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

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Abstract

No. In this paper we use a regression discontinuity approach to investigate whether affordable housing policies influenced origination or affected prices of subprime mortgages. We use merged loan-level data on non-prime securitized 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 the GSEs may have played a role in the crisis, our results indicate that it was not due to their affordable housing mandates.

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1 Introduction

Congress has enacted laws to encourage lending to low-income households and households that have historically been excluded from mortgage markets because of the neighborhoods in which they live. Some observers have argued that affordable housing policy was a causal factor in the subprime crisis and that the aim of Congress in enacting such policies is more sinister. 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." The Republican minority on the U.S. House of Representatives Committee on Oversight and Government Reform described Fannie Mae and Freddie Mac as "the central cancer of the mortgage market, which has now metastasized into the current financial crisis" (Davis, 2008). 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. - 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 private-label mortgage-backed securities (PLMBS). We show that almost 70% of such mortgages satisfied one or more of the affordable housing goals. 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.

To identify the effect of affordable housing goals, we use a regression discontinuity approach to ascertain whether the goals led to a difference in either mortgage rates or subprime loan volume. 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). 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 otherwise purchase as whole loans. Indeed, the GSEs vastly increased their purchases of PLMBS during the subprime mortgage boom and Manchester (2008) and Frame (2008) show that the GSEs generally purchased "goal rich" PLMBS during the subprime boom. Depository institutions may also count PLMBS toward their CRA goals provided the MBS are structured as CRA-qualified securities.

In contrast to studies of the effect of affordable housing goals in the prime market (e.g., Ambrose and Thibodeau, 2004; Bhutta, 2010, 2011), 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 possible that the GSEs encouraged subprime lending by purchasing large quantities of PLMBS. However, our results indicate that either any role the GSEs played in the subprime crisis was not due to their affordable housing mandates or that the GSEs' demand for these securities was not sufficient to affect the overall PLMBS market.

Our approach differs from the existing literature on the effect of affordable housing policy in three important respects. 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 using loan level data rather than simply the neighborhood-level goals. 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. Tract-level goals can be studied using originations per tract as an outcome measure. However, 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.

Second, the majority of subprime loans were securitized such that a complete picture of the effect of affordable housing legislation requires an evaluation of the effect of affordable housing policies on securitized loans. 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) identifying strategy focuses on differences in loan originators rather than the final holder of the loan. Similarly, Reid and Laderman (2011) study whether CRA-regulated institutions are more likely to originate high-cost loans than institutions not covered by the CRA. In contrast to Reid and Laderman (2011), our approach to identifying the effect of the CRA uses a regression discontinuity approach. We use this approach because CRA-regulated lenders may 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.

Finally, our matched dataset allows us to focus specifically on subprime mortgages whereas most of the existing literature (e.g., Ambrose and Thibodeau, 2004; Berry and Lee. 2008; Bhutta, 2010, 2011) has primarily studied the effects of affordable housing policy on the overall mortgage market or other housing market outcomes (e.g., home ownership rates by Bostic and Gabriel, 2006). An important exception is the study by Reid and Laderman (2011) which uses high-cost loans as a proxy for subprime loans.

Our paper contributes to a growing literature exploring the causes of the housing boom that preceded the financial crisis of 2007-2008. 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, 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. Finally, Piazzesi and Schneider (2009) and Burnside, Eichenbaum, and Rebelo (2011) study the effect of optimistic beliefs.

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 results in Section 4. Section 5 offers some discussion and 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.[1] 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.

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, although the GSEs are given specific targets for each of the three goal areas (UAG, SAG, and LMIG) 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.

[1] 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 (2010) and Ghent, Hernández-Murillo, and Owyang (2012).

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.

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), Garmaise and Natividad (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).[2] 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]The correlation between X and Z does not affect the estimation of the treatment effect.

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.

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.

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).[3] 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.[4] A summary of the various stages is as follows:

- 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.

[3]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.

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

- 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.

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. 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 and Garmaise, 2012), 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

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
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
Number of Loans	722,157				

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.

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 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 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

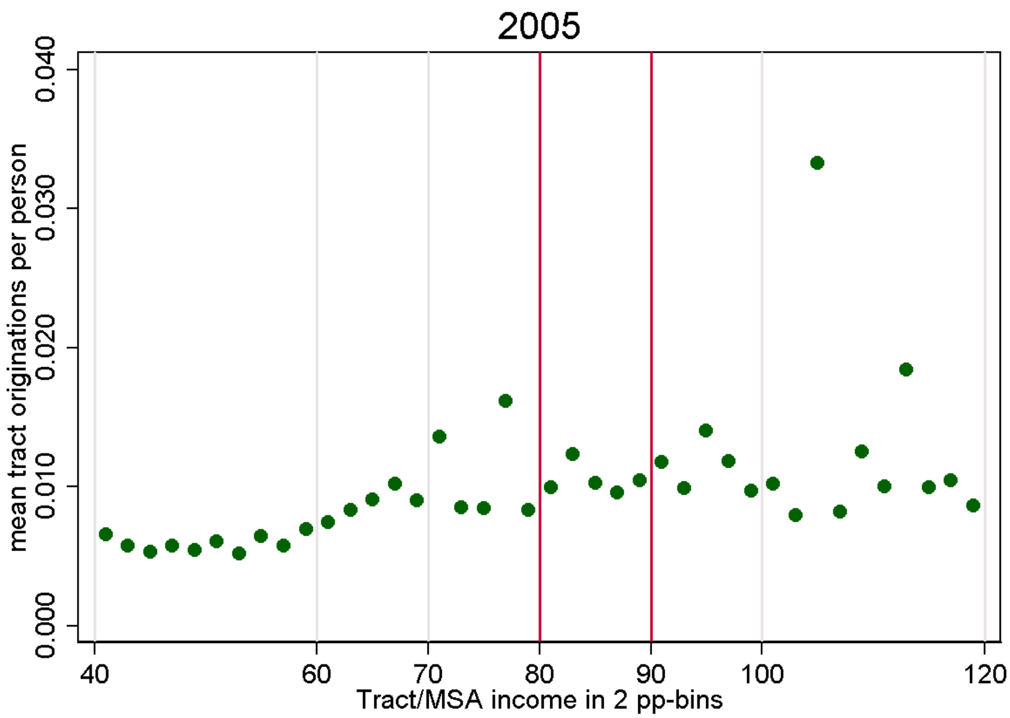

Figure 1: Effect of CRA1 and GSEs' UAG2 on Origination Volumes
Each dot represents observations in 2 percentage point intervals ranging from 40% to 120% of Census Tract/MSA Income ratio. For example, the 79% dot represents the data between 78% and the 80% cutoff. Similarly, the 81% dot represents observations 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).

