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State and local anti-predatory lending laws: The effect of legal enforcement mechanisms

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Abstract

Subprime mortgage lending has grown rapidly and so has enactment of state anti-predatory lending laws. Our analysis suggests that anti-predatory lending laws influence subprime market dynamics and that disaggregating them into component parts is essential for understanding their market impact. Restrictions, coverage, and enforcement provisions all have significant relationships with subprime outcomes, the latter being a new finding. One finding, that broader coverage is associated with higher subprime origination likelihoods, is consistent with a reverse lemons hypothesis. There is also evidence that newer mini-HOEPA laws affect the subprime market above and beyond the older preexisting laws, particularly for subprime originations.

Keywords: Subprime lending; Enforcement mechanisms; Anti-predatory lending laws; Mortgage lending; Homeownership

1. Introduction

By extending credit to borrowers with blemished credit, subprime loans can enhance welfare; the recent growth in subprime lending (Duncan, 2006) suggests such broad welfare gains.

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However, subprime loans can also reduce welfare benefits. Concerns about lending abuses and predatory lending have increased dramatically with the rise in subprime lending (Apgar & Duda, 2005; Engel, 2006; Engel & McCoy, 2007; Quercia, Cowan, & Moreno, 2005). In response, since 1994 over half the states and the federal government have enacted laws to address subprime loan abuses. These laws are in addition to older laws that regulate isolated practices associated with predatory lending, such as prepayment penalties.

The effect of these statutes is a matter of debate. Critics charge that the laws ration credit and increase the price of subprime loans. Supporters argue that regulation is needed to allay consumer fears about dishonest lenders and ensure that creditors internalize the cost of any negative externalities from predatory loans. Recently, this debate has sharpened with the rapid rise in subprime delinquencies (Mortgage Bankers Association, 2007).

The great variety of approaches taken by individual states to regulate lending – in terms of the loans they cover, the restrictions they impose, and the enforcement mechanisms they establish – permits the testing of the influence of state laws on the flow and cost of credit. Prior studies of these laws have yielded mixed results. There are also questions about the accuracy of their findings and the generalizations that can be drawn from them. This paper seeks to address some of the weaknesses in these earlier studies in order to produce sharper results that improve the understanding of this increasingly important issue. In particular, this study considers a broader set of statutes than prior research, which allows for a more comprehensive, and more accurate, characterization of the legal environment. Moreover, this study takes into account enforcement mechanisms of anti-predatory lending laws (APLs) that have not previously been examined in any detail, and thus broadens and deepens our understanding of the effects of government enforcement, private rights of action, and assignee liability provisions.

Our hypothesis is that law design matters and that consumers and lenders may react to lending laws in unanticipated ways. While laws can obviously decrease the demand and supply of subprime funds to consumers, they can increase them as well. For example, from an institution's perspective, passage of a law can reduce legal uncertainty thereby making lending less risky and counteracting reductions associated with restrictions and legal enforcement mechanisms designed to reduce the flow of subprime credit. From the consumer perspective, absent APLs some borrowers may fear that lenders will take advantage of them. Passage of a law may reduce these fears and actually stimulate demand for subprime loans.

Our results comport with the view that APLs influence subprime lending markets and that disaggregating the details of the overall legal framework into its component parts is essential for understanding subprime market dynamics. The restrictions, coverage, and enforcement components all have significant relationships with subprime market outcomes, with the coverage relationship found to be broadly consistent with the reverse lemons hypothesis put forward by Ho and Pennington-Cross (2007). The results also suggest that the newer laws have had an impact on the subprime market above and beyond the older preexisting laws, particularly for subprime originations. Broader coverage through these new laws is associated with higher origination likelihoods, while increased restrictions through the newer laws are associated with lower origination propensities.

This paper unfolds as follows. Part II outlines APLs applying to residential mortgages. Part III reviews empirical studies to date on the effect of these laws on home mortgage credit. Part IV describes the dataset and the design of the study and sets forth the results. Part V offers conclusions.

2. A brief sketch of anti-predatory lending laws

Since ancient times, governments have sought to regulate abusive loans. In the United States, states have regulated interest rates since the signing of the Constitution (Peterson, 2004). APLs differ from older usury laws by regulating loan practices beyond interest rates. While many view state APLs as a recent development, some states have regulated prepayment penalties and balloon clauses for decades.

In 1994, Congress enacted the first modern, comprehensive anti-predatory lending statute, the Home Ownership and Equity Protection Act (HOEPA). HOEPA regulates only "high-cost loans," which are loans exceeding one of the following two triggers (12 C.F.R. Section 226.32(a)(1), (b)(1)): (1) where the annual percentage rate (APR) at consummation exceeds the yield on comparable Treasury securities plus 8% (10%) for first-lien (subordinate-lien) loans; or (2) where the total points and fees exceed the greater of 8% of the total loan amount or $400 (subject to annual indexing). A study by the Office of Thrift Supervision concluded that HOEPA covers no more than 5% of subprime residential mortgages (Board of Governors of the Federal Reserve System, 2001). For those loans, HOEPA imposes substantive restrictions on lending terms and practices.

In the late 1990s, many states began adopting legislation to redress predatory lending. Some of these laws were patterned on older laws that pre-dated HOEPA.[1] Starting in 1999, states began taking a different tack by enacting statutes akin to HOEPA. North Carolina, the first state to adopt a true "mini-HOEPA" law, passed its statute in 1999. As of January 1, 2007, 29 states and the District of Columbia had mini-HOEPA statutes in effect.[2]

The mini-HOEPA laws display considerable variation. Most mini-HOEPA laws, but not all, lower one or both of the HOEPA triggers. Similarly, some states impose scant restrictions on covered loans while others impose heavier ones. Finally, enforcement provisions in mini-HOEPA laws vary considerably. Some laws only provide for government enforcement, while others afford injured borrowers the right to sue. Some laws authorize assignee liability, while others restrict private lawsuits to loan originators and assignees that do not qualify as holders-in-due course. Some laws authorize double or treble damages; others cap monetary relief in private lawsuits at compensatory damages only.[3]

Thirteen states with mini-HOEPA laws layered them on top of older predatory lending laws that are still in effect. These new mini-HOEPA statutes all supplement, rather than supplant, the older state laws.[4]

In total, only six states – Arizona, Delaware, Montana, North Dakota, Oregon, and South Dakota – had no mini-HOEPA statutes or other laws or regulations regulating prepayment penalties, balloon clauses, or mandatory arbitration clauses in residential mortgages as of January 1, 2007.

[1] Numerous states also enacted mortgage broker and banker licensing and regulation laws.

[2] 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.

