

**Mortgage Product Substitution and State Anti-Predatory Lending Laws: Better Loans and Better Borrowers?**

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*Abstract:*

Mounting foreclosures and disclosures of abusive lending practices led many states to adopt new anti-predatory lending (APL) laws. Researchers have examined the impact of such laws on credit flows and the cost of credit. This research extends the literature by examining whether the market responded to these laws by substituting different mortgage products for those restricted by APL provisions. The evidence indicates that the laws were effective in restricting loans with targeted characteristics and that the market substituted other product types to maintain access to credit and affordability in the face of these restrictions. The laws did reduce the involvement investor and second home purchases but did appear to impact borrower credit scores or down payments.

## **Introduction and Motivation**

The widely recognized collapse of housing markets was facilitated in part by changes in mortgage products and the channels through which borrowers obtained loans. An expansion of certain types of mortgage products, along with a weakening of underwriting standards, led to a proliferation of loans that were either unaffordable or put borrowers underwater if home values declined. Policy-makers have long been concerned about a rapid expansion of loan products and practices that could lead to such negative outcomes. Indeed, over the last 14 years more than half the states and several localities in the U.S. have enacted statutes and ordinances designed to regulate residential mortgage market practices and reduce abuses.

The growth and subsequent collapse of subprime lending provide indirect evidence that existing regulation and oversight of the subprime market were not successful in protecting the financial welfare of borrowers, lenders, or investors. That noted, it remains possible that anti-predatory lending (APL) laws<sup>1</sup> did influence the behavior of mortgage market players and led to a systematic change in the types of borrowers who took out loans. The laws may have also led to more frequent use of exotic loan products or the traditional plain-vanilla 30-year fixed rate fully amortizing loans. In this paper, we explore such possibilities.

The disruption for families, neighborhoods, and financial institutions that has accompanied the housing and mortgage market collapse will have wide-ranging, long-lived effects. These ramifications provide at least a preliminary argument for restricting risky lending practices and

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<sup>1</sup> Until passage of the Dodd-Frank Act in 2010, mortgage regulation was fragmented and many large originators were exempt from state APL laws. National banks, federal thrift institutions, and their operating subsidiaries were exempt from state APL laws. In addition, some states with those laws exempted state-chartered depository institutions from having to comply with state APL laws under so-called “wild card” provisions.

products. However, lenders and borrowers might chafe in the face of such restrictions. This could be the case if the restrictions limit mechanisms that lenders use to reduce borrowers' monthly payments and make loans more affordable—at least in the short run. For example, adjustable rate mortgages (ARMs) typically feature lower interest rates than fixed rate mortgages (FRMs) because borrowers bear some of the interest rate risk (Brueckner (1980), Sa-Aadu and Sirmans (1989), Pennington-Cross and Ho (2008)). In addition, costs among ARMs can vary considerably depending on the interest rate reset (6 months, 12 months, etc.) and maximum and minimum interest rate adjustments, among other items. Empirical evidence shows that manipulation of these types of characteristics can have large impacts on the risk premium on a loan (Pennington-Cross and Ho, 2008). Changes in interest rate risk can reduce the borrowers' annual percentage rate (APR) by over 300 basis points.<sup>2</sup>

If regulatory restrictions, which we characterize as APL laws, limit the ability of lenders to structure loans to minimize borrower monthly payments, then lenders and borrowers alike will seek alternatives for reducing monthly payments. Central to these efforts will be an attempt to avoid regulatory thresholds. State APL laws usually only apply if the APR or points and fees are above set triggers. One way to evade the triggers is to reduce the APR on loans. Lenders can achieve this by lengthening amortization schedules and increasing access to interest only and negative amortization loan products.

In addition, if APL laws impose compliance costs, such as those associated with mandatory reporting on loans that exceed triggers, the costs are likely to be passed on to consumers. The

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<sup>2</sup> The APR and the true borrower cost can deviate substantially because the APR does not include all fees, prepayment penalties, and the ability of the borrower to pay off the loan early. As a result APRs can be manipulated in many ways while maintaining lender yields (Green and Wachter, 2007).

evidence indicates that laws that are more restrictive do tend to drive up the cost of borrowing through slightly higher interest rates (Pennington-Cross and Ho, 2008; Li and Ernst, 2007).

Despite being small, this increase in costs is also likely to induce lenders to substitute loans that hit or exceed APL triggers with products that fall below the triggers.

This paper asks a plain question: in light of prior evidence that volume and pricing impacts of the APL laws are fairly moderate, did lenders and borrowers find substitutes for loans that APLs banned or limited? More specifically, did they avoid the laws by writing loans with alternative terms that fell below the triggers of the laws (APR avoidance) while meeting their own business needs (underwriting and yield requirements) or was the more popular substitution toward the traditional 30-year fixed rate fully amortizing loan? In addition, in terms of credit scores and down payments, did borrower characteristics change?

The next section provides some background about APL laws and prior research on their effects, followed by a description of the data and empirical methodology. After presenting the key results, the paper concludes with a discussion of the implications of the findings for policy makers and the mortgage market.

### **A Guide to State Anti-Predatory Lending (APL)Laws**

Historically, interest rate caps – also known as usury laws – were a popular way of policing abuses in consumer loans. Usury laws fell into disfavor, and in recent years legislators have taken a new approach to regulating credit. This newer generation of laws eschews interest rate caps in favor of restrictions on certain lending practices and the non-interest terms of loans.

Some states have regulated isolated loan terms as far back as the 1960s. More comprehensive APL laws did not arrive until the 1990s. Congress led the way by enacting the first modern APL law, the Home Ownership and Equity Protection Act (HOEPA), in 1994. HOEPA, like many of the state APL laws that followed, singles out “high-cost loans” and strictly limits their terms and practices. For purposes of HOEPA, before 2009, high-cost loans were defined as loans:<sup>3</sup>

(1) where the annual percentage rate (APR) at consummation exceeded the yield on the comparable Treasury security plus eight percent for first-lien loans or ten percent for junior-lien loans; or

(2) where the total points and fees exceeded the greater of eight percent of the total loan amount or \$400 (subject to annual indexing).

These triggers were so high, as of 2007, that HOEPA regulated no more than one percent of subprime home loans (Gramlich, 2007).

Subsequently, a series of states enacted their own comprehensive APL statutes, many of which were patterned after HOEPA.<sup>4</sup> In 1999, North Carolina became the first state to pioneer a state “mini-HOEPA” law. By January 1, 2007, twenty-nine states and the District of Columbia had mini-HOEPA laws.<sup>5</sup>

Most state mini-HOEPA laws that were in place by the end of 2006 depart from HOEPA in one way or another.<sup>6</sup> Some mini-HOEPA laws impose more stringent substantive restrictions than HOEPA. Similarly, lower triggers are a feature in many mini-HOEPA laws, but not in all. The

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<sup>3</sup> 12 C.F.R. § 226.32(a)(1), (b)(1). The Dodd-Frank Act amended HOEPA in 2009.

<sup>4</sup> Similarly, many states enacted mortgage broker and banker licensing and regulation laws.

<sup>5</sup> Specifically, Arkansas, California, Colorado, Connecticut, Florida, Georgia, Illinois, Indiana, Kentucky, Maine, Maryland, Massachusetts, Minnesota, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, Tennessee, Texas, Utah, West Virginia, Wisconsin, Washington, and the District of Columbia.

