Predatory lending and the subprime crisis ☆

Sumit Agarwal [a], Gene Amromin [b], Itzhak Ben-David [c,d,*],
Souphala Chomsisengphet [e], Douglas D. Evanoff [b]

[a] National University of Singapore, Singapore 119077, Singapore
[b] Federal Reserve Bank of Chicago, Chicago, IL 60604, USA
[c] Fisher College of Business, The Ohio State University, Columbus, OH 43210, USA
[d] NBER, Cambridge, MA 02138, USA
[e] Office of the Comptroller of the Currency, Washington DC 20219, USA

ARTICLE INFO

ABSTRACT

We measure the effect of a 2006 antipredatory pilot program in Chicago on mortgage default rates to test whether predatory lending was a key element in fueling the subprime crisis. Under the program, risky borrowers or risky mortgage contracts or both triggered review sessions by housing counselors who shared their findings with the state regulator. The pilot program cut market activity in half, largely through the exit of lenders specializing in risky loans and through a decline in the share of subprime borrowers. Our results suggest that predatory lending practices contributed to high mortgage default rates among subprime borrowers, raising them by about a third.

1. Introduction

Predatory lending has been the focus of intense academic and policy debate surrounding the recent housing crisis (2007–2010). Predatory lending—commonly defined as imposing unfair and abusive loan terms on borrowers, often through aggressive sales tactics, or loans that contain terms and conditions that ultimately harm borrowers (US Government Accountability Office, 2004 and Federal Deposit Insurance Corporation (FDIC), 2006)—has also captured much media attention and appears to be a major concern for borrowers.[1] While all agree that mortgages with abusive terms are costly to borrowers and to taxpayers, the extent of the phenomenon is hard to quantify and is politically charged (e.g., Agarwal and Evanoff, 2013; Engel and McCoy, 2007). Several journalistic accounts and

* Corresponding author at: Fisher College of Business, The Ohio State University, Columbus, OH 43210, USA.

E-mail address: bendavid@fisher.osu.edu (I. Ben-David).

[1] Guiso, Sapienza, and Zingales (2013) find that about half of surveyed borrowers would be willing to strategically default on their mortgage should they discover that their lender was involved in predatory lending.

industry reports take the position that predatory lending had a central role in creating and feeding the housing bubble, particularly through subprime loan originations (e.g., the Financial Crisis Inquiry Commission, 2011; Hudson, 2010; Center for Responsible Lending, 2009). To our knowledge, no systematic evidence to date measures the effect of predatory lending on mortgage performance. Our paper attempts to fill this gap.

In academic literature, predatory lending is modeled as cases in which lenders possess private information about borrowers' future ability to repay loans and encourage mortgages with terms that borrowers cannot afford (Bond, Musto, and Yilmaz, 2009). This model clearly portrays the empirical challenge in measuring predatory lending: Because observing lenders' informational advantage over borrowers is difficult, measuring the size of the phenomenon and assessing its role in precipitating the subprime mortgage crisis is hard.

In this paper, we attempt to overcome this challenge by analyzing the effects of a pilot antipredatory legislative program (HB 4050) implemented in Chicago near the peak of the real-estate boom. The pilot program required "low-credit-quality" applicants and applicants for "risky" mortgages to submit their loan offers from state-licensed lenders for third-party review by financial counselors certified by the US Department of Housing and Urban Development (HUD). The fact that the pilot applied only in certain areas during a specific time period, only to certain borrower and mortgage contract combinations, and only to a specific set of lenders allows us to parse out its effect on the availability of mortgage credit with predatory characteristics and to evaluate ex post mortgage performance. The study draws on detailed loan-level data from public and proprietary sources, as well as data provided by one of the largest counseling agencies involved in the pilot.

Our empirical strategy is based on classic difference-in-differences analysis that contrasts changes in mortgage market composition and loan performance in the treated sample with those in a control sample. Unlike bacteria in a petri dish, lenders and borrowers could respond to the mandated treatment either by leaving the pilot area or by adapting to the new rules. Hence, we pay particular attention to endogenous selection of lenders and borrowers out of treatment. If predatory lending resulted in significantly higher default rates and, thus, precipitated the crisis, we should observe a significant reduction in default rates in the targeted market as predatory lending declined.

We find that following passage of the pilot program, the number of active lenders declined disproportionately in the target geographic area. The decline was particularly pronounced among state-licensed lenders that specialized in the origination of subprime loans, many of which included contract features deemed objectionable by the legislation. Nearly half of the state-licensed lenders exited the pilot zip codes, more than double the exit rate in the control areas. The remaining lenders made fewer risky loans and originated credit to borrowers with higher credit quality. Specifically, we show that the volumes of loan applications and originations by state-licensed lenders in the pilot area declined by 51% and 61%, respectively. The

average FICO score of borrowers who were able to obtain credit during the pilot period was 8 points higher (15% of 1 standard deviation).

The resulting mortgages issued in the pilot area were less likely to feature risky characteristics (as defined by legislators) that would subject them to counselor review. For instance, there were fewer loans with negative amortization or prepayment penalties, as well as fewer low documentation and low down-payment loans. This set of findings suggests, therefore, that the legislation had a deep impact on market activity and likely drove much of the predatory lending activity from the market.

Although the pilot dramatically affected market activity, it had a relatively moderate effect on borrower default rates. When we restrict our analysis to the subset of market participants directly targeted by the pilot—subprime borrowers and state-licensed lenders—we find improvements in 18-month default rates of 6 to 7 percentage points, relative to the unconditional default rate of 27%. Moreover, all of the statistically measurable improvement in loan performance came from changes in the composition of lenders, many of whom were driven out by the legislation. These estimates suggest that while predatory lending contributed to high default rates, it might not have been as instrumental in precipitating the financial crisis as popularly believed.

In practice, distinguishing predatory lending practices from merely aggressive ones could be difficult. To make headway in separating the two, we exploit another feature of the antipredatory program. The heart of the HB4050 pilot was the imposition of a mortgage review requirement for risky borrowers and for those who chose risky loans. During the review, counselors identified loans that were suspected of having predatory characteristics, e.g., loans with above-market rates, loans appearing to be unaffordable based on borrower characteristics, and loans with indications of fraud. We analyze a sample of 121 loans for which we have detailed counselor assessment data.[2] We conjecture that loans that were flagged as predatory and yet were pursued by borrowers (i.e., borrowers ignored the counselors' advice) were more likely to default relative to nonflagged loans. We find that these predatory loans had 18-month delinquency rates that were 6.5 percentage points higher than nonflagged loans. The difference in delinquency rates was even higher for loans with fraud indicia, which had a 12.3 percentage point differential.

Our findings have important implications for policy makers. First, the pilot program was a blunt policy tool that swept up a wide swath of borrowers, lenders, and products and caused substantial market disruption. Second, despite the measureable improvements in mortgage performance in the subpopulation most affected by the pilot, default rates remained alarmingly high, suggesting that predatory lending practices could have played a relatively limited role in triggering the crisis. In fact, because some of the loans

---

[2] For an in-depth analysis of the role of mortgage counseling, see Agarwal, Amromin, Ben-David, Chomsisengphet, and Evanoff (2010, 2012).

eliminated by the pilot could have been aggressive instead of predatory, we are likely to be overstating the effect of predatory lending practices. Third, evaluation of welfare gains or losses stemming from such policy programs is fraught with difficulties, many of which are exacerbated by the distortions that exist in housing markets. Our paper does not attempt to gauge the welfare consequences of the pilot, and policy makers should be aware that such consequences are difficult to measure. Finally, the HB4050 pilot demonstrates the political difficulty of implementing policies that lean against asset bubbles.[3] Specifically, interest groups (real-estate professionals as well as community activists) protested against the legislation. Both groups viewed the preceding run-up in real-estate prices as an opportunity for their constituents to achieve their goals (profits or housing access), and they therefore perceived the legislation as harmful.

Our paper relates to two strands of the literature. The first explores the role of intermediaries in precipitating the financial crisis. Keys, Mukherjee, Seru, and Vig (2010) show that securitization leads to lax screening by mortgage lenders. Ben-David (2011, 2012) finds that intermediaries expanded the mortgage market by enabling otherwise ineligible borrowers to misrepresent asset valuations to obtain larger loans and by pushing buyers to overpay for properties. Rajan, Seru, and Vig (forthcoming) show that soft information about borrowers is lost as the chain of intermediaries in the origination process becomes longer, leading to a decline in the quality of originated mortgages. Finally, Agarwal and Ben-David (2013) study the role of loan officer compensation leading up to the financial crisis.

The second strand of the literature studies predatory lending in personal finance. In particular, researchers have focused on the debate about whether payday lending helps or exploits borrowers. Morse (2011) shows that borrowers in areas with payday lending are more resilient to natural disasters. In contrast, Melzer (2011) uses cross-border variation and finds no evidence that payday lending alleviates hardship. Bertrand and Morse (2011) find that providing additional information about loans to payday borrowers reduces loan take-up. Agarwal, Skiba, and Tobacman (2009) show that payday borrowers preserve access to formal credit through their credit cards while paying very high interest rates on their payday loans.

2. Illinois Predatory Lending Database Pilot Program (HB4050)

The pilot program that we use in this paper as our experiment took place from September 2006 to January 2007. The purpose of the current section is to provide background about the program.

2.1. Description of the pilot program

In 2005, the Illinois legislature passed a bill intended to curtail predatory lending. Although the state had a number of antipredatory provisions in place, like prevailing practices elsewhere in the country, they were based on loan characteristics. Some political leaders in Illinois became concerned about the ease with which lenders could avoid the trigger criteria of antipredatory programs by creatively packaging their loans. For instance, balloon mortgages targeted by regulations were replaced with adjustable rate mortgages (ARMs) with short fixed-rate periods and steep rate reset slopes (the so-called 2/28 and 3/27 hybrid ARMs).[4] Consequently, the new bill included a new enforcement mechanism and tougher penalties for noncompliance. It also sought to educate borrowers prior to closing on their new mortgage loans.

To that effect, the legislation sponsored by Illinois House Speaker Michael Madigan mandated review of mortgage offers for high-risk borrowers by HUD-certified housing counselors. High-risk borrowers were defined as applicants with sufficiently low credit scores or sufficiently risky product choices. The legislation set the FICO score threshold for mandatory counseling at 620, with an additional provision that borrowers with FICO scores in the 621–650 range receive counseling if they chose what the regulation defined as high-risk mortgage products. Such mortgages included interest-only loans, loans with interest rate adjustments within three years, loans underwritten on the basis of stated income (low documentation loans), and repeated refinancings within the past 12 months (Category I loans). Borrowers were subject to counseling regardless of their FICO score if they took out loans that allowed negative amortization, had prepayment penalties, or had closing costs in excess of 5% (Category II loans). The proposal was modeled on a Federal Housing Administration (FHA) program from the 1970s (Wall Street Journal Online, 2007) and it generated considerable excitement among Illinois lawmakers, who passed House Bill 4050 on the last day of the 2005 legislative session. HB4050 applied only to loans offered by state-licensed mortgage lenders, as the state lacks legal authority to regulate federally chartered institutions and generally exempts them from mortgage licensing requirements. Furthermore, HB4050 applied only to select neighborhoods, namely, ten zip codes on the City of Chicago's South Side.

The need for a high-risk borrower counseling session was determined on the day of the application, and the borrower had ten days to contact the agency to schedule it. The lender was required to cover the $300 cost of the session. The goal of these sessions, lasting one to two hours, was to discuss the terms of the specific offer for a home purchase or refinancing loan and to explain their meaning and consequences to the prospective borrower. The counselors were not supposed to advise borrowers about their optimal mortgage choice in the sense of Campbell and Cocco (2003); instead, they were to

---

[3] As discussed in detail in Section 2.2, the program was terminated early, providing further evidence of the high cost of identifying predatory lending, The regulators could not withstand the political pressure associated with implementing the program.

[4] For a detailed analysis of the impact of the state antipredatory lending laws on the type of mortgage products used in the market, see Bostic, Chomsisengphet, Engel, McCoy, Pennington-Cross, and Wachter (2012).

warn them against common pitfalls. The counselors were also expected to verify the loan application information about the borrower (e.g., income and expenses). None of the recommendations was binding. Borrowers could always choose to proceed with the loan offer at hand.

At the end of the session, the counselor recorded a number of findings in a state-administered database. These included whether the lender charged excessive fees, whether the loan interest rate was in excess of the market rate, whether the borrower understood the transaction or could afford the loan, and so forth. Even though HB4050 established the database for pilot evaluation purposes, lenders feared that the state's collection of this information could lead to potential regulatory (e.g., license revocation) or legal (e.g., class-action lawsuits) actions.

As another direct penalty for noncompliance, lenders lost the right to foreclose on a delinquent property. Under HB4050, title companies did not receive a "safe harbor" provision for "good faith compliance with the law." As a result, clerical errors at any point in the loan application process could potentially invalidate the title, making the lender unable to pursue foreclosure.[5] Finally, lenders reportedly feared losing some of their ability to steer borrowers toward high margin products.

The new regulation imposed costs on borrowers as well. Even though session fees had to be borne by the lender, anecdotal evidence suggests brokers attempted to pass them on to borrowers in the form of higher closing costs and administrative charges (Bates and Van Zandt, 2007; and personal communication with mortgage counselors). HB4050 also imposed time costs on borrowers. By lengthening the expected time until closing, the new law could force borrowers to pay for longer credit lock periods, further raising loan costs.