[3] Some cities and counties passed anti-predatory lending ordinances of their own. All of these ordinances have been enjoined by court order, preempted by state legislation, or limited in coverage to lenders who contract with the cities. In most cases, these ordinances never took effect or if they did take effect, they did so only briefly.

[4] Federal law has preempted portions of these state laws at various times for certain types of lenders and loan products. See Bostic, Engel, McCoy, Pennington-Cross, and Wachter (2007) for more detail.

3. Literature review

Since the passage of North Carolina's APL in 1999, a number of studies have examined the effect of state mini-HOEPA laws[5] on an assortment of outcomes using nationwide data. For example, Li and Ernst (2006) used Loan Performance data on securitized subprime loans to analyze the effect of state laws from January 1998 through December 2004. The authors concluded that APLs reduced predatory loan terms, without reducing subprime originations (except in Georgia and New Jersey), compared with unregulated states. Nominal interest rates on mortgages stayed level or dropped in all states with APLs except Georgia and Virginia, compared with the controls.

Ho and Pennington-Cross (2006b) constructed a legal index and used Home Mortgage Disclosure Act (HMDA) data to examine the effect of APLs on the probability of subprime applications, originations, and rejections. They focused on border counties of adjacent states with and without APLs, rather than entire states, to help hold labor and housing markets constant. APLs had no effect on the probability of origination and only a scant negative effect on the probability of application, while reducing the likelihood of being rejected. Stronger restrictions reduced the likelihood of origination and application, but had no effect on the likelihood of rejection. Conversely, stronger coverage increased the likelihood of origination and application but not rejection, suggesting that APLs with lower triggers boost demand by reducing consumer fears about abusive lenders.

Ho and Pennington-Cross (2006a) used the same methodology to study the effect of APLs on the cost of credit. Using 2004 HMDA data, they found that subprime loans originated in states with APLs had lower APRs than loans in unregulated states. Increasing a law's strength had the same effect. In both cases, greater coverage rather than stronger restrictions explained the lower APRs in regulated states.[6]

Elliehausen, Staten, Steinbuks (2006) used a proprietary database of subprime loans originated by eight large lenders from 1999 to 2004. Either modeling the presence of an APL as a binary variable or using the Ho and Pennington-Cross index, the authors found that the presence of a law was associated with a decrease in total subprime originations. Additionally, they conducted an event study of pre-law observations to predict expected subprime originations for the post-law period. Six of the states with strong laws (by our estimation)[7] had weaker than-expected originations and the other five had originations that were stronger-than-expected or level. The authors construed these findings as evidence that lenders shifted lending from covered high-cost loans to uncovered loans in response to adoption of mini-HOEPA laws. The study does not address what role coverage played versus restrictions or whether any decrease in originations was due to a reduction in demand, supply, or both.

[5] For a more complete survey of research on the effects of APLs, see Bostic et al. (2007).

[6] A comparable analysis by the researchers using Loan Performance data from 1998 to 2005 yielded somewhat different results. APLs modestly increased nominal interest rates on fixed-rate loans, but lowered them on adjustable-rate loans. For both types of loans, laws with more restrictions displayed the same or higher spreads; laws with greater coverage displayed lower spreads.

[7] After doing our own legal research, we recalculated the 22 individual state rankings based on the coverage and restrictions indexes in this paper. Arkansas, California, Colorado, Connecticut, Maryland, Massachusetts, North Carolina, New Mexico, Okalahoma, South Carolina, and Utah had the strongest combined rankings for coverage and restrictions of the 22 states.

4. Taking a deeper dive: our study of the impact of anti-predatory lending laws

This study builds on the prior literature by conducting a national examination of the impact of all APLs, not just mini-HOEPA laws, on subprime mortgage markets. We focus on three outcomes—(1) the probability of applying for a subprime loan relative to a prime loan, (2) the probability of originating a subprime loan relative to a prime loan, and (3) the probability of a subprime application being rejected.

Our study distinguishes itself from its predecessors in several respects. Most significantly, it reflects a more comprehensive canvassing of primary legal sources to paint a complete picture of the landscape regarding APLs. This new collection effort has highlighted 16 states with APLs that were not identified in prior studies that used surveys of state law compiled by law firms or trade associations. Our study takes into account the laws in 41 states (including Washington, D.C.) while others considered at most 25. In addition, we examine both modern mini-HOEPA laws and older laws that address only one or two loan terms.

This new legal dataset allows this study to make two advances. First, the data allow us to create two separate indexes measuring the independent presence of older laws and newer mini-HOEPA laws. We can then determine whether mini-HOEPA laws had an effect above and beyond the older laws.

Second, the new data permit us to assess whether legal enforcement mechanisms affect individual and lender behavior, which was previously not possible. Among other things, the new data allow us to control for the presence and strength of assignee liability provisions in APLs. Assignee liability laws allow borrowers to sue (or at least raise defenses against) investors who bought their loans.

Our study uses HMDA data, which enables us to examine most residential mortgage lending nationwide. The advent of HMDA pricing data in 2004 permits us to distinguish prime from subprime loans based on pricing information, which was not possible in prior years. We thus limit our focus to 2004 and 2005 to capitalize on this enhanced measure.

We use two methods to identify subprime loans. First, we use a list of subprime lenders generated by HUD through industry trade publications, HMDA data analysis, and phone calls to confirm the extent of subprime lending (Scheessele, 1999). Second, we define loans reported by HMDA as having an APR/comparable Treasury yield spread of 3 percentage points (300 basis points) or higher as being subprime loans. Unfortunately, the spread is only reported on originated loans so we cannot directly examine the demand for loans through the application function. For observations in 2004, all empirical tests are repeated using either the HUD list definition for subprime loans (denoted as "List" in the tables) or the HMDA pricing rule to define subprime loans (denoted as "Price" in the tables). For loans originated in 2005, the HUD list was not yet available, so we report results only for originations, using the HMDA pricing rule alone.

While we model the presence of a law as a binary variable, as do other studies, we also consider how the strength, coverage, and enforcement mechanisms of APLs influence market outcomes. In addition, we exclude any jurisdiction whose mortgage lending laws changed in any material respect during the calendar year. Finally, we control for borrower and location characteristics such as unemployment rates, housing vacancies, population growth, and income.

4.1. Description of the data

4.1.1. Legal dataset and index

We created a legal index that refines and builds on an index previously created by Ho and Pennington-Cross (2006a). The Ho and Pennington-Cross index coded state and municipal laws along two dimensions: coverage and restrictions, each of which contained four measures. In creating their index, Ho and Pennington-Cross relied on a chart of APLs produced by a private law firm.

