Does Mandatory Loan Review Affect Mortgage Contract Choice and Performance?

Sumit Agarwal[#]
Gene Amromin[#]
Itzhak Ben-David[*]
Souphala Chomsisengphet[§]
Douglas D. Evanoff[#]

April 2011

ABSTRACT

We explore the effects of mandatory third-party review of mortgage contracts on the terms, availability, and performance of mortgage credit. Our study is based on a legislative pilot carried out by the State of Illinois in a selected set of zip codes in 2006. Mortgage applicants with low FICO scores were required to attend loan reviews by financial counselors. Applicants with higher FICO scores had to attend counseling only if they chose "risky mortgages." We find that low-FICO applicants for whom counselor review was mandatory did not materially change their contract choice. Conversely, applicants who could avoid counseling by choosing less risky mortgages did so. Although ex post default rates among low-FICO borrowers in the pilot program declined by 25%, we find that the educational component of counselor review played only a minor role. Instead, external review presented strong incentives for lenders to impose tighter ex ante screening on low-credit-quality borrowers.

Keywords*:* Financial counseling, screening, subprime crisis, household finance
JEL Classification: D14, D18, L85, R21

We thank Edward Zhong and Robert McMenamin for outstanding research assistance and Dan Aaronson, Henrik Cronqvist, Tom Davidoff, Rüdi Fahlenbrach, David Laibson, Annamaria Lusardi, Olivia Mitchell, Adair Morse, Anthony Murphy, Jonah Rockoff, Amit Seru (WFA discussant), Sophie Shive, Peter Tufano, Annette Vissing-Jorgensen, Paul Willen, Luigi Zingales (NBER discussant) and seminar participants at the 2009 NBER Summer Institute, 2009 CEPR/Gerzensee Summer Symposium, 2009 AEA meetings, Bocconi University, Columbia University, the Federal Deposit Insurance Corporation (FDIC), Federal Reserve Banks of Atlanta, Chicago and Cleveland, Tel-Aviv University, Office of the Comptroller of the Currency, Ohio State University, University of California Berkeley, University of California San Diego, University of Illinois at Chicago, University of Illinois at Urbana-Champaign, Vanderbilt University, University of Minnesota, Penn State University, Financial Intermediation Research Society Conference, SUERF/Bank of Finland Conference, and the 16th Mitsui Financial Symposium at the University of Michigan for their comments. The authors thank the FDIC, Paolo Baffi Centre at Bocconi University, and the Dice Center at the Fisher College of Business, Ohio State University for supporting this research. The views in this paper are those of the authors and may not reflect those of the Federal Reserve System, the Federal Reserve Bank of Chicago, or of the Office of the Comptroller of the Currency.

Contact author: Itzhak Ben-David, Fisher College of Business, Ohio State University, bendavid@fisher.osu.edu

[#] Federal Reserve Bank of Chicago
[*] Fisher College of Business, The Ohio State University
[§] Office of the Comptroller of the Currency

1. Introduction

In the wake of the subprime mortgage crisis, policymakers have been urged to increase their intervention in credit markets—see, for example, Sheila Bair's (2007) testimony to the House Financial Services Committee. In particular, the leading policy initiatives include tightening oversight of lenders (Federal Truth in Lending Act, Regulation Z) and providing mandatory financial counseling to certain borrowers (President Obama's Homeownership Affordability and Stability Plan of 2009). Although it has been shown that these programs may disrupt market activity (Bates and Van Zandt, 2007), their effects on mortgage choice and ex post performance, as well as their overall effectiveness, are still debated.

In this paper, we study the effects of a legislative mandate for third-party review of mortgage contracts implemented in a pilot program in Cook County, Illinois. The program required "low-credit-quality" applicants and applicants for "risky" mortgages to submit their loan offers from state-licensed lenders for review by financial counselors. Similar to most regulatory initiatives,the mandate created numerous conflicting incentives for all parties: lenders, borrowers, and external reviewers. However, the empirical setting of this legislative experiment offers a number of unique opportunities to disentangle those effects. As described in greater detail below, the fact that the pilot applied only in certain areas at certain times, only to certain borrower and mortgage contract combinations, and only to a specific set of lenders allows us to parse out the effects of incentives and education on mortgage choice and to study the determinants of ex post 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.

The pilot program that we analyze was initiated in September 2006, and was restricted to mortgages originated by state-licensed lenders and secured by properties in ten contiguous zip codes on the South Side of Chicago. The program required mortgage applicants with FICO scores below 621 to attend a loan review session with a counselor certified by the Department of Housing and Urban Development (HUD) irrespective of their mortgage choice. Applicants with higher FICO scores had to attend a loan review session *only* if they chose "risky mortgages" (as

defined by the statute). The mandate required lenders to pay the $300 counseling fee. Following loan review, a counselor provided non-binding recommendations regarding the affordability of the loan, its riskiness, and its pricing. Both lenders and counselors had to record loan details and recommendations in a state-administered database. The counseling requirement was tightly monitored through the Cook County Recorder of Deeds: mortgages in the pilot area could not be registered unless borrowers provided a certificate of counseling or exemption. The pilot program was originally intended to last for four years, but bowing to pressure from community groups and mortgage brokers, it was discontinued in January 2007. Overall, over 1,200 mortgage applicants were counseled within a 20-week period.

Our empirical strategy is based on classic difference-in-differences analysis that contrasts changes in choices and outcomes in the treated sample with those in a control sample. Unlike bacteria in a Petri dish, lenders and borrowers could respond to the mandate treatment either by leaving the pilot area or by adapting to new game rules. Hence, we pay particular attention to endogenous selection of agents out of treatment, conditioning the analysis on lenders that remained active in the pilot area. We also force the control sample to reflect mandate-induced changes in the treated sample.

The heterogeneity in mandate applicability *within* pilot zip codes allows us to sharpen the identification of treatment effects further. In particular, we highlight the differences in treatment effects on borrowers who could not avoid counseling and those who could. Finally, we exploit differences in incentives of state-regulated and exempt lenders to actively steer borrowers from counselor review. This allows us to differentiate between borrowers acting on acquired information and lenders responding to the threat of external review.

Turning to the results, we document that the legislation resulted in substantial reduction in lending activity—the number of loan applications among state-licensed lenders in the pilot area declined by 73% and two-thirds of such lenders stopped originating loans altogether. We find no evidence that low-credit-quality borrowers previously served by such lenders were able to switch to exempt lenders.

The remaining lenders continued offering "risky mortgages" to prospective clients. However, we show that borrowers' choices changed substantially, as those who could avoid counseling by selecting less risky products did so. Put differently, designation of certain contracts as triggers for counseling concentrated such contracts in populations *exempt* from the counseling requirement. Designation of a mortgage type as "risky" arguably signaled their undesirability to borrowers throughout the county. Yet, we find that avoidance of risky contracts was concentrated in treated zip codes in which their choice was linked to tangible costs.

We further find that the legislation resulted in substantially lower ex post default rates among treated borrowers. These results hold after controlling for improvements in the credit quality of the borrower pool and for changes in the set of available lenders. Specifically, the 18-month default rate *among counseled low-FICO borrowers* was about 4.5 percentage points lower than that among similar borrowers in the control group (compared with the baseline pre-treatment rate of 17 percent). These borrowers could not eschew counseling by modifying their product choice. The finding of better loan performance among treated borrowers raises the obvious questions about the underlying mechanism.

Financial counseling mandates are often thought to work by providing better information to financially unsophisticated households. However, such mandates often have another important aspect in that they subject financial intermediaries to a certain degree of oversight by an outside party. In the case studied here, the legislation interjected counselors into the loan application process. This provided an incentive for lenders to screen out lower-quality borrowers to protect themselves from possible legal and regulatory action. On balance, we find more evidence that the reduction in default rates was a result of the oversight threat and the cost of compliance than information delivered to households in the counseling session.

In particular, we detect almost no effect of the counseling sessions themselves on interest rates, leverage, and the likelihood of obtaining "risky mortgage" products. Despite the common recommendation of counselors to search for alternative mortgage proposals, counseled borrowers rejected mortgage offers at a lower rate, possibly to avoid additional counseling sessions.

In contrast, we find stronger evidence for non-informational or incentive effects of the mandate. First, we document a spike in application rejection rates by lenders subject to the law, with rates returning to their normal level as soon as the law is rescinded. This pattern is due both to the exit of loosely screening lenders and further tightening of underwriting standards by the remaining lenders. Moreover, the lenders' rejection of poor-quality borrowers appears to have been based on unobservable information that could be elicited in counseling sessions. Second, we find a sizable decline in the prevalence of low-documentation mortgages. We attribute this change to counselors' demand that borrowers bring their income documentation to the session.

These results are consistent with the hypothesis that counselors' review transforms some of the soft information in the lending process into hard information. In the absence of external review, loan originators pass on only hard information to investors in securitized products. Thus, originators can shirk on screening borrowers on unobservable dimensions (Keys, Mukherjee, Seru, and Vig 2010). However, under the mandate, lenders may have been more cautious ex ante knowing that counselors may verify application details and borrower income. Hence, lenders may have chosen to ramp up their rejection rates and originate fewer low-doc loans. We argue that many of the rejected applicants would have defaulted had their mortgages been originated.

Overall, our results suggest that borrowers view counseling as a burden: applicants altered their mortgage choice to avoid counseling, shopping for additional quotes declined, and the pilot itself was halted due to pressure from community interest groups. Ironically, the legislation does not appear to have modified mortgage choice for those borrowers who attended the sessions. Further, it appears that the reduction in default rates resulted from the ex ante rejection of low-quality borrowers by the lenders.

Our paper contributes to two strands of research on the effect of mortgage choice on housing market outcomes. The first focuses on regulatory oversight and corresponding changes in incentives for various market participants. For instance, Keys et al. (2010) show that the securitization process resulted in lax screening by mortgage lenders. Ben-David (2011) finds that intermediaries expand the mortgage market by helping otherwise ineligible borrowers to

misrepresent asset valuations to obtain larger loans. Rajan, Seru, and Vig (2008) 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.

The second strand stresses the role of financial counseling and, more broadly, financial education in enabling more informed choices by households. Households may borrow too much at a high rate without realizing future consequences (Agarwal et al., 2007) or may have a hard time recalling the terms of their mortgage contracts (Bucks and Pence, 2008). Moore (2003) and Lusardi and Tufano (2009) find that respondents with poor financial literacy are more likely to have costly mortgages. Although there is a shared sense that household financial literacy is inadequate, and the resulting mistakes are consequential, there is less agreement on whether long-term financial education, shorter-term counseling, or regulation that seeks to correct behavioral biases are an effective means of addressing this shortcoming.[1] Collins and O'Rourke (2009) review the recent literature on this subject.

The rest of the paper proceeds as follows. In Section 2, we describe the counseling mandate in detail. Section 3 summarizes the data and outlines our methodology. Section 4 details the mandate effects on mortgage market activity. Sections 5 and 6 focus on specific channels for borrower and lender responses. The final section discusses policy implications of our findings.

2. Illinois Predatory Lending Database Pilot Program (HB 4050)

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 anti-predatory provisions in place, they were based on loan characteristics, in line with prevailing practices elsewhere in the country. Some political leaders in Illinois became concerned about the ease with which trigger criteria for anti-predatory

[1] For instance, Bernheim, Garrett, and Maki (2001) and Cole and Shastry (2008) study effects of high school financial education programs and reach opposite conclusions. Barr, Mullainathan, and Shafir (2008) argue that information disclosure and product restrictions are insufficient to prevent bad mortgage choices and provide an extensive outline of designing mortgage regulation to correct known behavioral biases. Agarwal et al (2010) evaluate the effects of a long-term voluntary financial education program aimed at prospective homebuyers.

programs could be avoided by creative loan packaging. 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).[2] Consequently, the legislature sought to shift focus from policing loan issuers to educating the borrowers.

