Financial Education versus Costly Counseling: How to Dissuade Borrowers from Choosing Risky Mortgages?

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This paper explores the effects of mandatory third-party review of mortgage contracts on consumer choice. The study is based on a legislative pilot carried out in Illinois in 2006, under which mortgage counseling was triggered by applicant credit scores or by their choice of "risky mortgages." Low-credit score applicants for whom counselor review was mandatory did not materially alter their contract choice. Conversely, higher-credit score applicants who could avoid counseling by choosing non-risky mortgages did so, decreasing their propensity for high-risk contracts between 10 and 40 percent. In the event, one of the key goals of the legislation—curtailment of high-risk mortgage products—was only achieved among the population that was not counseled. (JEL D14, D18, L85, R21)

There is mounting evidence that consumers make suboptimal decisions when it comes to financial issues. In contrast to economists' models in which rational agents choose contracts that maximize their utility (e.g., Campbell and Cocco

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2003), the empirical evidence suggests that households fail to make fully optimal decisions in many areas.[1] In response, some policy initiatives focus on making consumers better-informed without imposing restrictions on their choice set, while others rely on establishing an acceptable set of financial contracts. Regulatory efforts following the financial crisis incorporated elements of both of these approaches.[2] Yet, there is little empirical consensus about which elements are effective. Such evidence is critical to understanding consumer decision making and to designing effective policies.

In this paper, we study a particular mortgage counseling program that aimed to curtail use of certain mortgage products through a combination of informational elements and costs. Our analysis sets out to understand which features of the program caused borrowers to modify their choices. In the unique setting of the program, one set of applicants was counseled about mortgage products they were offered, allowing us to test the effectiveness of information provision relative to a control sample that did not receive counseling. Another set of applicants was required to attend counseling only if they had chosen certain mortgage products. These applicants were thus able to avoid the compliance costs of counseling by choosing an alternative contract. The results indicate that the information provided had little impact on consumer choice. Yet, the program's compliance costs made those who could avoid counseling by choosing an exempt mortgage product do so.

[1] Examples of suboptimal choices include mortgage refinancing: Keys, Pope, and Pope 2016; Agarwal, Rosen, and Yao 2016; mortgage points: Agarwal, Ben-David, and Yao, 2017; credit cards: Agarwal et al. 2015; reverse mortgages: Davidoff 2014; pension choice: Beshears et al. 2011, Baugh, Ben-David, and Erel 2019; stock market participation: Cole and Shastry 2009.

[2] For example, the updated Federal Truth in Lending Act (TILA) regulations mandated better disclosure of mortgage information to borrowers without limiting the contract space. On the other hand, the Ability-to-Pay and Qualified Mortgage (QM) rules established by the Consumer Financial Protection Bureau set requirements for mortgage affordability and effectively eliminated a subset of mortgage contracts from the lenders' menu.

The program that we analyze was initiated in September 2006 with the stated goal of lowering foreclosures stemming from predatory lending practices. The program was piloted in ten contiguous zip codes on the South and West Sides of Chicago. Under the program, mortgage applicants with FICO credit scores of 620 or lower were required to attend a loan review session with a third-party counselor irrespective of their mortgage choice. Applicants with higher FICO scores had to attend a loan review session only if they chose mortgages deemed risky by the statute. 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 analysis uses a difference-in-differences approach that contrasts borrower choices in the treated sample with those in a control sample. We investigate mortgage contract type, leverage, and interest rates, as well as borrower likelihood to reject or renegotiate mortgage offers, and ex post mortgage performance. Since borrowers and lenders could respond to regulation in various ways, we pay particular attention to endogenous selection of agents out of treatment. This is done by constructing a dynamically matched control sample and by conditioning the analysis on lenders that remained active in the pilot area. The heterogeneity in mandate applicability within pilot zip codes allows us to sharpen the identification of treatment effects further. In particular, we highlight differences in treatment effects on borrowers who could not avoid counseling and those who could. This allows us to differentiate between borrowers acting on acquired information and lenders/borrowers responding to the cost of external review.

The mandatory counseling program affected the mortgage market in multiple dimensions. In our previous work (Agarwal et al. 2014), we found that the legislation resulted in substantial reduction in lending activity, as many lenders with predatory characteristics exited pilot areas altogether. Here we show that while

lenders that remained in the treated zip codes continued to offer counseling-triggering mortgages to prospective clients, the borrowers' choices changed substantially. Higher credit score borrowers avoided counseling by selecting exempt contracts. We further find that low-FICO borrowers who were always counseled and thus received additional information about contract risks were not any more likely to reject their loan offers and failed to renegotiate their loan terms. The mandate also appears to have discouraged some low-FICO borrowers from applying for mortgages in the first place.

We also study the performance of borrowers in treated areas. We report that *both* groups of borrowers in treated areas – those who received counseling and those who avoided it – exhibited measurably lower default rates. These effects are quite long-lasting, as they are observed at horizons of up to four years following mortgage origination. We attribute this result to the exit of lenders with questionable practices catering to borrowers with weaker credit characteristics, as well as to changes in contract choice. Overall, our results indicate that the information conveyed to applicants in the counseling sessions had little effect on their choices. Rather, there are several indications that borrowers viewed counseling as a burden. First, applicants altered their mortgage choice to avoid counseling. Second, applicants avoided shopping for additional quotes that would trigger additional counseling sessions. Finally, the pilot itself was halted because of intense pressure from community interest groups. There is no evidence that counseling resulted in modified mortgage choice for those borrowers who actually attended the counseling sessions.

Our paper contributes to the literature studying the role of financial counseling and, more broadly, financial education in enabling more informed choices by households. Evidence from earlier research suggests that households may borrow too much at high rates (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 (2015) find that respondents with poor financial literacy are more likely to have high-cost mortgages. Gurun, Matvos, and Seru (2016) find that lenders are able to steer less sophisticated borrowers towards more expensive mortgages through advertising. Fernandes, Lynch, and Netemeyer (2014) argue that "just-in-time" financial counseling interventions carry promise for improved outcomes, particularly when coupled with nudges and other decision support systems. More broadly, there is consensus that household financial literacy is inadequate and that mistakes resulting from this inadequacy have serious negative consequences. However, there is less agreement on whether long-term financial education, short-term counseling, or regulation that seeks to correct behavioral biases is effective at addressing this issue.[3]

The rest of the paper is structured as follows. In Section I, we describe the counseling mandate in detail. Next, we summarize the data and outline our methodology in Section II. Section III details the mandate's effect on various margins of borrower choices. We discuss policy implications of our findings in the final section.

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

A. Description of the Pilot Program

In 2005, the Illinois legislature passed a bill designed to curb predatory lending. As was common in much of the country, the state had already established anti-predatory provisions based on loan characteristics. However, these anti-predatory

[3] For instance, Bernheim, Garrett, and Maki (2001) and Cole and Shastry (2009) study the effect 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. Two recent papers – Fernandes, Lynch, and Netemeyer (2014) and Hastings, Madrian, and Skimmyhorn (2013) – provide thorough reviews of literature on this subject.

regulations could be skirted by lenders through alternative loan packaging, causing concern among some Illinois policy makers. For example, lenders began to advertise adjustable-rate mortgages (ARMs) with short, fixed-rate periods and steep rate reset slopes (e.g., 2/28 and 3/27 hybrid ARMs) after regulators targeted balloon mortgages. Consequently, the legislature began to shift the focus from regulating loan issuers to educating borrowers.

The 2005 anti-predatory legislation, sponsored by Illinois House Speaker Michael Madigan, mandated counselor review of mortgage offers for "high-risk borrowers," defined as applicants with low credit scores or risky product choices. The FICO score threshold for mandatory counseling was set at 620. In addition, borrowers with FICO scores in the 621–650 range also had to receive counseling *if* they chose certain mortgage products that were deemed risky by legislators. This list of such "risky contracts" included interest-only loans, loans with interest rate adjustments within three years, loans underwritten on the basis of stated income (low-documentation loans), and repeat refinancings within a 12-month period. All borrowers regardless of their FICO scores were required to submit to counseling if they took out loans that allowed negative amortization, had prepayment penalties, or had closing costs in excess of 5%. The counseling requirement was tightly enforced: mortgages could not be recorded (and, thus, could not be foreclosed in the future) unless borrowers provided a certificate of counseling or of exemption from counseling.

The program was intended to run as a four-year pilot for all mortgages – first- and second-liens, purchase and refinance mortgages, HELOCs and closed-end loans – secured by properties in select parts of Cook County, which covers the metropolitan Chicago area. Community-based groups and affected lenders spoke out against the legislation, but Illinois politicians fought to have their districts included in the pilot (Merrick, 2007). In the end, the bill (titled HB 4050) was passed on the last day of the 2005 legislative session and was defined to cover ten

contiguous zip codes on the South and West Sides of Chicago. Illinois legislators were quite optimistic about the potential for the program, which was modeled on a 1970s Federal Housing Administration (FHA) program (Merrick, 2007).

HB 4050 mandated that each of the affected 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 schedule it. During the one- to two-hour session, the counselor was supposed to discuss the terms of the specific offer for a home purchase loan or refinancing and explain their meaning and consequences to the prospective borrower. The goal was not to advise borrowers about the best mortgage option (as in Campbell and Cocco (2003)), but rather to caution the borrower against common pitfalls. In addition, the counselor verified the loan application information about the borrower (e.g., income and expenses). Afterward, the counselor recorded loan-related information from the session – whether the fees were thought to be excessive, whether the interest rate was in excess of the market rate, whether the borrower understood the terms of the transaction and/or could afford the loan – in a state-administered database.

Both the interview and the independent collection of data on borrower income and expenses allowed the counselor to form an assessment of a borrower's creditworthiness that potentially went beyond what was conveyed by the lender. Effectively, the counselor was able to elicit private information that was not necessarily used by the lender to make approval and/or pricing decisions and to furnish it to state regulators. The potential for this information to be formally documented may well have induced 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 counseling 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 coalition Housing Action Illinois (2007) summarized the counselors' assessment of HB 4050. During the pilot, 41 HUD-certified counselors reviewed loan offers for approximately 1,200 borrowers. Indications of fraud were found in 9% of the cases. Counselors advised about half of the borrowers that they could not afford the loan or were on the cusp of not being able to do so. For 22% of the borrowers, loan rates were found to be more than 300 basis points above the market rate. Counselors also found a discrepancy between the loan documentation and the verbal description of the mortgage for 9% of the borrowers. Furthermore, the vast majority of borrowers with adjustable-rate loan offers did not understand that their mortgage payment was not fixed over the life of the loan.

