Did Affordable Housing Legislation Contribute to the Subprime Securities Boom?*

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Abstract

No, not directly. We use a regression discontinuity approach and present new institutional evidence to investigate whether affordable housing policies influenced the market for securitized subprime mortgages. We use merged loan-level data on non-prime mortgages with individual- and neighborhood-level data for California and Florida. We find no evidence that lenders increased subprime originations or altered pricing around the discrete eligibility cutoffs for the Government Sponsored Enterprises' (GSEs) affordable housing goals or the Community Reinvestment Act. Although we find evidence that the GSEs bought significant quantities of subprime securities, our results indicate that these purchases were not directly related to their affordable housing mandates.

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1 Introduction

It is widely agreed upon that the volume of subprime mortgages grew dramatically in the years immediately preceding the financial crisis of 2007-2008. The most novel sector of the residential mortgage market consisted primarily of private-label mortgage-backed securities (PLMBS). The nonprime PLMBS market barely existed before 2001 but grew by about 900% from 2001 and 2005 (see Demyanyk and Van Hemert, 2011). These mortgages were not intended to be purchased by the GSEs as whole loans to later be packaged into agency MBS. Rather, the GSEs gained exposure to these mortgages through their purchases of PLMBS.

What caused the subprime securities boom? The boom, and its subsequent implosion, had major consequences for the financial sector and the macroeconomy. Understanding its causes is crucial to preventing future financial crises and understanding what drives asset booms.

Some observers have argued that affordable housing policy was a causal factor in the subprime crisis. For instance, writing in the *Financial Times*, Raghuram Rajan (2010) writes "[t]he tsunami of money directed by a U.S. Congress, worried about growing income inequality, towards expanding low income housing, joined with the flood of foreign capital inflows to remove any discipline on home loans." When asked about the cause of the financial crisis, Eugene Fama states that "the global crisis was first a problem of political pressure to encourage the financing of subprime mortgages" (Fama and Litterman, 2012). Greenspan (2010) also asserts that affordable housing policies played a key role in the subprime crisis. In fall 2011, Michael Bloomberg, the mayor of New York, stated that

> *It was not the banks that created the mortgage crisis. It was, plain and simple, Congress who forced everybody to go and give mortgages to people who were on the cusp. [. . .] But they were the ones who pushed Fannie and Freddie to make a bunch of loans that were imprudent, if you will. They were the ones that pushed the banks to loan to everybody. And now we want to go vilify the banks because it's one target, it's easy to blame them and congress certainly isn't going to blame themselves.*
>
> - In Paybarah, 2011

In this paper, we use data on non-prime mortgages originated in 2004 through 2006 in California and Florida to examine the influence of affordable housing policies on subprime loan pricing and

the volume of originations. All mortgages in our sample were securitized into PLMBS. We show that almost 70% of such mortgages satisfied one or more of the affordable housing goals. We also use data from a random sample of 100 prospectus supplements to show that the fact that so many of the mortgages satisfied one or more housing goals was not by design. The prospectuses also reveal that the GSEs were major customers of PLMBS**.** Our review of the sample of prospectus supplements provides us with a rough estimate, 25%, of the GSEs' share of the market for the senior tranches of PLMBS deals.

To identify the effect of affordable housing goals, we use a regression discontinuity approach to ascertain whether the goals led to a difference in subprime loan volume, subprime interest rates, or default on subprime mortgages. We look at the effects of the two main affordable housing policies enacted by Congress. The first policy we examine is the Community Reinvestment Act (CRA). As we show, the majority of loans securitized into subprime securities were originated by non-depository institutions that are not subject to the CRA. However, depository institutions may also count PLMBS toward their CRA goals provided the MBS are structured as CRA-qualified securities. Our regression discontinuity approach enables us to identify demand channels that do not rely on the institutional type of the originator.

The second policy we examine is the mandate of the two main GSEs to promote affordable housing. Importantly, the GSEs can satisfy their affordable housing goals by purchasing packages of securitized mortgages that they cannot purchase as whole loans. Indeed, the GSEs vastly increased their purchases of PLMBS during the subprime mortgage boom. Manchester (2008) and Frame (2008) show that the GSEs generally purchased "goal rich" PLMBS during the subprime boom. Each loan may count towards more than one goal such that a goal that easily satisfies one goal may be close to the threshold for satisfying a different goal.

We find no evidence that affordable housing legislation affected the subprime market during the subprime crisis. Lending volumes, loan pricing, and default rates do not change in response to the goals. It remains plausible that the GSEs encouraged subprime lending by purchasing large quantities of PLMBS. However, our results indicate that any role the GSEs played in the subprime crisis was not due to their affordable housing mandates.

Finally, in our data we find that stated borrower income is twice as high as the income of the census tract. The average borrower in our sample reports an income of over $100,000 such that very

few subprime mortgages qualified for the borrower-level affordable housing goals. The discrepancy between stated borrower income and the income of the census tract also indicates that borrower income is much more likely to have been falsified upwards than downwards.[1] In contrast, if the borrower-level goals were binding, borrower income should have been understated.

Since the majority of subprime loans were securitized such that the effect of affordable housing legislation requires an evaluation of the effect of affordable housing policies on securitized loans. Some of the previous literature has focused on mortgages the GSEs and CRA-regulated institutions were likely to acquire as whole loans. For example, Avery and Brevoort's (2011), Reid and Laderman's (2011), and Agarwal, Benmelech, Bergman, and Seru's (2012) identifying strategies focus on differences in loan originators rather than the final holder of the loan. In contrast, our approach to identifying the effect of the CRA uses a regression discontinuity approach. We use this approach because, although the institutional evidence we uncover reveals that few if any PLMBS are CRA-qualified, CRA-regulated lenders may in theory get credit for CRA-eligible loans they buy on the secondary market; our approach thus does not assume that the lender gets credit only for loans it originates.

Existing literature on the effect of affordable housing on mortgage markets has not convinced the proponents of the view that affordable housing legislation caused the crisis. While our paper uses a methodology similar to that of Bhutta (2011, 2012), we address three important issues that previous literature, including Bhutta (2011, 2012), does not address. First, a key advantage of our matched data set is that we are able to examine the effect of all of the GSEs' affordable housing goals and both of the CRA goals. While some of the GSEs' affordable housing goals and one of the CRA goals are aimed at encouraging lending to households living in particular Census tracts, the aim of several other targets is to encourage lending to households with low incomes. To study these goals we look at pricing and loan performance measured at the loan level. The previous literature, including Bhutta, has focused on studying neighborhood-level outcome measures, such as originations per tract. However, the GSEs must meet both the tract and borrower level goals. Similarly, depository institutions must satisfy both the tract-specific and the borrower-specific components of the CRA.

[1]See Jiang, Nelson, and Vytlacil (2011) for similar evidence that borrowers' reported financial conditions were biased upwards rather than downwards.

Second, our matched data enables us to focus specifically on mortgages packaged into nonprime securities, which were at the heart of the financial crisis. In contrast, most of the existing literature has studied the effects of affordable housing policy on the overall mortgage market (e.g., Ambrose and Thibodeau, 2004; Bostic and Gabriel, 2006) or on the prime mortgage market. Bhutta's main analysis excludes loans from originators that the department of Housing and Urban Development (HUD) formerly designated as subprime lenders.[2] An exception is Reid and Laderman (2011) who study whether CRA-regulated institutions are more likely to originate high-cost loans than institutions not covered by the CRA. Consistent with Reid and Laderman's findings, we find that the majority of loans that were securitized in nonprime pools were originated by non-depository institutions that are not subject to the CRA. We find that only about half of our subprime loans meet the HMDA definition of a high cost loan, however.

Third, as the regression discontinuity approach identifies only local average treatment effects, we also adduce new relevant institutional details. The institutional evidence indicates that it is highly unlikely that institutions were satisfying their affordable housing goals in the PLMBS market. In a random sample of 100 prospectus supplements for nonprime PLMBS that we examine, not a single prospectus ever mentions the GSEs' affordable housing goals or the CRA despite discussing at length numerous other characteristics of the loans in the pools. Put differently, none of the pools we examine were CRA-qualified. This finding is particularly strong evidence that the CRA did not affect the market since depository institutions can only get credit for purchases of PLMBS that are specifically structured as CRA-qualified. Furthermore, the majority of loans securitized in PLMBS were originated by non-depository institutions that were not subject to the CRA indicating the presence of substantial incentives to originate such loans by institutions that were not subject to the CRA.

Our paper contributes to a growing literature exploring the causes of the housing boom. Favilukis, Kohn, Ludvigson, and Van Nieuwerburgh (2012) ask whether international capital inflows can explain the run-up in home prices. Ashcraft, Goldsmith-Pinkham, and Vickery (2010) and Ashcraft, Goldsmith-Pinkham, Hull, and Vickery (2011) examine the role of the credit ratings agencies. Foote, Gerardi, and Willen (2012) study the role of financial innovation in mortgage markets. Favara and Imbs (2011), Kiyotaki, Michaelides, and Nikolov (2011), Landvoigt, Piazzesi,

[2]HUD discontinued this list out of accuracy concerns.

and Schneider (2011), Adelino, Schoar, and Severino (2012), and Glaeser, Gottlieb, and Gyourko (2012) explore the influence of cheap credit and relaxing credit constraints on home prices. Campbell, Davis, Gallin, and Martin (2009) examine the effect of monetary policy on the rent-price ratio. Cociuba, Shukayev, and Ueberfeldt (2012) examine the role of monetary policy in shifting investors' preferences towards riskier assets. Finally, Piazzesi and Schneider (2009) and Burnside, Eichenbaum, and Rebelo (2011) study the effect of optimistic beliefs about the housing market.

In the next section, we outline the affordable housing legislation we study and describe our empirical methodology. We describe the data and the algorithm used to merge them in Section 3. We present our regression discontinuity results in Section 4. Section 5 provides some relevant institutional details and a discussion. We provide concluding remarks in Section 6.