To that effect, the legislation sponsored by the Illinois House Speaker Michael Madigan mandated counselor review of mortgage offers for "high-risk borrowers," 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 be subject to counseling *if* they chose certain 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-doc loans), and repeat refinancings within the past 12 months. 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 percent. The proposal was modeled on a Federal Housing Administration (FHA) program from the 1970's (Merrick, 2007), and it generated a lot of excitement among Illinois lawmakers.

The program was meant to run as a four-year pilot in select parts of Cook County that covers the metropolitan Chicago area, after which its coverage could be expanded. In spite of vocal opposition from community-based groups and affected lenders, Illinois politicians clamored to have their districts included in the pilot (Merrick, 2007). This choice looked particularly ironic in retrospect, given the eventual response of the population in the pilot areas. In the end, the bill (titled HB 4050) was passed on the last day of the 2005 legislative session.

HB 4050 mandated that each of the high-risk borrowers attend a counseling session with one of the HUD-certified counseling agencies. The determination of the need for such a session was made on the day of the application, and the borrower had 10 days to contact the agency to

[2] For a detailed analysis of the impact of the state anti-predatory lending laws on the type of mortgage products used in the market, see Bostic, Chomsisengphet, Engel, McCoy, Pennington-Cross, and Wachter (2008).

schedule it. 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 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); rather, they were to warn them against common pitfalls. The counselor was also expected to verify the loan application information about the *borrower* (e.g., income and expenses). 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 and/or could afford the loan, etc.

Both the interview and the independent collection of data on borrower income and expenses allowed counselors to form an assessment of a borrower's creditworthiness that potentially went beyond what was conveyed by the lender. Effectively, the counselors were able to elicit private information that was not necessarily used by lenders to make approval and/or pricing decisions and furnish it to state regulators. This may well have induced the lenders to screen better prior to referring approved applications to counseling for the fear of a regulatory response (e.g., license revocation) or legal response (e.g., class action lawsuits). It should be noted that none of the recommendations were binding in the sense that borrowers could *always* choose to proceed with the loan offer at hand.

A report by the non-profit Housing Action Illinois (2007) summarized the counselors' assessment of HB 4050. Over the course of the pilot, about 1,200 borrowers had their loan offers reviewed by 41 HUD-certified counselors. In 9% of the cases, 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 documents 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.

HB 4050 stipulated that the $300 cost of the session be borne by the mortgage lender, and not the borrower.[3] However, even if this were to be the case, HB 4050 imposed other time and psychic costs on borrowers. Finally, by lengthening the expected amount of time until closing, HB 4050 could force borrowers to pay for longer credit lock periods, raising loan costs.

HB 4050 imposed a substantial compliance burden on lenders as well. In addition to the cost of counseling (assuming it was not recovered through other loan charges), lenders had to make sure that the certification requirements of HB 4050 were implemented fully.[4] Otherwise, lenders could potentially lose the right to foreclose on the property. Finally, lenders reportedly feared losing some of their ability to steer borrowers toward high margin products.

As mentioned earlier, only loans offered by state-licensed mortgage lenders were subject to this requirement, as the state lacks legal authority to regulate any federally chartered institutions and generally exempts such institutions and state-chartered banks from mortgage licensing. However, much of the lending in disadvantaged neighborhoods has been done through state-licensed mortgage bankers that presented themselves as a local and nimble alternative to the more traditional bank lenders.[5] Consequently, the legislation was likely to increase the regulatory burden on the very entities providing credit in the selected pilot areas. The possibility that this could result in credit rationing prompted many observers to voice concern about the potential effect of HB 4050 on housing values in the selected zip codes.

The geographic focus of the legislation differed substantially from typical regulatory approaches that required counseling for certain loan types (Bates and Van Zandt, 2007). This feature of the legislation generated considerable opposition from community activists and

[3] There is substantial anecdotal evidence that brokers attempted to pass the $300 counseling fee to the borrowers in the form of higher closing costs or administrative charges (Bates and Van Zandt, 2007, and personal communication with a number of mortgage counselors.)

[4] Under HB 4050, title companies did not receive a "safe harbor" provision for "good faith compliance with the law." As a result, any clerical errors at any point in the loan application process could potentially invalidate the title, resulting in loss of the lender's right to foreclose on a nonperforming loan. According to the Cook County Recorder of Deeds, even federally regulated lenders had to procure a certificate of *exemption* from HB 4050 to obtain a clean title. Consequently, *all* lenders were affected to at least some degree by the legislation.

[5] Using the Home Mortgage Disclosure Act (HMDA) data described in detail in Section 3, we estimate that state-licensed mortgage bankers accounted for 64% of mortgage loans originations in the HB 4050 zip codes during 2005.

residents and prompted several lawsuits. Since the selected pilot areas were overwhelmingly (82%) populated by Hispanic and African-American residents, the selection prompted heated accusations of discriminatory intent on the part of lawmakers. As mortgage bankers threatened to withdraw from the pilot zip codes en masse, and as the tide of concerns about subprime mortgages began to rise, the opposition to HB 4050 reached fever pitch.[6] The pilot program was suspended indefinitely in January 17, 2007, after only 20 weeks of operation.

2.2. How Was the Pilot Program Area Selected?

HB 4050 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 Figure 1).[7] 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. The mortgage data come from the First American CoreLogic LoanPerformance data set on securitized non-prime mortgages (henceforth, the LP data described in greater detail below). As can be seen in Panel B of the table, IDFPR's decision at the time was based on the fact that these zip codes had substantially higher delinquency and default rates (Column (1)) compared to the rest of the city (Column (3)). The pilot zip codes are also predominantly minority-populated and have much higher rates of unemployment and poverty (Panel A). A simple comparison of population counts and the total number of loans in the LP data (Panel A) and FICO scores (Panel B) strongly suggests that the HB 4050 area had a disproportional share of subprime and Alt-A mortgages.

[6] The record of a public hearing held on November 27, 2006, provides a good illustration of the acrimony surrounding HB 4050 (it is available at http://www.idfpr.com/newsrls/032107HB4050PublicMeeting112706.pdf).

[7] The HB 4050 zip codes are: 60620, 60621, 60623, 60628, 60629, 60632, 60636, 60638, 60643, and 60652.

3. Data and Selection of Control Groups

3.1. Data sources

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 HB 4050, we turn to HMDA as the next best source of information on loan application volume, rejection rates, etc. 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. Since the effects of the legislation were 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 HB 4050 affected the credit supply along the extensive margin, i.e., to identify lenders that left the market altogether. In addition, we use the Census and IRS data to control for zip-code-level characteristics of income and population composition.

We also use the First American CoreLogic LoanPerformance (LP) database to assess the effect of HB 4050 on composition and performance of mortgages originated in the treated zip codes. This loan-level database covered over 90% of securitized subprime mortgages as of 2006, and it includes detailed borrower and loan information, such as FICO scores, 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 required full documentation in underwriting. These and other characteristics of LP data are summarized in Table 1, Panel C. FICO scores are used by lenders to assess borrower creditworthiness and set appropriate loan terms. For the purposes of our study, FICO scores also allow us to determine which borrowers in the treated zip codes were automatically or conditionally subject to loan counseling (see the discussion in Section 2.1 for details).

Finally, we received a sample of counseling data from one of the agencies that provided counseling services during the HB 4050. This data set is part of the database constructed under the HB 4050 legislation. The data include information on original mortgage offers reviewed in 191 counseling sessions. We matched these data to the Cook County Recorder of Deeds and LP data sets to identify which mortgages were originated and on what terms. We use this data set to gauge the extent to which counseling had a direct effect on mortgage choice.

3.2. Constructing a Zip-Code-Based Control Group

As discussed in Section 2.2, the selection of treated zip codes was driven by their characteristics as well as political considerations. In fact, the chosen set of pilot zip codes is far from unique in satisfying HB 4050 selection guidelines. We use this fact in constructing our control group that is meant to resemble the HB 4050 zip codes in terms of their pre-treatment socioeconomic characteristics and housing market conditions. Such areas could plausibly be expected to experience the same changes in outcome variables as HB 4050 zip codes in the absence of intervention. To fulfill this goal, 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, as well as the 2000 Census shares of minority population, of those living below the poverty level, and the unemployment rate to identify zip codes within the City of Chicago limits that have the smallest geometric distance from the HB 4050 zip codes. The resulting 12-zip-code area has about as many residents as the treatment area and is summarized in Column (2) of Panel A of Table 1. The statistics in Panel B of Table 1 corroborate our prior 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 subprime mortgage products.[8] Judging by the spirit and the

[8] In an earlier version of the paper, we used the reverse sequence for constructing 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-HB 4050 LP data. Afterward we checked for similarity on socioeconomic characteristics of treatment and control areas. All of the results reported below are robust to the definition of the control area and are available upon request.

letter of stated legislative guidelines, these areas (shown by the striped area in Figure 1) could have plausibly been selected for HB 4050 treatment.[9]

3.3. Constructing a Synthetic Zip Code Control Sample (Matched Sample)

To further establish the empirical robustness of our analysis, we construct a synthetic HB 4050-like area in the spirit of Abadie and Gardeazabal (2003).[10] Instead of identifying a similar but untreated set of zip codes, we build up a comparison sample loan by loan, matching each based on observable loan characteristics. Specifically, for each of the loans issued in the 10-zip-code HB 4050 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 here 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 HB 4050-area loan, it is removed from the set of potential matches and the process is repeated for the next HB 4050-area loan. The resulting synthetic HB 4050-like area is made up of observations from 42 out of 43 non-HB 4050 Chicago zip codes. Not surprisingly, more than half of the observations in this synthetic area come from the 12 comparable zip codes identified above on the basis of their socioeconomic characteristics.

In the subsequent analysis we will refer to the comparable zip codes and the synthetic area counterfactuals as the control and the matched samples, respectively.

3.4. Design of Tests: Micro-Level Analysis

Our empirical analysis is designed to exploit cross-sectional and temporal variation in a difference-in-differences framework. Specifically, our tests measure the difference in response of various variables (e.g., contract choice, default status, etc.) as a function of whether the loan was originated in a zip code subject to HB 4050. Our regressions include both time controls and

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

[10] It would be ideal to look at transactions that lie on either side of the border between HB 4050 and control zip codes to tease out the effect of the counseling mandate. Unfortunately, the LP data do not contain street addresses.

cross-sectional controls, as in classic difference-in-differences analysis. For example, when we study whether borrowers who attend counseling sessions altered their mortgage choices, we compare the choice of risky products by borrowers who were forced to attend counseling to those with similar characteristics in the control areas.

Our basic regression specifications have the following form:

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

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 the value of 1 if zip code *j* is subject to mandatory counseling in month *t* and 0 otherwise; and *Time* and *Zip code dummies* capture fixed time and location effects. In all regressions, we cluster errors at the zip code level.[11] For each loan, the response is evaluated at only one point in time (e.g., interest rate at origination or default status 18 months hence). 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 ratios at origination, borrower FICO score, loan interest rate, etc.

3.5. Addressing Endogenous Selection Out of Treatment

Our empirical analysis needs to make sure that differences vis-à-vis the control group are due to treatment and not changes in group composition. This is done primarily through the choice of comparison groups and by controlling for observable borrower characteristics.

We address the problem of lender selection by estimating two sets of regressions: those run on loans originated by all lenders and those originated by lenders that remained active in the HB 4050 zip codes during treatment. We call the latter the "Active Lenders" sample.[12] It holds the population of lenders constant, allowing for the identification of treatment effects unrelated to changes in lender composition.

[11] 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.

[12] The exact definition of an active lender is provided in Section 4.2.

We deal with borrower selection in two ways. Our more direct method accounts for changes in borrower population by controlling for a number of key observable variables such as FICO scores and LTV ratios. In addition, our second control sample—the matched sample—shuts down the effects of entry and exit (at least based on observables) as it is based on a loan-by-loan matching. That is, for each mortgage originated in the treated zip codes before, during, or after the legislation period, we find a matched mortgage based on FICO score, home and mortgage size, and leverage. The advantage of this method is that it forces the control sample to mirror changes in these observables in the treatment sample.

