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GSE Activity and Mortgage Supply in Lower-Income and Minority Neighborhoods: The Effect of the Affordable Housing Goals

I estimate the credit supply effect of the Underserved Areas Goal (UAG), which establishes GSE purchase goals for mortgages to lower-income and minority neighborhoods. Taking advantage of discontinuous census tract eligibility rules and abrupt changes in tract eligibility in 2005, I find evidence of a small UAG effect on GSE activity, and this increase does not appear to crowd-out FHA and subprime lending. The results also indicate that the GSEs exploit the law's lack of precision-targeting, yielding effects that might diverge from the law's intent.

1. Introduction

In the 1990's Congress promoted policies to increase credit access and homeownership among lower-income and minority neighborhoods and households. Beliefs that credit access and homeownership entail individual and societal benefits (DiPasquale and Glaeser 1999, Aaronson 2000, Kubrin and Squires 2004, Garmaise and Moskowitz 2006), and that discrimination (Munnell et al 1996) and information externalities (Lang and Nakamura 1992) plague credit markets motivated such actions. One such policy is the "GSE Act", which calls on Fannie Mae and Freddie Mac to devote a large share of their business (a combined $1.5 trillion in mortgage asset purchases in 2007) to "underserved" groups. In light of recent turmoil many argue that such policies pressed lenders to take undue risks.[1] In this paper I examine the first-order question of the extent to which this policy has affected these institutions' allocation of credit.

Before being conserved in 2008, Fannie Mae and Freddie Mac operated in the secondary mortgage market as private "government-sponsored enterprises" (GSEs), with implicit and explicit financial benefits from the government (CBO 2001).[2] In return for their sponsored status Congress expected the GSEs to lower mortgage interest rates for consumers and to promote

[1] In a 2008 press release Freddie Mac stated, "Our losses could have been even lower had it not been for the need to balance our exposures with our affordable housing goals." Also see Gavin (2008), White (2008), and Wallison and Calomiris (2008).

[2] Congress established the GSEs to help create a thick secondary market for mortgage originations. The GSEs do not lend directly to consumers; they purchase "conforming" mortgages – loans that conform to relatively conservative credit characteristics (i.e. not subprime mortgages) – from lenders. The GSEs then pool these mortgages and sell mortgage-backed-securities (MBS) to investors, passing along the interest and principal payments from borrowers to investors less a credit guarantee fee. Investors perceive that the U.S. government backs this guarantees and therefore value it highly, giving the GSEs a major competitive advantage. The GSEs also invest in their own MBS and other private-label MBS by issuing bonds at a nearly a risk-free rate (Bernanke 2007).

credit access and homeownership for lower-income and minority groups.[3] The 1992 Federal Housing Enterprise Financial Safety and Soundness Act (or "GSE Act") formalized this latter responsibility. It instructed the Department of Housing and Urban Development (HUD) to create "Affordable Housing Goals" for the GSEs and monitor progress towards those goals.[4] The 2008 Housing and Economic Recovery Act establishing a new GSE regulator retains the goals.

Figure 1 shows changes over time in the three HUD-determined goals (thick black lines) and the GSEs' corresponding purchase shares.[5] Increases in the GSEs' goal-qualifying purchase shares tend to *precede* increases in the goal levels, suggesting the goals do not bind. Non-binding goals could result from GSE and/or political pressure on HUD to weaken the goals, or excessive concern on HUD's part of focusing GSE activity on lower-income groups. On the other hand, GSE anticipation of a rise in the goals might lead them to alter their activity prior to actual goal increases. The GSEs may also want to maintain a cushion above the goals, which would make the goals appear as though they are not binding.[6] Also, in raising the goals in 2005, HUD stated their intention to make the GSEs stretch to reach the goals (Federal Register 2004).

In this paper, I use mortgage application data collected under the Home Mortgage Disclosure Act (HMDA) and evaluate the Underserved Areas Goal's (UAG) impact on GSE purchase activity and mortgage credit flow in targeted neighborhoods.[7] A purchased loan counts towards the UAG if the loan is for an owner-occupied property in a census tract that has a median family income (MFI) less than or equal to 90 percent of the MSA MFI, or minority population share of at least 30 percent and MFI less than or equal to 120 percent of the MSA

[3] Quigley (2006) summarizes estimates of the GSEs' effect on mortgage rates, and Sherlund (2008) provides a more recent analysis.
[4] The penalty for failing to meet the goals might include bad publicity and eventual loss of Congressional support.
[5] See Figure 1 notes for complete description of each of the GSE housing goals. Notably, a single loan purchase can count towards multiple goals. The data for Figure 1 come from Manchester's analyses (2002, 2008) of GSE records.
[6] I thank Paul Calem (Freddie Mac) for making this point.
[7] I focus on this particular goal because of the compelling increases in the goal since 1996, because there are several sources of variation to help identify its impact, and to build upon existing literature.

MFI. I use these discontinuous rules to help identify the UAG's impact, in essence testing whether tracts that barely meet the eligibility criteria experience greater credit flows than tracts that barely miss being targeted. Figure 2 illustrates this strategy. The discontinuity in the non-parametrically fitted lines at the income cutoff provides initial evidence of a modest UAG-induced increase in GSE purchases between 1997 and 2002.

The statistical analysis to come indicates a discontinuity in GSE purchases of about 4 percent over this period and a (presumably) subsequent 3 percent discontinuity in "GSE-eligible" mortgage originations. Also, I do not find evidence that this increased GSE activity crowds-out GSE-*in*eligible lending (e.g. FHA and subprime), which would be undesirable in that it would weaken the goal's effect on homeownership. Discontinuity estimates at the minority share threshold for the same period seem to suffer from omitted variables bias. Nevertheless, they similarly suggest against a large UAG-effect.

I also take advantage of abrupt changes in tracts' target status that arise because of intercensal tract demographic and income changes. I use these changes to help identify the UAG's effect in 2005 and 2006, years when HUD increased the goal sharply and years that have contributed heavily to the GSEs' credit losses.[8] While loan growth between 2001/02 and 2005/06 is highly positively correlated with becoming targeted in 2005, the threshold analyses I conduct suggest this relationship, for the most part, is not causal.

I find only that a subset of newly targeted tracts – those whose relative income changed only slightly during the 1990's, but just enough to cross the threshold – experienced an increase in GSE purchases. This evidence conforms to the intuition that the GSEs would expand in

[8] For instance, 2005 and 2006 vintage mortgages account for more than 50 percent of Fannie Mae's 2008 second quarter credit losses (Fannie Mae 2008).

relatively stable neighborhoods where expansion is least costly, and suggests that the GSEs goal-related efforts conflict with the law's intent to help stabilize neighborhoods in decline.

Overall, the results indicate that the goals bind but only slightly expanded lower-income and minority credit flow since the mid-1990s. Moreover, the model I present in Section 4 indicates that the discontinuity estimates overstate the UAG's effect since the GSEs optimally reduce their activity in not-targeted neighborhoods. Under more extreme conditions, the UAG would reduce GSE activity in *both* targeted and not-targeted areas.

In the next section I discuss how I build on previous research. Then I describe the data and regression discontinuity. In Sections 4 and 5 I focus on the UAG's effect at the income eligibility threshold for the 1997-2002 and 2005-2006 periods, respectively, and at the minority population share threshold in Section 6. Finally, I summarize and discuss some important caveats and policy considerations going forward.

2. Previous Research

Quercia et al (2003) describe and simulate the potential benefits of GSE flexible lending products and find that they could have a substantive effect on minority homeownership barring crowd-out of FHA lending. More similar to this paper, An et al (2007), An and Bostic (2008) and Gabriel and Rosenthal (2008a) use census tract level variation in target status to estimate the UAG's effect on housing and credit outcomes in 2000.[9] Since the UAG level rose from 24 percent to 38 percent after 2000, I fill an important gap by incorporating post-2000 outcomes. Also, I address some identification concerns with previous work, as I now describe.

[9] Bostic and Gabriel (2006) is similar to An et al (2007), but focuses just on California. An and Bostic (2006) is similar to An and Bostic (2008), but looks at crowd-out of subprime rather than FHA lending. Ambrose and Thibodeau (2004) use MSA-level variation in population share residing in UAG-targeted tracts to identify the UAG's effect between 1995 and 1998, and find a small impact.

