

## The Impact of State Anti-Predatory Lending Laws on the Foreclosure Crisis

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## ARTICLES

### THE IMPACT OF STATE ANTI-PREDATORY LENDING LAWS ON THE FORECLOSURE CRISIS

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*By the end of 2007, thirty states and the District of Columbia had passed some sort of subprime mortgage regulation statute, while the remaining states left the subprime mortgage market unregulated. Were these state mortgage laws effective in restraining risky mortgage lending and mitigating the surge in foreclosures? Our study takes advantage of this natural experiment and compares loan terms, mortgage foreclosures, and defaults in states with and without anti-predatory lending laws (APLs), using a sample of 1.2 million mortgage loans originated during the subprime boom and observed monthly through the end of 2008. Using these loan level data, we find that state APLs are associated with a 13% reduction in prepayment penalties and appear also to reduce the incidence of option adjustable-rate mortgage (ARM) loans under some conditions. The effects are particularly strong in the subprime market. APLs also reduce the risk of default, although results vary depending on the universe of loans analyzed and the specification of the APL. The degree of coverage and restrictions also seem to matter, with more stringent APLs having a stronger dampening effect on the origination of loans with riskier terms and subsequent default rates. This analysis, combined with other research, suggests that strong state APLs are an important tool for consumer protection and that federal preemption may have diluted their protective effect.*

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## INTRODUCTION

The unprecedented levels of mortgage defaults that led to the subprime crisis of 2007–2009 have been attributed in part to inadequate regulation of mortgage lending.<sup>1</sup> At the federal level, Congress fostered the growth of subprime and exotic mortgages by displacing state consumer protection and usury laws through federal preemption in the early 1980s.<sup>2</sup> However, a number of states, beginning with North Carolina in 1999, passed laws intended to restrain mortgage abuses, within the legal space left outside of Congressional preemption.<sup>3</sup> By the end of 2007, thirty states and the District of Columbia had passed some sort of mortgage regulation statute, while the remaining states left the mortgage market unregulated.<sup>4</sup> Were these state mortgage laws effective in restraining risky mortgage lending and mitigating the surge in foreclosures? Our study takes advantage of this natural experiment, and compares foreclo-

<sup>1</sup> See Patricia A. McCoy et al., *Systemic Risk Through Securitization: The Result of Deregulation and Regulatory Failure*, 41 CONN. L. REV. 1327, 1333 (2009); Benjamin S. Bernanke, Chairman, U.S. Fed. Reserve, Speech at the Annual Meeting of the American Economic Association: Monetary Policy and the Housing Bubble (Jan. 3, 2010), <http://federalreserve.gov/newsevents/speech/bernanke20100103a.htm>.

<sup>2</sup> Cathy Lesser Mansfield, *The Road to Subprime HEL was Paved with Good Congressional Intentions*, 51 S.C. L. REV. 473, 484–510 (2000).

<sup>3</sup> Rafael W. Bostic et al., *State and Local Anti-Predatory Lending Laws: The Effect of Legal Enforcement Mechanisms*, 60 J. OF ECON. & BUS. 47, 49 (2008).

<sup>4</sup> *Id.*

tures and defaults in states with and without anti-predatory mortgage laws (APLs), using a sample of 1.2 million mortgage loans originated during the subprime boom observed monthly through the end of 2008.

Using these loan level data, we find that state APLs are associated with a 13% reduction in prepayment penalties and appear also to reduce the incidence of option adjustable-rate mortgage (ARM) loans under some conditions. The effects are particularly strong in the subprime market. APLs also reduce the risk of default, although results vary depending on the universe of loans analyzed and the specification of the APL. The degree of coverage and restrictions also seem to matter, with more stringent APLs having a stronger dampening effect on the origination of loans with riskier terms and subsequent default rates.

The results demonstrate the importance of strong state law consumer protections in the mortgage market. Unfortunately, federal regulators excused some mortgage lenders from state oversight immediately preceding the boom in subprime lending. In 2004, the Office of the Comptroller of the Currency (OCC) joined the Office of Thrift Supervision (OTS) in ruling that nationally chartered banks—like the OTS-regulated thrifts—were not subject to these state regulations.<sup>5</sup> Consequently, preemption deprived some borrowers of additional protections that would have encouraged better underwriting and likely reduced the level of foreclosures.<sup>6</sup>

The proven record of success of state APLs should encourage additional states to adopt stricter mortgage regulations appropriate to their markets. However, more state legislation will only be marginally effective if federal regulators preempt it. The results of this study demonstrate that consumer protections should not be reduced to the least common denominator. Instead, reasonable federal regulation should act as a floor for consumer protection from which states may choose to build more protective legislation if desired based on local market conditions.

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<sup>5</sup> DAN IMMERGLUCK, FORECLOSED: HIGH-RISK LENDING, DEREGULATION, AND THE UNDERMINING OF AMERICA'S MORTGAGE MARKET 176–83 (2010); McCoy et. al., *supra* note 1, at 1349; Christopher Lewis Peterson, *Preemption, Agency Cost Theory, and Predatory Lending by Banking Agents: Are Federal Regulators Biting Off More Than They Can Chew?*, 56 AM. U. L. REV. 515, 529–31 (2007).

<sup>6</sup> Lei Ding et. al., *The Impact of Federal Preemption of State Anti-Predatory Lending Laws on the Foreclosure Crisis*, 41 J. OF POL'Y ANALYSIS AND MGMT. (forthcoming 2012) (prior version available at [http://www.ccc.unc.edu/documents/Preemption\\_final\\_August%2027.pdf](http://www.ccc.unc.edu/documents/Preemption_final_August%2027.pdf)).

## I. LITERATURE REVIEW

A. *Legal Environment: State High-Cost Mortgage Laws and Federal Preemption Before the Crisis*

Subprime mortgage lending was impossible before 1980 because of state usury laws that banned higher-rate mortgages.<sup>7</sup> Many states also imposed legal restrictions on adjustable-rate mortgages (ARMs), such as requiring annual and lifetime caps on upwards rate adjustments, and prohibiting negative amortization or prepayment penalties.<sup>8</sup> In 1980, however, Congress preempted all state restrictions on first mortgage interest rates and fees.<sup>9</sup> Then, in 1982, Congress responded to ARM restrictions with the Alternative Mortgage Transactions Parity Act of 1982 (AMTPA).<sup>10</sup> AMTPA overrode state laws restrictions on many terms in nontraditional mortgages—such as ARMs—including terms that came to be regarded as problematic after the subprime foreclosure crisis—such as balloon payments and negative amortization.<sup>11</sup> AMTPA also preempted state restrictions on prepayment penalties.<sup>12</sup>

AMTPA signaled a move towards federal mortgage market regulation in that it prohibited states from setting restrictions on certain loan products.<sup>13</sup> However, Congress did not replace state mortgage regulations with federal safeguards until 1994, when it enacted the Homeownership Equity Protection Act (HOEPA).<sup>14</sup> The primary abuses HOEPA sought to regulate were excessive fees (rent-seeking) and equity stripping.<sup>15</sup> The latter practice consists of repeated refinancings, together with inclusion in new loans of loan fees, credit insurance premiums, and other closing costs as well as additional cash advances to the homeowner. Because the problem was excessive loan rates and fees, HOEPA covered only mortgages with very high annual percentage rates (8% to 10% above Treasury rates) or very high prepaid points and fees (8% of the loan amount).<sup>16</sup> Although HOEPA also gave the Federal Reserve broad power to regulate mortgages below these price levels, the Federal Reserve did not exercise that authority until 2008.<sup>17</sup>

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<sup>7</sup> See KATHLEEN C. ENGEL & PATRICIA A. MCCOY, *THE SUBPRIME VIRUS: RECKLESS CREDIT, REGULATORY FAILURE AND NEXT STEPS* 16 (2011).

<sup>8</sup> See *id.*

<sup>9</sup> Mansfield, *supra* note 2, at 492–95.

<sup>10</sup> Pub. L. No. 97-320, 96 Stat. 1469 (codified at 12 U.S.C. §§ 3801–3806 (2006)).

<sup>11</sup> See McCoy et al., *supra* note 1, at 1333.

<sup>12</sup> *Id.*

<sup>13</sup> See *id.* at 1333–35.

<sup>14</sup> 15 U.S.C. § 1639 (2006).

<sup>15</sup> ENGEL & MCCOY, *supra* note 7, at 194–95.

<sup>16</sup> *Id.* at 194–95; Mansfield, *supra* note 2, at 562.

<sup>17</sup> Truth in Lending (Regulation Z), 12 C.F.R. § 226 (2008); see ENGEL & MCCOY, *supra* note 7, at 196.

The effect of HOEPA was not so much to constrain terms of expensive mortgages as to simply drive them out of the market. By 2005, while about one-quarter of all mortgages were subprime, based on the Federal Reserve's high-rate threshold, less than 1% of refinancing and home improvement mortgage loans had rates high enough to be covered by HOEPA.<sup>18</sup>

Some states passed their own APLs to extend HOEPA's coverage, but a complicated web of federal preemption statutes limited their reach.<sup>19</sup> In 1996, OTS adopted separate preemption regulations that covered all of their thrifts; in 2004, OCC followed suit, completely exempting federal lenders from coverage by state APLs.<sup>20</sup> The OTS and OCC preemption statutes also superseded a 2003 OTS ruling, which had lifted the federal AMPTA preemption of prepayment restrictions.<sup>21</sup> So, during the period covered by our study, 2004–2008, states could restrict prepayment penalties—but not other nontraditional mortgage loan terms on ARM and balloon mortgages—lenders with federal charters could continue to include prepayment penalties. As a result, APLs and federal preemption applied to both the type of lender and the nature of the mortgage terms. In practice, this meant that two different lenders—operating under different regulatory regimes and different levels of preemption—were potentially subject to different restrictions in the types of loans they could offer to subprime borrowers, even if they were both originating loans in the same state.

### 1. The Evolution of State APLs

In 1999, North Carolina enacted the first of the modern state anti-predatory lending laws. The North Carolina Predatory Lending Act, for the most part, followed the HOEPA approach of restricting mortgage terms for loans above a price threshold but set the price levels below those in HOEPA.<sup>22</sup> The law covered loans with points and fees exceeding 5%, rather than 8%.<sup>23</sup> Because the North Carolina law prohibited the

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<sup>18</sup> See Robert B. Avery et al., *Higher-Priced Home Lending and the 2005 HMDA Data*, 2006 FED. RES. BULL. A123, A146–47 (2006).

<sup>19</sup> See McCoy et al., *supra* note 1, at 1348.

<sup>20</sup> See *id.*

<sup>21</sup> Alternative Mortgage Transaction Parity Act of 1982; Preemption, 67 Fed. Reg. 60542 (Sep. 26, 2002); Patricia A. McCoy & Elizabeth Renuart, *The Legal Infrastructure of Subprime and Nontraditional Home Mortgages*, in *BORROWING TO LIVE: CONSUMER AND MORTGAGE CREDIT REVISITED* 424 n.35 (Nicolas P. Retsinas & Eric S. Belsky eds., 2008).

<sup>22</sup> North Carolina Predatory Lending Act, N.C. Gen. Stat § 24-1.1E (2009); see Christopher L. Peterson, *Federalism and Predatory Lending: Unmasking the Deregulatory Agenda*, 78 TEMP. L. REV. 1, 62 (2005).

<sup>23</sup> North Carolina Predatory Lending Act, N.C. Gen. Stat § 24-1.1E (2009).

financing of points and fees for covered loans, the result was a de facto usury cap.<sup>24</sup>

North Carolina's APL had a few restrictions applicable even to loans below the triggers. It prohibited prepayment penalties for all mortgages of less than \$150,000, prohibited certain repeated "flipping" refinancings, and banned the financing of single-premium credit insurance.<sup>25</sup> The Massachusetts, New Jersey, and New Mexico APLs followed this pattern, proscribing refinancing that did not provide a net tangible benefit to the homeowner under certain circumstances, for all mortgages, and prohibited financing of single-premium credit insurance.<sup>26</sup> The complex architecture of these state laws was driven partly by the limited regulatory space left to the states after the Depository Institutions Deregulation and Monetary Control Act's (DIDMCA)<sup>27</sup> preemption of direct interest and fee regulation, and AMPTA's preemption of most restrictions on adjustable-rate and balloon loan mortgages.<sup>28</sup>

A number of other states followed the North Carolina model but usually varied the coverage and nature of the restrictions so that no truly uniform state APL model emerged. At the same time, some cities and counties, such as Philadelphia, Cleveland, and Oakland, attempted to regulate subprime mortgages.<sup>29</sup> In these three instances, the lending industry sought protection from local regulation at the state level, and the three states enacted statutes intended primarily to preempt city and county regulation efforts.<sup>30</sup> The Pennsylvania and Ohio statutes transposed the coverage and restrictions of HOEPA to state law and, unlike North Carolina, did not reduce the high-cost coverage triggers or expand consumer protections beyond federal law.<sup>31</sup> Thus, their primary, if not sole, purpose was to prevent regulation of subprime lending at the local government level.<sup>32</sup> California, on the other hand, preempted local ordi-

<sup>24</sup> See *id.* § 24-1.1E(c)(3); see Peterson, *supra* note 22, at 62.