2 Empirical Methodology

To assess whether affordable housing legislation led directly or indirectly to the subprime housing boom, we must first examine the mechanisms through which the change in laws could affect lending behavior. We investigate whether the enactment of these laws led to a change in lender behavior to meet the programs' objectives. For example, changes in lending behavior could manifest as a relaxation in lending standards or a change in mortgage pricing. In this section, we outline the program objectives. We then describe three channels through which lenders could respond to the programs' objectives, thereby inducing a boom in subprime securities. We then test whether lender behavior did indeed change for these variables just below the programs' cutoffs.

2.1 The Affordable Housing Goals

The CRA was enacted in 1977 and was strengthened numerous times throughout its history. During our sample period, the policy was enforced by four separate regulators: the Federal Deposit Insurance Corporation, the Federal Reserve, the Office of the Comptroller of the Currency, and the Office of Thrift Supervision. The act encourages depository institutions to lend to low-income communities and to low-income individuals. While the CRA does not have an explicit racial component, the high correlation between the racial and income characteristics of neighborhoods and individuals implies that the CRA indirectly addresses concerns about racial disparities in credit

access.[3] The regulations regarding CRA compliance stipulate that some qualifying loans in a MBS that a depository institution acquires may be used to fulfill the goal (Office of the Comptroller of the Currency et al., 1997). In particular, MBS structured specifically to help an institutional MBS purchaser meet the CRA goals will generally count toward fulfilling the requirement. Importantly, the regression discontinuity approach allows us to capture the effect of affordable housing legislation on securitized loans.

Compliance with the CRA is accomplished by regularly scheduled evaluations. Depending on the size of the institution, regulators evaluate the institution either every two years or every five years. Institutions know the year in which their exam will occur although they may not know in exactly which quarter until the year of the exam. To our knowledge, the only event that may allow a bank to delay its regular CRA evaluation is a merger. The data used to evaluate each institution is all the institution's lending since the previous exam up until anywhere from a few days to three quarters before the exam. For example, Piedmont Federal Savings Bank is regulated as an intermediate small savings bank such that it must undergo evaluation every two years. Its most recent evaluation date was April 25th, 2011. The lending that the Office of Thrift Supervision considered for Piedmont Federal Savings Bank in the April 2011 evaluation was all lending from January 1, 2008 through December 31st, 2010 because Piedmont Federal Savings Bank's previous exam occurred in December 2008. Large banks are evaluated every two years and all of their lending data for the evaluation period is used during the evaluation period. The smallest banks are evaluated every five years and, for such institutions regulators sometimes choose to examine only a subsample of two to three years of lending data, rather than all lending over the full five year evaluation period.

Importantly, CRA evaluations explicitly consider the proportion of lending to both low income households as well as to low income neighborhoods. A loan may be used to meet more than one goal such that there is a greater benefit from a loan that meets two goals than one that meets only one goal. The lending component is only one aspect of the CRA evaluation. Regulators also consider any charitable contributions that the institution makes to its community and any community service its executives participate in.

[3]For a review of the literature on race, redlining, and mortgage lending, see Ross and Yinger (2002). More recent contributions to this literature include Haughwout, Mayer, and Tracy (2009) and Ghent, Hernández-Murillo, and Owyang (2012).

Since 1992, Congress has also given Fannie Mae and Freddie Mac numerical targets for the share of their lending to areas with large shares of minority residents or large shares of low-income households (the underserved areas goal [UAG]), borrowers with very low income or borrowers with low income living in Census tracts with low income (the special affordable goal [SAG]), and borrowers with low-to-moderate income (the low-to-moderate-income goal [LMIG]). Congress provides the GSEs with annual targets for their share of lending that meets the criteria of the UAG, SAG, and LMIG goals. A loan may be used to meet more than one goal such that there is a greater benefit from a loan that meets two goals than one that only meets one goal. The thresholds for each of the goals are defined by the Housing Enterprises Financial Safety and Soundness Act of 1992 (the 1992 GSE Act). Part 81.16 of Title 24 of the Code of Federal Regulations makes it clear that a qualifying loan acquired by a GSE via a purchase of PLMBS will generally count towards the GSE's affordable housing goals.

The affordable housing goals for the CRA and the GSEs are actually seven separate goals. Two of the goals are CRA goals and five are the GSEs' affordable housing targets. Some of the goals apply to borrowers living within a particular Census tract and some of the goals are specific to individual borrowers regardless of where they live. The loans that satisfy each of the goals are as follows:

1. CRA1: Loans to borrowers living in Census tracts with median tract to metropolitan statistical area (MSA) income of 80% or less.

2. CRA2: Loans to borrowers with incomes of 80% or less of the median MSA income.

3. UAG1: Loans to borrowers living in Census tracts with a minority population of 30% or more and median tract to MSA income of 120% or less.

4. UAG2: Loans to borrowers living in Census tracts with median tract to MSA income of 90% or less.

5. SAG1: Loans to borrowers with incomes of 60% or less of the median MSA income.

6. SAG2: Loans to borrowers with incomes of 80% or less of the median MSA income and who live in Census tracts with median tract to MSA income of 80% or less.

7. LMIG: Loans to borrowers with incomes of 100% or less of the median MSA income.

Institutions must meet both the borrower-specific and tract-specific goals. That is, the GSEs are given specific targets for each of the three goal areas (UAG, SAG, and LMIG) and depository institutions must satisfy both CRA1 and CRA2. As such, institutions cannot satisfy their goals solely by making loans to high income households that live in low-income neighborhoods.

None of our goal thresholds coincide with the major affordable rental program in the United States, the low income housing tax credit (LIHTC). See Baum-Snow and Marion (2009) for a discussion of the LIHTC. The CRA1 limit coincides with the moderate income definition for the community development block grant (CDBG) program of HUD. The CDBG program provides funds for a diverse set of community development projects such as public infrastructure, rehabilitating dilapidated homes, parks, homeless facilities, programmes for battered spouses, employment training, and other services for low income communities. The funding amounts are not discretely determined by a goal threshold but, rather, are allocated "using a formula comprised of several measures of community need, including the extent of poverty, population, housing overcrowding, age of housing, and population growth lag in relationship to other metropolitan areas" (HUD, 2012). Furthermore, although the amount of funding each state and city receives depends on the portion of its population that is moderate income, the organizational unit that receives the funds is not a census tract but rather a state, county, or municipality. The program is also not related to funding for home ownership. It is thus highly unlikely that it affects our identification strategy below.

2.2 Identifying the Effect of Affordable Housing Legislation

One direct way to determine whether affordable housing legislation contributed to the subprime securities boom is to measure the extent to which the laws led to more originations for the targeted groups than for other groups. For the tract-specific goals (CRA1, UAG1, and UAG2), we test whether there is a statistically significant increase in originations per Census tract divided by tract population just below versus just above the program cutoff. In this case, the dependent variable is the number of originations, a tract-level rather than a borrower-level variable. An increase in the number of originations would suggest that lenders made a conscious attempt to make loans to borrowers in the target group, which could have led to the subprime securities boom.

Another channel through which the programs could have encouraged lending is by inducing lenders to lower prices for the target groups. For all goals, we can test whether there is a discontinuity in the interest rate the borrower receives just above versus just below the program cutoff. Thus, the dependent variable in these tests is the mortgage rate charged at origination.

A third channel through which the programs could have encouraged lending is by relaxing lending standards, that is, by lending to borrowers targeted by the program who have an unusually high probability of defaulting on the loan. To explore this possibility, we can examine whether the programs affected the probability of default by the target group of borrowers. Thus, the dependent variable for these tests is a binary indicator of whether the borrower had a serious default within the first two years of origination. We follow the industry standard in defining a serious default as delinquency of 90 days or more or termination through foreclosure.

2.3 Regression Discontinuity Design

We can evaluate the affordable housing programs by estimating their effect on the variables in the preceding subsection using a regression discontinuity approach (Thistlethwaite and Campbell, 1960) which takes advantage of the precise cutoffs in the objectives of the affordable housing programs. The regression discontinuity approach has been used widely in economics and finance to improve identification of a "treatment" on a variable of interest, Y. Suppose that Y changes smoothly with an observable variable, X, and the treatment, affordable housing legislation in our case, is applied only to individuals whose X is restricted to be either below (or above) a known threshold c. The effect of the treatment can be identified from the difference between X's effect on individuals just above and just below c. Loutskina and Strahan (2009), Roberts and Sufi (2009), Iliev (2010), and Kerr, Lerner, and Schoar (2011) provide recent applications of the regression discontinuity approach in the finance literature. Lee and Lemieux (2010) survey its uses in other areas of economics.

To formalize, our regression discontinuity design begins by first considering the following regression:

$$Y = \alpha + X\beta + I_{[X<c]}\tau + Z\delta + \varepsilon, \tag{1}$$

where Y denotes, in separate regressions, originations or mortgage rates (the estimated probabilities of default are discussed separately below). The variable X represents the observable variable that

determines the treatment criteria reflected in the indicator $I_{[X<c]}$. Only those individuals with X less than the cutoff, c, receive the treatment. The coefficient, β, represents the effect of X on Y sans the treatment and τ is the magnitude of the treatment effect. Here, Z represents a second set of observable variables that are unrelated to the treatment criteria, $X < c$. Because the treatment criterion is known and a function of an observable variable, we need not include all variables that can affect Y in Z. That is, there is no omitted variable bias for excluded elements of Z so long as the excluded Z's are not correlated with $I_{[X<c]}$ (see Hahn, Todd, and van der Klaauw, 2001).[4] Including covariates in the regression can, however, reduce sampling uncertainty and thus provide more precise estimates (see Lee and Lemieux, 2010) for additional discussion of the use of covariates in regression discontinuity designs).

The treatment effect would be straightforward to estimate if the model were truly globally linear. An advantage of the regression discontinuity approach is that it relies only on local smoothness in the effect of the observable variable X to identify the treatment effect. To exploit this, we can restrict our attention to loans just above and just below the program cutoff. Thus, when estimating the baseline model, we include only data within a band of 2% of the goal cutoff. For example, to evaluate the effect of the CRA, we estimate using only loans made in Census tracts with median income of 78% to 82% of the MSA median income. The treatment group, i.e., the loans for which the indicator variable, $I_{[X<c]}$, takes a value of 1, are loans made in Census tracts with median income of 78% to 80% of that of the MSA. The size of the band, in this case, 2% on each side of the cutoff, must be small enough to ensure smoothness but large enough to obtain a sufficient amount of data. In a later section, we experiment with the bandwidth size to verify the robustness of our results.