Although HB 4050 required lenders to pay the $300 counseling fee, there is anecdotal evidence that lenders were able to shift this cost to borrowers.[4] Even if lenders did bear the monetary cost of counseling sessions, HB 4050 still imposed other time and psychological costs on borrowers. Additionally, by lengthening the expected time until closing, HB 4050 could force borrowers to pay for longer credit lock periods, again 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 fully implemented.[5] Otherwise, lenders could potentially lose the right to foreclose on the property. Finally, lenders reportedly feared losing some of their ability to market high-margin products.

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[4] There is substantial anecdotal evidence that brokers and lenders attempted to pass the $300 counseling fee to 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).

[5] 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 the 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.

As mentioned earlier, HB 4050 mandates were only imposed upon state-licensed mortgage lenders, because the state does not have the legal authority to regulate federally chartered institutions and generally exempts such institutions and state-chartered banks from mortgage licensing requirements. State-licensed mortgage bankers had done much of the lending in Chicago's disadvantaged neighborhoods, presenting themselves as a local and more approachable alternative to the more traditional bank lenders.[6] Consequently, HB 4050 was expected to add to the regulatory burden on the very institutions that were providing credit in the selected pilot areas. Many observers voiced concerns that the legislation might result in credit rationing, negatively affecting housing values in the selected zip codes.

HB 4050's geographic focus was a substantial departure from typical regulatory approaches that require counseling for certain loan types (Bates and Van Zandt, 2007). This aspect of the legislation generated considerable opposition among community activists and residents, prompting a number of lawsuits. Because the vast majority of residents in the pilot areas were Hispanic and African American (82%), the geographic nature of the regulation prompted heated accusations that it was designed with discriminatory intent. 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 intensified.[7] The pilot program was suspended indefinitely on January 17, 2007, after only 20 weeks of operation.

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[6] Using the Home Mortgage Disclosure Act (HMDA) data described in Section II, we estimate that state-licensed mortgage bankers accounted for 64% of mortgage loan originations in HB 4050 zip codes during 2005. This is likely to be a gross underestimate of the share of mortgages affected by HB 4050, as the pilot's mandate extended to correspondent loans. That is, a loan initiated by a mortgage broker but funded by a national bank that reports it to HMDA would appear to be exempt but in reality would fall under the counseling mandate.

[7] The record of a public hearing held on November 27, 2006, provides a good illustration of the acrimony surrounding HB 4050 (https://www.idfpr.com/News/newsrls/032107HB4050PublicMeeting112706.pdf).

B. How Was the Pilot Program Area Selected?

HB 4050 required the Illinois Department of Financial and Professional Regulation (IDFPR), the state regulatory body, to specify a pilot area based on "the high rate of foreclosure on residential home mortgages that is primarily the result of predatory lending practices." In February 2006, IDFPR identified the pilot area, made up of 10 contiguous zip codes on the South West side of Chicago,[8] four of which were in Illinois House Speaker Madigan's district.

Table 1 presents key demographic and mortgage characteristics for the pilot area and the rest of the City of Chicago as of the end of 2005, based on data from the U.S. Census (2000), the IRS (2005), and the CoreLogic LoanPerformance (LP) database on securitized nonprime mortgages described in greater detail below. Compared with the city as a whole, the pilot zip codes are overwhelmingly populated by minorities and have much higher unemployment and poverty rates (Panel A). Panel B indicates that the pilot zip codes had markedly higher loan default rates (column (1)) compared with the rest of the city (column (3))—the basis for IDFPR's delineation of the pilot area. In addition, a comparison of population counts (Panel A) and the total number of loans in the LP database (Panel B) along with FICO scores (Panel B), strongly suggests that the HB 4050 area had a disproportional share of subprime and Alt-A mortgages.

II. Data and Selection of Control Groups

A. Data Sources

This study combines a number of complementary data sources covering the calendar years 2005–2007. First, we rely on data collected under the Home Mortgage Disclosure Act (HMDA), which includes information on mortgage applications, rejection rates, etc., to examine elements of credit supply and demand.

[8] The HB 4050 zip codes are: 60620, 60621, 60623, 60628, 60629, 60632, 60636, 60638, 60643, and 60652. The map of the treated and control zip codes is available in the Online Appendix.

When possible, we augment that information with loan application and counseling data collected by HB 4050 counselors. We also use the HMDA data to evaluate how HB 4050 affected the credit supply along the extensive margin, identifying lenders that left the market altogether. Finally, we use U.S. Census and Internal Revenue Service (IRS) data to control for income and population composition at the zip code level.

We also use the CoreLogic Loan Performance (LP) data to examine the effect of HB 4050 on the composition and performance of mortgages originated in the pilot zip codes. This loan-level database covers over 90% of securitized subprime and alt-A mortgages, and it includes detailed borrower and loan information, such as FICO scores, debt-service-to-income (DTI) ratios, 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 provides information on whether a given loan has a prepayment penalty, allows negative amortization, or was underwritten on the basis of full documentation. These and other characteristics of the LP data are summarized in Table 1, Panel C. FICO scores are used by lenders to assess borrower creditworthiness and set appropriate loan terms. 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 I.A for details).

B. Constructing a Zip-Code-Based Control Group Sample

As discussed in Section I.B, the selection of treated zip codes was driven by their characteristics as well as political considerations. Indeed, 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, which is meant to resemble the HB 4050 zip codes in terms of their pre-treatment socioeconomic characteristics and labor market conditions. Such areas could plausibly be expected to experience the same

changes in outcome variables as the HB 4050 zip codes in the absence of intervention. To construct our control group, we identify economically disadvantaged inner-city neighborhoods adjacent to the treated area using a set of measures more detailed than the simple univariate metric of foreclosure rates required by the legislation. Specifically, we identify zip codes within the Chicago city limits that have the smallest geometric distance from the HB 4050 zip codes using several data metrics: 2005 IRS zip-code-level income statistics, the 2000 U.S. Census shares of minority population, shares of the population living below the poverty level, and the unemployment rate. The resulting 12-zip-code area has approximately the same number of residents as the treatment area and is summarized in column (2), Panel A of Table 1.

While the selection of control sample was not based on any housing market characteristics, we confirm their similarity on that dimension as well using data for the 2005 calendar year, which could have been available to IDFPR at the time of their treatment area decision. The statistics in Panel B of Table 1 indicate that the control group's zip codes are similar to the treated area in terms of their pre-treatment period high delinquency rates, low borrower FICO scores, and disproportionate reliance on subprime mortgage products.[9] Panel C of Table 1 provides a comparison of key mortgage and borrower characteristics in the pre-treatment period. The means of key mortgage market variables reportedly used by regulators to proxy for "predatory practices" and "high foreclosure rates" – prevalence of low documentation loans and default rates – are statistically indistinguishable for the control and treatment zip code samples. Even for variables where statistically significant differences are observed, the economic magnitude is

[9] 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 reported results reported below are robust to the definition of the control area and are available upon request.

very small (e.g., 2.6 points in FICO scores and 0.9 percentage points in LTV ratios. Based on the spirit and the letter of the stated legislative guidelines, we conclude that the areas in our control group could have plausibly been selected for HB 4050 treatment.[10]

C. Constructing a Dynamically-Matched Sample

To further establish the robustness of our analysis, we construct an alternative matched sample that reflects changing characteristics of borrowers over time. One might be concerned that the pilot affected the composition of borrowers that remained active in the market in terms of their credit scores, mix of refinancing or purchase transactions, and loan and property values. In this case, estimated effects of the pilot on borrower choices may derive indirectly from its influence on borrower characteristics. Having a control sample based solely on pre-treatment covariate balance would then conflate the direct and indirect effects of the pilot.

Consequently, rather than choosing a similar but untreated set of zip codes, we build a comparison sample loan by loan, matching each on the basis of observable loan characteristics. For each mortgage loan originated in the HB 4050 zip codes, we identify the loan most similar to it that was originated in the same month elsewhere within the City of Chicago to a borrower in the same FICO group (≤620, 621-650, >650). We define similarity by computing a propensity score as a function of the borrower's FICO score and the square of the FICO score, the loan's LTV ratio, and indicator of LTV ratio in excess of 80 percent, the log of home value, and the loan's intended purpose (purchase or refinancing). Each of these variables used in matching is unaffected by the pilot in any individual case.[11]

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[10] The control area includes the following zip codes: 60609, 60617, 60619, 60624, 60633, 60637, 60639, 60644, 60649, 60651, 60655, and 60827. The Online Appendix map shows the control zip codes as striped areas.

[11] For instance, the FICO scores are indeed indicative of borrower quality in that they cannot be manipulated in the short term; see Keys, Mukherjee, Seru, and Vig (2010).

When a loan is matched to an HB 4050-area loan, we remove it from the set of potential matches and repeat the process for the next HB 4050-area loan. The resulting control set of matched loans comprises observations from 42 out of 43 non-HB 4050 Chicago zip codes. Not surprisingly, more than 50% of the observations in this control set come from the 12 control zip codes that we identified on the basis of their socioeconomic characteristics. By construction, this control set mimics time-varying composition of the observable characteristics of borrowers and loans under the HB 4050 regime.

In the subsequent analysis, we will refer to the comparable zip codes and the dynamically matched sample counterfactuals as the geographic control sample and the matched sample, respectively. The key characteristics of the treatment, geographic control, and matched samples are summarized in Table 1, Panel C.

D. Design of Tests: Micro-Level Analysis

Our empirical analysis relies on a difference-in-differences framework that exploits cross-sectional and temporal variation. We measure the difference in response of a number of variables (such as contract choice) as a function of whether the loan was originated in one of the HB 4050 zip codes during the legislative pilot timeframe. We include both time and cross-sectional controls in our regressions. For example, when we study whether borrowers who attended counseling sessions altered their mortgage choices, we compare the choice of risky products by borrowers who were forced to attend counseling sessions under HB 4050 and the choice by those with similar characteristics in the control areas.