<sup>6</sup> For the most part, these laws are still on the books, either in their original form or as amended.

enforcement mechanisms in state mini-HOEPA laws also vary in a number of ways. Some state laws restrict enforcement to the government; other state laws also give aggrieved borrowers the right to sue. Among the laws that permit injured borrowers to sue, there are variations in who can be named as a defendant. Some of these laws only allow borrowers to sue their lender or broker. The more liberal laws also allow borrowers to obtain relief against the owners of their notes if specific requirements are met. State laws that authorize borrower lawsuits also differ in the type of relief they afford. Some permit double or treble damages. Others restrict damages to compensatory relief alone.<sup>7</sup>

Since the passage of the first state APL law in North Carolina, researchers have been working to understand how these laws affect the mortgage market. The first issue that researchers addressed was whether the introduction of these laws made the subprime market diminish in size. cursory observations of the growth in subprime loans indicated that the typical law did not have much impact on the overall flow of credit. In fact, an early survey of subprime branch managers by Morgan Stanley (2002) found that expectations of subprime growth in states with more aggressive regulations were similar to expectations in less regulated states. Morgan Stanley also reported that respondents found that increased disclosures associated with new laws helped to boost consumer comfort, leading to an increase in loan volumes. Nonetheless, state APL laws had divergent effects depending on their provisions. For example, laws with very few restrictions

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<sup>7</sup> A handful of cities and counties passed predatory lending ordinances of their own. As a result of state enactments or court decisions, these ordinances either never took effect or only took effect briefly. Mini-HOEPA laws are not the only type of state APL laws. Some states have older laws that regulate prepayment penalties or balloon payments. Of the states with mini-HOEPA laws, thirteen combine an older APL law with a newer mini-HOEPA statute. Other states have an older APL law, but no mini-HOEPA law. By January 1, 2007, only six states – Arizona, Delaware, Montana, North Dakota, Oregon, and South Dakota -- had no APL laws or laws regulating prepayment penalties, balloon clauses, or mandatory arbitration clauses in residential mortgages. Federal law preempted portions of these state laws at various times for certain types of lenders and loan products. See Bostic, et al. (2008) for more detail.

were associated with a relative increase in subprime lending, while state laws with stronger restrictions were associated with a decrease in subprime lending (Ho & Pennington-Cross, 2006; Bostic, et al., 2008; Elliehausen et al., 2006). Increasing the coverage of a state law by lowering the triggers or covering more loan products, however, helped mitigate the dampening effect of stronger restrictions on subprime loan volumes (Bostic, et al., 2008).

## **Data**

The First American Loan Performance Asset Backed Securities (LP) data provide a long time series that covers the first mini-HOEPA laws that went into effect in 1999, and contain extensive detail on loan characteristics such as whether the loans are interest only or no or low documentation, the depth or size and length of teaser rates, and other information..

The LP data are not perfect. At most, the loans in the LP data represent just over 90% of the Asset Backed Securities (ABS) market and include private-label securities backed by both subprime loans and Alt-A loans (non-traditional loans made to borrowers with strong credit scores). The LP data come from servicers that manage securitized loans and do not include subprime loans that are held in lenders' portfolios. Therefore, any systematic differences between loans held in portfolio and those that are securitized may limit the generality our results. The proportion of subprime and Alt-A loans in securitized pools changed.

Since we are interested in geographic variation over time and how that relates to changes in APL laws, we aggregate the data at the zip code level for the empirical analysis. Table 1 provides the summary statistics of that data for the lower 48 states from January 1999 through July 2007. The data set is an amalgamation of many different types of subprime loans. For example, it includes



details on different lien positions, loan types, interest types, amortization schedules, and penalties. The summary statistics are calculated at the loan level and before any sampling.

The average loan amount is a little over \$175,000, but that number is smaller for fixed rate loans because more fixed rate loans have second or third mortgages (higher liens). Likely because of the frequent use of additional liens, loans with a balloon payment due are the smallest on average and have the highest reported initial interest rate. The combined Loan-To-Value (LTV) ratio at origination for all loans is on average around 80% and the average Fair Isaac (FICO) credit score is a little over 645. A minority of all loans provide limited or no documentation. The majority of interest only and Alt-A loans also had low or no documentation.

Adjustable rate and hybrid loans have the lowest average credit score, of approximately 630. For adjustable rate loans, approximately 74% have a teaser rate (an initial interest rate below the fully adjusted rate<sup>8</sup>) which, on average, is approximately 1.9 percentage points below what the fully adjusted rate at origination and expires after 31 months. Almost all the adjustable rate loans were hybrid loans.<sup>9</sup> A hybrid loan acts like a fixed rate loan for a certain amount of time, typically 2 or 3 years for subprime loans, and then acts like an adjustable rate loan for the remaining life of the loan. For the vast majority of adjustable rate loans the lifetime floor for the interest rate is equal to the initial rate. Therefore, payments can only rise and never fall for most subprime adjustable rate loans. Loans with negative amortization are a very small part of the

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<sup>8</sup> The fully adjusted rate is typically calculated by adding the index defined in the loan documents (such as the London Interbank Overnight Rate or LIBOR) to the margin (or spread).

<sup>9</sup> The interest rate on a hybrid loan is initially fixed for a longer period of time than the reset period.

market.<sup>10</sup>

Figures 1 through 5 provide a visual presentation of some of the key mortgage characteristics on a map of US counties for the years 1999 and 2006. For example, Figure 1 reports the percent of loans that were Adjustable Rate Mortgages (ARMs) in the years 1999 and 2006. It shows that ARMs were increasing in use everywhere over the 1999-2006 time periods, but they were most prevalent in major population centers (West Coast, Florida, Boston to Norfolk along the Atlantic, and Chicago to Minneapolis near Lake Michigan). As shown in Figure 2, low documentation loans had spread across the nation but were more concentrated in California, Florida, and the Boston, New York City and Washington DC regions. There is also a similar pattern for interest only loans and loans with balloon features. Figure 3 indicates that the use of prepayment penalties varies substantially along state lines and does not reflect economic conditions. For example, the states of North and South Carolina, Georgia, Vermont, New Mexico, Iowa, Illinois, New Jersey and Massachusetts are all clearly identified in the figures as states with fewer prepayment penalties. This spatial variation is the result of state APL laws that restrict those penalties.

All of these features (ARM, hybrid, low documentation, interest only, balloons, and teasers) can be used to help people buy a home when they are having a hard time meeting the traditional underwriting standards. They also can be used to evade state law triggers.

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<sup>10</sup> Negative amortization allows the borrower to pay no principal and less than the interest due for a set initial period, resulting in a rising mortgage balance over time.

In summary, these statistics indicate that adjustable rate and hybrid loans were first popular in high cost areas such as San Francisco, Washington DC, and New York City, and then became increasingly popular across the U.S. At the same time other mortgage characteristics, such as low documentation, interest only, and negative amortization, were bundled with the adjustable rate loans.

### **Empirical Approach and Identification Strategy**

The primary objective of this paper is to determine if the introduction of state APL laws between 1999 and 2006 led to a change in the types of loans subprime lenders made or the types of borrowers who obtained subprime loans. The patchwork of APL laws across the United States, along with the fact that economic forces do not correspond to state lines, allow us to use a difference-in-differences approach to identify the effect of the laws.

The existence of different state level legal environments lends itself to empirical study because by moving just a few feet or miles you can cross a state line. Locations in states where the laws are introduced can be thought of as the treatment locations and the locations in states that do not have a law introduced can be thought of as the control locations. We sharpen this dichotomy by limiting the sample to zip codes whose center is within 10 miles of a control/treatment state border. The advantage to this sampling technique is that it includes zip codes that are likely to be in the same housing and labor markets. The cost associated with this sampling approach is that many observations are dropped from the data and large cities that are not near state borders are not included in the estimation.