Both the counseling session and the independent collection of borrower data allowed counselors to form their own assessment of the borrower's creditworthiness. Effectively, the counselors were able to elicit private information that might or might not have been used by lenders to make approval or pricing decisions and then give that information to state regulators. This external verification process, together with strict penalties for noncompliance, likely provided strong incentives for lenders to better screen out marginal applications prior to

referring approved applications to counseling. One extreme form of screening was to cease lending in HB4050 areas altogether.

A report by the nonprofit Housing Action Illinois (2007) summarized the counselors' assessments of HB4050 covered loans. Over the course of the pilot, about 12 hundred borrowers had their loan offers reviewed by 41 HUD-certified counselors from 11 agencies. Housing Action Illinois (2007) reports that 9% of the mortgages were deemed to have indications of fraud. About half of the borrowers were advised that they could not afford the loan or were close to not being able to do so. For 22% of the borrowers, loan rates were determined to be more than 300 basis points above the market rate. For 9% of the borrowers, the counselors found a discrepancy between the loan documentation and the verbal description of the mortgage. Perhaps most alarmingly, an overwhelming majority of borrowers who were receiving adjustable rate loans did not understand that their mortgage payment was not fixed over the life of the loan.

2.2. Early termination of the pilot program

The program was meant to run as a four-year pilot in select Chicago neighborhoods. Afterward, its coverage was expected to be expanded to the entire metropolitan area. In spite of vocal opposition from community-based groups and affected lenders, Illinois politicians clamored to have their districts included in the pilot. This effort by politicians looks particularly ironic in retrospect, given the eventual response of the population in the pilot area.

Only loans offered by state-licensed mortgage lenders were subject to HB4050. In disadvantaged Chicago neighborhoods, much of the lending had been done through state-licensed mortgage bankers, which presented themselves as a local and nimble alternative to the more traditional bank lenders.[6] Consequently, the legislation was likely to increase the regulatory burden on the very entities providing credit in the selected pilot area. The possibility that this could result in credit rationing prompted many observers to voice concern about the potential effect of HB4050 on housing values in the selected zip codes.

The geographic focus of the legislation differed substantially from typical regulatory approaches that require counseling for certain loan types (Bates and Van Zandt, 2007). This feature of the legislation generated considerable opposition from community activists and residents and prompted several lawsuits. Because the selected pilot area was overwhelmingly populated by Hispanic and African American residents (81%; see Table 1, Panel A), the selection also prompted heated accusations of discriminatory intent on the part of lawmakers. Specifically, community activists formed an organization named the Coalition to Rescind HB4050, led by John Paul (president of the Greater Englewood Family Taskforce) and Julie Santos

---

[5] According to the Cook County Recorder of Deeds, even federally regulated lenders had to procure a certificate of exemption from HB4050 to obtain a clean title. Consequently, all lenders were affected to at least some degree by the legislation. This feature of HB4050 caused some investors to warn about their willingness to purchase loans originated in pilot zip codes. Most of these warnings stipulated that to be eligible for purchase, a loan had to receive a certificate of counseling or of exemption from counseling. However, the presence of one of these certificates was a requirement for loan closing and recording, which itself is a prerequisite for sale or securitization under standard purchase criteria. It is thus unclear whether would-be investors had any additional reasons to worry about recorded loans under HB4050. In any event, the share of securitized loans in the treated zip codes declined from 83% to 70% during the pilot period. However, this decline was not appreciably different from that in the control zip codes. It thus appears that the pilot did not have a sizable impact on secondary market activity counter to the historical experience in Georgia and New Jersey discussed in Keys, Mukherjee, Seru, and Vig (2010).

[6] Using the Home Mortgage Disclosure Act data described in detail in Section 3, we estimate that state-licensed mortgage bankers accounted for 64% of mortgage loans originations in the HB4050 zip codes during 2005.

Table 1

Summary statistics.

The table presents summary statistics for the data used in the study. Panel A compares demographics [from the 2005 Internal Revenue Service (IRS) Statistics of Income ZIP code data and 2000 census] of the treated zip codes, the control zip codes, and the rest of Chicago. Panel B focuses on mortgages originated by state-licensed lenders. It compares means and standard deviations of the main variables used in the analysis across the treated zip codes, the control zip codes, and the matched loan (synthetic) sample and across periods of time (pretreatment, during treatment, and post-treatment). Panel C presents similar statistics for non-state-licensed lenders. Panel D shows summary statistics (means and standard deviations) for non-state-licensed lenders. Panel E presents summary statistics for the variables in the data set received from a counseling agency and for the matched sample for these loans. LP data=First American CoreLogic LoanPerformance data; ARM=adjustable rate mortgage; no/low doc=no or low documentation; LTV=loan-to-value.

Panel A: Construction of a control sample on the basis of pretreatment socioeconomic characteristics (2005 IRS statistics of income zip code data and 2000 census data)

Statistic	HB4050 zip codes (ten zip codes)	Control zip codes (12 zip codes)	Other Chicago zip codes (31 zip codes)
Total population	729,980	713,155	1,467,491
Total number of 2005 tax returns	259,884	244,326	642,281
Share of minority households*	0.813	0.863	0.416
Share of blacks	0.534	0.645	0.156
Share of Hispanics	0.282	0.222	0.263
Share of households below poverty level*	0.200	0.245	0.163
Average taxable income in 2005[#]	$31,579	$30,844	$66,004
Share of households with income < $50,000 in 2005	0.843	0.850	0.714
Unemployment rate (2000 Census)*	0.136	0.147	0.072
Yearly change in house price index (HPI) (LP data):°			
2005	10.14%	8.92%	7.59%
2006	2.36%	1.75%	3.73%
2007	− 7.47%	− 7.59%	− 4.57%
For state-licensed loans originated in 1/2005–12/2005:			
18-month default rate	0.151	0.150	0.089
36-month default rate	0.276	0.251	0.170

*Denotes population-weighted averages; [#]denotes weighted by number of 2005 IRS tax returns; and °denotes weighted.

Panel B: State-licensed lenders

Variable	Pre-treatment (1/2005–8/2006)			Treatment Period (9/2006–1/2007)			Post-treatment (2/2007–12/2007)		
	HB 4050	Control	Synthetic	HB 4050	Control	Synthetic	HB 4050	Control	Synthetic
FICO	621.75	622.47	623.07	630.11	624.67	629.42	614.68	620.77	618.96
	[60.95]	[62.02]	[60.05]	[59.30]	[60.46]	[58.11]	[56.37]	[59.26]	[54.85]
FICO ≤ 620 (0/1)	0.49	0.49	0.47	0.42	0.47	0.42	0.55	0.49	0.50
	[0.50]	[0.50]	[0.50]	[0.49]	[0.50]	[0.49]	[0.50]	[0.50]	[0.50]
Interest spread (percent)	5.13	5.16	5.09	4.87	5.22	5.04	5.05	5.08	5.09
	[1.08]	[1.11]	[1.14]	[0.84]	[0.96]	[1.09]	[0.76]	[0.85]	[0.87]
ARM (0/1)	0.86	0.86	0.87	0.74	0.77	0.80	0.64	0.61	0.63
	[0.35]	[0.35]	[0.33]	[0.44]	[0.42]	[0.40]	[0.48]	[0.49]	[0.48]
No/low doc (0/1)	0.43	0.45	0.46	0.41	0.44	0.48	0.35	0.34	0.37
	[0.50]	[0.50]	[0.50]	[0.49]	[0.50]	[0.50]	[0.48]	[0.48]	[0.48]
Category I (0/1)	0.89	0.90	0.91	0.82	0.84	0.88	0.72	0.70	0.74
	[0.31]	[0.31]	[0.29]	[0.39]	[0.37]	[0.33]	[0.45]	[0.46]	[0.44]
Category II (0/1)	0.16	0.16	0.15	0.13	0.18	0.16	0.11	0.16	0.16
	[0.37]	[0.37]	[0.36]	[0.34]	[0.38]	[0.36]	[0.32]	[0.37]	[0.37]
100% LTV (0/1)	0.16	0.15	0.15	0.14	0.15	0.14	0.06	0.05	0.05
	[0.37]	[0.35]	[0.35]	[0.35]	[0.36]	[0.34]	[0.24]	[0.22]	[0.21]
LTV (percent)	84.70	83.43	84.05	83.92	83.25	83.87	80.60	79.10	79.72
	[12.03]	[12.83]	[12.35]	[12.09]	[12.95]	[11.81]	[12.32]	[13.50]	[12.47]
Excessive (0/1)	0.12	0.11	0.12	0.09	0.10	0.10	0.03	0.03	0.03
	[0.33]	[0.32]	[0.33]	[0.28]	[0.31]	[0.30]	[0.17]	[0.18]	[0.17]
Defaulted within 18 months (0/1)	0.184	0.187	0.162	0.247	0.285	0.264	0.297	0.291	0.274
	[0.39]	[0.39]	[0.37]	[0.43]	[0.45]	[0.44]	[0.46]	[0.45]	[0.45]
Defaulted within 36 months (0/1)	0.340	0.320	0.287	0.530	0.538	0.517	0.536	0.527	0.510
	[0.47]	[0.47]	[0.45]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]
Zip codes:	10	12	43	10	12	43	10	12	43
N	13,321	11,433	13,321	1,089	2,469	1,089	1,016	920	1,016

Panel C: Non-state-licensed lenders

Variable	Pre-treatment (1/2005–8/2006)			Treatment Period (9/2006–1/2007)			Post-treatment (2/2007–12/2007)		
	HB 4050	Control	Synthetic	HB 4050	Control	Synthetic	HB 4050	Control	Synthetic
FICO	642.78	639.90	645.64	641.77	635.57	643.57	649.71	647.41	651.28
	[61.76]	[61.12]	[60.56]	[61.24]	[57.44]	[59.49]	[63.68]	[65.83]	[63.32]
FICO ≤ 620 (0/1)	0.37	0.38	0.35	0.34	0.38	0.33	0.34	0.36	0.32
	[0.48]	[0.49]	[0.48]	[0.47]	[0.49]	[0.47]	[0.47]	[0.48]	[0.47]
Interest spread (percent)	4.99	5.05	4.72	4.98	5.03	4.80	4.56	4.82	4.28
	[1.39]	[1.32]	[1.47]	[1.22]	[1.10]	[1.21]	[1.30]	[1.21]	[1.40]
ARM (0/1)	0.68	0.70	0.72	0.65	0.67	0.70	0.48	0.54	0.55
	[0.47]	[0.46]	[0.45]	[0.48]	[0.47]	[0.46]	[0.50]	[0.50]	[0.50]
No/low doc (0/1)	0.46	0.46	0.48	0.47	0.50	0.52	0.48	0.52	0.57
	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]
Category I (0/1)	0.86	0.86	0.86	0.77	0.81	0.84	0.74	0.78	0.80
	[0.35]	[0.35]	[0.34]	[0.42]	[0.39]	[0.37]	[0.44]	[0.42]	[0.40]
Category II (0/1)	0.39	0.40	0.35	0.19	0.20	0.18	0.36	0.29	0.26
	[0.49]	[0.49]	[0.48]	[0.39]	[0.40]	[0.39]	[0.48]	[0.46]	[0.44]
100% LTV (0/1)	0.15	0.14	0.13	0.13	0.13	0.12	0.01	0.04	0.01
	[0.36]	[0.35]	[0.33]	[0.34]	[0.34]	[0.32]	[0.12]	[0.20]	[0.12]
LTV (percent)	82.94	81.87	82.02	82.95	82.61	82.35	77.59	77.05	77.23
	[13.72]	[14.40]	[13.47]	[13.39]	[13.91]	[13.37]	[13.24]	[13.96]	[12.09]
Excessive (0/1)	0.09	0.08	0.09	0.09	0.10	0.09	0.00	0.04	0.03
	[0.29]	[0.28]	[0.28]	[0.28]	[0.30]	[0.29]	[0.07]	[0.19]	[0.18]
Defaulted within 18 months (0/1)	0.156	0.135	0.114	0.207	0.240	0.203	0.147	0.254	0.190
	[0.36]	[0.34]	[0.32]	[0.41]	[0.43]	[0.40]	[0.35]	[0.44]	[0.39]
Defaulted within 36 months (0/1)	0.300	0.276	0.239	0.471	0.471	0.428	0.379	0.465	0.389
	[0.46]	[0.45]	[0.43]	[0.50]	[0.50]	[0.50]	[0.49]	[0.50]	[0.49]
Zip codes:	10	12	43	10	12	43	10	12	43
N	2,276	1,994	2,276	811	758	811	211	185	211

Panel D: Mean and standard deviations of lender-level characteristics for state-licensed lenders in the HB4050 zip codes in 6/2006–8/2006

Variable	Mean [std dev]
Lender exited during HB4050 (0/1) ($> 90\%$ decline)	0.51
	[0.51]
Share of *Category I* loans	0.88
	[0.19]
Share of *Category II* loans	0.19
	[0.28]
Share of *Excessive* loans	0.15
	[0.22]
log(Avg monthly # transactions)	−0.16
	[2.07]
N	49

Panel E: Means and standard deviations of counseling agency data and matched loans

Variable	HB 4050 loans	Matched loans
Counseled	1.00	0.00
	0.00	0.00
90 days delinquent (18 months)	0.13	0.13
	[0.34]	[0.34]
90 days delinquent (36 months)	0.41	0.28
	[0.49]	[0.45]
Foreclosure (18 months)	0.07	0.10
	[0.26]	[0.30]
Foreclosure (36 months)	0.20	0.21
	[0.40]	[0.40]
Red flag	0.38	
	[0.49]	
Red flag (fraud)	0.12	
	[0.32]	
Category I	0.84	0.82
	[0.36]	[0.38]
Category II	0.15	0.12
	[0.36]	[0.33]

Table 1 (*continued*)

Panel E: Means and standard deviations of counseling agency data and matched loans		
LTV (percent)	78.22	77.38
	[12.80]	[13.68]
Excessive	0.38	0.38
	[0.49]	[0.49]
log(Loan amount)	12.08	12.14
	[0.35]	[0.38]
N	121	1,048

(an immigrants rights activist). In the media and through vocal protests at the grassroots level, the organization put legal and political pressure on politicians to revoke the legislation.[7]

The other group to oppose HB4050 was made up of mortgage lenders and real-estate brokers, who claimed that the bill imposed onerous costs on real-estate professionals and that it reduced market activity. This group also applied considerable pressure to abolish HB4050, ranging from highly publicized refusals to lend in the pilot zip codes to joining legal actions against the legislation.[8]

As mortgage lenders threatened to withdraw from the pilot zip codes en masse, and as the tide of concerns about credit access began to rise, opposition to HB4050 reached fever pitch.[9] The pilot program was suspended indefinitely on January 17, 2007 after only 20 weeks of operation. To provide some of the flavor of the public debate, we summarize the main news items about the HB4050 legislation in the national and local media in the Appendix.