Our basic regression specifications have the following form:

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

where $Response_{ijt}$ is the loan-level response variable, such as contract choice of loan i originated at time t in zip code j; $Treatment_{jt}$ is a dummy variable that takes a value

of 1 for loans in the ten HB 4050 zip codes originated during the five months of the pilot, and 0 otherwise; *Time* and *Zip code dummies* capture fixed time and location effects, and ε_{ijt} is an error term. In all regressions, we cluster errors at the zip code level.[12] For each loan, the response is evaluated at a single point in time (e.g., interest rate at origination). Thus, our data set is made up of a series of monthly cross-sections. The set of controls includes variables such as the LTV ratio at origination, borrower FICO score, loan's intended purpose and property type.

The difference-in-differences approach relies on the absence of divergent trends during the pre-treatment period. To assess the validity of this assumption, Figure 1 plots the time series of mortgage applications, default rates, and contract choices in each of the three subsamples. The top panel shows that application volumes in treatment and control samples followed a similar seasonal trend prior to the implementation of HB 4050 in September of 2006.[13] All three samples display similar trends in mortgage default rates prior to September 2006: the fraction of loans in default within 18 months of origination has been climbing steadily, with loans in the matched sample having consistently lower absolute rates. The bottom panel depicts the fraction of mortgage contracts classified as "riskiest" by HB 4050 (subject to counseling irrespective of the borrower's FICO score). The time series of the prevalence of this type of mortgage contracts in these three samples are virtually identical prior to September 2006.

[12] 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. We also bootstrap standard errors using the procedure specified in Cameron, Gelbach, and Miller (2008) via Stata 14 *boottest* command using the wild cluster resampling approach and Rademacher weights. We report the bootstrapped confidence intervals in the Appendix Table 1 that contains product choice regressions for the geographic control sample. This sample is chosen because it has the smallest number of zip code clusters (10 treatment and 12 controls) and thus has the greatest potential to underestimate standard errors. In the event, we found the differences in standard errors to be very small.

[13] The synthetic sample has origination counts that are identical to the treated sample by construction and is thus not shown as a separate series in Panel A.

E. Other Empirical Considerations

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

In addition to constructing a dynamically matched sample to account for borrower selection out of treatment, we address the problem of lender selection by estimating two sets of regressions: those conducted on loans originated by all lenders, and on those originated by lenders that remained active in the HB 4050 zip codes during the treatment period. We call the latter the Active Lenders sample. To be considered an active lender, a HMDA reporting institution must have average origination volume no less than 10% of the pre-HB 4050 volume and have at least one origination in each of the pilot months.[14] This 'Active Lender' sample holds the population of lenders constant, allowing for the identification of treatment effects unrelated to changes in lender composition.

Another identification assumption in the study design is lack of spillovers – informational or otherwise – between treated and control areas.[15] One example of such spillovers may take the form of potential homeowners buying houses just outside of the pilot treatment area. Another example may involve borrowers in non-pilot areas taking HB 4050 definitions as "risky" contracts seriously and adjusting

[14] The five-month period is chosen to match the duration of HB 4050. None of the patterns depends on the choice of the threshold level or geographic area. The "every month" condition is intended to eliminate lenders that withdraw from HB 4050 zip codes during the fall of 2006 after working off their backlog of earlier applications. Imposing the active lenders restriction limits the sample to 26 lenders, which account for 40% of all pre-HB 4050 loan volume. According to the Housing Action Illinois (2007) report, in the HB 4050 zip codes alone, these lenders were represented by more than 300 mortgage brokers.

[15] This is formally known as Stable Unit Treatment Value Assumption (SUTVA) after Rubin (1974). In our context, SUTVA requires that actions of borrowers and lenders outside of HB 4050 area be unaffected by the pilot.

their choices accordingly. Yet another may involve lenders interpreting HB 4050 as an indicator of greater regulatory scrutiny in general and adjusting their behavior in non-pilot areas as well. These types of spillovers would make strict causal interpretation of the pilot effects more difficult, and they would generally drive our DID estimates towards zero. While we cannot directly rule out such violations of non-interference between treatment and control groups, we undertake several robustness tests to allay concerns about their importance. For instance, we conduct a separate evaluation of borrower choices for refinancing transactions in which borrowers have no choice about property location.[16]

F. Dealing with Non-Random Treatment Sample Choice

An additional potential 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 analyzing 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 choice was more sensitive to economic conditions in low-income areas and more sensitive to the general decline in house prices following the market peak around November 2006.

We offer two solutions for this treatment zip code selection concern. 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 (those with FICO scores ≤ 620) were subject to counseling, while the mid-FICO (those with scores in the 621–650 range) and the high-FICO borrowers (those with scores > 650) were counseled conditional on their mortgage contract choice. This

[16] 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.

approach retains the structure of standard difference-in-differences analysis while also exploiting the within-zip-code heterogeneity in treatment.[17] We further interact the monthly time dummies with the log of the average income of a zip code, 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 each particular zip code. That is, the ability of borrowers in higher-income zip codes to make mortgage payments and their reliance on particular mortgage contracts may be less affected by an adverse economic shock. This effect would be captured by the time and area income interaction, alleviating some of the selection concerns. The regression specification that we therefore estimate is:

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

In all of our analysis we are evaluating the characteristics of securitized subprime and Alt-A mortgages contained in the LP data.

III. Mortgage Counseling and Contract Choice

The design of the HB 4050 program allows for an estimation of the effects of both information and compliance costs on borrower decisions.[18] First, to investigate the effects of information provided in counseling sessions, we exploit the fact that low-FICO applicants (those with FICO scores below 621) were required to attend

[17] 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 III, the mandate caused a sizable move away from mortgage contracts that triggered counseling for mid- and high-FICO-score borrowers.

[18] Our purpose here is to evaluate adjustments in consumer choice resulting from the HB 4050 program. However, Agarwal et al. (2014) also found there was significant reaction by mortgage lenders, which swiftly withdrew credit availability in the affected markets. Thus, mortgage supply was also significantly affected by the new program. Table 4A provides summary statistics on lender participation in mortgage lending in treatment and control areas.

counseling. Thus, we can test the effects of mandated counseling on mortgage contract selection by comparing the borrowing choices of this group under the mandate with the borrowing choices made in the pre-HB 4050 period or by a similar group in the control zip codes. We can also evaluate the effect of *signaling*, which is a variant of the information effect. Unlike specific information obtained directly in counseling sessions, the signaling effect of HB 4050 is disseminated indirectly through designation of certain products as risky (i.e., their selection triggered counseling). This effect would not be dependent on attending a counseling session.

Second, we explore the effects of imposing compliance costs by examining the contract choices of mid- and high-FICO applicants. These applicants could avoid counseling by not selecting mortgages that were specified as risky by the legislation. Again, we compare the change in mortgage choices over time and relative to those of similar groups in control zip codes. By segmenting our sample and accounting for different counseling requirements across product choices, we are able to distinguish between the effects of new information from the counseling process and new incentives to avoid counseling costs.

A. Multivariate Evidence on Borrower Contract Choice

From interviews with a number of counselors involved with HB 4050, we know that borrowers were typically warned about the 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 and toward fixed-rate mortgages available to this population; for instance those backed by the Federal Housing Administration (FHA).

As described above, information pertaining to broad product choices was provided not only through counseling sessions, but also by the mere designation of certain products as risky. These designations were known to everyone in the state and disseminated through statewide news coverage of the pilot. As such, they may

have constituted a credible signal to potential borrowers to avoid these mortgage products. If this signaling effect is at work, we would expect the incidence of risky product choices to decline for all borrowers in both the treated and control samples. If the signal was more salient in the affected areas, we would expect the incidence of risky product choices to decline for all FICO groups in the treated zip codes. This would produce negative estimates for β_1, β_2, and β_3.

That said, product choice may have been affected by the borrowers' desire to avoid costly counseling sessions. In this case, members of a given FICO group would avoid products that trigger counseling only for their particular group. That is, contract choices of low-FICO borrowers should be unaffected by HB 4050, since all choices led to counseling. On the other hand, we would expect fewer interest-only loans by mid-FICO households, but not for high-FICO households. Similarly, we would expect both mid- and high-FICO households (but not low-FICO ones) to choose fewer negative amortization loans and mortgages with a prepayment penalty. In other words, the counseling-avoidance hypothesis would lead us to expect $\beta_1 \sim 0$, $\beta_2 < 0$, and $\beta_3 \sim 0$ for mortgages that trigger counseling only for mid-FICO borrowers, and $\beta_1 \sim 0$, $\beta_2 < 0$, and $\beta_3 < 0$ for mortgages that require both mid- and high-FICO borrowers to be counseled.

Table 2 presents the results of difference-in-differences regressions of borrower contract choice, as outlined in equation (2). The first set of regressions focuses on choices that trigger counseling only for the mid-FICO borrowers—namely, choices to take out hybrid ARMs, interest-only loans, and low-documentation (low-doc) loans. These choices are labeled as Risky Products: Only Mid-FICO Counseled. The second set of regressions analyzes choices that trigger counseling for both mid- and high-FICO borrowers—loans with prepayment penalty, negative amortization or closing costs in excess of 5% of the loan size (Risky Products: Mid- and High-FICO Counseled). These regressions control for the borrower's FICO score at origination, log of 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 income of a zip code. As discussed in Section II, these regressions are estimated with different control samples and with different subsets of lenders.

The estimated coefficients for low-FICO borrowers offer little support for information-driven effects. If such borrowers acted on counselor advice to avoid risky products, one would expect to find negative values of β_1 (first row of Table 2). However, none of the coefficients that capture actions of low-FICO borrowers in treatment areas is significant at the 5% level. This holds for both types of risky contracts (columns (1)-(2) and (5)-(6)), whether in geographic or synthetically matched control sample.

The estimates are also inconsistent with the signaling effect of risky product designation, which would be manifested either in no significant difference-in-differences 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 Only Mid-FICO Counseled products than those in the control areas. We estimate a decrease of about 6-8 percentage points in the propensity of treated borrowers to choose these products during the pilot period (columns (1)-(2)). This difference is relative to the mean of 79-81 percent for the mid-FICO borrowers in the control group. However, high-FICO borrowers in treated areas are not any more likely to shy away from the Only Mid-FICO Counseled products, which do not trigger the counseling requirement for them. The absence of an effect for high-FICO borrowers is fairly precise. For instance, the 95% confidence interval for the high-FICO group coefficient in HB 4050 zip codes spans the range between −1.9% and 2.8%.