For all locations, mortgage outcomes are tracked both before and after the enactment of APL laws. We, thus, observe how mortgage patterns change pre- and post-enactment of the law which, combined with the location partition, permits us to identify the independent effect of anti-predatory laws on mortgage patterns. In order to implement this effectively, the control states must have a stable legal environment for the entire study period. The ultimate treatment sample is therefore defined as all zip codes in the state within 10 miles of a control state without any change in law status for the sample period. This incorporation of time reflects an innovation on prior studies that have capitalized on the spatial discontinuity of legal structures, such as Holmes (1998) and Pence (2006).

#### The Sampling Technique and an Example

The empirical tests include up to 26 states for which there is reliable data for the years before and after their mini HOEPA laws came into effect: California, Colorado, Connecticut, District of Columbia, Florida, Georgia, Illinois, Indiana, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Nevada, New Jersey, New Mexico, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, South Carolina, Texas, Utah, and West Virginia. The data can be thought of as a separate sample for each law. In the regressions the samples are stacked into a pseudo panel. The treatment locations, the control locations, and the passage of time (before and after the law comes into effect) are controlled for separately for each law under consideration. The impact of the laws coming into effect is pooled across all the individual laws.

For illustration, consider the state of California, whose law went into effect on July 1, 2002. The loans to identify the effect of California's law are limited to those zip codes in California and

those in neighboring states whose center points are within 10 miles of the California state border. The sample is further restricted by excluding all loans originated 6 months before and after the law went into effect. The pre-law time period is defined as the monthly zip code observations for the 6 months before the excluded time period. The post-law time period is defined as the next 6 months of observations after the excluded time period. Therefore, there are a total of 12 months of observations for each law in each zip code. This doughnut hole sampling technique is designed to remove observations from the data when lenders and borrowers might have been preparing for and adjusting to the new law, while still allowing enough observations to provide some precision in the results. Dummy variables are used to identify the pre-law and post-law time periods.

California borders Arizona, Nevada, Oregon, and the Pacific Ocean. Loans originated in zip codes nearest to the Pacific Ocean are not included in the sample. Loans originated in a bordering state's zip codes are only included in the sample if that state did not have a change in APL law status over the pre-law and post-law time periods. In California's case the three bordering states had no change in law status from June 2001 through December 2002. Therefore, all three bordering state zip codes within 10 miles are included to help identify the effect of the California law.

These restrictions on time and location help refine the identification strategy. For the California law, the California zip codes can be thought of as treatment locations and the three other state zip codes as control locations. This process is repeated for the remaining 25 state laws and the effect of the law coming into effect is pooled across all the different laws.

## Regression Method

We test the impact of the law on a series of different mortgage characteristics in separate regressions that use the same sampling technique and control variables. The regression structure can be written as follows:

$$\begin{aligned} Mortgage_{it} = & \beta^0 + \beta^1 Ineffect_{it} + \sum_j \beta_j^2 Sample_{ji} + \sum_j \beta_j^3 Law_{ji} + \sum_j \sum_k \beta_{jk}^4 NoLaw_{kji} \\ & + \sum_j \beta_j^5 Postlaw_{jit} + \varepsilon_{it} \end{aligned} \quad (1)$$

where  $i$ ,  $t$ ,  $j$  and  $k$  index, respectively, the individual zip codes, the time period, the law samples, and the control locations.

*Mortgage* is a variable that represents the prevalence or market share of a specific mortgage characteristic in the zip code. One example would be the percentage of loans in the zip code that have adjustable interest rates. The number of loans with an adjustable rate and the number of all loans would be used to calculate the percentage of loans that have adjustable rates in the zip code. The first row of Table 2, labeled “Adjustable Rate,” reports the coefficient estimate for the variable *Ineffect* where the dependent variable is the logistic transformation of the fraction of loans that have adjustable rate features. The second row of Table 3 reports the coefficient estimate of *Ineffect* when the dependent variable is the fraction of loans with balloon payment features. The rest of the reported results are all interpreted in the same way. Therefore, each table reports the coefficient estimate of *Ineffect* for over 20 separate regression results. The tables do not report the coefficient estimates for the variables used to control for the sample used for each of the  $j$  laws,  $\beta_j^2$ , the  $j$  locations where the law comes eventually into effect (treatment location),  $\beta_j^3$ , the  $k$  control locations for the  $j$ th law location,  $\beta_{jk}^4$ , and the control for the time period

when the law comes into effect,  $\beta_j^5$ . While these variables and their coefficients are vital to the identification strategy, they have no relevant economic interpretation for this study. In addition, it would take an inordinate amount of space to report the coefficient estimates.

The key independent variable is *Ineffect*, a treatment indicator that identifies if and where an APL law is in effect. It is created by interacting the post-law time period dummy with a treatment location dummy. *Sample* indicates the law sample, which includes both treatment and control locations. *Law* indicates locations where a law will be in effect at some point during the sample period. *NoLaw* indicates locations where there is no change in the law over the pre- and post-sampling time period. *Postlaw* indicates the post-law time period for each law sample. To control for unique characteristics of each state, a dummy variable or fixed effect is included for each state in the law sample. This will control for the many factors that make the states different but do not change over time. The location fixed effects in conjunction with pre-law and post-law fixed effects control for the passage of time for the region and the location of each zip code. The error terms are assumed to be dependent within zip codes.

A logistic transformation of the market share is used to limit the estimates between zero and one ( $\log(s_{it} / 1-s_{it})$ , where  $s$  is the market share for county  $i$  in time period  $t$ ). Each market share does not represent the same number of mortgages and the variance of the error term is expected to be  $\sigma_{it}^2 = \frac{1}{n_{it}s_{it}(1-s_{it})}$  where  $n_{it}$  is the total number of loans for county  $i$  in time period  $t$ . To reflect this non-constant variance we weight each observation by  $n_{it}s_{it}(1 - s_{it})$ . Therefore, observations that have more loan originations receive more weight and the closer a zip codes' market share of a particular mortgage characteristic is to 50% the more weight it receives in the regression. Zip

codes with 100% or 0% market share have a weight of zero. Not all mortgage characteristics are market shares. Some reflect an average. For example, it may be interesting to identify the impact of laws on the average strength of the teaser on adjustable rate loans. The model is then be estimated as a Weighted Least Squares (WLS) because the variable is not bounded by 0 and 1 and is weighted to reflect the number of loans used to calculate the market share. In Table 2 we can then interpret the coefficient reported in the row “ARM Teaser length” as the average increase in the teaser length in months when a law comes into effect.

Peterson (2009) shows that clustered standard errors provide unbiased estimates in panel data sets when the independent variables and error terms are both correlated within groups (in our data this is the zip codes). Therefore, a robust Huber/White Sandwich estimator of variance is used instead of the traditional approach. In addition, we explicitly estimate the correlation within zip codes. This procedure affects both standard error estimates and coefficient estimates.  $e_{it}$  represents an identically and independently distributed random error term. We require that a law sample has at least 120 loans and at least 5 loans in the treatment location, control location, post-law time period, and pre-law time periods.

## **Results**

Tables 2 through 6 provide a summary of the results. Each table include results for 22 separate regressions, each of which tests for changes in a single mortgage characteristic to the introduction of a law. The tables report the coefficient estimate for the *Ineffect* variable, its standard error, and the exponent of the coefficient estimate or odds ratio. The coefficient can be interpreted as the average or typical impact of a law on product use. The column “Laws”



indicates the number of laws that were used to estimate the impact of a law coming into effect,  $\beta_j^l$ . This varies for each loan type due to data constraints.