An et al (2007) and An and Bostic (2008) estimate the UAG's effect on neighborhood housing outcomes (e.g. housing values) and FHA market share, respectively, the latter testing for crowd-out. Both papers use a two-stage strategy, where in the first stage they estimate the link between tracts' target status and tracts' GSE-purchased share of originations. Although both papers conclude that the GSE Act substantively affects the outcomes of interest, neither paper's first-stage results provide strong evidence that target-status affects GSE activity. Gabriel and Rosenthal (2008a), focusing on total credit flow in 2000, find no evidence of a UAG effect.[10]

To mitigate omitted variables bias these papers focus on census tracts within five to ten percentage points of the relative income cutoff. But tract characteristics are highly correlated with relative income around the cutoff, and not controlling precisely for this correlation biases downward UAG-effect estimates. Indeed, An et al (2007) find that GSE market share is nearly 13 percentage points *lower* in treatment versus control tracts.[11]

Also, previous researchers have combined variation in target status stemming from differences in tract income *and* minority population share, which will generate downward bias. In other words, including all tracts with minority share above 30 percent in the treatment group makes the treatment and control groups less comparable. In this paper, I address both issues by identifying the UAG's effect solely from small differences in tract relative income, as in Figure 2. (I perform a separate, analogous analysis at the minority share threshold.)

Finally, using loan share variables yield estimates that are difficult to interpret. Take for example (1) below, which regresses tract-level FHA loan share on GSE-purchased share, similar to An and Bostic (2008):

[10] The authors suggest a finding of no effect may be because increased GSE purchasing crowds-out non-GSE purchases, as they provide evidence for in another paper (2008b).

[11] Gabriel and Rosenthal's (2008a) estimates are also negative, though not statistically significant. They use a five percentage point window, but, to my knowledge, do not control for the assignment variable.

$$(1) \qquad \left(\frac{FHA-Insured}{originations}\right)_i = \alpha + \beta\left(\frac{GSE\text{-}Purchases}{originations}\right)_i + \mathbf{X}_i\boldsymbol{\theta} + \varepsilon_i$$

Instrumenting GSE-purchase share with a UAG-treatment dummy variable as in previous research is problematic since the UAG may raise *originations*, which reduces the dependent variable even if the GSEs do not crowd-out FHA lending.

Another related issue is that HMDA data do not include GSE purchases of seasoned loans.[12] Imagine the extreme case where the GSEs react to the UAG only by purchasing seasoned loans, which nevertheless frees lenders' capital and increases *originations*. In this case, one would estimate that the UAG reduces GSE-purchased shares. With these issues in mind, I estimate separately the impact of the UAG on (1) the number of GSE purchases reported in HMDA, (2) the total number of "GSE-eligible" originations and (3) the number of "GSE-*in*eligible" (e.g. FHA, subprime) loans in targeted tracts.

3. Data & Empirical Strategy

3.1. Overview and Terminology

In this section and the next, I focus on estimating the UAG's effect at the income threshold between 1997 and 2002. In Section 6 I adapt this framework to estimate the UAG's effect at the minority share threshold.

Recall, GSE purchases of mortgages for owner-occupied properties in census tracts with a ratio of tract-to-MSA median family income less than or equal to 0.90 count towards meeting the UAG. This ratio is the "assignment" variable and I refer to it as *TM*. I estimate the UAG's impact by measuring the jump in GSE purchase and loan volume at $TM = 0.90$.

[12] "Seasoned" loans are those more than a year old. HMDA only requires lenders to provide loan sale data only for loans that are sold in the same calendar year of origination.

HUD measures *TM* using decennial Census data. Between 1994 and 2002 almost all census tracts had a constant value of *TM* based on 1990 Census data and 1993 MSA definitions.[13] In 2005, regulators re-calculated tracts' *TM* using 2000 Census data and new MSA definitions. In Section 5, I explain how I use this change to identify the UAG's effect in recent years.

3.2. Data & Summary Statistics

I generate tract-level mortgage data from lenders' records submitted under the Home Mortgage Disclosure Act (HMDA). Since 1990, HMDA has required covered lenders to provide information on individual mortgage applications; and since 1993, HMDA has covered most lenders, providing a nearly complete picture of MSA mortgage lending (Avery et al 2007).

Table 1 provides a list and short description of HMDA variables. Several are important for this analysis, including the census tract of the property where the loan is made[14]; lender ID, which I combine with data from HUD to identify subprime loans[15]; loan amount; disposition of the loan (e.g. approved); loan purpose (e.g. refinance); type of loan (FHA, VA or conventional); and the purchaser of the loan (e.g. Fannie Mae).

Although the GSE Act covers rural and urban areas, I focus on MSA census tracts since HMDA data are unreliable in rural areas (Avery et al 2007). I also exclude census tracts in Hawaii and Alaska and those in MSAs formed between 1993 and 1999 in order to maintain a constant set of geographies. I also drop tracts that in 1990 had fewer than 100 housing units, zero specified owner-occupied units, or more than 30 percent of the population living in group quarters. I also drop tracts with an extremely high (>10) or low (<0.2) number of originations

[13] The exception is tracts that are part of the few newly formed MSAs between 1994 and 2002.
[14] 1990 tract definitions apply to HMDA data from 1992 to 2002, and 2000 tract definitions apply since 2003.
[15] HUD publishes a yearly "subprime lender list", whose loans are considered subprime (www.huduser.org)

between 1997 and 2002 per (1990) owner-occupied unit. Nevertheless, I use about 98 percent of MSA census tracts within five percentage points of the GSE-eligibility income cutoff.

Table 2 provides means of tract-level mortgage activity and tract characteristics.[16] I use only owner-occupied home purchase and refinance loans from the HMDA data. Among these loans, I define "GSE-eligible" loans as conventional (i.e. not FHA- or VA- insured) originations with loan amounts below the GSE single-family conforming loan limit and not originated by a subprime lender. Despite this terminology, some "eligible" loans may not actually conform to GSE standards. Rather, eligible loans are those most likely to conform to GSE standards given the information available in the HMDA data. Panel A indicates that eligible loans account for about two-thirds of all loans across all sample tracts and the GSEs directly purchase just over 40 percent of eligible loans.[17] Table 2 also reveals that eligible mortgages in tracts just below the cutoff (first column) are about 10 percent lower than in tracts just above.

The GSEs purchase few (less than five percent) "GSE-ineligible" loans. FHA-insured mortgages and loans from subprime specialists make up most of the loans in this group; VA-insured mortgages and loans above the single-family conforming loan limit also contribute.[18]

Although tracts below the cutoff experienced less credit flow than those above in 1997-2002, initial housing and demographic characteristics (panel B) suggest that these two groups of tracts are substantively different. In other words, tract characteristics change quickly as a function of *TM* around the cutoff. Next, I describe the regression discontinuity strategy I use to

[16] I use Census tract-level data distributed by Geolytics.
[17] Non-GSEs may purchase eligible loans that are not actually conforming. Alternatively, non-GSE purchasers may be aggregators of conforming loans.
[18] GSE purchases of ineligible loans reflect several possibilities. First, the GSEs can and do purchase FHA-insured and subprime mortgages to a limited extent (Federal Register 2004). Second, subprime specialists may originate conforming loans that they sell to the GSEs. And third, some loans in HMDA above the single-family conforming loan limit may actually be for 2-4 unit structures and fall below the 2-4 unit loan limit and subsequently be eligible for purchase. HMDA only separately identifies multifamily unit (5+ units) loans.

help distinguish the UAG's independent effect from the effect of these underlying differences in tract characteristics.

3.3. Empirical Strategy: Regression Discontinuity

Consider the following tract-level regression of potential outcomes such as mortgage origination volume on a treatment indicator variable, $D_i = \mathbf{1}[TM_i \leq 0.90]$:

$$Y_i = \alpha + \beta D_i + e_i \tag{2}$$

The following expression captures the main idea of the regression discontinuity design:

$$\lim_{h \to 0} \left\{ \mathrm{E}[e_i \mid 0.90 - h \leq TM_i \leq 0.90] - \mathrm{E}[e_i \mid 0.90 < TM_i \leq 0.90 + h] \right\} = 0 \tag{3}$$

(3) implies that tracts arbitrarily close to the cutoff (TM = 0.90) are identical in expectation (except, of course, for their eligibility status). In other words, under (3) one can interpret a discontinuity in outcomes across the cutoff as a treatment effect.

One approach I take to estimate β is to compare the mean of Y_i across the cutoff using tracts within a small distance ("bandwidth"), h, from the cutoff. However, since mortgage activity is positively correlated with tract income (Table 2) such nonparametric estimates of β from will tend to be negatively biased (Porter 2003). I try to mitigate this bias by using a small bandwidth (h = 0.02), and adding tract-level covariates into the regression, including a lagged ("pre-treatment") value of the outcome variable.