Like Ho and Pennington-Cross, our legal index contains four coverage and four restriction measures. Our index differs, however, in several respects. First, we analyzed the actual statutes governing lending in every state to identify APLs and did not limit ourselves to mini-HOEPA laws. Second, our index includes a third dimension – enforcement – that has two dimensions: assignee liability and enforcement methods against originators. Lastly, through our legal expertise, we were able to detect subtleties in the laws that led to a more refined index and more accurate coding of the laws. Table 1 fully describes the index.

In measuring the breadth of a law's coverage, we consider the types of loans that are covered by the law and any annual percentage rate and points and fee thresholds that trigger application of the law. Laws that are HOEPA-equivalents in terms of coverage are scored 0. Laws that govern all loans or that have no triggers receive the highest scores on the index.

The first measure in the coverage index is the type of loan covered by the law. Laws that apply to more loan types receive a higher score. For example, if a law does not cover Federal Housing Administration or Veteran's Administration loans, it is scored 1. In contrast, laws that apply to all types of loans receive a score of 4. The APR and points and fees measures are graduated with HOEPA serving as the floor. Thus, HOEPA equivalents are scored 0 and laws without triggers receive the highest possible scores. Those laws with triggers lower than HOEPA's are assigned intermediate scores based on where they fall relative to HOEPA.

The restrictions measure considers four features of APLs: limits on prepayment penalties, restrictions on balloon payments, requirements for credit counseling, and restrictions on loan terms that limit or bar borrowers' access to the courts. As with the coverage measures, the laws that impose the strongest restrictions receive the highest number of points on the restrictions index. A ban, for example, on prepayment penalties would code a 4 while a law that was silent on prepayment penalties would be a 0, with gradations between based on the timing of bans on prepayment penalties. A similar scale holds for limits on balloon payments.

Credit counseling requirements are similarly coded, with laws requiring counseling coded as a 2 and those with no counseling requirements coded as a 0. Those laws that only recommend counseling or that only require that borrowers be given notice of the right to or the value of credit counseling receive a 1. The final measure highlights state variation in lenders having power to require borrowers to submit any disputes arising from their loans to arbitration or that otherwise restrict borrowers' access to the courts. States with laws prohibiting such terms are scored a 2, those with partial restrictions receive a 1, and laws without any restrictions are coded as 0.

Under enforcement mechanisms, assignee liability measures the potential liability that purchasers or other assignees of mortgages have for wrongdoing by originators. This measure is important because over 80% of subprime home mortgages have been securitized in recent years (Engel & McCoy, 2007), raising concerns about assignee liability provisions impeding the availability of subprime credit or driving up the price of loans. Our measure assigns a 0 to laws that do not contemplate assignee liability unless the assignee had notice of the violation of the law

Table 1
Coding rules for scoring state-level anti-predatory lending laws

Coverage dimensions	
Loan type	0 = HOEPA equivalent 1 = law does not cover government loans 2 = law does not cover reverse and/or open-ended loans 3 = law does not cover business and/or construction loans 4 = law covers all loans
APR trigger for first lien mortgages	0 = HOEPA equivalent 1 = 7–8% plus the comparable Treasury security yield 2 = 6–7% plus the comparable Treasury security yield 3 = no APR trigger
APR trigger for subordinate mortgages	0 = HOEPA equivalent 1 = 9–10% plus comparable Treasury security yield 2 = 8–9% plus comparable Treasury security yield 3 = 6–8% plus comparable Treasury security yield 4 = no APR trigger
Points and fees trigger	0 = HOEPA equivalent (8% of loan amount or $400) 1 = 6–8% of the total loan amount 2 = 5–6% of the total loan amount 3 = less than 5% of the total loan amount 4 = no points and fees trigger
Restrictions dimensions	
Prepayment penalties	0 = no prepayment penalty restrictions 1 = bans all penalties 60–84 months after origination 2 = bans all penalties 36–42 months after origination 3 = bans all penalties 24 months after origination 4 = bans all prepayment penalties
Balloon payments	0 = no restriction 1 = no balloons allowed in first 7 years of loan 2 = no balloons allowed in first 10 years of loan 3 = no balloons allowed after 10 or more years of loan 4 = no balloons allowed
Credit counseling requirements	0 = credit counseling not required 1 = credit counseling recommended 2 = credit counseling is required
Limits on judicial relief/mandatory arbitration	0 = does not prohibit restrictions on judicial relief 1 = limits restrictions on judicial relief 2 = prohibits restrictions on judicial relief
Enforcement mechanisms	
Assignee liability	0 = no assignee liability for holders in due course 1 = only relief against assignees is defensive 2 = assignee liability only if no due diligence 3 = assignees subject to limited claims and defenses 4 = assignees are liable even if they exercise due diligence
Enforcement against originators	0 = state government enforcement only 1 = borrower recovery limited to compensatory relief 2 = borrower relief compensatory and punitive

before or at the time of the assignment or participated in the violation of the law. Laws that allow borrowers to raise defenses, including claims of recoupment, to foreclosure or collection actions brought by assignees receive a 1. Stronger laws that impose liability on assignees who fail to engage in due diligence designed to identify predatory loans are assigned a 2. Laws that allow for limited claims against assignees even if they engage in due diligence are scored a 3. This score also applies if assignees are liable for willful violations by originators. The highest score is reserved for laws that impose liability on assignees with no due diligence safe harbors.

The final enforcement measure details the variation in enforcement schemes and remedies against originators. Laws that can only be enforced by state governments (*i.e.*, that preclude private lawsuits) are given the lowest score. Those laws that allow private lawsuits for compensatory damages but not punitive relief are scored a 1, and those that allow private lawsuits with punitive relief are assigned a 2.

4.1.2. Creating state level law indexes

To obtain state level law indexes for each dimension, we convert subcomponent scores to a 0–1 scale and then roll them up into a consolidated dimension score. This score is then rescaled by dividing by the component average score to insure that the indexes are not overly representative in terms of absolute value or variance of any single subcomponent.[8] Thus, the average value of a component index is 1.

Lacking a generally accepted method for using the three component values to create a consolidated index, we build additive and multiplicative indexes to evaluate whether index construction drives results and, if so, how. The additive version takes the value of each component and adds them together (Index^{A}). To explore whether the interaction of the components is important and to obtain a better sense of the contingent nature of the components (for example, restrictions should only matter if they cover some of the market) a multiplicative index is also created. This index multiplies the three components (Index^{M}).[9] In addition, we create separate indexes for the "new" mini-HOEPA laws and the older laws as well as a combined index derived from their sum.