If the exponent of the coefficient estimate is reported, the model is specified as a logistic transformation. Odds ratios are calculated as  $e^\beta$  for the coefficient and can be interpreted as the increase in the odds of using a particular product type when a law comes into effect. The number of laws used to estimate the results is also included. This will differ by product type and any subsampling for sensitivity tests. For example, the introduction of APL laws in 22 states is used to estimate the -0.36 coefficient for adjustable rate use. Fewer laws are used for balloon loans because balloon payments represent a much smaller portion of loans originated.

The results in Table 2 provide evidence supporting both the view that the APL laws were effective in changing mortgage product characteristics and the view that product substitution took place. For example, many state laws are designed to limit the availability of prepayment penalties and balloons. Our results show a significant decline in the prevalence of loans with prepayment penalties in states with such laws. This effect is seen across all loan types and is stronger for ARMs and hybrid loans. We also observe reductions for loans with balloon payments, although the magnitude is smaller.

Other evidence indirectly supports the view that APL laws have been effective in limiting the spread of loans with untraditional characteristics. The use of adjustable rate, hybrid rate, balloon payment, and interest only payment loans all decline between 16 and 30 percent. Thus, APL laws are associated with a reduction in the use of loans with characteristics that have been most

closely associated with the subprime market meltdown. Similarly, loans with low or no documentation, investor loans, and loans for second homes decline by 20-40 percent after an APL law comes into effect. These are loan attributes that have been associated with increased default rates in subprime (Pennington-Cross and Ho, 2010; Danis and Pennington-Cross, 2008; Ding, Quercia, and Ratcliffe, 2008). In addition, borrower credit scores are slightly higher.

At the same time, Table 2 offers evidence of product substitution to facilitate the flow of mortgage credit. Most significant in this regard is the lengthening and deepening of teaser interest rates for ARMs. For basic ARMs, teasers were one month longer and 21 basis points higher upon the introduction of an APL law. For interest only ARMs, the effects were far more dramatic with teaser terms extending by 6 months and teaser sizes increasing by 123 basis points. We also observe a significant rise in the likelihood of fixed rate interest only mortgages, which were relatively rare previously.

Tables 3 through 5 conducts the same empirical investigation but on different sub-samples that may have different response patterns. Overall, the results are remarkably qualitatively consistent. While the qualitative story remains the same, there is some variation in the magnitude of effects across the subsamples. For example, for Alt-A loans the introduction of a law is associated with much teaser rates that last a longer period of time and are bigger or deeper (teaser size is defined as the fully adjusted rate minus the initial rate).<sup>11</sup>

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<sup>11</sup> These are loans that have been marketed as Alt-A loans to distinguish them from the rest of subprime. They typically document less and have higher credit scores to compensate, but in practice many loans in Alt-A pools look very similar to subprime loans along many dimensions.

The results for particular subsamples, using the first liens (Table 3) and purchase (Table 4) subsamples, produce only minor differences from the basic results in Table 2. By contrast, other subsamples show interesting divergences. For example, within the subprime subsample (Table 3), loan to value ratios are higher and interest only mortgages are more likely upon the introduction of an APL law. These indicators might suggest stronger product substitution among subprime loans. A higher LTV ratio makes it easier for borrowers to qualify for loans and interest only mortgages are typically more affordable.

In the Alt-A subsample (Table 4), aside from the general basic findings, APL laws reduce the likelihood that Alt-A loans will be used by borrowers who occupy or intend to occupy homes. Since almost all other results show a decrease in investor lending, one interpretation is that the APL moved owner-occupiers away from the Alt-A market and into other market segments.

The lone subsample with large divergences from the basic results is the refinance subsample (Table 5). The teaser and most prepayment penalty results are similar in this subsample. However, we find insignificant coefficients for adjustable rate mortgages, hybrid rate mortgages, loans to investors, loans with no or low documentation, and prepayment penalties. While it is beyond the scope of this paper to provide definitive explanations for these results, some potential reasons include the fact that refinancing may be more relationship driven than home purchase lending and many subprime refinances are need based. For example, a borrower may be refinancing into another loan to avoid going into default on a current loan or extracted equity to cover a variety of other reasons. The use of the extracted equity could include other debt

obligations such as credit cards (consumption), fixing up the house (capital investment), or paying for education (human capital investment).

Finally, the findings reveal that several states experienced a dramatic decline in the incidence of prepayment penalties due to prepayment restrictions in APL laws. These states offer the most direct observation and cleanest test of how the loan mix shifts in the wake of binding legal requirements.<sup>12</sup> Table 5 presents the results using a sample limited to observations from “prepayment restricted” states. In interpreting these results, some caution is warranted because of the limited number of law samples involved. Given the restricted sample, it is not surprising that prepayment penalties are far less common in treatment locations. We also observe reduced likelihoods for adjustable rate, interest only, and hybrid rate mortgages in prepayment restricted states. Effects on teaser features vary from the basic results in that teaser depth is no greater in treatment locations. In addition, unlike the strengthening of FICO scores in the basic results, we see no similar improvement in credit quality. Finally, loans with balloon payments are more common upon the introduction of an APL law. This warrants further study, but may reflect an attempt to keep payments low as a substitute for taking on illegal prepayment penalties. This result notwithstanding, consistent with the earlier findings, we observe evidence that APL laws were effective in reducing the use of some types of products.<sup>13</sup>

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<sup>12</sup> The sample is reduced to the treatment and control zip codes associated with the following state APLs -- Massachusetts, Maryland, District of Columbia, North Carolina, South Carolina, New Mexico, and New Jersey.

<sup>13</sup> Given the aggregated nature of the empirical approach, it is impossible to provide very precise measures of how mortgage use changed while holding borrower credit quality constant. However, we created two subsamples to provide some preliminary results. The first sample restricts the observations to only those loans with credit scores one standard deviation below the mean (FICO of less than 589). The second sample restricts the observations to those loans with LTVs one standard deviation above the mean (LTV>90). The results for these two subsamples, which include credit and down payment constrained borrowers, are very similar to Table 2 on most dimensions except the use of interest only loans. In particular, interest only loans tend to be used more frequently in both of the constrained subsamples (odds ratios of 2.79 for FICO less than 589 and 3.21 for LTV greater than 90) when a law is introduced.

### *Robustness Check: Geographic Sampling*

Tables 2 through 5 provide results using a sample of zip codes that were within 10 miles of the treatment state's border. The use of a ten mile threshold is a compromise. Smaller geographies produce a cleaner identification by restricting the sample to loans in the same labor and housing markets. However, the cost of using a 10-mile restriction is that many loans are excluded.

Using the specifications in Table 2, Table 6 explores both of these issues to evaluate the robustness of the results by repeating the analysis using 15- and 25-mile restrictions as well as using all loans. Most of the results are very similar regardless of the aggressiveness of the geographic sampling. For example, prepayment penalties are substantially reduced for all types of loans and the impact is the strongest for hybrid loans regardless of the sampling restrictions. Fixed rate interest only loans are also always used more often regardless of the sampling restrictions. The prevalence of adjustable rate, hybrid, and second home loans are also reduced under all the geographic samples. In terms of teasers, both the length (months the teaser exists until the first rate reset) and the size (fully adjusted rate – initial rate) are larger when a law comes into effect using the 10-mile, 15-mile, and 25-mile criteria. These sets of results are more inconsistent using the full state samples. Although at very small magnitudes, the results in general support higher credit scores and larger LTVs when a law comes into effect.