Imbens and Lemieux (2008) recommend local linear regression to estimate β. This strategy controls explicitly for the correlation between lending and tract income, fitting a line to the data within a distance h on either side of the cutoff. The difference between the intercepts of these two lines gives an estimate of β, as in the following regression model:

$$Y_i = \alpha + \beta D_i + TM'_i + TM'_i * \mathbf{1}[TM_i < 0.90] + \mu_i, \tag{4}$$

where $TM'_i = TM_i - 0.90$. The term $TM'_i + TM'_i * \mathbf{1}[TM_i < 0.90]$ in (4) is the "control function". Notably, (4) only controls for *TM*. Under (3), and assuming a linear control function is correct, other controls are not necessary to estimate β consistently; the control function does the job. But including a good set of controls provides a specification check (i.e. the estimate of β should not change considerably when they are added) and improves efficiency (Imbens and Lemieux 2008).

This empirical test also assumes that there is no other reason, such as another policy, for a discontinuity in credit flows at the UAG cutoff. The Community Reinvestment Act encourages federally insured deposit institutions to provide credit in lower-income neighborhoods and is structured similarly to the UAG, but targets census tracts with *TM* below 0.80.[19]

4. Results

4.1. The Effect of the UAG on GSE Purchasing Activity

Table 3 displays various estimates of the UAG's effect on GSE purchase activity and their standard errors clustered at the MSA-level. The first three columns show "non-RD" estimates. The estimate in column 1 represents the difference in mean GSE purchases across the cutoff, adjusting only for tract size and MSA, and shows the GSEs purchase about 10 percent fewer loans in tracts with *TM* between 85 and 90 relative to those between 90 and 95.[20] After adjusting for the other tract characteristics listed in Table 2, the GSEs still purchase about 3 percent fewer loans in tracts below the cutoff (column 2). Since the GSE Act should not cause the GSEs to reduce their share of lower-income purchases, I consider these "non-RD" regression models to be misspecified.

[19] See Bhutta (2008) for an empirical evaluation of the CRA.
[20] I log-transform the outcome variables in all regressions.

As I mentioned in Section 2, some tracts with *TM* between 90 and 95 are actually targeted since their minority share in 1990 was at least 30 percent, and previous research mixes the two sources of variation in target-status, which may bias downward estimates of β. To illustrate, column 3 shows the result from using the true target-status variable, D^*, rather than D as in columns 1 and 2. The estimate does fall further below zero by about one percentage point.

Next I institute the local linear RD approach, similar to Figure 2. Controlling only for *TM* in column 4 raises the estimate substantially relative to columns 1 and 2, implying a UAG-effect of 3.4 percent. I include tract and MSA controls in column 5, and (log) GSE-purchases between 1994 and 1996 in column 6. This latter variable helps control for unobserved tract fixed effects.[21] As I mentioned earlier, including covariates should not affect the point estimate substantively if the identification assumptions are plausible. The point estimates in columns 5 and 6 are quite similar to column 4, and that in column 6 is significant at the 10 percent level.[22]

Columns 7-9 show "nonparametric" RD estimates (in that they do not control parametrically for *TM*) with a bandwidth (h) of just 0.02. The column 7 specification is identical to that in column 2, except for the difference in bandwidth. This reduction in bandwidth raises the point estimate by nearly five percentage points. Including the lag in column 8 raises the point estimate somewhat to 0.033, nearly identical to the baseline RD estimate (column 4), and is significant at the five percent level.[23]

Finally, in column 9 I use D as an instrument for D^*. Although D is an imprecise measure of tract target status since high-minority tracts are also targeted, I have used D thus far

[21] I consider 1994-1996 pre-treatment years since they come before the first goal increase in 1997. Of course, if the GSEs responded to the goals in these early years, then including this lag will reduce the estimate of β.

[22] Using triangular kernel weights that give most weight to data near the cutoff raises the point estimates in columns 5 and 6 slightly to 0.038 and 0.037, respectively. McCrary (2008) suggests comparing observation density across the cutoff to test the identification assumption. I find 49.4 (50.6) percent and 50.3 (49.7) percent of sample tracts are below (above) the cutoff within five and two percentage points, respectively, of the cutoff, suggesting that the number of tracts is balanced around the cutoff.

[23] The point estimates in column 8 rises slightly with the inclusion a linear control function.

as the regressor of interest because it is plausibly exogenous given *TM*. Instrumental variable (IV) estimation in column 9 scales the estimate in column 8 by the coefficient on *D* from a first-stage regression of *D** on *D* and the other regressors in column 8. IV raises the point estimate, as expected, by about one percentage point indicating that the UAG increased GSE purchases by just over 4 percent between 1997 and 2002. This estimate *under*states the UAG's effect to the extent that the GSEs purchase seasoned loans in response. Next, I explore how the UAG affected overall credit flow.

4.2. The UAG's Effect on Credit Flow

Table 4 shows RD estimates of the UAG's effect on eligible and non-eligible originations. To be more concise, I show only the specifications corresponding to those in columns 7,8 and 9 in Table 3. Other specifications shown earlier generate similar results. Columns 1-3 present estimates for (log) GSE-eligible originations. As before, including the lag as an independent variable yields a slightly higher and statistically significant estimate of 2.7 percent, and IV (column 3) increases the estimated effect to 3.4 percent.

If discontinuities exist at points other than *TM* = 0.90, that would confound the interpretation of the discontinuity at *TM* = 0.90. Figure 3 shows estimated discontinuities in (log) GSE-eligible originations using the specification in column 2 from Table 4 at 30 different values of *TM*. Other than the discontinuity at 0.90, there is a negative discontinuity at 0.88 and a positive discontinuity at 0.81. Although it is reasonable to expect one or two false positives, these discontinuities are somewhat disconcerting.[24]

[24] The discontinuity at 0.88 represents the conditional mean for tracts in the *TM* interval [86, 88] relative to that in the interval (88, 90] and therefore may reflect a heterogeneous UAG effect that is greatest in tracts right near the cutoff. However, I find it more reasonable to think that the effect would decline smoothly as *TM* decreases.

Finally I test whether increased GSE activity crowds out FHA and subprime lending. The GSEs' introduction of mortgage products with more flexible underwriting could displace FHA and subprime loans that typically cater to marginal borrowers (Quercia et al 2003). Alternatively, increased GSE outreach and demand for mortgages in targeted areas might shift borrowers at the margin of prime credit quality away from FHA loans and subprime lenders, as An and Bostic (2006, 2008) argue. Along the same line, a reduction in conforming loan rates due to increased GSE demand might help otherwise non-prime customers have a qualifying payment-to-income ratio. The estimates of the UAG's effect on GSE-*in*eligible lending in columns 4-6 of Table 4, however, do not provide evidence of crowd-out. All of the point estimates are positive, small (just over 1 percent) and not statistically significant.[25]

4.3. Discussion

The findings thus far demonstrate that the GSEs have some degree of market power. If not, a shift in their purchases towards targeted neighborhoods would not affect the ultimate allocation of credit. Also interesting is the absence of crowd-out. This finding does not conform to the hypothesis that the GSEs compete with FHA and subprime lenders at the margin. One possible explanation is that sharp differences exist between non-prime and marginally prime borrowers such that a small change in GSE underwriting standards or conforming loan prices does not affect the non-prime borrowers.

The 3.4 percent estimated effect on GSE-eligible lending in Table 4 translates into about 23 extra home purchase and refinance originations per tract *at the cutoff* from 1997 to 2002.[26]

[25] This test will not reveal crowd-out that occurs *within* subprime lending institutions, that is, if subprime lenders increase conforming originations and reduce subprime originations. Similarly, the net positive increase in GSE-eligible lending may mask some crowd-out within prime lenders.

[26] Tracts with $88<TM\leq 90$ averaged 679 originations in 1997-2002; deflating that amount by 0.034 is 23 originations.

Applying this number to the roughly 1100 sample tracts within two percentage points below the cutoff establishes a lower bound on the aggregate impact of the UAG of about 25,000 loans from 1997 to 2002. Less conservatively, if the UAG had a constant treatment effect in tracts with *TM* between 0.70 and 0.90, it would have generated just over 160,000 extra home purchase and refinance loans between 1997 and 2002.[27, 28]

However, the discontinuity estimates *over*state the credit expansion in targeted areas to the extent that the GSEs reduce credit supply in not-targeted neighborhoods.[29] To illustrate, consider the following model. The GSEs purchase mortgages, m, in lower- (L) and higher- (H) income neighborhoods at a price, p. They then produce securities, M, backed by these mortgages which they sell to investors for a fee, g.[30] The GSE objective function therefore is:

$$\Pi = g \cdot M - m_L \cdot p_L(m_L) + m_H \cdot p_H(m_H) \tag{5}$$

I assume that the GSEs exert some market power (i.e. $p'(m) > 0$). I also assume that fewer mortgages of a given credit quality can be made in lower-income neighborhoods at a given price. Figure 4 illustrates equilibrium in the absence of the UAG. The GSEs lower-income purchase share is $m_L^* / (m_L^* + m_H^*) \equiv A_0$.