3.6. Dealing with Non-Random Treatment Sample Choice

An additional complication lies in the quasi-experimental design of our analysis. In particular, the set of HB 4050 zip codes is patently non-random, as it concentrates on low-income neighborhoods in which foreclosure rates were high at the outset. The problem with selecting such zip codes is that there is a possibility that they have different resilience to economic shocks unrelated to treatment. For example, it is possible that mortgage defaults in low-income areas were more sensitive to the general decline in house prices following the market peak around November 2006.

We offer two solutions for the treatment zip code selection. First, we use the design of the pilot project and separate the effect of treatment across low-, mid-, and high-FICO score groups. Recall that all of the low-FICO borrowers (FICO scores < 620) were subject to counseling, while the mid-FICO (scores in the 621–650 range) and the high-FICO (scores > 650) borrowers were counseled conditional on their mortgage contract choice. This approach retains the structure of standard difference-in-differences analysis while also exploiting the within-zip-code heterogeneity in treatment.[13] We further interact time dummies with the log of the average zip

[13] The FICO-score-only partitioning of borrowers in treated zip codes has the advantage of being based on a characteristic that is exogenous to the treatment regime. As shown in Section 5, the mandate caused a sizable move away from mortgage contracts that trigger counseling for mid- and high-FICO-score borrowers. We also evaluated an alternative specification that evaluates the effects on *ex post* performance of counseled borrowers by partitioning on both FICO score and observed contract choice. The results of this approach are shown in Table 9, Panel C.

code income, as reported annually by the IRS. This allows the effects of unobservable shocks to vary with the level of economic resources available to households in a particular zip code, further alleviating some of the selection concerns.[14] The regression specification that we therefore run is:

$$
\begin{aligned}
(2)\quad Response_{ijt} = {} & \alpha + \beta_1 (Treatment_{jt} \times Low\text{-}FICO_{ijt}) + \beta_2 (Treatment_{jt} \times Mid\text{-}FICO_{ijt}) \\
& + \beta_3 (Treatment_{jt} \times High\text{-}FICO_{ijt}) + \\
& + \gamma (Time\ dummies_t) + \delta (Zip\ code\ dummies_j) + \\
& + \eta (Time\ dummies_t \times log\ IRS\ income_{jt}) + \theta\ Controls_{ijt} + \varepsilon_{ijt}.
\end{aligned}
$$

In each of these cases, we are evaluating the performance and characteristics of securitized subprime and alt-A mortgages contained in the LP data.

4. Market-Wide Effects of HB 4050

As described above, the legislation disrupted mortgage markets by changing the loan origination process for certain borrowers and products. This section evaluates its effects on loan volumes, as well as on the number and composition of borrowers and lenders in treated areas.

4.1. Impact of Legislation on the Supply and Demand for Credit

We measure mortgage market activity as the volume of loan applications captured in the HMDA database.[15] Figure 2a 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 that specialize in subprime loans and all other lenders (labeled exempt lenders in the figure).

There is a precipitous decline in loan applications among state-licensed mortgage bankers that specialize in subprime loans around the time the regulation became effective (September 1, 2006). For such lenders, the application volume dropped from nearly 4,000 in August 2006 to 2,341 in September. Although this decline may be exaggerated by the run-up of applications in

[14] For robustness, we also evaluate a specification with a full set of time and zip code interactions. In this case, identification derives strictly from within-zip-code variation across borrower categories at a point in time. The main results remain qualitatively the same with this approach.

[15] We count all HMDA records associated with owner-occupied properties that have one of the following action codes: originated, denied, approved but not taken, withdrawn, and incomplete.

anticipation of HB 4050, it is clearly not present in the control sample. Nor is it present among exempt lenders operating in HB 4050 zip codes. Following the repeal of HB 4050, activity levels in both control and treatment areas converged nearly instantaneously; then they proceeded to plummet jointly to less than one-sixth of those in the market heyday.[16]

Although not shown in Figure 2a, HMDA data also provide insight into lender specialization. While the majority of subprime lending was done by state-licensed mortgage lenders, most prime lending was done by entities exempt from state licensing, and thus from HB 4050. This specialization and the lack of any appreciable upward trend in the number of applications filed by the exempt lenders (the right-hand panel of Figure 2a) are consistent with the scenario in which low-FICO borrowers were not able to switch to exempt lenders.

Similar results are presented in regression form in Table 2. These regressions are run at the zip-code-month level. Column (1) shows a nearly 70% decline in loan application volume in treated zip codes among lenders most affected by the regulation. The declines are much smaller among other lenders, some of whom were also subject to regulation, e.g., state-licensed lenders that originated negative amortization mortgages to prime borrowers (Column (2)).

4.2. Lender Exit

Some of the dramatic drop in loan applications could be traced to the much publicized lender withdrawals. 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 geographic areas. To be counted as an active lender in a given geographic area, a HMDA reporting institution must originate an average of at least one loan per week over a given five-month period, with at least one origination in every month.[17] The results of this simple

[16] In an earlier version of the paper we examined whether house prices changed during the legislation period. Using a variety of home price measures, we did not detect any statistically significant change in prices. Price measures included logged prices, changes in market-adjusted (or unadjusted) prices since the last transaction on the same property, and transaction prices relative to the asking prices. The results are available upon request.

[17] The five-month period is chosen to match the duration of HB 4050. None of the patterns depend on the choice of the threshold level or geographic area. The "every month" condition is intended to eliminate lenders that withdraw from HB 4050 zip codes during the fall of 2006 after working off their backlog of earlier applications.

exercise are reported in Panel A of Table 3. The table shows a substantial decline in the number of lenders in treated zip codes. The magnitude of this decline is much greater and strongly statistically different from the pattern observed in the control area. The table also confirms that lender exit was disproportionately concentrated among state-licensed subprime lenders. These results suggest that the mandatory counseling requirement resulted not just in the reduction of demand for credit, but also in the abrupt exit of lenders from the affected zip codes.

It is worth noting that some of the subprime lenders that exited the pilot areas appear to have returned as soon as HB 4050 was rescinded (see Table 3, Panel A). Figure 2b illustrates the rapid run-up in loan applications filed by those lenders. As noted in footnote 4, the legislation created some legal uncertainty about enforceability of mortgage contracts in treated zip codes. This by itself may have accounted for the strong lender response along the extensive margin. It is also conceivable that exit from HB 4050 areas was a strategic response by lenders determined to emphasize the disruptive nature of this high-profile regulation.

This identification of active lenders allows us to check whether the drop off in loan applications in Table 2 is due entirely to lender exit. Column (3) of Table 2 shows that restricting the sample to lenders that remained *active* in the HB 4050 area still generates a substantial (albeit smaller) drop in volume. In other words, fewer applications were filed even by the subprime lenders that did not shut down their operations in HB 4050 zip codes.[18]

We further assess whether the lenders who stayed in the market have different characteristics than those that exited following the implementation of HB 4050. Panel B of Table 3 compares those two types of lenders, based on characteristics of their mortgage applications and originations prior to HB 4050. Two characteristics jump out. Lenders who remained in the market are much larger than those who exited. This fact is consistent with lenders' claims about high implementation and legal costs of HB 4050 (Bates and Van Zandt, 2007). These fixed costs

[18] We count 9 state-licensed subprime lenders that satisfy this definition of active in the HB 4050 zip codes. This number refers to the number of lenders funding loans and filing HMDA reports. According to the Housing Action Illinois (2007) report, these lenders were represented by more than 300 mortgage brokers. This correspondence looks less surprising given the large size of entities in the active lender subset.

are disproportionately higher for smaller lending houses. Lenders that exited also have lower rejection rates prior to the HB 4050 period, indicating less stringent screening practices.

4.3. Changes in Borrower Composition

Finally, we examine whether borrowers who were subject to counseling were more likely to be rationed from the market. In Figures 3a and 3b we compare the distribution of borrowers who originated their loans before and during the HB 4050 period across FICO ranges. There is a pronounced shift to the right in the FICO score distribution during the treatment period in the HB 4050 zip codes. The share of loans originated for borrowers with sub-620 FICO scores in treated areas shrank by 10 percentage points relative to the pre-HB 4050 period. In contrast, the FICO score distribution in the comparable (untreated) sample remains virtually unchanged.

In unreported analysis, we evaluate these changes in borrower credit quality in a regression framework, with one of the specifications limiting the sample to financial institutions that remained active in the HB 4050 zip codes during the treatment period. The restricted sample also shows a sizable improvement in borrower credit quality in HB 4050 zip codes, indicating that the change was not entirely due to the exit of lenders that catered to low-FICO borrowers.[19]

5. Mortgage Counseling and Contract Choice

The design of the HB 4050 program allows an interesting inspection of the effects of both information and incentives on borrower decisions. First, to investigate the effects of *information* provided in counseling sessions, we exploit the fact that low-FICO applicants (with FICO scores below 621) had no choice but to attend counseling. Thus, we can test the effects of mandated counseling on contract selection by comparing mortgage choices of this group under the mandate to choices made in the pre-HB 4050 period or in the control zip codes by similar groups.

Second, we explore the effects of *incentives* by examining contract choices of mid- and high-FICO applicants. These applicants could eschew counseling by avoiding mortgages that

[19] The results are available from the authors on request.

were specified as risky by the legislation. Again, we compare the change in their mortgage choices over time and relative to those of similar groups in control zip codes.

5.1. Multivariate Evidence on Borrower Contract Choice

From interviews with a number of counselors involved in HB 4050 we know that borrowers were typically warned about risks associated with hybrid ARM loans or loans carrying prepayment penalties. If the information effect is at work, we would expect counseled low-FICO borrowers to shift away from such products.

However, information pertaining to broad product choices was provided not only through counseling sessions, but also by the mere designation of certain products as risky (their selection triggered counseling). These designations were known to everyone in the state and may have constituted a credible signal to avoid such mortgage products. If this signaling effect is at work, we would expect the incidence of risky product choices to decline for all borrowers in the treated and control samples. If the signal was salient only in affected areas, we would expect the incidence of risky product choices to decline for *all* FICO groups in the treated zip codes.

That said, product choice may have been affected by the borrowers' (or lenders') desire to avoid costly counseling sessions. In this case, members of a given FICO group would avoid products that trigger counseling for *their* group. That is, we would expect fewer interest-only loans by mid-FICO households, but not high-FICO households. Similarly, we would expect both mid- and high-FICO (but not low-FICO) households to choose fewer negative amortization loans and mortgages with a prepayment penalty.

Table 4 presents the results of difference-in-differences regressions of borrower contract choice, as outlined in (2). These regressions control for the borrower's FICO score, house value, LTV ratio, property type, and refinancing status, and they include a set of zip code and month dummies, as well as time dummies interacted with the log of the average zip code income.

The first set of regressions focuses on choices that subject *only* the mid-FICO borrowers to counseling—namely, hybrid ARMs, interest-only loans, and low-doc loans. These choices are

labeled as Category I Risky Products. Similarly, Category II Risky Products denotes choices that trigger counseling for both mid- and high-FICO borrowers—loans with prepayment penalty or negative amortization. For either product category, we find little evidence consistent with the information-driven effects. In particular, we find no statistical evidence that low-FICO borrowers in treatment areas pulled back from either category of risky products from lenders that remained active during the pilot period (Columns (3)-(4) and (7)-(8)).[20] The estimates are also inconsistent with the signaling effect of risky product designation, which would be manifested in either no significant diff-in-diff? estimates or in significant differences across *all* FICO groups.

Instead, we find that changes in contract choice are closely associated with FICO-group-specific triggers for counseling sessions. In particular, mid-FICO borrowers in treated areas have a much lower propensity to choose Category I products. Looking at the constant set of lenders, we estimate a 6 to 7 percentage point lower propensity to choose interest-only or hybrid ARM loans under the HB 4050, relative to that in the control group. However high-FICO borrowers in treated areas are not any more likely to shy away from these products that do not trigger the counseling requirement for them. Although taking a Category II loan leads to counseling for all borrowers, only high-FICO borrowers in HB 4050 zip codes reduce their use of such products.

