nevertheless, we are fortunate to have access to multiple mortgage data sets and therefore take advantage of that to perform a robustness check on our results. Specifically, we also use the Columbia Collateral data set, which comprises approximately 8 million privately securitized loans for which Wells Fargo serves as the trust agent of the securitization vehicle. Although the Columbia Collateral data is smaller and less detailed in some respects than the CoreLogic data, it has the advantage of being comprised of 100% privately securitized loans, meaning that we encounter no issues with it regarding missing data classifications. As we discuss in Section 6 below on robustness checks, we re-run all of our analyses pertaining just to privately securitized mortgages on this other data set and the results very closely match those obtained with the CoreLogic data.

3.2 Timeframe for Study

We restrict the time period of this study to January 2004 though December 2006. Several of the states we study passed their APL laws in 2003. In order to ensure that we are measuring the impact of the S&P rating change and not of the state laws themselves, therefore, we begin our analysis in 2004. Starting in 2007, as evident in Figure 5, subprime lending activity declines precipitously, clearly due in large part to the collapse of the housing bubble in the United States and the onset of the global financial crisis. This paper aims to better elucidate the factors that contributed to the creation of the bubble, and thus the end of 2006 is the most natural time to conclude our study period.[24]

3.3 Categorizing Loans: High Cost, Purchase, and Refinance

As we describe in Section 2, the S&P rating change impacted different types of loans differently. Therefore, we divide our data into separate panels measuring the numbers of loans of different types that are issued. One key distinction that we draw is between loans designated as "high cost" (a technical legal term) and those that are not. As already mentioned, the federal HOEPA statute applied to loans with interest rates or fees above a certain threshold and many pieces of state legislation lowered these thresholds. Thus, it is natural to expect that the S&P rating change, instituted to directly respond to liability risk from loans impacted by state APL

[24]Nevertheless, as discussed in Section 6, changing the timeframe of the study has little impact on the results or conclusions presented here.

laws, will most significantly impact the "high cost" loans those statutes applied to.[25]

Because our data includes information on both the interest rate and the lien seniority of the mortgage loans, we are able to precisely identify many of the high cost loans that are directly impacted by the state APL laws and thus also the S&P rating change. Specifically, we identify loans in our data as "high cost" if they have interest rates exceeding the prime rate by 6% for first lien loans or by 8% for junior lien loans, a definition which closely follows the state APL laws in question.[26] At the same time, because our data does not contain information on points and fees that are assessed on loans, there is also a set of high cost loans that we do not directly identify.[27] Nevertheless, our ability to focus our analyses on subsets of loans that we know do qualify as high cost is crucial to being able to document and understand the impact of the S&P rating change. Our results and accompanying discussion in Section 5 below make this point more concrete.

We also build separate panels that distinguish between loans loans originated in order to purchase a house, "purchase loans," and loans issued in order to refinance a house that a homeowner already owns, "refinance loans." This is important for identifying which loans the S&P rating change will likely affect and how. While many state laws covered purchase loans, others only covered refinance loans. Clearly, it would make little sense to look for changes in purchase loan activity prompted by the S&P rating change in a state whose law did not impact purchase loans.

The distinctions we draw between purchase and refinance loans are important even for states whose laws affected both. Since HOEPA already covered some refinance loans, even if a state lowered its interest rate and fee thresholds to cover more refinance loans than HOEPA, the impact of the state's law (and thus also of the S&P rating change implemented in response to that law) on refinance loans overall is apt to be less than for purchase loans. This is because such state laws merely expanded upon existing coverage of refinance loans by HOEPA, whereas the laws introduced coverage where previously none existed for purchase loans. As we discuss

[25] It is still plausible that the rating change could have some impact on lending activity for loans not designated as high cost. Lenders might, for example, reduce overall lending operations in certain geographic areas in response to the rating change, because the added restrictions reduced the overall volumes of lending needed to be profitable or required cost-prohibitive additional screening measures. Conversely, lenders might conceivably expand activity for loans with terms just below the thresholds. Each effect might indeed be present to some extent simultaneously. We address some issues of this type in Section 5.

[26] Each state APL law is different, meaning that if we seek to use a common definition across multiple states - something important for the consistency of our analysis - we will not be able to match our definition precisely to each individual state. Nevertheless, our definition fits closely to those in the majority of the states, giving us confidence that we successfully identify loans impacted by the APL laws. Our results in Section 5 bear this out in further detail.

[27] Thus, for example, the total ratio of high cost loans to all loans is higher than would be suggested in Table 2.

in Section 5, this ability to differentiate our analysis of loans based on which we anticipate being more heavily impacted by the S&P rating change is crucial to the success of our empirical strategy.

3.4 Legal Analysis and Loan Categorization

The discussion above highlights both the importance and the complexity of the legal analysis necessary to craft a successful empirical strategy in this study. Because each state APL law is different, our analysis requires a careful and individual review of each law in order to understand its impact. Furthermore, it is only once we consider the laws within the context of a complex web of federal laws and regulations that we are able to assess their real effects.

In some of the state laws we analyze, we are able to identify explicit provisions that make their coverage clear. Arkansas' APL law, for instance, states directly that it excludes "loan(s) made for the purchase of a one-family to four-family residential structure" (Ark. Code Ann. § 23-53-103). For other state laws, however, we must search through the the entire law's text to identify the *absence* of provisions. Thus, Connecticut's law, for example, which *does* cover purchase loans, never explicitly mentions purchase loans, it simply omits any express exclusions of them. By comparing Connecticut's law to Arkansas' and noting the ways they differ, we are able to get a clearer understanding of the effect of each one.

At times, this investigation requires us not just to analyze a given law or regulation, but other completely different legal documents whose provisions impact those of the specific text we are investigating. To interpret the federal HOEPA regulations, for instance, we start by examining 12 C.F.R. § 226.32, the primary regulation that defines the coverage of HOEPA. The language of this regulation is very similar to that in Connecticut's statute and makes no explicit references to purchase loans. This might suggest that HOEPA, like Connecticut's APL, covers purchase loans. One provision of 12 C.F.R. § 226.32 does mention an exclusion of "residential mortgage transactions," but it gives no direct indication of what is included in this term or suggestion that it might mean purchase (but not refinance) loans. We continue our analysis, however, by reviewing other key legal documents associated with HOEPA, including 15 U.S.C.A. § 1602., which specifies that for the purposes of the statue a "residential mortgage transaction" includes, amongst other things, home purchase loans. By reviewing many different sections of different federal texts governing HOEPA, therefore, we are able to put these two disparate provisions together in order to accurately see that HOEPA covers refinance but not purchase loans.

Another issue we confront is the frequency with which laws and regulations change. Thus, when examining HOEPA, we consult archived versions of the laws and regulations that were effective during the time of our study. Current versions of these documents would lead to an erroneous result because HOEPA, as part of Dodd-Frank, was expanded to include purchase loans which it had previously excluded. Our analysis doesn't stop, however, at just considering the versions of the laws that were effective at the time our study began. We also review subsequent changes to those laws that occurred during our study period. Massachusetts, for instance, passed a new APL law in August 2004, after the main S&P rating change we study but still during our study period. Thus, by reviewing not just the laws in effect at the beginning of our study, but all changes to those laws during the time of our study, we are able to know to exclude Massachusetts from our consideration because it would not be possible to disentangle the impact of the S&P rating change from the impact of their new APL law.

We also analyze each law and regulation in the context of laws and regulations from other jurisdictions in order to understand their interrelationships and impacts upon one another. Thus, for instance, by considering state laws in the context of HOPEA, we are able to identify that the state laws, and thus the S&P rating change, are likely to have a lesser impact on refinance loans than purchase loans, since refinance loans were already partially covered by HOEPA. The interpretations of some state laws are particularly complex in this respect. Our analysis of Colorado's APL statute, for example, includes not just the basic provisions which define high cost loans and the restrictions placed on them, but rather a full analysis of every provision of the law. This is essential because the Colorado statute contains a provision, buried deep in the law and far from the main text defining affected loans and restrictions, which states:

> Any provision of this article preempted by federal law with respect to a national bank or federal savings association shall also, to the same extent, not apply to ... a bank chartered under the laws of Colorado or any operating subsidiary of such a state chartered bank. Colo. Rev. Stat. Ann. § 5-3.5-303.

In order to interpret this provision of the Colorado's law and its reference to the legal term "national bank," we refer to the National Bank Act (and subsequent statutes) which establish the so-called "dual banking system" whereby banks can be regulated either by state or federal regulators. We then turn to the regulatory actions of the Office of the Comptroller of the Currency (OCC) in order to find that it had announced a preemption of the type conceived of in the Colorado Statute in 2003 (OCC, 2003). Thus, only by exhaustively reviewing the disparate

provisions of the Colorado statute, and then comparing them to multiple other pieces of federal law and regulation, are we able to identify that despite having an APL law on its books, the law practically speaking had very little effect and thus Colorado should not be considered a "treatment state" in our analysis.

Each one of these steps in our legal analysis is essential to the ultimate conclusions we reach.[28] Missing even one of these crucial legal issues could easily lead to an erroneous understanding of which loans would be affected and how by the S&P rating change and thus an invalid and statistically meaningless empirical strategy. Furthermore, this discussion is by no means a comprehensive summary of the legal issues we confront in identifying which loans would be impacted and how by the S&P rating change. Instead, it is simply meant to convey the complexity of analysis that an empirical strategy based on measuring the impact of legal changes can require. Nevertheless, while there are many crucial legal judgments that we must draw in this analysis, none of these are subjective judgments.[29] There is a definite answer to which loans were impacted by the S&P rating change, it is simply the case that we frequently must go through a long series of steps to get to that answer. We present the results of our legal analysis and its implications for which types of loans in which states were affected by the S&P rating change in Table 1.