We observe a similar pattern for Mid- and High-FICO Counseled contracts. Low-FICO borrowers appear unaffected by the fact that these contracts subject higher-

FICO borrowers to counseling. In contrast, high-FICO borrowers in HB 4050 zip codes do reduce their use of such products. Their propensity to take out Mid- and High-FICO Counseled contracts drops by between 4 and 7 percentage points (columns (5) and (6)), relative to the control group mean of 16-18 percent. However, we do not observe a measurable effect of HB 4050 on propensity of mid-FICO borrowers to take up these products.

It might be possible that the estimated effects of the pilot on borrower contract choice reflect large-scale exit of lenders unwilling to abide by the counseling and reporting mandate requirements.[19] To check for this possibility, we re-estimate borrower choice regressions on a sample limited to those lenders that remained active in the HB 4050 zip codes during the pilot months. This restriction ensures that the control and the treatment groups consist of loans issued by a largely similar set of lenders both during the pilot timeframe and outside of it. The resulting Active Lender sample produces estimates (columns (3)-(4) and (7)-(8)) that are very similar to those obtained with the full lender sample. The mid-FICO borrowers step back from Only Mid-FICO Counseled contracts about as much in the Active Lender sample as in the full sample. The same is true for the high-FICO borrowers' choice of Mid- and High-FICO Counseled contracts under the pilot. For these contracts, the coefficient estimates for the mid-FICO borrowers become consistently negative in the Active Sample, but they are too noisy. Overall, the comparison of the full and Active Lender samples suggests that borrower responses to HB 4050 were not driven primarily by the exit of lenders specializing in targeted contracts. Section III.C explores this issue further.

On net, the evidence on product choice is consistent with the counseling-avoidance hypothesis that the counseling requirement constituted a costly burden that was avoided by those borrowers who were not automatically subject to it. The

[19] Table 4, Panel A documents the extent of the reduction in the number of active lenders triggered by HB 4050.

law led to a change in the product mix (whether initiated by borrowers or lenders) by the mere threat of counseling and not by the content of that counseling.

B. Robustness Checks of Results on Borrower Contract Choice

As discussed in Section II.E, the key identification assumption in any causal inference exercise is the lack of spillovers between treatment and control units of analysis. This condition is extremely difficult to satisfy in any setting that allows for general equilibrium effects, as is the case in this study. However, we can attempt to partially allay concerns about the importance of these possible violations by subjecting the results in the preceding section to a number of additional tests. We use the Active Lender sample for the synthetically matched control group as the baseline for these comparisons.[20]

One concern in the implementation of HB 4050 was that potential homeowners might choose to escape the counseling mandate by buying houses just outside of the treatment area. This might be particularly problematic since a substantial part of the treatment and control areas are geographically contiguous (see Online Appendix), and adjusting the choice along the pilot boundary is feasible. To deal with this, we restrict the estimation sample to refinancing transactions. Unlike home purchases, such transactions involve borrowers who are already locked into their location and thus the treatment regime. The results for Only Mid-FICO Counseled and Mid- and High-FICO Counseled contract choices, shown in columns (2) and (6) of Table 3 suggest that there is no appreciable difference in point estimates between the refinancing-only sample and the baseline sample (columns (1) and (5)).

Another type of informational spillover may occur temporally. In particular, the announcement of the pilot treatment area in February of 2006 preceded its September 1st implementation. This resulted in a marked spike in mortgage

[20] The results are not sensitive to the choice of the data sample.

originations in August, as market participants rushed to beat the onset of the counseling mandate (Agarwal et al. 2014). If market participants who pushed through their transactions just before the onset of HB 4050 were savvier or somehow different in terms of their unobservable characteristics, the parallel trends assumptions in the pre-treatment period will be violated and the DID estimates will be biased. To address this possibility, we eliminate front-running transactions (i.e., transactions that occurred in August 2006) from the sample and re-estimate (2) for risky product choice. The results shown in columns (3) and (7) of Table 3 are very close to their respective baselines.

We also want to address the possibility that HB 4050 may have signaled intensifying political scrutiny of lending practices. Such concerns could arguably have been most acute in locations represented by officials with greater political clout. As discussed in Section I.B, four out of ten HB 4050 zip codes were located in Speaker Madigan's district. If borrowers in these zip codes received special treatment from lenders, we could be misidentifying lender reaction to a general political change in a specific area as informational or steering effects of the pilot program. We drop these four zip codes from the sample and re-estimate the contract choice regression. The results, shown in columns (4) and (8) of Table 3 are once again very close to the baseline. As an additional check of the functional form, we allow unique (linear) time trends for each of the zip codes in the baseline sample. The results in columns (5) and (10) show that doing so yields qualitatively similar results.

C. Lender Response and Contract Menu Options

One could argue that the evidence presented above is consistent with lenders simply removing products that trigger counseling from their menu of choices. Because lenders tend not to specialize in either mid- or high-FICO borrowers, the

FICO-group-specific pattern of changes casts some doubt on this hypothesis. Nevertheless, we can also conduct a direct test of mortgage menu options of lenders that remained active in HB 4050 areas. The results are summarized in Table 4, Panel B.

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 its menu if it originated at least one such loan over a given calendar period. The main finding is that conditional on continuing to lend in HB 4050 areas during the pilot, the share of lenders willing to offer products that trigger counseling changed very little. This holds true for all contract types. In particular, interest-only mortgages, hybrid ARMs, and contracts with prepayment penalties appear to be offered by nearly all remaining lenders during the pilot period. Yet, the likelihood of originating such loans declined markedly, as shown in Table 2. Moreover, there is little evidence to suggest that the menu of contract types (with the possible exception of option ARMs) evolved differently in HB 4050 and control zip codes. Contract menu does not appear to reflect area-specific requirements, but rather lender-level choices over time.

Table 5 repeats the analysis of Section III.A, concentrating on a specific subset of Only Mid-FICO Counseled mortgages—low-documentation loans. This subset of loans is particularly interesting, since HB 4050 forced collection and recording of information, which might have undermined the appeal of low doc underwriting. Similar to the other products in the Only Mid-FICO Counseled set, low doc mortgages became less prevalent among mid-FICO borrowers under the pilot, dropping by about 7-8 percentage points (relative to the control group mean of about 50 percent). Interestingly, we found an even greater retrenchment from low-doc mortgages among low-FICO borrowers who were subject to counseling regardless of contract choice. Relative to the pre-pilot control group mean of 22-26

percent, the take up of low doc mortgages dropped by about a half, or 11-12 percentage points.

These results cannot be explained only by counseling-avoidance motives, since low-FICO borrowers would not have been able to avoid counseling by shying away from low doc loans. On the other hand, a pure information effect is also wanting – we would expect all groups to lessen their take up of low doc loans if HB 4050 signaled their high risk to consumers, but we see no such evidence for high FICO borrowers. Rather, these results are consistent with a nuanced narrative in which different borrower groups turn to low-doc loans for various reasons. As shown in Jiang, Nelson, and Vytlacil (2014), low-FICO borrowers turn to low documentation (or "liar") loans in order to qualify for credit. Since all low-FICO borrowers are counseled, their actual income and asset documentation is entered into the state-administered database. This, in turn exposes prospective lenders to legal risk if they choose to underwrite a loan on the basis of stated income, which decreases the appeal of low-doc loan underwriting. As a result, low-FICO borrowers who cannot qualify for loans based on their documented income drop out of the market,[21] whereas those who can qualify decrease their reliance on low-documentation mortgages.[22]

In contrast, high-FICO borrowers avail themselves of low-documentation loans (known in their case as alt-A loans) for convenience of not having to gather evidence of self-employment income and various asset holdings. As shown in Table 5, the majority of high-FICO borrowers relied on low-documentation mortgage contracts. Since choosing a low-doc mortgage does not trigger counseling and its

[21] Table 1, Panel C shows a substantial drop in the share of low-FICO borrowers in HB 4050 zip codes during the pilot. This is discussed in greater detail in Agarwal et al. (2014) and also in Section III.F.

[22] Comparing columns (1)-(2) and (3)-(4) of Table 5 indicates that a small part of the decline in take-up of low-doc loans by low-FICO borrowers is due to the exit of lenders that offered these loans in the pre-HB 4050 period.

attendant costly documentation process for high-FICO borrowers, they continue to utilize low-doc contracts extensively.

D. Mortgage Terms

An additional way to evaluate the hypothesis that loan review provided useful information to borrowers is through an analysis of 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 leverage and negotiate better loan terms.[23] If borrowers were able to follow such advice, one would expect to observe lower LTV ratios and 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 in equation (2) for the samples described earlier. We find a significant decrease in the LTV ratio for the low-FICO borrowers (columns (1)-(2)).[24] These relative improvements translate to a decrease in debt levels of about $2,500 for an average borrower. Among low- and mid-FICO borrowers, we find no identifiable effects of HB 4050 on interest rate spreads, when the sample is restricted to lenders that remained active during the treatment period (columns (5)-(6)).[25] These groups display statistically significant, if small, improvements in spreads in the all-lenders sample (columns (3)-(4)), suggesting that lenders who exited HB 4050 areas were charging higher interest spreads than

[23] Lower leverage for refinancing transactions is typically accomplished by reducing the amount of cash taken out. For purchase transactions, however, the only way to lower leverage is to come up with a larger down payment.

[24] Note that for LTV and DTI ratio regressions, we do not present matched sample results because LTV and DTI ratios were used in the matching process.

[25] For ARMs, the LP data provide the information for the spread (margin) relative to the contract reference rate. For fixed-rate mortgages, loan spread is calculated as the difference between the contract interest rate and the rate of standard 30-year fixed rate contract reported monthly on the Freddie Mac (FHMLC) survey.

those that remained active. However, the high-FICO borrowers in treated zip codes experienced about a 25 basis points drop in interest rate spreads. It is worth noting that even among the high-FICO borrowers in our analysis sample, interest rate spreads are very high, averaging between 370 and 390 basis points.

In Panel B of Table 6, we explore measures of loan affordability by looking at the debt-service-to-income (DTI) ratio that captures borrowers' ability to service their mortgage obligations (columns (1)-(2)). This measure is constructed by forming a ratio of the average scheduled monthly payment during the first mortgage year and the monthly income reported on the mortgage application. Although treated low-FICO borrowers lower their leverage and have marginally lower interest rate spreads, their DTI ratios are a few percentage points higher than those in the control group. This may be explained by declines in reported income by low-FICO borrowers forced away from low documentation "liar" loans by the pilot. To check whether the pilot meaningfully affected loan repayment schedules, we also evaluate changes in the ratio of the annual mortgage payment to the original loan size (columns (3)-(6)). We fail to detect any effect of the pilot treatment on the payment-to-loan ratios, which suggests little change in loan terms in HB 4050 zip codes.