On net, the evidence on product choices is consistent with the hypothesis that the counseling requirement constituted a costly burden that was actively avoided by those borrowers who were not automatically subject to it. The law led to a change in the product mix (whether initiated by borrowers or lenders) by the threat of counseling and not by the content of that counseling.

5.2. Lender Response and Contract Menu Options

One could argue that the evidence above is consistent with lenders simply removing products that trigger counseling from their menu of choices. Since lenders tend not to specialize in mid- or high-FICO borrowers, the FICO-group-specific pattern of changes casts some doubt

[20] A contrast in HB 4050 x Low FICO coefficients between columns (1)-(2) and (3)-(4) suggests that changes in contract choice of treated low-FICO borrowers derived largely from exit of lenders that made such risky loans.

on this hypothesis. Nevertheless, we also conducted a direct test of mortgage menu options at lenders that remained active in HB 4050 areas. The results are summarized in Table 3, Panel C.

The table looks at the population of active lenders and reports the fraction that offered products deemed "risky" by the legislation. A lender is considered to have a certain product type on their menu if they originated at least 20 such loans over a given five-month period, with at least one origination in every month. The main finding is that while the share of lenders willing to offer interest-only or hybrid ARMs loans in HB 4050 areas declined a bit during the mandate period, a strong majority (89 percent) retained such products on their menus. Yet, the likelihood of originating such loans declined markedly, as shown in Table 4.[21]

However, the legislation did appear to influence the availability of one subset of mortgages— low-doc loans. Table 5 repeats the analysis of Section 5.1 for choice of low-doc loans. We find a substantially lower likelihood of low-documentation mortgages for both low- and mid-FICO borrowers. Even though low-FICO borrowers were subject to counseling regardless of contract choice, lenders appear to have lost much of the appetite for low-doc loans because applicants had to bring income and asset documentation to the meeting with the counselor.[22] The fact that this information was entered into the state-administered database potentially subjected lenders to legal risk and further lessened the appeal of low-doc underwriting. The similar effect may have been in play for the mid-FICO borrowers, although low-doc loans triggered the counseling requirement for this group.

5.3. Mortgage Terms

An additional way to evaluate the hypothesis that loan review provided actionable information to borrowers is through an analysis of the mortgage terms. According to Housing Action Illinois (2007), counselors commonly observed that applicants took on too much debt at high interest rates. One would thus expect that treated borrowers were advised to reduce their

[21] Panel C also shows that negative amortization loans (Risk Category II) were pitched primarily to prime borrowers served by lenders exempt from state licensing requirements.

[22] Comparing Columns (1)-(2) and (3)-(4) indicates that some of the decline in the take-up of low doc loans by low-FICO borrowers is due to the exit of lenders that offered many of these loans in the pre-legislation period.

leverage and negotiate better loan terms. If such information translated into greater borrower bargaining power, one would expect lower LTVs nd interest rates among counseled borrowers.

Panel A of Table 6 presents evidence of changes in some of the key contract terms of loans originated during the treatment period. For each dependent variable, we estimate the difference-in-differences specification (2) for the samples described earlier. We find a marginally significant decrease in the LTV ratio for the low-FICO borrowers (Columns (1)-(2)).[23] These relative improvements translate to a decrease in debt levels of about $1,500 for an average borrower. Turning to interest rate spreads, we find no identifiable effects of HB 4050 when the sample is restricted to lenders that remained active during the treatment period (Columns (5)-(6)).[24] For the broader sample (Columns (3)-(4)), it is the mid- and high-FICO groups that show statistically significant, if small, improvements in spreads. This suggests that lenders that exited HB 4050 areas were charging higher interest spreads than those that remained active.

Panel B of Table 6 explores measures of loan affordability by looking at the debt-service-to-income ratio that captures borrowers' ability to service their loan obligation (Columns (1)–(2)) and the ratio of the annual mortgage payment to the original loan size (Columns (3)–(6)). For either of these measures we fail to detect any effect of the treatment on the low-FICO-score population. Somewhat surprisingly, we find slightly higher mortgage payments for mid- and high-FICO-score borrowers in the HB 4050 areas. However, the magnitude of the estimated effect is very small, never exceeding 20 basis points.

5.4. Direct Evidence of Counseling Advice and Actions

In a further attempt to evaluate the potential informational effect of counselor loan review we turn to an analysis of the actual counselor recommendations. At this point in the origination process, session avoidance through contract choice or market exit is irrelevant. Instead, we can evaluate the correspondence between counselor suggestions and subsequent borrower actions. To

[23] Note that for LTV and Debt-Service-to-Income (DTI) regressions we do not present matched sample results since LTV and DTI were used in matching.

[24] For ARMs, the LP data provide the relevant data item. For fixed-rate mortgages, loan spread is calculated as the difference between the contract interest rate and the matching-maturity Treasury.

do so, we obtained detailed counseling session data from one of the agencies providing services under HB 4050. For each of the 191 sessions we compared the original loan offer terms (as recorded by the agency) to mortgage details in the LP data set.[25] The top panel of Table 7 presents a breakdown of these mortgage offers organized by counselor recommendation.

About 19% of the initial mortgage offers were abandoned by the borrowers, with the rejection rates substantially higher among borrowers who were told that their loans were either "unaffordable" or "fraudulent." The majority of the reviewed offers that proceeded to closing (101 out of 155) received a "no issues" entry, indicating that the counselor had no concerns about the loan's affordability, the borrower's understanding of the terms, or the original offer's disclosures. Yet, about half of these "no issues" loans were modified after counseling, with slightly over 40% of renegotiated loans resulting in lower monthly payments. Loans deemed "unaffordable" or "fraudulent" were renegotiated at a similar rate, but a substantially higher fraction of such renegotiations yielded lower monthly payments.

Probit regressions in Panel B of Table 7 confirm that counselor recommendations were systematically related to loan and borrower characteristics. For example, we found the debt-service-to-income ratio (DTI) and initial interest rate to be strongly associated with a greater likelihood of the "unaffordable" classification. Similarly, a "no issues" assessment was much less likely for loans that fall into the risky mortgage category or borrowers with high DTI ratios.

Looking more closely into the specifics of renegotiated problem loans highlights some of the complexities in establishing a direct mapping between counseling recommendations and the eventual loan choice. Some contract changes appear incongruous with the recommendation. For example, some unaffordable loans were renegotiated to loans with shorter amortization periods or longer resets. This may have made such choices less risky, but also less affordable at the time of origination. Although counselors commonly recommended fixed-rate mortgages as the best

[25] To match counseling records with those in the LP database, we first use the property address and counseling date to obtain the amount of originated loan in the Illinois Recorder of Deeds database. If there is no record of a mortgage transaction in the month following the counseling session, the loan offer is considered to have been abandoned. For matched properties we use the Deeds data set values on loan amount and loan recording dates, and the agency's data on the counseling date and applicant's FICO score to find a matching loan in the LP data.

means to lessen the risk of mortgage obligations, few borrowers switched away from their original ARM offers. In fact, as many borrowers went from fixed rate mortgages to ARMs as the other way around. Among those renegotiating their ARM deals, extending reset periods (by switching from, say, 2/28 to 3/27 loans) was also nearly as common as shortening the terms. Thus, it may not be surprising that, on average, counseling did not appear to significantly change the debt burden and interest costs of originated mortgages substantially.

An open question then is whether the evidence in this small sample of treated borrowers is consistent with direct information effects of counseling. On the one hand, we find higher rejection rates of fraudulent loans and a high prevalence of lower payments for renegotiated unaffordable loans. On the other hand, about half of all problematic loans that went to origination did so without *any* changes. Moreover, if we assume that recorded recommendations reflect relevant information provided by counselors, the fact that many loan changes do not seem to line up with such recommendations weakens the hypothesis of direct information effects.

5.5. Borrower Rejection of Loan Offers

HB 4050 required additional sessions for each mortgage offer from a new lender or a renegotiated offer from the original lender that worsened the initial terms. Hence, if counseling is regarded as a burden instead of a source of valuable information, we would anticipate fewer rejections of loan offers by counseled borrowers. Conversely, if counseling is informative, we would expect to see a spike in rejections by better informed borrowers if they cannot favorably renegotiate their loan terms.

Table 8 presents a test of these hypotheses, using aggregate HMDA applications data. The regressions are run at the loan level, with borrower rejection of a loan offer as the dependent variable. The table shows that in the sample of active lenders, rates of mortgage rejection by borrowers *did not increase* during the HB 4050 period either among prime or subprime lenders. In fact, borrower rejection rates actually declined during the HB 4050 period by about 5 percentage points in the full sample of subprime lenders (Columns (1)-(2)), again suggesting a difference between lenders that exited the market and those that remained active.

This finding is rather remarkable because the majority of the counseled were advised that they could not afford the loan and/or that they should seek alternative mortgage offers. Since we find little evidence of significant improvement in loan terms post counseling (e.g., loan spread), a likely explanation for the lack of change in the rejection rate is that borrowers preferred to accept the offer at hand and not to return for further counseling with offers from alternative lenders.[26]

In the process of collecting data on the actual counseling recommendations, we noticed that many sessions, especially those involving cash-out refinance loans, took place only a few days prior to scheduled closings. In such cases, rejecting an offer would mean a significant delay in obtaining funds that may have been critical in satisfying a borrower's other obligations. For such borrowers, the attendant cost likely far outweighed the expected benefits of new offers.

In sum, the analysis in Section 5 contains only marginal evidence for the beneficial effects of information obtained in counseling sessions. Although debt burdens improve somewhat for counseled borrowers, the economic magnitude of these effects is fairly small. Flat loan rejection rates, the inability of counseled borrowers to negotiate lower loan spreads, and the short time span between the loan review sessions and the scheduled closings all suggest limited bargaining power on the part of borrowers. In contrast, the pattern of changes in product choices is broadly consistent with borrowers' (and lenders') desire to avoid oversight whenever possible.

6. The Effects of the Counseling Mandate on Mortgage Performance

One of the primary goals of HB 4050 was to reduce the rate at which borrowers defaulted and had their properties foreclosed on. This section evaluates differences in ex post default rates of treated borrowers and investigates various hypotheses for the documented change.

6.1. Default Rates

We measure loan performance by flagging borrowers who default on their loans within 18 months of origination. We then estimate a series of ordinary least squares (OLS) regressions

[26] This result also allays concerns that counselors' incentives led them to convince borrowers to reject loans, ultimately leading to low origination volumes.

as defined in (2), where the set of controls includes measures of borrower characteristics (FICO score and flags for being an investor or second-home owner), contract terms (LTV ratio, loan spread, and logged property valuation), contract type (low-documentation, negative amortization, interest-only, prepayment penalty, or refinancing loans), and property characteristics (indicators of whether a property is a single-family home, condo, or townhouse). [27]

The results of difference-in-differences tests are reported in Table 9, Panel A. Columns (1)-(4) display the results of specification (2) that differentiates between borrowers on the basis of their FICO scores. As discussed in Section 3.5, each difference-in-differences specification is estimated for four samples: the control sample, the matched sample, and the control sample and the matched sample restricted to lenders that remained active during the HB 4050 period.

The results in Columns (1)-(4) suggest that treatment had a strong effect on low-FICO borrowers, each of whom had to attend a counseling session. For such borrowers, the ex post default rates are substantially lower than those among similar borrowers in the control group. The difference ranges from 4.1 to 5.4 percentage points across the four samples, but is uniformly economically and statistically significant. In contrast, there is no statistically measurable effect of HB 4050 for borrowers with high- or mid-FICO scores. The results are qualitatively the same if contract type controls (which determine counseling requirements for mid- and high-FICO borrowers) are added as regressors—Columns (5)-(8).

These specifications allow us to account for the possibility that the superior performance of counseled borrowers is due to factors other than counseling, such as changes in the composition of borrowers or of lenders. For instance, limiting the sample to lenders that remained active during the HB 4050 period (Columns (3)-(4)) tests whether lower post-treatment defaults are due to the exit of predatory lenders that previously accepted unqualified borrowers. The results indicate that our conclusions remain fully robust to this restriction. Even among loans made by this static group of lenders, there is a marked improvement in ex post defaults for HB 4050 period originations by low-FICO borrowers relative to those in either control group.