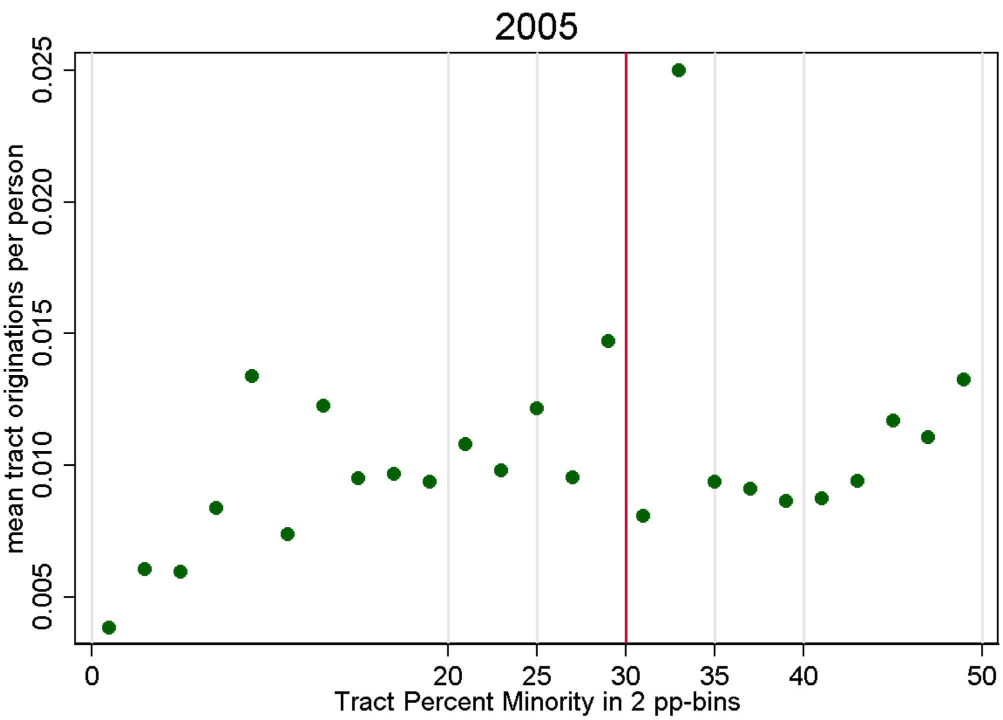

Figure 2: Effect of GSEs' UAG1 on Origination Volumes

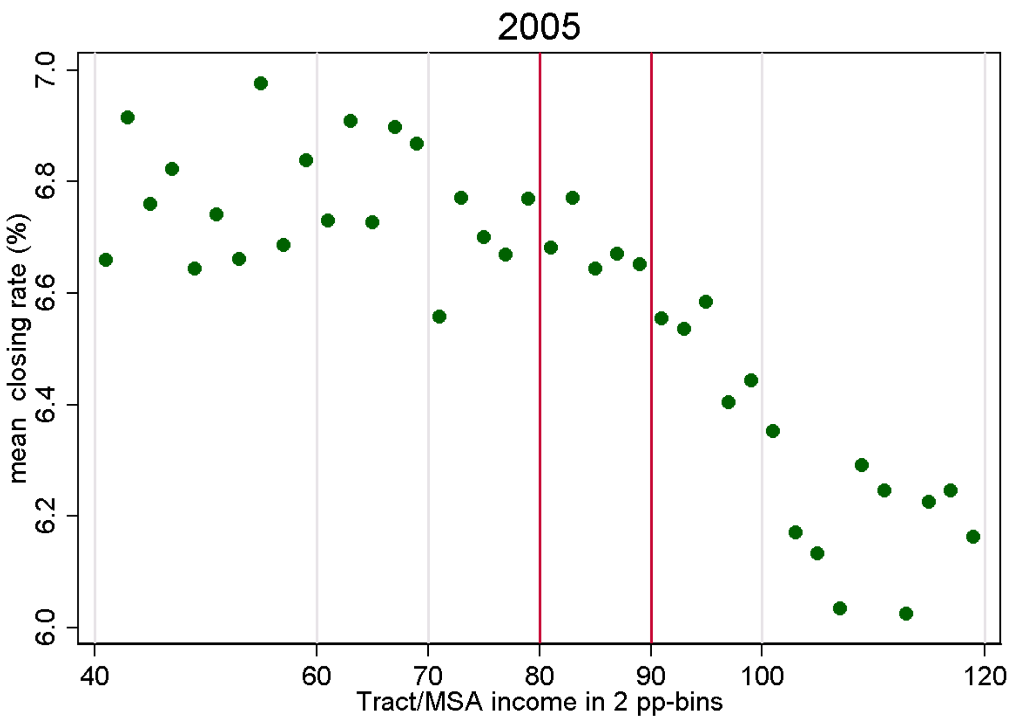

Figure 3: Effect of CRA1 and GSEs' UAG2 on Contract Interest Rates

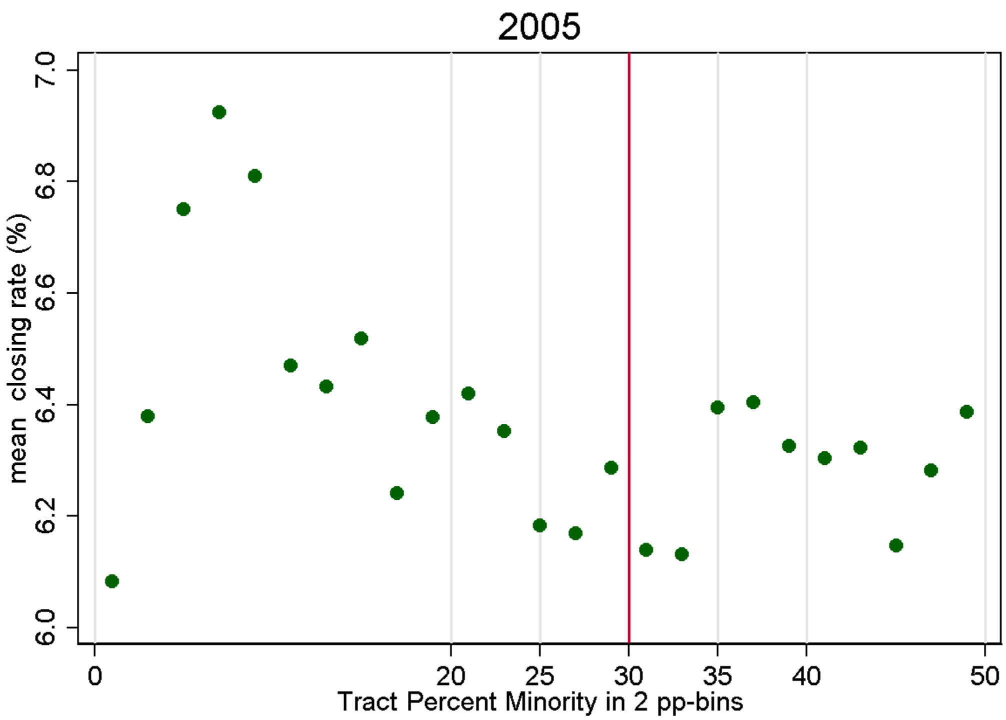

Figure 4: Effect of GSEs' UAG1 on Contract Interest Rates

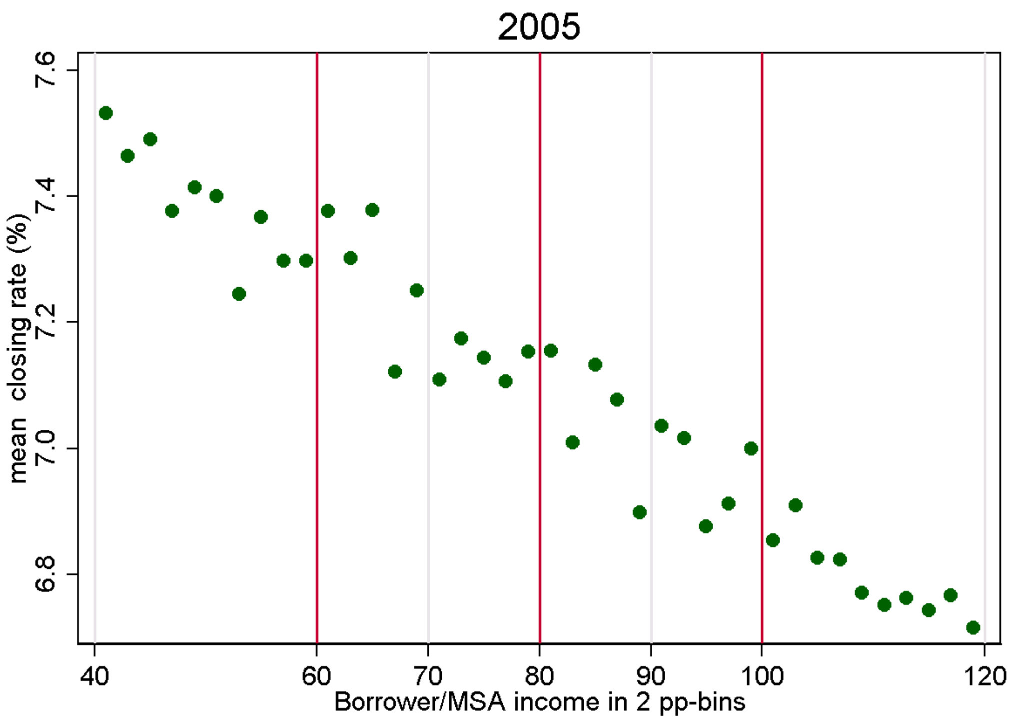

Figure 5: Effect of CRA2 and GSEs' SAG1, SAG2, and LMIG on Contract Interest Rates