E. Mortgage Performance

The results presented up to this point suggest that borrowers affected by HB 4050 tried to avoid counseling whenever possible by adjusting their contract choice (Tables 2, 3 and 5). However, affordability of their mortgages did not change substantially (Table 6B). Both contract choice and affordability metrics capture mortgage features at the time of origination. Still, it is critically important to assess whether responses to the counseling mandate extended beyond that point in time. Evaluating mortgage performance over sufficiently long horizons represents a

natural and important metric of the pilot's effectiveness, as it captures some of long-term impact of the mandate on borrowers and their communities.

We begin this analysis by presenting raw default rates for mortgages originated during the HB 4050 pilot period (September 2006 to January 2007) as a function of mortgage age in Figure 2. We differentiate between performance of mortgages outside of the HB 4050 geographic area (the control group) and mortgages originated in the 10 HB 4050 zip codes (the treatment group).[26] The treated mortgages are further subdivided into two groups: those that received counseling prior to origination and those that were exempt from counseling by virtue of their sufficiently high FICO scores and contract choice. We refer to these two groups as "treated/counseled" and "treated/avoided counseling", respectively. A mortgage of age *n* is considered to be in default if it experienced a 90+ day delinquency or foreclosure at any point during its first *n* months after origination. The resulting series capture cumulative default rates of mortgages originated in each of the three groups.[27]

The first striking feature of Figure 2 are the very high delinquency rates for all borrower groups in out sample. One year after origination, default rates on control group mortgages exceed 18 percent. By the two-year mark, nearly 40% of mortgages in this group are in default, and by their fourth anniversary, cumulative default rates stand at 63%. These rates capture experience of mortgages originated at the very peak of the housing bubble in some of the inner-city geographic areas worst hit by the Great Recession and are similar to figures reported elsewhere in

[26] The analysis in Figure 2 uses the geographic control group. The results are substantively similar with the matched sample.

[27] This classification approach regards each default as an absorbing state. Thus, it misclassifies instances in which a severely delinquent loan becomes current again or when a borrower is able to resolve foreclosure by paying off mortgage arrears. These events are very rare in our sample. Furthermore, loans that disappear from the sample as a result of refinancing are considered non-delinquent, as they are certain to not have experienced default over their observed life span. This classification allows us to compute unbiased cumulative default rates in our sample.

the literature (e.g., Demyanyk and van Hemert, 2011). The second key feature of Figure 2 is that mortgages in both of the treated groups – those counseled and those that avoided counseling – display consistently lower default rates at all horizons.

In order to ascribe some part of the observed differences to the effect of the counseling mandate, we need to filter out influences of borrower and loan quality. This is particularly relevant for the avoided counseling group, which by construction has higher credit-quality borrowers and would thus be expected to realize lower default rates. We construct predicted default rates for each of the treated and control groups by using the following process. First, we fit a loan-level logit model of mortgage default at or before age *n* in a sample of control loans as a function of FICO, LTV, log of home value, property type, and time and location fixed effects. Next, we use the estimated coefficients to predict loan-level default by age *n* and compute the mean predicted default rate among all loans in a given group. Finally, excess default rates are computed for each group as the difference between the realized and predicted values. This is done iteratively for each of the loan ages up to 50 months after origination.[28]

Figure 3 depicts the excess (adjusted) cumulative default rates along with the corresponding two standard deviation bands for each of the treated groups. Once we adjust for observable differences in borrower and loan characteristics, the difference in default rates between the treated and control groups shrinks somewhat. However, for horizons up to 18 months, the low-FICO score borrowers who invariably received counseling realized statistically lower mortgage default rates. Their excess performance diminishes over time and it becomes statistically indistinguishable from zero after 18 months.

[28] This approach is similar in spirit to the analysis in Demyanyk and vanHemert (2011) and Agarwal and Ben-David (2018). All points estimates and calculation details are reported in the Online Appendix.

In contrast, those borrowers who could avoid counseling appear to have experienced a lasting improvement in their loan performance. Over the time window spanning 12 to 36 months following origination, their adjusted default rates average about 6 percentage points less than those in the control group. The counseling mandate resulted in improved performance for those borrowers that never attended the counseling session but whose contract choices were affected by the desire to avoid counseling. However, it is worth noting that even though a 6 percentage point improvement in performance is both economically and statistically meaningful, it is small in comparison with the high rate of realized defaults.[29]

F. Borrower Extensive Margin Responses

HB 4050 required additional counseling 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 more of a burden than a source of valuable information, we would anticipate fewer rejections of loan offers to counseled borrowers—regardless of the counselor's recommendation. 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 7 presents a test of these hypotheses, using HMDA application data.[30] 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

[29] Agarwal et al. (2014) traces the decline in defaults among some treated borrowers to exit of predatory lenders from the pilot area. We confirm those results in this setting by recomputing excess default rates for the active lender subsample only. As in Agarwal et al. (2014), we observe no statistically measurable improvement in the performance of loans originated in the HB 4050 zip codes during the pilot period by active lenders.

[30] These are loans in HMDA classified as "approved, but not taken." As HMDA data do not contain borrower FICO scores, these regressions identify potential treatment solely on the basis of geography.

mortgage rejection by borrowers *did not increase* during the HB 4050 period (column (2)). In fact, borrower rejection rates actually declined during the HB 4050 period in the lender sample (columns (1)), again suggesting a difference between lenders that exited the market and those that remained active.

This finding is rather remarkable given that the majority of the counseled borrowers 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 after counseling (e.g., narrower loan spreads as shown in Table 6), 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.[31]

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 costs associated with searching for an alternative loan likely far outweighed the expected benefits of new offers.

An alternative mechanism by which HB 4050 may have affected borrower actions is by discouraging applications in the first place. Borrowers who knew that they could not escape the cost of counseling might have been willing to apply for a loan in the first place. If we were to posit that lender approval rates remained unaffected by the pilot, such self-censoring would be manifested through lower shares of mortgages originated to the low-FICO borrowers in HB 4050 zip codes. The summary statistics in Table 1, Panel C suggest that such a shift in borrower composition did take place, as the share of low-FICO borrowers dropped by 9

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

percentage points in HB 4050 zip codes and only 3.5 percentage points in the control zips.

We carry out a more rigorous analysis of the effect of HB 4050 on share of loans originated to treated borrowers. Specifically, we regress monthly zip-level shares of mortgages by low-FICO borrowers on the treatment indicator, along with a full set of time and zip code controls, and zip-month average values of incomes, home values, and mortgage amounts. The resulting difference-in-differences estimates reported in columns (3)-(6) of Table 7 suggest that HB 4050 produced a drop of about 5.5 percentage points in the share of mortgages originated to low-FICO borrowers. A similar analysis of mid-FICO borrower mortgage shares shows no effect. As shown earlier, these borrowers were able to avoid counseling by adjusting their mortgage contract choices.

In sum, our analysis in Section III identifies only marginally 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 borrower loan rejection rates, the absence of measurable improvement in loan spreads, and the short time span between the loan review sessions and the scheduled closings all suggest limits to borrower ability to use counseling information to renegotiate the terms of their original mortgage offers. In contrast, the pattern of changes in product choices is broadly consistent with borrowers' (and lenders') desire to avoid oversight when possible.

IV. Conclusion

Regulators have responded to the financial crisis by deploying a variety of policy tools that included financial counseling measures and increased oversight of lenders.[32] Both strategies limit free contracting between borrowers and lenders. As

[32] On July 21, 2010, the Dodd–Frank Wall Street Reform and Consumer Protection Act became law and introduced the new Consumer Financial Protection Bureau, which, among other things, was to

such, they are likely to shrink access to credit markets, particularly for the financially disadvantaged segments of society.

In this paper, we evaluate the impact of one such policy tool: a mortgage counseling program implemented in Chicago in 2006. The program combined lender oversight and counseling of high-risk borrowers, with both aspects of the program being administered through third-party counseling agencies. The pilot program's 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 mortgage applicants responded to (dis)incentives of compliance costs of counseling, but were less influenced by information provided by counselors. Borrowers often altered their contract choice to minimize interaction with counselors. Specifically, those borrowers who could eschew counseling by choosing less risky products did so, decreasing their propensity to choose such products by between 10 and 40 percent. However, those who went through a counseling session did not appear, on average, to follow the counselor's advice, and seemed to have only limited ability to renegotiate their mortgage offers. They tended not to walk away from the original offer following counseling, nor to reapply for a different mortgage; either of which would have required another counseling session.

Second, the legislation had material effects on the market composition of both lenders and borrowers, as well as on borrower and lender behavior. Incentives do matter. As indicated above, borrowers adjusted their borrowing to avoid having to be subject to counseling. Similarly, as shown in our earlier work (Agarwal et al. 2014) lenders with predatory characteristics exited the market to avoid the potential

provide information and educational programs to financial consumers, assist borrowers during the mortgage application process, and consider the potential benefits of counseling in the mortgage application process—for example, see Consumer Financial Protection Bureau (2013). For a discussion of the Dodd–Frank Act and the role of the new Bureau, see Evanoff and Moeller (2014).

scrutiny from both regulators and the marketplace. This finding is reinforced by the fact that lenders returned relatively quickly to the market once the counseling requirement was rescinded.

The unique design of HB 4050 allows us to disentangle various effects of the pilot on borrower choice. However, particularities of the program also raise questions of the external validity of our findings. While Chicago does not stand out among cities that experienced the boom-and-bust in house prices, all of the pilot treatment areas (and all of the control samples) represent low-income urban areas. As such, their experiences with financial counseling and their mortgage access channels are not representative of the U.S. as a whole. Although at a first glance HB 4050 had direct jurisdiction only over state-licensed lenders, the true reach of the pilot extended much further. As we discuss in Section I.A, the mandate applied to correspondent loans as well. That is, a loan originated by a mortgage broker and sold to a national bank still had to comply with the counseling mandate. This lessens some of the concerns about broad applicability of the program in the treated area. Finally, some of our analysis uses the sample of active lenders to identify effects unrelated to changes in lender composition. In our interpretation of the data, the active lenders tend to be larger and less likely to engage in questionable business practices. In this sense, they are not entirely representative of the lender mix in low-income urban areas. For this reason, we always couple the active sample results with those of the entire lender population.