The results that differ the most are for loans with balloon payment features and loans with interest only payment features. For balloon payments the coefficient estimates alternate between positive and negative and are marginally significant and insignificant. Therefore, the balloon results do not appear to be robust. The interest only results show a distinct pattern. The interest

only results shift from negative and significant using 10 miles and turn positive using 15 miles and are positive and significant using 25 miles and the full state samples.

### **Discussion and Conclusion**

This research represents an extension of the growing literature seeking to understand how legal structures and restrictions influence the provision of mortgage credit, particularly subprime mortgage credit. Prior research has shown that the existence of laws influences the cost of subprime credit to a limited degree and also influences the flow of subprime credit. Very little is known about how lenders and borrowers react to these restrictions. The results described in this article take a first step toward improving our understanding whether borrowers seek and lenders offer alternative loan products and, if so, which loan characteristics and what types of borrowers become more or less common with the enactment of APL laws.

In terms of national trends, the data show significant variation in the loans that are used, both geographically and over time. We find that over the 1999-2006 time period there is a consistent pattern of product change designed to stretch income. Tools included adjustable rate loans, hybrid loans, interest only loans, and loans with balloon payments. This trend began in high cost areas, but by 2006 these products spread into other areas. Thus, the raw data suggest that product substitution was at least initially driven by targeted efforts to expand the market through increased affordability in affordability constrained states, and then morphed over time into a mechanism to increase the purchasing power for all borrowers and locations.

Over this same time period (1999 - 2006) states were increasingly regulating the provision of mortgage credit through APL laws.<sup>14</sup> A natural question is whether new laws encouraged the shift into more exotic or more affordable loan types or helped to retard it. Using difference-in-difference and sampling methods, we find evidence that APL laws, holding all else constant, reduced the incidence of loans with certain characteristics. The evidence shows a reduction in the likelihood of loans having prepayment penalties as well as a reduced incidence of adjustable rate, hybrid rate, balloon payment, interest only, and low and no documentation loans. The end result is that more fixed rate fully amortizing loans were originated in states that introduced APL laws.

Therefore, while in the aggregate the use of exotic mortgage types was rapidly increasing over the 1999-2006 time period, APL laws did retard this trend for many mortgage characteristics and promoted the more traditional fully amortizing fixed rate mortgage.

At the same time, the analysis finds clear evidence of product substitution. For the remaining adjustable rate loans teaser features both lengthened and deepened in locations with APL laws. We also find a significant rise in the likelihood of previously rare fixed rate interest only mortgages. These results suggest that a key driver in the market was making loans that were more affordable during the initial repayment period.

The analysis does not find any consistent evidence that the introduction of an APL law leads to any meaningfully “better” or more risk averse borrowers. In contrast, there is consistent evidence that borrowers who were purchasing as an investment or second home are less likely to be

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<sup>14</sup> The analysis in this paper is based on the laws as they stood in the sampling period. Since 2006, some states have amended their lending laws.

involved in the market. For example, using the overall sample results in Table 2, the odds of a loan being used to purchase a second home fell 38 percent and the odds of the borrower being an investor fell 18 percent.

The robustness of the results was verified across multiple subsamples created by partitioning loan types, mortgage motives, and legal environments. Though magnitudes of the effects varied using the various partitions, the qualitative results were largely confirmed. The lone exception to this involves the refinance subsample, in which there was a more limited responsiveness to the introduction of APL laws.<sup>15</sup>

These findings, which are based on state APL laws that were in effect between 1999 and 2006, have clear relevance for the on-going policy debate about how to best address predatory lending concerns. In particular, the Consumer Financial Protection Bureau created by the Dodd Frank Wall Street Reform and Consumer Protection Act, and many states will be considering laws and regulations governing loan terms and lending practices. The presence of product substitution suggests that legal restrictions based on fee and APR triggers may have limits in terms of their ability to protect borrowers and shield the market from problematic loan products. That noted, the evidence of APL law overall effectiveness argues that the pursuit of such legislative restrictions can move many homeowners toward more simple and amortizing fixed rate loans.

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<sup>15</sup> Additional tests (results available from the authors) looked for evidence that the introduction of a law could lead to higher costs. Overall, the results were inconsistent and largely insignificant for both fixed rate and hybrid loans.



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**Table 1: Summary Statistics**

<b>Mortgage Information</b>	<b>All Loans</b>	<b>Fixed Rate</b>	<b>Adjustable Rate</b>	<b>Interest Only</b>	<b>Balloon Payment</b>	<b>Hybrid</b>
Loan Amount (\$s)	\$177,792	\$137,976	\$211,981	\$288,109	\$124,123	\$211,223
Term (months)	332.3	300.4	359.7	355.6	238.9	360.4
Initial Interest Rate	8.13	8.64	7.69	6.66	9.56	7.71
LTV @ Origination (%)	81.6	83.0	80.5	78.9	89.6	80.5
FICO @ Origination	647.3	666.1	631.4	689.0	653.8	630.0
Prepay Penalty Flag (% of loans)	55.7%	43.9%	65.9%	49.5%	54.5%	66.3%
Prepay Penalty Length (months)	30.5	36.0	27.4	28.6	29.9	27.4
Purchase (% of loans)	45.0%	43.5%	46.4%	60.3%	59.5%	46.2%
Owner Occupied (% of loans)	87.5%	85.8%	88.9%	84.3%	92.4%	89.2%
Investor (% of loans)	10.6%	12.3%	9.2%	12.4%	5.8%	9.0%
Full Documentation (% of loans)	56.6%	56.3%	56.9%	42.5%	54.3%	57.4%
Low Documentation (% of loans)	40.5%	39.7%	41.2%	53.8%	43.3%	40.7%
No Documentation (% of loans)	1.9%	2.4%	1.4%	3.2%	1.1%	1.4%
Negative Amortization (% of loans)	0.44%		0.82%			0.51%
First Lien (% of loans)	84.5%	67.3%	99.2%	95.9%	42.0%	99.8%
FRM all (% of loans)	46.2%			17.7%	72.3%	
ARM all (% of loans)	53.8%			82.3%	27.7%	
IO (% of loans)	17.0%	6.5%	25.9%		0.0%	25.2%
IO ARM (% of loans)	14.0%		25.9%	82.3%	0.0%	25.2%
Balloon (% of loans)	11.7%	18.4%	6.1%	0.0%		6.2%
Balloon ARM (% of Balloon loans)	27.7%				27.7%	
Margin (ARM loans in percentage points)	2.9		5.4			5.5
Teaser Flag (% of ARM loans)	73.6%		73.6%			73.9%
Teaser Size (ARM loans in percentage points)	1.9		1.9			2.0
Teaser Length (ARM loans in months)	30.5		30.5			31.2
Hybrid Flag (% of ARM loans)	97.0%		97.0%			100.0%
Rate Floor = Initial Rate (% of ARM loans with Teasers)	94.9%		94.9%			95.0%