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 $\leq$ 0.8)	0.00036	0.0085	-	Yes	3.5%	1,547
	(0.00055)	(0.0240)				
CRA1 (tract/MSA income $\leq$ 0.8)	0.00039	0.0105	0.0016***	Yes	4.1%	1,547
	(0.00055)	(0.0240)	(0.0005)			
UAG1 (tract minority share $\geq$ 0.3)	-0.00144	-	-0.0215	Yes	2.3%	1,145
	(0.0011)		(0.0487)			
UAG1 (tract minority share $\geq$ 0.3)	-0.00157	0.0028*	-0.0139	Yes	2.5%	1,145
	(0.00111)	(0.0016)	(0.0488)			
UAG2 (tract/MSA income $\leq$ 0.9)	0.00027	0.0152	-	Yes	3.3%	1,399
	(0.00058)	(0.0252)				
UAG2 (tract/MSA income $\leq$ 0.9)	0.00033	0.0165	0.0021***	Yes	4.2%	1,399
	(0.00057)	(0.0250)	(0.0006)			

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.

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.[5]

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

[5]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 for whether the loan had a prepayment penalty.

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)				

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.

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

As discussed above, the regression discontinuity approach may be sensitive to the choice of the bandwidth and the manipulation of reported income in response to the affordable housing program goals. In this section, we consider whether these issues affect our results. First, we reestimate the discontinuity models with alternative bandwidths. We then address the possibility of income manipulation by focusing only on full documentation loans.

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

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)				

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.

and Lemieux, 2010). In our case, the assignment variable may be either the income of the Census tract, the income of 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

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
0.084	0.057	0.101*	Yes	No
(0.051)	(0.087)	(0.058)		
-0.005	-0.023	0.009	Yes	Yes
(0.039)	(0.064)	(0.045)		
-0.113*	-0.089	-0.124*	Yes	No
(0.062)	(0.129)	(0.065)		
-0.049	-0.114	-0.028	Yes	Yes
(0.046)	(0.091)	(0.051)		
-0.076	-0.368	0.092	Yes	No
(0.155)	(0.270)	(0.201)		
-0.040	-0.067	0.004	Yes	Yes
(0.116)	(0.198)	(0.161)		
0.166***	0.306***	0.015	Yes	No
(0.053)	(0.084)	(0.059)		
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.

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.

Finally, we note that, although it is theoretically possible that borrower incomes were misrepresented 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 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. 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 reestimated 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 Discussion

Our analysis has shown no evidence of any discontinuity in either 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. We also only consider privately securitized subprime mortgages and it is possible that the affordable housing mandates had large effects on the conforming or jumbo markets. In this section, we therefore consider other reasons why it seems unlikely affordable housing mandates caused the subprime crisis.

First, 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

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.

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) and Garmaise (2012) present also indicates borrowers overstated rather than understated their incomes.

Second, 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 7 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.

Finally, it is worth noting that many other countries with different mortgage market institutions and regulations experienced a boom in housing prices around the same time as the US. Figure 6 plots the quarterly FHFA US home price index against those from other industrialized countries for which a comparable home price series is available through the BIS at a quarterly frequency. By comparison with other industrialized countries, the US home price boom actually seems quite mild. Although we do not have evidence to argue that the subprime securities boom caused the unsustainable increase in home prices in the US, it was certainly related to the boom in house prices. As Figure 6 shows, affordable housing policy is not a necessary condition for a boom in home prices.

There is some evidence that affordable housing policy modestly affected other areas of the housing market over different sample periods. For example, Bhutta (2010) studies loans

Table 7: 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%

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

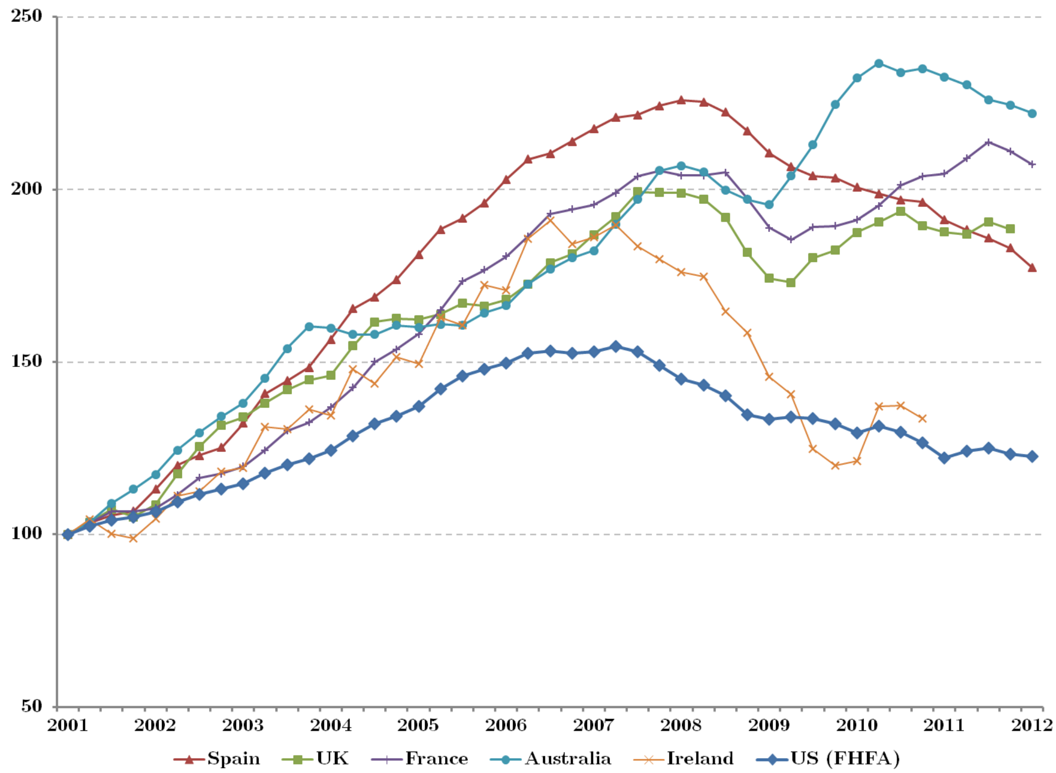

Figure 6: International Housing Prices (2001Q1 = 100)

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) finds that, over the 1994 - 2006 period, the CRA usually had a statistically insignificant impact on the overall mortgage market but that it had a small effect on lending in large cities in the late 1990s and early 2000s. Berry and Lee (2008) also use a regression discontinuity approach to study the effect of the CRA over the 1995-2002 period and find that it had no effect on overall loan volume except for in Los Angeles. In summary, although other studies have found some effects of affordable housing legislation on the mortgage market prior to the subprime boom, the evidence suggests very moderate effects.