For the regression discontinuity approach, we also must assume that agents (i.e., borrowers) cannot control X, which is innocuous for the affordable housing criteria applied to an area (e.g., a Census tract). However, in three cases (CRA2, SAG2, and LMIG), the goal is defined for an individual's income alone. Thus, it is possible that a borrower could report income just below the threshold to qualify for treatment. This assumes, however, that borrowers are keenly aware of the goals and they know lenders will, say, lower their mortgage rate. We address these issues in Section 4.2.

[4]The correlation between X and Z does not affect the estimation of the treatment effect.

In addition to the linear model, we can estimate the effects of the affordable housing legislation on, for example, the probability of default. For the regression discontinuity model of default, we must modify the linear specification (1) to account for the binary default indicator as the dependent variable. This is also straightforward in the regression discontinuity framework, as the underlying assumption is smoothness as opposed to linearity. Thus, we can estimate the standard probit model augmented with the treatment indicator and restricted to the loans just above and just below the program cutoff. We can then assess whether the programs had an effect on the probability of default as

$$\Pr[D = 1] = \Phi\left(\alpha + X\beta + I_{[X<c]}\tau + Z\delta\right),$$

where D is the default indicator and $\Phi(.)$ represents the standard normal cumulative distribution function.

In the case of affordable housing programs, the cutoffs are based on either borrower income or Census tract characteristics as described in Section 2.1. The advantage of our regression discontinuity approach is that we need not know who the final holder of the loan is. This point is important because financial institutions receive credit for loans that they acquire by purchasing securitized pools, not just the loans they originate or acquire as whole loans. The majority of subprime loans were securitized such that the originator is highly unlikely to be the final holder of the loan. Because depository institutions and the GSEs can satisfy their affordable housing goals by purchasing securitizations, whether the originator is subject to the CRA, whether the loan is in the financial institution's CRA assessment area, and whether the loan is conforming conveys at best incomplete information about the impact of the regulations.

In total, we estimate our three outcome measures on the following subsamples of the population of loans:

1. CRA1: Loans in Census tracts with median income of $78\% - 82\%$ of MSA median income.
2. CRA2: Loans to borrowers with income of $78\% - 82\%$ of MSA median income.
3. UAG1: Loans in Census tracts with a minority population of $28\% - 32\%$ and with a median income of no more than 120% of MSA median income.

4. UAG2: Loans in Census tracts with median income of $88\% - 92\%$ of MSA median income.

5. SAG1: Loans to borrowers with income of $58\% - 62\%$ of MSA median income.

6. SAG2: Loans to borrowers with income of $78\% - 82\%$ of MSA median income and who live in a Census tract with median income of $78\% - 82\%$ of MSA median income. For SAG2, the treatment group is the set of borrowers that have an income of $78\% - 80\%$ of MSA median income *and* who live in a Census tract with a median income of $78\% - 80\%$ of MSA median income.

7. LMIG: Loans to borrowers with income of $98\% - 102\%$ of MSA median income.

If any of the affordable housing goals affect the subprime market, we would expect to see a discontinuity in originations, interest rates, or default rates related to either 1) the median income in the Census tract relative to the MSA, 2) the minority population share in the Census tract, or 3) the ratio of borrower income to median MSA income. This would manifest in the statistical significance of the coefficient τ.

In all models, we include the goal variable (e.g., tract-to-MSA income ratio in the regressions and probit for CRA1) as a control. In the regressions for the number of originations, we always include year dummies. In the regressions for the rate and the probits, we include dummies for the month of origination. As a robustness check, we include other covariates in the equations.

3 Data

Our data are non-prime, securitized, first-lien mortgages originated in 2004 through 2006 in metropolitan areas of California and Florida. We chose our sample period to coincide with the height of the subprime mortgage boom (see Demyanyk and Van Hemert, 2011). We focus on California and Florida as these states had large shares of subprime mortgage originations and experienced a large share of defaults in the aftermath of the subprime boom. We merge detailed data on the performance and terms of loans securitized into private-label asset-backed securities from First American CoreLogic (CL) with data on borrower income, borrower race, Census tract income, and Census tract racial composition obtained under the Home Mortgage Disclosure Act (HMDA). HMDA requires residential mortgage originators to report to the Federal Financial Institutions Examination

Council certain key information on most of the loans they originate to facilitate the evaluation of compliance with the Fair Housing Act (1968) and the CRA. We restrict our sample to loans made in metropolitan areas because rural originations are often exempt from the HMDA reporting requirements. Some of our mortgages were securitized in packages that CL designates as alt-A and some were part of securities designated as B/C collateral in CL. The designations of either alt-A or B/C are security-level rather than loan-level designations. Although alt-A deals were marketed to investors as comprised of loans that were very high quality except for a lower level of documentation, most alt-A deals contain many loans with very high LTVs and low FICO scores. Similarly, most securities designated as B/C contain many loans with low or no documentation.

3.1 Merging Datasets

The matching procedure considers first-lien loans with the same purpose (purchase or refinance) and occupancy status (owner-occupied). CL associates each loan with a 5-digit U.S. Postal Service ZIP code, whereas HMDA loans are associated with Census tracts. To match ZIP codes with Census tracts we used Census Bureau ZIP Code Tabulation Areas (ZCTAs).[5] We also use the geographic information systems program ArcView to establish Census tract search areas associated with any given ZCTA as follows: For each loan in CL, we determined the smallest set of Census tracts that intersect with the associated ZCTA and we allowed for the union of the Census tracts in the intersection to extend over the geographic area defined by any given ZCTA.

Except for the use of ZCTAs, we followed Haughwout, Mayer, and Tracy's (2009) matching algorithm very closely. The procedure entails six stages that use the originator's name, the loan amount, and the origination dates to obtain the matches. The names are provided by the lenders themselves in the HMDA data but not in the CL data. As a result, lender names in CL must be cleaned manually before the matching. Loan amounts are provided in dollars in CL, while they are provided in thousands of dollars in HMDA. Furthermore, HMDA allows lenders to round up loan amounts to the nearest thousand dollars if the fraction equals or exceeds $500. The dates are matched to within 5 business days if the CL dates are not imputed or to the same month if they are.[6] A summary of the various stages is as follows:

[5] ZCTAs are statistical entities developed by the Census to tabulate summary statistics from the 2000 Census for geographic areas that approximate the land area covered by each ZIP code.

[6] CL origination dates are considered to be imputed if they are exactly two months before the first payment date.

- Stage 1 considers loans with matched originator names and uses the larger 4-digit ZCTA search areas. Loan amounts are matched allowing a difference of up to and including $1,000.
- Stage 2 ignores originator names and uses 4-digit ZCTA search areas, as in stage 1.
- Stage 3 again considers originator names, but uses the smaller 5-digit ZCTA search areas. Loan amounts are matched allowing a difference of up to but not including $1,000.
- Stage 4 is similar to stage 3 but ignores originator names.
- Stage 5 is similar to stage 1 but loan amounts are matched to within 2.5% of the CL amount.
- Stage 6 is similar to stage 2 but loan amounts are matched to within 2.5% of the CL amount.

At the conclusion of each stage, only one-to-one matches are kept and are removed from the datasets, while loans with multiple matches (either one CL loan to many HMDA loans or many CL loans to one HMDA loan) are returned to the matching pool for the subsequent stages. We also applied various data checks to the final sample of loans, including dropping observations with missing or erroneous FICO scores and dropping observations with contract rates smaller than the reported HMDA spread of the loan's annual percentage rate with a Treasury security of comparable maturity. For additional details on the matching algorithm, see the appendix of Haughwout, Mayer, and Tracy (2009). We are able to match 67% and 83% of the CL loans in California and Florida, respectively, with HMDA data.

We focus on mortgages packaged into PLMBS because much of the controversy surrounding the GSEs regards their holdings of PLMBS. There is good reason for concern regarding the GSEs' holdings of these securities. First, by 2005 Fannie Mae and Freddie Mac held more than $350 billion of PLMBS (Congressional Budget Office [CBO], 2010). The pattern of the GSEs' holdings of PLMBS mimics the shape of the subprime mortgage bubble (CBO, 2010). Further, the initial credit losses at the GSEs came from their holdings of PLMBS (CBO, 2010). Although PLMBS accounted for only one third of Fannie Mae's business, they accounted for more than 70% of their credit losses through the end of 2010 (CBO, 2010). In this paper, we do not dispute the role of PLMBS in the GSEs' downfall. Our question is whether the affordable housing mandates were responsible for the GSEs' role in this market.

We focus on 30-year adjustable-rate mortgages (ARMs) as we have the most data for these product types; our samples for other product types are much smaller, making it more difficult to detect any regression discontinuity that may exist. Our 30-year ARM definition emphasizes amortization; all mortgages in our sample amortize on a 30 year schedule. We focus on a single product type as the regression discontinuity approach works better with greater uniformity in the variable of interest along other dimensions. The appendix provides results for the two next most common product types as well as for all products combined together with product type controlled for using product dummies.

In our analysis, we focus on the initial contract interest rate rather than the annual percentage rate (APR) or the margin for the ARM because there is little evidence that lenders price the default or prepayment risk of subprime ARMs using the reset rate (see Haughwout, Mayer, and Tracy, 2009 and Ghent, Hernández-Murillo, and Owyang, 2012 for discussions of this issue). The reason lenders seem to price ARMs using the initial contract rate is that a large fraction of mortgages terminate before they reach the reset date (see, e.g., Demyanyk, 2009) such that the reset rate that the margin determines is largely a hypothetical interest rate. As such, it is highly unlikely that originators offer a lower margin to borrowers whose loans meet the housing goal criteria. Because the APR is computed assuming the mortgage is held to maturity, it largely also reflects the reset rate, a rate that is hypothetical for most borrowers.