Our study also highlights several design elements of the program that likely affected its effectiveness by limiting borrower options to renegotiate their mortgage offers. For instance, not offering bright-line "safe harbor" provisions for lender compliance likely amplified lender exit from the pilot areas, leaving borrowers with fewer choices. Allowing counseling sessions to take place just a few days prior to a scheduled closing also eliminated options to renegotiate without disrupting the transaction. Finally, having a one-off counseling session late in the mortgage

origination process may not have been sufficient to put borrowers in a position to negotiate effectively with savvy lenders.

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TABLE 1 — SUMMARY STATISTICS

The table compares key characteristics of the treated and control samples. Panels A and B focus on socioeconomic and mortgage market metrics in 2005, which would have been available to the Illinois regulators determining HB 4050 pilot area boundaries. These panels contrast the 10 HB 4050 zip code areas with the rest of the City of Chicago, as well as with the 12 similar control zip codes whose construction is described in Section II.B. Panel C compares mortgage market metrics for the zip-code based treatment and control samples, as well as for the dynamically matched synthetic control sample, described in Section II.C. Panel C provides summaries of both the pre-treatment (columns 1-3) and treatment periods (columns 4-6). The panel lists means of the key variables in each of the samples and it reports the p-values of two-tailed t-tests of the difference in sample means. The p-values next to columns (2) and (5) show the significance of differences in means of the HB4050 sample and the zip-code-based control sample. The p-values next to columns (3) and (6) show the significance of differences in means of the HB4050 sample and the matched synthetic control sample.

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

	(1) HB 4050 zip codes (10 zip codes)	(2) Control ZIP codes (12 zip codes)	(3) all non-HB4050 Chicago zip codes (43 zip codes)
Total population	729,743	709,549	2,181,445
Total number of 2005 tax returns	259,884	249,968	886,723
Share of minorities*	0.726	0.731	0.535
Share of individuals below poverty level*	0.200	0.228	0.190
Average taxable income (AGI) in 2005#	$31,579	$32,065	$56,404
Share of households with AGI < $50,000 in 2005	0.823	0.825	0.721
Unemployment rate (2000 Census)*	0.136	0.133	0.092

* 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=14,286)	Control ZIP codes (n=11,137)	all non-HB4050 Chicago zip codes (n=26,559)
	(1)	(2)	(3)
Share defaulting within 18 months (x 100)	14.69	14.46	10.67
FICO	634.11	636.71	656.35
LTV (%)	84.28	83.38	81.99
log(Valuation)	12.15	12.27	12.54
Income ($K)	68.24	74.70	101.31

Note: Based on CoreLogic LP data, authors calculations. For more information, see text.

TABLE 1 — SUMMARY STATISTICS (CONT.)

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

	(1)	(2)		(3)		(4)	(5)		(6)	
	pre-HB4050: 1/2005-8/2006					HB4050: 9/2006-1/2007				
	HB 4050 Zip Codes	Geo Control Sample		Matched Sample		HB 4050 Zip Codes	Geo Control Sample		Matched Sample	
	n = 22,823	n = 18,158		n = 21,929		n = 2,531	n = 3,760		n = 2,493	
	mean	mean	p-value	mean	p-value		mean	p-value	mean	p-value
Share defaulting within 18 months (x 100)	18.15	18.15	0.998	15.27	0.000	23.63	29.44	0.000	25.11	0.221
Share defaulting within 36 months (x 100)	34.89	33.27	0.001	28.77	0.000	52.71	56.44	0.004	50.46	0.112
Fraction of low-FICO Borrowers	39.04	38.23	0.094	38.70	0.462	29.88	34.70	0.000	29.80	0.955
Fraction of mid-FICO Borrowers	21.71	20.60	0.007	21.72	0.983	22.29	20.31	0.061	22.30	0.990
Fraction of high-FICO Borrowers	39.26	41.17	0.000	39.59	0.475	47.83	44.99	0.027	47.89	0.966
FICO score	635.79	638.05	0.000	636.84	0.070	648.50	642.87	0.000	647.66	0.623
Share of Risky Products: Only Mid-FICO	83.40	84.19	0.032	84.03	0.075	79.93	82.69	0.006	81.75	0.101
Share of Risky Products: Mid and High-FICO	18.93	19.54	0.119	18.41	0.159	12.88	15.82	0.001	17.25	0.000
Share of low-documentation loans	47.50	49.10	0.001	47.91	0.382	49.41	52.59	0.013	53.40	0.005
Loan Margin (%)	4.46	4.46	0.020	4.42	0.393	4.03	4.31	0.000	4.17	0.238
Annual Mortgage Payment/Loan Size (%)	9.01	8.92	0.902	8.94	0.029	8.94	9.32	0.000	9.45	0.003
Loan-to-Value Ratio at Origination (%)	84.20	83.35	0.000	84.16	0.759	83.90	83.62	0.493	83.78	0.776
Debt-Service-to-Income (%)	21.06	21.79	0.000	20.36	0.171	22.99	23.19	0.191	21.38	0.253
log(House Value ($))	12.18	12.30	0.000	12.23	0.000	12.30	12.38	0.000	12.29	0.462

Note: Based on CoreLogic LP data, authors calculations. For more information, see text.

TABLE 2 — MORTGAGE PRODUCT CHOICE

The table examines the effects of counseling on mortgage contract choice. The table reports results using OLS regressions to test for changes in choice of contracts deemed risky by HB 4050. Risky Products: Only Mid-FICO Counseled refers to mortgages that subject only the mid-FICO borrowers to counseling (hybrid ARMs, low-doc and interest-only loans, and properties refinanced within 12 months). Risky Products Mid and High-FICO Counseled denotes contracts that trigger counseling for both mid- and high-FICO borrowers (loans with prepayment penalties, negative amortization or high closing costs). Low-FICO borrowers are subject to counseling irrespective of their contract choice. 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 and are reported in parentheses.

	I(Risky Products: Only Mid-FICO Counseled) x 100				I(Risky Products: Mid- and High-FICO Counseled) x 100			
	All lenders		Active lenders		All lenders		Active lenders	
	Control	Matched	Control	Matched	Control	Matched	Control	Matched
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HB 4050 x Low FICO	-3.71	-2.47	-0.48	0.36	-0.58	-3.33	-2.10	-4.57
	(1.79)	(1.74)	(2.44)	(2.56)	(2.07)	(2.18)	(2.12)	(2.37)
HB 4050 x Mid FICO	-7.87	-6.37	-8.55	-7.49	0.37	-2.15	-0.07	-2.30
	(1.81)	(1.77)	(2.51)	(2.70)	(1.57)	(1.59)	(1.56)	(1.99)
HB 4050 x High FICO	0.42	1.65	1.08	2.02	-4.51	-7.08	-4.09	-6.44
	(1.14)	(1.13)	(1.88)	(2.06)	(1.10)	(1.16)	(1.64)	(2.01)
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	49,095	52,622	19,142	20,684	49,095	52,622	19,142	20,684
Adj. R^2	0.052	0.048	0.049	0.044	0.040	0.040	0.050	0.048
Dependent variable means for the control group during pilot:								
Low FICO borrowers	76.3	74.0	77.1	76.5	13.8	16.6	10.8	14.1
Mid FICO borrowers	80.5	78.8	81.7	78.9	18.3	17.4	14.2	16.3
High FICO borrowers	88.7	87.9	88.5	87.4	16.4	17.6	14.3	15.9

Note: Based on CoreLogic LP data, authors calculations. For more information, see text.

TABLE 3 — MORTGAGE PRODUCT CHOICE: ROBUSTNESS CHECKS

The table examines the robustness of estimated effects of counseling on mortgage contract choice presented in Table 2. The regression setup and variable definitions in this table are identical to those in Table 2 with the exception of sample choice. Columns (2) and (6) restrict the analysis to refinancing transactions, while columns (3) and (7) remove the month immediately preceding the implementation of HB 4050. The sample in columns (4) and (8) excludes zip codes in Speaker Madigan's legislative district. In all specifications the control sample is set to the 12 HB 4050-like zip codes and the lender universe is restricted to entities that remained active throughout the pilot period. This corresponds to the "Control Active" sample in Table 2, with columns (3) and (7) from that table listed as baseline results here as columns (1) and (6), respectively. Columns (5) and (10) correspond to baseline specifications with an addition of zip-level linear time trends. 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 and are reported in parentheses.

	I(Risky Products: Only Mid-FICO Counseled) x 100					I(Risky Products: Mid- and High-FICO Counseled) x 100				
	Baseline	Refi Only	Drop August 2006	Drop Speaker ZIP codes	Zip time trends	Baseline	Refi Only	Drop August 2006	Drop Speaker ZIP codes	Zip time trends
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
HB 4050 x Low FICO	-0.48	0.47	-0.58	-2.20	0.40	-2.10	-4.08	-2.17	0.56	-3.08
	(2.44)	(2.59)	(2.45)	(2.82)	(2.69)	(2.12)	(2.78)	(2.04)	(2.65)	(2.39)
HB 4050 x Mid FICO	-8.55	-6.14	-8.59	-9.36	-7.64	-0.07	-1.72	-0.11	-0.20	-0.85
	(2.51)	(2.97)	(2.53)	(2.52)	(2.30)	(1.56)	(2.85)	(1.58)	(1.70)	(1.44)
HB 4050 x High FICO	1.08	-0.15	1.09	0.25	2.05	-4.09	-7.69	-4.14	-5.21	-4.89
	(1.88)	(3.21)	(1.86)	(1.50)	(1.78)	(1.64)	(3.63)	(1.66)	(1.87)	(1.92)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract Terms Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property Type Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE, Zip Code FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE * log(Avg Inc)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,142	10,382	18,241	14,428	19,142	19,142	10,382	18,241	14,428	19,142
Adj. R^2	0.049	0.058	0.048	0.049	0.049	0.05	0.029	0.05	0.05	0.051
Dependent variable means for the control group during pilot:										
Low FICO borrowers	77.11	75.64	77.11	77.11	77.11	10.84	12.99	10.84	10.84	10.84
Mid FICO borrowers	81.74	76.63	81.74	81.74	81.74	14.20	21.20	14.20	14.20	14.20
High FICO borrowers	88.47	84.00	88.47	88.47	88.47	14.27	25.82	14.27	14.27	14.27

Note: Based on CoreLogic LP data, authors calculations. For more information, see text.