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.

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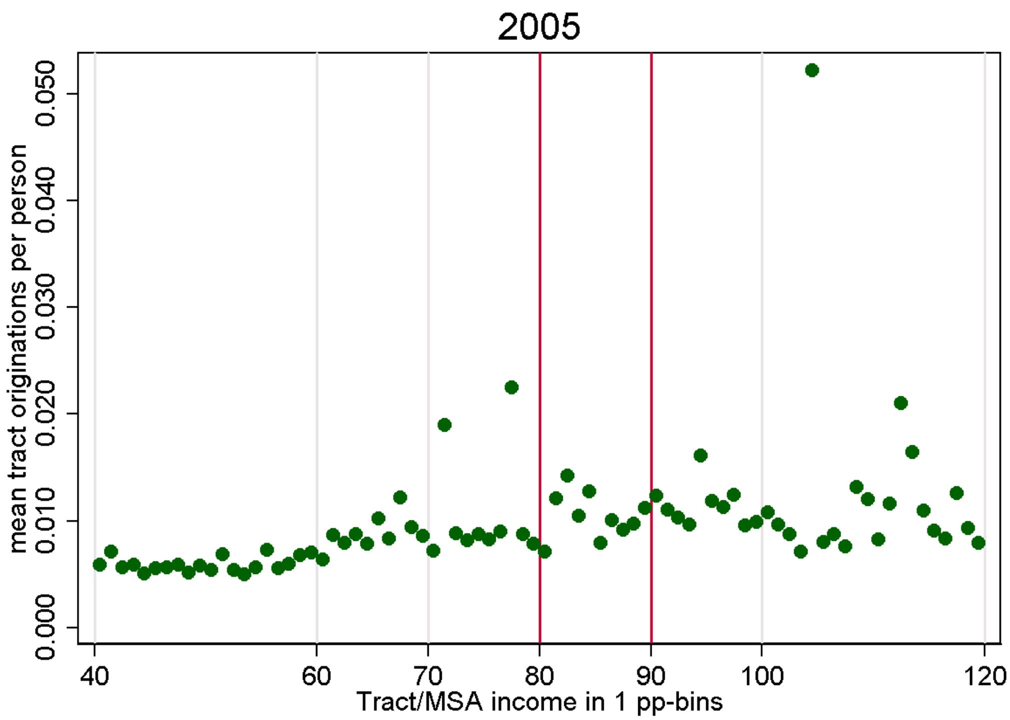
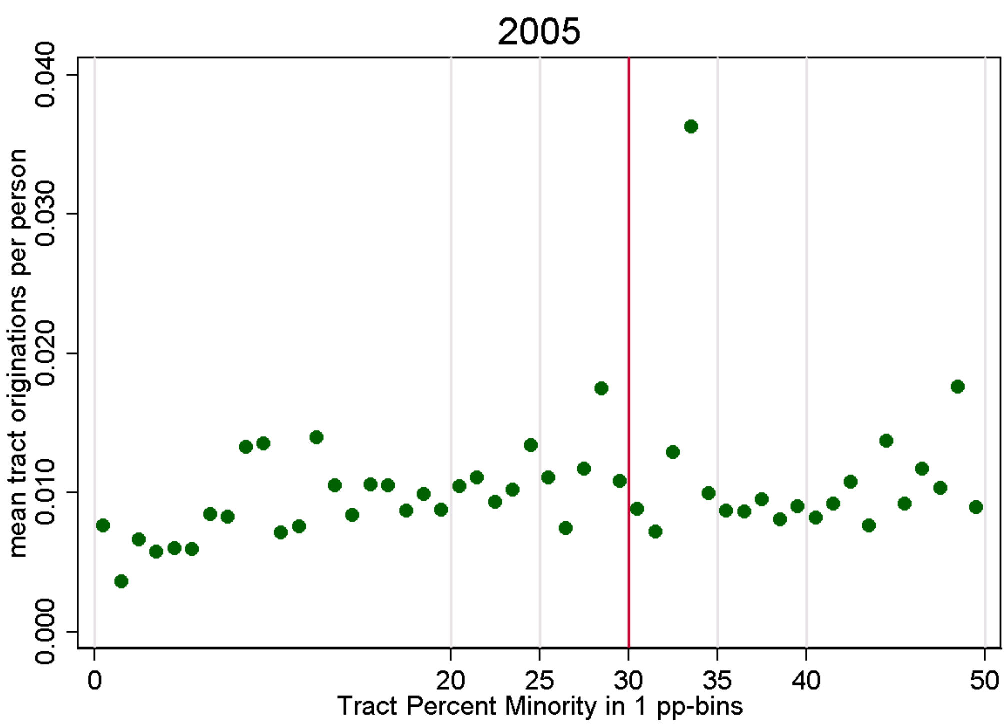
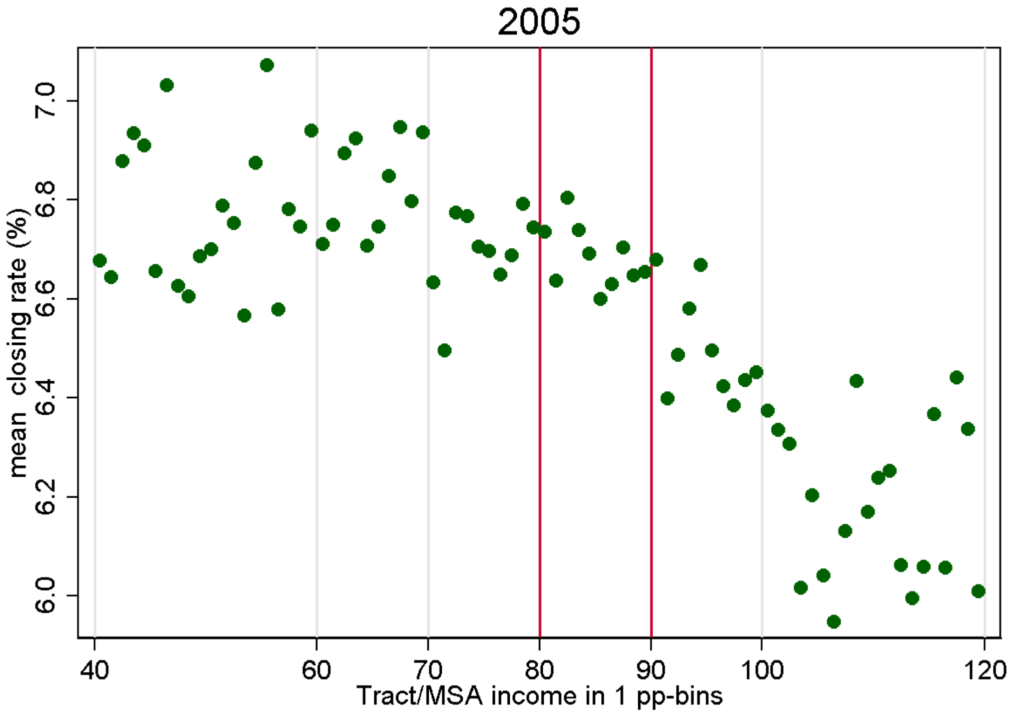
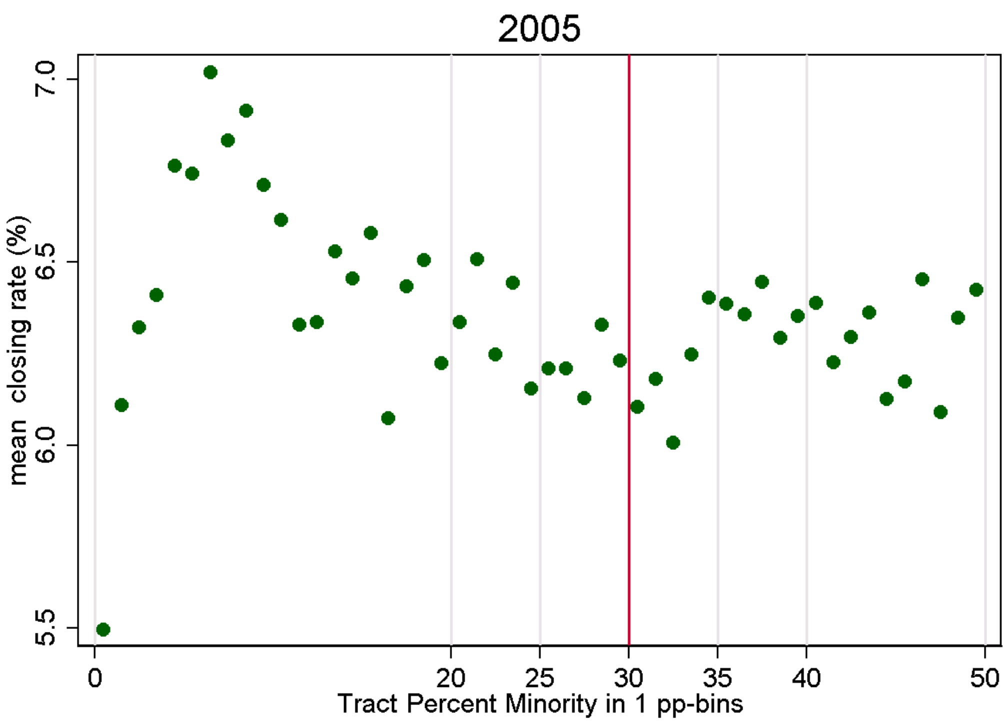
Appendix