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

Another potential interpretation of the results is that risky borrowers self-selected out of the market or were rejected by lenders (as shown in Figures 3a and 3b). However, all of Table 9 specifications control for borrower credit scores, implying that the improvement in performance is not due solely to higher FICO scores of the remaining borrowers. They also include a control for the loan spread paid by borrowers as an additional measure of borrower riskiness not captured by the credit score. The validity of these variables as risk measures is corroborated by the consistent association of lower FICO scores and higher loan spreads with higher defaults.

As a test of our identification strategy, we estimate a specification with a full set of interactions between zip code and time dummies. This setting allows us to identify the effects of HB 4050 by exploiting within-zip-code heterogeneity in applicability of the counseling requirement. This specification represents a triple difference-in-differences estimator, with the additional set of differences taken with respect to the performance of the omitted (high-FICO) group. The results shown in Columns (1)-(4) of Table 9, Panel B, once again indicate a statistically and economically significant effect of HB 4050. Finally, to test the importance of the functional form assumptions, we rerun the regressions in a probit framework, despite the difficulty in interpreting coefficient estimates on interacted terms (Ai and Norton, 2003). The estimates in Columns (5)-(8) suggest a treatment effect of 3 to 4 percentage points on defaults.

Finally, we split the borrower population according to whether they actually received counseling, as determined both by their FICO score and mortgage choice. The results are presented in Table 9, Panel C. The panel shows that, as a group, counseled borrowers did not have significantly lower default rate (Columns (1)-(4)). However, the population subject to mandatory counseling—low-FICO borrowers—exhibits a materially lower default rate.

In sum, we find that the loan review requirement improved ex post default rates for the counseled low-FICO borrowers relative to similar borrowers outside the treatment area. The effect on default is impressive in its economic magnitude and does not seem to be driven solely by documented changes in the borrower and lender pools.

6.2. What Caused the Decline in Default Rates?

The finding of better loan performance among treated borrowers brings up questions about the underlying mechanism. Regressions in the previous section control for mortgage terms, as well as for changes in composition of borrowers and lenders. They also take into account changes in mortgage contract choice, as described in Section 5. Altogether, the results suggest that legislation-induced changes in observable borrower, contract, and lender characteristics documented above cannot account fully for improved mortgage performance.

One of the unexplored alternatives is that the legislation altered the information structure in the market. Specifically, we conjecture that counselor review transformed some of the previously unreported soft information into hard (documented) information. For instance, a mortgage broker may not have previously had an incentive to record a borrower's family composition or income prospects. With counselors collecting this information and sending it to the state regulator, however, a broker has an incentive to screen out lower-quality borrowers to protect themselves from possible legal and regulatory action. These pre-emptive lender actions could result in improved ex post performance by the counseled borrowers after controlling for observable borrower quality characteristics. We test this hypothesis in the subsection below.

6.3. Rejection by Lenders

If lenders allow fewer doubtful cases to enter the counseling process for fear of possible legal or regulatory action we would expect a temporary jump in rejection rates among state-regulated lenders during the HB 4050 period.[28] The simple time series in Figure 4 indeed show a dramatic spike in the rejection rates of state-licensed subprime mortgage bankers in the treated area. After oscillating around 30 percent prior to HB 4050, their rejection rates jump above 45 percent during the first three months of the mandate. This does not occur among similar lenders in the control areas or among lenders exempt from the law (the right panel). Rather, we conjecture that this spike comes from two sources: the exit of loosely screening lenders and the further tightening of underwriting standards by lenders that remain active during HB 4050.

[28] A cutback in offering low-documentation loans, as shown in Section 5.2, has a similar flavor.

These sources are captured by the regression results in Table 10, Panel A. Columns (3) and (4) show rejection rates rising by 3.4 to 3.9 percentage points among active subprime lenders, suggesting further tightening of lending standards during HB 4050. When we do not restrict the regression sample to active lenders (Columns (1) and (2)), the spike in rejection rates is much greater, suggesting the exit of lax lenders (and consistent with results in Table 3, Panel B). Note that the overall rejection rate comes down when HB 4050 is rescinded, as lenders with looser screening practices return to the market (Figure 2b) and active lenders are free from counselor oversight.[29]

The HMDA data records lender-reported reasons for loan rejections. For each potential rejection reason, we run a probit regression on the HB 4050 indicator, logged mortgage size and income on the sample of lenders that remained active in HB 4050 zip codes. The sample is further split into lenders that specialize in subprime population (and are likely to send most of their borrowers to counseling) and all others. Table 10, Panel B reports the marginal effects on the HB 4050 dummy. For the population likely subject to the counseling treatment, there is a large decline in credit-related reasons for rejection. This supports our earlier observation that the credit quality of the applicant pool, based on observed characteristics, improved. Instead, many more rejections under HB 4050 were ascribed to the catchall category "Other," suggesting lender screening on unobservable (soft) information.

7. Discussion and Conclusion

Mandated financial counseling and increased oversight of lenders are important policy tools being considered in the wake of the housing market crisis.[30] 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.

[29] In unreported regressions, we interact the HB 4050 indicator with loan size to test the hypothesis that the $300 counseling fee forced lenders to disproportionately reject low-value loans. We find no supporting evidence for this.

[30] On June 17, 2009, President Obama announced a proposal to create a new Consumer Financial Protection Agency to safeguard consumers from unfair, deceptive, and abusive practices in the financial sector. The bureau will include an educational function that will consider increasing access to financial counseling.

In this paper, we evaluate the impact of one such pilot program implemented in Chicago in late 2006. The program combined lender oversight and counseling of high-risk borrowers, with both aspects of the program being administered through HUD-certified financial counseling agencies. The pilot design allows us to disentangle the effects of the informational content of counseling from those of lender oversight and compliance costs.

We present two main results. First, we find that applicants responded to incentives to attend (or, rather, avoid) counseling, but were less influenced by information provided by counselors. We report that borrowers altered their mortgage choice to minimize interaction with counselors. Specifically, those borrowers who could eschew counseling by choosing less risky products did so. Those who went through a counseling session did not appear, on average, to follow the counselor's advice, and seemed to have only limited bargaining power in renegotiations. They tended to not walk away from the original offer following counseling, nor to reapply for a different mortgage, which would have required another counseling session.[31]

Second, the legislation had material effects on the market composition of both lenders and borrowers, on borrower default rates, and on borrower and lender behavior. We find that the pilot caused low-FICO borrowers and smaller lenders to exit the market. Judging by the volume of loan applications alone, the decline in market activity was quite dramatic. Yet, controlling for observable characteristics of the remaining borrowers and holding the sample of lenders constant, we find substantial declines in default rates among counseled low-FICO borrowers. Our preferred explanation for this finding centers on stronger incentives for lenders to reject marginal borrowers under the mandate regime. To avoid public scrutiny, lenders appear to have fine-tuned their lending model and rejected applications they may have previously accepted. The forced disclosure of borrower information and its recording by an outside party thus appear to have been key reasons for the observed decline in default rates.

[31] In ongoing work, we are evaluating the effects of the follow-up program that expanded the counseling requirement on the basis of product choice to the entire Cook County. This will allow us to address the questions of external validity of the pilot study.

It may 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 counseling itself.[32] 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 guided by avoidance of counseling sessions, and others. Moreover, evaluating the overall welfare effect of this intervention requires weighing the relative costs and benefits accruing to different market participants.[33] It is further complicated by various distortions that already exist in the housing market due to unique tax treatment, zoning restrictions, etc., as well as potential externalities produced by individual housing decisions.

Our results suggest several policy recommendations. First, blunt policy interventions can lead to severe market disruptions. Additionally, details of policy design matter. For instance, offering "safe harbor" provisions would likely have mitigated lender exit, while requiring counseling sessions to take place prior to scheduling a closing would likely have improved borrower bargaining power. Third, one-off counseling sessions late in the mortgage origination process may be perceived as a burden by borrowers, who would then either stay away from the market altogether or switch to mortgages that allow them to avoid counseling. At that point, borrowers may overweight the current costs of counseling and discount future benefits stemming from more informed contract choice, à-la Laibson (1997). Fourth, gains from the informational content of counseling are tempered by the limited negotiating power of the borrowers. It is likely that even after the (admittedly brief) counseling session, mortgage applicants cannot negotiate well with mortgage brokers who steer them between products, without real improvement in borrower loan terms. Finally, the mere presence of the regulator in the marketplace and the third-party loan review seem to have a significant effect on the quality of originated mortgages.

[32] For instance, we could have taken the average house value in HB 4050 zip codes of $190,000 and assumed the deadweight loss from foreclosure of about 30% (Campbell, Giglio, and Pathak, 2009). Using the point estimate of a 5.04% improvement in default rates of the low-FICO borrowers (column 1 of Table 9), the expected benefit of counseling would be $2,850 (0.05*$190,000*30%). Since about 60% of counseled borrowers had low FICO scores, their gains would have to be offset by the $300 fee charged to all counseled borrowers.

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

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Table 1. Summary Statistics

Panel A: Construction of a Control Sample on the Basis of pre-Treatment Socioeconomic Characteristics (2005 IRS and 2000 Census data)

	HB 4050 zip codes (10 zip codes)	Control ZIP codes (12 zip codes)	all non-HB4050 Chicago zip codes (43 zip codes)
Total population	729,980	713,155	2,181,267
Total number of 2005 tax returns	259,884	244,326	888,354
Share of minority households*	0.813	0.863	0.505
Share of households below poverty level*	0.200	0.245	0.174
Average taxable income in 2005#	$31,579	$30,844	$56,976
Share of households with income < $50,000 in 2005	0.823	0.837	0.720
Unemployment rate (2000 Census)*	0.141	0.151	0.101

* population-weighted averages

weighted by number of 2005 IRS tax returns

Panel B: Pre-Treatment Mortgage and Borrower Characteristics of HB 4050 and Control Zip Codes (Loan Performance data, January 2005 - December 2005)

	HB 4050 zip codes (n=15,216)	Control ZIP codes (n=12,925)	all non-HB4050 Chicago zip codes (n=28,060)
Share defaulting within 18 months (x 100)	14.01	13.69	9.06
FICO	627.68	628.64	648.77
LTV (%)	84.14	82.92	81.85
Debt Service-to-Income (%)	39.94	40.28	40.20
log(Valuation)	12.12	12.22	12.47

Panel C: Key Variable Means in LoanPerformance Data (1/2005-12/2007)

	1/2005-8/2006			9/2006-12/2007		
	HB 4050 Zip Codes	Control Sample	Matched Sample	HB 4050 Zip Codes	Control Sample	Matched Sample
	n = 24,014	n = 20,686	n = 24,014	n = 2,802	n = 4,445	n = 2,802
Share defaulting within 18 months (x 100)	17.36	17.29	15.03	21.66	25.47	20.92
Fraction of low-FICO Borrowers	44.14	44.21	40.69	35.58	42.32	36.54
Fraction of mid-FICO Borrowers	19.93	19.57	20.79	20.91	20.13	20.74
Fraction of high-FICO Borrowers	35.93	36.22	38.52	43.50	37.55	42.73
FICO score	629.66	629.92	634.19	641.19	632.39	639.90
Share od Risky Products Category I	88.39	88.43	91.36	81.66	84.52	86.23
Share of Risky Products Category II	20.34	20.10	18.05	13.20	15.84	15.15
Share of low-documentation loans (x 100)	44.66	45.62	49.94	46.57	48.03	51.90
Loan Margin (%)	4.69	4.70	4.77	4.33	4.57	4.54
Annual Mortgage Payment/Loan Size (%)	8.55	8.49	8.34	8.66	8.58	8.42
Loan-to-Value (%)	84.20	83.01	83.91	83.32	82.67	83.66
Debt-Service-to-Income (%)	40.46	40.85	41.07	40.32	41.28	41.02
log(House Value ($))	12.15	12.23	12.33	12.29	12.37	12.32

Table 2. Effects of HB 4050 on Market Activity: Application Volumes (Source: HMDA)

The table presents zip code-month level regressions of logged application volumes on an indicator of HB 4050 regime, as well as zip code and month fixed effects. The regressions are carried out separately for applications filed with state-licensed subprime lenders subject to HB 4050 and applications filed with all other lenders. Columns (3) and (4) repeat the exercise for a subset of lenders that remained active in HB 4050 treatment areas during the time the law was in force. The exact definition of an active lender is provided in Section 4.2. Standard errors are clustered at the zip code level. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable: log(# Applications)			
	All Lenders		Active Lenders	
	State-Licensed Subprime Lenders	Other Lenders	State-Licensed Subprime Lenders	Other Lenders
	(1)	(2)	(3)	(4)
HB 4050	-0.694***	-0.246***	-0.081***	-0.019
	(0.038)	(0.045)	(0.020)	(0.025)
Month FE	Yes	Yes	Yes	Yes
Zip Code FE	Yes	Yes	Yes	Yes
Observations	936	936	936	936
Adj. R^2	0.98	0.95	0.96	0.95

Table 3. Effects of HB 4050 on Credit Supply

The table summarizes the number (Panel A) and characteristics (Panel B) of lenders in the HB 4050 and in the zip code-based control sample.