<sup>25</sup> N.C. Gen. Stat. §24-1.1A(b)(1), § 24-10.2(b), §24-10.2(c) (2009); see Peterson, *supra* note 22, at 62-63.

<sup>26</sup> Massachusetts Predatory Home Loan Practices Act, MASS. ANN. LAWS ch. 183C (2004); New Jersey Home Ownership Security Act of 2002, N.J. STAT. ANN. §§ 46:10B-23 to 46:10B-34 (2002); New Mexico Home Loan Protection Act, N.M. STAT. ANN. §§ 58-21A-1 to 21A-14 (2003).

<sup>27</sup> Pub. L. No. 96-221, 94 Stat. 132 (1980) (codified as scattered sections of 12 U.S.C. (2006)).

<sup>28</sup> See Peterson, *supra* note 22, at 64-67.

<sup>29</sup> OAKLAND, CA., CODE OF ORDINANCES 12361 § 2 (Oct. 2, 2001); CLEVELAND, OHIO., CITY ORDINANCES No. 737-02 (Mar. 4, 2002) amended at CITY ORDINANCES No. 45-03 (Jan. 14, 2003); PHILA. PA., CODE §§ 9-2400 to 9-2408 (Apr. 9, 2001); Jonathan L. Entin & Shadya Y. Yazbak, *City Governments and Predatory Lending*, 34 *FORDHAM URB. L. J.* 757, 773-79 (2007).

<sup>30</sup> Peterson, *supra* note 22, at 64-65.

<sup>31</sup> See *id.*

<sup>32</sup> See *Am. Fin. Serv. Ass'n. v. City of Cleveland*, 858 N. E. 2d. 776 (Ohio 2006) (holding that the Cleveland predatory lending ordinance was expressly preempted by the Ohio

nances with an APL that extended beyond HOEPA coverage and restrictions.<sup>33</sup>

Although abusive refinancing of mortgages, high loan fees, and credit insurance premiums were the focus of the North Carolina statute, it also included provisions aimed at excessive foreclosure risk.<sup>34</sup> Like HOEPA, the North Carolina APL prohibited the making of high-cost loans without regard to the borrower's repayment ability.<sup>35</sup> It added a presumption that a payment exceeding 50% of the borrower's income was unaffordable and also required counseling before a borrower obtains a high-cost loan.<sup>36</sup> Repayment ability and counseling requirements were included in many states' APLs.<sup>37</sup> In this regard, the laws were intended to reduce default and foreclosure risk directly as well as through restrictions on pricing and terms.

#### *B. Prior Research: Impact of State Anti-Predatory Lending Laws on Mortgage Markets*

Since North Carolina passed the first state anti-predatory lending law in 1999, researchers have tried to understand how APLs affect the mortgage market, looking to their impact on lending volume, the price of credit, and mortgage product substitution. Recent research has started to examine how APLs affect the use of more exotic loan types and how state laws have impacted mortgage foreclosure rates across states and neighborhoods.

State APLs were intended to curb so-called predatory practices while permitting non-abusive subprime lending to develop.<sup>38</sup> As a result, most empirical studies on the impact of APLs have focused on the effect of state laws on lending volume and loan prices. Some of these studies focus on one jurisdiction's law; others analyze outcomes nationally. One group of studies that focused on North Carolina's state anti-predatory lending law found that the subprime market in the state diminished in size as a result of the law's passage.<sup>39</sup> This is consistent with other stud-

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APL); Peterson, *supra* note 22, at 66; Kimm Tynan, Note, *Pennsylvania Welcomes Predatory Lenders: Pennsylvania's Act 55 Preempts Philadelphia's Tough Ordinance But Provides Little Protection for Vulnerable Borrowers*, 34 RUTGERS L.J. 837, 837, 861-63 (2003).

<sup>33</sup> Peterson, *supra* note 22, at 66-67.

<sup>34</sup> N.C. Gen. Stat. § 24-1.1A(c)(2) (2009).

<sup>35</sup> *Id.*

<sup>36</sup> N.C. Gen. Stat. § 24-1.1A(c)(1)-(2) (2009).

<sup>37</sup> See *infra* Table 11 and Appendix A for states with repayment ability restrictions.

<sup>38</sup> See, e.g., Wei Li & Keith S. Ernst, *Do State Predatory Lending Laws Work? A Panel Analysis of Market Reforms*, 18 HOUSING POL'Y DEBATE 347, 347 (2007); Roberto G. Quercia, et al., *The Impact of North Carolina's Anti-Predatory Lending Law: A Descriptive Assessment*, 15 HOUSING POL'Y DEBATE 573, 575 (2004).

<sup>39</sup> KEITH S. ERNST, ET AL. *North Carolina's Subprime Home Loan Market after Predatory Lending Reform* iv (Aug. 13, 2002), <http://www.responsiblelending.org/north-carolina/nc->



ies that have looked at APLs and subprime lending in Chicago and Philadelphia, even though taken as a whole, the national data show a lack of a consistent relationship between state laws and lending volume.<sup>40</sup> Studies using state-level law indices find that APLs appear to have little impact on subprime originations, applications, or rejections at the aggregate level.<sup>41</sup> But APLs with stronger restrictions are associated with a decrease in subprime lending.<sup>42</sup>

There are some possible explanations for these results. First, there is a fundamental difference between the states that extended restrictions on subprime mortgages beyond federal requirements and states that simply copied federal HOEPA restrictions into their state statutes. Some state laws did not extend coverage beyond mortgages covered by federal law. In several instances, the laws were meant to preempt local laws and ordinances that imposed greater restrictions than federal law.<sup>43</sup> Therefore, it is important to distinguish between these two types of state laws when evaluating results.

Second, in addition to examining overall lending volume, it is also important to examine which segment of the subprime market declined, which remained stable, and which increased. Because APLs were intended to reduce the number of predatory or abusive subprime loans, a decline in subprime lending, especially of loans with these traits, would not be surprising. If anti-predatory lending laws have curbed so-called predatory practices while permitting non-abusive subprime lending to develop, the laws have done what they were intended to do. Except for a few studies, current research has not examined this issue closely.<sup>44</sup>

Another group of studies focuses on APLs' impact on the cost of credit. Any costs incurred in complying with regulatory requirements

mortgage/research-analysis/HMDA\_Study\_on\_NC\_Market.pdf; Gregory Elliehausen et al., *The Effects of State Predatory Lending Laws on the Availability of Subprime Mortgage Credit*, 38 CRED. RES. CTR. MONOGRAPH iv (Mar. 2006), [http://www.namb.org/Images/namb/GovernmentAffairs/Reports\\_and\\_Studies/Georgetown%20Study%20\(March%202006\).pdf](http://www.namb.org/Images/namb/GovernmentAffairs/Reports_and_Studies/Georgetown%20Study%20(March%202006).pdf); Keith D. Harvey & Peter J. Nigro, *Do Predatory Lending Laws Influence Mortgage Lending? An Analysis of the North Carolina Predatory Lending Law*, 29 J. OF REAL EST. RES. 435, 439 (2004); Quercia et al., *supra* note 38, at 7–9.

<sup>40</sup> Keith D. Harvey & Peter J. Nigro, *How Do Predatory Lending Laws Influence Mortgage Lending in Urban Areas? A Tale of Two Cities*, 25 J. OF REAL EST. RES. 479, 479–83 (2003); see Elliehausen et al., *supra* note 39, at 17–18.

<sup>41</sup> Bostic et al., *supra* note 3, at 61–64.

<sup>42</sup> See Giang Ho & Anthony Pennington-Cross, *The Impact of Local Predatory Lending Laws on the Flow of Subprime Credit*, 60 J. OF URB ECON. 210, 226 (2006); Bostic et al., *supra* note 3, at 64–65.

<sup>43</sup> See *supra* notes 30–32 and accompanying text.

<sup>44</sup> Anthony Pennington-Cross et al., *Mortgage Product Substitution and State Anti-Predatory Lending Laws: Better Loans and Better Borrowers?* (Univ. Pa., Inst. for Law & Econ. Res., Working Paper No. 09-27), available at [http://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=1460871](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1460871); see ERNST ET AL., *supra* note 39; Quercia et al., *supra* note 38.

might be, at least in part, passed on to the consumer through higher interest rates or higher points and fees. However, the evidence suggests that APLs generally do not drive up loan prices. Although more restrictive laws may drive up the cost of borrowing through higher interest rates, this effect is limited to fixed-rate loans and its magnitude is typically fairly small.<sup>45</sup>

*C. Prior Research: Impact of State Anti-Predatory Lending Laws on Foreclosure Risk*

Researchers have started to investigate whether differences in regulatory environment and state APLs contribute to differences in the quality of loans originated and subsequent rates of foreclosure. Data suggest that high default risk is associated with many of the features restricted by APLs, such as prepayment penalties, balloon payments, lack of verification of borrowers' repayment capacity, and very high interest rates and fees. One study found that subprime ARMs have a higher risk of foreclosure because of interest-rate risk and practices, such as underwriting using teaser rates.<sup>46</sup> Additionally, at the aggregate level, the share of ARMs appears to be positively associated with market risk, as measured by house price declines,<sup>47</sup> and subprime hybrid ARMs, which usually have prepayment penalties, bear particularly high risk of default at the time the interest rate is reset.<sup>48</sup>

As to prepayment penalties and balloons, Roberto Quercia and colleagues found that refinance loans with prepayment penalties are 20% more likely to experience a foreclosure, and loans with balloon payments are about 50% more likely to do so.<sup>49</sup> Prepayment penalties also tend to reduce prepayments and increase the likelihood of delinquency and default among subprime loans.<sup>50</sup> ARMs, prepayment penalties, and broker originations are all significant factors that increase risk of default for subprime loans.<sup>51</sup>

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<sup>45</sup> Li & Ernst, *supra* note 38, at 380–89; Anthony Pennington-Cross & Giang Ho, *Predatory Lending Laws and the Cost of Credit*, 36 REAL EST. ECON. 175, 203 (2008).

<sup>46</sup> Roberto G. Quercia et al., *The Impact of Predatory Loan Terms on Subprime Foreclosures: The Special Case of Prepayment Penalties and Balloon Payments*, 18 HOUSING POL'Y DEBATE 311, 335–37 (2007).

<sup>47</sup> Dan Immergluck, *From the Subprime to the Exotic: Excessive Mortgage Market Risk and Foreclosures*, 74 J. OF THE AM. PLAN. ASS'N 59, 60–64 (2008).

<sup>48</sup> B.W. Ambrose, et al. *A Note on Hybrid Mortgages*, 33 REAL EST. ECON. 765, 781 (2005); Anthony Pennington-Cross & Giang Ho, *The Termination of Subprime Hybrid and Fixed Rate Mortgages*, 38 REAL EST. ECON. 399, 401 (2010).

<sup>49</sup> Quercia et al., *supra* note 46, at 335–37.

<sup>50</sup> IMMERGLUCK, *supra* note 5, at 60–64.

<sup>51</sup> Lei Ding et al., *Risky Borrowers or Risky Mortgages: Disaggregating Effects Using Propensity Score Models* 19 (U.N.C., Chapel Hill Ctr. for Cmty Capital, Working Paper, 2010), available at <http://www.ccc.unc.edu/documents/Risky.Disaggreg.5.17.10.pdf>.