2.3. How was the pilot program area selected?

HB4050 instructed the state regulatory body (Department of Financial and Professional Regulation, or IDFPR) to designate a pilot area on the basis of "the high rate of foreclosure on residential home mortgages that is primarily the result of predatory lending practices." The pilot area announced by IDFPR in February 2006 encompassed ten contiguous zip codes on the southwest side of Chicago (the solid shaded areas in Fig. 1).[10] Four of these zip codes were located in Illinois House Speaker Madigan's district.

Table 1 summarizes some of the key demographic and mortgage characteristics for the pilot area and the rest of

the City of Chicago.[11] The mortgage data come from the First American CoreLogic LoanPerformance data set on securitized, subprime, and Alt-A mortgages (henceforth, the LP data). Panel A shows that IDFPR's decision at the time was based on the fact that these zip codes had substantially higher default rates (Column 1) compared with the rest of the city (Column 3), even though they experienced stronger growth in house prices.[12] The pilot zip codes were also predominantly minority-populated and had much higher rates of unemployment and poverty (Panel A) relative to the rest of Chicago. A simple comparison of population counts and the total number of loan originations in the nonprime-LP data strongly suggests that the HB4050 area had a disproportional share of subprime and Alt-A mortgages.

3. Data and selection of control groups and empirical test design

Our study relies on several complementary sources of data that cover the calendar years 2005–2007. First, we use data collected under the Home Mortgage Disclosure Act (HMDA) to assess elements of supply and demand for credit. In the absence of loan application and counseling data collected under the statutory authority of HB4050, we turn to HMDA as the next best source of information on loan application volume, rejection rates, and so forth. Using information from HUD and hand-collected data, we distinguish between lenders who specialize in prime and subprime loans, as well as between lenders that are licensed by Illinois and those exempt from licensing. Because the effect of the legislation was likely to be felt most acutely by state-licensed subprime lenders, we use this list to refine our analysis. Furthermore, the HMDA data allow us to examine how HB4050 affected credit supply along the extensive margin, i.e., to identify lenders that left the market altogether. Overall, the HMDA data contain

---

[7] The *Chicago Tribune* reported on November 2, 2006 that a group of residents and members of the real-estate community submitted a lawsuit against the state, claiming discrimination.
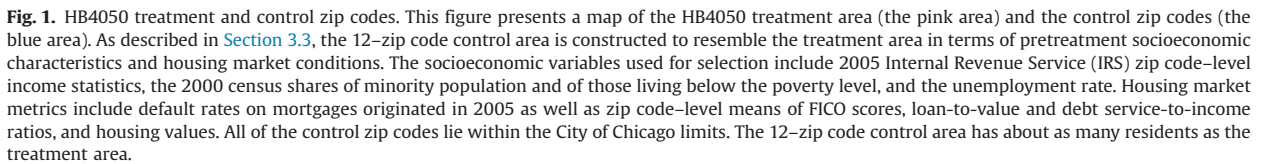
[8] The unusual confluence of interests between community activists and real-estate professionals in opposing the same regulatory action is reminiscent of the Yandle (1983, 1999) "Bootleggers and Baptists" theory. The classic example of this theory is the banning of Sunday sales of alcohol, a regulation supported by both bootleggers and Baptists. The former endorsed the legislation because it allowed them to maintain the business of illegally selling liquor without competition. The latter approved of the regulation because it directly supported their objective of discouraging consumption of alcoholic beverages.

[9] The record of a public hearing held on November 27, 2006 provides a good illustration of the acrimony surrounding HB4050 (see http://www.idfpr.com/newsrls/032107HB4050PublicMeeting112706.pdf).

[10] The HB4050 zip codes are 60620, 60621, 60623, 60628, 60629, 60632, 60636, 60638, 60643, and 60652.

[11] Panel A also provides this information for the set of 12 zip codes that comprise one of our control samples: zip codes similar to those affected by HB4050 but not chosen for the pilot. Their selection is discussed in detail in Section 3. The comparisons here are made between the ten HB4050 zip codes and the 31 Chicago zip codes that exclude both the HB4050 and the 12 control zip codes.

[12] In Table 2, we use mortgage characteristics and performance for 2005 because this was the information set available to legislators at the beginning of 2006, when the legislation was voted on. Default is defined as a 90+ day delinquency, foreclosure, or real-estate owned within 18 months of origination.

Fig. 1. HB4050 treatment and control zip codes. This figure presents a map of the HB4050 treatment area (the pink area) and the control zip codes (the blue area). As described in Section 3.3, the 12–zip code control area is constructed to resemble the treatment area in terms of pretreatment socioeconomic characteristics and housing market conditions. The socioeconomic variables used for selection include 2005 Internal Revenue Service (IRS) zip code–level income statistics, the 2000 census shares of minority population and of those living below the poverty level, and the unemployment rate. Housing market metrics include default rates on mortgages originated in 2005 as well as zip code–level means of FICO scores, loan-to-value and debt service-to-income ratios, and housing values. All of the control zip codes lie within the City of Chicago limits. The 12–zip code control area has about as many residents as the treatment area.

92,658 loans that were originated in the HB4050 zip codes during the 2005–2007 period.

We also use the First American CoreLogic LoanPerformance database to assess the effect of HB4050 on contract type and performance of mortgages originated in the treated zip codes during 2005–2007. The LP data set includes detailed borrower and loan information such as FICO scores and debt service-to-income (DTI) and loan-to-value (LTV) ratios as well as mortgage terms, including maturity, product type (e.g., fixed or adjustable rate mortgage), interest rate,

and interest rate spread. It also contains information on whether a given loan has a prepayment penalty, allows negative amortization, or requires full documentation in underwriting. These and other characteristics of the LP data are summarized in Table 1, Panels B and C. FICO scores allow us to determine which borrowers in the HB4050 zip codes were automatically or conditionally subject to loan counseling. The LP data set has 37,564 mortgage loans originated in Chicago zip codes in 2005–2007.

Because the LP data do not include information about the identity of the mortgage originator for loans, we need to match observations in the HMDA and LP data sets to examine the effects of the legislation. We match on the basis of the zip code, loan amount, and date of origination. Our matched data set yields 18,724 observations in the HB4050 zip codes.

In the later part of our analysis, we use information from one of the counseling agencies. These data are part of the database constructed under the HB4050 legislation, which includes information on original mortgage offers reviewed during 191 counseling sessions. We match these data to the Cook County Recorder of Deeds and LP data sets to obtain loan characteristics on the counseled loans. The resulting data set contains 121 loans (other loans might not have been securitized and, therefore, are not included in the LP data set). We use this data set to gauge the extent to which counseling had a direct effect on mortgage choice.

Finally, we use US Bureau of the Census and Internal Revenue Service data to control for zip code–level characteristics of income and population composition.

3.1. Constructing a zip code–based control group

To evaluate the effect of the HB4050 legislation, we develop control samples that are similar to the pilot area but are unaffected by the legislation. As discussed in Section 2.3, the selection of treated zip codes was driven by their demographic and mortgage characteristics, as well as by political considerations. In fact, HB4050 zip codes exhibit characteristics that are far from unique in the Chicago area. We use this information to construct a control group that is meant to resemble the pilot area in terms of its pretreatment socioeconomic characteristics and housing market conditions. Without the intervention, we plausibly expect the HB4050 zip codes would have experienced the same changes in outcome variables as our control group zip codes. To develop the control group, we move beyond the univariate metric of foreclosure rates to a set of measures identifying economically disadvantaged inner-city neighborhoods.

In particular, we use 2005 IRS zip code–level income statistics, the 2000 census shares of minority population and of those living below the poverty level, and the unemployment rate to identify zip codes within the City of Chicago limits that have similar characteristics and the smallest geographic distance from the HB4050 zip codes. The resulting 12–zip code area has about as many residents as the treatment area and experienced a similar path of house price changes, as summarized in Column 2, Panel A of Table 1. The statistics in Panel B of Table 1 corroborate

our conclusion that the control zip codes are similar to the treated area in terms of their high default and delinquency rates, low borrower FICO scores, and disproportionate reliance on riskier mortgage products.[13] Judging by the spirit and the letter of stated legislative guidelines, these zip codes (shown by the striped area in Fig. 1) could have plausibly been selected for HB4050 treatment.[14]

The HMDA database contains 80,876 loans originated in the 12–zip code control sample during the 2005–2007 period. The control sample contains 34,451 loan originations in the LP data set, 17,759 of which can be matched with HMDA data.

3.2. Constructing a synthetic zip code control sample

To further establish the empirical robustness of our analysis, we construct a synthetic HB4050-like area in the spirit of Abadie and Gardeazabal (2003).[15] Instead of identifying a similar but untreated set of zip codes, we build up a comparison sample loan by loan, by matching on the basis of observable loan characteristics. Specifically, for each of the loans issued in the ten–zip code HB4050 area, we look for a loan most similar to it that was issued elsewhere within the City of Chicago in the same month. The metric for similarity is the geometric distance in terms of standardized values of the borrower's FICO score, the loan's DTI and LTV ratios, the log of home value, and the loan's intended purpose (purchase or refinancing). Once a loan is matched to an HB4050-area loan, it is removed from the set of potential matches and the process is repeated for the next HB4050-area loan. The resulting synthetic HB4050-like area is made up of observations from all 43 of the non-HB4050 Chicago zip codes. Not surprisingly, 65% of the observations in this synthetic area come from the 12 comparable zip codes identified in Section 3.2 on the basis of their socioeconomic characteristics.

In Table 1, Panel B, we compare the characteristics of borrowers and mortgages in the treated zip codes sample with those in the synthetic control sample. The panel shows that for each loan characteristic the samples have very similar properties. Because we are not constrained by geographic proximity, the synthetic sample more closely matches the loans in the HB4050 treatment area than does the control sample. However, all three samples display remarkably similar characteristics.

---

[13] In an earlier version of the paper, we used the reverse sequence to construct the control sample. That is, we built up the set of control zip codes by minimizing the distance in observed mortgage characteristics in the pre-HB4050 LP data. Afterward we checked for similarities in socioeconomic characteristics between the treatment and control areas. All of the results reported below are robust to the definition of the control area and are available upon request.

[14] The control area comprises the following zip codes: 60609, 60617, 60619, 60624, 60633, 60637, 60639, 60644, 60649, 60651, 60655, and 60827.

[15] It would be ideal to look at transactions that lie on either side of the border between the HB4050 and the control zip codes to tease out the effect of the counseling mandate. Unfortunately, the LP data do not contain street addresses.

3.3. Design of tests: HB4050 legislation as an exogenous shock to predatory lending

To recap our data summary, the majority of lending in the HB4050 area was done by state-licensed lenders specializing in subprime loans. Many of these loans had short reset periods (hybrid ARMs) and prepayment penalties, and then did not require full documentation of income—all characteristics that are commonly associated with predatory lending. These loans also had been defaulting at very high rates (more than 20% of subprime loans originated in HB4050 zip codes in the year prior to the pilot defaulted within their first 18 months). Thus, to the extent that HB4050 made it more difficult for this subset of lenders to originate such loans with high ex post default rates, we regard the pilot as an exogenous shock to lending practices with potentially predatory characteristics.

Our empirical analysis is based on the idea that HB4050 did not have a material effect in untreated but similar areas. If predatory lending leads to higher default rates, we would expect the negative exogenous shock to such lending practices to have a sizable effect on loan performance.

Another way to think about the proposed empirical design is as a two-stage analysis. In the first stage, we verify that the legislation had a material effect on mortgage origination practices in the treated area. For example, we show that in the treated area the fraction of high-risk mortgages declined significantly, as did the overall volume of originations and the number of active lenders.

The second stage of the analysis measures the effect of the shock to the lending market on mortgage performance. This stage is based on cross-sectional and temporal variation in a difference-in-differences framework. Specifically, our tests measure the difference in the response of various variables (e.g., default status, contract choice, etc.) as a function of whether the loan was originated in a zip code subject to HB4050. Our regressions include both time controls and cross-sectional controls, as in classic difference-in-differences analysis.

Our basic regression specifications have the following form:

$$Response_{ijt} = \alpha + \beta\ Treatment_{jt} + \gamma\ Time\ dummies_t$$
$$+ \delta\ Zip\ dummies_j + \theta\ Controls_{ijt} + \varepsilon_{ijt}, \qquad (1)$$

where $Response_{ijt}$ is the loan-level response variable, such as default status of loan i originated at time t in zip j; $Treatment_{jt}$ is a dummy variable that receives a value of one if zip code j is subject to mandatory counseling in month t and the loan is originated by a state-licensed lender and zero otherwise; and $Time$ and Zip dummies capture fixed time and location effects, respectively. In all regressions, we cluster errors at the zip code level.[16] For each loan, the response is evaluated at only time (e.g., interest rate at origination or default status 18 months thereafter). Consequently, our data set is made up of a

series of monthly cross sections. The set of controls varies with the underlying data source, but it includes variables such as LTV at origination, borrower FICO score, and loan interest rate.