Panel A: Supply of Credit – Total number of active lenders (Source: HMDA)

	State-Licensed Lenders Specializing in Subprime loans		All Other Lenders	
	HB 4050	Control	HB 4050	Control
Before HB 4050 (9/05 - 8/06)	31	30	83	76
During HB 4050 (9/06 - 1/07)	9	23	56	65
After HB 4050 (2/07 - 6/07)	13	15	66	66

Panel B: Which State-Regulated Lenders Stayed in the Market? (Source: HMDA)

Comparison of pre-HB4050 characteristics

	Stayed in Market (n = 9)		Left Market (n = 21)*	
	Mean	Median	Mean	Median
Mortgage Amount	147.9	147.0	144.4	147.6
Income	72.8	70.8	76.4	72.4
Total Originations	459.4	169.0	231.2	126.0
Rejection Rate (%)	33.4	33.8	27.9	27.2
Share of Refinancings (%)	50.0	51.1	56.6	53.0
Share of Second Lien Loans (%)	22.4	19.8	19.8	18.9

* The summary statistics are based on 1/2006 to 8/2006. Two lenders did not lend in this period.

Panel C: Contract Choice Menu Under the Mandate (Source: LoanPerformance)

Share of lenders originating Risk Category I (IO, hybrid ARM, low doc) / Risk Category II (NegAm, prepayment penalty) loans

	State-Licensed Lenders Specializing in Subprime loans		All Other Lenders	
	HB 4050	Control	HB 4050	Control
Before HB 4050 (9/05 - 8/06)	1.00/0.18	0.93/0.14	1.00/1.00	1.00/0.67
During HB 4050 (9/06 - 1/07)	0.89/0.06	0.86/0.18	1.00/0.50	1.00/0.75
After HB 4050 (2/07 - 6/07)	0.96/0.13	0.95/0.11	1.00/0.75	1.00/0.75

* Shares are taken with respect to the total number of active lenders in Panel A present in LoanPerformance data

Table 4. Mortgage Product Choice (Source: LoanPerformance)

The table examines the effects of counseling on mortgage contract choice. The table uses OLS regression to test for changes in choice of contracts deemed risky by HB 4050. Risky Products Category I refers to mortgages that subject *only* the mid-FICO borrowers to counseling (hybrid ARMs, low-documentation and interest-only loans). Risky Products Category II denotes contracts that trigger counseling for both mid- and high-FICO borrowers (prepayment penalty and negative amortization loans). The list of controls includes measures of borrower's FICO score, house value, LTV, property type, and refinancing status, as well as a number of time and location fixed effects. All standard errors are clustered at the zip code level. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	I(Risky Products: Category I) x 100				I(Risky Products: Category II) x 100			
	Control	Matched	Control Active	Matched Active	Control	Matched	Control Active	Matched Active
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HB 4050 x Low FICO	-2.90**	-3.04**	-2.04	-1.36	-0.40	-1.91	-1.67	-2.31
	(1.36)	(1.44)	(1.59)	(1.56)	(1.79)	(1.86)	(1.79)	(1.85)
HB 4050 x Mid FICO	-5.28***	-5.07***	-6.67***	-5.80***	-0.15	-1.57	-1.04	-1.47
	(1.27)	(1.24)	(1.44)	(1.28)	(1.27)	(1.42)	(1.14)	(1.22)
HB 4050 x High FICO	0.37	0.87	-0.99	0.39	-3.95***	-5.76***	-4.37***	-5.17***
	(1.15)	(1.20)	(1.31)	(1.27)	(1.34)	(1.44)	(1.26)	(1.30)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract Terms Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property Type Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE, Zip Code FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE * log(Avg Income)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	55,600	57,619	40,041	41,891	55,600	57,619	40,041	41,891
Adj. R^2	0.20	0.19	0.20	0.19	0.03	0.03	0.05	0.06

Table 5. Availability of Low-Doc Loans (Source: LoanPerformance)

The table uses OLS framework to examine the likelihood of taking low-doc loans in HB 4050 treatment zip codes. All standard errors are clustered at the zip code level. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	I(Low Doc) x 100			
	Control	Matched	Control Active	Matched Active
	(1)	(2)	(3)	(4)
HB 4050 x Low FICO	-5.48***	-7.55***	-4.03**	-5.23**
	(1.76)	(1.99)	(1.89)	(2.04)
HB 4050 x Mid FICO	-7.26***	-8.61***	-7.17***	-8.03***
	(2.24)	(2.27)	(2.41)	(2.48)
HB 4050 x High FICO	0.72	1.98	1.12	3.26*
	(1.36)	(1.43)	(1.58)	(1.71)
Borrower Controls	Yes	Yes	Yes	Yes
Contract Terms Controls	Yes	Yes	Yes	Yes
Property Type Controls	Yes	Yes	Yes	Yes
Month FE, Zip Code FE	Yes	Yes	Yes	Yes
Month FE * log(Avg Income)	Yes	Yes	Yes	Yes
Observations	55600	57619	40041	40425
Adj. R^2	0.20	0.18	0.20	0.18

Table 6. Effects of HB 4050 on Mortgage Leverage and Spread

The table uses examines the effects of the mandate on key mortgage terms. Panel A uses OLS regression to test whether leverage and loan margin are different for the population with mandatory counseling. Panel B examines proxies for mortgage affordability. The set of controls not shown in the table includes: borrower characteristics (FICO score and FICO score ranges, investor and second mortgage flags); contract terms (LTV (only for margin and mortgage payment regressions) and log of appraised value); and property type (flags for single family residence, townhouse, or condominium). All standard errors are clustered at the zip code level. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Key Mortgage Terms (Source: LoanPerformance)

	Loan-to-Value (%)		Loan Margin (bp)			
	Control	Control Active	Control	Matched	Control Active	Matched Active
	(1)	(2)	(3)	(4)	(5)	(6)
HB 4050 x Low FICO	-0.75*	-0.81*	-7.64**	-2.49	-3.06	5.10
	(0.41)	(0.42)	(3.01)	(3.35)	(3.16)	(3.16)
HB 4050 x Mid FICO	-0.12	-0.24	-13.53***	-11.37***	-4.71	0.22
	(0.37)	(0.38)	(4.04)	(3.75)	(4.64)	(3.95)
HB 4050 x High FICO	-0.02	0.13	-16.92***	-15.55***	-5.60	-2.87
	(0.37)	(0.41)	(4.23)	(4.20)	(4.32)	(4.45)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Contract Controls	Yes	Yes	Yes	Yes	Yes	Yes
Property Type Controls	Yes	Yes	Yes	Yes	Yes	Yes
Month FE, Zip Code FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE * log(Avg Income)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	55,600	40,041	55,600	57,619	40,041	40,425
Adj. R^2	0.26	0.26	0.22	0.21	0.22	0.21

Panel B: Mortgage Affordability (Source: LoanPerformance)

	Debt-Service-to-Income (%)		Annual Mortgage Payment (%)			
	Control	Control Active	Control	Matched	Control Active	Matched Active
	(1)	(2)	(3)	(4)	(5)	(6)
HB 4050 x Low FICO	-0.49	-0.31	0.04	0.08	0.02	0.02
	(0.41)	(0.42)	(0.05)	(0.05)	(0.05)	(0.05)
HB 4050 x Mid FICO	-0.34	-0.41	0.13*	0.16**	0.13*	0.13*
	(0.71)	(0.74)	(0.06)	(0.07)	(0.07)	(0.08)
HB 4050 x High FICO	-0.05	0.07	0.07**	0.17***	0.06*	0.12***
	(0.40)	(0.38)	(0.03)	(0.03)	(0.03)	(0.03)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Contract Controls	Yes	Yes	Yes	Yes	Yes	Yes
Property Type Controls	Yes	Yes	Yes	Yes	Yes	Yes
Month FE, Zip Code FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE * log(Avg Income)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	40,024	26,604	55,600	57,619	40,041	40,425
Adj. R^2	0.07	0.08	0.22	0.24	0.22	0.23

Table 7. Effects of Counseling on Borrower Behavior

The table examines the effects of counseling on borrower propensity to renegotiate or reject loan offers. The data used is provided by a counseling agency. The top panel of the table compares pre-counseling and post-counseling mortgage characteristics. The bottom panel presents marginal effects from probit regressions of counselor recommendations on key borrower and loan characteristics.

Panel A: Summary of Recommendations

		Counselor recommendation			
Data summary	Total Sessions	No issues	Cannot afford or close to it	Indicia of fraud	Loan above market rate / Seek another bid
Number of counseling sessions	191	117	39	25	10
Loans not pursued after counseling	36	17	10	8	1
memo: abandoned loans re-originated after HB 4050	*14*	*7*	*3*	*4*	*0*
Share of loans not pursued after counseling	19%	15%	26%	32%	10%
Loans originated after counseling	155	101	28	17	9
Total matched originations	148	96	27	17	8
Comparison of loan terms before and after counseling sessions					
No changes at all	73	49	14	8	2
Loans with changes post counseling	75	47	13	9	6
(percent with changes)		49%	48%	53%	75%
Lower monthly payments		20	10	5	5
(percent of all changed loans)		43%	77%	56%	83%
Switch from ARM to fixed		8	5	4	1
(percent of all changed loans)		17%	38%	44%	17%
Switch from fixed to ARM		12	3	1	2
(percent of all changed loans)		26%	23%	11%	33%
Lower interest rate		23	11	5	5
(percent of all changed loans)		49%	85%	56%	83%

Panel B: Recommendations and Loan/Borrower Characteristics

	Counselor recommendation (0/1)	
	No issues	Unaffordable
"Risky mortgage" flag	-0.259***	0.017
	(0.082)	(0.060)
Debt-to-Income ratio (DTI)	-0.025***	0.010***
	(0.005)	(0.002)
Loan-to-Vaue ratio (LTV)	0.001	-0.002
	(0.002)	(0.001)
Initial interest rate	0.001	0.047**
	(0.030)	(0.020)
FICO score	0.000	-0.000
	(0.000)	(0.000)
log(Mortgage)	0.246*	0.021
	(0.137)	(0.071)
Observations	191	191
Pseudo R^2	0.32	0.23