TABLE 4 — EFFECTS OF HB 4050 ON CREDIT SUPPLY

The table summarizes the effects of HB 4050 on the number of lenders and lender contract menu choice. Panel A presents counts if lenders that generated any loans during three distinct periods – before, during, and after HB 4050 – in the 10 zip code pilot area and the 12 zip code control area. Using the same time periods and the same treatment and control samples, Panel B shows the fraction of active lenders that offer mortgage contracts in each of the Risky Produts: Only Mid-FICO Counseled and Risky Products: Mid- and High-FICO Counseled groups, as well as their subgroups. A lender is considered to offer a particular mortgage contract if any of the mortgages it originated during a given period in a given geography contains the relevant contract features.

Panel A: Supply of Credit – Total Number of Active Lenders (Source: HMDA)

	Lenders with any activity in		HMDA loan origination volume	
	HB 4050	Control	HB 4050	Control
Before HB 4050 (9/05 - 8/06)	114	106	3,036	2,935
During HB 4050 (9/06 - 1/07)	65	88	1,992	2,822
After HB 4050 (2/07 - 6/07)	79	81	2,119	2,268

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

Share of lenders originating specific mortgage contracts in HB 4050 or Control zip codes

	Risky Products: Only Mid-FICO Counseled (HB4050 | Control)		Risky Products: Mid- and High-FICO Counseled (HB4050 | Control)	
	Low Doc	IO/Hybrid ARM	Option ARM	Pre-pmt penalty
Before HB 4050 (9/05 - 8/06)	1.00 | 1.00	1.00 | 1.00	0.65 | 0.69	0.96 | 0.92
During HB 4050 (9/06 - 1/07)	1.00 | 1.00	1.00 | 0.96	0.58 | 0.42	0.88 | 0.85
After HB 4050 (2/07 - 6/07)	0.85 | 0.92	0.85 | 0.88	0.35 | 0.38	0.69 | 0.69

Note: Based on CoreLogic LP data, authors calculations. For more information, see text.

TABLE 5 — AVAILABILITY OF LOW-DOC LOANS

The table examine the likelihood of taking low-documentation loans in HB 4050 treatment zip codes. 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 and are reported in parentheses.

	I(Low Doc) x 100			
	All lenders		Active lenders	
	Geo. Control	Matched	Geo. Control	Matched
	(1)	(2)	(3)	(4)
HB 4050 x Low FICO	-12.10	-12.14	-11.23	-11.18
	(2.00)	(1.86)	(2.27)	(2.64)
HB 4050 x Mid FICO	-6.87	-7.06	-8.23	-8.47
	(2.72)	(2.66)	(3.14)	(3.42)
HB 4050 x High FICO	1.90	1.71	3.08	2.92
	(1.43)	(1.36)	(1.48)	(1.89)
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	48,963	52,485	19,062	20,591
Adj. R^2	0.204	0.182	0.205	0.185
Dependent variable means for the control group during pilot:				
Low FICO borrowers	22.96	25.24	22.03	25.66
Mid FICO borrowers	51.06	44.77	51.74	44.72
High FICO borrowers	76.43	74.94	75.87	73.39

Note: Based on CoreLogic LP data, authors calculations. For more information, see text.

TABLE 6 — EFFECTS OF HB 4050 ON MORTGAGE LEVERAGE AND INTEREST RATE SPREAD

The table examines the effects of the mandate on key mortgage terms. Panel A focuses on whether leverage and loan margins are different for the population with mandatory counseling. Panel B examines proxies for mortgage affordability. All variables are defined in Section III.D. The set of controls not shown in the table includes borrower's FICO score, house value, 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 and are reported in parentheses.

Panel A: Key Mortgage Terms (Source: LoanPerformance)

	Loan-to-Value (%)		Loan Margin (bp)			
	Geo. Control	Geo. Control Active	Geo. Control	Matched	Geo. Control Active	Matched Active
	(1)	(2)	(3)	(4)	(5)	(6)
HB 4050 x Low FICO	-1.48	-2.54	-13.90	-14.98	-8.63	-5.51
	(0.40)	(0.71)	(8.14)	(8.21)	(9.75)	(9.87)
HB 4050 x Mid FICO	1.25	0.57	-15.38	-13.45	-8.01	-3.28
	(0.42)	(0.64)	(5.72)	(5.48)	(7.77)	(8.11)
HB 4050 x High FICO	-0.69	-1.56	-26.56	-22.40	-25.20	-17.41
	(0.40)	(0.70)	(6.75)	(7.31)	(9.76)	(10.14)
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	47,657	18,793	48,814	52,319	19,063	20,595
Adj. R^2	0.15	0.15	0.21	0.21	0.21	0.21
Dependent variable means for the control group during pilot:						
Low FICO borrowers	80.94	81.04	474.77	459.52	462.70	448.51
Mid FICO borrowers	84.90	86.27	447.61	444.57	451.99	437.43
High FICO borrowers	84.53	85.08	391.06	378.01	385.40	367.95

Note: Based on CoreLogic LP data, authors calculations. For more information, see text.

TABLE 6 — EFFECTS OF HB 4050 ON MORTGAGE LEVERAGE AND INTEREST RATE SPREAD (CONT.)

Panel B: Mortgage Affordability (Source: LoanPerformance)

	Debt-Service-to-Income (%)		Annual Mortgage Payment-to-Loan Amount (%)			
	Geo. Control	Geo. Control Active	Geo. Control	Matched	Geo. Control Active	Matched Active
	(1)	(2)	(3)	(4)	(5)	(6)
HB 4050 x Low FICO	2.28	1.51	-0.46	-0.72	0.01	0.01
	(1.19)	(0.63)	(0.32)	(0.49)	(0.06)	(0.07)
HB 4050 x Mid FICO	0.04	0.65	-0.40	-0.69	0.14	0.15
	(0.78)	(0.79)	(0.31)	(0.51)	(0.09)	(0.10)
HB 4050 x High FICO	-0.59	-0.14	-0.36	-0.60	0.12	0.18
	(0.71)	(0.62)	(0.31)	(0.50)	(0.06)	(0.08)
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	46,937	18,456	48,814	52,319	19,063	20,595
Adj. R^2	0.086	0.158	0.028	0.017	0.254	0.266
Dependent variable means for the control group during pilot:						
Low FICO borrowers	27.60	27.10	9.81	10.26	9.14	8.98
Mid FICO borrowers	23.35	23.79	8.95	8.97	8.92	8.90
High FICO borrowers	19.52	19.06	9.12	9.16	8.59	8.51

Note: Based on CoreLogic LP data, authors calculations. For more information, see text.

TABLE 7 — BORROWER EXTENSIVE MARGIN RESPONSES TO HB 4050

This table analyses whether borrowers in HB 4050 zip codes during the pilot period exhibited different .rejection rates of their mortgage offers and whether they were discouraged from applying. Columns (1) and (2) present loan-level OLS results of difference-in-differences specification for the likelihood of mortgage rejection, with standard errors clustered at the zip code level. Columns (3)-(6) present zip-level analysis of origination shares accounted for by low- and mid-FICO borrowers, respectively. Under the assumption that lender approval rates remained unaffected by the pilot, the share of loans originated to treated borrowers reflects application activity by such borrowers. These DID specifications also include a full set of time and zip code controls, and zip-month average values of incomes, home values, and mortgage amounts. Standard errors level are reported in parentheses.

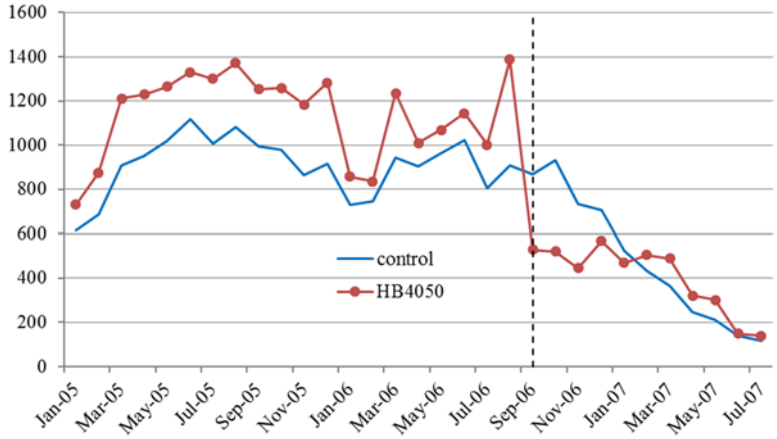
	Source: HMDA		Source: LoanPerformance			
	I(Applicant Rejects Offer) x 100		Share of low-FICO mortgages		Share of mid-FICO mortgages	
	Control	Control Active	Control	Control Active	Control	Control Active
	(1)	(2)	(3)	(4)	(5)	(6)
HB 4050	-1.36	-0.47	-5.48	-5.68	0.81	-1.01
	(0.28)	(0.36)	(1.29)	(2.05)	(1.46)	(2.14)
log(Mortgage)	-0.89	-0.94	5.91	-7.6	-4.76	1.4
	(0.09)	(0.09)	(5.81)	(9.79)	(4.61)	(8.18)
log(Income)	0.96	0.45	-8.19	-10.93	-0.94	-3.08
	(0.16)	(0.15)	(2.20)	(4.60)	(1.58)	(2.96)
log(Home Value)			-14.51	-2.37	8.72	-5.6
			(7.84)	(13.43)	(6.23)	(12.38)
Month FE, Zip Code FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE x log(income)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	445,251	292,023	742	717	742	717
Adj. R^2	0.004	0.003	0.655	0.468	0.093	0.073
Dependent variable means for the control group during pilot:						
	9.48	7.22	35.2	37.6	20.1	21.7

Note: Based on CoreLogic LP data and non-public HMDA daat, authors calculations. For more information, see text.