One study examined the impact of state APLs on mortgage-product mix, and found that state APLs significantly reduced the use of mortgage features thought to increase foreclosure risk, including prepayment penalties, balloon payments, non-amortizing loans, investor loans, and reduced income documentation.<sup>52</sup> In other words, APLs resulted in less risky mortgage products. The study also found insignificant differences in borrower credit quality, tending to rule out the hypothesis that lenders in APL states systematically sought better borrowers.<sup>53</sup>

Although the literature does document a clear link between risky mortgage product features and foreclosures, very few studies have explicitly examined the link between state APLs and local- or state-level foreclosure rates. In a working paper, Goodman and Smith suggest that the laws governing mortgage underwriting, mortgage foreclosures, and the potential costs to the lender differ substantially across states.<sup>54</sup> They found some evidence that mini-HOEPA laws—APLs—reduce the level of foreclosure, which suggests that higher lender costs for foreclosure and stringent controls on predatory lending are connected to lower foreclosure rates.<sup>55</sup> However, since Goodman and Smith only use a cross-sectional dataset for one particular month, their paper's applicability may be limited. As regulations are being proposed and amended to address the current mortgage crisis, further research in the area of laws and regulations and the measurement of their effectiveness is needed.<sup>56</sup>

## II. ANALYSIS OF THE IMPACT OF ANTI-PREDATORY LENDING LAWS DURING THE SUBPRIME FORECLOSURE CRISIS: DATA AND METHODOLOGY

APLs were designed to protect consumers by restricting the origination of loans with predatory features. Some of the restricted loan terms are known to be associated with increased risk of default and foreclosure.<sup>57</sup> In addition, APLs may restrict the origination of the riskiest loans by putting some outer limits on subprime mortgage pricing. If APLs work, for example, by improving the quality of loans originated and by reducing the likelihood of default, then they can help to inform the regulatory landscape going forward. But do they work in this fashion? And

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<sup>52</sup> Pennington-Cross et al., *supra* note 44, at 23–24.

<sup>53</sup> *Id.* at 22–24.

<sup>54</sup> Allen C. Goodman & Brent Smith, *Hierarchical Modeling of Residential Default: Does State Level Foreclosure and Predatory Lending Legislation Limit 'Bad' Loans?* (Working Paper, 2009), available at <http://ssrn.com/abstract=1408212>.

<sup>55</sup> *Id.* at 27.

<sup>56</sup> Francisca Richter, *An Analysis of Foreclosure Rate Differentials in Soft Markets* (Fed. Reserve Bank of Cleveland, Working Paper 08-11, 2008), available at <http://ssrn.com/abstract=1316978>.

<sup>57</sup> See *supra* notes 35–40.

what features of APLs are the most important in ensuring consumer protection in the mortgage market?

Following the literature on various effects of APLs described above, we take advantage of the natural experiment resulting from the progressive enactment of APLs in 30 states and the District of Columbia.<sup>58</sup> Our study first examines whether or not APLs are associated with lower rates of originations of loans with risky features, and whether or not certain aspects of APLs are more important than others in ensuring responsible lending. Using a national, loan-level database of mortgages originated between 2002 and 2006, we examine differences in loan terms and outcomes in states with and without APLs, controlling for a wide range of borrowers, neighborhood, and economic characteristics that might influence underwriting practices and loan performance. We also examine the effect of different components of APLs by testing whether certain restrictions, such as banning prepayment penalties or requiring the lender to verify a borrower's ability to repay, influence the product features of originated loans.

Then, we look explicitly at whether APLs served to mitigate the foreclosure crisis. Using the same loan-level data, we examine whether loans in states with APLs were less likely to be ninety or more days delinquent than loans made in non-APL states. This effect of APLs is hypothesized to work in two ways. First, APLs may reduce the incidence of foreclosure by limiting the number of loans with risky loan features, such as prepayment penalties or option ARM features. Second, by improving overall underwriting quality, APLs may help to ensure that lenders are more attuned to the borrower risk profile and ability to repay the loan over the long term.

Because there is significant variation in the coverage and strength of APLs across different states, most researchers have developed a set of indices to quantify the substantial variation in the laws.<sup>59</sup> This paper relies on a state law coding system that includes measures that not only test for the presence of a state law, but also its strength and the nature of its restrictions.<sup>60</sup>

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<sup>58</sup> See Benjamin Lindy, *The Impact of Teacher Collective Bargaining on Student Achievement: Evidence from a New Mexico Natural Experiment*, 120 YALE L. J. 1130, 1151 (2011) (describing natural experiment studies generally). An important limitation of any natural experiment is the possible endogeneity of the variable of interest, in our case the enactment of state APLs. We are not aware of omitted variables associated with the APL states that would have an independent effect on the outcome variables, i.e. on mortgage product mix or default rates. Our models attempt to control for demographic and economic variables that would separately influence the outcomes measured, but as with any natural experiment study, we cannot entirely rule out the possibility of omitted variable bias.

<sup>59</sup> See Pennington-Cross & Ho, *supra* note 45, at 176.

<sup>60</sup> See *infra* Appendix A.

The binary variable *ineffect*, modeled on prior studies and combined with the effective date variable for the same state and law, is intended to identify states with mortgage statutes that could plausibly have an impact on high-cost or subprime mortgage lending.<sup>61</sup> In a departure from some prior studies, states that simply copied the federal HOEPA restrictions are not identified as having an anti-predatory lending statute by the *ineffect* variable. In several instances, the intent of these laws was to preempt local laws and ordinances that imposed greater restrictions than federal law.<sup>62</sup> However, these eight states are included in an alternative specification, *ineffectb*, which allows us to test our coding approach.

In addition to the presence of a state law, this study also identifies the nature of its restrictions. The variable *pointstrigger* quantifies the coverage of state APLs below the federal HOEPA thresholds. The variable *prepaymentpenalty* captures the scope and extent of prepayment penalty restrictions on a scale of zero to four. Finally, the binary variable *repaymentability* identifies states with two types of requirements subject to enforcement or sanction. One requirement is that repayment ability be determined, verified, or both for a significant portion of the mortgage market. The other requirement is that borrowers receive counseling as a precondition to obtaining a mortgage loan.<sup>63</sup>

The quantitative analysis used in this Article relies on a proprietary dataset on loan performance collected by Lender Processing Services Applied Analytics, Inc. (LPS).<sup>64</sup> We selected a random 10% sample of the 14.6 million loans in the LPS dataset, limiting the sample to owner occupied, conventional, and first lien loans in this analysis. The data were cleaned to account for missing information—like missing FICO score, LTV, or income documentation level—and to remove data with clear reporting errors—such as reported zip code not in property state, and LTV less than 10. The final study sample used in the analysis consists of approximately 1.2 million loans that originated between January 1, 2002 and December 31, 2006. Performance was observed from origination through December 2008; after 2008, the combination of the recession and financial crisis, sharp increase in unemployment, and the rapidly changing policy environment make it difficult to isolate the impact of APLs on either loan originations or loan performance.<sup>65</sup> Each loan was

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<sup>61</sup> See Bostic et al., *supra* note 3, at 58; Pennington-Cross et al., *supra* note 44, at 16–17.

<sup>62</sup> See Bostic et al., *supra* note 3, at 49–50.

<sup>63</sup> For more details on the coding system used for state laws, see *infra* Appendix A.

<sup>64</sup> For more details on the LPS dataset, see *infra* Appendix B.

<sup>65</sup> See Robert B. Avery et al., *The 2009 HMDA Data: The Mortgage Market in a Time of Low Interest Rates and Economic Distress*, 2010 FED. RESERVE BULL. A39, A39 (2010); Timothy Dunne & Kyle Fee, *Economic Trends: Changes in Foreclosure and Unemployment across States*, ECON. TRENDS, FED. RESERVE BANK OF CLEVELAND, Economic Trends, July 6, 2010, <http://www.clevelandfed.org/research/trends/2010/0710/01regact.cfm>.

repeatedly observed from the month it is originated until the loan (1) was paid off, (2) became ninety-days delinquent, or (3) was still active and never experienced a delinquency of more than ninety days by December 2008 (right censored). We focused on ninety-day delinquency as opposed to foreclosure status, because state laws governing foreclosure can have strong impacts on the length of the foreclosure spell, thus clouding the analysis of the effect of state anti-predatory lending laws.<sup>66</sup>

We tested the representativeness of the study sample against the overall mortgage market. A drawback of the LPS data is that it underrepresents the subprime market and does not include either combined loan-to-value (LTV) ratio or debt-to-income (DTI) information. However, its broad geographic coverage and inclusion of both securitized and portfolio loans allow for an examination of a wider swath of the mortgage market than other available loan performance data. In terms of the geographic coverage at the state level, the sample is not significantly different from either the geographic distribution of loans in the full LPS database or the originations as reported in the Home Mortgage Disclosure Act (HMDA) data.<sup>67</sup> The one exception is a light overrepresentation of loans originated in California. The performance of the loans in the sample parallels national delinquency rates for all loans. Of the loans in the sample, 32.08% were paid off, 4.23% became ninety days delinquent, and the remaining 63.69% were still active as of December 2008. The relatively strong performance of loans in the sample attests to the distribution of loans in LPS and its underrepresentation of the subprime market.

Because differences in both loan originations and loan performance are driven by factors such as borrower risk profiles and local and national economic conditions,<sup>68</sup> we also included borrower, housing, and labor market characteristics in our modeling. At the borrower level, we included FICO score and LTV at origination, which are both significant and strong predictors of loan product and loan outcomes. Unfortunately, the LPS data do not allow us to control for other borrower characteristics, such as income or race. To account for the possibility of loans with second liens, we constructed a dummy variable for loans originated at exactly 80% LTV.<sup>69</sup> We also included variables for whether the loan was used for purchase or refinance and whether it was originated without

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<sup>66</sup> See generally Amy Crew Cutts & William A. Merrill, *Interventions in Mortgage Default: Policies and Practices to Prevent Home Loss and Lower Costs* (Freddie Mac Working Paper 08-01, 2008), available at [http://www.freddiemac.com/news/pdf/interventions\\_in\\_mortgage\\_default.pdf](http://www.freddiemac.com/news/pdf/interventions_in_mortgage_default.pdf) (discussing the effects of state foreclosure laws).

<sup>67</sup> See *infra* Table 1.

<sup>68</sup> See generally Avery et al., *supra* note 65.

<sup>69</sup> During the subprime boom, many lenders offered "80/20" loans, i.e. a first mortgage for 80% of the home value and a junior mortgage for the remaining 20%. These loans were

documentation of the borrower's income or assets. To control for mortgage market channel, we included information as to whether the loan was originated by a mortgage broker, also known as the "wholesale" mortgage market channel. Studies suggest that loan origination channels play an important role in determining the types of mortgages borrower receive, the cost of credit, and consequently the foreclosure rate.<sup>70</sup>

To account for housing-market dynamics, we constructed a house-price change variable for each loan. House-price dynamics are important predictors of foreclosure,<sup>71</sup> and falling house prices have been shown to be a primary driver in the recent foreclosure crisis.<sup>72</sup> House-price changes, based on the Federal Housing Finance Agency (FHFA) house price index, are measured at the MSA/Metropolitan Division, and are calculated as a year-over-year rate of change from the month of observation. We also included a yearly measure of housing affordability at the county level.<sup>73</sup> Areas with lower affordability scores saw a large gap between local incomes and housing prices, suggesting that fewer households were able to afford a home in that county and making them more likely to rely on riskier mortgage products.

We also used other variables that capture general housing, mortgage market, and economic conditions. These included monthly county unemployment rates from Bureau of Labor Statistics (BLS) and the prevailing contract interest rates on commitments for conventional fixed rate mortgages from Freddie Mac each month. To control for general neighborhood effects, we incorporated the percent of minority residents, the median household income, and the percent of college graduates at the zip code level from the 2000 Census.

Table 2 presents the variables used in the analysis, and Table 3 provides summary statistics and descriptions of the estimation data. For each step of the analysis, we present the full model results for all loans, and then provide additional analyses for various loan types reporting only the coefficients for the state APL variables.

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regarded as particularly risky because the borrower had no investment in the property. See ENGEL & MCCOY, *supra* note 7, at 36.

<sup>70</sup> See Keith S. Ernst et al., *Steered Wrong: Brokers, Borrowers, and Subprime Loans*, Ctr. for Responsible Lending, Apr. 8 2008, at 13, 21, 33, <http://www.responsiblelending.org/mortgage-lending/research-analysis/steered-wrong-brokers-borrowers-and-subprime-loans.pdf>; Ding et al., *supra* note 51, at 33–34.

<sup>71</sup> Tim S. Campbell & J. Kimball Dietrich, *The Determinants of Default on Conventional Residential Mortgages*, 38 J. OF FIN. 1569, 1569–70 (1983).