4 Empirical Methodology

In this study, we use negative binomial panel regressions with random effects, as developed by Hausman et al. (1984), to model the numbers of mortgages made in each MSA each month. Specifically, we let N_{it} represent the number of mortgages made per MSA-month observation and model N_{it} as a negative binomial random variable with shape and scale parameters γ_{it}, δ_i, respectively. Here $\gamma_{it} = e^{\mathbf{x}'_{it}\beta}$ where $\mathbf{x}'_{it}$ represents the vector of explanatory variables for a given MSA-month observation and β is the vector of parameters to be fit in our model. For its part the scale parameter, δ_i, which is a time invariant random effect unique to each MSA, is described by modeling the ratio $\delta_i/(1+\delta_i)$ as a beta random variable. Given this, the log-likelihood function for all of the MSA-month observations N_{it} can be constructed based on the

[28] The S&P Announcements do contain some rudimentary discussion of legal issues and classification of states by impact. A tempting shortcut therefore might be to simply "outsource" our legal analysis by relying on these documents others have created. Yet, the announcements cover almost none of these crucial details we discuss in this section. This is not by any error on the part of S&P. S&P never purports to present a full analysis in any of these documents, instead noting that it will conduct its own more thorough investigations as the issues arise with particular rated securities and particular loans in those securities (Abrams, 2004d).

[29] This is contrary to many other areas of legal analysis, which often do require judgments that are to some degree subjective.

densities of the random variables described in the model above. Maximizing this log-likelihood then yields the vector of fitted estimates for the predictive variables in our model.[30]

This model has several important advantages for this study. First, negative binomial regressions model count data, which is exactly what these mortgage counts represent. Secondly, the distribution of numbers of mortgages per MSA follows a roughly log-normal distribution (Figure 6). In a linear regression, this would generally require a logarithmic transformation of the response variable. In this case, that transformation would be highly problematic because in some months and MSAs there were zero mortgages of certain specific types originated, taking the response outside the support of the transformation. Yet, because in a negative binomial regression, the covariates enter the expression for the conditional mean of the response through an exponential link, such a transformation is unnecessary. Thirdly, the negative binomial regression is able to handle data where there is a cutoff value, in this case zero, below which there can be no observations. This produces a clustering of observations at zero that is uncharacteristic of continuous distributions. Finally, our data on loan counts is "overdispersed," meaning that the variance of observations exceeds their conditional mean. Given this, the negative binomial regression is a more efficient and effective estimator than certain other count models would be, such as a Poisson regression.

In the context of this regression framework we employ an identification strategy inspired by the popular difference in differences methodology. Because, however, our regressions no longer model a linear relationship between response and predictors, we use a slightly adapted "ratio of ratios" technique.[31] In the regression context (linear or otherwise), the difference in differences methodology is implemented by including an indicator that equals one for MSAs in states affected by the S&P rating change and months following the change and zero elsewhere. If such an indicator variable (in position j) switches from zero to one, while holding all other variables constant, the ratio comparing expected numbers of loans in a given MSA where the indicator equals one to the expected number of loans in an MSA where the indicator equals zero would be given by e^{β_j}. Thus, the coefficients we present in our results section can be interpreted by exponentiating them, after which they represent the proportional change in expected numbers of loans per MSA-month. A parameter value of -0.4 for an indicator, for instance, means that the indicator signifies a reduction of lending by approximately 33% since $\exp(-0.4) \approx 0.67$.

[30] For more details, see Cameron and Trivedi (2013).
[31] See, e.g. Winkelmann 2008, p.71.

In addition to standard demographic controls,[32] our negative binomial panel regressions provide for random, time invariant effects at the MSA level (the δ_i term discussed above). These geographic random effects are able to capture any unique, time-invariant aspects of the economic situation in an area that affect mortgage lending, but which are not fully reflected in observable economic controls. Thus, for example, some MSAs may happen to have a historically high concentration of lenders or a regional economy in which workers frequently change jobs and thus are often moving, buying new houses, and taking on new mortgages. Such factors could easily affect the numbers of loans made in that region but not be directly or at least fully reflected in other observable economic controls. To the extent that these unobservable factors are constant over the three years of our study, they will be picked up by and thus controlled for in the MSA-level random effects.

Our regressions also employ fixed effects for origination month to capture and control for any national trends in lending behavior that are common across MSAs. Despite these efforts to include robust controls, it is conceivable that the states in our treatment group could have been on a downward trend in subprime lending activity prior to the S&P rating change, even after controlling for all of the observable factors (such as population, unemployment, etc.) that we use in our regressions. Accordingly, we also include a separate variable in our regressions defined as S&P_State * Time. S&P_State is an indicator for states that eventually will be affected the S&P rating change, but is equal to 1 for all such states during all periods of our study. Time measures months since January 2004 when our study began. This allows the states affected by the S&P rating change to have a separate linear time trend as compared to those states in the control group. Therefore, even if the states affected by the rating change were already on a downward trajectory in lending activity before the S&P rating change took effect[33] this term in our regressions will control for that.[34]

It is impossible to design an empirical strategy that does not rest upon some untestable assumptions. In our case, to invalidate our strategy would require that there were some factor other than the S&P rating change that: (1) reduced subprime lending in states targeted by the

[32] See Table 3 for a full description of these controls, which include factors such as MSA-level population, income, unemployment, etc. One control that we omit is house prices. Although house prices undoubtedly impact mortgage availability, mortgage availability also undoubtedly impacts house prices, making the two variables jointly determined. Without, therefore, either a way to instrument for house prices or to develop a simultaneous equation model, inclusion of this factor would result in an endogeneity issue with our analyses.

[33] Such a trajectory could perhaps be due to the original APL laws producing a gradual reduction in lending activity in affected states, rather than a quicker adjustment between their passage and the enactment of the S&P rating change.

[34] See Besley and Burgess (2004) for further discussion of this kind of robustness technique.

S&P rating change but not others,[35] (2) caused this reduction in lending just for the high cost loans impacted by the S&P rating change but not others and (3) disproportionately impacted purchase loans over refinance loans, just as the S&P rating change did. Furthermore, this other factor must have: (4) not been constant throughout the period of our study but instead have started just about the time the S&P rating change went into effect[36] and (5) this factor must have been uncorrelated with any of our observable explanatory variables such as population, income, unemployment, etc. that would otherwise control for its influence. There is no way to conclusively prove that such a factor did not exist. Yet, the existence of such an unaccounted for factor that meets all five of these very specific requirements strikes us as highly implausible to say the least. Barring revelation of such a factor meeting all of these criteria that has escaped our analysis, therefore, we feel confident in drawing causal conclusions from the results of our empirical tests.

Lastly, as with any regression analysis, our empirical strategy depends upon standard regression assumptions, such as the independence of observations after controlling for all of the factors described above. One particular concern in panel regression settings such as this is that there may be temporal correlation between the errors for a given MSA, even after controlling for observable factors and incorporating the MSA-level random effect. Such correlation of errors would cause us to over-estimate the precision of our parameter estimates by essentially treating two correlated observations as if they were independent and thereby inflating the effective number of data points we have available. This has been shown to be a particular issue for difference in differences panel techniques such as we use in this paper.[37] Therefore, all of our regressions use block bootstrapped standard errors of the type Bertrand et al. (2004) suggest. Such block bootstrapped errors, which we group at the MSA-level, have been shown to be effective at addressing concerns with the assumption of conditionally independent observations in panel regressions.[38]

[35] This rules out any kind of national or likely even regional trends in lending patterns, given that the states affected by the S&P announcement are spread across the United States and represent a wide variety of sizes, economies, and so forth.

[36] Otherwise, this factor would be controlled for by our time invariant MSA-level effects or our unique linear time trend for states affected by the S&P rating change.

[37] See Bertrand et al. (2004).

[38] Bertrand et al. (2004) demonstrate that difference in differences methodology can be susceptible to a high rate of type I errors, even when standard clustered robust standard errors are used. They demonstrate using simulation studies, however, that block bootstrapped standard errors can rectify this problem, although the success is highly dependent upon the number of "individuals" in the panel being sufficiently high. Bertrand et al. (2004) specifically note that with fifty individuals in the study (in their case states) the results of the block bootstrap are very good, with the null hypothesis rejected at 95% confidence only 6.5% of the time, whereas simulations using fewer individuals achieve less satisfactory results. Since our study uses MSAs as our units of individual measure, of which there are over 300 in our data, we have a high degree of confidence in the robustness of our tests.

In order to assess the goodness of fit of our models, we use two of the measures that Cameron and Trivedi (2013) suggest as alternatives to the traditional R^2 of linear regression which is not applicable to a model such as the negative binomial one we employ. The first of these measures, which we term R^2_C (R^2 - correlation) simply represents the squared correlation between the predicted and observed numbers of loans in each MSA-Month. The second measure, R^2_M (McFadden's Pseudo-R^2, also sometimes called the "likelihood ratio index") measures the improvement in the log-likelihood of our fitted model (LL_1) compared to a model with an intercept only (LL_0). It is defined as $(LL_0 - LL_1)/LL_0$. Both measures have shortcomings compared to the relatively desirable properties of the R^2 in linear regression, but they nonetheless give a rough indication of goodness of fit. Our R^2_C values average around 0.48, indicating a correlation of 0.69 between predicted values and actual observations and thus represent a good model fit. Likewise, our R^2_M - values average around 0.13. There is no agreed upon interpretation of values for McFadden's R^2, apart from the fact that very low values may indicate a poor model fit (Hilbe, 2011), which is not the case for the values of R^2_M that we observe.