3.4. Discussion of the exclusion restriction and the context of the estimates

Our empirical tests provide estimates of the effect of the antipredatory program on the performance of loans. Here we discuss whether our estimates of improved mortgage performance can be attributed to the reduction in predatory lending and whether the result can be generalized to the entire national market. We identify the effect of predatory lending on borrower default based on the assumption that HB4050 affected default rates only through its impact on predatory lending (the exclusion restriction). This assumption might not hold for two main reasons: (1) the legislation is likely to have altered additional aspects of borrower decision making, and (2) the legislation is likely to have induced spillovers of borrowers and lenders from the treated zip codes into neighboring zip codes. Below, we analyze the potential effects of violating the exclusion restriction assumption.

The antipredatory program affected the performance of loans through two main channels: oversight and education. First, the program imposed oversight on lenders by subjecting their loan offers to external review, thus causing predatory lenders to be more cautious. Second, the program provided a detailed review to borrowers, which could have improved their decision making. During the 20 weeks in which the pilot program took place, more than 12 hundred borrowers received information about mortgages. In our sample of 191 loans, about 19% did not pursue their loan application following the counseling and another 40% modified some of the mortgage characteristics. Although it is difficult to clearly distinguish between the channels, our measurement of the effect of predatory lending relies on the direct effect of the program through oversight. It is plausible, however, that the indirect channel of education violates the exclusion restriction and that some of the effect of the antipredatory program on default rates came through this indirect channel.

Moreover, spillovers of loans from the treatment sample to the control sample violate the exclusion restriction because they could have adversely affected the quality of loans originated in the control sample. Such spillovers could have happened along three dimensions: spatial, cross sectional, and temporal. First, potential purchasers could have moved from the treated area to surrounding areas (most likely to the control zip codes, as they have similar characteristics). Fig. 2, Panel A, presents the volume of applications in the treated zip codes and the control sample zip codes, per state-licensed lenders (treated) and non-state-licensed lenders (not treated). Fig. 2, Panel B, presents similar analysis for originated mortgages. Panels A and B show no apparent spillover in volume. Second, borrowers could have shifted to lenders in the treated area that were not subject to the legislation, i.e., non-state-chartered lenders. However, again Fig. 2 does not show evidence of such a move. One possible explanation for the

---

[16] Clustering allows for an arbitrary covariance structure of error terms over time within each zip code and, thus, adjusts standard error estimates for serial correlation, potentially correcting a serious inference problem (Bertrand, Duflo, and Mullainathan, 2004). Depending on the sample, there are 22 or 53 zip codes in our regressions.
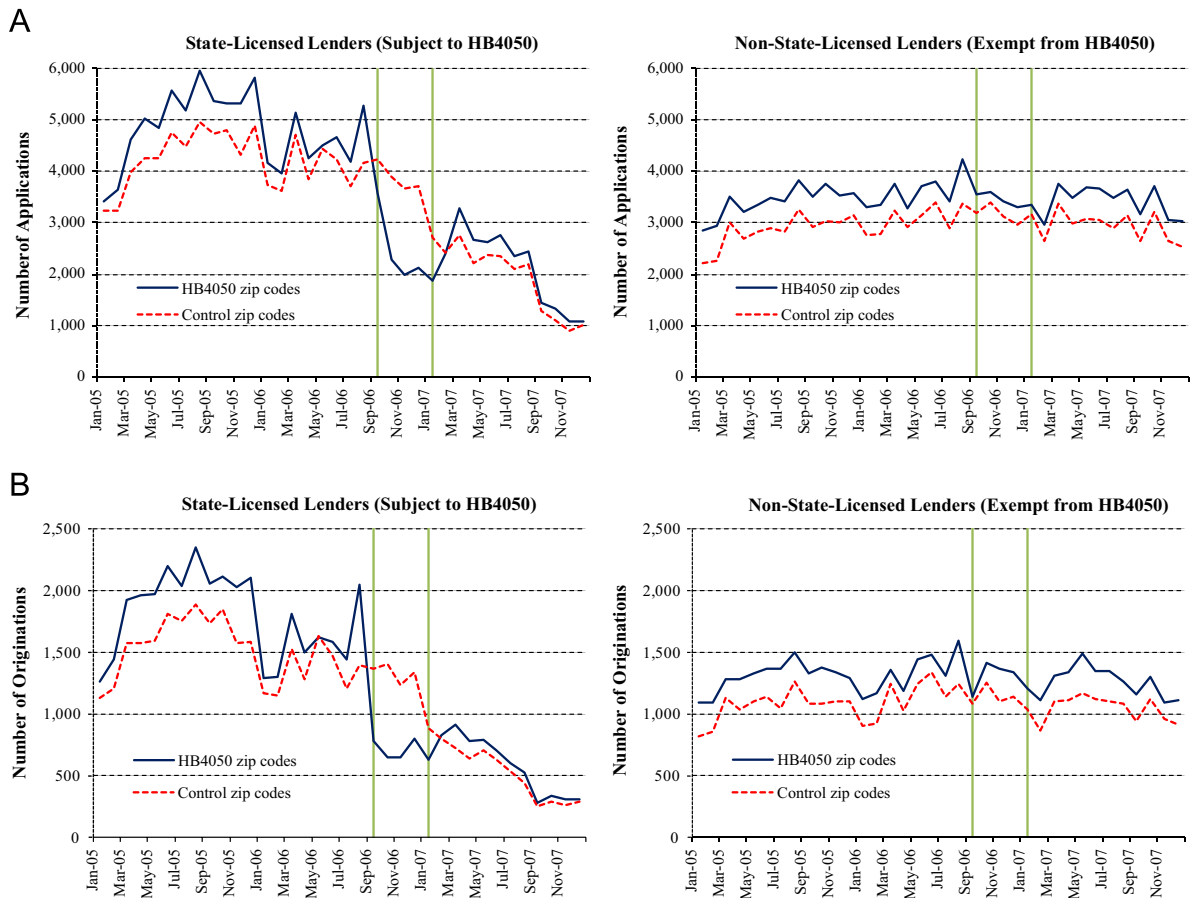
Fig. 2. HMDA loan application filings. This figure presents a time series of loan application filings. Panel A depicts filings in both the treatment and control areas, separating lenders subject to HB4050 and those exempt from it. Panel B focuses only on the treatment areas, differentiating between lenders that remained active during the mandate period and those that exited HB4050 zip codes. The solid vertical lines denote the time during which HB4050 was in force. Panel A: Number of mortgage applications in HB4050 zip codes and in control zip codes, per state-licensed and non-state-licensed lenders. Panel B: Number of mortgage originations in HB4050 zip codes and in control zip codes, per state-licensed and non-state-licensed lenders.

lack of spillover is that the market was segmented and state-licensed lenders and non-state-licensed lenders serviced different populations (subprime and prime borrowers, respectively). Third, there is the possibility of a run-up in mortgage applications in the treated area before the starting date of the program (which was known in advance). Panels A and B do suggest some buildup in applications and approvals before the onset of HB4050. They also show a minor tick-up immediately after the termination of the program, in March 2007. Potentially this is a spillover effect, and these loans could have been originated during the legislation period.

We argue that both violations of the exclusion restriction assumption cause our estimates of the effect of predatory lending on borrower default rates to be overstated. If the program affected default rates through the education channel, then we cannot ascribe the entire measured effect to the elimination of predatory lending. Further, if there were spillovers, then the credit quality of the control group is worse than it would be otherwise, creating a greater spread in default rates between the treated and control groups.

In a similar vein, the legislation likely had an effect not only on predatory lending, but also on merely aggressive

lending practices that pushed the boundaries of legally permissible behavior without violating them. In this case, the effect that we attribute to predatory lending should be ascribed to both predatory and aggressive lending practices. With the exception of our analysis in Section 6, distinguishing between the two types of practices is difficult. Nevertheless, this limitation also bounds our results from above. Even if we overstate the number of predatory loans eliminated by the pilot, our results show that the effect on default rates is relatively small.

A second issue is whether the effects of predatory lending measured in the context of the HB4050 legislation can be extrapolated to the national level. There are several reasons to be skeptical of this. The treatment area was characterized by high delinquency rates due, supposedly, to predatory lending. This lending was done by a particular subset of financial intermediaries who were readily identifiable and subject to state regulation. The penalties for noncompliance were fairly harsh, partially because of lack of clarity regarding enforcement. All of these factors are unlikely to hold for the country as a whole, limiting the effect of such regulatory intervention. Furthermore, the limited geographic scope of HB4050 made it relatively

easy for lenders to exit, whether because of high compliance costs or for the strategic goal of highlighting the contractionary effects of the pilot on credit availability. This amplified the effect of the pilot but would not be applicable at the national level. For all of these reasons, it is likely that our estimates are an upper bound for the effect of predatory lending.

4. The effects of HB4050 on predatory lending

The legislation disrupted mortgage markets by changing the loan origination process for certain borrowers and products. This section empirically evaluates its effect on loan volumes, borrower and mortgage characteristics, and lender participation.

4.1. Impact of the legislation on application and mortgage volumes

We measure mortgage market activity by the volume of loan applications and loan originations captured in the HMDA database.[17] Fig. 2, Panel A, depicts the total number of loan applications in the treated zip codes (the solid line) and in the control zip codes (the dashed line).

This information is reported in two panels that further subdivide applications reported by state-licensed lenders (who are subject to the legislation) and all other lenders (labeled exempt).

A precipitous decline is evident in loan applications among state-licensed mortgage lenders in HB4050 zip codes around the time the regulation became effective (September 1, 2006). For these lenders, the application volume dropped from 5,276 in August 2006 to 3,584 in September (32% decline), and to 2,275 in October. We observe some run-up in applications in the treated areas prior to the legislation period, though it is much smaller than the subsequent drop. In contrast, application levels in control zip codes hold steady through December 2006. Following the repeal of HB4050, activity levels in both control and treatment areas converge nearly instantaneously, and then they plummet jointly to about half that of the market heyday. For non-state-licensed lenders (graphic on the right), we observe no differential effect in the HB4050 and control zip codes throughout the period examined.

We observe similarly striking evidence when examining mortgage originations. In Fig. 2, Panel B, the left graphic shows mortgage originations for state-licensed lenders. Originations in HB4050 zip codes collapsed from 2,046 in August 2006 to 785 in September 2006 (a 62% decline) and remained at this depressed level until the end of 2006. Their levels completely converged with originations in the control zip codes following the termination of the program in February 2007, by which time subprime lending activity was grinding to a halt nationwide. Again, we do not observe any effects of the legislation for

---

[17] We count all relevant HMDA records that have one of the following action codes: originated, denied, approved but not taken, withdrawn, or incomplete.

non-state-licensed lenders in either the HB4050 or the control zip codes.

Table 2 presents the triple difference (diff-in-diff-in-diff) analysis of the drop in activity, as captured by the HMDA data. We calculate the difference between the number of applications before and during the legislation period in HB4050 and control zip codes among state-licensed lenders. We then repeat the same calculation for non-state-licensed lenders and calculate the difference in the results between the two lender subsets. Panel A focuses on changes in the monthly rate of applications. It shows a substantial decline in the treated zip codes relative to the controls among state-licensed lenders: 51% versus 14%. In contrast, no measurable difference is evident in the number of applications to non-state-licensed lenders, which increased marginally during the pilot period in both the treated and the control areas. Consequently, the difference between changes in treatment and control applications for state-licensed and non-state-licensed lenders is striking at nearly 29%. This leads us to conclude that HB4050 significantly lowered borrower applications for mortgage credit and that its impact was, in fact, concentrated among state-licensed lenders.

Next, we use the same method to estimate relative changes in origination activity. As shown in Table 2, Panel B, we find a decrease of 61% in the total number of mortgage originations by state-licensed lenders in HB4050 zip codes, with a 68% decline in purchase mortgages and a 54% decline in mortgage refinance transactions. As with applications, the drop in originations by state-licensed lenders in control zip codes is much smaller. Although there is a marginal rise in originations by non-state-licensed lenders, the increase is far too small to compensate for losses in credit origination by state-licensed lenders. Overall, the difference in the relative decline in total originations by state-licensed lenders in the treatment area and time period amounted to 39%. This further underscores our contention that the two sets of lenders served different segments of the market.

In preparation for the default analysis that follows, we also perform a robustness check of these results based on the matched sample between the LP data set (which contains default information) and the HMDA data. The results are reported in Panel C, and they follow exactly the same pattern. The magnitude of the declines, both relative and absolute, is even stronger in the matched sample, which is heavily tilted toward state-licensed lenders that originated subprime loans. For example, the panel shows a 67% drop in originations among such lenders in treated zip codes, relative to a 14% runoff in the control area.

Also, the effect of the legislation is more pronounced for refinancing transactions relative to purchase mortgages. This result is consistent with Choi (2011), who examines the effect of antipredatory legislation on origination volumes and finds a small effect for purchase mortgages and a larger effect for refinancing mortgages. Two potential explanations can be cited for this result. First, refinancing activity is often discretionary. In particular, borrowers who refinance a loan can often wait or find alternative sources of financing. For purchases, however, if a buyer wants to complete the transaction, she usually has to take out a mortgage in a timely manner.

Table 2
Effects of HB4050 on market activity: application and mortgage volumes.