<sup>72</sup> Mark Doms et al., *Subprime Mortgage Delinquency Rates* (Fed. Reserve Bank of S.F., Working Paper 2007-33, 2007), available at <http://www.frbsf.org/publications/economics/papers/2007/wp07-33bk.pdf>; Kenneth Gerardi, et al., *Subprime Outcomes: Risky Mortgages, Homeownership Experiences, and Foreclosures* (Fed. Reserve Bank of Bos., Working Paper 07-15, 2007), available at <http://www.bos.frb.org/economic/wp/wp2007/wp0715.htm>.

<sup>73</sup> Moody's Analytics, Economic & Consumer Credit Analytics, <http://www.economy.com> provides economic analysis, data, and credit risk services.

To better capture the relation between APLs and foreclosure rates and to test whether our general model sufficiently accounted for economic and other non-legal differences between and among states, we also used the cross-border approach similar to the one used in other studies.<sup>74</sup> The cross-border sampling method takes into account intrastate variation in economic conditions that can significantly influence mortgage performance by focusing on loans in zip codes that are in the same housing and labor markets. Borrowers on either side of the border are likely to be experiencing similar housing market and economic conditions, thereby eliminating unobservable differences between different markets that we cannot control for in the model.<sup>75</sup> As a result, the major remaining difference is the legal landscape between the neighboring zip codes. The approach focuses on zip codes located on state borders (with the center of the zip code within five miles of the state lines) where one of the two bordering states had an APL.

While the cross-border method can help to eliminate unobservable differences among housing and mortgage markets, it faces some limitations of its own in examining patterns of subprime lending. Areas of the country that saw the highest rates of subprime lending, such as California, Arizona, Nevada, and Florida, do not have any major markets located at state borders.<sup>76</sup> However, by presenting both the analysis for the cross-border zip codes and the analysis for all the loans in the sample, we believe that we contributed additional information on the effect of state APLs on loan performance that would not have been accounted for by presenting either method alone. Our hope is that this Article will lay the groundwork for more detailed examination of the role that APLs play in shaping mortgage market outcomes for consumers.

### III. EMPIRICAL RESULTS

#### A. *Effect of Anti-Predatory Lending Laws on Mortgage Originations*

In the first stage of the analysis, we examine whether the existence of APLs influenced the characteristics of originated loans, reducing the incidence of loans with risky features, such as prepayment penalties, interest only, negative amortization, balloon payments, and low-documentation. For our model specification, we use a *logit* regression model,

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<sup>74</sup> Bostic et al., *supra* note 3, at 50; Pennington-Cross et al., *supra* note 44, at 13.

<sup>75</sup> Bostic et al., *supra* note 3, at 56.

<sup>76</sup> See Katalina M. Bianco, *The Subprime Lending Crisis: Causes and Effect of the Mortgage Meltdown*, CCH MORTGAGE COMPLIANCE GUIDE AND BANK DIGEST, 2008, available at [http://business.cch.com/bankingfinance/focus/news/Subprime\\_WP\\_rev.pdf](http://business.cch.com/bankingfinance/focus/news/Subprime_WP_rev.pdf) (describing the hardest-hit areas).



with loan type as the dependent variable, and cluster the standard errors by zip code.<sup>77</sup> We focus on two key loan types in this analysis.

First, we examine whether state APLs influence the origination of loans with prepayment penalties. Prepayment penalties, the focus of many state APLS, were frequently included in subprime mortgages and have been found to increase the risk of foreclosure. Second, we examine whether APLs had any effect on the likelihood of the origination of option ARMs, which we define as ARMs with payment options, including interest only loans, loans with negative amortization, and ARM loans with balloon payments. During the subprime boom, option ARMs became widely prevalent as “affordability” products. They were particularly risky given the possibility that lenders did not assess the long-term affordability of the mortgage after the interest-only or negative amortization period ended.<sup>78</sup> In addition, option ARMs were often offered with a very low teaser rate (often as low as 1%), which translates into very low minimum payments for the first year of the ARM. There has been significant concern that during the subprime boom, lenders underwrote borrowers for the initial payments that were significantly below the fully amortizing payment level, leading to potentially severe subsequent payment shock. For these loans, we only look at the subset of ARMs to assess whether or not the loan was originated with a riskier payment structure.

In Table 4, we compare simple descriptive statistics on the origination rates of option ARM loans and loans with prepayment penalties in states with and without APLs. The table shows that there are significant differences in the origination of these riskier loan types and that these loans were more common rather than less in APL states. The popularity of nontraditional mortgages in a single large state, California, whose APL was not effective in curbing risky loan features, may help explain much of the difference here. In addition, the table also shows that socioeconomic conditions in APL states and non-APL states varied significantly, with APL states having higher housing costs and lower levels of housing affordability, higher rates of unemployment, and a higher percentage of minority residents.

However, once we control for borrower, housing, and labor market characteristics, we see that borrowers in APL states were less likely to receive a loan product with risky features, particularly when it comes to the likelihood of receiving a prepayment penalty.<sup>79</sup> Importantly, we find that APLs reduce the likelihood of the origination of loans with prepay-

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<sup>77</sup> Further details of the regression models used, the underlying assumptions, and our testing of alternative models, are set forth in Appendix B.

<sup>78</sup> ENGEL & MCCOY, *supra* note 7, at 35.

<sup>79</sup> See *infra* Table 5.

ment penalties by approximately 13%. The strong effect on prepayment penalties is consistent with expectations and prior work,<sup>80</sup> because many state laws focus on restricting or banning prepayment penalties in their design.<sup>81</sup>

Consistent with expectations, we find that borrowers with lower credit scores are more likely to receive a loan with a prepayment penalty, as are loans that are used to refinance, loans originated by mortgage brokers, and loans that have no income documentation. The dummy variable indicating the likely presence of a second lien is also positive and highly significant. Housing affordability affects the likelihood of receiving a loan with prepayment penalties as well. Neighborhoods with higher percentages of minorities are also more likely to receive a loan with a prepayment penalty.

In the second half of Table 5, we examine whether the presence of any APL had an effect on the incidence of option ARM loans. In contrast to the prepayment penalty analysis, we do not find any effect of state APLs on the origination of option ARMs. In fact, the state APL variable is positive and significant. Several factors could account for this finding. The high rate of option ARMs originations in California, an APL state that did not have any repayment ability restrictions in its APLs, may be skewing the results. In additional analysis, we find that excluding California from the analysis does influence the results, making the APL variable slightly negative but not significant. More likely, however, is that AMTPA restricted the ability of states to regulate mortgage terms on nontraditional mortgages such as ARMs, thereby limiting state APLs effectiveness in this market.<sup>82</sup> The full model also shows the degree to which option ARMs were offered as affordability products in “hot markets”—housing markets that saw rapid price appreciation and a large gap between house values and incomes saw a greater incidence of option ARM loans. Lower FICO scores, higher LTVs, and a lack of documentation also significantly increased the likelihood of receiving an option ARM, as did mortgage broker originations.

Table 6 presents results using alternative specifications of the APL variable. For states with APLs that did not go beyond the federal statutes (APL *ineffectb*), we still see a reduction in the incidence of prepayment penalties, but it is lower than in states with stronger APLs. By contrast, stronger restrictions—such as the repaymentability requirement—greatly reduced the likelihood of both prepayment penalties and option ARMs. A *pointstrigger* specification of 3 points or higher has a stronger effect for loans with prepayment penalties than for loans with option ARMs.

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<sup>80</sup> See, e.g., Pennington-Cross et al., *supra* note 44.

<sup>81</sup> See ENGEL & MCCOY, *supra* note 7, at 16; Mansfield, *supra* note 2, at 556.

<sup>82</sup> See McCoy et al., *supra* note 1, at 1333.

This suggests that the strength of the APL, in particular APLs that restrict more contract terms, can significantly influence the types of loan products that are originated. For example, in California where the APL statutes lacked repayment ability restrictions and therefore did not require either the verification of a borrower's ability to repay or financial counseling, there was insufficient consumer protections built into its APL to limit the number of option ARM originations, particularly given its rapidly rising house prices.

Table 6 also demonstrates the importance of the scope and coverage of state APLs in influencing lender underwriting behavior. States with the strongest restrictions on prepayment penalties (coded "4") reduced the likelihood of borrowers receiving a loan with a prepayment penalty by a third (odds ratio = 0.65).<sup>83</sup> However, we do not observe a monotonic relationship between the strength of prepayment penalty laws and their effect; states with weaker prepayment penalty laws (coded "1") also saw large reductions in the incidence of prepayment penalties. More research is needed to understand the relationship between the elements of state APLs and their effects in the marketplace. States with strong prepayment penalty APLs also saw lower rates of option ARMs, however this is likely more due to the substantial overlap between states with strong prepayment penalty laws and repaymentability requirements than to the fact that prepayment penalty APLs have a direct effect on the incidence of option ARM originations. There is significant overlap between states that have strong prepayment penalty restrictions and those that have *repaymentability* coverage and higher *pointstrigger* values as well.<sup>84</sup>

In the bottom half of the table, we examine the effect of APL specifications for only those loans originated along the border zip codes. Overall, the results are quite consistent with the results for all loans, with APLs decreasing the likelihood of prepayment penalties. Interestingly, among border zip codes, state APLs do reduce the incidence of option ARMs by about 17%. Also of note, *repaymentability* and *pointstrigger* restrictions emerge as more important for option ARMs, proving to be less significant in the likelihood of the origination of loans with prepayment penalties. Prepayment penalty restrictions logically have a stronger negative effect for loans with prepayment penalties. If the border zip codes are effectively controlling unobservable differences between communities that might influence the origination of loans with these characteristics (thus clouding the interpretation of the model including all loans), these results would suggest that targeted APLs really do influence

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<sup>83</sup> The state bans were not 100% effective because federal lenders could continue including prepayment penalties due to OCC and OTS preemption.

<sup>84</sup> See *infra* Appendix A.

the origination of loans they are intended to: stronger prepayment penalty restrictions reduce loans with prepayment penalties but have less of an effect on option ARMs, and repayment ability restrictions may have a greater influence on the origination of option ARMs. More research into these relationships is certainly warranted, especially as policymakers struggle with finding a balance between consumer protections on the one hand and avoiding undue restrictions on access to credit on the other.

In Table 7, we examine the effect of various APL specifications on loan originations within the subprime market, which we define as borrowers with a FICO score of less than 620.<sup>85</sup> For loans with prepayment penalties, we find a much stronger effect of the state APL variable, which aligns with the fact that many state APLs are targeted at the subprime market. The effect is similarly strong when we look at the border zip code loans only. Within the subprime market, state APLs reduced the incidence of prepayment penalties by 43%. States with strong APLs that included repaymentability requirements saw reductions in prepayment penalties of nearly 50%, and of option ARMs from between 25% and 40%, depending on whether we consider all the loans or only those on state borders. On balance, these findings suggest that state APLs can effectively reduce the incidence of loan originations with riskier product features. However, the analysis also shows that the strength of the APL matters, and that APLs were not a panacea. In high-cost markets in particular, existing state APLs were not sufficient for reducing the origination of riskier loan products.

### *B. Effect of Anti-Predatory Lending Laws on Loan Performance*

In the next stage of the analysis, we examine whether or not state APLs have an effect on loan performance. We expect that once we control for borrower, housing, and labor market characteristics, states with stronger restrictions on predatory loan terms would have, on balance, lower defaults and hence lower foreclosure rates. We theorize that APLs will affect default rates either by limiting the origination of loans with risky loan features or by improving the overall quality of underwriting, thereby ensuring that borrowers do not obtain loans that they cannot afford over the long term.

To account for changes over time in macroeconomic conditions, the variables capturing interest-rate changes, unemployment, house price

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<sup>85</sup> To check for robustness, we also tested the model with subprime defined as “B” and “C” mortgage originations, a designation by the originator identified in the data. The results are consistent across both definitions, with a stronger effect of the APL variables for “B” and “C” mortgage originations (e.g. an odds ratio of 0.24 compared with 0.72 for the APL in effect model). We chose to use the former because the overall number of “B” and “C” mortgages in the same is quite small, limiting our ability to replicate the analysis for border zip codes.

changes, and affordability are allowed to be time-varying covariates. As previously mentioned, each loan is repeatedly observed from its month of origination until (1) it is paid off, (2) it becomes ninety-days delinquent, or (3) it continues past December 2008 (right censored). To capture changes in interest rates that might influence the prepayment decision, we calculate the month-over-month change in the Freddie Mac prevailing contract interest rate. For models predicting the performance of ARMs, we include additional variables to account for potential payment shocks related to changes in the loan interest rate, including a measure of payment shock and the spread between the prevailing thirty-year fixed rate and the prevailing one-year ARM rate. We also include the mortgage origination channel. Since the wholesale mortgage origination channel was more conducive to mortgage fraud—by brokers and correspondents, appraisers, and, some borrowers—we expect to see higher levels of both serious delinquencies and prepayments in this channel. Finally, in each of the models, we include time dummies for the month and year of origination.