5 Results

5.1 The Impact of the S&P Rating Change on Subprime Securitization

As a threshold question we investigate whether the S&P rating change significantly affected the subprime securitization market. If there were no evidence that the rating change affected the securitized subset of the subprime market, then it would not be a useful tool to study the relationship between securitization and total volume of subprime lending. We thus begin by considering all loans in the data set marked as subprime and sold to a private securitization vehicle. Table 3 presents the results of our analysis for these loans. The crucial feature in this and all of our subsequent regression tables is the sign and significance of the variable "S&P Rating Change - in Effect." This is the indicator that implements our differences in differences technique. Specifically, it is set to one in states impacted by the S&P rating change, in months following the rating change, and zero elsewhere. Given our assumptions as described in Section 4, it represents the causal impact of the S&P rating change; in other words, the causal impact of increasing the cost of privately securitizing subprime loans while holding all other relevant factors constant.

The negative and statistically significant effect that we measure for the S&P indicator when examining purchase loans in Table 3 gives solid evidence to support the hypothesis that the rating change had a significant impact on reducing the number of subprime mortgage securitizations. The point estimate for this effect suggests an approximately 27% decrease in securitization volume. The effect for refinance loans is smaller and less significant than it is for purchase loans. This matches predictions from our legal analysis that suggested the S&P rating change would likely impact these loans less than purchase loans.

If the results that we are observing are indeed attributable to the S&P rating change, we should expect to see an amplification of the effect when we focus our analysis on a subset of loans more likely to be influenced by the rating change. Thus, we next restrict our analysis to the "high cost" loans discussed in Section 3.3. Table 4 presents the results from this analysis. The magnitude and statistical significance for the S&P indicator increases substantially in this table for both purchase and refinance loans, indicating an estimated reduction of 52% in numbers of high cost purchase loans securitized in the affected states following the S&P rating change. These results confirm our predictions precisely, giving us additional confidence that the S&P rating change did indeed have a meaningful impact on the subprime securitization market and that we are observing the effects of it in these regressions.

5.2 The Impact of the S&P Rating Change on Total Subprime Lending

The ultimate goal of this analysis is not simply to document whether the S&P rating change impacted the securitization market. Rather, we seek to use that impact as a tool to determine if an increase in the cost of securitizing loans led to a reduction in total loans being originated. We proceed therefore to consider all subprime loans in the data set, this time combining those sold to private securitizations, those held on books, and those sold to GSEs. Table 5 presents the results of this analysis. The S&P indicator for purchase loans in this table is now essentially zero in both magnitude and statistical significance.[39] At first then, it would appear that while the S&P rating change reduced the number of subprime loans being securitized, it had zero or close to zero effect on the total number of subprime loans being made.

Yet, upon closer examination, we see that there is in fact evidence showing that the S&P

[39]Table 5 does show a small and positive effect for refinance loans. It is possible that this reflects some movement of lending away from purchase loans and towards refinance loans which, as we've discussed, were not as heavily affected by the S&P rating change. It also seems plausible to us that this may simply be an anomalous result - its magnitude, for instance, is far smaller than any of the other significant results we measure and present in this section. Given the total number of regressions we run and present here, a small number of anomalous, but statistically significant results are to be expected.

rating change reduced subprime loan originations and not just subprime securitization. This evidence is only manifest when we focus more precisely on those loans most likely to be impacted, namely, the "high cost" loans analyzed above. We present these results in Table 6. Now, we again see a significantly negative parameter estimate for the S&P indicator, suggesting an approximately 37% decrease in loan originations (rather than just loan securitizations) for high cost subprime purchase loans.

Interestingly also, there is a significant effect only for the purchase loans (which we hypothesized would be the loans most affected by the rating change). In order to uncover this evidence of the impact of the S&P rating change on loan originations, therefore, we needed both to identify the disparate effects of the rating change on purchase vs. refinance loans as well as on high cost vs. regular loans. Missing either point would have led to an entirely different conclusion.[40]

Figure 7 provides a partial visualization of these regression results. It calculates monthly lending volume for high cost loans in states affected and unaffected by the S&P rating change. As expected, the left panel of the figure, which plots high cost purchase loan volume, shows a clear reduction in lending in affected states following the S&P rating change. The right panel, which plots high cost refinance loan volume, shows no such clear trend, a result consistent with both our legal analyses as well as our regression results reported in this section.

Overall, we see from the results presented here that while the S&P rating change resulted in a large and significant reduction in the numbers of high cost subprime loans being securitized, the change in total number of high cost subprime loans being originated was noticeably smaller and in the case of refinance loans statistically indistinguishable from zero. Given that securitized subprime loans are a subset of the total subprime market, some reduction in effect size is to be expected when moving from an analysis of securitized subprime loans to all subprime loans. Yet, the size of the discrepancy between the measured effects for securitized subprime loans vs. all subprime loans at least suggests the possibility that there may be some partial displacement occurring, with some subprime loans that would have been securitized instead moving to be held on the books of financial institutions following the rating change.[41]

[40] Not surprisingly, when we conduct the analyses presented in Table 6 by pooling purchase and refinance loans together into an aggregate measure of lending it yields no significant results for the S&P rating change, meaning that an analysis that failed to distinguish between loan types would have led to an erroneous conclusion. For brevity these results are omitted.

[41] It is unfortunately impossible for us to precisely estimate the size of the "expected" discrepancy between the measured effects for subprime securitized vs. all subprime loans because we do not know with precision the percentage of our subprime data set that is securitized. Almost certainly the percentage is greater than the overall market statistics indicating approximately 75% of subprime loans being securitized and depicted in Figure 2. This is because data on subprime securitized loans is much more readily available than for non-securitized subprime

In order to investigate this further, we turn our attention just to loans held on books. Table 7 gives some mild support for the possibility that more subprime loans were held on books of institutions in response to the S&P rating change. Specifically, Table 7 shows positive parameter estimates for the S&P indicator (rather than negative as in all previous analyses), though the magnitude of the estimates is relatively small and in the case of purchase loans not statistically significant.

As with before then, we focus on those "high cost" loans most prone to be affected. We present the results from that analysis in Table 8. Again, focusing on high cost loans increases the estimated size of the impact of the S&P rating change, with the parameter estimates for the S&P indicator increasing noticeably as compared to Table 7. Despite this increase in magnitude, the precision of the results remains outside the range of statistical significance. A challenge with these analyses is the relatively small pool of loans we have to draw on that are identified as both held on books and high cost. As discussed in Section 3.1, only about 20% of the loans in our data set identify the current owner, and since only a fraction of these are held on books and only a fraction of those are high cost, the precision of our results for this analysis is limited. In reality, there are many more high cost subprime loans held on books, including in our sample, we simply don't have detailed and comprehensive enough data to identify them all.

Nevertheless, because we observe a much larger negative impact from the S&P rating change on subprime securitized loans as compared to all subprime loans, and based on the suggestive results in Table 7 and Table 8, we conclude that there is some evidence to support the hypothesis that some subprime loans moved on books rather than being securitized as a result of the S&P rating change, but this evidence is not fully conclusive.

6 Robustness Checks

In addition to the results we present above, we also perform several tests to verify that our findings do not depend upon the precise ways in which we assemble our data or formulate our models. In every case the results of these tests strongly confirm the robustness of the findings

loans, since mortgage securitization vehicles generally received the right to obtain regular updates on the performance of the mortgages they owned, therefore conveniently aggregating this information in an accessible location. Thus, it is likely that all or nearly all subprime securitized loans are represented in our data, whereas only a portion of subprime non-securitized loans are. Neither we nor CoreLogic, however, have the information necessary to calculate the exact composition. We are unable therefore to rule out the possibility that the discrepancy in measured effects for the S&P rating change between Table 3 and Table 5 and between Table 4 and Table 6 is driven mostly or totally by the simple fact that subprime securitized loans are a subset of the total subprime market. Our results in this respect are suggestive rather than conclusive, a fact born out by the regression results in Table 7 and Table 8.

we present above. For the purposes of brevity we omit the specific results of these tests from this paper but all are available upon request.

As discussed in Section 3.1, a challenge of the CoreLogic data is that it only identifies the owner of approximately 20% of loans, meaning that our analyses specifically on privately securitized loans and loans held on books are based on reduced segments of the data.[42] As a robustness check, therefore, we also perform our analyses for privately securitized loans using the Columbia Collateral data set which consists exclusively of privately securitized loans and thus is not subject to the loan identification limitations of CoreLogic. In particular, we use it to run the same tests we present in Table 3 and Table 4.

The results are nearly identical in every essential respect. Specifically, these results confirm our conclusions that: (a) the S&P rating change was a significant event in the subprime securitization markets for the affected states; (b) high cost loans were the primary loans impacted by the change; and (c) purchase loans were impacted substantially more than refinance loans. Thus, we have confirmation, using a completely different set of data,[43] of many of the findings essential to the overall conclusions of this paper. Doing this considerably boosts our confidence in the robustness of our results.