Now imagine that HUD imposes a binding constraint, $A > A_0$, so that the GSEs maximize (5) subject to $m_L / (m_L + m_H) \geq A$. While this constraint is likely to increase GSE purchases in lower-income neighborhoods, it will reduce m_H^* since additional higher-income purchases make

[27] Just over 4.7 million loans were originated in sample tracts with *TM* between 0.70 and 0.90 in this period.

[28] The estimated UAG-effect on GSE purchases presented in Section 4.1 is only slightly larger than that for GSE-eligible originations. Since the GSEs only purchase about 40 percent of eligible originations (Table 2), these estimates indicate that the level increase in originations is actually greater than the level increase in GSE purchases. As I discussed in Section 2, one reason for this discrepancy is that the GSEs respond to the goals by purchasing seasoned loans that are not reported in HMDA (Federal Register 2004).

[29] The GSEs have discussed that their response to the goals may include reducing their purchases of mortgages that do not count towards the goals. On the other hand, their charter requires them to be willing to purchase all qualified loans and may therefore limit such a response (Federal Register 2004).

[30] For simplicity, imagine a simple production function $M = m_L + m_H$

attaining the UAG more difficult. In effect, the constraint pushes the marginal cost curves towards each other (dotted lines in Figure 4) and therefore suggests that any measured discontinuity in the data reflects a combination of increasing credit supply below the cutoff and reduced credit supply above the cutoff.[31] More pessimistically, the model suggests that if the supply elasticity of m_L is very high relative to that of m_H, the UAG could result in fewer GSE purchases in *both* neighborhoods.

5. The Underserved Areas Goal and GSE Activity in 2005 and 2006

5.1. Basic Empirical Strategy & Summary Statistics

In addition to cross-sectional variation, *TM* also varies over time. HUD updated *TM* in 2005 to reflect tract and MSA income measured in the 2000 Census and new MSA definitions. I refer to the new value as TM_{new} and the old value as TM_{old}. I use the set of tracts not targeted through 2002 (i.e. $TM_{old} > 0.90$) and the change in target status for some of them (i.e. $TM_{old} > 0.90$ and $TM_{new} \leq 0.90$) to identify the UAG's effect on credit supply in 2005 and 2006.[32]

For a sample of tracts *not* targeted through 2002, consider the following model:

$$\Delta Y_i = \alpha + \beta \Delta D_i + \Delta e_i \tag{6}$$

where Δ represents the change between 2001/02 and 2005/06 and $\Delta D_i = \mathbf{1}(TM_{i,new} \leq 0.90 \,|\, TM_{i,old} > 0.90)$. Equation (6) is not generally identified since unobserved factors could drive both changes in treatment status and changes in mortgage activity (i.e. unobserved deterioration in neighborhood quality could cause both treatment status and

[31] Interestingly, the value of *A* that maximizes lower-income GSE purchases is generally less than one; at *A*=1, the benefits to the GSEs of increasing m_H exceed the cost of also having to raise m_L in order to meet the constraint.

[32] Although HUD continued to use 1990 Census income data in 2003 and 2004 and therefore targeted the same tracts as in 2002 (except in cases due to tract boundary changes), 2000 Census income data was available and may have affected lending and business decisions in 2003 and 2004. As such, I use 2001/02 as the pre-period.

mortgage activity to change). However, ΔD is a deterministic function of TM_{new} (given that the sample is comprised of tracts with $TM_{old} > 0.90$). This observation leads to the following estimating equation:

$$\Delta Y_i = \alpha + \beta \Delta D_i + \mathrm{E}[\Delta e_i \mid TM_{i,new}] + \eta_i \tag{7}$$

where $\eta_i = \Delta Y_i - \mathrm{E}[\Delta Y_i \mid TM_{i,new}]$. The third term in (7) represents the control function. Again, this term is a function of the assignment variable, TM_{new}, that controls for *all* variables correlated with ΔD and ΔY not explicitly included in the regression.

Intuitively, this strategy aims to compare the change in lending in tracts that *just switched* treatment status to those that *almost switched*. (7) therefore merges a difference-in-difference (DD) identification strategy with an RD strategy.

Table 5 provides group means of various housing and credit flow variables for tracts that switched treatment status ("switchers") versus tracts that did not switch ("non-switchers"). I limit the sample to tracts used in the earlier analysis (see Section 2.2) with TM_{old} between 0.90 and 1.10 because most switchers (1850 of 1999 tracts) come from this group. I also exclude tracts with minority population share in 1990 above 0.30 since these would have been targeted under the UAG between 1997 and 2002. Finally, I use tracts with only minor boundary changes between 1990 and 2000 so that I can reliably compare pre and post outcomes.[33]

Not surprisingly, comparing TM_{old} and TM_{new} reveals that switchers and non-switchers had opposing income trends in the 1990's. The other tract characteristics in Table 5 also demonstrate these divergent trends. These differential pre-trends underline the importance of implementing the RD strategy to isolate the effect of switching into the treatment group.

[33] I use the Census' population-based relationship file to link 1990 census tracts to their 2000 counterpart. I link just over 85 percent of the census tracts from my 1997-2002 analysis to a specific 2000 census tract, where a link requires that at least 90 percent of the 2000 tract's population resides within the 1990 tract boundary and vice versa. Most of the unlinked tracts were relatively large in 1990 and were split up for the 2000 Census.

Panels C and D show that GSE-eligible origination volume fell between 2001/02 and 2005/06, and more so for *non*-switching tracts.[34] At the same time, ineligible lending rose, indicative of the growing subprime market share during this period.[35] GSE-purchase shares of eligible mortgages fell significantly for both switchers and non-switchers. One reason may be that an increasing number of "eligible" loans were not conforming during this period. Finally, the GSEs still purchase very few subprime-lender originated loans in 2005/06. Notably, the GSEs also purchase very few higher-priced mortgages (not shown), an alternative proxy for subprime mortgages in the HMDA data.

5.2. Results

The first three columns of Table 6 provide estimates of the effect of the UAG on GSE purchases in 2005/06. Column 1 shows that GSE purchase growth was ten percentage points higher in switching tracts, but because I use a broad window around TM_{new}=0.90 (h = 0.20) this estimate lacks a clear causal interpretation.

In column 2 I use a bandwidth of 0.05 and control for a few tract characteristics. This estimate has a clearer causal interpretation and is about 80 percent smaller than that in column 1 (0.021 versus 0.105).

If it is more costly for the GSEs to expand in sharply deteriorating tracts, then the UAG's effect on credit supply should be a function of how much the tract's income has fallen.[36] In

[34] Loan growth measures are confounded by the inclusion of junior lien loans in the data in so far as such loans grew as a share of all loans during this period.

[35] HUD did not publish a subprime lender list for 2006. I identify subprime lenders in 2006 as those in the 2005 list (194 of the 210 lenders match in 2006), plus those highly likely to be subprime specialists, which I identify as lenders that made at least 500 site-built, owner-occupied home purchase or refinance originations (including junior liens), and that at least 75 percent of these originations were "higher-priced" (i.e. *Rate Spread* reported by the lender; see Table 1). In 2005, HUD classified 85 percent of lenders fitting this description as subprime.

[36] Changes in tracts' *relative* income are highly correlated with changes in tracts' median family income *level*. The correlation in the percent change in both measures between 1990 and 2000 is 0.68 for tracts in the h = 0.05 sample.

column 3 I show the results from a regression (h = 0.05) that allows for the effect of switching into the treatment group to vary with TM_{old} :

$$\Delta Y_i = \alpha + \beta_1 \Delta D_i + \beta_2 \Delta D_i * TM'_{i,old} + \lambda TM'_{i,old} + \mathbf{X}_i \boldsymbol{\delta} + \eta_i \quad (8)$$

where $TM'_{i,old} = TM_{i,old} - 0.90$. The coefficient β_1 represents, in essence, the estimated effect of the UAG for tracts that fell from immediately above the threshold to immediately below.

The results imply that the UAG had nearly a six percent effect on GSE purchases for these "stable" tracts, while the estimate of β_2 indicates that this effect trails off by 0.5 percentage points per unit increase in TM_{old}. Columns 6 and 9 show similar results, but for GSE –eligible and –ineligible originations, respectively. Although neither estimate is statistically significant, the sign and size of the estimates point toward a modest increase in eligible originations in stable, switching tracts, and some crowding-out of FHA and subprime lending.[37]

Overall, these results do not support the notion that the sharp increase in the UAG goal level in 2005 and the relative rise in credit flow in newly targeted tracts were causally related. The evidence suggests the UAG's effect was limited to stable tracts. In so far as these stable neighborhoods are a relatively low priority for policy advocates, these results demonstrate how the broadly-targeted GSE Act might interact with the GSEs' profit-motive to yield effects that are out of line with the intent of the law.