The table presents mortgage application and origination statistics for the pre-HB4050, HB4050, and post-HB4050 periods. The sample is stratified by lender type (all lenders, state-licensed lenders, and non-state-licensed lenders) and by transaction type (all, purchases, and refinances). The "Purchases" and "Refinances" categories do not necessarily add up to the "All" category because some mortgages have other purposes, e.g., home improvement. Panel A and Panel B present an analysis of mortgage applications using Home Mortgage Disclosure Act (HMDA) data. Panel C presents an analysis of mortgage originations using the matched First American CoreLogic LoanPerformance (LP)-HMDA data. Panel D limits the sample to loans to borrowers with FICO scores lower than 620.

Panel A: The effect of HB4050 on the number of mortgage applications (HMDA sample)

	Number of applications per month for state-licensed lenders					
Period	All HB4050 zip codes			All control zip codes		
	All	Purchases	Refinances	All	Purchases	Refinances
1/2005–8/2006	4,813	2,201	2,507	4,218	1,949	2,175
9/2006–1/2007 (HB4050 period)	2,371	1,086	1,238	3,642	1,631	1,937
2/2007–12/2007	2,136	619	1,453	1,882	593	1,240
Diff (9/2006–1/2007 vs. 1/2005–8/2006)	−50.7%	−50.7%	−50.6%	−13.7%	−16.3%	−11.0%
Diff-in-diff	−37.1%	−34.3%	−39.7%			

	Number of applications per month for non-state-licensed lenders					
Period	All HB4050 zip codes			All control zip codes		
	All	Purchases	Refinances	All	Purchases	Refinances
1/2005–8/2006	1,676	561	984	1,362	479	780
9/2006–1/2007 (HB4050 period)	1,808	644	1,000	1,585	615	851
2/2007–12/2007	1,885	623	1,091	1,585	561	884
Diff (9/2006–1/2007 vs. 1/2005–8/2006)	7.9%	14.7%	1.6%	16.4%	28.6%	9.1%
Diff-in-diff	−8.5%	−13.9%	−7.5%			
Diff-in-diff-in-diff	−28.6%	−20.5%	−32.2%			

Panel B: The effect of HB4050 on the number of originated mortgages (HMDA sample)

	Number of mortgages per month for state-licensed lenders					
Period	All HB4050 zip codes			All control zip codes		
	All	Purchases	Refinances	All	Purchases	Refinances
1/2005–8/2006	1,803	912	854	1,507	760	716
9/2006–1/2007 (HB4050 period)	703	294	394	1,245	529	693
2/2007–12/2007	582	154	406	508	153	339
Diff (9/2006–1/2007 vs. 1/2005–8/2006)	−61.0%	−67.7%	−53.9%	−17.4%	−30.4%	−3.2%
Diff-in-diff	−43.6%	−37.3%	−50.7%			

	Number of mortgages per month for non-state-licensed lenders					
Period	All HB4050 zip codes			All control zip codes		
	All	Purchases	Refinances	All	Purchases	Refinances
1/2005–8/2006	711	252	409	552	199	315
9/2006–1/2007 (HB4050 period)	772	261	440	627	222	355
2/2007–12/2007	722	240	418	586	209	326
Diff (9/2006–1/2007 vs. 1/2005–8/2006)	8.5%	3.4%	7.5%	13.6%	11.9%	12.7%
Diff-in-diff	−5.0%	−8.5%	−5.1%			
Diff-in-diff-in-diff	−38.6%	−28.8%	−45.5%			

Panel C: The effect of HB4050 on the number of originated mortgages for borrowers with FICO ≤ 620 (LP-HMDA sample)

	Number of mortgages per month for state-licensed lenders					
Period	All HB4050 zip codes			All control zip codes		
	All	Purchases	Refinances	All	Purchases	Refinances
1/2005–8/2006	666	278	388	572	237	335
9/2006–1/2007 (HB4050 period)	218	71	147	494	163	331
2/2007–12/2007	92	16	76	84	16	68
Diff (9/2006–1/2007 vs. 1/2005–8/2006)	−67.3%	−74.5%	−62.2%	−13.6%	−31.2%	−1.2%
Diff-in-diff	−53.7%	−43.3%	−61.0%			

Table 2 (*continued*)

Panel C: The effect of HB4050 on the number of originated mortgages for borrowers with FICO ≤ 620 (LP-HMDA sample)

| | Number of mortgages per month for non-state-licensed lenders | | | | | |
| | All HB4050 zip codes | | | All control zip codes | | |
Period	All	Purchases	Refinances	All	Purchases	Refinances
1/2005–8/2006	114	45	68	100	36	64
9/2006–1/2007 (HB4050 period)	162	57	105	152	51	100
2/2007–12/2007	19	3	16	17	4	13
Diff (9/2006–1/2007 vs. 1/2005–8/2006)	42.5%	25.1%	54.1%	52.1%	42.2%	57.7%
Diff-in-diff	−9.5%	−17.1%	−3.6%			
Diff-in-diff-in-diff	−44.2%	−26.2%	−57.4%			

Second, the HB4050 legislation specified frequent refinancing transactions as one of the triggers for identifying risky mortgages that would require counseling. Therefore, one would expect a greater decline in refinancing transactions.

In sum, the results show that the legislation had far-reaching effects on the volumes of mortgage applications and originated loans. Moreover, these effects were most pronounced in the targeted population: state-licensed lenders originating loans for low FICO score borrowers.

4.2. Differential impact of the legislation by borrower and mortgage characteristics

Given that the legislation had a significant effect on mortgage originations, we examine whether a change occurred in the composition of borrower and mortgage types during the treatment period. After all, the pilot effectively increased the cost of originating mortgages to low-credit-quality borrowers as well as the cost of originating what were considered risky loans.

To explore this issue, we use a sample including all mortgages in the LP-HMDA data set that were originated in the treatment and control zip codes between 2005 and 2007. The dependent variables are borrower and mortgage characteristics. The independent variable of interest is the interaction between the HB4050 dummy and the state-licensed lender dummy, which takes a value of one if the loan was originated subject to the HB4050 legislation. The regressions include month fixed effects interacted with a state-licensed dummy and zip code fixed effects interacted with the state-licensed dummy. This specification assures that there are fixed effects for each dimension that is differenced out (time, zip code, and type of lender). Standard errors are clustered by zip code to account for correlation within geographical areas.

The regression results in Table 3, Panel A, show that the composition of borrowers and mortgage types changed significantly following the legislation. Column 1 indicates that the average FICO score of loans originated in the treated zip codes during the HB4050 period was 7.8 points higher. This result is material as it reflects a shift of 0.13 standard deviations in the distribution of borrowers (see Table 1, Panel B). Column 2 shows further evidence that the credit quality of borrowers increased. The average interest spread declined by 0.43 percentage points, or 0.43 standard deviations. Overall, this evidence suggests

that during the treatment period, the population of borrowers was of appreciably better credit quality.

We also observe that the originated mortgages are less likely to fall into risky categories as defined by HB4050. In Columns 3 to 8, we examine the change in a variety of mortgage characteristics: whether loans are adjustable rate mortgages, have low documentation, are classified as risky mortgages by the HB4050 regulation (Category I or Category II), or are 100% loan-to-value, and whether loans are considered excessively risky (i.e., mortgages that are ARM, no or low documentation, interest only, and ≥ 95% LTV).

The regressions show that mortgages originated in the treated areas would be considered less risky by the legislation on most dimensions. Following implementation of HB4050, ARM originations declined by 5.2% ($t = 2.60$, where the base rate in the control sample is 76%), Category I loans declined by 2.6% ($t = 1.53$; the base rate is 83%), Category II loans declined by 3.9% ($t = 2.78$; the base rate is 20%), 100% LTV loans declined by 2.5% ($t = 1.39$; the base rate is 16%), and excessive loans declined by 2.2% ($t = 2.00$; the base rate is 10%). We do not detect a decline in no and low documentation loans.

In Panel B we perform a robustness test for the above results. Here we restrict the sample to lenders who did not exit from the HB4050 zip codes during the legislation period. As described in greater detail below, we define the exit group as lenders who reduced their average monthly lending rate by more than 90% relative to the prepilot period. The results show that the change in the composition of borrowers and products was independent of lender exit, i.e., the quality of borrowers and loans also increased for the remaining lenders.

Overall, these findings show that new borrowers in the treated group were of better credit quality and the originated loans were materially less risky, as defined by the legislation, than those in the control group.

4.3. Impact of the legislation on lender exit

Part of the dramatic drop in loan applications can be traced to a number of much-publicized lender withdrawals from the market. We tackle the question of market exit by counting the number of unique lenders filing HMDA reports before, during, and after the treatment period in both the treated and the control zip codes. To be counted as an active lender in a given geographic area, a HMDA-reporting institution must originate at least 10% of

Table 3

Effects of HB4050 on mortgage characteristics.

The table presents regressions of borrower and mortgage characteristics on the HB4050 indicator. The sample used in Panel A contains all lenders. The sample used in Panel B contains only lenders who stayed in the market during the treatment period in the HB4050 zip codes. *HB4050* is an indicator of whether the loan was originated in the treated HB4050 zip codes during the treatment period. *FICO* is the FICO credit score of the borrower. *Interest spread* measures the interest spread over the same-maturity Treasury rate (for fixed rate mortgages) or the quoted interest spread (for adjustable rate mortgages). *ARM* is an indicator of whether a mortgage is an adjustable rate mortgage. *No/low doc* is an indicator of whether the loan required no or low documentation. *Category I* is an indicator of interest-only loans, loans with interest rate adjustments within three years, or loans underwritten on the basis of stated income (no/low doc loans). *Category II* includes loans with negative amortization or prepayment penalties. *100% LTV* is an indicator of whether the loan has a 100% loan-to-value ratio. *Excessive* is mortgages that are defined by HB4050 as mortgages that are ARM, low documentation, interest-only, and $\geq$ 95% LTV. Standard errors, presented in parentheses, are clustered at the zip code level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively. FE=fixed effects.

Panel A: All lenders

Dependent variable:	FICO (1)	Interest spread (%) (2)	ARM (0/1) (3)	No/low doc (0/1) (4)	Category I (0/1) (5)	Category II (0/1) (6)	100% LTV (0/1) (7)	Excessive (0/1) (8)
HB 4050	1.261	−0.239***	−0.016	−0.016	−0.018	−0.042**	−0.008	−0.015
	(2.548)	(0.046)	(0.024)	(0.028)	(0.021)	(0.017)	(0.020)	(0.019)
Month*state licensed FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip*state licensed FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,277	17,516	22,277	22,277	22,277	22,277	22,277	22,277
Adj. R^2	0.072	0.101	0.077	0.075	0.055	0.091	0.025	0.028

Panel B: Lenders who stayed in HB4050 zip codes during the treatment period

HB4050 zip code	7.770***	−0.295***	−0.022	0.019	−0.011	−0.043**	−0.035*	−0.021
× State-licensed lender	(1.888)	(0.037)	(0.023)	(0.021)	(0.021)	(0.016)	(0.018)	(0.013)
Month × State-licensed FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip × State-licensed FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	22,311	17,385	22,311	22,311	22,311	22,311	22,311	22,311
Adj. R^2	0.042	0.067	0.071	0.072	0.05	0.082	0.022	0.023

its prepilot average per month during the pilot period, with at least one origination in every month.[18] Panel A of Table 4 summarizes the results of this exercise. Of the 89 active state-licensed lenders in the treated zip codes in the prelegislative period (January 2005–August 2006), only 46 continued to lend during the treatment period. In a reprise of mortgage origination results, the decline in the number of lenders is much greater in the treatment areas, and exit is concentrated among state-licensed lenders.

The legislation created some legal uncertainty about the enforceability of mortgage contracts in the treated zip codes. This ambiguity by itself could have accounted for the strong lender response along the extensive margin. It is also conceivable that exit from HB4050 areas was a strategic response by lenders determined to emphasize the disruptive nature of this high-profile regulation.

We explore the characteristics of exiting lenders in Panel B of Table 4. Because we want to focus on mortgage contract features and performance, we need to work with the LP-HMDA data set, which contains fewer lenders. The sample includes 55 lenders that were active in the HB4050 zip codes during the prepilot period (January 2005–August 2006). The majority of these lenders (43) were state-licensed. We focus on loans originated during calendar year 2005, when HB4050 discussions were not prevalent.

---

[18] None of the patterns depends on the choice of the threshold level or geographic area. The "every month" condition is intended to eliminate lenders that withdrew from HB4050 zip codes during the fall of 2006 after working off their backlog of earlier applications.

Exiting lenders were smaller. They originated mortgages to borrowers with somewhat lower credit scores but charged a slightly lower credit spread. A higher share of those mortgages had adjustable-rate contracts. They originated a higher share of Category I and II loans, but a lower fraction of their loans had no equity (100% LTV). Yet, on net, the 18-month default rate on mortgages originated in 2005 is measurably higher for state-licensed lenders that ended up exiting HB4050 areas—11.6% versus 10.4% for lenders that continued operating. Also, non-state-licensed lenders in the LP-HMDA sample appear to have had very low lending volumes and to have been serving a much higher credit quality population.

Overall, our results show state-licensed lenders were more likely to exit the HB4050 zip codes during the legislation period. These lenders appear to have served a population with lower credit quality and to have provided loans that were categorized by regulators as risky.

5. The effect of the antipredatory program on default rates

Sections 3 and 4 establish that the legislation reduced market activity in general and, in particular, improved the average credit quality of borrowers, improved the risk profile of mortgages, and affected lenders that originated risky loans. From the point of view of the legislators, these effects are in line with their objective—to reduce what was perceived to be predatory lending activity.

Given these changes in the market, we now examine the effects of the legislation on mortgage performance. The

Table 4
Exit of lenders due to HB4050.