Another challenging aspect of pursuing a research topic such as ours is the ambiguous nature of the term "subprime." No standard definition exists and thus the term frequently ends up taking on different meanings in different settings, both within the mortgage marketplace as well as in academic literature. All of the results we present above are based on a designation of loans as prime vs. subprime that is made by our data provider, CoreLogic. These designations are informed in part by loan categorizations made by the originators and servicers who provide the mortgage data to CoreLogic. This approach appeals to us because it reflects the way that these loans are viewed by the key economic actors whose decisions are being influenced by the policy changes we study. It is also very common in the literature.[44]

Nevertheless, in order to verify that our results do not depend on this particular judgment we make, we consider three other definitions of subprime loans based on loan characteristics rather than issuer/servicer designations. The first definition encompasses all loans made to borrowers with a FICO score that is less than or equal to 640 as well as loans that are marked as either low documentation or no documentation and loans that have a negative amortization

[42] Yet, as shown in Table 2, our analyses in these cases still incorporate hundreds of thousands of mortgage loans.
[43] It is nevertheless likely that there is some amount of overlap between loans in each data set.
[44] See, e.g. Keys et al. (2010); Arentsen et al. (2015); Agarwal et al. (2012); Krainer and Laderman (2014)

structure. The second and third definitions repeat the first but with FICO score cutoffs at 660 and 680 respectively.[45] The results of our negative binomial panel regressions using these other definitions are nearly identical in every essential respect. The results for the logistic regressions using these alternative definitions also closely match those for our main analyses, though the significance levels for parameters fluctuate up and down somewhat more than for the negative binomial results. Overall, we consider the conformity of results across different subprime definitions to be unsurprising. The core of our analysis in this paper centers around the "high cost" loans that the S&P rating change most impacted. Regardless of how one defines subprime, high cost loans are quite likely to be considered subprime and few loans that aren't considered subprime will classify as high cost.[46] Thus, the pool of loans at the heart of our analysis is quite stable over varying subprime definitions.

In Section 3.2 above, we give our rationale for choosing to end our study period at the close of 2006, with our last observation being loans originated in December, 2006. As Figure 5 shows, 2007 marks the beginning of the end of the housing bubble, whereas our objective is to study the factors that led to the creation of that bubble. Nevertheless, as a robustness check, we also consider four other possible end dates for our study: June 2006, September 2006, March 2007, and June 2007. Lastly, as indicated in Table 1, we identify six states as being primarily impacted by the S&P rating change, exclude six others, and use the remainder as controls. As a robustness check, we run permutations whereby we exclude each of these six treatment states from our data in order to verify that our results are not attributable to a single state's influence. In both the tests for different time periods and for excluding treatment states, the results and conclusions match those presented in the body of this paper very closely, with only a relatively minor amount of variance of the significance levels and magnitudes of the parameter estimates, as is to be expected when running over one hundred additional regressions.

[45] See, for example Staten (2004) for a discussion of a variety of subprime definitions, including FICO cutoffs such as those we use here.

[46] Specifically, we identify 144,005 high cost loans using our primary subprime definition and 118,738, 113,658, and 107,720 using the alternative subprime definitions with FICO cutoffs of 680, 660, and 640, respectively. Despite the similarities of these numbers, the somewhat greater number of these high cost loans identified using our primary subprime identification technique also points to a reason we favor it: a borrower taking out a loan whose interest rate exceeds prime rates by 6%, 8%, or more surely is not someone to whom prime or even Alt-A credit is available, regardless of whether or not they qualify as "subprime" according to the somewhat more blunt definitions we use as the robustness checks.

7 Conclusions

The subprime lending market has, quite deservedly, garnered an enormous amount of attention in the past decade. Scholarship has extensively documented many of the negative consequences of the market and the housing bubble it helped create.[47] Understanding what drove the development of this market is therefore important for understanding the economic history of the past decade and for informing any forward looking efforts to prevent damage from future financial meltdowns. The results in this paper show that when the costs of securitizing certain subprime loans increased, it caused a reduction in the total volume of those loans being originated. To the extent that this incident can be generalized, it suggests that securitization played a causal role in expanding the number of subprime mortgage loans that were made during the last decade. Yet, the results here also suggest that lenders were willing and able to substitute to some degree so that when securitization became more expensive, some of the loans that would have been securitized, rather than not being made at all, were instead simply made and then held on books of financial institutions. Together, these results suggest that explanations of the subprime lending boom as purely a product of the securitization process are inappropriately simplistic. Efforts to learn from the recent financial crisis and prevent future crises must therefore take into account the flexibility in funding channels that can emerge in order to satisfy a market demand for particular investment opportunities.

[47] See, e.g. Mian and Sufi (2009), Mian and Sufi (2014). Some lines of more recent literature has suggested, however, that at least certain segments of the subprime market were either relatively benign or at least no worse in some respects than the prime market (Park, 2011; Ferreira and Gyourko, 2015).

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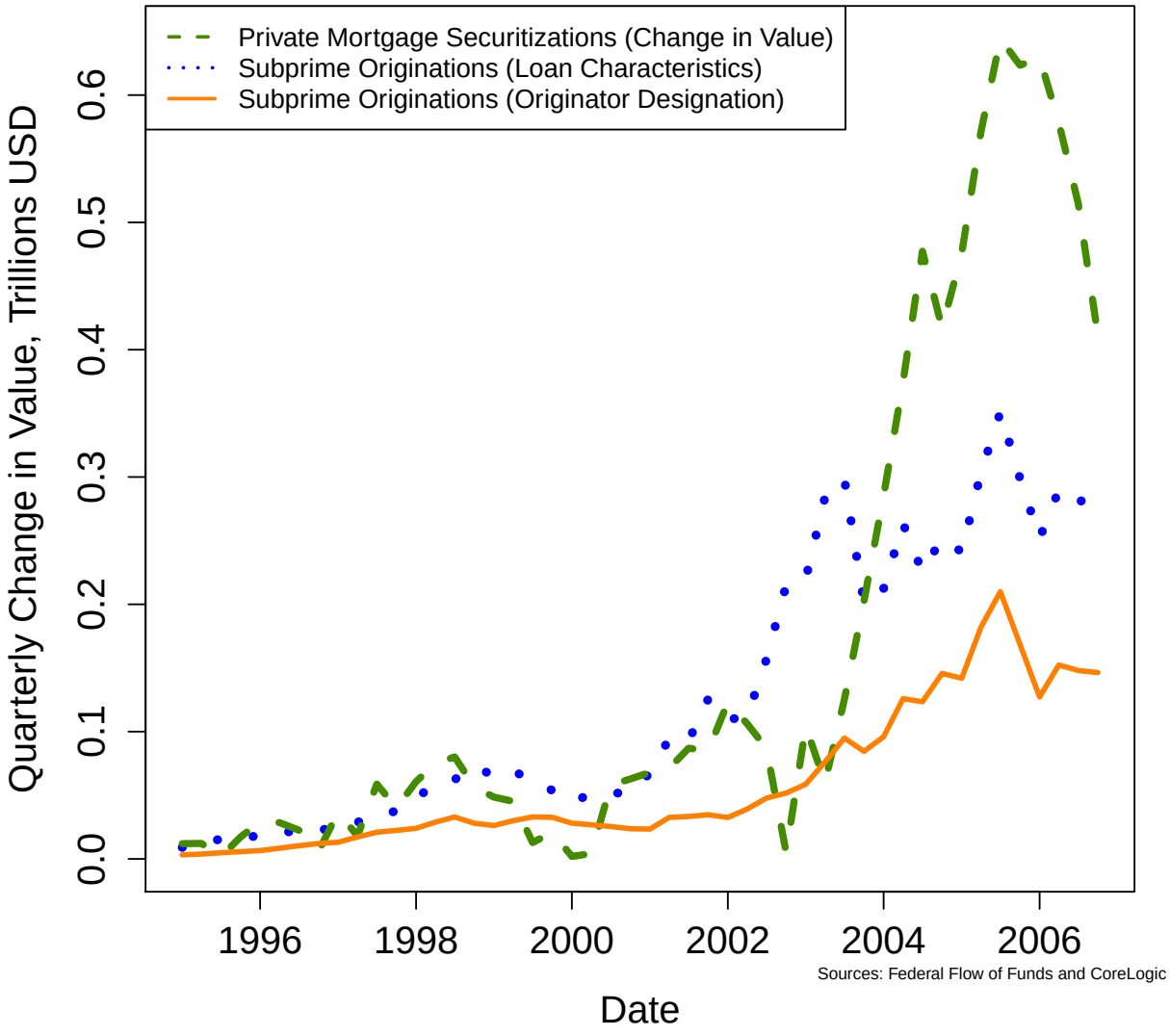


Figure 1. Subprime Mortgage Lending & Private Mortgage Securitization
Different definitions of "subprime" yield different measures of total subprime lending, though here overall trends are similar. Subprime is defined in this figure either by loan characteristics (loans with either FICO < 660, no doc / low doc loans, or negative amortization loans) or by originator designation. Private mortgage securitizations in this graph measure total change in value of outstanding private mortgage securitizations per quarter. Subprime originations in this graph measure total value of new mortgage originations per quarter.