Finally, our data include a handful of observations that have implausibly small or large loan amounts, FICO scores, or LTVs. To remove the effect of such observations, which are most likely due to data entry errors, we winsorize observations in the bottom 0.5% or top 0.5% of the distribution of loan amount, FICO score, or LTV.

3.2 Summary Statistics

Table 1 contains summary statistics on the loans in our sample. In total, our sample contains 722,157 loans. Only 30% of the loans in our sample do not satisfy any of the affordable housing goals. More than half the loans (56%) are in Census tracts with a minority share of at least 30% such that they satisfy the GSEs' UAG1 goal. More than half the loans (54%) also satisfy the GSEs' UAG2 goal insofar as they are for properties in Census tracts with tract income no more than 90% of that of the MSA. About 40% of the loans are made to borrowers in Census tracts with tract

income of no more than 80% of MSA income such that they meet the CRA1 goal.

A smaller proportion of the loans meet the borrower-specific affordable housing goals than satisfy the tract-specific affordable housing goals. The average borrower income is over $\$100,000$; these loans did not, in general, go to households that were low income according to the loan application. [7] Only 27% of the loans are to borrowers with less than the median MSA income such they qualify for the GSEs' LMIG goal. Only 14% of the loans are made to households with income of less than 80% of the MSA's income such that they meet the CRA's borrower-specific component (CRA2). A mere 5% of loans are made to households with income of less than 60% of the median MSA income such that they meet the SAG1 criterion.

The first three rows of Table 1 provide further evidence that subprime loans were not made to households that stated they had low incomes but were disproportionately originated in low-income and minority neighborhoods. The average borrower-to-MSA median income ratio in our sample is 173% which indicates that the typical subprime borrower had a much higher stated income than the typical household in the MSA. The typical borrower in our sample lived in a Census tract where 47% of the population belonged to a racial minority and where the income in the Census tract was lower than that of the MSA.

The picture that emerges of the subprime borrower is that of a high-income household that lives in a low-income neighborhood. Given the level of misrepresentation in the low documentation or no documentation loans (see, e.g., Jiang, Nelson, and Vytlacil, 2011), it is quite possible that the difference between the borrower's and the neighborhood's income is due to income misreporting. Fewer than half the loans in our sample are made with full documentation but even the full documentation loans may have overstated income. Regardless of the reason for the difference between the stated income of the borrower and the income in his or her neighborhood, the *stated* income determines eligibility for the borrower-specific goals such that few of the loans in our sample qualify for the borrower-specific goals.

Less than half the loans in our sample were originated by depository institutions. The share of loans originated by non-depository institutions is similar for other product types: for all products, 55% of loans are originated by non-depository institutions. Thus, if the CRA affected the subprime

[7] High borrower income is also a characteristic of products other than the 30 year ARM. The average borrower income over all product types is over $107,000.

Table 1: Summary Statistics for Non-prime 30-yr ARMs Originated in 2004-2006 in California and Florida

Variable	Mean	Std. Dev.	Min	Max	Definition
Borrower Income	1.73	1.91	0.01	212.74	Borrower/MSA median income
Borrower Income ($)	102,320	113,641	1,000	9,999,000	Borrower Income ($)
Tract Income	0.927	0.349	0.109	4.376	Tract median/MSA median income
Tract Percent Minority	0.47	0.28	0.01	1.00	Minority share of tract population
Interest Rate (%)	6.74	1.98	0.88	13.99	Initial contract rate (%)
LTV Ratio	77.06	12.40	28.56	100.00	LTV ratio at origination (%)
Prepayment Penalty	0.89	0.31	0	1	Prepayment penalty at origination = 1
FICO	632	73	501	800	FICO score at origination
PMI	0.17	0.37	0	1	Private Mortgage Insurance (PMI) at origination = 1
Origination Amount	$294,984	$196,232	$ 57,000	$1,344,000	Loan amount
Full Documentation	0.42	0.49	0	1	Full documentation = 1
Refinance	0.65	0.48	0	1	Refinance = 1
Florida	0.39	0.49	0	1	Property in Florida = 1
Default within 2 Yrs	0.15	0.35	0	1	90-day or more severe delinquency or foreclosure within 2 years or origination
CRA1 eligible	0.40	0.49	0	1	Tract/MSA income ≤ 0.8
CRA2 eligible	0.14	0.35	0	1	Borrower/MSA income ≤ 0.8
UAG1 eligible	0.56	0.50	0	1	Percent Minority ≥ 0.3
UAG2 eligible	0.54	0.50	0	1	Tract/MSA income ≤ 0.9
SAG1 eligible	0.05	0.22	0	1	Borrower/MSA income ≤ 0.6
SAG2 eligible	0.09	0.29	0	1	Borrower and tract / MSA income ≤ 0.8
LMIG eligible	0.27	0.44	0	1	Borrower/MSA income ≤ 1.0
Not goal eligible	0.30	0.46	0	1	Does not satisfy any goal
Depository Inst.	0.44	0.50	0	1	Loan originated by a depository institution
Number of Loans	722,157				

securities market, it must have been because depository institutions purchased large amounts of the securities rather than because the CRA compelled them to originate subprime loans.

The remaining characteristics of the loan in our sample are as follows:

- Average loan amount: $\$294,984$
- Average FICO score in our sample is 632. This is consistent with the typical characterization of a subprime loan as one made to a borrower with a weak credit history
- Loans with a prepayment penalty at origination: 89%
- Loans made to refinance an existing loan (rather than to purchase a property): 65%
- Average interest rate at origination: 6.74%
- Loans defaulting within 2 years of origination: 15%
- Loans originated in Florida: 39%.

4 Results

4.1 Baseline Results

Figures 1 through 5 present the relationship of originations and interest rates with the goal variables using data from 2005. The figures are quite similar using data for 2004 and 2006. We group both originations and interest rates into 2-percentage-point bins for the relevant goal variable. The figures include the data associated with a particular point as all the data from the bottom of the bin cutoff to the top of the bin cutoff. For example, the point associated with 79% includes all the data from 78% to 80%. The results are similar when we group originations and interest rates in 1- and 5- percentage-point bins; these results are available in the appendix.

Figures 1 and 2 show the relationship between the number of originations per tract (scaled by tract population) with the tract-to-MSA median income ratio and the percent of minority residents in the Census tract. Figures 3 through 5 show the relationship between the average initial contract interest rate and the goal variables. Figure 3 shows the relationship between the average borrower interest rate and the tract-to-MSA median income ratio; Figure 4 illustrates the relationship between the average borrower interest rate and the percent of minority residents in the

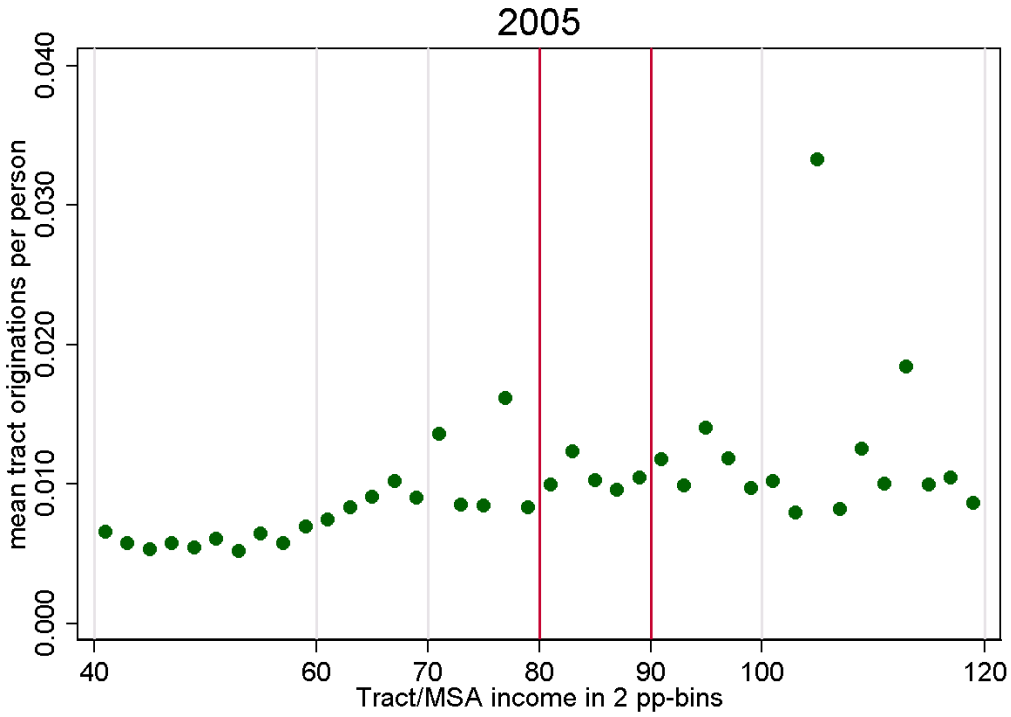

Figure 1: Effect of CRA1 and GSEs' UAG2 on Origination Volumes
Each dot represents observations in 2 percentage point intervals ranging from 40% to 120% of Census Tract/MSA Income ratio. For example, the 79% dot represents the data between 78% and the 80% cutoff. Similarly, the 81% dot represents observations in the 80% to 82% band. The regressions use only observations immediately below and immediately above the cutoff (e.g., the data represented by the 79% and 81% points for CRA1).

Census tract. Figure 5 plots the relationship between the average borrower interest rate and the borrower-to-MSA income ratio.

If either the CRA1 or UAG1 goals fueled the subprime mortgage boom, we would expect to see a discontinuity around 80% (CRA1) or 90% (UAG1) in Figures 1 and 3. No discontinuity exists around either of these points. Similarly, in Figures 2 and 4, we would expect to see a discontinuity around 30% (UAG1) if the minority share goal for the GSEs has an effect on the subprime market. We see no such effect. Finally, an effect of the borrower-specific affordable housing goals would result in a discontinuity at 60% (SAG1), 80% (CRA2 and SAG2), or 100% (LMIG) in Figure 5. The results are striking: There is no visible discontinuity in either interest rates or loan originations in any of the figures.