Tables 2–4 provide the indexes for each of the 50 states and the District of Columbia for the mini-HOEPA laws, the preexisting laws, and combined compilation. There is substantial variation in the laws across all components, but in general, it appears that laws with strong market coverage tend to have stronger restrictions and enforcement mechanisms. In addition, the mini-HOEPA and preexisting law components are only very weakly correlated (less than 10%).

4.2. Identification strategy

To identify the impact of APLs on the subprime market, most of the literature uses state-level analyses that focus on an indicator variable signifying whether a law is in effect. However, inclusion of all loans in a state implicitly assumes that loans made on opposite ends of a state are similar and that all parts of the state are experiencing similar economic conditions. These

[8] For illustration, consider the creation of the old law coverage index (Table 3) for Alabama. Because Alabama's score for loan purpose was a 2 and the maximum loan purpose score is 4, Alabama's converted loan purpose value is 0.5. The same procedure yields scores of 1 for the APR trigger first Lien, the APR trigger higher liens, and the points and fees trigger subcomponents. The sum of these values equals 3.5 for Alabama. This sum is then divided by the average coverage value for all states (2.068627), resulting in Alabama's coverage index value being 1.69 (3.5/(2.068627).

[9] Therefore, the average for Index^{A} is 3 by design, and the average for Index^{M} will deviate slightly depending on the distribution of scores. In addition, relative to Index^{A}, the standard deviation of the Index^{M} will be larger.

Table 2
New laws index—mini-HOEPA laws, 2004–2005

State	Coverage	Restrictions	Enforcement	Index^{A}	Index^{M}
Alaska	0.00	0.00	0.00	0.00	0.00
Alabama	0.00	0.00	0.00	0.00	0.00
Arizona	0.00	0.00	0.00	0.00	0.00
Arkansas	1.72	2.73	2.11	6.56	9.92
California	2.15	1.36	1.41	4.93	4.13
Colorado	0.43	1.64	2.11	4.18	1.49
Connecticut	0.86	1.91	2.11	4.88	3.47
Delaware	0.00	0.00	0.00	0.00	0.00
District of Columbia	3.74	1.91	2.11	7.75	15.05
Florida	0.00	1.64	2.11	3.75	0.00
Georgia	1.72	3.00	2.11	6.83	10.91
Hawaii	0.00	0.00	0.00	0.00	0.00
Idaho	0.00	0.00	0.00	0.00	0.00
Illinois	3.74	1.91	2.46	8.11	17.56
Indiana	1.29	3.00	2.46	6.76	9.55
Iowa	0.00	0.00	0.00	0.00	0.00
Kansas	0.00	0.00	0.00	0.00	0.00
Kentucky	0.86	2.18	2.81	5.86	5.29
Louisiana	0.00	0.00	0.00	0.00	0.00
Maine	0.00	0.55	2.46	3.01	0.00
Maryland	1.44	0.55	1.41	3.39	1.10
Massachusetts	2.15	3.82	2.46	8.44	20.26
Michigan	5.17	0.82	0.00	5.99	0.00
Minnesota	6.46	0.55	0.00	7.01	0.00
Mississippi	0.00	0.00	0.00	0.00	0.00
Missouri	0.00	0.00	0.00	0.00	0.00
Montana	0.00	0.00	0.00	0.00	0.00
Nebraska	0.00	0.00	0.00	0.00	0.00
Nevada	0.00	0.00	2.81	2.81	0.00
New Hampshire	0.00	0.00	0.00	0.00	0.00
New Jersey	2.15	2.73	2.46	7.34	14.47
New Mexico	4.17	3.27	2.46	9.90	33.57
New York	2.15	1.91	1.76	5.82	7.23
North Carolina	1.72	3.27	1.41	6.40	7.94
North Dakota	0.00	0.00	0.00	0.00	0.00
Ohio	0.00	1.36	2.11	3.47	0.00
Oklahoma	0.00	2.18	2.11	4.29	0.00
Oregon	0.00	0.00	0.00	0.00	0.00
Pennsylvania	0.00	1.36	2.11	3.47	0.00
Rhode Island	0.00	0.00	0.00	0.00	0.00
South Carolina	0.86	2.18	1.76	4.80	3.31
South Dakota	0.00	0.00	0.00	0.00	0.00
Tennessee	0.00	0.00	0.00	0.00	0.00
Texas	0.86	1.36	2.11	4.34	2.48
Utah	1.72	2.18	0.00	3.91	0.00
Vermont	0.00	0.00	0.00	0.00	0.00
Virginia	0.00	0.00	0.00	0.00	0.00
Washington	0.00	0.00	0.00	0.00	0.00
West Virginia	5.60	1.64	1.76	9.00	16.12
Wisconsin	0.00	0.00	0.00	0.00	0.00
Wyoming	0.00	0.00	0.00	0.00	0.00

Table 2 (*Continued*)

State	Coverage	Restrictions	Enforcement	Index^{A}	Index^{M}
Average	1.00	1.00	1.00	3.00	3.61
Min	0.00	0.00	0.00	0.00	0.00
Max	6.46	3.82	2.81	9.90	33.57
Standard deviation	1.62	1.17	1.10	3.19	6.90

If all columns are zeros then no law was identified. ^{A}additive; ^{M}multiplicative.

assumptions are very unlikely to hold and are especially problematic when using data sets with missing information.

We use a different geographic sampling approach that focuses on lending activity along state borders. Loans are included in the sample if they originate in a county that is geographically along a state border and if at least one of the two abutting states has an APL. However, if the status of APLs in either state changes during the calendar year (*e.g.*, new laws become effective or old ones are amended), the loans are excluded from the sample for that year.

This approach takes advantage of the non-economic nature of state boundaries. Because economic forces are typically quite similar in such neighboring counties, border pairs reflect a strong natural experiment which makes differences in the legal framework a focal point. This recognition has made the border pair geographic sampling approach a standard in academic research on the effects of laws. As examples, it has been used by Holmes (1998) to establish the relationship between right-to-work laws and employment, Pence (2006) to study the impact of foreclosure laws on mortgage markets, and Ho and Pennington-Cross (2006b) to examine the effect of lending laws on mortgage markets.