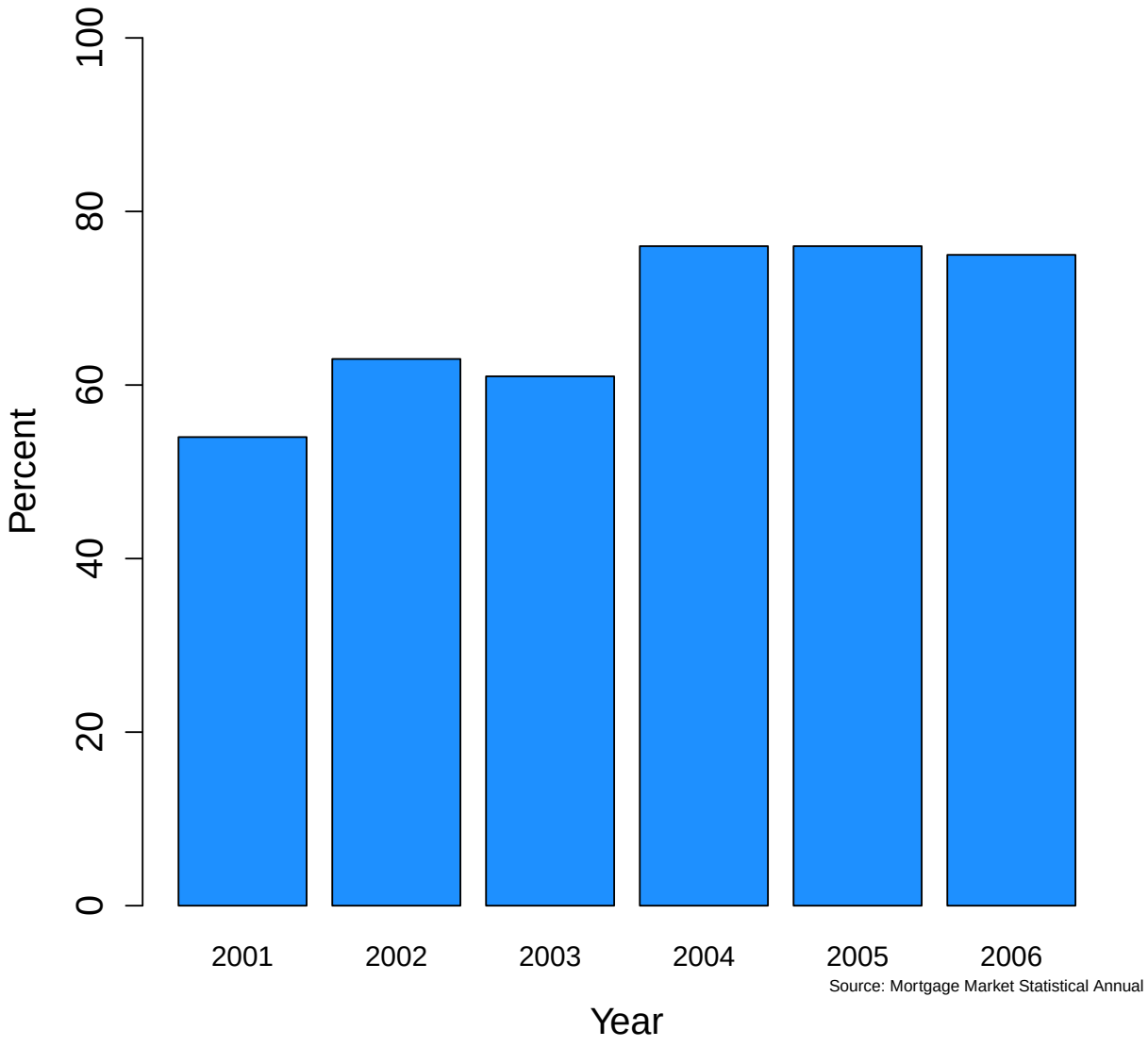


Figure 2. Securitized Portion of Subprime Loan Market

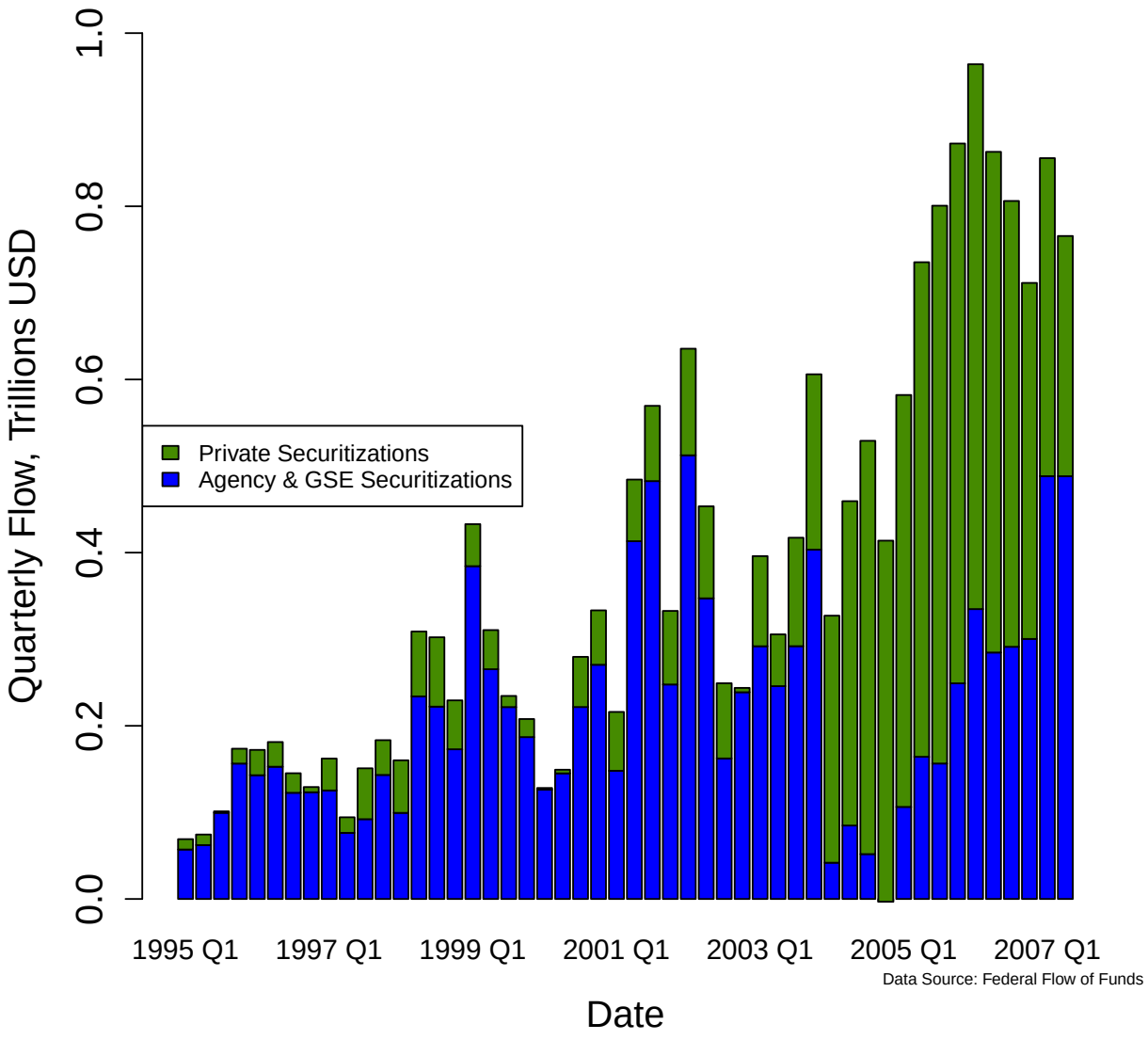
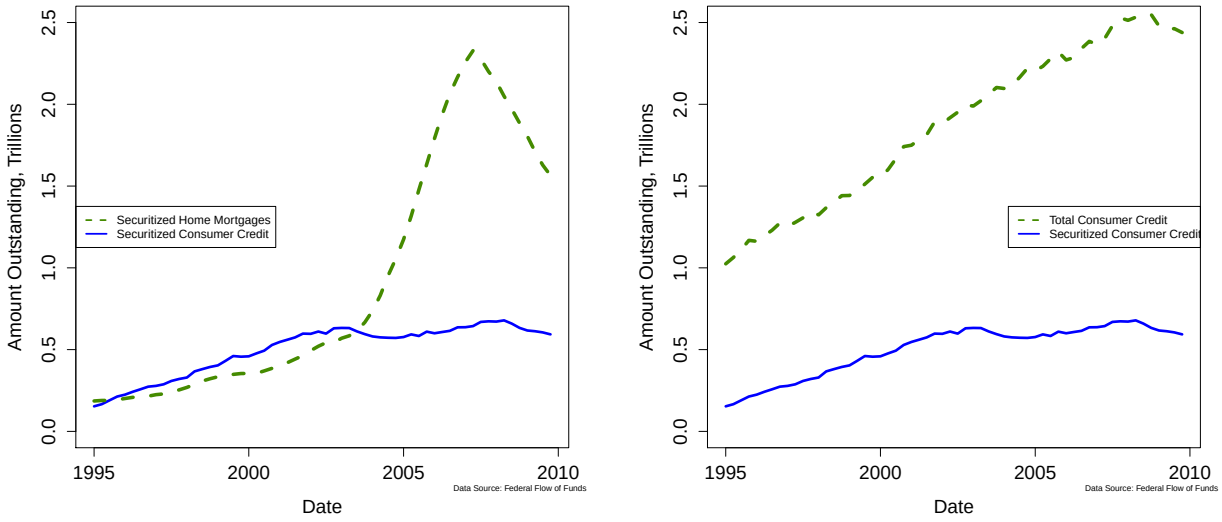


Figure 3. Comparing Agency & GSE Securitizations to Private Securitizations: Quarterly Change in Value Securitized



(a) Private Securitizations

(b) Consumer Credit

Figure 4. Comparing Private Securitization of Mortgages to Private Securitization of Consumer Credit.
The left panel shows that while private securitizations of home mortgages spiked dramatically in the mid to late 2000s, private securitization of consumer credit receivables experienced no such change. The right panel shows that the growth of total consumer credit was actually much steeper than the growth of securitized consumer credit, suggesting that securitization need not always drive large credit expansions.