Table 2 presents the results from our regression discontinuity approach for originations per tract

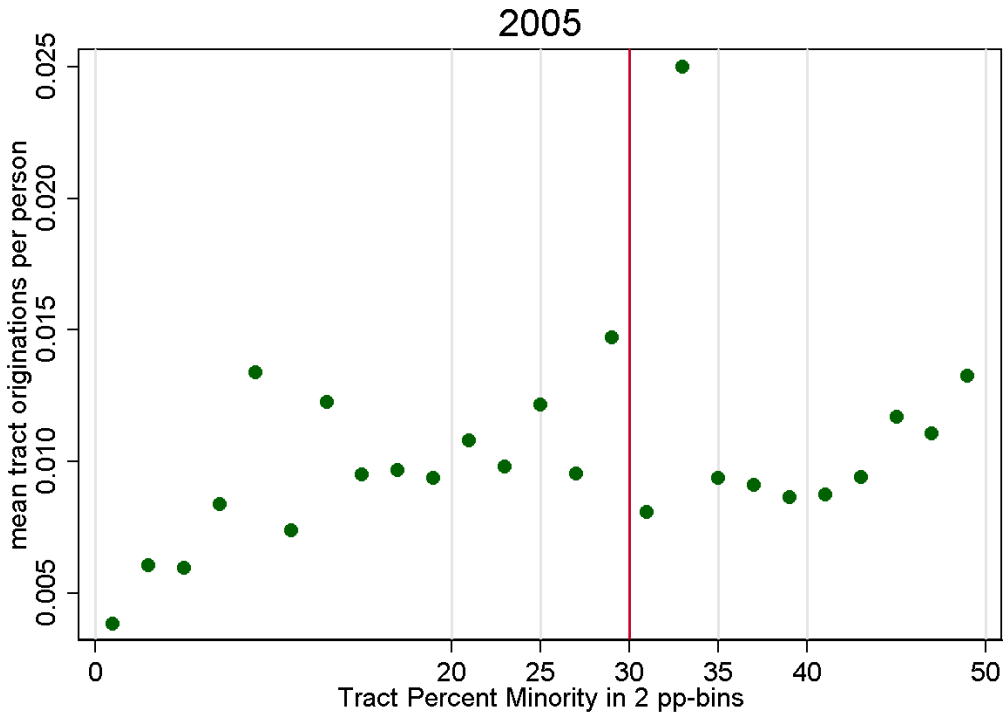

Figure 2: Effect of GSEs' UAG1 on Origination Volumes

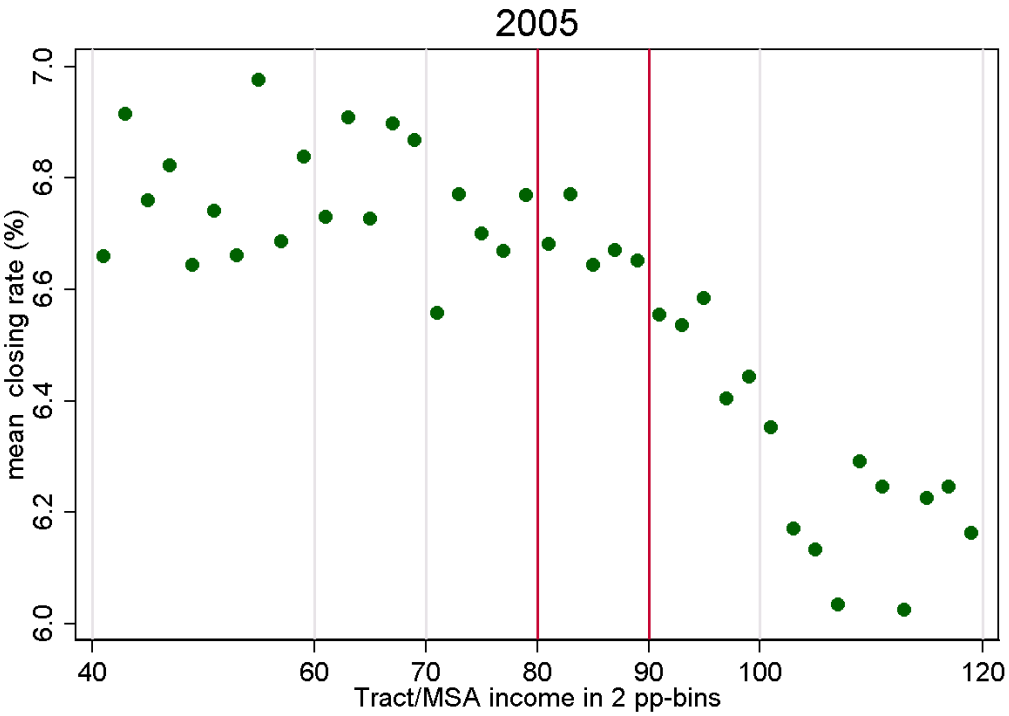

Figure 3: Effect of CRA1 and GSEs' UAG2 on Contract Interest Rates

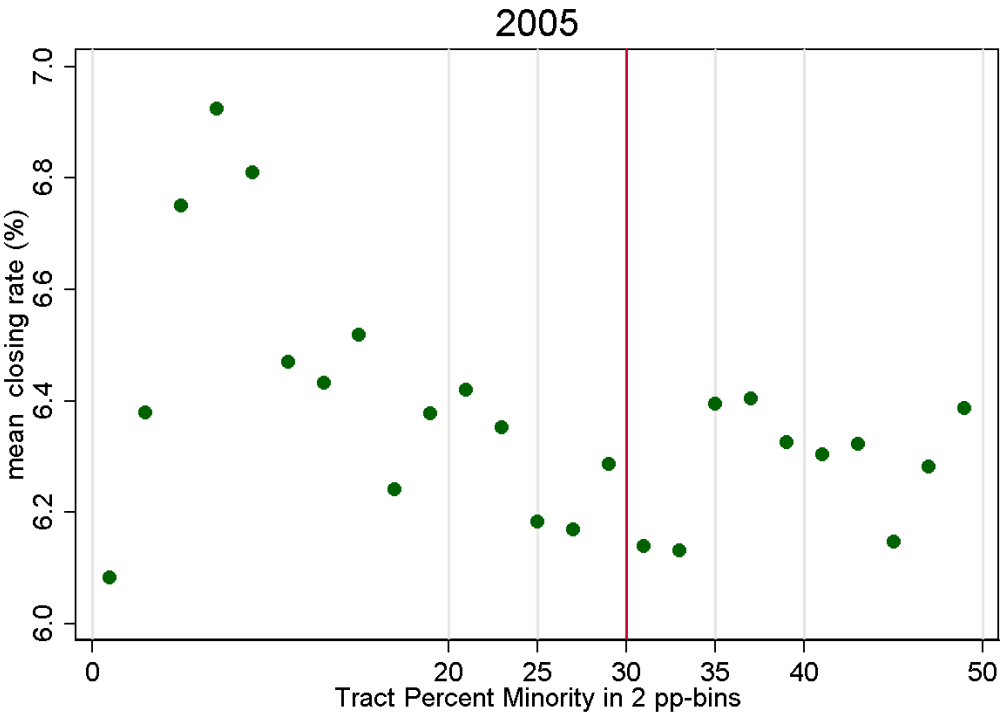

Figure 4: Effect of GSEs' UAG1 on Contract Interest Rates

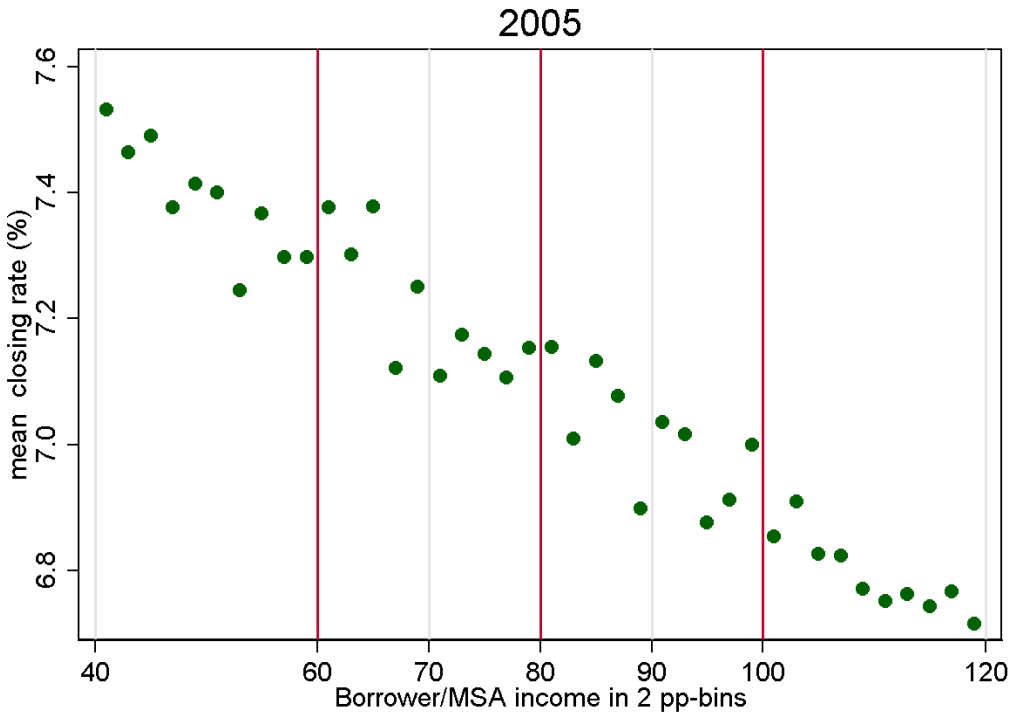

Figure 5: Effect of CRA2 and GSEs' SAG1, SAG2, and LMIG on Contract Interest Rates

per year. The regressions use data from 2004 through 2006 such that there are three observations for each Census tract. None of the goal variables are significant at any conventional statistical significance level regardless of what controls we include.