Table 8. Loan Offers Rejected by Borrowers

This table explores whether borrowers in HB 4050 treatment zip codes are more likely to have rejected loan offers. All standard errors are clustered at the zip code level. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent: I(Applicant Rejects Offer) x 100							
	State-Licensed Lenders (Subprime)				All Other Lenders			
	Control	Matched	Control Active	Matched Active	Control	Matched	Control Active	Matched Active
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HB 4050	-5.38***	-4.75***	-0.85	-0.43	-1.68**	-0.98	0.17	0.04
	(0.68)	(0.73)	(0.77)	(1.00)	(0.60)	(0.65)	(0.72)	(0.69)
log(Mortgage)	0.64*	0.35	0.03	-0.28	2.54***	2.20***	2.16***	1.89***
	(0.36)	(0.38)	(0.56)	(0.52)	(0.35)	(0.29)	(0.25)	(0.24)
log(Income)	2.44***	2.79***	1.01***	0.54*	1.00***	0.64***	0.04	-0.22
	(0.29)	(0.30)	(0.22)	(0.28)	(0.17)	(0.16)	(0.19)	(0.18)
Month FE, Zip Code FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE * log(Income)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	158,307	168,789	56,900	61,929	236,007	269,138	155,002	179,842
Adj. R^2	0.02	0.01	0.04	0.04	0.01	0.01	0.01	0.01

Table 9. Effects of HB 4050 on Mortgage Performance

The table examines the effects of the mandate on loan performance. Panel A uses OLS regression to test the effect of HB 4050 on borrower default rates. Borrowers are differentiated by their FICO score group, which is a key determinant of whether they are subject to treatment. The set of controls not shown in the table includes the following variables: contract type (flags for low doc loans, negative amortization loans, interest only loans, loans with a prepayment penalty, and refinance loans); contract terms (log of appraised value, LTV ratio); borrower characteristics (FICO score range (low- and mid-), investor and second mortgage flags); and property type (flags for single family residence, townhouse, or condominium). Panel B repeats these tests with alternative specifications (probit and full set of zip code-month effects). All standard errors are clustered at the zip code level. Standard errors are clustered at the zip code level. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Default Rates (Source: LoanPerformance)

	Dependent variable: I(Default within 18 months) (x 100)							
	Regression: OLS							
	Control	Matched	Control Active	Matched Active	Control	Matched	Control Active	Matched Active
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HB 4050 x Low FICO	-5.04**	-4.72**	-5.39**	-4.11*	-4.75**	-4.34**	-5.19**	-3.85*
	(1.97)	(1.88)	(2.26)	(2.12)	(1.96)	(1.88)	(2.26)	(2.12)
HB 4050 x Mid FICO	0.12	1.23	0.43	2.34	0.50	1.67	0.84	2.81
	(2.65)	(2.66)	(2.59)	(2.50)	(2.60)	(2.61)	(2.54)	(2.44)
HB 4050 x High FICO	-1.48	-0.27	-1.25	0.77	-1.58	-0.44	-1.33	0.60
	(1.43)	(1.44)	(1.49)	(1.39)	(1.45)	(1.46)	(1.51)	(1.41)
FICO Score	-0.08***	-0.07***	-0.08***	-0.08***	-0.08***	-0.08***	-0.09***	-0.08***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
Loan Margin (%)	1.77***	1.54***	1.68***	1.46***	1.72***	1.43***	1.61***	1.35***
	(0.13)	(0.11)	(0.15)	(0.12)	(0.16)	(0.14)	(0.18)	(0.14)
Contract Type Controls					Yes	Yes	Yes	Yes
Contract Terms Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property Type Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip Code FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE * log(Avg Income)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	55,600	57,619	40,041	40,425	55,600	57,619	40,041	40,425
Adj. R^2	0.10	0.09	0.10	0.09	0.10	0.09	0.10	0.09

Table 9. Effects of HB 4050 on Mortgage Performance (continued)

Panel B: Default Rates, Robustness to Functional Form and Identification Strategy
(Source: LoanPerformance)

	Dependent variable: I(Default within 18 months) (x 100)							
	Regression: OLS				Regression: Probit			
	Control	Matched	Control Active	Matched Active	Control	Matched	Control Active	Matched Active
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HB 4050 x Low FICO	-5.20***	-3.90**	-5.35**	-3.90**	-0.040***	-0.032**	-0.042**	-0.034**
	(1.72)	(1.66)	(1.96)	(1.88)	(0.015)	(0.014)	(0.017)	(0.015)
HB 4050 x Mid FICO	0.49	2.55	0.93	3.11	0.002	0.017	0.004	0.016
	(2.87)	(2.61)	(2.77)	(2.47)	(0.022)	(0.023)	(0.022)	(0.021)
HB 4050 x High FICO					-0.003	0.018	-0.002	0.016
					(0.012)	(0.013)	(0.014)	(0.014)
FICO Score	-0.08***	-0.08***	-0.08***	-0.08***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.000)	(0.000)	(0.000)	(0.000)
Loan Margin (%)	1.71***	1.46***	1.63***	1.34***	0.020***	0.017***	0.019***	0.017***
	(0.13)	(0.11)	(0.15)	(0.11)	(0.001)	(0.001)	(0.002)	(0.001)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract Terms Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property Type Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip Code FE					Yes	Yes	Yes	Yes
Month FE					Yes	Yes	Yes	Yes
Month FE * log(Avg Income)					Yes	Yes	Yes	Yes
Zip Code * Month FE	Yes	Yes	Yes	Yes				
Observations	55,600	57,619	40,041	39,935	55,600	48,114	40,041	40,416
Adj. R^2 (pseudo R^2)	0.09	0.08	0.09	0.07	0.11	0.11	0.11	0.11

Table 9. Effects of HB 4050 on Mortgage Performance (continued)

Panel C: Default Rates, by Counseling Requirement (Source: LoanPerformance)

	Dependent variable: I(Default within 18 months) (x 100)							
	Rergression: OLS							
	Control	Matched	Control Active	Matched Active	Control	Matched	Control Active	Matched Active
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HB 4050 x Counseled	-2.49	-1.96	-2.61	-1.16				
	(1.99)	(1.96)	(2.19)	(2.08)				
HB 4050 x Mandatory Counseling					-5.14**	-4.75**	-5.42**	-4.09*
					(1.91)	(1.82)	(2.20)	(2.06)
HB 4050 x Voluntary Counseling					1.83	2.67	2.14	3.91
					(2.77)	(2.78)	(2.77)	(2.67)
HB 4050 x Exempt	-2.21	-0.93	-2.06	-0.05	-2.21	-0.90	-2.06	-0.01
	(1.32)	(1.31)	(1.43)	(1.31)	(1.32)	(1.32)	(1.43)	(1.32)
Observations	55,600	57,619	40,041	40,425	55,600	57,619	40,041	40,425
Adj. R^2	0.10	0.09	0.10	0.09	0.10	0.09	0.10	0.09

Regression also includes all controls found in specifications in Panel B

Table 10. What Led to the Decline in Default Rate?

The table examines whether and why lenders rejected more borrowers under the HB 4050 regime. Panel A uses OLS regressions to examine the determinants of loan rejection in HB 4050 areas. Panel B analyzes the stated reasons for loan rejection. Lenders can report multiple reasons for rejection for each loan. The panel is based on a series of probit regressions

$$\text{Prob}(\textit{Rejection for reason } j) = \Phi(\alpha + \beta\, \textit{HB 4050} + \gamma \log(\textit{size mortgage}) + \delta \log(\textit{income})) + \varepsilon,$$

estimated separately on a sample of rejections by active subprime lenders and by all other active lenders. The panel reports only the marginal effect of *HB 4050* for the average borrower for each of the evaluated rejection reasons. All standard errors are clustered at the zip code level. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Were Lenders More Likely to Reject Mortgages? (Source: HMDA)

	Dependent: I(Lender Rejects Application) x 100							
	State-Licensed Lenders Specializing in Subprime loans				All Other Lenders			
	Control	Matched	Control Active	Matched Active	Control	Matched	Control Active	Matched Active
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HB 4050	11.15***	11.33***	3.93***	3.44***	3.25***	2.45***	1.52**	1.28
	(1.37)	(1.22)	(1.26)	(1.23)	(0.63)	(0.73)	(0.66)	(0.82)
log(Mortgage)	0.41	-3.57***	-1.31***	0.18	-2.91***	-6.18***	-2.87***	-6.98***
	(0.27)	(0.32)	(0.32)	(0.61)	(0.26)	(0.35)	(0.28)	(0.38)
log(Income)	-3.67***	-0.25	-1.04	-1.83***	-6.58***	-3.35***	-7.40***	-3.44***
	(0.35)	(0.28)	(0.62)	(0.33)	(0.36)	(0.22)	(0.41)	(0.25)
Month FE, Zip Code FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE * log(Income)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip Code FE * log(Income)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	158,307	168,789	56,900	61,929	236,007	269,138	155,002	179,842
Adj. R^2	0.02	0.02	0.07	0.07	0.02	0.03	0.03	0.04

Table 10. What Led to the Decline in Default Rate? (continued)

Panel B: Stated Rejection Reasons (Source: HMDA)

Memo: Category	Reason for rejection	State-Licensed Active Subprime Lenders	All Other Active Lenders
Credit-related reasons	Debt-to-income	-0.026***	0.0046
		(0.006)	(0.006)
	Employment History	-0.0022	-0.003***
		(0.002)	(0.001)
	Credit History	-0.052***	-0.025***
		(0.007)	(0.007)
	Collateral	-0.063***	0.003
		(0.007)	(0.005)
	Insufficient Cash (downpayment, closing costs)	-0.007**	-0.001
		(0.003)	(0.002)
Incomplete information	Unverifiable Information	-0.062***	0.009**
		(0.007)	(0.004)
	Credit Application Incomplete	-0.038***	0.007
		(0.006)	(0.005)
"Other" reasons	Mortgage insurance denied	0.000	-0.001
		(0.000)	(0.000)
	Other	0.148***	0.021***
		(0.011)	(0.006)
	Observations	18,580	55,859

Figure 1. HB 4050 Treatment and Control Zip Codes

This figure presents a map of the HB 4050 treatment area (the shaded area) and the control zip codes (the striped area). As described in Section 3.3, the 12-zip control area is constructed to resemble the treatment area in terms of their pre-treatment socioeconomic characteristics and housing market conditions. The socioeconomic variables used for selection include: 2005 Internal Revenue Service (IRS) zip-code-level income statistics, as well as the 2000 Census shares of minority population, 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-level means of FICO scores, LTV and DTI ratios, and housing values. All of the control zip codes lie within the City of Chicago limits. The 12-zip control area has about as many residents as the treatment area.