6. Discontinuity Estimates at the Minority Population Share Eligibility Threshold

From 1997 to 2006, the Underserved Areas Goal (UAG) roughly doubled from around 20 percent to nearly 40 percent of GSE purchases. In the previous two sections, I provided evidence

[37] I also ran the same set of regressions in Tables 6 and 8 using only GSE purchases of home-purchase loans since HUD introduced a home purchase mortgage subgoal in 2005. The results are similar, if not more modest. Second, I redid the regressions in Tables 6 and 8 after excluding junior liens in the 2005/06 period to test if junior liens are driving differential growth across the cutoff. These results are basically identical to those in Table 6 and 8.

suggesting that this large change in the goal had only a small effect on credit flow in census tracts just below the *income* threshold. I now look for evidence of an effect at the minority population share threshold using analogous techniques. As mentioned earlier, GSE purchases of mortgages in tracts with minority population share (*minshare*) of at least 30 percent and $TM \leq 1.20$ count toward the UAG. Since there are very few high minority tracts around the $TM = 1.20$ cutoff, I focus on the minority cutoff (i.e. $minshare = 0.30$).

I first test for a discontinuity in lending at $minshare = 0.30$ between 1997 and 2002, similar to the exercise presented in Section 3. Table 7 provides the main results. The sample for the test includes tracts within 3 percentage points of the *minshare* cutoff (i.e. $h = 0.03$) and *TM* less than 1.20. As in Table 4, I show specifications excluding a control function in *minshare*.

Unfortunately, the negative estimates in Table 7 (although none are statistically significant) suggest that the model is not well identified. Again, the UAG should not cause a relative decline in targeted purchases. Reinforcing this view, I also find a negative and almost statistically significant difference in GSE-*in*eligible lending across the cutoff (not shown in Table 7) of -0.034 using the specification in column 2 of Table 7.

Although I can not precisely interpret the point estimates, they still suggest against the UAG having a large effect at the minority population share cutoff. The near equivalence of the point estimates in columns 1 and 2 implies that, conditional on the included covariates, there was no substantive difference across the cutoff in GSE-eligible lending in 1994-1996. Therefore, lending grew somewhat less quickly in targeted tracts relative to not-targeted tracts after 1996. While the UAG may have reduced the difference in growth across the cutoff, it is unlikely that the two groups of tracts are so different that credit flows would have trended in vastly different ways in the absence of the UAG.

I next estimate the UAG's effect at the minority threshold in 2005 and 2006. Table 8 shows the results of a test similar to that described in Section 5, except that I now focus on tracts that became targeted after 2005 because of an increase in their minority population share. One factor driving increases in minority population shares in the 1990's was a surge in immigration and the tendency for immigrants to settle in immigrant neighborhoods (Cutler et al 2008).

Again, I use a set of tracts not targeted prior to 2002: tracts with TM_{old} between 0.90 and 1.30 and with minority population share in 1990 ($minshare_{old}$) between 0.15 and 0.30. Then for this test, $\Delta D_i = 1$ if tract i's minority population share in 2000 ($minshare_{new}$) is at least 0.30.

Table 8 is structured identically to Table 6. Columns 1, 4 and 7 provide estimates using a broad window, controlling only for MSA, and show that tracts that crossed the minority population share threshold between 1990 and 2000 experienced relatively greater loan growth between 2001/02 and 2005/06. Unlike in Table 6, growth in FHA and subprime lending is strongly correlated with growing neighborhood minority population share (column 7).

As in Table 6, the results in columns 2, 5 and 8, which use a smaller bandwidth and therefore have a more plausible causal interpretation, are considerably smaller in magnitude. And again, the negative estimate in column 8 is consistent with crowd out, although its magnitude is very small (-0.0062). Finally, columns 3, 6 and 9 provide IV estimates where I use ΔD as an instrument for the true change in treatment status, ΔD^*. The discrepancy between ΔD and ΔD^* arises because some tracts that do not cross the minority population share threshold still become targeted by crossing the relative income threshold (i.e. $TM \leq 0.90$), and some tracts that cross the minority population share threshold do not actually become targeted because their relative income in 2000 surpasses 1.20. As expected, the IV estimates for GSE purchases and GSE-eligible loans are larger, but none are statistically significant. Overall, Table 8 provides

evidence of a modest but imprecise effect of the UAG on conforming credit flow in targeted neighborhoods.

7. Summary, Caveats and Policy Considerations

In this paper I estimate the impact of the Underserved Areas Goal (UAG) established under the GSE Act by taking advantage of discontinuities in the census tract eligibility rules. Previous research has had difficulty providing evidence of a positive effect of the UAG on credit supply. Using data on more recent outcomes to capture the potential effect of recent sharp increases in the UAG, and addressing identification concerns in previous studies, the results nevertheless suggest that the UAG has had only a limited effect on GSE purchases and total mortgage credit flow, inconsistent with the GSE Act having had a major impact on homeownership and household debt by expanding credit supply to marginal groups.

On the other hand, the other two housing goals (Figure 1) not analyzed in this paper may be more binding for the GSEs. These goals, which target mostly low-income *borrowers*, may be more difficult for the GSEs to achieve, and attaining them may lead to attainment of the UAG as well in so far as lower-income borrowers tend naturally to live in UAG-targeted neighborhoods.

This analysis might also understate the UAG's effect because the regression discontinuity strategy only identifies the goal's impact for tracts near the eligibility thresholds. While the impact in threshold tracts is small, it could be large in tracts further from the cutoff. However, the finding that the UAG mostly affects relatively stable tracts (Table 7) indicates that the GSEs respond where it is least costly. I suspect that the UAG's effect further from the cutoff is small since expansion costs are likely negatively related to tract income and minority share.[38]

[38] For instance, credit scores are generally lower in lower-income and predominantly minority neighborhoods (Board of Governors 2007).

Finally, due to data limitations I am unable to explicitly address the extent to which the UAG encouraged the GSEs to purchase private-label securities (PLS) backed largely by "goal-rich" non-prime mortgages (Manchester 2007). GSE purchases of PLS peaked in 2005 at $221 billion, compared to $951 billion in direct mortgage purchases that year (OFHEO 2008). Of course, one might expect to observe a discontinuity in subprime mortgage originations if the UAG drove recent GSE PLS purchases and subsequently the market supply of funds for mortgages, but I do not (Tables 6 and 8).

The 2008 Housing and Economic Recovery Act establishes a new GSE regulator and retains the goals, suggesting that the GSEs will reemerge as private entities with public ties. Goals that the GSEs can easily attain yield little public benefit. And easy goals may actually provide a net benefit to the GSEs. For one, the goals even if not binding can provide a convenient "excuse" to take risk. Second, attainment of well-publicized, regulator-established goals promotes a perception that the GSEs help provide certain public benefits, and this perception helps justify the institutions' sponsored status and ensuing advantageous market position. At the same time, elevated goals coupled with the GSEs' profit-motive may have undesired effects. They are more likely to channel funds to stable rather than unstable neighborhoods where society might value the funds most. Higher goals could even reduce GSE activity in targeted neighborhoods as I discuss in Section 4. Finally, elevated goals would encourage the GSEs to take risks that can simultaneously satisfy the profit-motive and the goals, such as recent subprime investments. Because of the GSEs' public ties, debt holders will exert little discipline on the GSEs and thus facilitate the acquisition of risky high-return assets.