Building on previous literature on mortgage defaults,<sup>86</sup> we use option theory to develop our modeling approach for this question. Option theory posits that borrowers decide each month either to make a mortgage payment, to exercise the prepayment option (e.g., sell the home or refinance), or exercise the default option.<sup>87</sup> These options are competing risks; choosing one eliminates the possibility of the other until the next monthly payment is due. Loan performance is observed each month, and we assume that prepayment and default (as opposed to the reference group making mortgage payments) are distinct events that are influenced by different underlying mechanisms. We model these competing risks using the multinomial logit (MNL) framework.<sup>88</sup> Although researchers often rely on the Cox proportional hazards framework to analyze prepayment and default, Clapp and his colleagues have shown that using the MNL framework allows for estimating a flexible baseline hazard.<sup>89</sup> The information for each loan is restructured to include one observation for each time period in which that loan is active—from origination up to and including the period of termination. Once the data are restructured, the likelihood function is identical, in discrete time, to the continuous-time likelihood function for the Cox model.

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<sup>86</sup> See Ambrose et al., *supra* note 48, at 772–76. See generally Pennington-Cross & Ho, *supra* note 48.

<sup>87</sup> C. Foster & Robert Van Order, *An Option-Based Model of Mortgage Default*, 3 HOUSING FIN. REV. 351, 352–55 (1984).

<sup>88</sup> See, e.g., John M. Clapp et al., *Movers and Shuckers: Interdependent Prepayment Decisions*, 29 REAL EST. ECON. 411, 422–23 (2001). For more detail, see *infra* Appendix B.

<sup>89</sup> See Clapp et al., *supra* note 88, at 422–23.

Table 8 presents the results of our competing-risk model. Delinquency and prepayment are the competing risks, and the reference group is loans that remained active without serious delinquency as of December 2008. We report the coefficient, standard error, and “relative risk” ratio. The relative risk ratio gauges the effect of an independent variable on the two reported outcomes—delinquency and prepayment—relative to the likelihood of remaining active, given that the other variables in the model are held constant.

The model shows a strong and significant effect of state APLs on the likelihood of default. Loans originated in APL states are significantly less likely to be delinquent by ninety or more days, with a relative risk ratio of 0.75. The full model reveals important information about the determinants of default and prepayment. First, it is clear that borrowers with riskier credit profiles—most notably captured in FICO scores—are much more likely to be in default than are borrowers with higher credit scores. Second, a high loan-to-value ratio significantly increases the likelihood of default. Third, borrowers that saw a drop or smaller increase in neighborhood house values were more likely to be in default than those who saw continued house-price appreciation after origination. As expected, we also see a strong, positive effect of wholesale origination channel on both the likelihood of delinquency and prepayment.

In contrast, borrowers who saw house price declines were much less likely to prepay. This is as we expect. Borrowers who saw their equity erode as a result of declining house prices would have to pay a higher risk premium on a new mortgage, therefore reducing the likelihood of prepayment, all other things being equal. The model also attests to the importance of interest rates. If the prevailing interest rate at last point of observation is higher than the prevailing rate at origination, the likelihood of prepayment decreases significantly. Borrowers living in neighborhoods with higher percentages of minorities were more likely to experience a delinquency, and less likely to prepay, than areas with lower shares of minority households.

Table 9 presents the results of the effect of state APLs using alternative samples of loans, reporting only the coefficients and relative risk ratios for the state APL variable, and its relationship to the risk of delinquency. For the sample of subprime loans, state APLs reduced the likelihood for both the full sample and for loans only in border zip codes. The effect of APLs appears to be relatively consistent across loan types, reducing the likelihood of serious delinquency in the purchase/refinance and fixed interest rate/adjustable interest rate markets. For border zip code loans, the only effect is in the refinance market. This is not surprising, given that some state APLs had broader coverage and more restrictions in the refinance market during the study period. The HOEPA law

adopted in 1994 and the 2002 revision did not cover home purchase loans.<sup>90</sup> As a result, many mini-HOEPA laws, built upon HOEPA, have limited coverage and restrictions in the home purchase market. For example, five of the APL states—Arkansas, Colorado, Illinois, Maryland, and Michigan—did not cover home purchase mortgages in their state APLs.<sup>91</sup>

Lastly, in Table 10, we examine alternative specifications for the APL state law variable. While in all of the previous model specifications the general *ineffect* variable reduced the likelihood of serious delinquency, the components of the legal framework also turn out to be important. Notably, when we include states with HOEPA copycat statutes as APL states, the protective effect of APLs virtually disappears (relative risk ratio of 0.96). This could be due to the inclusion of states such as Nevada and Florida, both states that had APLs that did not go beyond federal law and that saw high rates of foreclosure during our period of observation.

Results for other state law specifications are mixed. We find that the requirement for lenders to verify borrower's ability to repay (*repaymentability*) is associated only with a slight reduction in the likelihood of delinquency. In contrast, borrowers in states with laws that had a broader coverage as measured by points and fee triggers were significantly less likely to be seriously delinquent than those in states with APLs with no coverage. Restrictions on prepayment penalties reduce the likelihood of delinquency across the board.

In the bottom panel, we examine the effects of APLs for loans originated in border zip codes. While we still see an effect of the APL variable, its strength and significance is greatly reduced. The only main difference in the border zip code analysis is that states that banned prepayment penalties for only a subset of mortgages (*prepayment penalty = 1 or 3*), based on loan amount or price, saw a greater effect than states with both the weakest and strongest prepayment penalty restrictions.

The results of our analysis suggest that state APLs did reduce the incidence of default of loans originated during the subprime boom, but their effect is highly contingent on both the sample choice and the specification of the APL law. At the loan level, many other factors influence default. While we control for factors such as house price and interest rate changes, we cannot do so for other triggers of default, such as job losses or divorce. In that sense, we do not expect to see a large effect of state APLs on defaults, other than through better product origination at

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<sup>90</sup> 15 U.S.C. § 1602(w)(aa)(1) (2006).

<sup>91</sup> ARK. CODE ANN. §§ 23-53-101 to 106 (2000); COLO. REV. STAT. § 5-3.5-101; 815 ILL. COMP. STAT. 137 (2004); MD. CODE ANN., §§ 12-124 to 12-127 (2005) (prior to 2008 and 2009 amendments); MICH. COMP. L. §§ 445.1631-1645 (2011).

the front end and a better alignment of loan products with borrowers' ability to repay. Given our findings of strong and consistent effects of state APLs on the origination of loans with prepayment penalties and option ARMs under some conditions, we believe that state APLS also help to protect borrowers from originating loans that are not sustainable over the long term. Future research is necessary to further tease out the various elements of state APLs that have the greatest effects in terms of consumer protection. Expanding the sample of border state loans, for example, or looking specifically at some case study states with similar housing and mortgage markets could further illuminate how state APLs can be designed to best protect consumers from predatory loan terms. Also warranted are similar analyses on other datasets, including national datasets such as Loan Performance, which has better coverage of the subprime market. Nevertheless, this study provides important new evidence on the effectiveness of state APLs that should help policymakers as they reexamine the regulatory landscape for mortgage products.

#### CONCLUSION

In this study, we used a nationally representative loan level dataset to examine whether state anti-predatory lending laws reduce the likelihood of origination of loans with riskier loan terms and the likelihood of serious delinquency.

In this loan-level analysis, we found strong evidence that APLs affected the mix of mortgage products by, *inter alia*, reducing the incidence of prepayment penalties. The conclusions for negative amortizing loans or option ARMs are more mixed because APLs do not have a significant effect on reducing the origination of loans with option ARMs for the full sample. However, we did see a significant and positive decline in the origination of option ARMs during the subprime boom once we considered alternate samples or law specifications, such as loans originated in border states or states with broader restrictions in their APLs, such as repayment ability requirements. This, coupled with the effects of preemption, led us to hypothesize that more general APLs without specific restrictions on repayment ability requirements were not strong enough to counteract the deterioration of underwriting standards that occurred during the latter half of the subprime boom, particularly in high-cost states such as California. Importantly, we also found that APLs significantly reduce the likelihood of default, especially in the subprime market, even after controlling for a wide range of borrower, housing, and mortgage market characteristics. The results are robust enough that we found an effect among loans made in zip codes along state borders, although there was a reduction in strength and significance of the state APL variable in the border zip model.



Generally, the goal of anti-predatory lending laws is to ban or restrict the origination of loans with unfair or abusive terms. By reducing the origination of loans with exotic features, such as prepayment penalties, balloon payments, equity-stripping fees, interest only, and lack of verification of borrowers' repayment capacity, the overall mortgage default risk can be significantly reduced. In fact, strong APLs may have an even broader impact on the mortgage market. Many studies have demonstrated that the loose underwriting and the increased availability of nontraditional non-prime mortgages was one primary reason for the run-up and subsequent collapse in house prices in the first half of the last decade.<sup>92</sup> With a strong APL in place, lenders should be less likely to adopt risky underwriting standards that would allow loan applicants to borrow more than they could afford. This would limit loan loss risk when house prices decline, interest rates change, or mortgage payments become unsustainable.

This study provides compelling evidence that state regulation of subprime mortgages can serve as an important tool in the landscape of mortgage-market regulation and consumer protection. To the extent that APLs improve underwriting standards and limit riskier loan products, they also decrease the likelihood of serious delinquencies and foreclosures. Given the dire consequences of foreclosure on individual households as well as the negative spillover effects on neighborhoods and even regional economies, there is a strong case to be made for allowing states to continue implementing and enforcing effective APLs without federal preemption.

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<sup>92</sup> Adam J. Levitin & Susan M. Wachter, *Explaining the Housing Bubble*, 100 GEO. L. J. (forthcoming 2012) (prior version available at <http://ssrn.com/abstract=1669401>); U.S. DEP'T OF HOUS. & URBAN DEV. REPORT TO CONGRESS ON THE ROOT CAUSES OF THE FORECLOSURE CRISIS at ix (2010), available at [http://www.huduser.org/Publications/PDF/Foreclosure\\_09.pdf](http://www.huduser.org/Publications/PDF/Foreclosure_09.pdf) ("In fact, the growth in risky lending seems likely to have fueled the dramatic rise in house prices.").

TABLE 1: GEOGRAPHIC DISTRIBUTION OF SAMPLE

State	LPS Sample	LPS Full Data	HMDA
Alabama	0.79	0.91	1.08
Alaska	0.15	0.15	0.17
Arizona	2.91	2.84	2.90
Arkansas	0.46	0.54	0.60
California	19.28	18.03	15.67
Colorado	2.51	2.47	2.34
Connecticut	1.39	1.35	1.29
Delaware	0.3	0.31	0.33
District of Columbia	0.32	0.27	0.21
Florida	6.77	6.9	7.26
Georgia	2.79	2.85	2.96
Hawaii	0.42	0.47	0.38
Idaho	0.61	0.59	0.52
Illinois	4.14	4.58	4.61
Indiana	1.52	1.53	1.95
Iowa	0.69	0.82	0.75
Kansas	0.73	0.79	0.74
Kentucky	0.8	0.87	1.03
Louisiana	0.63	0.68	1.00
Maine	0.29	0.29	0.39
Maryland	2.82	2.72	2.62
Massachusetts	2.44	2.35	2.56
Michigan	3.06	3.27	3.80
Minnesota	2.4	2.48	2.03
Mississippi	0.28	0.3	0.56
Missouri	1.89	0.31	1.94
Montana	0.27	0.31	0.24
Nebraska	0.31	0.44	0.46
Nevada	1.33	1.41	1.30
New Hampshire	0.47	0.51	0.50
New Jersey	3.39	3.56	3.14
New Mexico	0.55	0.55	0.52
New York	5.02	4.56	3.63
North Carolina	2.28	2.33	2.56
North Dakota	0.12	0.16	0.13
Ohio	2.39	2.53	3.50
Oklahoma	0.62	0.68	0.82
Oregon	1.71	1.57	1.33
Pennsylvania	3.15	3.38	3.73
Rhode Island	0.37	0.35	0.45
South Carolina	1.05	1.07	1.20
South Dakota	0.15	0.2	0.19
Tennessee	1.34	1.44	1.62
Texas	5.32	5.36	5.38
Utah	0.99	0.95	0.98
Vermont	0.15	0.14	0.18
Virginia	3.36	3.21	3.19
Washington	3.56	3.16	2.66
West Virginia	0.19	0.22	0.37
Wisconsin	1.32	1.37	2.11
Wyoming	0.15	0.17	0.13