Our approach is best described as a differences-in-differences identification approach, which is augmented through a geographic-based sampling approach. Index values establish APL variation, while each border pair incorporates regional variation through fixed effects (dummy variables). Remaining differences will be associated with control variables describing the borrower or applicant, the location of the property at the county and tract level, and the lender. The base model can thus be specified as:

$$\text{Outcome}_{it} = \beta_0 + \beta_1 \text{Law}_i + \sum_{j=\text{ALFL}}^{\text{VAWV}} \beta_{2j} \text{Border}_{ji} + \beta_3 \text{Borrower}_i + \beta_4 \text{Location}_i + \beta_5 \text{OCC}_i + \varepsilon_i \quad (1)$$

where i and j index, respectively, the individual loans and the state border pair, Law indicates whether the state where the loan originates has any APL law and the strength of the laws across various dimensions, Border indicates that loans are in border counties for the indicated pair of states (North Carolina and Virginia, NCVA, being the excluded state border pair), Borrower represents borrower characteristics, Location represents location specific characteristics, OCC identifies whether the lender was regulated by the Office of the Comptroller of the Currency (OCC), and ε represents an identically and independently distributed random error term. Because the outcomes are binary, we estimate all relationships using the logit specification.[10]

[10] The logit specification is given by $Pr(Y = 1) = e^{x_i\beta}/(1 + e^{x_i\beta})$, where Y is the outcome (application, origination, or rejection), x is a vector of explanatory variables as defined in the specification, and β is a vector of parameters to be estimated.

Table 3
Old law index—enacted before mini-HOEPA laws and in effect 2004–2005

State	Coverage	Restrictions	Enforcement	Index^{A} (additive)	Index^{M} (multiplicative)
Alaska	1.69	2.68	0.64	5.02	2.91
Alabama	1.57	2.68	2.57	6.82	10.82
Arizona	0.00	0.00	0.00	0.00	0.00
Arkansas	1.93	1.34	0.00	3.28	0.00
California	1.93	0.67	1.92	4.53	2.50
Colorado	1.45	2.68	1.92	6.06	7.49
Connecticut	1.81	1.34	0.00	3.15	0.00
Delaware	0.00	0.00	0.00	0.00	0.00
District of Columbia	1.93	1.34	1.28	4.56	3.33
Florida	0.00	0.00	0.00	0.00	0.00
Georgia	0.00	0.00	0.00	0.00	0.00
Hawaii	0.85	0.67	1.92	3.44	1.09
Idaho	1.81	1.34	2.57	5.72	6.24
Illinois	0.00	0.00	0.00	0.00	0.00
Indiana	1.93	1.34	1.92	5.20	4.99
Iowa	1.93	2.68	0.64	5.26	3.33
Kansas	1.93	2.68	2.57	7.18	13.32
Kentucky	1.57	0.67	1.92	4.17	2.03
Louisiana	1.81	0.67	0.00	2.48	0.00
Maine	1.57	2.68	1.92	6.18	8.12
Maryland	1.57	2.68	2.57	6.82	10.82
Massachusetts	0.00	0.00	0.00	0.00	0.00
Michigan	1.57	1.34	0.96	3.88	2.03
Minnesota	0.00	0.00	0.00	0.00	0.00
Mississippi	1.93	0.67	1.92	4.53	2.50
Missouri	1.81	0.67	1.92	4.41	2.34
Montana	0.00	0.00	0.00	0.00	0.00
Nebraska	1.93	0.00	0.64	2.58	0.00
Nevada	0.00	0.00	0.00	0.00	0.00
New Hampshire	0.00	0.00	0.00	0.00	0.00
New Jersey	0.00	0.00	0.00	0.00	0.00
New Mexico	1.93	2.68	2.57	7.18	13.32
New York	0.00	0.00	0.00	0.00	0.00
North Carolina	1.69	1.34	1.92	4.96	4.37
North Dakota	0.00	0.00	0.00	0.00	0.00
Ohio	1.93	0.67	1.92	4.53	2.50
Oklahoma	0.97	2.68	2.57	6.22	6.66
Oregon	0.00	0.00	0.00	0.00	0.00
Pennsylvania	0.00	0.00	0.00	0.00	0.00
Rhode Island	1.93	2.01	0.00	3.95	0.00
South Carolina	1.93	2.68	2.57	7.18	13.32
South Dakota	0.00	0.00	0.00	0.00	0.00
Tennessee	0.00	0.00	0.00	0.00	0.00
Texas	0.00	0.00	0.00	0.00	0.00
Utah	1.81	2.68	2.57	7.06	12.49
Vermont	1.57	2.68	1.92	6.18	8.12
Virginia	1.81	0.00	2.57	4.38	0.00
Washington	0.00	0.00	0.00	0.00	0.00
West Virginia	0.00	0.00	0.00	0.00	0.00
Wisconsin	0.00	0.00	0.00	0.00	0.00
Wyoming	0.85	2.68	2.57	6.10	5.83

Table 3 (*Continued*)

State	Coverage	Restrictions	Enforcement	Index^{A} (additive)	Index^{M} (multiplicative)
Average	1.00	1.00	1.00	3.00	2.95
Min	0.00	0.00	0.00	0.00	0.00
Max	1.93	2.68	2.57	7.18	13.32
Standard deviation	0.88	1.12	1.09	2.75	4.22

If all columns are zeros then no law was identified.

In the base model the vector Law only includes the variable Ineffect which indicates that the loan is in a state where an APL is effective. We lack a strong prior regarding the sign of the coefficient on Ineffect, as Ho and Pennington-Cross (2006b) and Li and Ernst (2006) show that such laws can have either positive or negative impacts on the volume of subprime lending but tend to depress rejection rates on average. In alternative model specifications, Law includes (1) the full index or (2) the coverage, restrictions, and enforcement mechanisms indexes. The full index is not expected to have much impact on the subprime mortgage market, because restrictions may depress the market by limiting the availability of some loan types, while coverage may stimulate demand in the market by making potential customers less fearful of predation. There is no prior evidence on the impact of enforcement mechanisms on the subprime mortgage market, but they may stimulate the market if potential customers are convinced that the law will eliminate any predation or depress the market if compliance is difficult or expensive.

Table 5 reports variable descriptions and the source for the borrower, location, and OCC controls. We expect that borrowers will be more likely to use subprime loans in locations with depressed economic conditions, in areas with a high percentage of minority households, and when applicants have low income or are requesting large loan amounts relative to income (Calem, Gillen, & Wachter, 2004; Pennington-Cross, 2002). We include OCC, designating the agency that regulates national banks, because the OCC has exempted national bank companies from state and local APLs, which suggests that law impacts may differ for national banks. Unfortunately, borrower credit score data are not publicly available despite the fact that credit score is an important factor in underwriting both prime and subprime loans.