In this appendix we present the figures and tables of the robustness checks involving 1- and 5- percentage point windows.


2005
mean tract originations per person
0.000
0.010
0.020
0.030
0.040
0.050
40
60
70
80
90
100
120
Tract/MSA income in 1 pp-bins


2005
mean tract originations per person
0.000
0.010
0.020
0.030
0.040
0
20
25
30
35
40
50
Tract Percent Minority in 1 pp-bins


2005
mean closing rate (%)
7.0
6.8
6.6
6.4
6.2
6.0
40
60
70
80
90
100
120
Tract/MSA income in 1 pp-bins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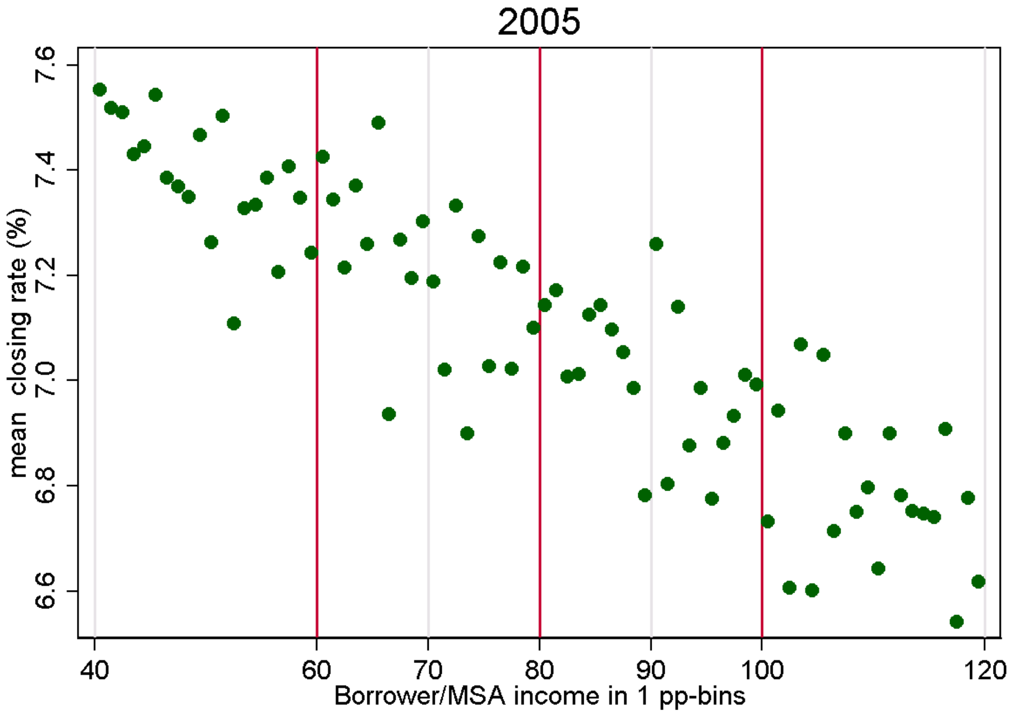
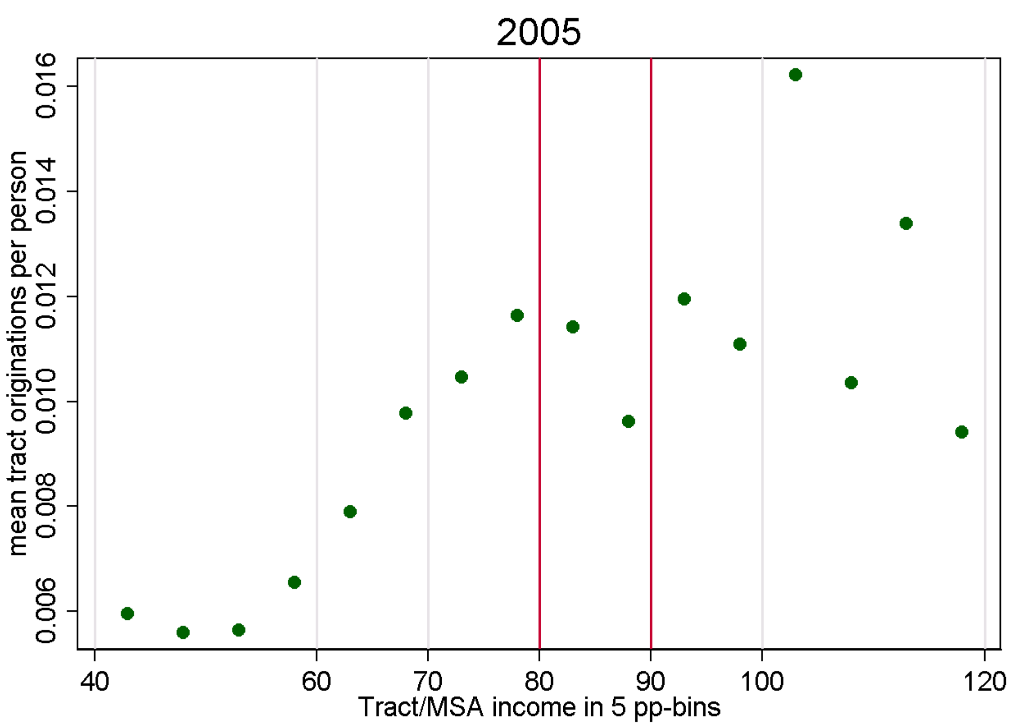
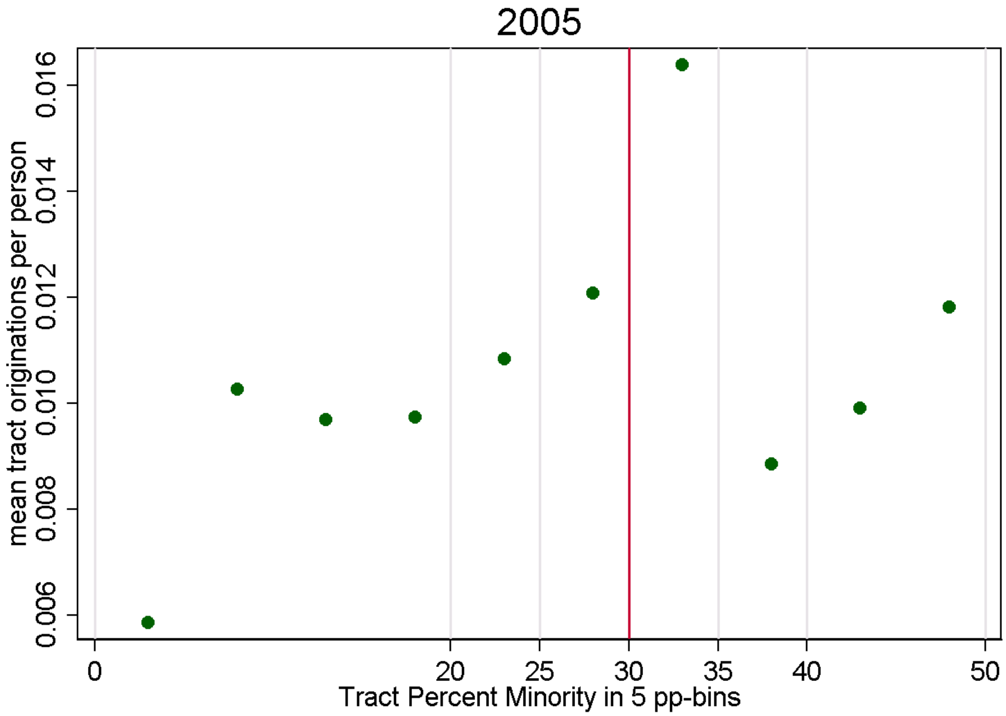
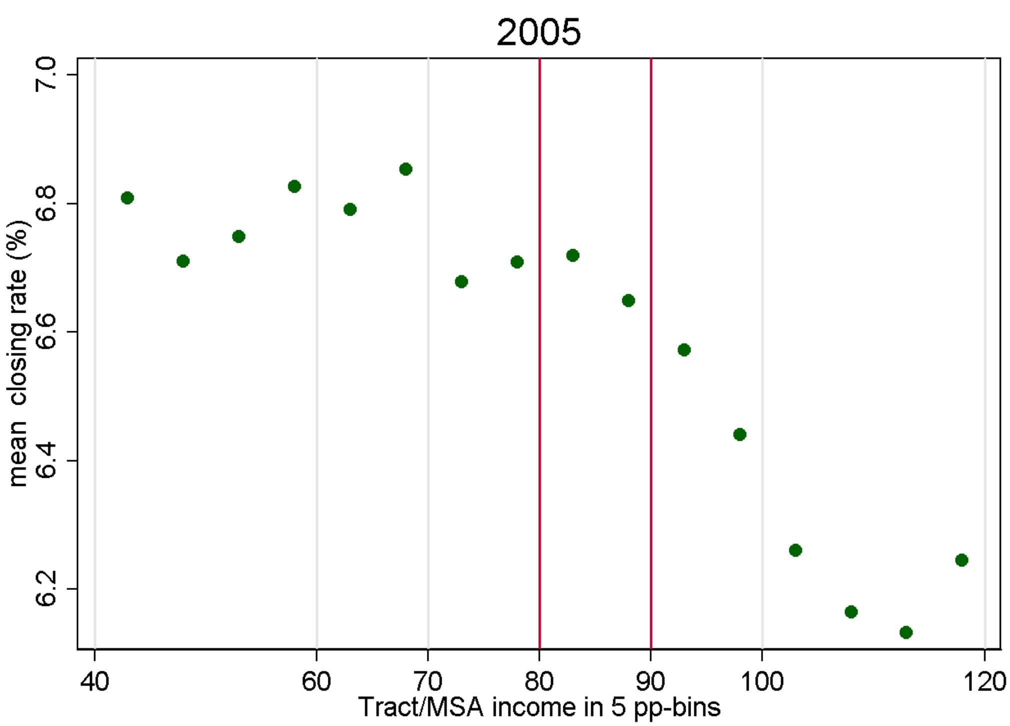