**\*Percent of originations 2002–2006.**

TABLE 2: VARIABLE DEFINITIONS

Variable	Definition
FICO	Borrower FICO Score measured at origination
LTV	Borrower LTV measured at origination
Second Lien	Dummy variable indicating that LTV = 80
Loan Purpose	Dummy indicating whether loan was for purchase or refinance (purchase=1)
No Documentation	Loan was originated with no or limited documentation
Broker/Wholesale Origination	Loan was originated by a broker or wholesale channel
Prepayment Penalty	Dummy indicating whether or not the loan includes a prepayment penalty
Adjustable Rate Mortgage	Dummy indicating whether or not the loan has an adjustable interest rate
House Price Change	Year over year change in house prices in the observation months
Interest Rate Change	Year over year change in the prevailing Freddie Mac contract interest rate
Rate Spread	Spread between the prevailing 30-year fixed rate and the prevailing 1-year ARM rate
Payment Shock	Percentage increase in the monthly payment relative to that in the previous adjustment period
Affordability	County affordability in the observation month (a higher score translates into greater affordability)
Unemployment	County unemployment rate in the observation month from the BLS
Neighborhood Racial Composition	Percent of non-whites in the zip code from 2000 Census
Neighborhood Income	Median household income in the zip code from the 2000 Census
Neighborhood Educational Level	Percent of college graduates in the zip code from the 2000 Census
<b>APL Coding</b>	
State APL in Effect	State APL variable
State APL in Effect B	Alternative specification of state APL variable (including laws with substantial protection and those with marginal protection)
Repaymentability	Repaymentability requirement in APLs
Pointstrigger	Extent of APL coverage, measured by difference between state law points and fees trigger and the federal HOEPA thresholds.
Prepaymentpenalty	A complete or partial ban on prepayment penalties in APLs.

TABLE 3: SAMPLE MEANS

	Sample Means All Loans	Sample Means Border Zips
<b>Sample Size</b>	1,200,735	158,486
<b>Legal Framework</b>		
APL In Effect	49.7	36.72
APL In Effect B	63.6	48.51
Repaymentability	20.6	42.43
Pointstrigger	45.1	35.45
Prepayment Penalty		
0	37.7	42.4
1	8.1	8.37
2	26.9	11.71
3	17.6	20.27
4	9.7	17.26
<b>Loan Terms</b>		
Purchase	32.52	35.01
ARM	24.04	21.84
Option ARM	8.05	6.05
Prepayment Penalty Loan	13.65	10.76
No Documentation	11.11	10.23
Loan to Value	68.17	69.05
FICO at Origination	720.37	718.39
<b>Loan Performance</b>		
90+ Days Delinquent	4.23	3.48
Prepayment	32.08	32.21
<b>Housing and Socio-economic Conditions</b>		
FHFA House Price Change (2 years prior to origination)	19.39	19.67
County Unemployment Rate	5.42	5.27
Percent Minority in Zip Code	26.79	24.11
Percent College in Zip Code	37.21	36.70
County Housing Affordability	134.02	141.38
Median House Values in Zip Code	181,891	183,709
Median Household Income in Zip Code	54,074	53,566
Interest Rate	5.35	5.36

TABLE 4: DIFFERENCES BETWEEN APL AND NON-APL STATES

	Sample Means All Loans		Sample Means Border Zips	
	Non-APL States	APL States	Non-APL States	APL States
<b>Loan Terms</b>				
Purchase	32.65	32.39	33.32	37.92 ***
ARM	20.35	27.77 ***	19.41	26.02 ***
Option ARM (as share of ARMs)	30.78	41.50 ***	26.18	34.88 ***
Prepayment Penalty Loan	12.63	14.68 ***	10.74	10.79 ***
No Documentation	8.19	14.05 ***	7.97	14.13 ***
Loan to Value	69.83	66.49 ***	69.96	67.48 ***
FICO at Origination	720.60	720.20 ***	719.50	716.50 ***
<b>Loan Performance</b>				
90+ Days Delinquent	4.11	4.34 ***	4.07	3.87 ***
Prepayment	33.84	30.30 ***	33.35	30.23 ***
<b>Housing and Socio-economic Conditions</b>				
FHFA House Price Change (2 years prior to origination)	17.79	21.26 ***	17.96	23.08 ***
County Unemployment Rate	5.20	5.65 ***	5.30	5.22 ***
Percent Minority in Zip Code	20.55	33.09 ***	19.58	31.92 ***
Percent College in Zip Code	35.99	38.45 ***	35.14	39.38 ***
County Housing Affordability	149.70	118.10 ***	157.20	114.10 ***
Median House Values in Zip Code	153,499	210,647 ***	155,079	233,144 ***
Median Household Income in Zip Code	51,336	56,854 ***	51,848	56,527 ***
Interest Rate	5.3	5.4 ***	5.33	5.40 ***
*** p < .0001, ** p < .001, * p < .01				

TABLE 5: LOGIT MODEL PREDICTING THE EFFECT OF STATE APLS ON MORTGAGE PRODUCT ORIGINATION

<b>All Loans: Likelihood of Receiving a Prepayment Penalty</b>				
	<b>Coefficient</b>	<b>Standard Error</b>		<b>Odds Ratio</b>
Intercept	1.0371	0.0982 ***		
FICO at Origination	-0.004	0.0001 ***		1.00
LTV at Origination	-0.0038	0.0003 ***		1.00
Second Lien	0.1296	0.0132 ***		1.14
Purchase	-0.3586	0.0117 ***		0.70
No Documentation	0.5128	0.0153 ***		1.67
Broker/Wholesale Origination	0.1548	0.0157 ***		1.17
House Price Change	-0.0039	0.0006 ***		1.00
Affordability	-0.0025	0.0002 ***		1.00
Unemployment Rate	0.063	0.0051 ***		1.07
Median Household Income	0.0021	0.0006 ***		1.00
Percent Minority	0.0099	0.0004 ***		1.01
Percent College	0.0009	0.0007		1.00
APL in Effect	-0.1403	0.0172 ***		0.87
<b>Adjustable Rate Loans: Likelihood of Receiving an Option ARM</b>				
	<b>Coefficient</b>	<b>Standard Error</b>		<b>Odds Ratio</b>
Intercept	4.8742	0.1555 ***		
FICO at Origination	-0.0084	0.0001 ***		0.99
LTV at Origination	0.006	0.0006 ***		1.01
Second Lien	0.2155	0.0183 ***		1.24
Purchase	-0.4839	0.0167 ***		0.62
No Documentation	1.9123	0.0268 ***		6.77
Broker/Wholesale Origination	1.7452	0.0255 ***		5.73
House Price Change	0.0094	0.0009 ***		1.01
Affordability	-0.0088	0.0004 ***		0.99
Unemployment Rate	0.0715	0.0095 ***		1.07
Median Household Income	-0.0022	0.0011 *		1.00
Percent Minority	-0.0044	0.0006 ***		1.00
Percent College	-0.0104	0.0012 ***		0.99
APL in Effect	0.1173	0.0251 ***		1.12
*** p < .001, ** p < .01, * p < .1 Standard errors are clustered by zip code.				

TABLE 6: ANALYSIS OF THE EFFECT OF STATE APLS ON MORTGAGE PRODUCT ORIGINATION FOR ALTERNATIVE LAW SPECIFICATIONS

<b>All Loans: Likelihood of Receiving Prepayment Penalty</b>				
	<b>Coefficient</b>	<b>Standard Error</b>		<b>Odds Ratio</b>
APL In Effect	-0.140	0.017	***	0.87
APL in Effect B (includes 8 additional states whose restrictions do not go beyond federal law)	-0.041	0.016	**	0.96
Repaymentability	-0.245	0.019	***	0.78
<b>Pointstrigger (reference group: no pointstrigger)</b>				
Trigger 2.5 points or lower	0.066	0.021	**	1.07
Trigger 3 points or higher	-0.176	0.022	***	0.84
<b>Prepayment Penalty (reference group: no restrictions)</b>				
1	-0.505	0.024	***	0.60
2	-0.094	0.021	***	0.91
3	-0.242	0.023	***	0.79
4	-0.436	0.030	***	0.65
<b>Border Loans Only</b>				
APL In Effect	-0.128	0.049	**	0.88
APL in Effect B (includes 8 additional states whose restrictions do not go beyond federal law)	-0.198	0.041	***	0.82
Repaymentability	-0.091	0.049	*	0.91
<b>Pointstrigger (reference group: no pointstrigger)</b>				
Trigger 2.5 points or lower	-0.107	0.075		0.90
Trigger 3 points or higher	-0.110	0.058	*	0.90
<b>Prepayment Penalty (reference group: no restrictions)</b>				
1	-0.357	0.070	***	0.70
2	0.080	0.058		1.08
3	-0.055	0.076		0.95
4	-0.344	0.051	***	0.71
*** p < .001, ** p < .01, * p < .1				
Standard errors are clustered by zip code.				
Note: These models include all the predictors shown in Table 5.				

TABLE 6 CONT'D: ANALYSIS OF THE EFFECT OF STATE APLS ON  
MORTGAGE PRODUCT ORIGINATION FOR ALTERNATIVE  
LAW SPECIFICATIONS

Adjustable Rate Loans: Likelihood of Receiving an Option ARM				
	Coefficient	Standard Error		Odds Ratio
APL In Effect	0.117	0.025	***	1.12
APL in Effect B (includes 8 additional states whose restrictions do not go beyond federal law)	0.182	0.026	***	1.20
Repaymentability	-0.195	0.026	***	0.82
<b>Pointtrigger (reference group: no pointtrigger)</b>				
Trigger 2.5 points or lower	0.186	0.032	***	1.21
Trigger 3 points or higher	0.084	0.035	*	1.09
<b>Prepayment Penalty (reference group: no restrictions)</b>				
1	0.235	0.037	***	1.27
2	0.021	0.032		1.02
3	0.109	0.038	**	1.12
4	-0.437	0.041	***	0.65
<b>Border Loans Only</b>				
APL In Effect	-0.191	0.069	**	0.83
APL in Effect B (includes 8 additional states whose restrictions do not go beyond federal law)	-0.045	0.062		0.96
Repaymentability	-0.376	0.067	***	0.69
<b>Pointtrigger (reference group: no pointtrigger)</b>				
Trigger 2.5 points or lower	-0.202	0.091	*	0.82
Trigger 3 points or higher	-0.205	0.092	*	0.82
<b>Prepayment Penalty (reference group: no restrictions)</b>				
1	0.341	0.101	***	1.41
2	-0.492	0.128	***	0.61
3	-0.280	0.119	*	0.76
4	-0.385	0.072	***	0.68
*** p < .001, ** p < .01, * p < .1				
Standard errors are clustered by zip code.				
Note: These models include all the predictors shown in Table 5.				



TABLE 7: ANALYSIS OF THE EFFECT OF STATE APLS ON MORTGAGE  
 PRODUCT ORIGINATION FOR SUBPRIME LOANS AND ALTERNATIVE  
 LAW SPECIFICATIONS

All Subprime Loans: Likelihood of Receiving a Prepayment Penalty				
	Coefficient	Standard Error		Odds Ratio
<b>All Loans</b>				
APL In Effect	-0.325	0.034	***	0.723
APL in Effect B (includes 8 additional states whose restrictions do not go beyond federal law)	-0.2331	0.0318	***	0.792
Repaymentability	-0.6673	0.0405	***	0.513
<b>Pointstrigger (reference group: no pointstrigger)</b>				
Trigger 2.5 points or lower	0.1197	0.0473	**	1.13
Trigger 3 points or higher	-0.760	0.044	***	0.47
<b>Prepayment Penalty (reference group: no restrictions)</b>				
1	0.047	0.047		1.05
2	0.2678	0.0393	***	1.307
3	-0.7347	0.0492	***	0.48
4	-0.7903	0.0592	***	0.45
<b>Border Loans Only</b>				
APL In Effect	-0.5608	0.096	***	0.571
AAPL in Effect B (includes 8 additional states whose restrictions do not go beyond federal law)	-0.4903	0.0806	***	0.612
Repaymentability	-0.6781	0.1004	***	0.508
<b>Pointstrigger (reference group: no pointstrigger)</b>				
Trigger 2.5 points or lower	-0.6265	0.1634	***	0.534
Trigger 3 points or higher	-0.5729	0.1079	***	0.564
<b>Prepayment Penalty (reference group: no restrictions)</b>				
1	-0.1866	0.12	*	0.83
2	-0.1284	0.1093		0.879
3	-0.8836	0.1517	***	0.413
4	-0.8022	0.1179	***	0.448
*** p < .001, ** p < .01, * p < .1 Standard errors are clustered by zip code. Models include all the predictors shown in Table 5.				