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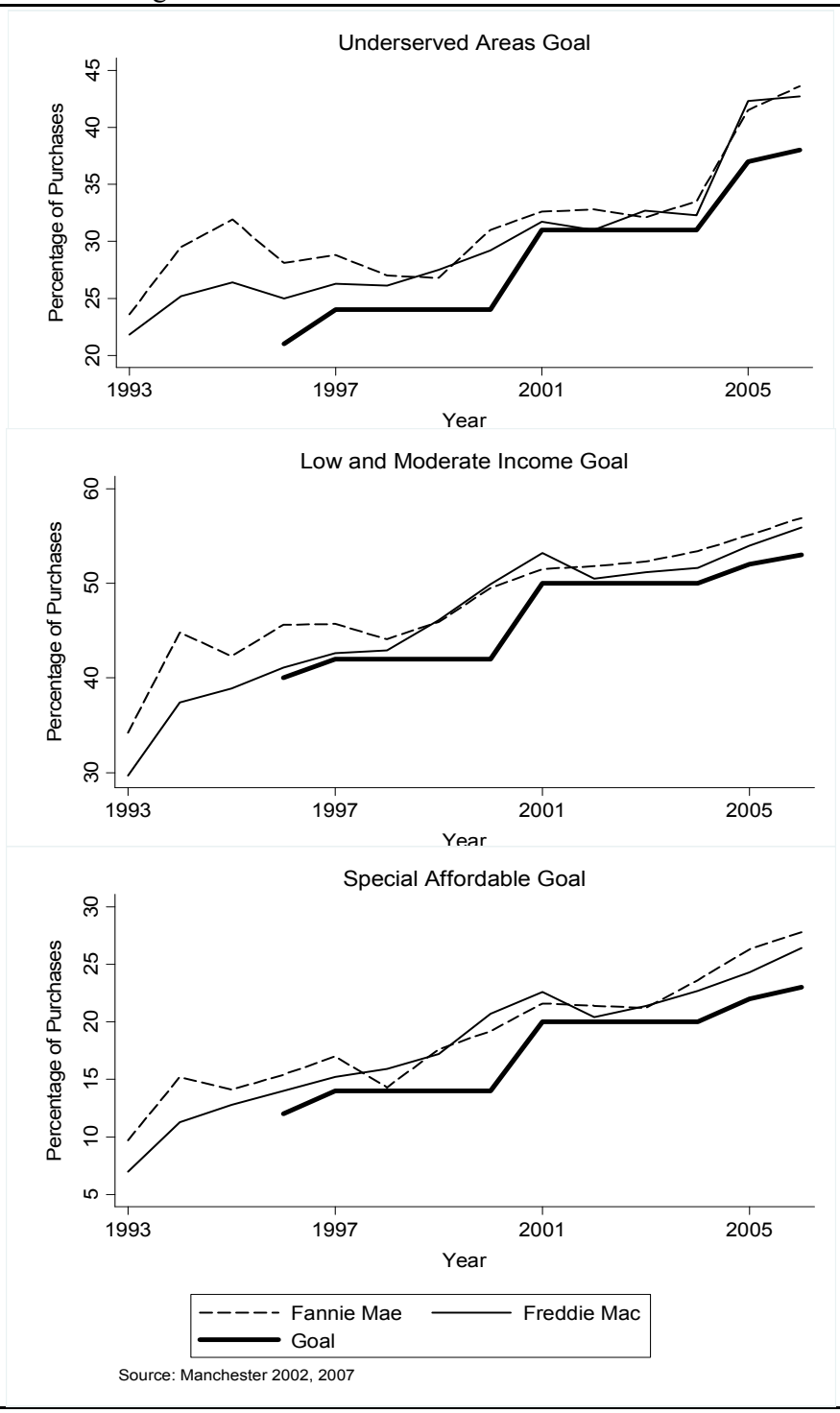
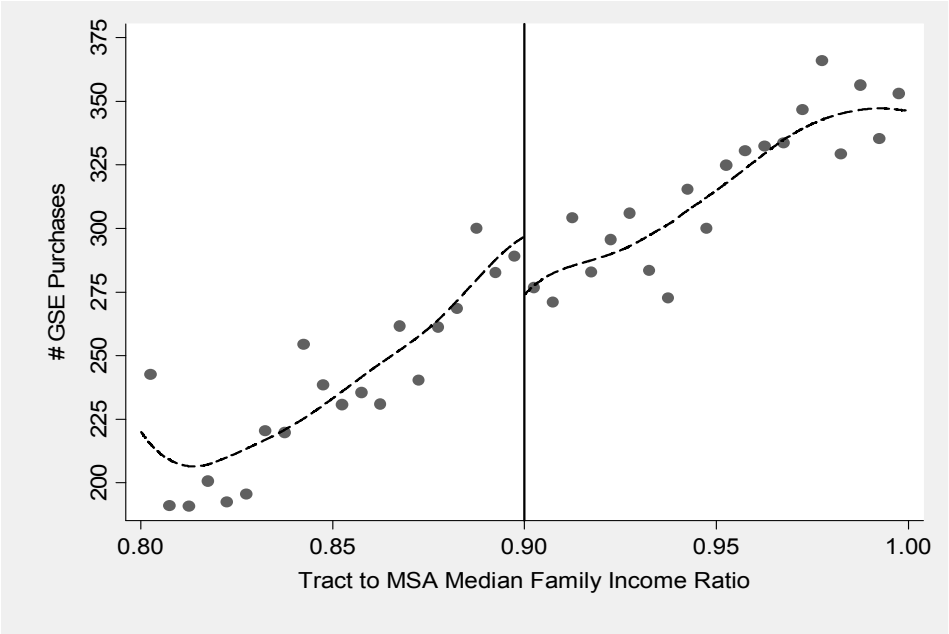
Figure 1: GSE Housing Goals and GSE Purchase Shares



Notes: The "Underserved Areas" goal targets GSE purchases in low-income & minority neighborhoods: tracts with median family income less than or equal to 90% of the MSA median family income, or tracts with a minority population share of at least 30% and a median family income no greater than 120% of the MSA median family income. The "Low and Moderate Income" goal specifies a target share of purchases to borrowers with income below the MSA median family income. The "Special Affordable" goal targets borrowers with income below 60% of the MSA median family income and borrowers with income below 80% of the MSA median family income in a census tract that has a median family income less than or equal to 80% of the MSA median family income.

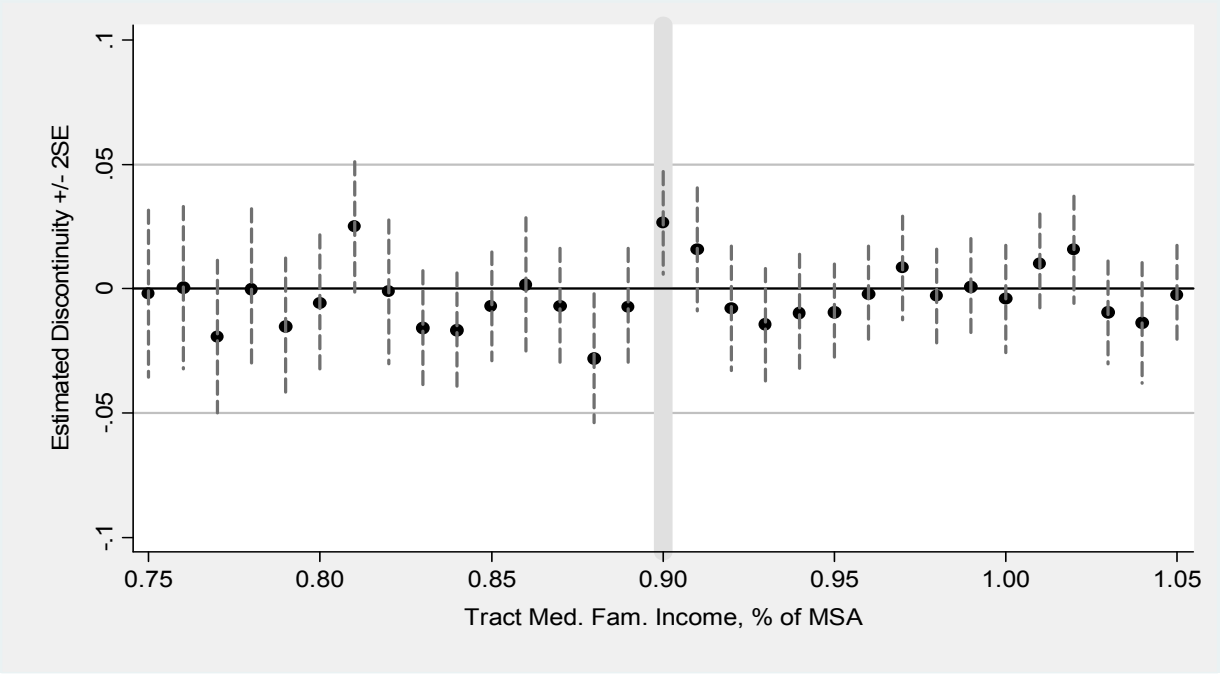
Figure 2

Discontinuity in GSE-Purchases, 1997-2002



Notes: Each data point represents the mean number of GSE purchases of GSE-eligible originations between 1997 and 2002 for tracts within half percentage point bins of the X-axis variable. Also shown are local linear regression generated fits of the underlying tract-level Y-axis values, created seperately on either side of the cutoff.

Figure 3: Falsification Exercise: Discontinuity in GSE-Eligible Originations at GSE and non-GSE Cutoffs



Notes: Each point represents the estimated discontinuity in (log) number of GSE-eligible originations in a tract between 1997 and 2002 from a seperate regression. Each regression includes tract covariates and scale variables and MSA-fixed effects, and bandwidth (h) is set to 0.02. Standard errors are clustered at MSA-level.