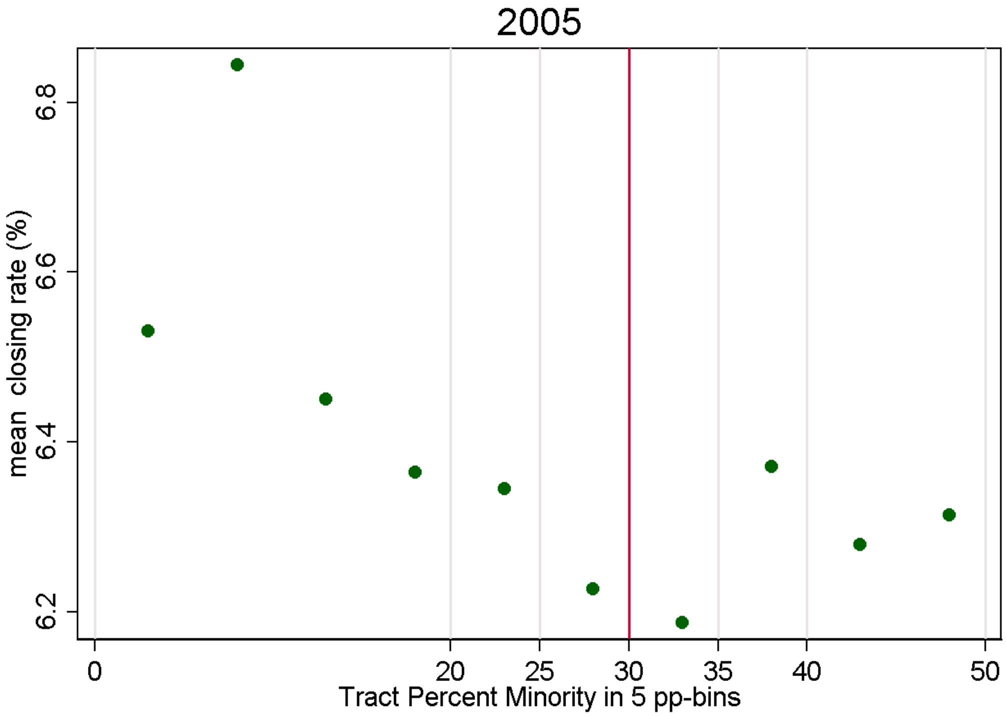
6.5
6.0
5.5
0
20
25
30
35
40
50
Tract Percent Minority in 1 pp-bins


2005
mean closing rate (%)
6.6
6.8
7.0
7.2
7.4
7.6
40
60
70
80
90
100
120
Borrower/MSA income in 1 pp-bins


2005
mean tract originations per person
0.006
0.008
0.010
0.012
0.014
0.016
40
60
70
80
90
100
120
Tract/MSA income in 5 pp-bins


2005
mean tract originations per person
0.006
0.008
0.010
0.012
0.014
0.016
0
20
25
30
35
40
50
Tract Percent Minority in 5 pp-bins


2005
mean closing rate (%)
6.2
6.4
6.6
6.8
7.0
40
60
70
80
90
100
120
Tract/MSA income in 5 pp-bins


2005
mean closing rate (%)
6.2
6.4
6.6
6.8
0
20
25
30
35
40
50
Tract Percent Minority in 5 pp-bins