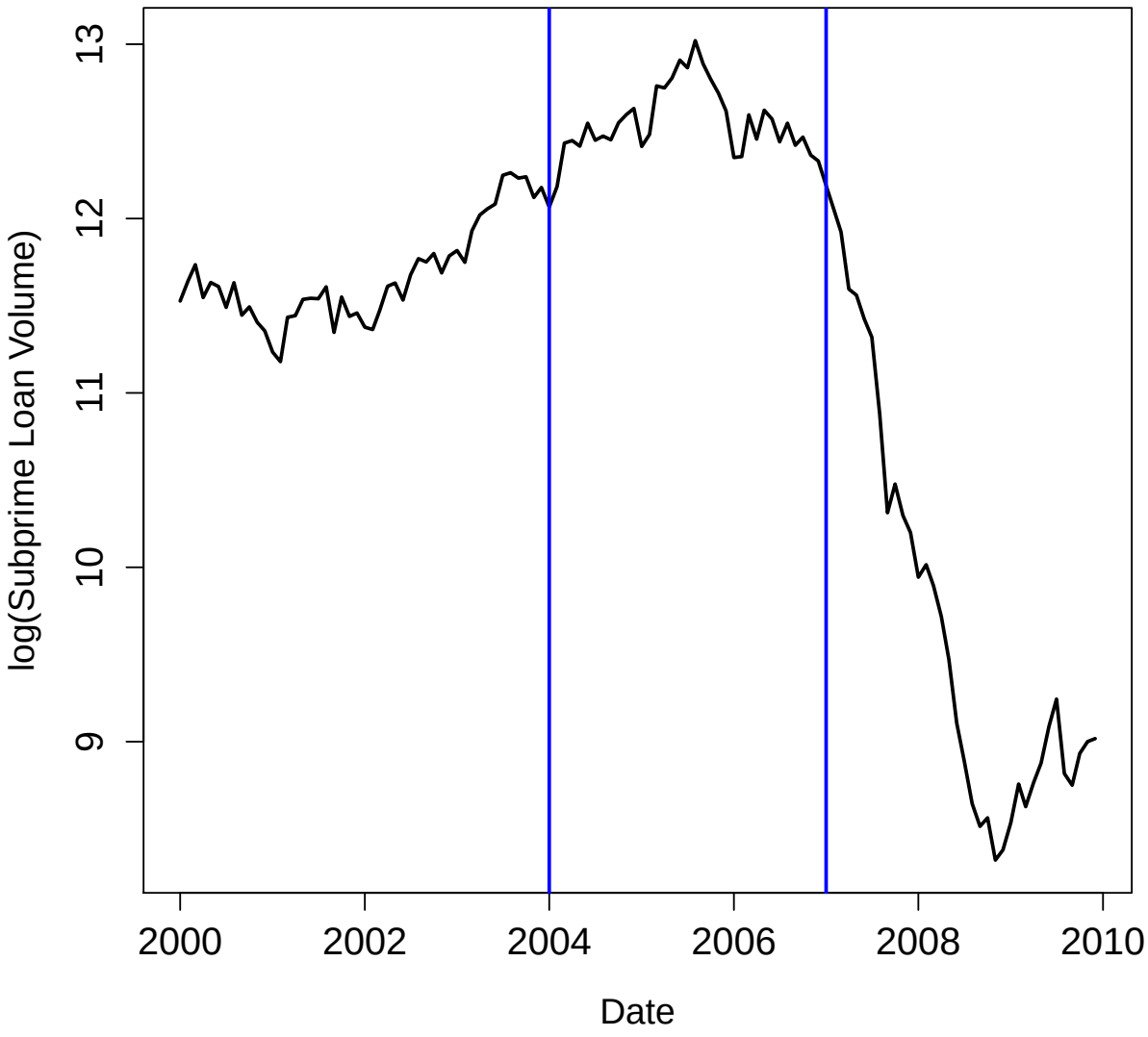


Figure 5. Monthly log number of total subprime loans originated in our data set. Vertical lines indicate period of study

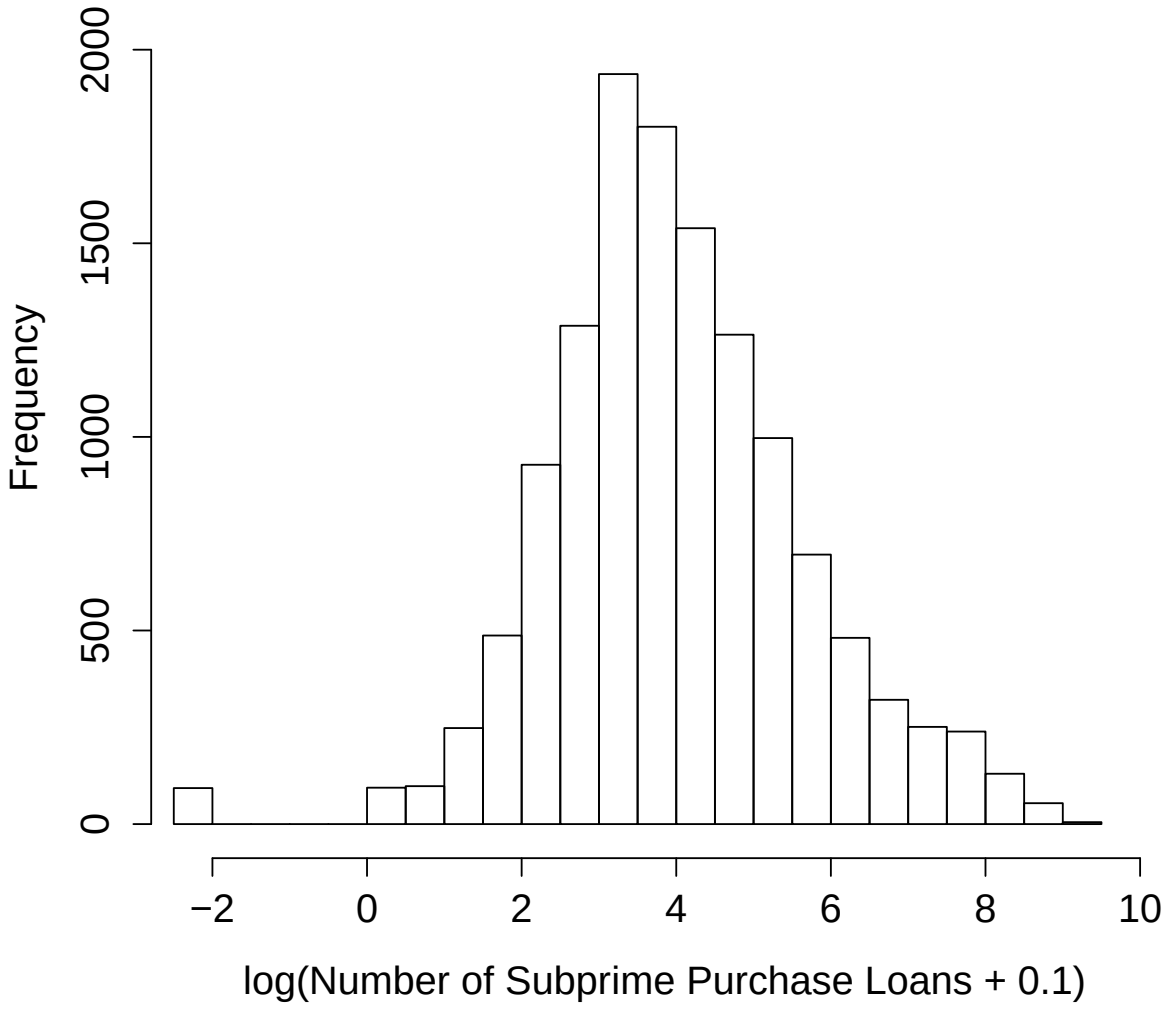
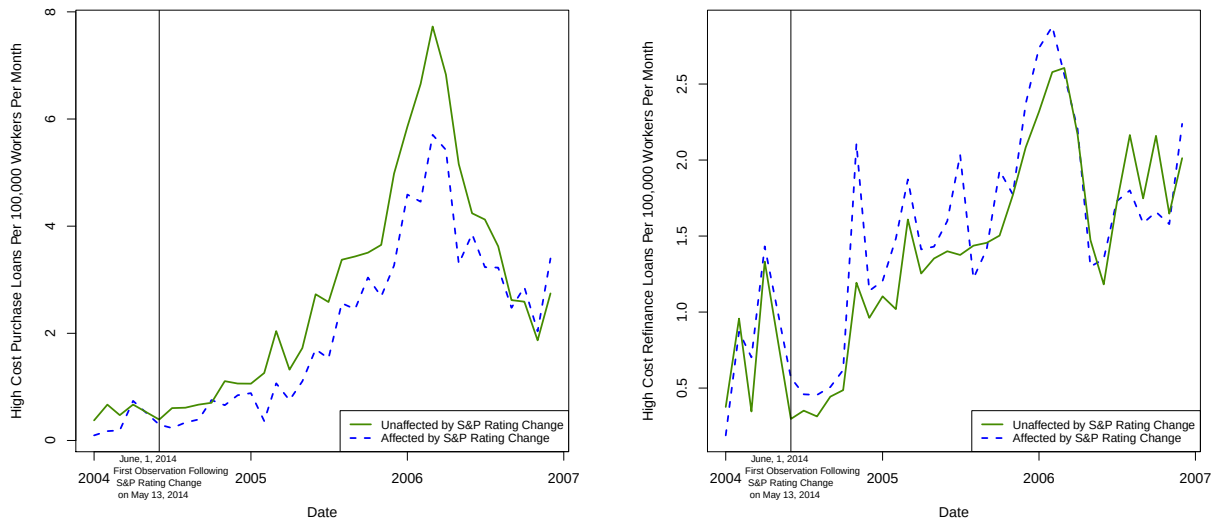