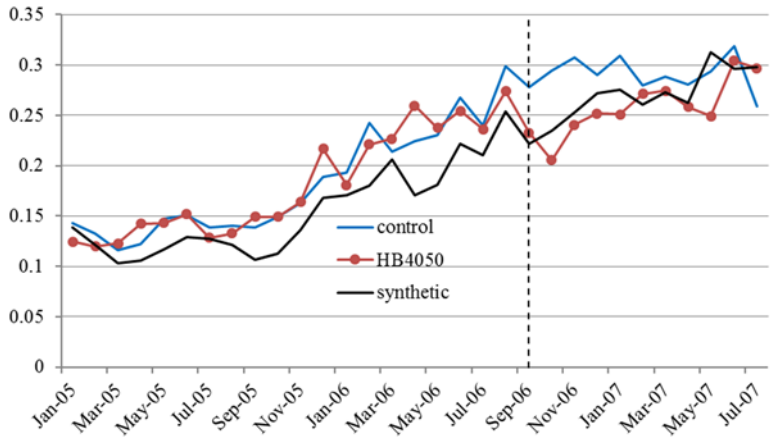
FIGURE 1. PRE-PILOT TIME TRENDS IN ORIGINATIONS, DEFAULT RATES, AND CONTRACT CHOICES

This figure depicts time series of mortgage originations from HMDA, realized 18-month default rates and shares of "risky" (Category II, as defined in Section III.A) mortgage contracts between January 2005 and July 2007 for each of the three samples described in text. The dashed vertical line denotes the onset of HB 4050 pilot period, which ended in January 2007.

Panel A: Mortgage Origination Volumes



Panel B: Fraction of mortgages defaulting within 18 months of origination



Panel C: Share of "risky" mortgage contracts

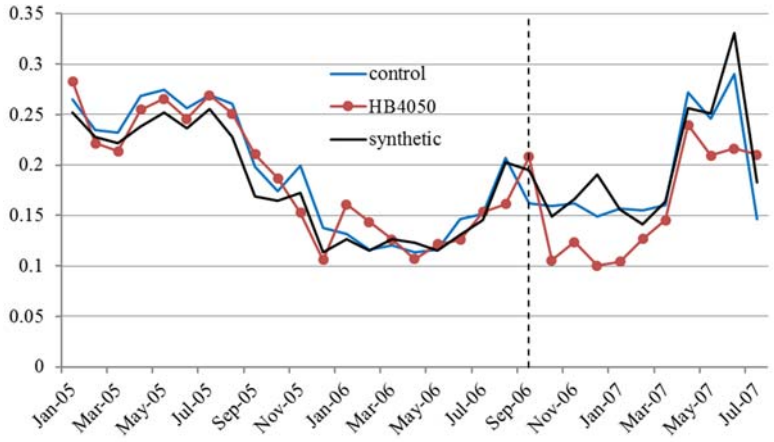

0.35
0.3
0.25
0.2
0.15
0.1
0.05
0
control
HB4050
synthetic
Jan-05
Mar-05
May-05
Jul-05
Sep-05
Nov-05
Jan-06
Mar-06
May-06
Jul-06
Sep-06
Nov-06
Jan-07
Mar-07
May-07
Jul-07

FIGURE 2. CUMULATIVE DEFAULT RATES FOR HB 4050 PERIOD ORIGINATIONS, BY TREATMENT GROUP

This figure depicts cumulative default rates for mortgages originated during the HB 4050 pilot period: September 2006 through January 2007. The control group is comprised of mortgages originated in the 12-zip code geographic control area. Mortgages originated in the HB 4050 zip codes are subdivided into two groups. Treated/counseled mortgages are those that received counseling prior to origination. Treated/avoided counseling are those that were exempt from counseling by virtue of their sufficiently high FICO scores *and* contract choice. A mortgage of age *n* is considered to be in default if it experienced a 90+ day delinquency or foreclosure at any point during its first *n* months after origination.

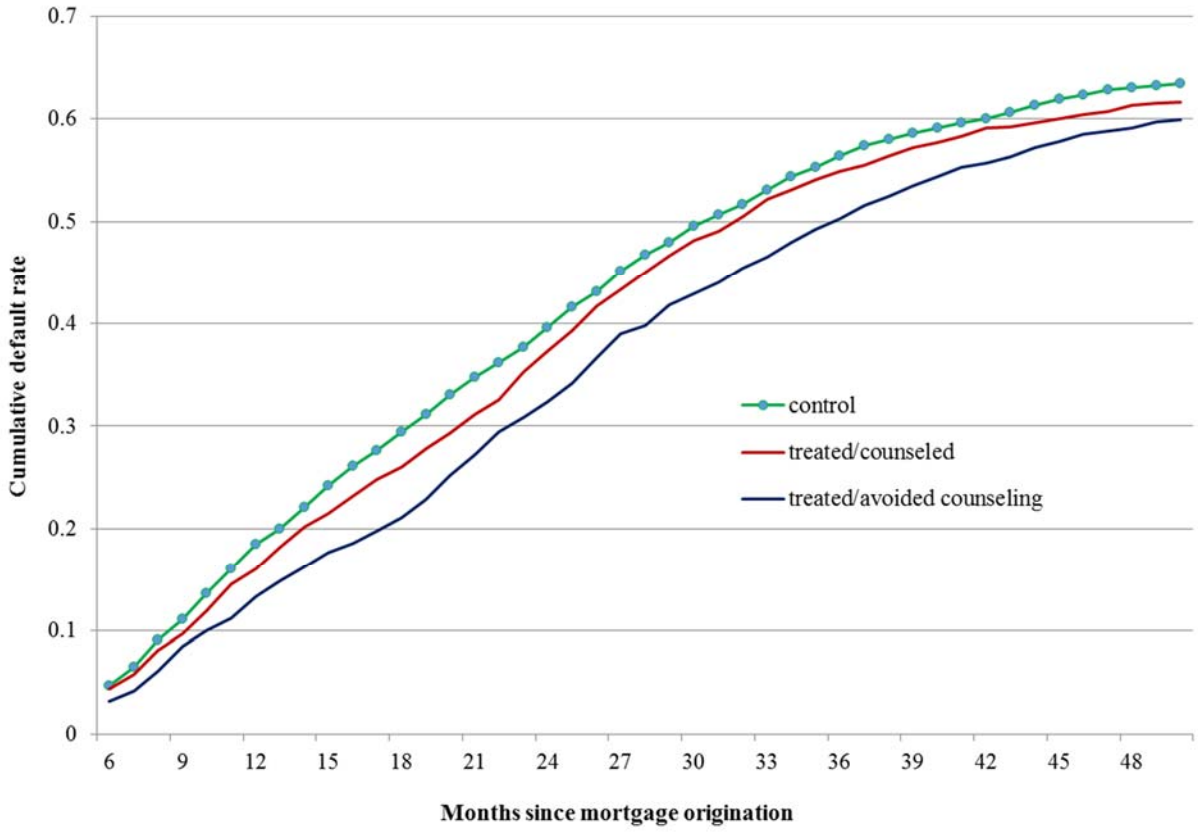
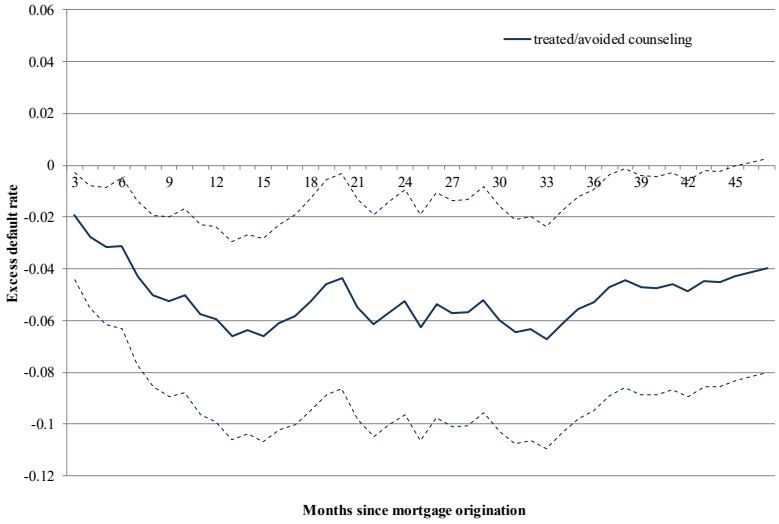


FIGURE 3. ADJUSTED DEFAULT RATES FOR HB 4050 PERIOD ORIGINATIONS, BY TREATMENT GROUP

This figure depicts cumulative default rates for mortgages originated during the HB 4050 pilot period adjusted to remove the influence of observable factors. The adjustment process is described in Section III.E. Panel A presents results for the counseled borrowers, while Panel B shows excess cumulative default rates for those borrowers that avoided counseling through their choice of mortgage contract. The two standard deviation bands for each of the treated groups are depicted by dashed lines.

Panel A. Counseled borrowers

Panel B. Borrowers that avoided counseling



APPENDIX TABLE 1 — BOOTSTRAPPED STANDARD ERRORS

The table replicates the results in Table 2 for specifications based on the geographic control sample. Since this sample only includes 22 zip codes, we want to test whether clustering standard errors at the zip code level is robust to alternative econometric approaches. In particular, we bootstrap standard errors using the procedure specified in Cameron, Gelbach, and Miller (2008). This procedure is implemented via Stata 14 *boottest* command using the wild cluster resampling approach and Rademacher weights. The table below reports confidence intervals using zip-level clustered errors in the first line and bootstrapped standard errors in the second line.

	I(Risky Products: Only Mid-FICO Counseled) x 100		I(Risky Products: Mid- and High-FICO Counseled) x 100	
	All lenders Control	Active lenders Control	All lenders Control	Active lenders Control
	(1)	(3)	(5)	(7)
HB 4050 x Low FICO	-3.71	-0.48	-0.58	-2.10
confidence interval	[-7.44 0.02]	[-5.56 4.60]	[-4.88 4.60]	[-6.51 4.60]
bootstrapped CI	[-7.82 0.98]	[-6.40 5.14]	[-5.54 4.08]	[-6.39 3.61]
HB 4050 x Mid FICO	-7.87	-8.55	0.37	-0.07
confidence interval	[-11.63 -4.11]	[-13.77 -3.33]	[-2.90 3.63]	[-3.30 3.17]
bootstrapped CI	[-12.06 -3.78]	[-14.02 -2.73]	[-4.22 3.50]	[-4.15 3.13]
HB 4050 x High FICO	0.42	1.08	-4.51	-4.09
confidence interval	[-1.95 2.80]	[-2.83 4.99]	[-6.81 -2.21]	[-7.51 -0.67]
bootstrapped CI	[-2.40 2.79]	[-3.18 5.38]	[-6.99 -2.25]	[-8.29 -0.50]
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	49,095	19,142	49,095	19,142
Adj. R^2	0.052	0.049	0.040	0.050

Note: Based on CoreLogic LP data, authors calculations. For more information, see text.