Although we do not find an effect in origination volumes, the volume of originations is not well suited to studying the borrower-level goals using our data. It remains possible that some or all of the borrower-level goals described in the previous section have an effect on the subprime market. One could use the HMDA data alone to determine the likelihood of a loan application being denied to study the effect of the borrower-specific affordable housing goals on the volume of originations. However, the HMDA data do not indicate the final disposition of the loan (i.e., whether the loan is held by the originator in portfolio, securitized by the GSEs, or securitized in a PLMBS) since the data are collected at loan origination. The interest of this paper is specifically on PLMBS such that we focus on rates and performance to measure the effects of the borrower-specific goals on the subprime market.

One way the goals might manifest themselves is by borrowers receiving a lower interest rate if they meet one or more of the program goals. Alternatively, affordable housing policies may lead to lenders holding borrowers to a lower standard because of the benefit lenders receive by complying with the affordable housing policies. If lenders apply a lower quality threshold to loans that satisfy the affordable housing goals, we would thus expect to see lower performance for loans that satisfy the goals. To look at the borrower-level goals, we thus also look at the effect of affordable housing goals on interest rates and default.

Table 3 presents the results from our regression of the contract interest rate, measured in percentage points, on the goal variables and controls. The goal indicator variables are usually insignificant and small in magnitude. For three goals, the goal indicator variable is statistically significant: UAG1, SAG1, and LMIG. However, in two of the three cases (UAG1 and LMIG), the sign of the goal variable is *positive* such that the results suggest that the affordable housing goal increases rather than lowers the cost of borrowing for eligible borrowers. Furthermore, the effect of the goals on the cost of borrowing is never significant once we include a broader set of controls for loan-level characteristics.[8]

[8]The other controls are the FICO score, the LTV ratio at origination, the origination amount, a dummy for whether the loan was full documentation, a dummy for whether the loan was for refinancing, a dummy for whether the loan was originated in California, a dummy for whether the loan required the borrower to pay PMI, and a dummy

Table 2: Regression Discontinuity Results for Effect of Affordable Housing Goals on Originations per Tract per Year (2-Percentage-Point Window)

	Goal Dummy	Tract Percent Income	Tract Minority	Year Controls	R-Squared	No. of Obs.
CRA1 (tract/MSA income ≤ 0.8)	0.00036	0.0085	-	Yes	3.5%	1,547
	(0.00055)	(0.0240)				
CRA1 (tract/MSA income ≤ 0.8)	0.00039	0.0105	0.0016***	Yes	4.1%	1,547
	(0.00055)	(0.0240)	(0.0005)			
UAG1 (tract minority share ≥ 0.3)	-0.00144	-	-0.0215	Yes	2.3%	1,145
	(0.0011)		(0.0487)			
UAG1 (tract minority share ≥ 0.3)	-0.00157	0.0028*	-0.0139	Yes	2.5%	1,145
	(0.00111)	(0.0016)	(0.0488)			
UAG2 (tract/MSA income ≤ 0.9)	0.00027	0.0152	-	Yes	3.3%	1,399
	(0.00058)	(0.0252)				
UAG2 (tract/MSA income ≤ 0.9)	0.00033	0.0165	0.0021***	Yes	4.2%	1,399
	(0.00057)	(0.0250)	(0.0006)			

Notes: 1) Standard errors are listed in parentheses. 2) Each Regression is estimated with all data for 2004 through 2006 that are within 2 percentage points of the goal cutoff. 3) ***, **, and * denote significance at 1%, 5%, and 10% levels. 4) All regressions also include a constant. 5) The dependent variable in all regressions is the number of originations in the tract / tract population.

Table 3: Regression Discontinuity Results for Effect of Affordable Housing Goals on Interest Rates (2-Percentage-Point Window)

	Goal Dummy	Tract Income	Tract Percent Minority	Borrower Income	Month of Orig. Controls	Other Controls	R-Squared	No. of Obs.
CRA1 (tract/MSA income $\leq$ 0.8)	0.044	0.87796	-	-	Yes	No	6.1%	40,442
	(0.091)	(3.967)						
CRA1 (tract/MSA income $\leq$ 0.8)	-0.009	-0.965	-	-	Yes	Yes	42.8%	40,442
	(0.037)	(1.535)						
CRA2 (borrower/MSA income $\leq$ 0.8)	0.084	-	-	4.061*	Yes	No	8.0%	15,925
	(0.051)			(2.214)				
CRA2 (borrower/MSA income $\leq$ 0.8)	-0.005	-	-	1.584	Yes	Yes	46.5%	15,925
	(0.039)			(1.688)				
UAG1 (tract minority share $\geq$ 0.3)	0.459***	-	-17.28***	-	Yes	No	6.7%	36,000
	(0.111)		(5.54)					
UAG1 (tract minority share $\geq$ 0.3)	0.007	-	-0.27	-	Yes	Yes	42.2%	36,000
	(0.330)		(1.29)					
UAG2 (tract/MSA income $\leq$ 0.9)	0.056	-1.287	-	-	Yes	No	6.0%	39,660
	(0.093)	(3.785)						
UAG2 (tract/MSA income $\leq$ 0.9)	-0.042	-1.651	-	-	Yes	Yes	41.6%	39,660
	(0.033)	(1.426)						
SAG1 (borrower/MSA income $\leq$ 0.6)	-0.113*	-	-	-6.852**	Yes	No	9.1%	9,750
	(0.062)			(2.687)				
SAG1 (borrower/MSA income $\leq$ 0.6)	-0.050	-	-	-1.069	Yes	Yes	47.4%	9,750
	(0.460)			(1.980)				
SAG2 (borrower&tract /MSA income $\leq$ 0.8)	-0.076	-3.673	-	-3.745	Yes	No	10.5%	1,176
	(0.154)	(4.479)		(4.928)				
SAG2 (borrower&tract /MSA income $\leq$ 0.8)	-0.040	-4.08702	-	-1.817	Yes	Yes	49.0%	1,176
	(0.116)	(3.590)		(3.758)				
LMIG (borrower/MSA income $\leq$ 1.0)	0.166***	-	-	4.035*	Yes	No	7.8%	18,687
	(0.053)			(2.402)				
LMIG (borrower/MSA income $\leq$ 1.0)	0.063	-	-	4.178**	Yes	Yes	45.6%	18,687
	(0.040)			(1.851)				

Notes: 1) Standard errors are listed in parentheses. 2) Each regression is estimated with all data for 2004 through 2006 that are within 2 percentage points of the goal cutoff. 3) ***, **, and * denote significance at 1%, 5%, and 10% levels. 4) All regressions also include a constant. 5) The dependent variable is the contract interest rate. 6) Standard errors are clustered by Census tract. 7) Other controls are the loan's LTV, the borrower's FICO score, a full documentation dummy, a refinance dummy, a Florida dummy, a dummy if the loan has PMI, a dummy if the loan has a prepayment penalty, and the loan amount.

Table 4 illustrates the effect of the affordable housing goals on the performance of the loan. The dependent variable in the probit is an indicator variable that takes a value of 1 if the loan goes into serious default (i.e., experiences a delinquency of 90 days or more or terminates through foreclosure) within two years of origination. The table shows the marginal effects of a change in the dependent variable on the likelihood of default. The goal variables are statistically insignificant with two exceptions. The coefficients indicate that a loan that is eligible for the UAG2 goal by virtue of being made in a tract with median income less than or equal to 90% of that in the MSA is about 2% more likely to default. However, the effect is statistically significant only at the 10% level after we include other loan controls. Furthermore, the results in Table 4 indicate that a loan made to a borrower with income less than the median income in the MSA is 2% *less* likely to default than one that did not qualify for the GSEs' LMIG goal. Overall, the results in Table 4 are not supportive of the notion that goal-eligible loans were of worse quality than goal-ineligible loans.

4.2 Robustness

4.2.1 Alternative Bandwidths

It is possible that our chosen benchmark bandwidth of 2 percentage points is not the appropriate bandwidth for one of two reasons. The first possibility is that it is too broad such that our loans are not sufficiently similar along the key dimension of interest for evaluating the goal. If this is the case, our regressions will not pick up the effect of the affordable housing program. The second possibility is that our bandwidth is too small for us to have sufficient data to detect the effect of the affordable housing programs. To ensure our results are robust to these concerns, we also explore the effect of the affordable housing goals on all three outcome measures using 1- and 5-percentage-point windows. The results are quite similar to the benchmark results and are reported in the appendix in the interest of brevity.

4.2.2 Documentation

An important requirement for the regression discontinuity approach to be valid is that households and originators cannot precisely manipulate the assignment variable (see Lee and Lemieux, 2010). In our case, the assignment variable may be either the income of the Census tract, the income of

for whether the loan had a prepayment penalty.