Figure 6. Distribution of log number of subprime purchase loans per MSA-month.
The bar near −2 on this graph indicates MSA-month combinations in which no subprime purchase loans were observed in the data. 0.1 is added to these observations so that the logarithm can be taken and they can be plotted on this graph. Naturally though, such a transformation of zero values in any data analysis setting would be problematic. The negative binomial regressions we employ require no such transformation.



(a) High Cost Purchase Loans

(b) High Cost Refinance Loans

Figure 7. Average lending volume for high cost loans in MSA affected and unaffected by the S&P rating change.
In each of these figures, the number of loans per 100,000 workers per month is calculated for each MSA in our sample. Means of these monthly figures are then taken separately for states affected and unaffected by the S&P rating change, and the results are plotted above. The left panel shows the consistent reduction in volume of high cost purchase loans in states impacted by the S&P rating change in months following that change. The right panel shows no such pattern - consistent with both our legal analyses and the regression results presented in this paper.

Table 1
Classifying states into treatment, control, and exclusion groups based on interaction between state APL laws and S&P rating change. Table covers only states mentioned in S&P rating announcement re: APL laws. "Effective Date" represents when a law became effective, rather than the earlier date on which it became law. "S&P Loss Severity" represents S&P's estimate of potential losses from assignee liability and thus dictates the amount of additional credit enhancement (ACE) required in order to include a loan in a rated securitization pool. "Exclude/NA" indicates that some types of loans are excluded from S&P rated transactions whereas others are allowed and require no ACE because S&P deems there to be no additional loss severity. S&P included states in its announcements, even if their laws just mirrored HOEPA, which is why these states are included in this table.

State Name	Refinance Loans	Purchase Loans	Effective Date	S&P Loss Severity	Comments
Arkansas	Treatment	Control	2003-07-16	196	Law only applied to refinance loans. May 2004 S&P announcement significantly increased restrictions as compared to initial 2003 announcement re: AR.
Colorado	Control	Control	2002-06-07	119	Law states that if preempted for federally chartered banks (which happened), then it doesn't apply to other lenders either.
Connecticut	Treatment	Treatment	2001-10-01	NA	No ACE required, but loans cannot be included in securitization if they charge default fee which we consider a significant restriction.
Florida	Control	Control	2002-10-02	119	Law closely mirrors HOEPA.
Georgia	Treatment	Treatment	2003-03-07	110	None.
Illinois	Treatment	Control	2004-01-01	110	Law only applied to refinance loans.
Indiana	Exclude	Exclude	2005-06-01	Exclude/NA	Law passed during study period.
Kansas	Control	Control	1999-04-14	NA	S&P simply required lenders to verify compliance with the law and to repurchase non-complying loans.
Kentucky	Control	Control	2003-06-24	NA	Law closely mirrors HOEPA.
Maine	Control	Control	2003-09-13	119	Law closely mirrors HOEPA.
Massachusetts	Exclude	Exclude	2004-11-07	Exclude/116	Law passed during study period.
Nevada	Control	Control	2003-10-01	NA	Law closely mirrors HOEPA.
New Jersey	Exclude	Exclude	2004-06-06	Exclude/NA	Law changed after start of study period.
New Mexico	Treatment	Treatment	2004-01-01	110	None.
New York State	Treatment	Treatment	2003-04-01	163	None.
North Carolina	Exclude	Exclude	1999-10-01	NA	SP Announcement re: NC came after start of study period, but before 2004-05-13.
Ohio	Control	Control	2002-05-24	37	Law closely mirrors HOEPA.
Oklahoma	Control	Control	2004-01-01	119	Law closely mirrors HOEPA.
Rhode Island	Exclude	Exclude	2006-12-31	Exclude/NA	Law passed during study period.
South Carolina	Control	Control	2004-01-01	NA	S&P simply required lenders to verify compliance with the law and to repurchase non-complying loans.
Tennessee	Exclude	Exclude	2007-01-01	110	Law passed during study period.
West Virginia	Control	Control	2002-06-05	NA	S&P simply required lenders to verify compliance with the law and to repurchase non-complying loans.

Table 2
Loan Summary Statistics
Median months to default is calculated just amongst loans that default. As discussed in Section 3.3, high cost loans here and elsewhere in our analysis refer only to those we identify based on interest rate and lien characteristics. Thus, high cost loans constitute a larger percent of total subprime loans than the number of loans identified here would suggest. One challenge of the CoreLogic data is that not all loans have information specified for every field. Percentages specified here therefore use the set of loans for which the given characteristic is specified as the denominator. Means are taken with respect to the set of loans for which the characteristic is specified. In nearly all cases, more than 90% of the loans have these given characteristics specified. Nevertheless, one notable exception is that only about 20% of the loans in the CoreLogic dataset specify an investor. We discuss this challenge and our responses to it in more depth in Section 6 below which covers a variety of robustness checks.

	All Loans	**Purchase**	**Refinance**	**High Cost**	**Private Securitization**	**On Books**
Percent Default	18	19	17	21	36	18
Median Time to Default	30	29	32	19	42	47
Mean FICO Score	632	646	617	585	652	652
Mean LTV	75	75	76	81	75	68
Mean Interest Rate	8	8	8	12	7	8
Mean Original Balance (1000s)	176	165	189	82	217	124
Percent Owner Occupied	93	92	94	91	94	98
Percent First Lien	85	75	95	53	90	79
Percent ARM	46	46	46	51	30	14
Percent No-Doc / Low Doc	22	27	17	26	23	10
Number Identified	5,908,404	3,112,663	2,795,741	144,005	501,005	144,697

Table 3
All Subprime Securitized Loans
In addition to the parameters presented here, the model also includes fixed effects for each loan origination date in our study, a unique linear time trend for states affected by the S&P rating change, and random effects at the MSA level. The indicator "S&P Rating Change - In Effect" is set to equal one in MSAs affected by the S&P rating change in months after May 2004 and set to zero elsewhere. We exclude May 2004 from the analysis because the S&P rating change was instituted in the middle of the month. log(Number Employed) refers to the total number of workers employed in an MSA and is obtained, along with MSA level unemployment rate (as a percentage), from the Bureau of Labor Statistics. Δ_{12} log(Total Wages) means the change in the log of total wages paid to workers in an MSA as compared to 12 months prior. Percent black and Hispanic refer to percentages of the employed population in an MSA. Percent occupied refers to the percentage of housing units that are occupied within an MSA's state. Data on total wages, racial/ethnic percentages, and housing occupancy are obtained from the US Census. R^2_C represents the squared correlation between the predicted and observed numbers of loans in each MSA-Month. R^2_M measures the improvement in the log-likelihood of our fitted model (LL_1) compared to a model with an intercept only (LL_0). It is defined as $(LL_0 - LL_1)/LL_0$.

	Purchase Loans	Refinance Loans
log(Unemployment Rate)	-0.330***	-0.369***
	(0.0760)	(0.0767)
log(Number Employed)	0.445***	0.298***
	(0.0409)	(0.0594)
Δ_{12} log (Total Wages)	0.448**	-0.121
	(0.181)	(0.180)
Percent Occupied	-2.799**	1.174
	(1.236)	(1.621)
Percent Black	-0.335	0.663
	(0.518)	(0.626)
Percent Hispanic	0.399	0.231
	(0.313)	(0.256)
S&P Rating Change - In Effect	-0.316**	-0.149
	(0.158)	(0.0925)
N (MSA-Month Observations)	12950	12950
R^2_C	0.614	0.488
R^2_M	0.113	0.079

Block bootstrap standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4
High Cost Subprime Securitized Loans
In addition to the parameters presented here, the model also includes fixed effects for each loan origination date in our study, a unique linear time trend for states affected by the S&P rating change, and random effects at the MSA level. The indicator "S&P Rating Change - In Effect" is set to equal one in MSAs affected by the S&P rating change in months after May 2004 and set to zero elsewhere. We exclude May 2004 from the analysis because the S&P rating change was instituted in the middle of the month. log(Number Employed) refers to the total number of workers employed in an MSA and is obtained, along with MSA level unemployment rate (as a percentage), from the Bureau of Labor Statistics. Δ_{12} log(Total Wages) means the change in the log of total wages paid to workers in an MSA as compared to 12 months prior. Percent black and Hispanic refer to percentages of the employed population in an MSA. Percent occupied refers to the percentage of housing units that are occupied within an MSA's state. Data on total wages, racial/ethnic percentages, and housing occupancy are obtained from the US Census. R^2_C represents the squared correlation between the predicted and observed numbers of loans in each MSA-Month. R^2_M measures the improvement in the log-likelihood of our fitted model (LL_1) compared to a model with an intercept only (LL_0). It is defined as $(LL_0 - LL_1)/LL_0$.