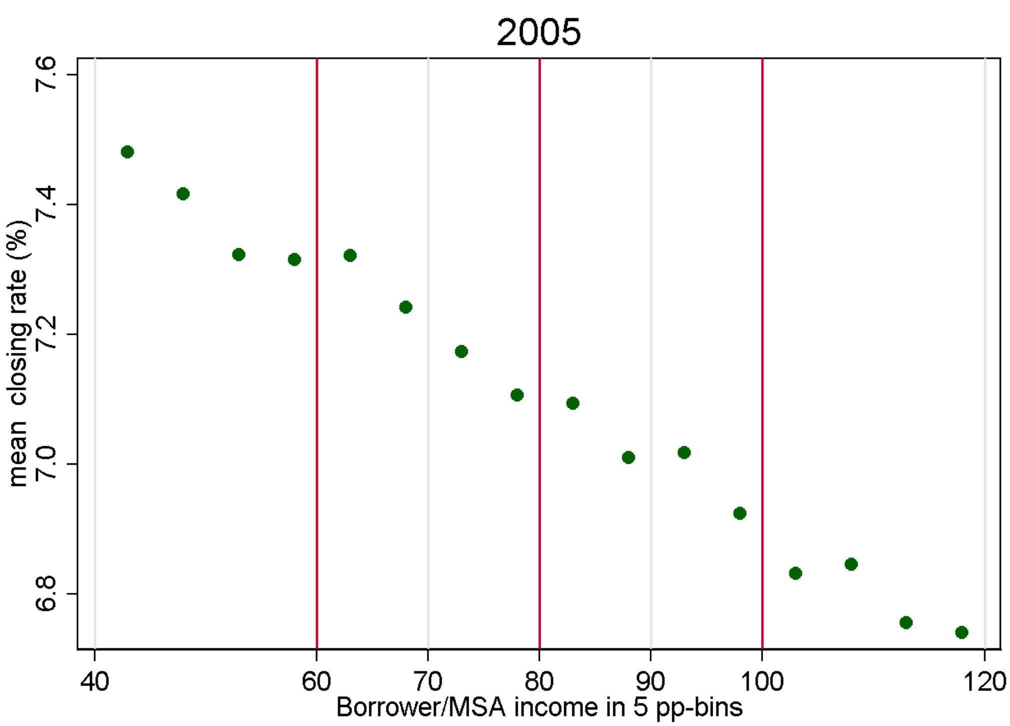

2005
mean closing rate (%)
6.8
7.0
7.2
7.4
7.6
40
60
70
80
90
100
120
Borrower/MSA income in 5 pp-bins

Table 8: Regression Discontinuity Results for Effect of Affordable Housing Goals on Originations per Tract per Year (1-Percentage-Point Window)

	Goal Dummy	Tract Income	Tract Percent Minority	Year Controls	R-Squared	No. of Obs.
CRA1 (tract/MSA income ≤ 0.8)	-0.00066	-0.124*	-	Yes	5.4%	754
	(0.00073)	(0.064)				
CRA1 (tract/MSA income ≤ 0.8)	-0.00066	-0.125*	0.0096	Yes	5.6%	754
	(0.00073)	(0.064)	(0.0007)			
UAG1 (tract minority share ≥ 0.3)	0.00076	-	-0.0705	Yes	2.9%	577
	(0.00117)		(0.1052)			
UAG1 (tract minority share ≥ 0.3)	-0.00089	0.002	-0.0616	Yes	3.1%	577
	(0.00119)	(0.002)	(0.1045)			
UAG2 (tract/MSA income ≤ 0.9)	0.00113	0.116	-	Yes	2.9%	716
	(0.00094)	(0.083)				
UAG2 (tract/MSA income ≤ 0.9)	0.00112	0.112	0.0014	Yes	3.1%	716
	(0.00094)	(0.083)	(0.0010)			

1) Standard errors are listed in parentheses.
2) Each Regression is estimated with all data for 2004 through 2006 that are within 1 percentage point 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 9: Regression Discontinuity Results for Effect of Affordable Housing Goals on Interest Rates (1-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 ≤ 0.8)	-0.025	-6.528	-	-	Yes	No	6.9%	19,134
	(0.136)	(12.800)						
CRA1 (tract/MSA income ≤ 0.8)	0.057	6.372	-	-	Yes	Yes	43.2%	19,134
	(0.063)	(5.617)						
CRA2 (borrower/MSA income ≤ 0.8)	0.263***	-	-	25.578***	Yes	No	10.4%	8,469
	(0.074)			(5.917)				
CRA2 (borrower/MSA income ≤ 0.8)	-0.004	-	-	1.434	Yes	Yes	47.0%	8,469
	(0.056)			(4.534)				
UAG1 (tract minority share ≥ 0.3)	0.556***	-	-43.08***	-	Yes	No	7.4%	17,066
	(0.142)		(13.17)					
UAG1 (tract minority share ≥ 0.3)	0.017	-	-1.236	-	Yes	Yes	41.7%	17,066
	(0.045)		(3.62)					
UAG2 (tract/MSA income ≤ 0.9)	0.042	-4.508	-	-	Yes	No	6.1%	19,704
	(0.142)	(13.04)						
UAG2 (tract/MSA income ≤ 0.9)	-0.052	-2.451	-	-	Yes	Yes	41.8%	19,704
	(0.048)	(4.36)						
SAG1 (borrower/MSA income ≤ 0.6)	0.009	-	-	5.797	Yes	No	10.4%	5,095
	(0.091)			(6.92)				
SAG1 (borrower/MSA income ≤ 0.6)	-0.044	-	-	0.814	Yes	Yes	47.6%	5,095
	(0.066)			(5.15)				
SAG2 (borrower&tract /MSA income ≤ 0.8)	0.418	12.118	-	39.607	Yes	No	19.4%	303
	(0.397)	(23.39)		(25.03)				
SAG2 (borrower&tract /MSA income ≤ 0.8)	0.178	-6.832	-	10.330	Yes	Yes	55.6%	303
	(0.288)	(16.18)		(17.50)				
LMIG (borrower/MSA income ≤ 1.0)	0.476***	-	-	27.550***	Yes	No	9.2%	10,379
	(0.081)			(6.48)				
LMIG (borrower/MSA income ≤ 1.0)	0.094	-	-	7.373	Yes	Yes	45.6%	10,379
	(0.063)			(4.96)				

1) Standard errors are listed in parentheses.
2) Each regression is estimated with all data for 2004 through 2006 that are within 1 percentage point 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 dummy for full documentation, 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 10: Probit Results for Effect of Affordable Housing Goals on Default Likelihood (1-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)	-0.028**	-2.609*	-	-	Yes	No	10.4%	19,134
	(0.014)	(1.56)						
CRA1 (tract/MSA income ≤ 0.8)	-0.019*	-1.656	-	-	Yes	Yes	17.4%	19,134
	(0.012)	(1.29)						
CRA2 (borrower/MSA income ≤ 0.8)	0.004	-	-	1.416	Yes	No	8.4%	8,469
	(0.015)			(1.21)				
CRA2 (borrower/MSA income ≤ 0.8)	-0.012	-	-	-0.217	Yes	Yes	15.3%	8,469
	(0.014)			(1.17)				
UAG1 (tract minority share ≥ 0.3)	0.012	-	-1.938**	-	Yes	No	9.8%	17,066
	(0.010)		(0.98)					
UAG1 (tract minority share ≥ 0.3)	-0.010	-	0.147	-	Yes	Yes	18.0%	17,066
	(0.009)		(0.71)					
UAG2 (tract/MSA income ≤ 0.9)	0.016	-0.310	-	-	Yes	No	9.4%	19,704
	(0.014)	(1.34)						
UAG2 (tract/MSA income ≤ 0.9)	0.006	-0.739	-	-	Yes	Yes	16.0%	19,704
	(0.012)	(1.20)						
SAG1 (borrower/MSA income ≤ 0.6)	0.033*	-	-	2.021	Yes	No	7.2%	5,095
	(0.020)			(1.49)				
SAG1 (borrower/MSA income ≤ 0.6)	0.024	-	-	1.098	Yes	Yes	12.1%	5,095
	(0.019)			(1.47)				
SAG2 (borrower&tract /MSA income ≤ 0.8)	-0.028	-1.966	-	-0.002	Yes	No	14.6%	259
	(0.069)	(5.16)		(0.003)				
SAG2 (borrower&tract /MSA income ≤ 0.8)	0.030	-1.001	-	0.003	Yes	Yes	28.2%	259
	(0.065)	(4.74)		(0.004)				
LMIG (borrower/MSA income ≤ 1.0)	0.013	-	-	1.900	Yes	No	9.6%	10,379
	(0.015)			(1.20)				
LMIG (borrower/MSA income ≤ 1.0)	0.002	-	-	1.577	Yes	Yes	16.1%	10,379
	(0.015)			(1.17)				

1) Standard errors are listed in parentheses.
2) Each probit is estimated with all data for 2004 through 2006 that are within 1 percentage point 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 dummy for full documentation, 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.