Table 4: Probit Results for Effect of Affordable Housing Goals on Default Likelihood (2-Percentage-Point Window)

	Goal Dummy	Tract Income	Tract Percent Minority	Borrower Income	Month of Orig. Controls	Other Controls	Pseudo R-Squared	No. of Obs.
CRA1 (tract/MSA income ≤ 0.8)	-0.006	-0.319	-	-	Yes	No	10.3%	40,442
	(0.011)	(0.409)						
CRA1 (tract/MSA income ≤ 0.8)	-0.005	-0.260	-	-	Yes	Yes	17.2%	40,442
	(0.009)	(0.353)						
CRA2 (borrower/MSA income ≤ 0.8)	-0.011	-	-	-0.297	Yes	No	7.2%	15,925
	(0.010)			(0.443)				
CRA2 (borrower/MSA income ≤ 0.8)	-0.011	-	-	-0.221	Yes	Yes	13.6%	15,925
	(0.010)			(0.429)				
UAG1 (tract minority share ≥ 0.3)	0.011	-	-0.768**	-	Yes	No	9.4%	36,000
	(0.008)		(0.354)					
UAG1 (tract minority share ≥ 0.3)	-0.007	-	-0.018	-	Yes	Yes	17.3%	36,000
	(0.006)		(0.259)					
UAG2 (tract/MSA income ≤ 0.9)	0.025**	0.644	-	-	Yes	No	9.8%	39,660
	(0.011)	(0.533)						
UAG2 (tract/MSA income ≤ 0.9)	0.019*	0.450	-	-	Yes	Yes	16.6%	39,660
	(0.010)	(0.473)						
SAG1 (borrower/MSA income ≤ 0.6)	0.012	-	-	0.403	Yes	No	5.8%	9,750
	(0.014)			(0.629)				
SAG1 (borrower/MSA income ≤ 0.6)	0.013	-	-	0.508	Yes	Yes	11.0%	9,750
	(0.014)			(0.605)				
SAG2 (borrower&tract /MSA income ≤ 0.8)	-0.044	0.052	-	-0.001	Yes	No	7.4%	1,176
	(0.028)	(0.865)		(0.001)				
SAG2 (borrower&tract /MSA income ≤ 0.8)	-0.033	0.236	-	0.002	Yes	Yes	14.1%	1,176
	(0.027)	(0.879)		(0.002)				
LMIG (borrower/MSA income ≤ 1.0)	-0.017*	-	-	-0.897*	Yes	No	8.6%	18,687
	(0.010)			(0.470)				
LMIG (borrower/MSA income ≤ 1.0)	-0.020**	-	-	-0.868*	Yes	Yes	15.4%	18,687
	(0.010)			(0.450)				

Notes: 1) Standard errors are listed in parentheses. 2) Each probit is estimated with all data for 2004 through 2006 that are within 2 percentage points of the goal cutoff. 3) ** and * denote significance at 5% and 10% levels. 4) All probits also include a constant. 5) The dependent variable is whether the loan defaults within two years of origination. 6) Standard errors are clustered by Census tract. 7) Other controls are the loan's LTV, the borrower's FICO score, a full documentation dummy, a refinance dummy, a Florida dummy, a dummy if the loan has PMI, a dummy if the loan has a prepayment penalty, and the loan amount. 8) Entries show marginal effects averaged over all observations.

the borrower, or the minority share of the Census tract. Since the income and minority share of the Census tract are determined by HUD, clearly neither households nor originators can manipulate these assignment variables. However, it seems possible that the lender or borrower may be able to precisely manipulate income for low documentation or no documentation loans. If the results for the full sample are driven primarily by the no and low documentation loans, it is possible that the reason we find no effect for the borrower-specific goals is because borrowers are lying downwards about their income to satisfy the affordable housing goals such that the regression discontinuity approach is not valid.

To ensure our results are robust to this possibility, for CRA2, SAG1, SAG2, and LMIG, we consider the sensitivity of our results for the subsample of loans with full documentation and the subsample of loans having partial or no documentation. Restricting the sample in this manner reduces our sample but still leaves over $7,000$ observations to evaluate the effect of CRA2 and LMIG, over $3,000$ observations to evaluate the effect of SAG1, and only about 500 to evaluate the effect of SAG2 in the no or low documentation sample.

Table 5 reports the results of the rate regressions on the borrower-specific affordable housing goals (CRA2, SAG1, SAG2, and LMIG) from the benchmark specification, using only the subset of loans with full documentation, and using only the subset of loans with no or low documentation. The results are similar in character across the three samples. Only one of the goal indicator variables (LMIG) is statistically significant when we include additional controls. However, the sign of the coefficient for the goal variable for LMIG indicates that the program in fact *increases* the cost of borrowing for borrowers who meet the program requirements. The effect is about 31 basis points in the no/low documentation sample, and falls to about 13 basis points when we use the full set of controls.

Table 6 reports the results of the probit estimation of the effect borrower-specific affordable housing goals on the likelihood of default. The results for the full documentation and low/no documentation samples are quite similar to the results for the full sample. Only the indicator for LMIG is statistically significant (in the full sample and in the full documentation sample) but it has the 'wrong' sign in the sense that the results indicate a loan that satisfies the goal decreases rather than increases the risk of default.

We note that, although it is theoretically possible that borrower incomes were misrepresented

Table 5: Coefficients on Goal Indicator Variables in Interest Rate Regressions by Documentation (2-Percentage-Point Window)

	Full Sample	No/Low Doc Sample	Full Doc Sample	Month of Orig. Controls	Other Controls
CRA2 (borrower/MSA income $\leq$ 0.8)	0.084	0.057	0.101*	Yes	No
	(0.051)	(0.087)	(0.058)		
CRA2 (borrower/MSA income $\leq$ 0.8)	-0.005	-0.023	0.009	Yes	Yes
	(0.039)	(0.064)	(0.045)		
SAG1 (borrower/MSA income $\leq$ 0.6)	-0.113*	-0.089	-0.124*	Yes	No
	(0.062)	(0.129)	(0.065)		
SAG1 (borrower/MSA income $\leq$ 0.6)	-0.049	-0.114	-0.028	Yes	Yes
	(0.046)	(0.091)	(0.051)		
SAG2 (borrower&tract /MSA income $\leq$ 0.8)	-0.076	-0.368	0.092	Yes	No
	(0.155)	(0.270)	(0.201)		
SAG2 (borrower&tract /MSA income $\leq$ 0.8)	-0.040	-0.067	0.004	Yes	Yes
	(0.116)	(0.198)	(0.161)		
LMIG (borrower/MSA income $\leq$ 1.0)	0.166***	0.306***	0.015	Yes	No
	(0.053)	(0.084)	(0.059)		
LMIG (borrower/MSA income $\leq$ 1.0)	0.063	0.125*	-0.002	Yes	Yes
	(0.041)	(0.067)	(0.044)		

Notes: 1) Standard errors are listed in parentheses. 2) Each regression is estimated with all data for 2004 through 2006 that are within 2 percentage points of the goal cutoff. 3)***, **, and * denote significance at 1%, 5%, and 10% levels. 4) All regressions also include a constant. 5) The dependent variable is the contract interest rate. 6) Standard errors are clustered by Census tract. 7) Other controls are the loan's LTV, the borrower's FICO score, a refinance dummy, a Florida dummy, a dummy if the loan has PMI, a dummy if the loan has a prepayment penalty, and the loan amount.

Table 6: Marginal Effect of Goal Indicator Variables on Default by Documentation (2-Percentage-Point Window)

	Full Sample	No/Low Doc Sample	Full Doc Sample	Month of Orig. Controls	Other Controls
CRA2 (borrower/MSA income $\leq$ 0.8)	-0.011	-0.013	-0.011	Yes	No
	(0.010)	(0.015)	(0.014)		
CRA2 (borrower/MSA income $\leq$ 0.8)	-0.011	-0.010	-0.013	Yes	Yes
	(0.010)	(0.014)	(0.014)		
SAG1 (borrower/MSA income $\leq$ 0.6)	0.012	0.009	0.014	Yes	No
	(0.015)	(0.024)	(0.018)		
SAG1 (borrower/MSA income $\leq$ 0.6)	0.013	0.004	0.018	Yes	Yes
	(0.014)	(0.023)	(0.017)		
SAG2 (borrower&tract /MSA income $\leq$ 0.8)	-0.044	-0.060	-0.005	Yes	No
	(0.028)	(0.050)	(0.041)		
SAG2 (borrower&tract /MSA income $\leq$ 0.8)	-0.033	-0.035	-0.002	Yes	Yes
	(0.027)	(0.049)	(0.039)		
LMIG (borrower/MSA income $\leq$ 1.0)	-0.017*	-0.00697	-0.029*	Yes	No
	(0.010)	(0.014)	(0.015)		
LMIG (borrower/MSA income $\leq$ 1.0)	-0.020**	-0.015	-0.026*	Yes	Yes
	(0.010)	(0.013)	(0.015)		

Notes: 1) Standard errors are listed in parentheses. 2) Each probit is estimated with all data for 2004 through 2006 that are within 2 percentage points of the goal cutoff. 3) ** and * denote significance at 5% and 10% levels. 4) All probits also include a constant. 5) The dependent variable is whether the loan defaults within two years of origination. 6) Standard errors are clustered by Census tract. 7) Other controls are the loan's LTV, the borrower's FICO score, a refinance dummy, a Florida dummy, a dummy if the loan has PMI, a dummy if the loan has a prepayment penalty, and the loan amount. 8) Entries show marginal effects averaged over all observations.

downwards to qualify for affordable housing programs, the existing evidence suggests that borrower incomes were much more likely to be overstated than understated. We discuss the misrepresentation of borrower income in greater detail in the next section.

4.2.3 Multidimensional Regression Discontinuity Estimates

The goals are not mutually exclusive, i.e., a loan that already satisfies one goal is more valuable if it satisfies a second or third goal, such that our one-dimensional, goal-by-goal approach to evaluating the effect of the goals is valid. However, we may be able to gain more power by comparing loans that satisfy two or more goals with loans that satisfy one or no goal. In most cases, a loan that is near the threshold of two goals could satisfy one geographic and one borrower-level goal. The exception is a combination of the CRA geographic goal with the UAG's racial goal. We have far fewer observations for some of these regressions since, as noted earlier, few loans in the PLMBS market were close to satisfying the borrower-specific goals. In some cases for the SAG2 goal, we do not have enough observations to accurately estimate the multidimensional specification. We present the results from estimating these specifications in an appendix.

4.2.4 Other Specifications

We also estimated the model separately for 2004, 2005, and 2006 to see whether the goals had influenced the PLMBS market in any particular year. We found no substantive difference in the results from our benchmark. These results are available in an appendix available from the authors on request. We furthermore examined whether our results differ when we estimate the model on other types of products. We looked at the next two most common products in our sample which are 1) adjustable rate loans that are interest only for five years and then fully amortizing over the next 25 years, and 2) fixed rate loans that fully amortize over 30 years. We then grouped all nonprime products in the dataset (eight products in total) and estimated the model. For the interest rate regressions and default probits, we included product dummies to control for product heterogeneity. These results are also in an appendix available upon request. Finally, although the majority of the loans in our sample are below the conforming loan limits since our dataset does not include loans in the jumbo category, we re-estimated the model using only loans below the conforming loan limits. These results were also very similar to our benchmark results. The results are available upon request.