	Purchase Loans	Refinance Loans
log(Unemployment Rate)	0.244	0.250
	(0.153)	(0.194)
log(Number Employed)	1.126***	0.738***
	(0.0375)	(0.0459)
Δ_{12} log (Total Wages)	0.255	-1.317**
	(0.741)	(0.621)
Percent Occupied	-12.95***	-18.80***
	(1.696)	(2.342)
Percent Black	0.697*	2.720***
	(0.401)	(0.595)
Percent Hispanic	0.199	2.243***
	(0.265)	(0.351)
S&P Rating Change - In Effect	-0.743**	-0.617**
	(0.362)	(0.308)
N (MSA-Month Observations)	12950	12950
R^2_C	0.575	0.352
R^2_M	0.199	0.125

Block bootstrap standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5
All Subprime Loans
In addition to the parameters presented here, the model also includes fixed effects for each loan origination date in our study, a unique linear time trend for states affected by the S&P rating change, and random effects at the MSA level. The indicator "S&P Rating Change - In Effect" is set to equal one in MSAs affected by the S&P rating change in months after May 2004 and set to zero elsewhere. We exclude May 2004 from the analysis because the S&P rating change was instituted in the middle of the month. log(Number Employed) refers to the total number of workers employed in an MSA and is obtained, along with MSA level unemployment rate (as a percentage), from the Bureau of Labor Statistics. Δ_{12} log(Total Wages) means the change in the log of total wages paid to workers in an MSA as compared to 12 months prior. Percent black and Hispanic refer to percentages of the employed population in an MSA. Percent occupied refers to the percentage of housing units that are occupied within an MSA's state. Data on total wages, racial/ethnic percentages, and housing occupancy are obtained from the US Census. R^2_C represents the squared correlation between the predicted and observed numbers of loans in each MSA-Month. R^2_M measures the improvement in the log-likelihood of our fitted model (LL_1) compared to a model with an intercept only (LL_0). It is defined as $(LL_0 - LL_1)/LL_0$.

	Purchase Loans	Refinance Loans
log(Unemployment Rate)	-0.0762**	-0.272***
	(0.0340)	(0.0658)
log(Number Employed)	0.378***	0.222***
	(0.0462)	(0.0543)
Δ_{12} log (Total Wages)	0.207**	0.0491
	(0.0921)	(0.109)
Percent Occupied	2.297***	5.597***
	(0.800)	(1.015)
Percent Black	-0.284	-0.914
	(0.660)	(0.688)
Percent Hispanic	-0.361	-0.541
	(0.275)	(0.385)
S&P Rating Change - In Effect	-0.0538	0.0945***
	(0.0422)	(0.0326)
N (MSA-Month Observations)	12950	12950
R^2_C	0.582	0.324
R^2_M	0.104	0.06

Block bootstrap standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6
All High Cost Subprime Loans
In addition to the parameters presented here, the model also includes fixed effects for each loan origination date in our study, a unique linear time trend for states affected by the S&P rating change, and random effects at the MSA level. The indicator "S&P Rating Change - In Effect" is set to equal one in MSAs affected by the S&P rating change in months after May 2004 and set to zero elsewhere. We exclude May 2004 from the analysis because the S&P rating change was instituted in the middle of the month. log(Number Employed) refers to the total number of workers employed in an MSA and is obtained, along with MSA level unemployment rate (as a percentage), from the Bureau of Labor Statistics. Δ_{12} log(Total Wages) means the change in the log of total wages paid to workers in an MSA as compared to 12 months prior. Percent black and Hispanic refer to percentages of the employed population in an MSA. Percent occupied refers to the percentage of housing units that are occupied within an MSA's state. Data on total wages, racial/ethnic percentages, and housing occupancy are obtained from the US Census. R^2_C represents the squared correlation between the predicted and observed numbers of loans in each MSA-Month. R^2_M measures the improvement in the log-likelihood of our fitted model (LL_1) compared to a model with an intercept only (LL_0). It is defined as $(LL_0 - LL_1)/LL_0$.

	Purchase Loans	Refinance Loans
log(Unemployment Rate)	-0.125*	-0.243
	(0.0733)	(0.154)
log(Number Employed)	0.481***	0.670***
	(0.134)	(0.0917)
Δ_{12} log (Total Wages)	0.396*	-0.686**
	(0.226)	(0.287)
Percent Occupied	-2.523	-4.800**
	(1.859)	(2.357)
Percent Black	0.900*	2.199***
	(0.533)	(0.583)
Percent Hispanic	-0.830**	0.679**
	(0.374)	(0.319)
S&P Rating Change - In Effect	-0.461***	0.253*
	(0.153)	(0.145)
N (MSA-Month Observations)	12950	12950
R^2_C	0.579	0.705
R^2_M	0.193	0.112

Block bootstrap standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7
All Subprime Loans Held on Books
In addition to the parameters presented here, the model also includes fixed effects for each loan origination date in our study, a unique linear time trend for states affected by the S&P rating change, and random effects at the MSA level. The indicator "S&P Rating Change - In Effect" is set to equal one in MSAs affected by the S&P rating change in months after May 2004 and set to zero elsewhere. We exclude May 2004 from the analysis because the S&P rating change was instituted in the middle of the month. log(Number Employed) refers to the total number of workers employed in an MSA and is obtained, along with MSA level unemployment rate (as a percentage), from the Bureau of Labor Statistics. Δ_{12} log(Total Wages) means the change in the log of total wages paid to workers in an MSA as compared to 12 months prior. Percent black and Hispanic refer to percentages of the employed population in an MSA. Percent occupied refers to the percentage of housing units that are occupied within an MSA's state. Data on total wages, racial/ethnic percentages, and housing occupancy are obtained from the US Census. R^2_C represents the squared correlation between the predicted and observed numbers of loans in each MSA-Month. R^2_M measures the improvement in the log-likelihood of our fitted model (LL_1) compared to a model with an intercept only (LL_0). It is defined as $(LL_0 - LL_1)/LL_0$.

	Purchase Loans	Refinance Loans
log(Unemployment Rate)	0.663***	-0.129
	(0.134)	(0.108)
log(Number Employed)	0.508***	0.306***
	(0.104)	(0.118)
Δ_{12} log (Total Wages)	0.195	0.200
	(0.364)	(0.242)
Percent Occupied	-7.949***	-2.407
	(2.258)	(1.585)
Percent Black	1.160	0.512
	(0.814)	(0.659)
Percent Hispanic	-1.704***	-1.398***
	(0.379)	(0.431)
S&P Rating Change - In Effect	0.0957	0.150**
	(0.170)	(0.0587)
N (MSA-Month Observations)	12950	12950
R^2_C	0.355	0.451
R^2_M	0.07	0.071

Block bootstrap standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8
All High Cost Subprime Loans Held on Books
In addition to the parameters presented here, the model also includes fixed effects for each loan origination date in our study, a unique linear time trend for states affected by the S&P rating change, and random effects at the MSA level. The indicator "S&P Rating Change - In Effect" is set to equal one in MSAs affected by the S&P rating change in months after May 2004 and set to zero elsewhere. We exclude May 2004 from the analysis because the S&P rating change was instituted in the middle of the month. log(Number Employed) refers to the total number of workers employed in an MSA and is obtained, along with MSA level unemployment rate (as a percentage), from the Bureau of Labor Statistics. Δ_{12} log(Total Wages) means the change in the log of total wages paid to workers in an MSA as compared to 12 months prior. Percent black and Hispanic refer to percentages of the employed population in an MSA. Percent occupied refers to the percentage of housing units that are occupied within an MSA's state. Data on total wages, racial/ethnic percentages, and housing occupancy are obtained from the US Census. R_C^2 represents the squared correlation between the predicted and observed numbers of loans in each MSA-Month. R_M^2 measures the improvement in the log-likelihood of our fitted model (LL_1) compared to a model with an intercept only (LL_0). It is defined as $(LL_0 - LL_1)/LL_0$.

	Purchase Loans	Refinance Loans
log(Unemployment Rate)	0.436*	0.137
	(0.249)	(0.171)
log(Number Employed)	0.982***	1.002***
	(0.0586)	(0.0442)
Δ_{12} log (Total Wages)	1.798	-1.148
	(1.404)	(0.785)
Percent Occupied	-12.72***	-14.07***
	(3.267)	(2.400)
Percent Black	0.962	-0.344
	(0.754)	(0.510)
Percent Hispanic	-1.321*	-2.774***
	(0.751)	(0.631)
S&P Rating Change - In Effect	0.364	0.275
	(0.619)	(0.459)
N (MSA-Month Observations)	12950	12950
R_C^2	0.253	0.437
R_M^2	0.224	0.195

Block bootstrap standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$