    The table presents analysis of lender exit during the HB4050 pilot. Panel A presents statistics about the number of lenders in the treatment and control areas during different time periods. Panel B shows summary statistics of lenders by whether they stayed or exited the HB4050 zip codes during the antipredatory pilot. The statistics presented are based on 55 lenders identified in the matched First American CoreLogic LoanPerformance and Home Mortgage Disclosure Act sample and are based on pre-announcement statistics from January to December 2005.

Panel A: Number of lenders operating in the treatment and controls areas

	# State-licensed lenders			# Non-state-licensed lenders		
	HB4050	Control	Synthetic	HB4050	Control	Synthetic
Pre HB4050 (1/2005–8/2006)	89	79	87	33	33	35
HB4050 (9/2006–1/2007)	46	63	61	23	27	24
Post HB4050 (2/2007–12/2007)	28	29	30	15	14	18
Diff (9/2006–1/2007 vs. 1/2005–8/2006)	−48.3%	−20.3%	−29.9%	−30.3%	−18.2%	−31.4%
Diff-in-diff (control)	−18.4%			1.1%		
Diff-in-diff (synthetic)	−28.1%			−12.1%		
Diff-in-diff-in-diff (control)	−19.6%					
Diff-in-diff-in-diff (synthetic)	−15.9%					

Panel B: Characteristics of lenders who exited the HB4050 zip codes (based on 2005 originations)

	State-licensed lenders		Non-state-licensed lenders	
Statistics in January to December 2005	Stayed	Exited	Stayed	Exited
Number of mortgages (per month)	17.7	11.6	4.8	0.7
Average *FICO*	645.8	639.6	675.5	710.5
Average *Interest spread* (percent)	4.84	4.73	4.10	3.30
Percent *ARM* (0/1)	74.0	83.8	59.2	50.0
Percent *No/low doc*	45.0	52.6	61.3	100.0
Percent *Category I*	86.6	93.0	81.3	100.0
Percent *Category II*	26.8	29.0	35.7	0.0
Percent *Default within 18 months*	10.4	11.6	9.2	12.5
Percent *100% LTV*	22.7	14.6	8.9	0.0
Percent *Excessive*	11.2	14.0	6.6	0.0
# Lenders	21	22	11	1

hypothesis that we test is whether lower predatory lending activity had a material effect on mortgage default. We measure loan performance by flagging borrowers who defaulted on their loans within 18 months of origination.[19] We then estimate a series of ordinary least squares (OLS) regressions, as defined in Eq. (1), in which the set of controls includes measures of borrower credit quality (FICO score), contract terms (LTV ratio, interest spread, and logged property valuation), and contract type (no or low documentation, and indicators for Categories I and II). In addition, we include three sets of fixed effects: month dummies interacted with a state-licensed lender indicator, zip code dummies interacted with a state-licensed lender indicator, and zip code interacted with calendar month. These fixed effects control for variation in all three dimensions that define the treatment: zip code, month, and lender type.

    We present two sets of base regressions in Table 5, Panels A and B. Panel A uses a sample based on all mortgages between 2005 and 2007 originated in the pilot zip codes in addition to mortgages in the 12–zip code control group. Panel B uses the same treatment group but uses the synthetic control sample of matched loans (as defined in Section 3.3). The dependent variable in all

regressions is an indicator of loan default within 18 months. In all regressions, the variable of interest is an indicator of whether the loan is part of the treatment group (i.e., originated by a state-licensed lender during the pilot period in the pilot zip codes).

    Each panel has six regression specifications. In Column 1 in each panel, we include no controls other than the fixed effects described above. In Column 2, we add controls for FICO score and logged loan amount. In Column 3, we add the no or low documentation indicator, indicators of whether the loans fall under Category I or Category II, and the LTV variable. Columns 4 to 6 repeat the specifications of Columns 1 to 3 but add lender fixed effects.

    The results in Table 5, Panels A and B, show that when we consider the entire treated group, the effects of the legislation on default rates are virtually nonexistent. In Panel A, there is no discernible effect. In Panel B, the effect on the treatment loans is negative, but it is economically small and statistically insignificant. The magnitude of legislation-related declines in default rates is about 1 percentage point (no lender fixed effects) or about 2.5 percentage points (with lender fixed effects). The unconditional likelihood of default in the loan-matched control sample during the pilot time is 23.8%. Hence, the legislation caused a decline in default of up to 11% for the treated group.

    In Panels C and D, we present additional specifications of the baseline regressions to hone down on drivers of

---

[19] A loan is considered defaulted if it is 90+ days past due, in bankruptcy, or in foreclosure or is real-estate owned by the lender.

default in different subsamples, focusing on subgroups that are likely to exhibit stronger results. Panel C uses a control sample that is based on the 12 control zip codes, and Panel D is based on the matched synthetic sample. In the first specification, the sample includes only subprime borrowers (FICO ≤ 620). In the second specification, the sample is restricted to loans originated by state-licensed lenders. The third specification includes only loans made to subprime borrowers by state-licensed lenders. The fourth specification is restricted even further to state lenders who did not exit from the sample.

Comparing the results in the different specifications sheds light on the source of the decline in default rates. Specifically, the decline in the default rate is steepest in the matched synthetic sample that is restricted to the subprime population and state-licensed lenders. It is also the only sample in which the improvement in default rates is statistically significant. For this population, the decline in default is about 6 to 7 percentage points, out of a base rate of about 27.2% (third specification in Panel D). This means that the improvement in mortgage performance is concentrated in the subprime population.

To answer the question of whether the decline in default is due to the exit of predatory lenders, we compare the third and fourth specifications. The samples used in these specifications are the same except that the latter is limited to lenders that continued lending in the treatment zip codes during the pilot period. The regressions show that the decline in default dissipates once this restriction is imposed. Put differently, we observe no improvement in the performance of subprime loans originated by lenders that continued to operate in HB4050 zip codes during the pilot period. The juxtaposition of these results leads us to conclude that the decline in default was driven by lender exit.

In Panel E, we provide additional specifications for robustness purposes. The dependent variable in these regressions is an indicator of whether the loan defaulted within 36 months (as opposed to 18 months earlier). The motivation for this is to verify that our results are not driven by a difference in the sensitivity of the explored populations to the financial crisis. A period of 36 months extends well into the crisis period. Then, we rerun the third and fourth specifications from Panels C and D. The results show that the decline in default mirrors the panels above.

To summarize, our results show that the overall effect of the legislation on mortgage default rates in treated areas was relatively minor: a decline of 1 to 2.5 percentage points, for a base rate default of 24% to 27%. We find, however, that the pilot had a stronger effect on the subprime population, for whom the improvement in default rates was statistically and economically significant: between 5 and 7 percentage points, for a base rate of default of 27% (18% to 26% relative decline). Furthermore, our evidence shows that the decline in defaults was driven by lender exit. Hence, these findings suggest the withdrawal from the market of some lenders resulted in a measurable improvement in loan performance for those subprime borrowers who were able to access the credit market.

6. Estimating the default rate of predatory loans

With estimates obtained in Sections 3–5, we can calculate the default rate of predatory loans, after making some assumptions about loan distribution across markets. In this exercise, predatory loans are defined as loans that were not originated during the HB4050 pilot period, presumably because of the effect of the treatment. Our goal here is to approximate the hypothetical default rate of predatory loans.

We estimate that the default rate on subprime loans that were not originated due to HB4050 would have been about 32%. We perform this algebraic calculation in the following manner. From Table 1, Panel B, we see the default rates in the treated and control samples were very similar both before and after the treatment period. It was only during the treatment period that they diverged. Thus, assuming they would have been similar without the treatment, we can reconstruct (back out) the hypothetical default rate for the precluded predatory loans. In the synthetic control sample of state-licensed subprime loans, all originated loans during the treatment period had a default rate of 26.4% (Table 1, Panel B, Column 6). In the treated group, loan activity declined by 53.7% (Table 2, Panel C, state-licensed lenders), and the default rate declined by about 7.0 percentage points (Table 5, Panel D, third specification, Columns 4–6. Therefore, the precluded predatory loans would have required a default rate of 32.4% to reach an average default rate of 26.4%.[20] With this calculation, predatory loans appear to have a default rate 6.0 percentage points (32.4%–26.4%) higher than loans that are not predatory.

It is important to note that the pilot also disrupted mortgage activity outside of the narrowly defined subprime segment of the market. Those segments did not realize any gains from improved mortgage performance (e.g., Table 5, Panels A and B) but did experience the costs of the disruption.

Because of the assumptions required for the above calculation, we utilize alternative data to test for robustness of this estimate using a different methodology. Specifically, we calculate the default rate of predatory loans using data provided by one of the HUD counseling agencies that collected loan and borrower information as part of the HB4050 mandate. Our sample includes 121 loans that we could match to loans in the LP data set.

The counselors flagged some loans as potentially being predatory. They used three main criteria in making this determination: the loan was priced above the market rates, the borrower was thought not to be able to afford the loan, and the loan was thought to have fraud indicia (Housing Action Illinois, 2007). We use these flags to measure the effect of predatory lending in two empirical settings.

Our first test compares the performance of loans with predatory flags with those not identified as predatory. Table 6, Panel A, shows the results. A loan flagged as predatory yet still originated had a 6.5 percentage points higher default rate (90+ days delinquent) within 18

---

[20] $(26.4\% - 7.0\%) * (100\% - 53.7\%) + X * 53.7\% = 26.4\%$; $X = 32.4\%$.

Table 5
The effect of HB4050 on borrower default.

   The table presents regressions of borrower defaults. Panels A and B present baseline regressions. Panels A and C use the treatment sample and a control sample based on zip codes with similar demographics. Panels B and D use the treatment sample and a control sample based on one-to-one loan matching (the synthetic control sample). Panel E uses both control samples. In each panel, there are six regression specifications. In all regressions, we regress the default indicator on the treatment dummy with the following: zip code interacted with calendar month, zip code interacted with a state-licensed lender indicator, and calendar month interacted with a state-licensed lender indicator. These three sets of fixed effects (FE) control for variations in all three dimensions that define the treatment: treatment zip code, treatment month, and treatment lender group. In Column 1 in each panel, we include no other controls. In Column 2, we include controls for FICO score and logged loan amount. In Column 3, we include a no or low documentation indicator, indicators of whether the loan is Category I or Category II, and an LTV (loan-to-value) variable. Columns 4 to 6 repeat the specifications of Columns 1 to 3 but add lender fixed effects. The dependent variable is an indicator of borrower default (within 18 or 36 months). Default is defined as a 90-day delinquency. *HB4050* is an indicator of whether the loan was originated in the treated HB4050 zip codes during the treatment period. *FICO/100* is the FICO credit score of the borrower (divided by one hundred). *No/low doc* is an indicator of whether the loan required no or low documentation. *Category I* is an indicator for interest-only loans, loans with interest rate adjustments within three years, or loans underwritten on the basis of stated income (no or low documentation loans). *Category II* includes loans with negative amortization or prepayment penalties. The tables also show statistics about the default rate in the control group. Standard errors, presented in parentheses, are clustered at the zip code level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively. LP-HMDA=First American CoreLogic LoanPerformance and Home Mortgage Disclosure Act.

Panel A: All LP-HMDA loans; HB4050 loans and control sample

	All mortgages, 2005–2007; control is based on 12 zip codes					
Dependent variable: *Defaulted within 18 months* (0/1)						
Default rate for the control group during pilot: 0.274						
	(1)	(2)	(3)	(4)	(5)	(6)
HB4050 zip code × month × lender	0.010	0.009	0.008	0.001	0.000	0.002
	(0.025)	(0.024)	(0.023)	(0.028)	(0.027)	(0.026)
FICO/100		−0.045***	−0.071***		−0.044***	−0.068***
		(0.006)	(0.005)		(0.006)	(0.005)
log(Loan amount)		0.167***	0.098***		0.152***	0.094***
		(0.015)	(0.016)		(0.014)	(0.015)
No/low doc			0.048***			0.047***
			(0.007)			(0.007)
Category I			0.059***			0.060***
			(0.007)			(0.007)
Category II			−0.016**			−0.021**
			(0.005)			(0.007)
LTV			0.003***			0.003***
			(0.000)			(0.000)
Zip code FE × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Zip code FE × State-licensed FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE × State-licensed FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	No	No	No	Yes	Yes	Yes
Number of observations	36,483	36,483	36,483	36,483	36,483	36,483
Adj. R^2	0.043	0.064	0.082	0.059	0.077	0.092

Panel B: All LP-HMDA loans; HB4050 loans and loan-matched sample

	All mortgages, 2005–2007; control is based on loan matching					
Dependent variable: *Defaulted within 18 months* (0/1)						
Default rate for the control group during pilot: 0.238						
	(1)	(2)	(3)	(4)	(5)	(6)
HB4050 zip code × month × lender	−0.009	−0.013	−0.011	−0.026	−0.029	−0.022
	(0.025)	(0.025)	(0.025)	(0.030)	(0.030)	(0.029)
FICO/100		−0.050***	−0.077***		−0.049***	−0.073***
		(0.006)	(0.005)		(0.006)	(0.005)
log(Loan amount)		0.153***	0.087***		0.139***	0.085***
		(0.016)	(0.016)		(0.015)	(0.015)
No/low doc			0.047***			0.046***
			(0.006)			(0.006)
Category I			0.058***			0.060***
			(0.006)			(0.007)
Category II			−0.011*			−0.015*
			(0.005)			(0.006)
LTV			0.003***			0.003***
			(0.000)			(0.000)
Zip code FE × Month FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 5 (*continued*)