TABLE 7 CONT'D: ANALYSIS OF THE EFFECT OF STATE APLS ON  
MORTGAGE PRODUCT ORIGINATION FOR SUBPRIME LOANS AND  
ALTERNATIVE LAW SPECIFICATIONS

Subprime Adjustable Rate Loans: Likelihood of Receiving an Option ARM				
	Coefficient	Standard Error		Odds Ratio
<b>All Loans</b>				
APL In Effect	0.2103	0.0683	**	1.234
APL in Effect B (includes 8 additional states whose restrictions do not go beyond federal law)	0.2483	0.0698	***	1.282
Repaymentability	-0.2712	0.0734	***	0.762
<b>Pointtrigger (reference group: no pointtrigger)</b>				
Trigger 2.5 points or lower	0.5048	0.0909	***	1.657
Trigger 3 points or higher	0.1571	0.0854	*	1.17
<b>Prepayment Penalty (reference group: no restrictions)</b>				
1	0.1127	0.1018		1.119
2	0.0661	0.0898		1.068
3	0.063	0.099		1.065
4	-0.4523	0.1166	***	0.636
<b>Border Loans Only</b>				
APL In Effect	-0.2045	0.1713		0.815
APL in Effect B (includes 8 additional states whose restrictions do not go beyond federal law)	-0.1908	0.17		0.826
Repaymentability	-0.5067	0.1835	**	0.603
<b>Pointtrigger (reference group: no pointtrigger)</b>				
Trigger 2.5 points or lower	-0.5486	0.2451	*	0.578
Trigger 3 points or higher	0.0752	0.2059		1.078
<b>Prepayment Penalty (reference group: no restrictions)</b>				
1	0.2037	0.2766		1.226
2	-0.8738	0.214	***	0.417
3	-0.1743	0.3034		0.84
4	-0.706	0.2366	**	0.494
*** p < .001, ** p < .01, * p < .1 Standard errors are clustered by zip code. Models include all the predictors shown in Table 5.				

TABLE 8: MULTINOMIAL LOGIT MODEL ESTIMATING THE EFFECT OF APLs ON LOAN PERFORMANCE

90+ Delinquency				
	Coefficient	Standard Error		Relative Risk Ratio
Intercept	0.495	0.072	***	
FICO at Origination	-0.013	0.000	***	0.99
LTV at Origination	0.039	0.000	***	1.04
Second Lien	0.264	0.011	***	1.30
Purchase	0.263	0.010	***	1.30
Prepayment Penalty	0.372	0.012	***	1.45
ARM	0.606	0.011	***	1.83
No Documentation	0.494	0.011	***	1.64
Broker/Wholesale Origination	0.417	0.010	***	1.52
House Price Changes	-0.029	0.000	***	0.97
Interest Rate Changes	-0.023	0.000	***	0.98
Affordability	0.003	0.000	***	1.00
Unemployment Rate	0.078	0.002	***	1.08
Median Household Income	0.000	0.000	***	1.00
Percent Minority	0.004	0.000	***	1.00
Percent College	-0.013	0.000	***	0.99
APL in Effect	-0.284	0.0104	***	0.75

\*\*\* p < .001, \*\* p < .01, \* p < .1  
Standard errors are clustered by zip code.  
Competing risk outcomes are measured against loans still active in December 2008.  
Model includes time dummies for month and year of origination.

TABLE 8 CONT'D: MULTINOMIAL LOGIT MODEL ESTIMATING THE EFFECT OF APLS ON LOAN PERFORMANCE

Prepayment				
	Coefficient	Standard Error		Relative Risk Ratio
Intercept	-3.253	0.078	***	
FICO at Origination	-0.002	0.000	***	1.00
LTV at Origination	0.003	0.000	***	1.00
Second Lien	-0.026	0.015	*	0.98
Purchase	0.007	0.011		1.01
Prepayment Penalty	0.008	0.012		1.01
ARM	0.825	0.011	***	2.28
No Documentation	0.057	0.013	***	1.06
Broker/Wholesale Origination	0.080	0.011	***	1.08
House Price Changes	0.016	0.000	***	1.02
Interest Rate Changes	-0.008	0.000	***	0.99
Affordability	-0.001	0.000	***	1.00
Unemployment Rate	-0.036	0.004	***	0.97
Median Household Income	0.000	0.000	***	1.00
Percent Minority	-0.001	0.000	***	1.00
Percent College	-0.002	0.000	***	1.00
APL in Effect	0.0228	0.0108	*	1.02
*** p < .001, ** p < .01, * p < .1				
Standard errors are clustered by zip code.				
Competing risk outcomes are measured against loans still active in December 2008.				
Model includes time dummies for month and year of origination.				

TABLE 9: ANALYSIS OF EFFECT OF STATE APLS ON LOAN PERFORMANCE FOR DIFFERENT LOAN SAMPLES

90+ Delinquency				
	Coefficient (ineffect = 1)	Standard Error		Relative Risk Ratio
<b>All Loans</b>				
Subprime				
Purchase Fixed	-0.1216	0.0206 ***		0.886
Purchase ARM				
Refinance Fixed	-0.303	0.0236 ***		0.739
Refinance ARM	-0.2386	0.0215 ***		0.788
	-0.2982	0.018 ***		0.742
	-0.2921	0.0215 ***		0.747
<b>Border Loans Only</b>				
Subprime				
Purchase Fixed	-0.0894	0.0199 ***		0.914
Purchase ARM				
Refinance Fixed	0.0526	0.0224 *		1.054
Refinance ARM	0.0386	0.026		1.039
	-0.0378	0.0178 *		0.963
	-0.0397	0.0246 *		0.961
*** p < .001, ** p < .01, * p < .1 Standard errors are clustered by zip code. Competing risk outcomes are measured against loans still active in December 2008. Models include all variables in Table 8 and month/year time dummies; in addition, models predicting performance for ARM loans include payment shock and rate spread covariates.				

TABLE 10: ANALYSIS OF THE EFFECT OF STATE APLS ON LOAN PERFORMANCE FOR ALTERNATIVE LAW SPECIFICATION

All Loans: 90+ Delinquency				
	Coefficient (ineffect = 1)	Standard Error		Relative Risk Ratio
<b>All Loans</b>				
APL In Effect	-0.2836	0.0104	***	0.75
APL in Effect B (includes 8 additional states whose restrictions do not go beyond federal law)	-0.0442	0.0109	***	0.96
Repaymentability	-0.03	0.0112	**	0.97
<b>Pointstrigger (reference group: no pointstrigger)</b>				
Trigger 2.5 points or lower	-0.3266	0.0145	***	0.72
Trigger 3 points or higher	-0.1478	0.0117	***	0.86
<b>Prepayment Penalty (reference group: no restrictions)</b>				
1	-0.2022	0.0158	***	0.82
2	-0.4674	0.0134	***	0.63
3	-0.4248	0.0142	***	0.65
4	-0.0915	0.0162	***	0.91
<b>Border Loans Only</b>				
APL In Effect	-0.0148	0.0109	*	0.985
APL in Effect B (includes 8 additional states whose restrictions do not go beyond federal law)	0.00949	0.00951		1.01
Repaymentability	-0.0154	0.011	*	0.985
<b>Pointstrigger (reference group: no pointstrigger)</b>				
Trigger 2.5 points or lower	-0.2154	0.0211	***	0.806
Trigger 3 points or higher	0.0572	0.0115	***	1.059
<b>Prepayment Penalty (reference group: no restrictions)</b>				
1	0.0798	0.0162	***	1.083
2	-0.1851	0.0155	***	0.831
3	-0.1606	0.0133	***	0.852
4	0.1222	0.0133	***	1.13
Standard errors are clustered by zip code.				
* Competing risk outcomes are measured against loans still active in December 2008. Models include all variables in Table 8 as well as month/year time dummies.				
*** p < .001, ** p < .01, * p < .1				

## APPENDIX A: STATE ANTI-PREDATORY LENDING LAW DATA CODING

To develop a state-law coding system for high-cost or predatory-mortgage laws, we reviewed existing studies.<sup>93</sup> We also reviewed the description of state laws in several treatises,<sup>94</sup> various rate matrices that reflect mortgage originators' understanding of state laws, particularly for prepayment penalty restrictions, and then statutory language itself.

We determined that mini-HOEPA laws were adopted in 25 states and the District of Columbia on or before December 31, 2007. In addition, five states—Michigan, Minnesota, Nevada, Texas, and West Virginia—passed significant subprime mortgage regulation statutes that were not HOEPA extension statutes and not based on rate-and-fee triggers. Of the mini-HOEPA laws, eight states—Utah, Pennsylvania, Nevada, Oklahoma, Ohio (prior to 2007 amendments), Maine (prior to 2007 amendments), Kentucky, and Florida—did not extend coverage beyond mortgages covered by federal law. In several instances, the intent of these laws was to preempt local laws and ordinances that imposed greater restrictions than federal law. Because there is thus a fundamental difference between the states that extended restrictions on subprime mortgages beyond federal requirements and states that simply copied federal HOEPA restrictions into their state statutes, we developed and coded a set of law variables to describe state laws that could affect the type of subprime mortgages made and the default and foreclosure rates of mortgages in a given state.

The binary variable *ineffect*, modeled on prior studies,<sup>95</sup> in combination with the effective date variable for the same state and law, is intended to identify states with mortgage statutes that could plausibly have an impact on high-cost or subprime mortgage lending. A value of 1 was assigned for the *ineffect* variable to states with any restrictions on charging or financing points and fees, credit insurance, prepayment penalties, balloon payments, negative amortization, determination or documentation of income or repayment ability, and significant counseling requirements, so long as the state law covers any share of the subprime mortgage market below the HOEPA rate or fee triggers. A value of 0 was assigned to the *ineffect* variable for the eight states with HOEPA copycat statutes. This is a departure from some prior studies.<sup>96</sup> While some of these eight statutes imposed minor additional restrictions not

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<sup>93</sup> Bostic et al., *supra* note 3; Li & Ernst, *supra* note 38; Pennington-Cross et al., *supra* note 44.

<sup>94</sup> See generally ELIZABETH RENUART ET AL., *THE COST OF CREDIT* (3d ed. 2005 & Supp. 2008); GRANT S. NELSON & DALE A. WHITMAN, *REAL ESTATE FINANCE LAW* (5th ed. 2007).

<sup>95</sup> Bostic et al., *supra* note 3, at 58; Pennington-Cross et al., *supra* note 44, at 16.

<sup>96</sup> Bostic et al., *supra* note 3; Li & Ernst, *supra* note 38; Pennington-Cross et al., *supra* note 44.

found in federal law on high-cost loans above the HOEPA triggers, it is doubtful that a difference in regulation of a negligible slice of the mortgage market would affect the outcome variables. Thus, based on our definition, states with strong APLs prior to 2007 were Arkansas, California, Connecticut, Georgia, Illinois, Indiana, Maryland, Massachusetts, Michigan, Minnesota, New Jersey, New Mexico, New York, North Carolina, Rhode Island, South Carolina, Tennessee, Texas, West Virginia, and Wisconsin, as well as the District of Columbia. In this analysis, unless otherwise noted, we classify APL states and non-APL states based on the more stringent *ineffect* definition for this analysis.

In order to further measure the reach of state laws, an additional variable was created, *pointstrigger*. Most states laws that do not cover all mortgages use HOEPA-like price triggers in an attempt to cover only subprime or higher-cost loans. The *pointstrigger* variable is calculated as the numeric difference between the federal HOEPA threshold for points and fees and the corresponding state law trigger. For example, the North Carolina APL covers loans with points and fees exceeding 5%, compared with the HOEPA trigger of 8%. The value for the *pointstrigger* variable for North Carolina is therefore 3. A state law that prohibits points and fees above a certain threshold is treated as having a *pointstrigger* at that price level. West Virginia, for example, prohibits points and fees in excess of 3% for most mortgages and so is assigned a *pointstrigger* value of 5. For states without a mini-HOEPA law, a value of zero is assigned to the *pointstrigger* variable.