Figure 4: GSEs' Optimal Response to the UAG

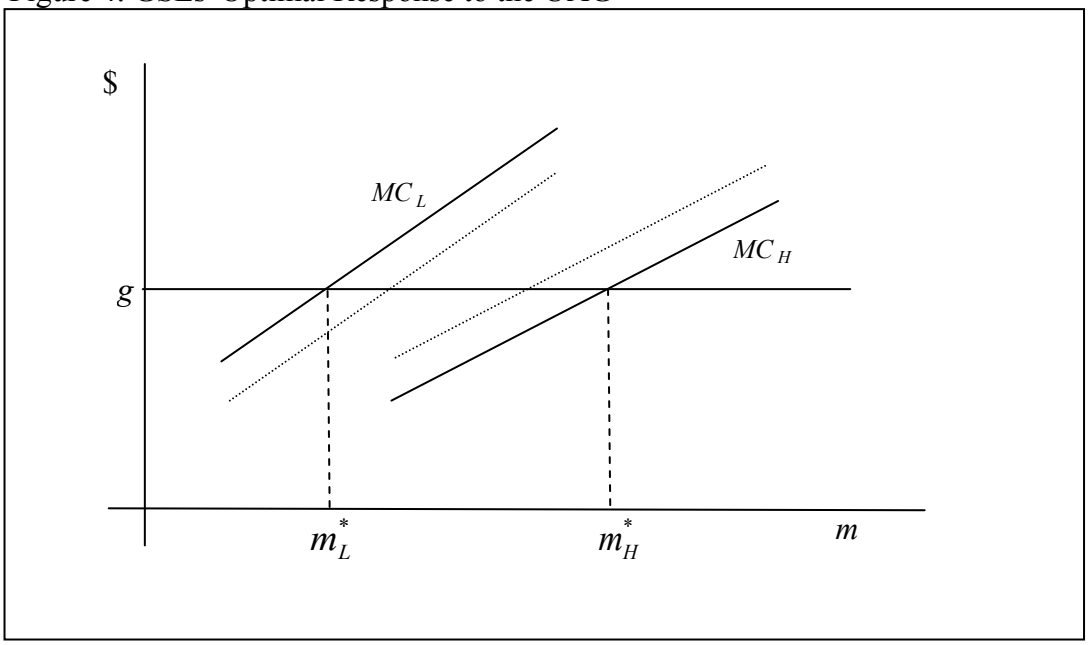


Table 1: Primary Variables Available in HMDA Dataset, 1992-2006

Variable	Availability	Description
Year	all years	Year of mortgage application or purchase
Institution ID	all years	10 Character Lender Identifier
Regulatory Agency ID	all years	Code indicating OCC, Fed, FDIC, OTS, NCUA (credit unions) or HUD as supervisory agency
Loan Type	all years	Conventional or government insured (e.g. FHA, VA)
Loan Purpose	all years	Home purchase, refinance, home improvement or multifamily (i.e. 5+ family property)
Property Type	2004-2005	1-4 Family, manufactured housing or multifamily structure
Occupancy	all years	Owner-occupied or investment property/second home
Loan Amount	all years	Dollar amount of loan
Lien Status	2004-2006	Loan secured by first or subordinate lien
Rate Spread	2004-2006	APR spread above comparable Treasury, conditional on the spread exceeding 3 percentage points for first-lien mortgages and 5 percentage points for subordinate-lien loans
HOEPA Status	2004-2006	Indicator for high-cost loan (e.g. APR at consummation on first-lien mortgage exceeds yield for comparable Treasury by more than 8 percentage points.)
Action Taken	all years	Six possibilities: (1) Loan originated, (2) Borrower rejects lender offer (3) Application denied, (4) Application withdrawn by applicant (5) Application incomplete, (6) Loan purchased by the institution
Denial Reason (optional)	all years	Institution can provide primary reason(s) for denial (e.g. credit history, insufficient collateral, debt load, etc)
Geography	all years	State, county and census tract of property
Income	all years	Gross annual family income, rounded to the nearest thousand dollar
Applicant(s) Ethnicity	2004-2006	Indicator for being Hispanic/Latino; may not be provided if telephone/internet application. "Hispanic" is a choice under *Race* variable in prior years
Applicant(s) Race	all years	Race of primary applicant; race of co-applicant if applicable. May not be provided if telephone/internet application
Applicant(s) Sex	all years	Sex of primary applicant; sex of co-applicant if applicable. May not be provided if telephone/internet application
Purchaser	all years	For loans sold at time of origination, specifies purchaser of loan (e.g. Fannie Mae, commercial bank, etc.)

Table 2: Census Tract Mortgage Credit Flow and Characteristics

	85≤ *TM*≤90		90<*TM*≤95		All Tracts	
# of Census Tracts (N)	2,728		2,798		42,381	
A. Mortgages per Tract per Year (1997-02)						
# GSE-Eligible Originations[1]	104.4***	(95.5)	115.3	(106.3)	126.9	(154.4)
Purchased by GSEs	43.3***	(45.6)	48.5	(51.1)	55.3	(74.7)
Purchased by non-GSEs	22.4***	(24.4)	24.4	(26.9)	27.9	(38.7)
# GSE-Ineligible Originations[2]	58.0***	(57.9)	60	(62.5)	63	(79.5)
Purchased by GSEs	1.6***	(2.6)	1.8	(3.3)	2.8	(5.9)
Purchased by non-GSEs	42.2***	(45.5)	43.3	(48.7)	42.8	(55.5)
B. Tract Characteristics, 1990						
Owner-Occupied Units	1044.5***	(585.6)	1105.4	(596.2)	1050.6	(686.0)
Total Housing Units	1856***	(966.2)	1870.4	(981.3)	1814.2	(1018.8)
Med Home Value ($2007, 000's)	142.66***	(94.16)	151.35	(102.21)	178.03	(137.72)
Prop. Units Detached	0.559***	(0.252)	0.597	(0.235)	0.584	(0.278)
Prop. Units Mobile Home	0.077***	(0.128)	0.069	(0.116)	0.047	(0.100)
Prop. Units Built 1980-1989	0.143***	(0.147)	0.146	(0.146)	0.160	(0.169)
Prop. Units Built 1940-1969	0.442	(0.217)	0.447	(0.221)	0.426	(0.226)
Prop. Units Built pre-1940	0.217***	(0.220)	0.200	(0.213)	0.204	(0.225)
Prop. Units in Multifamily Bldg	0.169***	(0.199)	0.157	(0.181)	0.177	(0.209)
Prop. Population Age >65	0.139	(0.072)	0.138	(0.073)	0.128	(0.072)
Prop. Population Black	0.103***	(0.194)	0.088	(0.172)	0.137	(0.250)
Prop. Population Hispanic	0.079***	(0.146)	0.071	(0.131)	0.088	(0.162)
Prop. Population in Group Qtrs	0.015***	(0.032)	0.015	(0.034)	0.016	(0.036)

Notes: Notes: Standard deviations in parentheses. * p<0.10, ** p<0.05, *** p<0.01; p-value from test of differences between tracts just below versus above the cutoff, clustered at MSA-level. See text for discussion of sample selection. Mortgages include owner-occupied home purchase and refinance loans. (1) GSE-eligible loans are conventional, conforming loans originated by lenders not classified as a subprime lender. (2) Loans with loan amount above the conforming limit, FHA or VA loans, or loans originated by subprime lenders.

Table 3: Estimates of the Effect of the UAG on GSE Purchases in Census Tracts Below the Income Cutoff, 1997-2002

Dependent variable: *(log) # GSE Purchases of Eligible Originations, 1997-2002*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)†
	non-RD			RD, parametric control			RD, non-parametric control		
Below Cutoff Dummy (D)	-0.1045***	-0.0309**		0.0337	0.0226	0.0307*	0.0180	0.0329**	
	(0.0160)	(0.0133)		(0.0486)	(0.0232)	(0.0166)	(0.0216)	(0.0154)	
UAG-Targeted Dummy (D^*)			-0.0394**						0.0425**
			(0.0155)						(0.0189)
Tract-to-MSA MFI (TM')				0.0070	0.0095*	0.0071*			
				(0.0119)	(0.0053)	(0.0043)			
(TM')*(D)				0.0614***	0.0029	-0.0020			
				(0.0194)	(0.0078)	(0.0059)			
(log) # GSE Purchases, 1994-96						0.6264***		0.6302***	0.6303***
						(0.0218)		(0.0343)	(0.0318)
R-Squared	0.766	0.839	0.839	0.0112	0.839	0.910	0.851	0.919	0.919
N	5525	5525	5525	5525	5525	5509	2208	2202	2202
Bandwidth (h)[1]	0.05	0.05	0.05	0.05	0.05	0.05	0.02	0.02	0.02
MSA & Tract Size[2] Controls	Y	Y	Y		Y	Y	Y	Y	Y
Other tract controls[3]		Y	Y		Y	Y	Y	Y	Y

Notes: Standard errors, clustered at MSA-level, shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01. (1) Tracts with TM between 0.90 +/- h; (2) tract scale variables are (log) owner-occupied units and (log) total housing units. (3) See table 2 for full list of covariates. † Instrumental variable regression using D as an instrument for the true treatment status dummy variable, D^*.