**Table 2: Basic Regression Results**

<b>Dependent Variable</b>	<b>Coefficient</b>	<b>SE</b>	<b><math>e^\beta</math></b>	<b>Laws</b>
<b><u>Basic Product Type</u></b>				
Adjustable Rate	-0.36***	0.10	0.70	22
Balloon Payment	-0.17*	0.10	0.84	11
Interest Only Payment	-0.25**	0.12	0.78	10
Hybrid Rate	-0.31***	0.10	0.74	22
<b><u>Balloon Type</u></b>				
15 to 30-Year	-0.20*	0.11	0.82	10
<b><u>Interest Only Type</u></b>				
Adjustable Rate	-0.05	0.09	0.95	9
Fixed Rate	0.50***	0.10	1.64	2
<b><u>Documentation</u></b>				
Full Documentation	-0.02	0.07	0.98	22
Low Documentation	-0.28***	0.03	0.76	22
<b><u>Occupancy</u></b>				
Owner	0.02	0.05	1.02	22
Second Home	-0.48***	0.09	0.62	3
Investor	-0.20*	0.12	0.82	15
<b><u>Prepayment Penalties</u></b>				
All Loans	-0.52***	0.17	0.59	22
Fixed Rate	-0.45***	0.10	0.64	22
Adjustable Rate	-0.96***	0.11	0.38	21
Hybrid Rate	-1.02***	0.11	0.36	21
<b><u>Teasers</u></b>				
ARM Teaser Length	0.82***	0.25	.	19
IO ARM Teaser Length	6.88***	0.50	.	3
ARM Teaser Size	0.21**	0.09	.	19
IO ARM Teaser Size	1.23***	0.08	.	3
<b><u>FICO &amp; LTV</u></b>				
FICO	1.73**	0.77	.	22
LTV	0.25	0.16	.	22

\*, \*\*, \*\*\* indicate significance at the 10%, 5%, and 1% level. Each row represents the results for a separate regression using the indicated locations or laws and dependent variables. If the odds ratio is reported the results are estimated in a grouped logit specification. If the odds ratio is not reported the results are estimated in ordinary least squares. This set of results includes all loans to calculate the fractions or the averages used as the dependent variables.

**Table 3: Regression Results using the First Lien and Subprime Subsamples**

<b>Dependent Variable</b>	<b>First Lien</b>				<b>Subprime</b>			
	<i>Coef.</i>	<i>SE</i>	$e^\beta$	<i>Laws</i>	<i>Coef.</i>	<i>SE</i>	$e^\beta$	<i>Laws</i>
<b><u>Basic Product Type</u></b>								
Adjustable Rate	-0.34***	0.10	0.71	22	-0.42***	0.11	0.66	17
Balloon Payment	-0.57***	0.11	0.56	4	-0.20*	0.11	0.82	11
Interest Only								
Payment	-0.24**	0.13	0.79	8	1.87***	0.14	6.51	3
Hybrid Rate	-0.26***	0.10	0.77	22	-0.34***	0.11	0.71	17
<b><u>Balloon Type</u></b>								
15 to 30-Year	-0.77***	0.13	0.46	2	-0.21*	0.11	0.81	9
<b><u>Interest Only Type</u></b>								
Adjustable Rate	-0.03	0.16	0.97	8	-2.29***	0.14	0.10	3
Fixed Rate	0.57***	0.09	1.78	2	.	.	.	
<b><u>Documentation</u></b>								
Full Documentation	-0.08	0.08	0.93	22	-0.16**	0.08	0.85	22
Low Documentation	-0.22**	0.10	0.81	20	-0.19*	0.11	0.83	18
<b><u>Occupancy</u></b>								
Owner	0.01	0.05	1.01	22	-0.04	0.05	0.96	22
Second Home	-0.47***	0.10	0.62	1	.	.	.	
Investor	-0.16	0.12	0.85	14	-0.27**	0.13	0.77	12
<b><u>Prepayment Penalties</u></b>								
All Loans	-0.64***	0.19	0.53	22	-1.30***	0.21	0.27	22
Fixed Rate	-0.61***	0.10	0.54	22	-0.75***	0.10	0.47	22
Adjustable Rate	-0.99***	0.12	0.37	20	-0.96***	0.13	0.38	20
Hybrid Rate	-1.04***	0.11	0.35	20	-1.00***	0.13	0.37	20
<b><u>Teasers</u></b>								
ARM Teaser Length	0.80***	0.26	.	20	0.81***	0.24	.	20
IO ARM Teaser								
Length	7.32***	0.50	.	3	5.11***	0.40	.	3
ARM Teaser Size	0.19**	0.09	.	20	0.19**	0.09	.	20
IO ARM Teaser Size	1.30***	0.08	.	3	1.12***	0.08	.	3
<b><u>FICO &amp; LTV</u></b>								
FICO	0.92	0.80	.	22	2.42***	0.75	.	22
LTV	0.01	0.16	.	22	0.34**	0.16	.	22

\*, \*\*, \*\*\* indicate significance at the 10%, 5%, and 1% level. Each row represents the results for a separate regression using the indicated locations or laws and dependent variables. If the odds ratio is reported the results are estimated in a grouped logit specification. If the odds ratio is not reported the results are estimated in ordinary least squares. The first lien subsample set of results includes only first lien loans to calculate the fractions or the averages used as the dependent variables. The subprime subsample set of results includes only subprime loans to calculate the fractions or the averages used as the dependent variables.

**Table 4: Regression Results using the Alt-A and Purchase Subsamples**

Dependent Variable	Alt-A				Purchase			
	Coef.	SE	$e^\beta$	Laws	Coef.	SE	$e^\beta$	Laws
<b><u>Basic Product Type</u></b>								
Adjustable Rate	0.00	0.17	1.00	11	-0.33**	0.15	0.72	16
Balloon Payment	.	.	.		-0.03	0.14	0.98	8
Interest Only								
Payment	-0.54***	0.18	0.58	7	-0.10	0.15	0.91	12
Hybrid Rate	-0.10	0.17	0.91	11	-0.31**	0.15	0.73	16
<b><u>Balloon Type</u></b>								
15 to 30-Year	.	.	.		-0.05	0.14	0.95	8
<b><u>Interest Only Type</u></b>								
Adjustable Rate	-0.51	0.92	0.60	6	-0.96***	0.16	0.38	6
Fixed Rate	0.81***	0.14	2.26	2	0.36***	0.12	1.43	1
<b><u>Documentation</u></b>								
Full Documentation	0.11	0.17	1.11	14	0.11	0.12	1.12	21
Low Documentation	-0.44**	0.16	0.64	15	-0.42***	0.14	0.66	16
<b><u>Occupancy</u></b>								
Owner	-0.34**	0.16	0.71	16	0.03	0.09	1.03	21
Second Home	.	.	.		.	.	.	
Investor	-0.12	0.18	0.89	12	-0.51***	0.16	0.60	13
<b><u>Prepayment Penalties</u></b>								
All Loans	-0.23	0.15	0.80	9	-1.11***	0.19	0.33	16
Fixed Rate	-0.04	0.15	0.96	9	-0.61***	0.13	0.54	11
Adjustable Rate	-1.13***	0.16	0.32	9	-1.43***	0.14	0.24	16
Hybrid Rate	-1.03***	0.15	0.36	9	-1.43***	0.14	0.24	16
<b><u>Teasers</u></b>								
ARM Teaser Length	9.35***	0.91	.	3	0.79**	0.37	.	15
IO ARM Teaser								
Length	7.17***	1.01	.	2	5.06***	0.62	.	3
ARM Teaser Size	1.77***	0.16	.	3	0.21*	0.13	.	15
IO ARM Teaser Size	1.24***	0.13	.	2	1.05	0.11	.	3
<b><u>FICO &amp; LTV</u></b>								
FICO	1.91*	1.13	.	22	0.46	1.21	.	22
LTV	-0.42	0.35	.	22	0.13	0.18	.	22

\*, \*\*, \*\*\* indicate significance at the 10%, 5%, and 1% level. Each row represents the results for a separate regression using the indicated locations or laws and dependent variables. If the odds ratio is reported the results are estimated in a grouped logit specification. If the odds ratio is not reported the results are estimated in ordinary least squares. The Alt-A subsample set of results includes only Alt-A loans to calculate the fractions or the averages used as the dependent variables. The purchase subsample set of results includes only purchase loans to calculate the fractions or the averages used as the dependent variables.