Panel B: All LP-HMDA loans; HB4050 loans and loan-matched sample

	All mortgages, 2005–2007; control is based on loan matching					
Dependent variable: *Defaulted within 18 months* (0/1)						
Default rate for the control group during pilot: 0.238						
	(1)	(2)	(3)	(4)	(5)	(6)
Zip code FE × State-licensed FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE × State-licensed FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	No	No	No	Yes	Yes	Yes
Number of observations	37,448	37,448	37,448	37,448	37,448	37,448
Adj. R^2	0.039	0.058	0.075	0.054	0.070	0.084

Panel C: Default regression with sample limited to low FICO score; control zip codes

	(1)	(2)	(3)	(4)	(5)	(6)	N	Default rate for control
FICO ≤ 620 loans by state-licensed and non-state-licensed lenders								
Treatment	−0.008	−0.006	−0.012	−0.038	−0.034	−0.033	16,967	0.257
	(0.043)	(0.043)	(0.041)	(0.041)	(0.040)	(0.039)		
Adj. R^2	0.028	0.043	0.058	0.039	0.051	0.065		
All loans by state-licensed lenders								
Treatment	−0.034	−0.035	−0.028	−0.036	−0.038	−0.029	30,248	0.285
	(0.020)	(0.021)	(0.019)	(0.023)	(0.023)	(0.022)		
Adj. R^2	0.038	0.058	0.076	0.049	0.066	0.082		
All FICO ≤ 620 loans by state-licensed lenders								
Treatment	−0.045	−0.048*	−0.039	−0.048	−0.051*	−0.041	14,668	0.264
	(0.026)	(0.026)	(0.025)	(0.028)	(0.028)	(0.027)		
Adj. R^2	0.027	0.040	0.056	0.034	0.045	0.060		
All FICO ≤ 620 loans by state-licensed lenders that did not exit								
Treatment	−0.027	−0.031	−0.020	−0.033	−0.036	−0.027	8,583	0.204
	(0.028)	(0.027)	(0.026)	(0.028)	(0.027)	(0.026)		
Adj. R^2	(0.026)	0.043	0.061	0.031	0.047	0.064		

Panel D: Default regression with sample limited to low FICO score; synthetic control

	(1)	(2)	(3)	(4)	(5)	(6)	N	Default rate for control
FICO ≤ 620 loans by state-licensed and non-state-licensed lenders								
Treatment	−0.040	−0.039	−0.042	−0.069	−0.065	−0.064	17,080	0.248
	(0.046)	(0.047)	(0.045)	(0.047)	(0.047)	(0.045)		
Adj. R^2	0.028	0.041	0.055	0.039	0.049	0.062		
All loans by state-licensed lenders								
Treatment	−0.038	−0.044*	−0.034	−0.043	−0.049*	−0.037	30,852	0.264
	(0.023)	(0.024)	(0.023)	(0.025)	(0.025)	(0.025)		
Adj. R^2	0.040	0.057	0.074	0.050	0.065	0.080		
All FICO ≤ 620 loans by state-licensed lenders								
Treatment	−0.064*	−0.071**	−0.059*	−0.070**	−0.075**	−0.064*	14,776	0.272
	(0.032)	(0.032)	(0.031)	(0.034)	(0.034)	(0.033)		
Adj. R^2	0.028	0.039	0.055	0.036	0.045	0.060		
All FICO ≤ 620 loans by state-licensed lenders that did not exit								
Treatment	−0.036	−0.042	−0.026	−0.042	−0.047	−0.033	8,529	0.193
	(0.037)	(0.036)	(0.035)	(0.035)	(0.034)	(0.034)		
Adj. R^2	0.054	0.070	0.093	0.067	0.079	0.099		

Panel E: 36-month default regressions

	(1)	(2)	(3)	(4)	(5)	(6)	N	Default rate for control
Control is based on 12 zip codes; all loans by state-licensed and non-state-licensed lenders								
Treatment	−0.006	−0.008	−0.009	−0.017	−0.019	−0.017	36,483	0.522
	(0.035)	(0.033)	(0.032)	(0.037)	(0.035)	(0.034)		
Adj. R^2	0.076	0.106	0.133	0.094	0.118	0.141		
Control is based on 12 zip codes; all FICO ≤ 620 loans by state-licensed lenders								
Treatment	−0.045	−0.050	−0.036	−0.050	−0.053	−0.039	14,668	0.515

Table 5 (*continued*)

Panel E: 36-month default regressions

	(1)	(2)	(3)	(4)	(5)	(6)	N	Default rate for control
	(0.031)	(0.032)	(0.030)	(0.032)	(0.032)	(0.031)		
Adj. R^2	0.057	0.077	0.102	0.070	0.086	0.108		
Control is based on loan matching; all loans by state-licensed and non-state-licensed lenders								
Treatment	−0.026	−0.031	−0.028	−0.036	−0.039	−0.031	37,448	0.479
	(0.040)	(0.039)	(0.038)	(0.041)	(0.040)	(0.039)		
Adj. R^2	0.076	0.099	0.125	0.092	0.111	0.133		
Control is based on loan matching; all FICO ≤ 620 loans by state-licensed lenders								
Treatment	−0.054	−0.063*	−0.045	−0.057	−0.065*	−0.048	14,776	0.500
	(0.037)	(0.037)	(0.035)	(0.036)	(0.036)	(0.034)		
Adj. R^2	0.054	0.070	0.093	0.067	0.079	0.099		

months (Column 1). This magnitude is very similar to the above calculation of a 6.0 percentage point differential in default rates (although it is not statistically different from zero given our limited sample size). We also measure the probability of default of the subsample of loans that were flagged as having indicia of fraud (as opposed to using the three criteria discussed above). We find the default rate differential for these loans to be even higher at 12.3 percentage points (Column 3).

An additional question is whether counselors flagged loans as problematic based on readily observable information or whether they also relied on soft information collected during the counseling sessions. This question is relevant for policy design because if only observable information was used in making the predatory designation, the same outcome could have been achieved with a specific underwriting protocol and without counselor review.

To tackle this question, we construct a sample of loans in the control zip codes that are matched to the 121 counseled loans for which we have contract and performance data. Specifically, we match each counseled loan with up to ten other loans in the control zip codes that were originated during the same time period, were originated for the same purpose (purchase or refinance), and had similar house prices, adjustable or fixed rate types, leverage, documentation levels, interest rates, and FICO scores.[21] Overall, our matched sample includes 1,048 loans (summary statistics are provided in Table 1, Panel E). If a counseled loan is flagged, we give it a red flag indicator and similarly tag each of its matched control loans. Assuming that the predatory designation was given based on observables only, we would expect to continue to see higher default probabilities on all red flag loans but not to see any differential impact among the red flag loans that were counseled.

Table 6, Panel B presents the regression results. Consistent with the previous evidence, the regression shows that the red flag loans have about a 6 percentage point higher default rate than nonflagged loans. The interaction with the counseling indicator suggests that the loans that were counseled are not materially different from those that were matched to them, which is consistent with the idea that loans were flagged based solely on observables.

We repeat the same exercise with red flag defined as only cases of suspected fraud. The coefficient on the fraud red flag and its interaction with the counseling indicator (Columns 3–4) are positive but very imprecisely estimated. This finding suggests that our loan-matching procedure did not match well on the fraud dimension, most likely because indicia of fraud are based on soft information collected by counselors that is unobservable in the raw mortgage servicer data. The results in Panel B indicate that some dimensions of predatory lending, such as loan affordability and excessive interest rates, are readily captured with observable data, while others such as fraud indicia are not.

To summarize, this section used multiple methods to measure the default rates of predatory loans. The different methods generated strikingly similar estimates: Predatory loans originated in this period had an 18-month default rate that was about 6 percentage points higher than similar loans that were not predatory.

7. Conclusion

Whether predatory lending was an important factor in precipitating the subprime crisis is one of the key questions in the academic and policy debate about the sources of the crisis and its aftermath. The main empirical challenge in answering this question is that it is difficult to distinguish predatory loans from nonpredatory loans on an ex ante basis. In general, predatory lenders exploit borrowers by having an informational advantage over them. This informational advantage is not easily discernible when examining the observable characteristics alone.

In this study we use an antipredatory legislative pilot (HB4050) enacted in 2006 in Chicago as an instrument for identifying predatory lending. The legislation required certain borrowers in ten zip codes in Chicago to seek counseling when they took out a mortgage from state-licensed lenders. Following implementation of the pilot, market activity dropped by about 40%, largely through the exit of lenders specializing in risky loans and through

---

[21] More precisely, we include loans that were originated within 31 days of one another and that had house prices within $8,000, leverage within 10%, interest rates within 1%, and FICO scores within 20 points.

Table 6

Measuring the default rate of predatory loans using the HB4050 data set.

The table presents analysis of a sample of counseled loans (121 loans) and a control sample of loans matched from other Chicago zip codes (every counseled loan is matched with up to ten similar loans from Chicago). Panel A presents the sample of 121 counseled loans. Panel B presents the sample of 121 together with the 1,048 matched loans. The dependent variable is an indicator of borrower default within 18 months. Default is defined as a 90-day delinquency. *Red flag* is an indicator of whether the counseled loan was identified as unaffordable or fraudulent by the US Department of Housing and Urban Development–certified counselors. *Red flag (fraud)* is an indicator of whether the counseled loan was identified as having fraud indicia. *No/low doc* is an indicator of whether the loan required no or low documentation. *Category I* is an indicator for interest-only loans, loans with interest rate adjustments within three years, or loans underwritten on the basis of stated income (no or low documentation loans). *Category II* includes loans with negative amortization or prepayment penalties. *Excessive* is mortgages that are adjustable rate mortgages, no or low documentation, interest-only, or $\geq 95\%$ loan-to-value (LTV). Standard errors, presented in parentheses, are clustered at the zip code level. *, and ** denote statistical significance at the 10%, and 5% level, respectively. FE=fixed effects.

Panel A: Predatory loans in the counseled sample				
Red flag	0.065	0.036		
	(0.068)	(0.072)		
Red flag (fraud)			0.123	0.111
			(0.106)	(0.111)
No/low doc		−0.081		−0.085
		(0.079)		(0.079)
Category I		0.210*		0.224**
		(0.112)		(0.111)
Category II		0.024		0.036
		(0.103)		(0.102)
LTV		0.001		0.002
		(0.003)		(0.003)
Excessive		0.100		0.066
		(0.207)		(0.210)
log(Loan amount)		0.161		0.144
		(0.124)		(0.124)
Month FE	Yes	Yes	Yes	Yes
Number of observations	121	115	121	115
Adj. R^2	−0.046	−0.018	−0.042	−0.010

Panel B: Comparing the counseled sample to a comparable sample in the control zip codes (18-month default rate)				
Red flag	0.061*	0.063*		
	(0.036)	(0.037)		
Counseled × Red flag		−0.011		
		(0.065)		
Red flag (fraud)			0.061	0.055
			(0.075)	(0.075)
Counseled × Red flag (fraud)				0.051
				(0.102)
Counseled	0.000	0.004	−0.001	−0.007
	(0.032)	(0.041)	(0.032)	(0.034)
Month FE	Yes	Yes	Yes	Yes
Number of observations	1,159	1,159	1,159	1,159
Adj. R^2	0.012	0.011	0.008	0.007

a decline in the share of subprime borrowers. Default rates in the subpopulation targeted by the pilot—subprime borrowers served by state-licensed lenders—improved by 6 to 7 percentage points, relative to the unconditional default rate of 26%. We use these estimates to impute a hypothetical default rate of about 32% on the presumably predatory loans that were not originated due to HB4050. We obtain similar estimates using other techniques based on subsamples of loans that were counseled and flagged as being predatory.

In interpreting our results, the first thing to note is that the pilot represented a very blunt policy tool. Its bluntness reflects the severe informational problems that exist in identifying predatory lending. Predatory lending is about unobservable information. Lenders engage in predatory lending when they exploit borrowers by having superior information about their ability to repay the loan (Bond, Musto, and Yilmaz, 2009). Given the ambiguity and subjectivity in identifying which loans are predatory and which are not, regulators must resort to rough proxies (Litan, 2001). In the case of HB4050, predatory loans were proxied as loans made in the poorer parts of the city to borrowers with low credit scores and with a certain risk profile. These proxies are imperfect and, therefore, could impose costs on lenders who do not make predatory loans and on borrowers who are not being exploited.

The bluntness of HB4050 resulted in severe market disruptions, as it drove about half of the state-licensed lenders out of the market, lowered the number of originated mortgages by about a third, and imposed high counseling costs on the remaining borrowers and lenders. The wide and fairly indiscriminate reach of the pilot also triggered intense

public opposition from all of the affected parties—communities and lenders alike—ultimately leading to its early termination. This response highlights the political difficulties involved in policy interventions in an environment with inherently complex incentive structures and asymmetric information held by lenders, borrowers, and regulators.

The pilot did appear to lower default rates among its primary target population: subprime borrowers served by state-licensed lenders. Although these improvements in mortgage performance were measurable, the default rates among the treated subset of borrowers remained alarmingly high. This suggests that potentially predatory lending practices by themselves cannot account for the meltdown in the subprime mortgage market.

Our finding that eliminating bad lenders had a greater impact on mortgage defaults than eliminating bad loans also suggests that reckless, not predatory, lending practices deserve greater scrutiny. Although answering this question is beyond the scope of this paper, these results are consistent with recent research that highlights the role of long intermediation chains in mortgage origination leading up to the crisis. In particular, the majority of lenders subject to HB4050 were mortgage banks and brokers that securitized the lion's share of their loans. Such lending practices have been found to be associated with aggressive misrepresentation of information throughout the mortgage origination and funding process and with subsequently higher defaults (Keys, Mukherjee, Seru, and Vig, 2010; Jiang, Nelson, and Vytlacil, 2014; Piskorski, Seru, and Witkin, 2013). In a similar vein, our finding that borrowers often chose to pursue bad loans in spite of (justified) warnings from counselors brings up questions of the role played by the borrowers themselves. These results join the body of literature showing that reckless lending and borrowing, combined with distorted incentives of intermediaries, were at the heart of the subprime crisis (e.g., Ben-David, 2011; Agarwal and Ben-David, 2013).