Prepayment penalties were frequently included in subprime mortgages and have been found to increase the risk of foreclosure.<sup>97</sup> Many state laws limit the ability of lenders to impose prepayment penalties.<sup>98</sup> The *prepaymentpenalty* variable captures both the scope and extent of prepayment penalty restrictions on a scale of 0 to 4. A statute banning all prepayment penalties for all, or nearly all, residential mortgages is coded as 4. Statutes banning prepayment penalties for only a subset of mortgages, based on loan amount or price, are coded as 3. Statutes restricting but not banning prepayment penalties for all, or nearly all, mortgages are coded as 2, and statutes banning prepayment penalties only beyond twenty-four months and covering only a subset of mortgages are coded as 1. Statutes without any restriction on prepayment penalties or only restricting but not banning prepayment penalties are coded as 0 for this analysis.

The binary *repaymentability* variable identifies states in which a significant portion of the mortgage market was subject to two types of requirements. One requirement is that repayment ability be determined

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<sup>97</sup> See Quercia et al., *supra* note 46, at 333.

<sup>98</sup> See ENGEL & MCCOY, *supra* note 7, at 16; Mansfield, *supra* note 2, at 556.



or verified. The other requirement is that borrowers receive counseling as a precondition to obtaining a mortgage loan. This variable necessarily includes some judgmental assessment of various state law provisions. State laws that either impose repayment ability standards, but only on loans above the federal HOEPA triggers, or do not regulate mortgage repayment ability are assigned a value of 0.<sup>99</sup>

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<sup>99</sup> See *infra* Table 11 for a summary of all of the described the state law variables.

TABLE 11: STATE LAW VARIABLES

State	Pennington- Cross et al. 2009	Li and Ernst (2007)	Bostic et al. 2008a	Effective Date	In effect	In effectb	Prepayment- penalty	Repayment- ability	Points trigger
Alabama	0	NA	0	5/20/1996	0	0	0	0	0
Alaska	0	8	0	12/31/1968	0	0	4	0	0
Arizona	0	NA	0	.	0	0	0	0	0
Arkansas	0	10	6.56	7/16/2003	1	1	3	1	2
California	1	NA	4.93	7/1/2002	1	1	3	0	2
Colorado	1	NA	4.18	7/1/2003	1	1	1	1	2
Connecticut	1	NA	4.88	1/1/2002	1	1	1	0	3
Delaware	0	NA	0	.	0	0	0	0	0
D.C.	1	11	7.75	5/7/2002	1	1	3	1	3
Florida	1	8	3.75	10/2/2002	0	1	0	0	0
Georgia	1	12	6.83	3/7/2003	1	1	2	1	3
Hawaii	0	NA	0	1/1/1967	0	0	4	0	0
Idaho	0	7	0	7/1/1996	0	0	3	0	0
Illinois	1	12	8.11	1/1/2004	1	1	4	1	3
Indiana	1	NA	6.76	1/1/2005	1	1	2	1	3
Iowa	0	8	0	4/13/1979	0	0	4	0	0
Kansas	0	7	0	1/1/1994	0	0	4	0	0
Kentucky	1	7	5.86	6/24/2003	0	1	1	0	0
Louisiana	0	NA	0	.	0	0	0	0	0
Maine	1	8	3.01	12/31/1995	0	1	1	0	0
Maryland	1	8	3.39	10/1/2002	1	1	0	1	1
Massachusetts	1	16	8.44	11/7/2004	1	1	2	1	3
Michigan	1	8	5.99	12/23/2002	1	1	3	0	0
Minnesota	1	10	7.01	1/1/2003	1	1	3	1	0
Mississippi	0	NA	0	.	0	0	3	0	0
Missouri	0	NA	0	.	0	0	3	0	0
Montana	0	NA	0	.	0	0	0	0	0
Nebraska	0	NA	0	3/20/2003	0	0	0	0	0
Nevada	1	NA	2.81	10/1/2003	0	1	1	1	0
New Hampshire	0	NA	0	1/1/2004	0	0	0	0	0
New Jersey	1	15	7.34	11/27/2003	1	1	4	0	3.5
New Mexico	1	18	9.9	1/1/2004	1	1	2	1	3
New York	1	15	5.82	4/1/2003	1	1	2	1	3
North Carolina	1	17	6.4	7/1/2000	1	1	2	1	3
North Dakota	0	NA	0	.	0	0	0	0	0
Ohio	1	7	3.47	5/24/2002	0	1	1	0	0
Oklahoma	1	NA	4.29	1/1/2004	0	1	4	0	0
Oregon	0	NA	0	.	0	0	0	0	0
Pennsylvania	1	NA	3.47	6/25/2002	0	1	2	0	0
Rhode Island	0	NA	0	12/31/2006	1	1	2	1	3
South Carolina	1	13	4.8	1/1/2004	1	1	4	1	3
South Dakota	0	NA	0	.	0	0	0	0	0
Tennessee	0	NA	0	1/1/2007	0	0	1	0	0
Texas	1	10	4.34	9/1/2001	1	1	2	0	5
Utah	1	NA	3.91	5/3/2004	0	1	1	0	0
Vermont	0	8	0	1/1/1997	0	0	4	0	0
Virginia	0	8	0	6/26/2003	0	0	4	0	0
Washington	0	NA	0	.	0	0	0	0	0
West Virginia	1	17	9	6/8/2000	1	1	4	1	3
Wisconsin	0	7	0	2/1/2005	1	1	1	1	2
Wyoming	0	NA	0	.	0	0	0	0	0

## APPENDIX B: DATA AND METHODOLOGY

A. *Lender Processing Services Applied Analytics, Inc. Data*

In conducting this mortgage default analysis, we rely on proprietary loan performance information compiled by Lender Processing Services Applied Analytics, Inc. (LPS) and aggregated the data to the zip code level.<sup>100</sup>

The LPS data is rich in detail, including over 70 variables related to loan characteristics and performance.<sup>101</sup> Additionally, the performance of each loan can be monitored over time. For each month in which a given loan is in the dataset, we know its outstanding balance, the current interest rate, and the borrower's payment status—that is, current; thirty-, sixty-, or ninety-days delinquent; in foreclosure. We also know whether a loan ended in prepayment or foreclosure. Unfortunately, the dataset does not permit us to identify the lender's regulator or whether individual loans were subject to OCC or OTS preemption.

The LPS dataset is one of the few loan-level databases that provide excellent coverage in both the conventional mortgage market and the FHA market as well as for both securitized loans and those that are held in portfolio. For the 2004–2006 cohorts, LPS data covered roughly 40% of that of the Home Mortgage Disclosure Act (HMDA) data, while its coverage in the conventional market varies from year to year.<sup>102</sup> The LPS dataset has grown over time as new servicers have been added, with a substantial increase in coverage of the market in 2005 and in late 2008. Based on our estimation using Mortgage Bankers Association (MBA) data of active loans, the LPS covered about 50% of active mortgages in the United States during the period from June 2006 to June 2008 (48% in 2006 and 53% in 2008).<sup>103</sup>

Of course, one major concern with the LPS is its coverage in the subprime market. Nationally, as of June 2006, the share of subprime

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<sup>100</sup> The dataset is sometimes still called the McDash data, although LPS acquired McDash in July 2008. Information on this dataset is available at <http://www.lpsvcs.com/Products/Mortgage/RiskManagement/MortgagePerformanceData/Pages/default.aspx>.

<sup>101</sup> Variables include zip codes for property, FICO score, debt-to-income ratio (DTI), loan amount, property value, contract-rate type (fixed or adjustable), loan purpose (purchase or refinance), loan type (conventional, FHA, VA, or subprime), occupancy status (owner-occupied or not), documentation status (full documentation or not), existence of a prepayment penalty, loan term to maturity, origination channel (wholesale or retail), and delinquency and foreclosure status in each month.

<sup>102</sup> See Ernst et al., *supra* note 70, at 10.

<sup>103</sup> The LPS dataset has grown over time as new servicers have been added, with a substantial spread in coverage of the market in 2005. As of December 2008, the coverage of LPS further increased to nearly 60% of active residential mortgages in the United States, representing about 29 million loans with a total outstanding balance of nearly \$6.5 trillion. See Christopher Foote et al., *Reducing Foreclosures: No Easy Answers* 10 (Nat'l Bureau of Econ. Research, Working Paper No. 15063, 2009), available at <http://www.nber.org/papers/w15063>.

loans among all active loans in the LPS was about 2.8%, lower than the 13.4% of subprime reported by the MBA.<sup>104</sup> But LPS only counts *B&C loans* as subprime, which inevitably leads to an underestimation of the size of the subprime market. As Lei Ding and colleagues documented, the LPS coverage in the subprime market by volume increased from 14% for the 2004 cohort to over 30% for the 2006 cohort.<sup>105</sup> While any empirical approach needs to consider the changing nature of the LPS data, it should not be a serious problem here since we focus on active loans in the recent years, which have seen a significant expansion in the coverage of LPS.

### B. Model Details

To test the effectiveness of state APLs, we measure two potential impacts that state APLs should have in the mortgage market. First, using a logistic regression model, we test the assumption that state APLs protect borrowers by restricting the origination of loans with riskier product features. In this case, we assess the likelihood of receiving a loan with a prepayment penalty—as opposed to a loan without a prepayment penalty—and an adjustable rate loan with payment options—as opposed to an adjustable rate mortgage without payment options. Logistic regression—also referred to as a logit model—is used when the dependent variable is dichotomous. We chose a logit model instead of a probit because of the large sample size and the ease of interpretation of the coefficients; however, separate analysis using the probit specification leads to qualitatively similar results.

Second, we test to see whether state APLs have an impact on mortgage default rates. Building on previous literature on mortgage defaults, we use option theory to develop our modeling approach for this question.<sup>106</sup> Option theory posits that borrowers decide each month either to make a mortgage payment, to exercise the prepayment option (e.g., sell the home or refinance), or exercise the default option.<sup>107</sup> These options are competing risks; choosing one eliminates the possibility of the other until the next monthly payment is due. Loan performance is observed each month, and we assume that prepayment and default—as opposed to the reference group of making the mortgage payment—are distinct events that are influenced by different underlying mechanisms. We model these competing risks using the multinomial logit (MNL) frame-

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<sup>104</sup> See MORTGAGE BANKERS ASSOCIATION OF AMERICA, National Delinquency Survey, Fourth Quarter 2006, available at [http://media.miamiherald.com/smedia/2007/03/13/21/mortgage\\_NDS.source.prod.affiliate.56.pdf](http://media.miamiherald.com/smedia/2007/03/13/21/mortgage_NDS.source.prod.affiliate.56.pdf).

<sup>105</sup> Ding et al., *supra* note 51, at 13.

<sup>106</sup> Ambrose et al., *supra* note 48; Pennington-Cross & Ho, *supra* note 48.

<sup>107</sup> See Foster & Van Order, *supra* note 87, at 352–55.

work.<sup>108</sup> The MNL regression identifies a function to predict an outcome variable with more than two nominal values (in this case, prepayment, default or on-time payment) based on a specified set of independent variables. Although researchers often rely on the Cox proportional hazards framework to analyze prepayment and default, Clapp and his colleagues have shown that using the MNL framework allows for estimating a flexible baseline hazard, as opposed to requiring the proportional hazards assumption.<sup>109</sup> The information for each loan is restructured to include one observation for each time period in which that loan is active—from origination up to and including the period of termination. Once the data are restructured, the likelihood function is identical, in discrete time, to the continuous-time likelihood function for the Cox model. Our model takes the following functional form:

$$L = \prod_{i=1}^N \prod_{t=1}^{T_i} \prod_{j=0}^2 Z_{ijt} * \Pr(Y_{it} = j)$$

where

$$\Pr(Y_{it} = j) = \frac{e^{\alpha_j + APL_i \beta_j + X_{it} \gamma_j + \theta(t)}}{1 + \sum_{k=1}^2 e^{\alpha_k + APL_i \beta_k + X_{it} \gamma_k + \theta(t)}}$$

$$\Pr(Y_{it} = 0) = \frac{1}{1 + \sum_{k=1}^2 e^{\alpha_k + APL_i \beta_k + X_{it} \gamma_k + \theta(t)}}$$

Here,  $N$  refers to the number of observations,  $t$  equals the number of months the loan is observed in the data,  $j$  represents the competing risk outcomes, and APL is the state anti-predatory law dummy. Estimation of this model identifies the effect of state APLs on prepayment and default, after controlling for observed borrower and housing market risk characteristics.

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<sup>108</sup> See Clapp et al., *supra* note 88, at 422–23.

<sup>109</sup> *Id.*