**Table 5: Regression Results using the Refinance and Prepayment Restricted Subsamples**

<b>Dependent Variable</b>	<b>Refinance</b>				<b>Prepayment Restricted States</b>			
	<i>Coef.</i>	<i>SE</i>	$e^\beta$	<i>Laws</i>	<i>Coef.</i>	<i>SE</i>	$e^\beta$	<i>Laws</i>
<b><u>Basic Product Type</u></b>								
Adjustable Rate	-0.18	0.11	0.84	20	-0.37***	0.14	0.69	5
Balloon Payment	-0.34***	0.11	0.71	6	0.55***	0.15	1.73	3
Interest Only								
Payment	-0.29**	0.14	0.75	5	-0.29*	0.16	0.75	4
Hybrid Rate	-0.15	0.11	0.86	20	-0.32**	0.14	0.73	5
<b><u>Balloon Type</u></b>								
15 to 30-Year	-0.48***	0.11	0.62	6	0.42**	0.15	1.52	3
<b><u>Interest Only Type</u></b>								
Adjustable Rate	-0.35	0.16	0.71	6	-0.40	0.38	0.67	4
Fixed Rate	0.52***	0.09	1.67	1	0.71***	0.10	2.03	1
<b><u>Documentation</u></b>								
Full Documentation	-0.02	0.09	0.98	22	0.06	0.08	1.06	5
Low Documentation	-0.17	0.11	0.84	18	-0.37***	0.12	0.69	4
<b><u>Occupancy</u></b>								
Owner	-0.06	0.06	0.95	22	-0.02	0.07	0.98	5
Second Home	.	.	.		-0.51***	0.10	0.60	1
Investor	-0.16	0.13	0.85	13	-0.54***	0.18	0.58	4
<b><u>Prepayment Penalties</u></b>								
All Loans	0.03	0.20	1.03	21	-2.89***	0.36	0.06	5
Fixed Rate	-0.45***	0.11	0.64	21	.	.	.	
Adjustable Rate	-1.06***	0.12	0.35	21	-3.44***	0.18	0.03	4
Hybrid Rate	-1.13***	0.12	0.32	21	-3.51***	0.18	0.03	4
<b><u>Teasers</u></b>								
ARM Teaser Length	0.39	0.30	.	18	0.69*	0.38	.	5
IO ARM Teaser								
Length	6.13***	0.50	.	3	9.41***	0.61	.	1
ARM Teaser Size	0.05	0.11	.	18	0.05	0.13	.	5
IO ARM Teaser Size	1.26***	0.08	.	3	.	.	.	
<b><u>FICO &amp; LTV</u></b>								
FICO	2.34**	0.91	.	22	1.62	1.14	.	5
LTV	0.43**	0.20	.	22	0.20	0.24	.	5

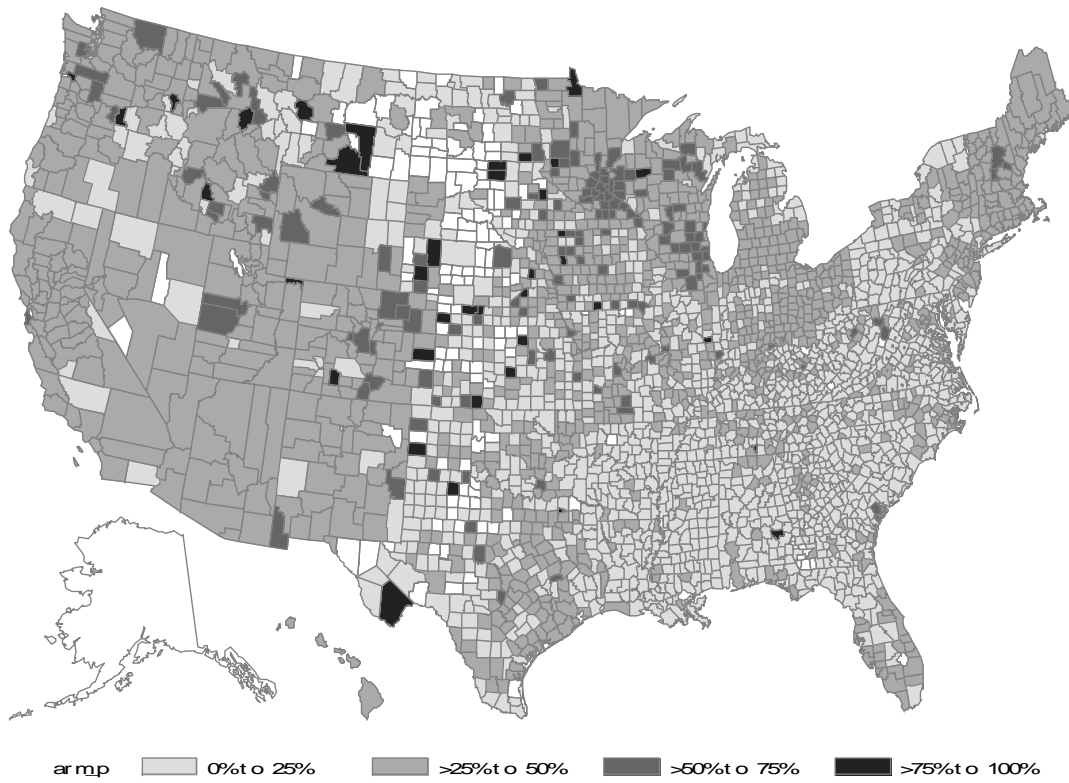
\*, \*\*, \*\*\* indicate significance at the 10%, 5%, and 1% level. Each row represents the results for a separate regression using the indicated locations or laws and dependent variables. If the odds ratio is reported the results are estimated in a grouped logit specification. If the odds ratio is not reported the results are estimated in ordinary least squares. The refinance subsample set of results includes only refinance loans to calculate the fractions or the averages used as the dependent variables. The prepayment restricted states subsample set of results includes only loans in states where prepayment s are strongly restricted to calculate the fractions or the averages used as the dependent variables.