Table 4: Estimates of the UAG's Effect on Credit Flow in Tracts Below the Income Cutoff, 1997-2002 (h = 0.02)

Outcome variable:	(1)	(2)	(3)†	(4)	(5)	(6)†
	(log) Number of GSE-Eligible Originations			(log) Number of GSE-Ineligible Originations		
Below Cutoff Dummy (D)	0.0211	0.0266**		0.0123	0.0134	
	(0.0167)	(0.0104)		(0.0198)	(0.0127)	
UAG-Targeted Dummy (D^*)			0.0343***			0.0173
			(0.0128)			(0.0152)
Tract-to-MSA MFI (TM')						
(TM')*(D)						
(log) # GSE Purchases, 1994-96		0.7045***	0.7041***		0.5774***	0.5774***
		(0.0332)	(0.0309)		(0.0207)	(0.0191)
R-Squared	0.863	0.934	0.934	0.804	0.911	0.911
N	2208	2207	2207	2208	2206	2206

Notes: Standard errors, clustered at MSA-level, shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01. All regressions include MSA fixed effects, covariates (see table 2 for list) and two tract-level scale variables measured in 1990: (log) owner-occupied units and (log) total housing units. All regressions use tract in bandwidth (h) of 0.02 around TM = 0.90. Loans include owner-occupied refinance and home purchase mortgages originated between 1997 and 2002. † Instrumental variable regression using D as an instrument for the true treatment status dummy variable, D^*.

Table 5: Summary Statistics - Tracts that Fell Below the Income Threshold versus Tracts that Remained Above the Threshold between 2001/02 and 2005/06

	Switchers ($\Delta D = 1$)[1]		Non-Switchers ($\Delta D = 0$)[2]	
Number of Tracts (N)	1,850		5,759	
A. Tract Characteristics, 2000				
TM_{new}	0.827***	(0.064)	1.045	(0.119)
Owner-Occ Units	1080.9***	(524.6)	1340.3	(631.7)
Total Housing Units	1883.3***	(827.8)	1973.3	(894.9)
Minority Pop Share	0.212***	(0.163)	0.128	(0.119)
Med House Value ($2007)	135,432.00***	(71,495.71)	166,516.20	(93,116.69)
B. Tract Characteristics, 1990				
TM_{old}	0.969***	(0.049)	1.009	(0.055)
Owner-Occ Units	1015.1***	(459.5)	1153.4	(474.9)
Total Housing Units	1773.5	(743.8)	1744.8	(725.7)
Minority Pop Share	0.105***	(0.082)	0.077	(0.074)
Med House Value ($2007)	136,363.40***	(78,397.57)	160,108.50	(97,344.43)
C. Mortgage Originations, 2005/06				
GSE-Eligible Originations (per year)	124.5***	(98.1)	161.7	(149.3)
Purchased by GSEs	29.8***	(23.0)	41.7	(38.0)
GSE-Ineligible Originations	59.5	(85.8)	63.7	(74.5)
Purchased by GSEs	0.8***	(1.6)	1.3	(1.9)
D. Mortgage Originations, 2001/02				
GSE-Eligible Originations (per year)	129.4***	(89.8)	197.5	(152.9)
Purchased by GSEs	58.3***	(44.6)	89.6	(75.9)
GSE-Ineligible Originations	54.5***	(42.4)	61.6	(65.9)
Purchased by GSEs	1.5***	(2.6)	3.1	(6.1)
E. Home-Purchase Mortgage Originations, 2005/06				
GSE-Eligible Originations (per year)	60.1***	(53.3)	77.9	(93.4)
Purchased by GSEs	14.7***	(12.9)	21.2	(24.4)
GSE-Ineligible Originations	30.5	(53.3)	31.7	(43.2)
Purchased by GSEs	0.3***	(0.9)	0.6	(1.1)
F. Home-Purchase Mortgage Originations, 2001/02				
GSE-Eligible Originations (per year)	39.2***	(31.5)	56.9	(53.7)
Purchased by GSEs	17.3***	(15.3)	25.1	(25.2)
GSE-Ineligible Originations	27.6	(22.8)	28.4	(33.6)
Purchased by GSEs	0.6***	(1.1)	1.0	(2.3)

Notes: Standard deviations in parentheses. * p<0.10, ** p<0.05, *** p<0.01; p-value from test of differences between switchers and non-switchers, clustered at MSA-level. Sample tracts are those with TM_{old} between 0.90 and 1.10 and minority share in 1990 less than 0.30. (1) Sample tracts with $TM_{new} \leq 0.90$. (2) Sample tracts $TM_{new} > 0.90$. Mortgages includes those for home-purchase or refinance of an owner-occupied property, first and junior liens. Ineligible mortgages have a loan amount above the single-family conforming limit, or are government insured, or are originated by subprime specialist institutions.

Table 6: Crossing the Income Threshold and Changes in Credit Flow, 2001/02-2005/06

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome Variable:	Δ (log) GSE Purchases			Δ (log) GSE-Eligible Originations			Δ (log) GSE-Ineligible Originations		
ΔD	0.1045***	0.0211	0.0581**	0.1222***	0.0196	0.0257	0.0042	0.0033	-0.0121
	(0.0146)	(0.0160)	(0.0294)	(0.0129)	(0.0124)	(0.0250)	(0.0125)	(0.0130)	(0.0261)
$TM'_{old} * \Delta D$			-0.0047			-0.0008			0.0019
			(0.0031)			(0.0028)			(0.0030)
R-Squared	0.570	0.641	0.642	0.581	0.699	0.699	0.404	0.511	0.511
N	6113	1980	1980	6117	1981	1981	6118	1981	1981
Bandwidth (h)[1]	0.20	0.05	0.05	0.20	0.05	0.05	0.20	0.05	0.05
Tract controls[2]	no	yes	yes	no	yes	yes	no	yes	yes

Notes: Standard errors, clustered at MSA-level, shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01. All regressions include MSA-level fixed effect. Mortgages include those for home purchase and refinance of owner-occupied sites. (1) The bandwidth around $TM_{new} = 0.90$. (2) Includes (log) median home value in 2000, (log) number of owner-occupied units in 2000, (log) total number of housing units in 2000, minority population share in 2000 and TM_{old}.

Table 7: Estimates of the UAG's Effect in Tracts Below the Minority Population Share Cutoff (h = 0.03)

Dependent variable: *(log) # GSE-eligible originations, 1997-2002*		
	(1)	(2)
Below the Cutoff Dummy	-0.0221	-0.0250
	(0.0258)	(0.0200)
(log) # GSE-eligible originations, 1994-96		0.7247***
		(0.0354)
R-Squared	0.869	0.937
N	1505	1504

Notes: Standard errors, clustered at MSA-level, shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01. All regressions include MSA fixed effects, covariates (see table 2 for list) and two tract-level scale variables measured in 1990: (log) owner-occupied units and (log) total housing units. Dependent variable is the (log) number of GSE-eligible refinance and home purchase mortgages originations. For all regressions, bandwidth (h) is 0.03.

Table 8: Crossing the Minority Share Threshold and Changes in Credit Flow, 2001/02-2005/06

	(1)	(2)	(3)†	(4)	(5)	(6)†	(7)	(8)	(9)†
Outcome Variable:	Δ (log) GSE Purchases			Δ (log) GSE-Eligible Originations			Δ (log) GSE-Ineligible Originations		
ΔD	0.0731***	0.0261		0.1239***	0.0285		0.0965***	-0.0062	
	(0.0186)	(0.0304)		(0.0167)	(0.0287)		(0.0313)	(0.0340)	
ΔD^*			0.0433			0.0472			-0.0102
			(0.0435)			(0.0405)			(0.0489)
R-Squared	0.634	0.747	0.748	0.636	0.781	0.782	0.385	0.572	0.572
N	2002	703	703	2006	705	705	2005	704	704
Bandwidth (h)[1]	0.15	0.04	0.04	0.15	0.04	0.04	0.15	0.04	0.04
Tract controls[2]	no	yes	yes	no	yes	yes	no	yes	yes

Notes: Standard errors, clustered at MSA-level, shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01. All regressions include MSA-level fixed effect. Sample is tracts with TM_{old} between 0.90 and 1.30, and $minshare_{old}$ between 0.15 and 0.30. Mortgages include those for home purchase and refinance of owner-occupied sites. (1) The bandwidth around $minshare_{new} = 0.30$. (2) (log) median home value in 2000, (log) # of owner-occupied units in 2000, (log) total # of housing units in 2000, TM_{old}, TM_{new}, Hispanic share of population, African-American share of population, foreign-born share of population, share of population older than 25 with college degree, and share of population age 25-44. † Instrumental variable regression where ΔD is used to instrument for the true change in tract-eligibility status (ΔD^*).