Table 6 provides summary statistics for the estimation data set. For each of the potential outcomes, 200,000 loans are randomly sampled from the appropriate border counties for estimation to reduce estimation time. The 2004 data indicate that 16–18% of loan originations are classified as subprime, 33% of applications are subprime, and 44% of subprime applications are rejected. In addition, subprime borrowers tend to have substantially lower incomes, request higher loan-to-income ratios, and live in locations with more minorities, higher vacancy rates, higher unemployment rates, and lower density (subprime versus prime comparison not shown). The 2005 data show a substantially higher fraction of subprime loans than in 2004 (27% versus 16%). There are many small changes in the magnitude of all variables when comparing 2004 to 2005, but the summary statistics are very similar and these differences could reflect changes in the economic environment as well as the different composition of the sample.

4.3. Logit results

Table 7 presents the coefficient estimates, the standard errors, and the odds ratios for the base model in Eq. (1). Based on the coefficient and associated odds ratio for the Ineffect variable, the typical law appears to have no measurable impact on the odds of applying for or entering

Table 4
Complete or combined index, 2004–2005

State	Coverage	Restrictions	Enforcement	Index^{A} (additive)	Index^{M} (multiplicative)
Alaska	1.69	2.68	0.64	5.02	2.91
Alabama	1.57	2.68	2.57	6.82	10.82
Arizona	0.00	0.00	0.00	0.00	0.00
Arkansas	3.66	4.07	2.11	9.84	9.92
California	4.09	2.03	3.33	9.45	6.63
Colorado	1.88	4.32	4.03	10.24	8.98
Connecticut	2.67	3.25	2.11	8.04	3.47
Delaware	0.00	0.00	0.00	0.00	0.00
District of Columbia	5.67	3.25	3.39	12.31	18.38
Florida	0.00	1.64	2.11	3.75	0.00
Georgia	1.72	3.00	2.11	6.83	10.91
Hawaii	0.85	0.67	1.92	3.44	1.09
Idaho	1.81	1.34	2.57	5.72	6.24
Illinois	3.74	1.91	2.46	8.11	17.56
Indiana	3.23	4.34	4.39	11.96	14.54
Iowa	1.93	2.68	0.64	5.26	3.33
Kansas	1.93	2.68	2.57	7.18	13.32
Kentucky	2.43	2.85	4.74	10.02	7.32
Louisiana	1.81	0.67	0.00	2.48	0.00
Maine	1.57	3.23	4.39	9.19	8.12
Maryland	3.01	3.23	3.97	10.21	11.92
Massachusetts	2.15	3.82	2.46	8.44	20.26
Michigan	6.74	2.16	0.96	9.87	2.03
Minnesota	6.46	0.55	0.00	7.01	0.00
Mississippi	1.93	0.67	1.92	4.53	2.50
Missouri	1.81	0.67	1.92	4.41	2.34
Montana	0.00	0.00	0.00	0.00	0.00
Nebraska	1.93	0.00	0.64	2.58	0.00
Nevada	0.00	0.00	2.81	2.81	0.00
New Hampshire	0.00	0.00	0.00	0.00	0.00
New Jersey	2.15	2.73	2.46	7.34	14.47
New Mexico	6.10	5.96	5.03	17.08	46.89
New York	2.15	1.91	1.76	5.82	7.23
North Carolina	3.42	4.61	3.33	11.36	12.31
North Dakota	0.00	0.00	0.00	0.00	0.00
Ohio	1.93	2.03	4.03	8.00	2.50
Oklahoma	0.97	4.87	4.68	10.51	6.66
Oregon	0.00	0.00	0.00	0.00	0.00
Pennsylvania	0.00	1.36	2.11	3.47	0.00
Rhode Island	1.93	2.01	0.00	3.95	0.00
South Carolina	2.80	4.87	4.32	11.99	16.63
South Dakota	0.00	0.00	0.00	0.00	0.00
Tennessee	0.00	0.00	0.00	0.00	0.00
Texas	0.86	1.36	2.11	4.34	2.48
Utah	3.54	4.87	2.57	10.97	12.49
Vermont	1.57	2.68	1.92	6.18	8.12
Virginia	1.81	0.00	2.57	4.38	0.00
Washington	0.00	0.00	0.00	0.00	0.00
West Virginia	5.60	1.64	1.76	9.00	16.12
Wisconsin	0.00	0.00	0.00	0.00	0.00
Wyoming	0.85	2.68	2.57	6.10	5.83

Table 4 (*Continued*)

State	Coverage	Restrictions	Enforcement	Index^{A} (additive)	Index^{M} (multiplicative)
Average	2.00	2.00	2.00	6.00	6.56
Min	0.00	0.00	0.00	0.00	0.00
Max	8.40	6.50	5.38	17.08	46.89
Standard deviation	2.50	2.29	2.19	4.18	8.39

If all columns are zeros then no law was identified.

into a subprime loan. In addition, the typical law tends to reduce the odds of being rejected on a subprime application by 7%. In summary, the typical law has very little impact on the subprime mortgage market in terms of overall credit flows. A similar result is found when one uses the multiplicative in place of the additive index (results not shown). Most coefficients on this index were not significant and were of very small magnitudes when they were. In short, the results here corroborate the results in Ho and Pennington-Cross (2006b) and Li and Ernst (2006) showing that the existence of a law has a very small impact on the flow of subprime credit.

However, our hypothesis that the particulars of the legal framework are important suggests that further investigation is warranted. Table 8 reports results from a series of specification tests that aim to establish the accuracy of our conjecture. Each column reports the results for five alternative specifications for a given combination of dependent variables years, and subprime definitions. The first two specifications distinguish between the effects of older preexisting laws and newer mini-HOEPA laws. The results lend some weak support to the view that both sets of

Table 5
Variable definitions

Variable	Definition	Source
Outcome variables		
Origination (price ID)	Indicator variable = 1 for subprime origination; 0 for prime origination; subprime loans identified by reported APR spread	HMDA
Origination (list ID)	Indicator variable = 1 for subprime application; 0 for prime application; subprime loans identified by HUD subprime lender list	HMDA and HUD
Application	Indicator variable = 1 for subprime application; 0 for prime application; subprime loans identified by HUD subprime lender list	HMDA and HUD
Rejection	Indicator variable = 1 for rejected subprime application; 0 for accepted subprime application; subprime loans identified by HUD subprime lender list	HMDA and HUD
Control variables		
Income (thousands $)	Borrower's gross annual income (in thousands $)	HMDA
Loan2inc	Ratio of requested loan amount to borrower's income	HMDA
Minority	Tract's minority population percentage	HMDA
Population	County's percentage growth over the prior calendar year	Census Bureau
Vacant	County's percentage of vacant housing units	Census Bureau
Unemployment	County's unemployment rate	Bureau of Labor and Statistics
Density	Census tract population density (100s per square mile)	Census Bureau
OCC	Lending institution regulated by the Office of Currency Comptroller	HMDA

During estimation income is expressed in millions, all percentages are expressed as fractions.