5 Institutional Evidence and Discussion

Our analysis has shown no evidence of any discontinuity in the volume, pricing, or performance in subprime mortgages around the affordable housing cutoffs. One limitation of the regression discontinuity analysis is that the approach only detects a local average treatment effect. In this section, we therefore consider other reasons why it seems unlikely affordable housing mandates caused the subprime crisis.

5.1 Prospectuses

First, we examined a random sample of 100 prospectus and prospectus supplements from the subprime or alt-A deals into which our loans were packaged. We acquired the prospectuses from Bloomberg for the deals for which they were available.[9] Subprime and Alt-A PLMBS prospectuses (including the prospectus supplement) are typically 200-300 page documents that carefully describe the pool of loans using many criteria as geography, the interest rates on the mortgages, the property type (e.g., single-family or condo), the purpose of the loan (purchase or refinancing), FICO score, documentation level, and the LTVs. Not one of the 100 prospectuses we examined, however, described the eligibility of the loans in the pool to satisfy either the CRA or the GSE goals. Any prospective buyer of the pools to satisfy affordable housing programmes would thus have had to calculate itself for each loan whether or not it satisfied the goal. The lack of any mention whatsoever of the affordable housing programmes suggests they were not important motivations for the buyers of these securities. Furthermore, the evidence from the prospectuses indicates that few, if any, nonprime PLMBS were CRA-qualified. As such, banks were not, in general, able to count any loans they acquired exposure to through purchases of PLMBS towards their CRA commitments.

However, our review of the sample prospectus supplements reveals clear evidence that the GSEs were significant customers of PLMBS. In some deals, the prospectus supplement clearly states that all of the loans in the pool conform to one of the GSEs' limits for principal balances such that the GSE mentioned could purchase the securities. In other deals, the loans are divided into two or more loan pools with one or more loan pool consisting strictly of loans with principal balances

[9] We looked for 131 randomly selected deals on Bloomberg. Prospectuses for the other 31 deals were not available because either the deal was a private placement (4 deals), the deal prospectus was in a format that made reading and searching the document exceptionally difficult (3 deals), or the pool name in CL was not a close enough match to the pool name in Bloomberg. The Bloomberg names of the pools for which we examined the prospectuses are available in the appendix.

below the conforming loan limit. Rather than stating that the loans have principal balances below the conforming loan limit, which is the same for both Fannie Mae and Freddie Mac, the prospectus supplements usually state the name of one of the two GSEs when discussing the limit which strongly suggests a particular GSE as the purchaser. Some deals make it even clearer that certain securities are designed for Fannie Mae and Freddie Mac by separating the loans into separate pools for Fannie Mae and for Freddie Mac. For example, SAIL 2005-10 has one pool of loans, pool 1, with "principal balances that do not exceed the applicable Freddie Mac maximum original loan amount limitations", and another pool of loans, pool 2, with "principal balances that do not exceed the applicable Fannie Mae maximum original loan amount limitations".

Table 7 summarizes our findings from reviewing the random sample of 100 prospectuses. Of the 100 prospectuses, 47 state explicitly that one or more underlying loan pools consists exclusively of loans with principal balances below the conforming loan limit. The total dollar volume of these exclusively conforming pools represents 24% of the total dollar volume of all pools described in the prospectuses.

Table 7: Summary of Sample of Prospectuses

Total no. of deals	100
No. of deals that mention CRA or GSE Affordable Housing Goals	0
No. of deals with one or more pools exclusively conforming	47
Total deal volume	$ 100,409,451,614
Total volume of conforming pools	$ 24,494,562,317
Conforming share of all pools ($ volume)	24%

5.2 Did Reduced Documentation Loans Understate Borrower Income?

Second, the picture that emerges from our summary statistics in Table 1 is that subprime borrowers have stated incomes much higher than the typical incomes of the neighborhoods in which they live. This suggests that borrowers and loan originators *overstated* borrower incomes in order to get loans originated. If lenders were struggling to meet their affordable housing mandates, we would expect to see *understatement* of borrower incomes so that more loans were eligible for the goals. The evidence Jiang, Nelson, and Vytlacil (2011) present also indicates borrowers overstated rather than understated their financial positions.

5.3 Evolution of the GSEs' Affordable Housing Goals over Time

Third, the changes in affordable housing policy over time do not seem consistent with it causing a boom in subprime. There was no substantive change in the CRA at any point in 2003-2007. Table 8 shows the evolution of the GSEs' affordable housing goals since 1996. There is a fairly substantial increase between 2000 and 2001 with the three subgoals increasing by six to eight percentage points. However, there is no change in the goals between 2001 and 2004. Between 2004 and 2005 there is a 2 percentage point increase in the SAG and LMIG and a six percentage point increase in the UAG. Given that the largest increase in the affordable housing goals occurs about two years before the boom in subprime PLMBS begins (see, for example, the descriptive statistics in Demyanyk and Van Hemert, 2011), it is hard to understand how the affordable housing goals could be responsible for the boom.

Table 8: The GSEs' Affordable Housing Goals over Time

	UAG	SAG	LMIG
1996	21%	12%	40%
1997	24%	14%	42%
1998	24%	14%	42%
1999	24%	14%	42%
2000	24%	14%	42%
2001	31%	20%	50%
2002	31%	20%	50%
2003	31%	20%	50%
2004	31%	20%	50%
2005	37%	22%	52%
2006	38%	23%	53%
2007	38%	25%	55%
2008	39%	27%	56%
2009	32%	18%	43%

Notes: 1) Source, FHFA (2010). 2) UAG refers to the underserved areas goal, SAG refers to the Special Affordable Goal, and LMIG refers to Low and Moderate Income Goal. 3) See text of paper for goal eligibility criteria.

5.4 Discussion

There is some evidence that the geographic affordable housing policy goals modestly affected other areas of the mortgage market, usually over different sample periods. For example, Bhutta (2012) uses a regression discontinuity approach and studies loans originated between 1997 and 2002 that the GSEs could purchase as whole loans and finds that the UAG had a small effect on the number of originations. He finds no evidence that the goals affected the number of originations that were

not eligible for GSE purchase as whole loans consistent with our finding over the 2004-2006 period for subprime PLMBS. Bhutta defines such mortgages as mortgages originated by institutions designated as subprime originators by HUD. HUD has since discontinued publication of this list out of accuracy concerns. Ambrose and Thibodeau (2004) also study the effect of the UAG goal, over the period 1995-1999, and find it affected the quantity of mortgages in 1998 but not in other years. Using a regression discontinuity approach similar to ours, Bhutta (2011) shows that, over the 1994 - 2006 period, the CRA geographic goal had an economically and statistically significant effect on lending in large cities in the late 1990s and early 2000s but that the effect had disappeared by the mid-2000s. Agarwal, Benmelech, Bergman, and Seru (2012) look at whether the volume of loan originations changes around the CRA evaluation date and find significantly more originations in the three quarters before the evaluation date (not the evaluation period) and three quarters after the evaluation date. In contrast to Bhutta's (2011) findings, Agarwal, Benmelech, Bergman, and Seru (2012) find that these effects of the CRA on loan volume were largest during the subprime boom. See Reid et al. (2013) for a discussion of Agarwal, Benmelech, Bergman, and Seru (2012).

We also note that the affordable housing goals may have affected the market indirectly. For example, the GSEs affordable housing goals may have given the GSEs some political cover to purchase substantial quantities of PLMBS. Our results suggest that, if GSE demand for PLMBS moved the market as a whole, the goals were either not binding or the GSEs were satisfying their affordable housing goals in the prime sphere of the mortgage market.

It is possible that regulators were not astute enough to realize that the GSEs purchased substantially more PLMBS than were necessary to satisfy the goals, or that loans in the PLMBS market were not rich in the borrower-specific goals which were likely more difficult for the GSEs to satisfy. The GSEs might have been able to successfully argue that they had no choice but to purchase substantial quantities of PLMBS to meet their affordable housing goals. However, this is a failure of regulators to understand the mortgage market, and the incentives of the entities they regulate, rather than the goals themselves causing the subprime crisis. The GSEs likely would have been successful in subverting their regulator even if the affordable housing goals were set at much lower levels.

Similarly, the GSEs may have used their AHGs to convince regulators to permit them to change their underwriting standards to include low documentation and no documentation loans

as Calomiris (2011) argues. While our data indicates that reduced documentation mortgages were used to exaggerate borrower income rather than to understate it, and were thus not used to increase affordable housing lending, it is unclear whether regulators such as the FHFA were aware that this was the case. While we are not able to quantify the effect of the change in the GSEs underwriting policy, it seems very likely that the change encouraged risky lending both by the GSEs and other market participants.

Legislators may also have been hesitant to enact legislation to reduce risky lending during the boom for fear of reducing the supply of affordable housing. While such a regulatory failure is not directly due to the affordable housing legislation that we study, the attitude may have contributed to the subprime boom.

Finally, we note that our paper does not provide any proof of welfare benefits of Federal affordable housing legislation. When the goals are binding, they may reallocate resources in an inefficient manner and there may be more effective ways to achieve the intent of the legislation (e.g., cash transfers to low income borrowers). Even when the goals are not binding, the sheer paperwork involved in compliance imposes costs on lenders that are surely passed on to borrowers in some form.

6 Conclusions

In this paper we examined the effect of affordable housing legislation on the volume, pricing, and performance of subprime mortgages originated in California and Florida in 2004 through 2006. Using a regression discontinuity approach, we find no evidence that the affordable housing goals of the CRA or of the GSEs affected any of these outcome measures. This finding is robust to the inclusion of alternative controls, to the sample of only full documentation loans, and to different bandwidths for the regression discontinuity specification. While it is unquestionable that Fannie Mae and Freddie Mac held substantial amounts of subprime mortgages, and that their holdings of these securities played a significant role in their demise, the evidence in this paper refutes the claim that the affordable housing mandates were responsible for the subprime crisis. We hope our findings stimulate researchers to seek other explanations for the subprime securities boom.

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