**Table 6: Regression Results Using Different Geographic Radius**

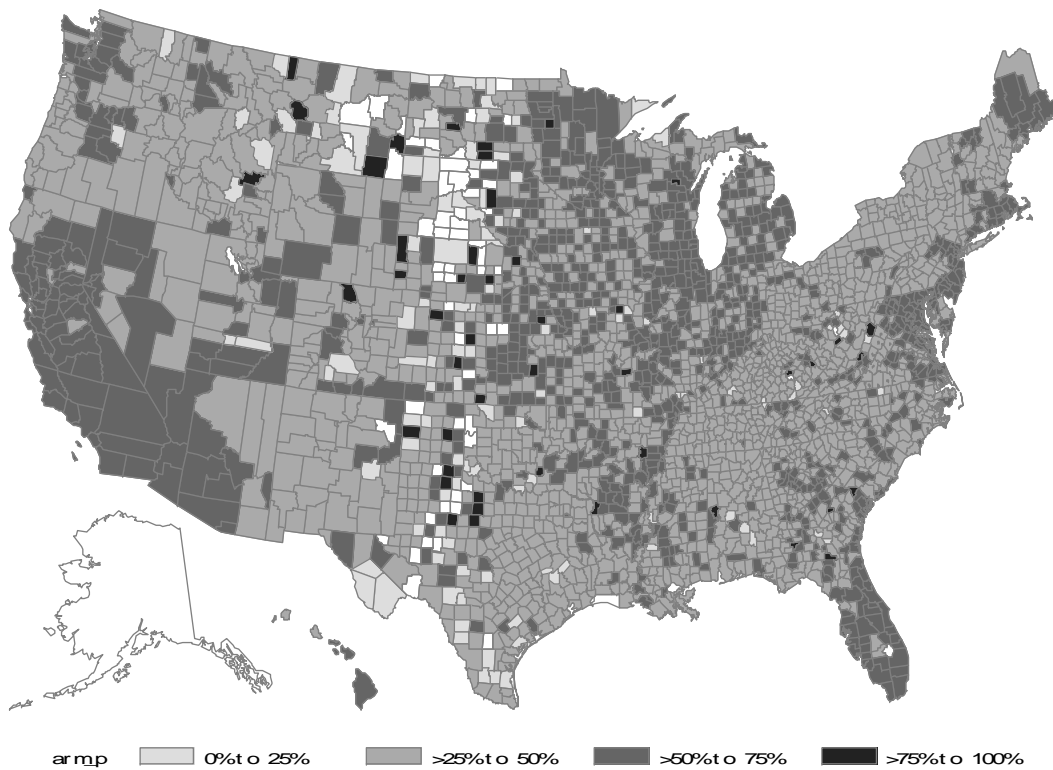
<b>Dependent Variable</b>	<b>15 Miles</b>			<b>25 Miles</b>			<b>Full States</b>		
	<i>Coefficient</i>	<i>SE</i>	<i>Laws</i>	<i>Coefficient</i>	<i>SE</i>	<i>Laws</i>	<i>Coefficient</i>	<i>SE</i>	<i>Laws</i>
<b><u>Basic Product Type</u></b>									
Adjustable Rate	-0.19**	0.08	23	-0.32***	0.06	24	-0.18***	0.03	25
Balloon Payment	0.13	0.10	16	-0.16**	0.07	17	0.07*	0.03	25
Interest Only Payment	0.12	0.09	12	0.14**	0.06	13	0.99***	0.03	23
Hybrid Rate	-0.16**	0.08	23	-0.47***	0.06	24	-0.15***	0.03	25
<b><u>Balloon Type</u></b>									
15 to 30-Year	0.07	0.09	16	-0.02	0.07	19	0.03	0.03	25
<b><u>Interest Only Type</u></b>									
Adjustable Rate	0.17**	0.07	11	0.26***	0.05	14	0.73	0.09	23
Fixed Rate	0.60***	0.08	2	0.36***	0.04	4	0.13***	0.02	15
<b><u>Documentation</u></b>									
Full Documentation	-0.08	0.06	23	-0.13***	0.04	24	-0.02	0.02	25
Low Documentation	-0.12	0.08	23	-0.12**	0.08	24	-0.28***	0.03	25
<b><u>Occupancy</u></b>									
Owner	0.00	0.04	23	-0.02	0.03	24	0.03***	0.01	25
Second Home	-0.25***	0.08	5	-0.10**	0.05	12	-0.16***	0.02	25
Investor	-0.10	0.10	18	-0.23***	0.07	21	0.03	0.03	25
<b><u>Prepayment Penalties</u></b>									
All Loans	-0.58***	0.14	23	-0.79***	0.11	24	-2.17***	0.04	25
Fixed Rate	-0.59***	0.08	22	-0.65***	0.07	24	-0.77***	0.03	25
Adjustable Rate	-0.83***	0.09	23	-0.99***	0.08	24	-1.39***	0.03	25
Hybrid Rate	-0.88***	0.09	23	-1.02***	0.08	24	-1.42***	0.03	25
<b><u>Teasers</u></b>									
ARM Teaser Length	1.00***	0.21	23	0.81***	0.17	24	-0.64***	0.08	25
IO ARM Teaser Length	2.68***	0.48	4	0.32	0.31	7	2.18***	0.10	18
ARM Teaser Size	0.22***	0.07	23	0.11*	0.06	24	-0.35***	0.02	25
IO ARM Teaser Size	0.59***	0.07	4	0.28***	0.05	7	0.47***	0.02	18
<b><u>FICO &amp; LTV</u></b>									
FICO	0.63	0.65	23	1.55***	0.52	24	-0.14	0.25	25
LTV	0.32**	0.13	23	0.70***	0.11	24	0.03	0.05	25

\*, \*\*, \*\*\* indicate significance at the 10%, 5%, and 1% level. Each row represents the results for a separate regression using the indicated laws and dependent variable. If the odds ratio is reported it is a grouped logit specification. If the odds ratio is not reported it is an OLS specification. Just as in Table 2, these results include all loans to calculate the dependent variables.

**Figure 1: Percent of All Loans –Adjustable Rate  
1999**

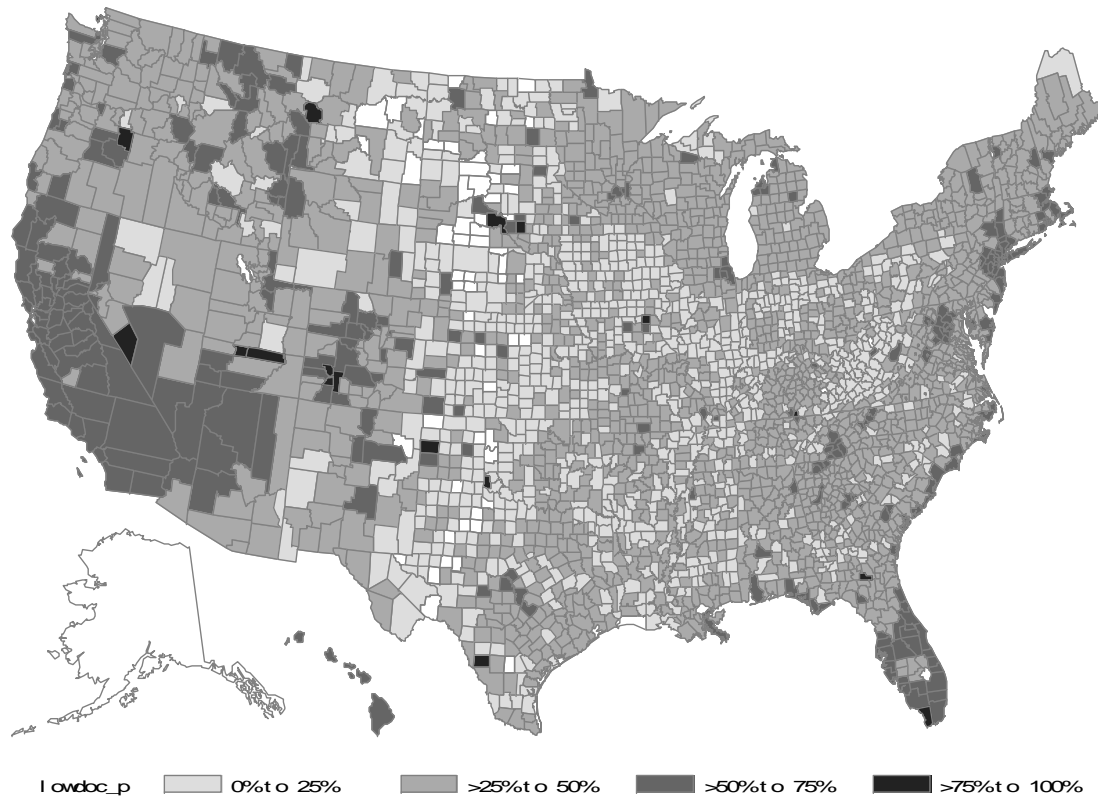


**2006**





**Figure 2: Percent of All Loans – Low Documentation in 2006**



**Figure 3: Percent of All Loans – Prepayment Penalty in 2006**