Table 6
Descriptive statistics

Variable	2004				2005			
	Origination sample		Application sample		Rejection sample		Origination sample	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Outcome variables								
Origination (price ID)	0.16	0.36					0.27	0.45
Origination (list ID)	0.18	0.39						
Application			0.33	0.47				
Rejection					0.44	0.50		
Control variables								
Income (thousands $)	83.10	58.92	77.95	57.47	65.88	46.12	86.68	61.98
Loan2inc	2.22	1.37	2.25	1.52	2.37	1.66	2.19	1.42
Minority (%)	26.44	25.50	28.86	27.38	35.91	30.73	25.90	25.49
Population (%)	1.56	1.85	1.51	1.82	1.48	1.84	1.36	1.78
Vacant (%)	7.49	4.55	7.61	4.55	7.78	4.25	8.72	6.41
Unemployment (%)	5.26	1.46	5.35	1.48	5.54	1.46	5.02	1.52
Density	1.97	5.17	1.97	5.02	1.85	4.05	1.72	4.62
OCC (%)	27.29	44.54	23.20	42.21	2.86	16.65	24.66	43.10
Number of loans	200,000		200,000		200,000		200,000	

laws had an impact on subprime mortgage markets. The existence of the older law is associated with an increased probability of a subprime loan being originated in some cases and not in others. Similarly, results are mixed for the newer laws, indicating that the laws had no effect on or led to a slight decline in the probability of subprime origination. In all cases with significant coefficients, however, the magnitude of the effect is found to be quite small. Further, the similarity of results using the additive and multiplicative indexes suggests that the method of index construction is not driving the findings.

Of course, the aggregated index is a blunt measure. Two APLs can have the same full index but actually be very different, depending on the relative strength of the three index components. These variations can result in substantially different market responses. As a result, we can gain more meaningful insights by analyzing the effect of each component of the index.

Specification 3 shows the impact of the combined regulatory forces associated with the older laws and the more recent mini-HOEPA laws. The results show that disaggregating the laws into their component parts is important. For example, using the HUD list definition, the results for 2004 originations indicate that both restrictions and coverage rules have effects, and that these serve to negate each other. An increase in the restrictions component by one standard deviation (2.29 units) decreases the odds of originating a subprime loan by 9%, while an increase in the coverage component by one standard deviation (2.5 units) increases the odds of originating a subprime loan by 11%. Similar findings for combined restrictions and coverage are observed in 2005 using the HMDA price definition.

The results for 2004 originations using the HMDA price definition of subprime (second column) differ from the other two sets of originations results. However, there are reasons to discount the 2004 HMDA price data. Avery, Brevoort, & Canner (2006) provide evidence suggesting that many subprime loans were not identified as such in the 2004 data because of yield curve

Table 7
Logit base model results—origination, applications, and rejection probabilities

Dep. Var.	Origination prob.						Applications prob.		Rejection prob.	
Year	2004				2005		2004		2004	
Subprime def.	HUD list		HMDA price		HMDA price		HUD list		HUD list	
Variable	Estimate	Odds ratio	Estimate	Odds ratio	Estimate	Odds ratio	Estimate	Odds ratio	Estimate	Odds ratio
Intercept	−1.28** (0.07)	–	−0.77** (0.07)	–	−0.75** (0.06)	–	−0.60** (0.06)	–	−0.30** (0.05)	–
Ineffect	0.07 (0.05)	1.07	−0.06 (0.05)	0.95	0.03 (0.04)	1.03	0.05 (0.04)	1.05	−0.08* (0.04)	0.93
Income	−5.57** (0.16)	0.95	−8.31** (0.18)	0.92	−5.68** (0.12)	0.94	−4.64** (0.12)	0.95	−1.90** (0.12)	0.98
Loan2inc	−0.01* (0.00)	0.99	−0.27** (0.01)	0.76	−0.14** (0.00)	0.87	0.04** (0.00)	1.04	0.06** (0.00)	1.06
Minority	1.59** (0.03)	4.9	1.56** (0.03)	4.77	1.68** (0.02)	5.34	1.28** (0.02)	3.61	0.12** (0.02)	1.13
Vacant	−0.42 (0.22)	0.66	0.47* (0.21)	1.61	−0.09 (0.12)	0.91	−0.26 (0.17)	0.77	−0.37* (0.16)	0.69
Population	2.03** (0.64)	7.58	−1.42* (0.68)	0.24	0.83 (0.49)	2.30	0.47 (0.54)	1.61	−0.69 (0.49)	0.50
Unemployment	2.52** (0.72)	1.03	3.05** (0.72)	1.03	3.22** (0.51)	1.03	4.36** (0.62)	1.04	5.36** (0.53)	1.06
Density	−0.02** (0.00)	0.98	−0.03** (0.00)	0.97	−0.03** (0.00)	0.97	−0.02** (0.00)	0.98	0.00* (0.00)	1.00
OCC	−4.25** (0.06)	0.01		0.27	−1.09** (0.01)	0.34	−3.19** (0.03)	0.04	−0.53** (0.03)	0.59
Log likelihood	−78,948		−78,189		−107,182		−106,164		−134,143	

Note: standard errors are in parentheses. State–County border pair fixed effects included in all specifications. NCVA is the excluded border area. State–County border pair coefficient estimates are available on request from the authors.

* Significance at 5% level.