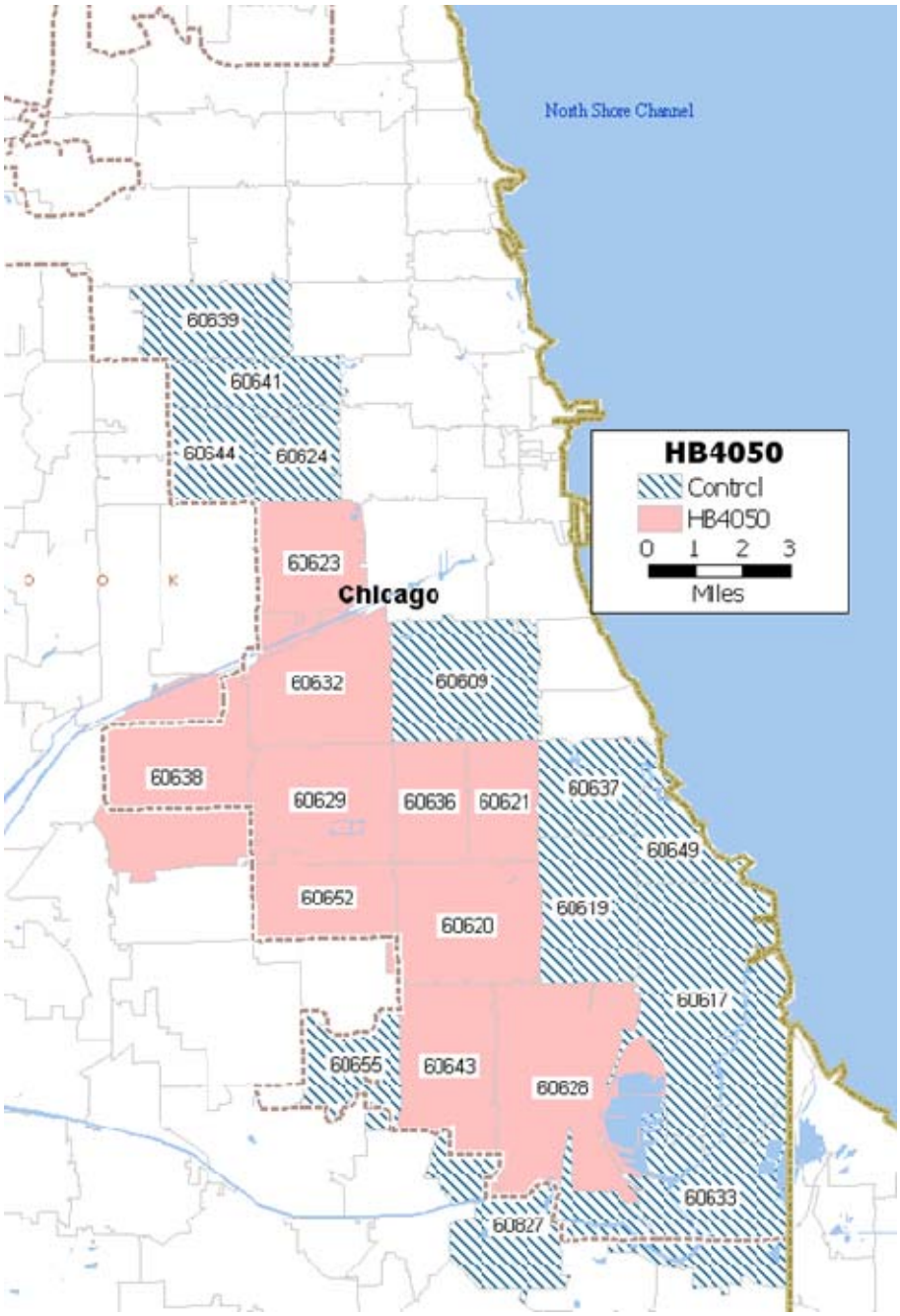

Figure 2. HMDA Loan Application Filings

This figure presents a time series of loan application filings. Figure 2a depicts filings in both treatment and control areas, separating lenders subject to HB 4050 and those exempt from it. Figure 2b focuses only on the treatment areas, differentiating between lenders that remained active during the mandate period and those that exited HB 4050 zip codes. The solid vertical lines denote the time during which HB 4050 was in force.

Figure 2a. HB 4050 and Control Areas: Lenders Subject to HB 4050 vs. Exempt Lenders
(Source: HMDA)

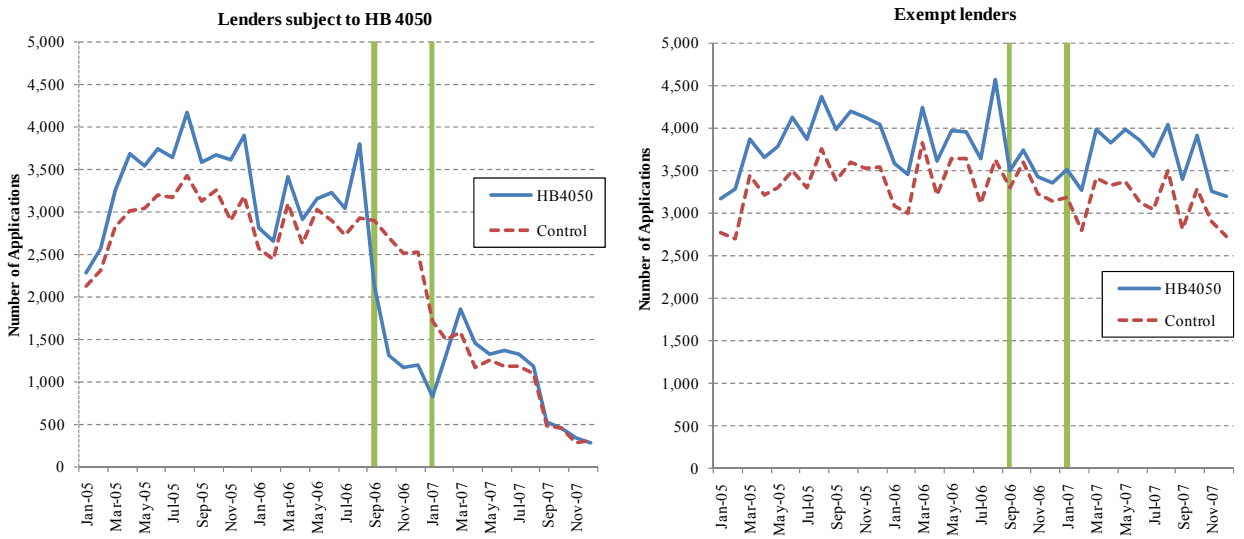

Figure 2b. HB 4050 Area: Lenders that Remained Active and Those who Exited
(Source: HMDA)

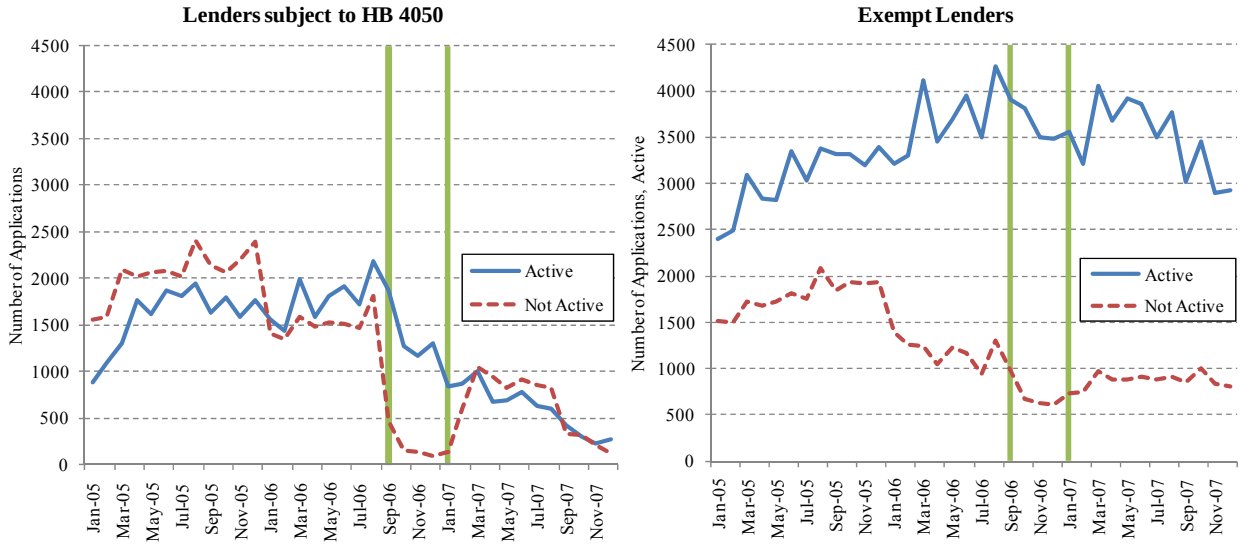

Figure 3. Borrower Quality: Cumulative Distribution of FICO Scores

This figure presents cdfs of borrower FICO scores on mortgages originated in the HB4050 area (the solid line) and the control area (the dashed line). Figure 3a depicts the FICO distributions of loans originated prior to the HB 4050 mandate, and Figure 3b does so for loans originated during the mandate period.

Figure 3a. Mortgages Originated Before the HB 4050 Period (1/2005 – 8/2006)
(Source: LoanPerformance)

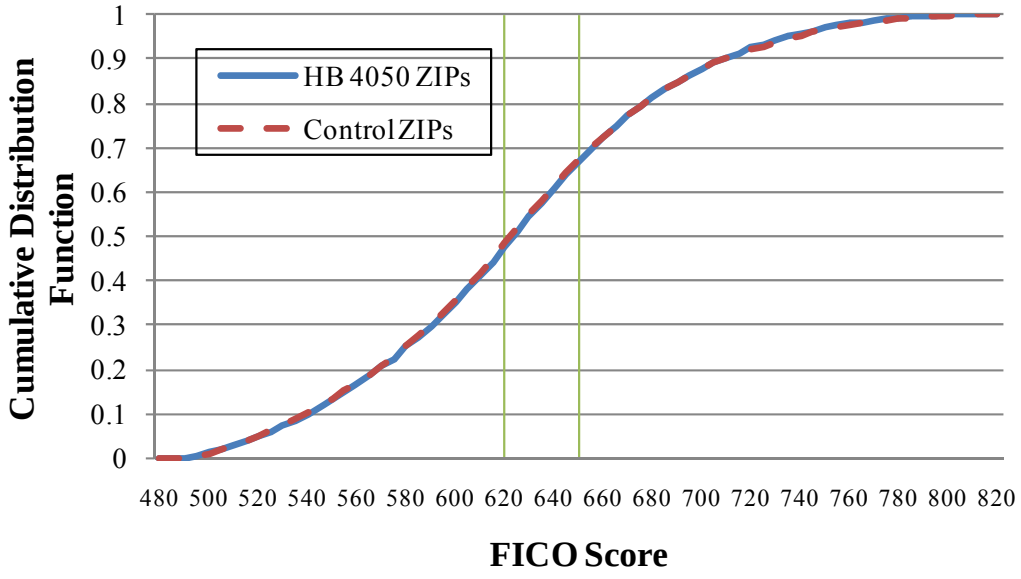

Figure 3b. Mortgages Originated During the HB 4050 Period (9/2006 – 1/2007)
(Source: LoanPerformance)

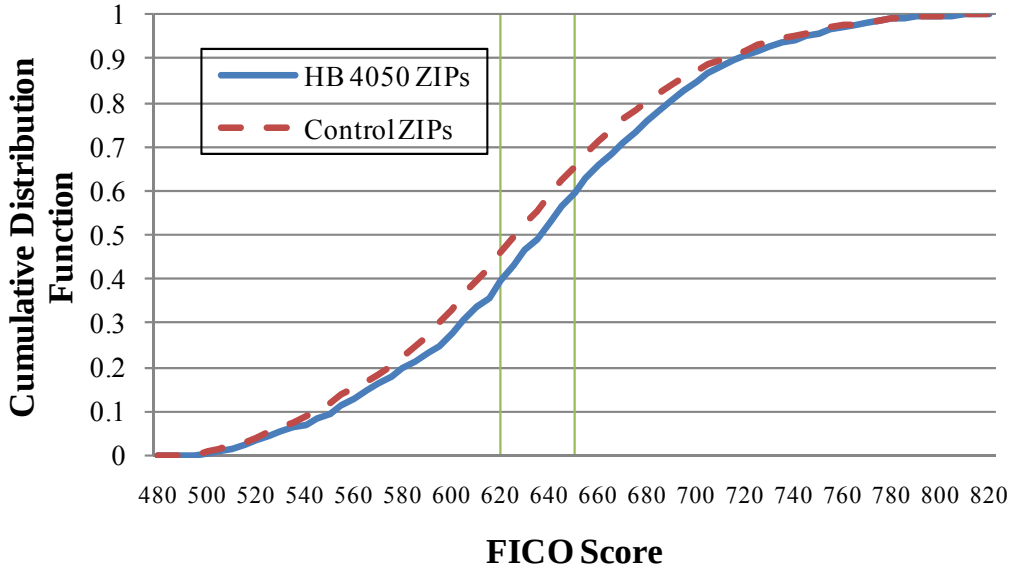

Figure 4. Shares of HMDA-Reported Applications Rejected by Lenders

This figure presents time series of loan applications rejected by lenders as recorded in the HMDA database. The rejections are reported as a share of total loan applications. Both panels depict lender rejection rates in treatment (solid line) and control (dashed line) areas. The left panel does so for lenders subject to HB 4050, while the right panel focuses on lenders exempt from the mandate. The solid vertical lines denote the time during which HB 4050 was in force.