Table 11: Regression Discontinuity Results for Effect of Affordable Housing Goals on Originations per Tract per Year (5-Percentage-Point Window)

	Goal Dummy	Tract Income	Tract Percent Minority	Year Controls	R-Squared	No. of Obs.
CRA1 (tract/MSA income $\leq$ 0.8)	0.00079*	0.014*	-	Yes	2.6%	4,020
	(0.00045)	(0.008)				
CRA1 (tract/MSA income $\leq$ 0.8)	0.00081*	0.016**	0.0020***	Yes	3.1%	4,020
	(0.00045)	(0.008)	(0.0004)			
UAG1 (tract minority share $\geq$ 0.3)	-0.00197**	-	0.0353***	Yes	1.5%	2,823
	(0.00079)		(0.0134)			
UAG1 (tract minority share $\geq$ 0.3)	-0.00200**	0.004***	0.0375***	Yes	2.0%	2,823
	(0.00079)	(0.001)	(0.0134)			
UAG2 (tract/MSA income $\leq$ 0.9)	0.00037	0.017**	-	Yes	4.3%	3,511
	(0.00038)	(0.007)				
UAG2 (tract/MSA income $\leq$ 0.9)	0.00041	0.018***	0.0015***	Yes	4.7%	3,511
	(0.00038)	(0.007)	(0.0004)			

1) Standard errors are listed in parentheses.
2) Each Regression is estimated with all data for 2004 through 2006 that are within 5 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 12: Regression Discontinuity Results for Effect of Affordable Housing Goals on Interest Rates (5-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 ≤ 0.8)	0.059	1.010	-	-	Yes	No	6.1%	106,035
	(0.059)	(0.991)						
CRA1 (tract/MSA income ≤ 0.8)	0.014	0.188	-	-	Yes	Yes	42.8%	106,035
	(0.022)	(0.370)						
CRA2 (borrower/MSA income ≤ 0.8)	0.045	-	-	0.327	Yes	No	8.6%	41,217
	(0.031)			(0.528)				
CRA2 (borrower/MSA income ≤ 0.8)	-0.020	-	-	0.204	Yes	Yes	46.8%	41,217
	(0.023)			(0.391)				
UAG1 (tract minority share ≥ 0.3)	0.403***	-	-3.75***	-	Yes	No	6.2%	93,982
	(0.081)		(1.24)					
UAG1 (tract minority share ≥ 0.3)	0.034	-	-0.54	-	Yes	Yes	41.2%	93,982
	(0.028)		(0.43)					
UAG2 (tract/MSA income ≤ 0.9)	0.102	0.293	-	-	Yes	No	5.7%	98,203
	(0.068)	(1.23)						
UAG2 (tract/MSA income ≤ 0.9)	-0.006	-0.080	-	-	Yes	Yes	41.8%	98,203
	(0.022)	(0.38)						
SAG1 (borrower/MSA income ≤ 0.6)	-0.028	-	-	-0.392	Yes	No	8.4%	24,999
	(0.037)			(0.64)				
SAG1 (borrower/MSA income ≤ 0.6)	-0.027	-	-	0.772	Yes	Yes	47.0%	24,999
	(0.028)			(0.49)				
SAG2 (borrower&tract /MSA income ≤ 0.8)	0.023	0.446	-	0.525	Yes	No	9.4%	7,713
	(0.058)	(0.86)		(0.72)				
SAG2 (borrower&tract /MSA income ≤ 0.8)	0.001	0.067	-	0.858	Yes	Yes	47.2%	7,713
	(0.042)	(0.53)		(0.52)				
LMIG (borrower/MSA income ≤ 1.0)	0.133***	-	-	1.497***	Yes	No	8.6%	48,468
	(0.030)			(0.52)				
LMIG (borrower/MSA income ≤ 1.0)	-0.007	-	-	-0.077	Yes	Yes	46.7%	48,468
	(0.023)			(0.39)				

1) Standard errors are listed in parentheses.
2) Each regression is estimated with all data for 2004 through 2006 that are within 5 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 dummy for full documentation, 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 13: Probit Results for Effect of Affordable Housing Goals on Default Likelihood (5-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 $\leq$ 0.8)	0.0008	0.0381	-	-	Yes	No	9.7%	106,035
	(0.0065)	(0.106)						
CRA1 (tract/MSA income $\leq$ 0.8)	-0.0008	-0.0057	-	-	Yes	Yes	16.2%	106,035
	(0.0057)	(0.091)						
CRA2 (borrower/MSA income $\leq$ 0.8)	-0.0060	-	-	-0.056	Yes	No	7.4%	41,217
	(0.0065)			(0.11)				
CRA2 (borrower/MSA income $\leq$ 0.8)	-0.0071	-	-	-0.081	Yes	Yes	13.2%	41,217
	(0.0062)			(0.11)				
UAG1 (tract minority share $\geq$ 0.3)	0.0088	-	-0.017	-	Yes	No	9.1%	93,982
	(0.0057)		(0.09)					
UAG1 (tract minority share $\geq$ 0.3)	-0.0043	-	0.103	-	Yes	Yes	17.2%	93,982
	(0.0043)		(0.07)					
UAG2 (tract/MSA income $\leq$ 0.9)	0.0190***	0.2493**	-	-	Yes	No	9.3%	98,203
	(0.0068)	(0.11)						
UAG2 (tract/MSA income $\leq$ 0.9)	0.0145**	0.2348**	-	-	Yes	Yes	16.3%	98,203
	(0.0058)	(0.094)						
SAG1 (borrower/MSA income $\leq$ 0.6)	-0.0016	-	-	0.009	Yes	No	5.1%	24,999
	(0.0086)			(0.146)				
SAG1 (borrower/MSA income $\leq$ 0.6)	-0.0023	-	-	-0.011	Yes	Yes	10.9%	24,999
	(0.0084)			(0.143)				
SAG2 (borrower&tract /MSA income $\leq$ 0.8)	-0.0199**	-0.0774	-	-0.002***	Yes	No	8.7%	7,713
	(0.0100)	(0.155)		(0.000)				
SAG2 (borrower&tract /MSA income $\leq$ 0.8)	-0.0114	-0.0555	-	0.001	Yes	Yes	14.2%	7,713
	(0.0099)	(0.151)		(0.001)				
LMIG (borrower/MSA income $\leq$ 1.0)	0.0004	-	-	0.008	Yes	No	9.0%	48,468
	(0.0059)			(0.10)				
LMIG (borrower/MSA income $\leq$ 1.0)	-0.0045	-	-	-0.090	Yes	Yes	15.8%	48,468
	(0.0057)			(0.10)				

1) Standard errors are listed in parentheses.
2) Each probit is estimated with all data for 2004 through 2006 that are within 5 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 dummy for full documentation, 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.