It could be tempting to conduct a back-of-the-envelope welfare analysis by linking the estimates of reductions in defaults with the costs of such defaults and of the counseling itself.[22] However, doing so fails to take into account a number of important effects—losses in utility incurred by excluded borrowers, positive spillovers on neighborhood property values from lower defaults, losses from inefficient contract choices that resulted from trying to avoid counseling sessions, and so forth. Moreover, evaluating the overall welfare effect of this intervention requires weighing the relative costs and benefits accruing to different market participants.[23] It is further complicated by various distortions that already exist in the housing market due to unique tax treatment, zoning restrictions,

and so forth, as well as potential externalities produced by individual housing decisions.

Mandated financial counseling and increased oversight of lenders are important policy tools being considered in the wake of the housing market crisis.[24] Both policies impose restrictions on free contracting between borrowers and lenders. As such, they can be expected to shrink credit markets, particularly for the financially disadvantaged segments of the population. In addition to providing an estimate of the quantitative impact of antipredatory lending programs, our paper sounds a cautionary note about the difficulties of implementing effective policy in an informationally opaque setting with complex and conflicting incentives.

Appendix. Media news around the treatment period

The following list of articles, with brief summaries, reflects the public information and sentiment regarding the HB4050 program.

July 2005
Illinois Governor Rod R. Blagojevich signs House Bill 4050, which is designed to protect homebuyers from predatory lending in Cook County's at-risk communities and reduce the incidence of foreclosures; the project is scheduled to begin January 1, 2006 (US Fed News, July 21, 2005).
Mortgage brokers are concerned about the impact of the new legislation on origination business (Origination News, July 1, 2005).
January 2006
HB4050 is postponed due to technical and logistic issues (National Mortgage News, January 9, 2006).
March 2006
Mortgage lenders object to HB4050 and argue that its compliance costs could prompt firms to withdraw from the market (Chicago Tribune, March 19, 2006).
August 2006
HB4050 goes into effect on September 1, 2006 (Chicago Tribune, August 20, 2006).
September 2006
"Mortgage pros fear new state regulations will hurt business" (Chicago Sun-Times, September 11, 2006).
"Critics say counseling requirement amounts to redlining" (Chicago Tribune, September 18, 2006).
October 2006
"State mortgage law backfires on most vulnerable homeowners" (Chicago Sun-Times, October 5, 2006)
Governor Blagojevich advertises the "[HB4050] pilot program [as] providing borrowers with critical information on home loans and help[ing] state regulators and law enforcement track and crack down on dishonest lenders" (Office of the Governor Press Release, October 31, 2006).

---

[22] For instance, we could have taken the average house value in HB4050 zip codes of $190,000 and assumed a deadweight loss from foreclosure of about 30% (Campbell, Giglio, and Pathak, 2011). Based on the estimate of a 6% higher default rate resulting from predatory lending, the expected benefit from curbing predatory lending would be $3,420 (0.06∗$190,000∗30%) per borrower. Therefore, the pre-borrower net direct benefit (net of counseling fee and approximately $100 for the cost of time) would be approximately $2,000.

[23] Some recent attempts to model the welfare effects of policy choices in household financial markets include Carlin and Gervais (2009), Bolton, Freixas, and Shapiro (2007), and Carlin (2009).

[24] In 2010 Congress passed the Dodd-Frank Wall Street Reform and Consumer Protection Act, which created a new Consumer Financial Protection Bureau (CFPB) to safeguard consumers from unfair, deceptive, and abusive practices in the financial sector. Among other approaches, CFPB is likely to consider increasing access to financial counseling.

November 2006

A group of residents and members of the real-estate community submit a lawsuit against the state claiming discrimination (Chicago Tribune, November 2, 2006).

"Governor Blagojevich announces results of increased crackdown on unlicensed mortgage loan originators" (US Fed News, November 3, 2006)

The Coalition to Rescind HB4050 delivers petitions demanding amendments to HB4050 (November 6). The Coalition is led by John Paul (president of the Greater Englewood Family Taskforce) and Julie Santos (an immigrants rights activist). They work toward the final veto session before the deadline of December 1, 2006.

John Paul says, "We are calling our State officials to 'Get on Board' and 'Get on FIRE' working to rescind this discriminatory law and head-off declining property value."

Chicago mayoral candidate Dorothy Brown (who supports the coalition) says, "At issue is whether HR 4050, the state's pilot program designed to fight predatory lending practices, is meeting its objective or whether it is hurting the very people it is supposed to help… [HB4050] makes it harder for potential homeowners to get credit in those designated areas" (South Street Journal, November 23, 2006).

"Neighborhood Housing Services (NHS) of Chicago, a nonprofit housing organization, and its lender partners are providing affordable financing to meet the needs of homebuyers and homeowners throughout Chicago and in the House Bill 4050 Pilot Program Area" (US Newswire, November 6, 2006).

Critics say the counseling requirement is racist. Julie Santos (Coalition to Rescind HB4050) says that the law has made undocumented Hispanic immigrants afraid to buy homes because they hesitate to include their legal status in the state program's database (Chicago Tribune, November 14, 2006).

"Mortgage counseling law draws 2nd suit" The suit is filed by three consumers who say they had difficulties buying, selling, or refinancing because of the law and one real-estate agent who reports a significant decline in business because of it, according to the federal complaint (Chicago Tribune, November 16, 2006).

Mortgage brokers, realtors, home builders, and community groups agree about HB4050: "[HB4050] targets and stigmatizes primarily African American neighborhoods" (Chicago Sun-Times, November 17, 2006).

Under the direction of Governor Blagojevich, Department of Financial and Professional Regulation Secretary Dean Martinez announces that he will hold a meeting with community leaders, legislators, homeowners, and real-estate professionals to discuss ways to improve the pilot program created by HB4050 (US Fed News, November 20, 2006; Chicago Tribune, November 28, 2006).

December 2006

Groups call the mortgage counseling law unfair and unnecessary. Jesse Jackson (a civil rights activist) says the law imposes unnecessary burdens for those trying to buy and sell homes in those neighborhoods (Chicago

Sun-Times, December 24, 2006).

January 2007

"Governor Blagojevich shelves predatory-loan program." Blagojevich releases the following statement: "Even though this law was designed to fight predatory loans, it is clear that the program may be negatively affecting the communities it is designed to protect" (Chicago Tribune, January 20, 2007).

A study by the University of Illinois (Bates and Van Zandt, 2007) reports that home sales declined by 30% in the targeted areas relative to comparable areas (Chicago Tribune, January 21, 2007).

March 2007

The State of Illinois revises HB4050 so that it can be implemented throughout Cook County. The new program is targeted at first-time homebuyers (Chicago Tribune, March 23, 2007).

April 2007

"Community groups argue that they are not sufficiently staffed for the new anti-predatory law" (Chicago Tribune, April 5, 2007).

June 2007

"Mortgage brokers oppose new anti-predatory lending program" (Home Equity Wire, June 15, 2007).

November 2007

Governor Blagojevich signs the new antipredatory bill protecting homebuyers from predatory lending. The project is scheduled to begin July 1, 2008 (Chicago Tribune, November 3, 2007).

References

Abadie, A., Gardeazabal, J., 2003. The economic costs of conflict: a case study of the Basque country. American Economic Review 93 (1), 113–132.

Agarwal, S., Evanoff, D., 2013. Do lenders steer borrowers to high risk mortgage products. Unpublished working paper. National University of Singapore, Singapore.

Agarwal, S., Amromin, G., Ben-David, I., Chomsisengphet, S., Evanoff, D., 2010. Learning to cope: voluntary financial education programs and the housing crisis. American Economic Review 100 (2), 495–500.

Agarwal, S., Amromin, G., Ben-David, I., Chomsisengphet, S., Evanoff, D., 2012. The effects of financial education on household financial decision making: evidence from a natural experiment of mortgage advice. Unpublished working paper. Ohio State University, Columbus, OH.

Agarwal, S., Ben-David, I., 2013. Do loan officers' incentives lead to lax lending standards? Unpublished working paper. Ohio State University, Columbus, OH.

Agarwal, S., Skiba, P., Tobacman, J., 2009. Payday loans and credit cards: new liquidity and credit scoring puzzles? American Economic Review 99 (2), 412–417.

Bates, L.K., Van Zandt, S., 2007. Illinois' new approach to regulating predatory lending: unintended consequences of borrower triggers and spatial targeting. Spatial Policy Analysis Research Consortium working paper 2007-02. University of Illinois, Urbana and Champaign, IL.

Ben-David, I., 2011. Financial constraints and inflated home prices during the real-estate boom. American Economic Journal: Applied Economics 102 (3), 559–578.

Ben-David, I., 2012. High leverage and high prices: evidence from the residential real-estate market. Working paper. The Ohio State University.

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should we trust difference-in-differences estimates? Quarterly Journal of Economics 119 (1), 249–275.

Bertrand, M., Morse, M., 2011. Information disclosure, cognitive biases and payday borrowing. Journal of Finance 66 (6), 1865–1893.

Bolton, P., Freixas, X., Shapiro, J., 2007. Conflicts of interest, information provision, and competition in the financial services industry. Journal of Financial Economics 85 (2), 297–330.

Bond, P., Musto, D.K., Yilmaz, B., 2009. Predatory mortgage lending. Journal of Financial Economics 94, 412–427.

Bostic, R.W., Chomsisengphet, S., Engel, K.C., McCoy, P.A., Pennington-Cross, A., Wachter, S., 2012. Mortgage product substitution and state anti-predatory lending laws: better loans and better borrowers? Atlantic Economic Journal 40 (3), 273–294.

Campbell, J.Y., Cocco, J.F., 2003. Household risk management and optimal mortgage choice. Quarterly Journal of Economics 118 (4), 1449–1494.

Campbell, J.Y., Giglio, S., Pathak, P., 2011. Forced sales and house prices. American Economic Review 101 (5), 2108–2131.

Carlin, B., 2009. Strategic price complexity in retail financial markets. Journal of Financial Economics 91 (3), 278–287.

Carlin, B., Gervais, S., 2009. Legal protection in retail financial markets. Unpublished working paper #14972. National Bureau of Economic Research, Cambridge, MA.

Center for Responsible Lending, 2009. Restoring integrity to the financial system predatory lending and the economic crisis. Faith and Credit Issue Guide (September).

Choi, H.-S., 2011. The impact of anti-predatory lending laws on mortgage volume. Unpublished working paper. Princeton University, Princeton, NJ.

Engel, K.C., McCoy, P.A., 2007. Turning a blind eye: Wall Street finance of predatory lending. Fordam Law Review 75, 101–165.

Federal Deposit Insurance Corporation (FDIC), 2006. Challenges and FDIC efforts related to predatory lending. US Government Printing Office, Washington, DC (Report by the Office of Inspector General).

Financial Crisis Inquiry Commission, 2011. The Financial Crisis Inquiry Report: Final Report of the National Commission on the Causes of the Financial and Economic Crisis in the United States. US Government Printing Office, Washington, DC.

Guiso, L., Sapienza, P., Zingales, L., 2013. The determinants of attitudes towards strategic default on mortgages. Journal of Finance 68 (4), 1473–1515.

Housing Action Illinois, 2007. Findings from the HB4050 Predatory Lending Database Pilot Program. Housing Action Illinois, Chicago, IL.

Hudson, M.W., 2010. The Monster: How a Gang of Predatory Lenders and Wall Street Bankers Fleeced America—and Spawned a Global Crisis. Macmillan, New York, NY.

Jiang, W., Nelson, A., Vytlacil., E., 2014. Liars loan? Effects of origination channel and information falsification on mortgage delinquency. Review of Economics and Statistics 96 (1), 1–18, http://dx.doi.org/10.1162/REST_a_00387.

Keys, B.J., Mukherjee, T., Seru, A., Vig, V., 2010. Did securitization lead to lax screening? Evidence from subprime loans. Quarterly Journal of Economics 125 (1), 307–362.

Litan, R.E., 2001. A Prudent Approach to Preventing "Predatory" Lending. American Bankers Association, Washington, DC.

Melzer, B., 2011. The real costs of credit access: evidence from the payday lending market. Quarterly Journal of Economics 126 (1), 517–555.

Morse, A., 2011. Payday lenders: heroes or villains? Journal of Financial Economics 102 (1), 28–44.

Piskorski, T., Seru, A., Witkin, J., 2013. Asset quality misrepresentation by financial intermediaries: evidence from RMBS market. Unpublished working paper #18843. National Bureau of Economic Research, Cambridge, MA.

Rajan, U., Seru, A., Vig, V. The failure of models that predict failure: distance, incentives, and defaults. Journal of Financial Economics, forthcoming.

US Government Accountability Office, 2004. Consumer Protection: Federal and State Agencies Face Challenges in Combating Predatory Lending. US Government Printing Office, Washington, DC (Report 04-280).

Wall Street Journal Online, 2007. Illinois Tries New Tack Against Predatory Loans. Amy Merrick. August 21. ⟨http://online.wsj.com/news/articles/SB118765937527803664⟩.

Yandle, B., 1983. Bootleggers and Baptists: the education of a regulatory economist. Regulation 7 (3), 12–16.

Yandle, B., 1999. Bootleggers and Baptists in retrospect. Regulation 22 (3), 5–7.