** Significance at the 1% level.

Table 8
Logit results for alternative model specifications

Dep. Var.	Origination prob.						Applications prob.		Rejection prob.	
Year	2004				2005		2004		2004	
Subprime def.	HUD list		HMDA price		HMDA price		HUD list		HUD list	
Specification variable	Estimate	Odds ratio	Estimate	Odds ratio	Estimate	Odds ratio	Estimate	Odds ratio	Estimate	Odds ratio
Specification 1										
Old Index^{A}	0.01 (0.01)	1.01	0.02** (0.01)	1.02	0.02** (0.00)	1.02	0.00 (0.00)	1.00	−0.01** (0.00)	0.99
New Index^{A}	0.00 (0.00)	1.00	0.00* (0.00)	1.00	0.00 (0.00)	1.00	−0.01** (0.00)	0.99	0.00 (0.00)	1.00
Specification 2										
Old Index^{M}	0.00 (0.00)	1.00	0.01** (0.00)	1.01	0.01** (0.00)	1.01	0.00 (0.00)	1.00	−0.01** (0.00)	0.99
New Index^{M}	−0.01** (0.00)	0.99	0.00 (0.00)	1.00	0.00 (0.00)	1.00	−0.01** (0.00)	0.99	0.00** (0.00)	1.00
Specification 3										
Combined restrictions	−0.04* (0.01)	0.96	0.04** (0.01)	1.04	−0.02* (0.01)	0.98	0.01 (0.01)	1.01	0.08** (0.01)	1.08
Combined coverage	0.04** (0.01)	1.04	0.00 (0.00)	1.00	0.03** (0.01)	1.03	−0.02** (0.01)	0.98	−0.08** (0.01)	0.92
Combined enforcement	0.01 (0.02)	1.01	−0.02 (0.02)	0.98	0.03* (0.01)	1.03	0.00 (0.01)	1.00	−0.03** (0.01)	0.97
Specification 4										
Old restrictions	−0.01 (0.02)	0.99	0.05** (0.02)	1.05	0.00 (0.01)	1.00	0.03* (0.01)	1.03	0.04** (0.01)	1.04
Old coverage	0.00 (0.03)	1.00	−0.02 (0.03)	0.98	0.03 (0.02)	1.03	−0.11** (0.03)	0.90	−0.08** (0.02)	0.93
Old enforcement	0.05 (0.03)	1.05	0.03 (0.02)	1.03	0.03* (0.02)	1.03	0.06** (0.02)	1.06	−0.03 (0.02)	0.97
New restrictions	−0.05* (0.02)	0.95	0.06** (0.02)	1.06	−0.05* (0.02)	0.95	0.03 (0.02)	1.03	0.13** (0.02)	1.14
New coverage	0.04** (0.01)	1.04	0.00 (0.01)	1.00	0.03** (0.01)	1.03	−0.02** (0.01)	0.98	−0.08** (0.01)	0.92
New enforcement	−0.01 (0.02)	0.99	−0.07** (0.02)	0.93	0.02 (0.02)	1.02	−0.05** (0.02)	0.95	−0.04* (0.02)	0.96

Note: standard errors are in parentheses. The unit of change when evaluating the odds ratio is one.

* Significance at 5% level.

** Significance at the 1% level.

dynamics. Another consequence is that many loans originated by specialized subprime lenders were not reported as such in the 2004 HMDA pricing data.[11] In addition, because 2004 was the first year institutions were required to report pricing information, errors in reporting are likely (Board of Governors of the Federal Reserve System, 2004). Indeed, the Federal Financial Institutions Examination Council issued a guidance alerting HMDA reporters of potential problems after an initial review of submissions (Federal Financial Institutions Examination Council, 2004).

The originations results for enforcement are weaker than those for restrictions or coverage. No relationship is observed between originations and enforcement in 2004 using either definition, but we do observe an elevated probability of subprime originations in states with stronger enforcement (most notably, private rights of action and assignee liability) in 2005.

Components are also significantly associated with applications and rejections (final columns of Table 8). In contrast to Ho and Pennington-Cross (2006a), here the coverage component is negatively associated with applications. All the components have a significant relationship with rejections. Applicants in states with stronger restrictions are more likely to have their applications rejected. However, greater coverage and stricter enforcement are associated with fewer rejections of subprime mortgage applications.

The fourth specification decomposes these variables further by separately analyzing the components of mini-HOEPA laws and the older laws that govern only one or a few loan terms. For originations, the evidence suggests that the mini-HOEPA laws have a greater effect than the older laws. These effects have the same offsetting pattern we observed for the aggregated component indexes. New restrictions are significantly associated with a decreased likelihood of subprime origination, while new laws featuring expanded coverage tend to increase the probability of a loan being originated.

For applications and rejections, the story is a bit different in that both the older and newer laws appear to play a role in subprime market outcomes. Both sets of laws are significantly associated with both applications and rejections and the component effects for both sets of laws are often quite similar. Expanded coverage is associated with reduced probabilities of both and restrictions are associated with higher probabilities of both (weakly for applications). Interestingly, new increased enforcement is significantly associated with decreased likelihoods of subprime applications and rejections.

Regarding the controls, as indicated in Table 7, the results in the probability of origination estimation are very consistent across different years and using different definitions of subprime. Put simply, a subprime origination is most likely in locations with high percentages of minority residents and people with low incomes, and in areas with high rates of unemployment in both 2004 and 2005. In terms of applications, borrowers in locations with a concentration of minority households tend to apply for subprime loans much more often (odds ratio = 3.61), as do households with lower incomes (odds ratio = 0.95) and those that live in places with higher unemployment rates (odds ratio = 1.04). The probability of being rejected on a subprime application also meets expectations. For example, lower incomes, higher unemployment rates, and living in a predominantly minority area all increase the odds of having a subprime application rejected.

[11] For example, Aames Funding Corporation reported that 55% of their loans were subprime in 2004 but 90% were subprime in 2005. This pattern repeats among many subprime specialists.

5. Discussion and conclusion

Our results are consistent with the view that APLs influence subprime lending markets. While the presence of state-level laws that regulate subprime lending, when considered in the aggregate, appears to have little impact on subprime originations, applications or rejections, disaggregating the details of the overall legal framework into its component parts is shown to be important for understanding the true subprime market dynamics. The results for the restrictions component are consistent with expectations and the prior literature. More restrictive state laws reduce subprime originations and increase subprime rejection likelihoods. Regarding coverage, although laws with broader coverage are associated with reduced subprime application propensities, they are also associated with lower subprime rejection rates. On balance, the reduction in rejection propensity appears to outweigh the application effect, as we observe higher subprime origination propensities in states with broader coverage. Finally, variation in enforcement provisions is not consistently associated with subprime origination or application likelihoods, but we do observe a significant reduction in rejection probabilities as enforcement methods strengthen.

In terms of other implications of the research, the component analysis suggests that the lack of an overall relationship is explained by the opposing restrictions and coverage effects. The evidence here argues strongly that the design of the legal framework is important for market outcomes. The results also suggest that the newer mini-HOEPA laws have had an impact on the subprime market above and beyond the older preexisting laws, particularly for subprime originations. Finally, the evidence that laws with broader coverage lead to an increased probability of applications is broadly consistent with a reverse lemons hypothesis. This theory argues that a legal framework can stimulate creditworthy applicants who had opted out of the market to apply for loans.

The results also point to the need for additional research. Questions remain as to how the interaction of the various legal framework components shapes subprime market outcomes. For example, restrictions and enforcement might be mutually reinforcing or might negate the effect of the other. Additional research might focus on how credit quality drives observed relationships, particularly in the context of subprime underwriting and rejection rates. Finally, the analysis indicated that OCC activity influences subprime market outcomes. Future research might explore whether different regulatory schemes for institutions, including OCC preemption, affect the distribution of applications and originations.

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