The Government-Sponsored Enterprises and the Mortgage Crisis: The Role of the Affordable Housing Goals[*]

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ABSTRACT

The U.S. mortgage crisis that began in 2007 generated questions about the role played by Fannie Mae and Freddie Mac, the Government-Sponsored Enterprises (GSEs), in its causes. Some have claimed that the Affordable Housing Goals (AHGs), introduced by Congress through the GSE Act of 1992, and the resulting purchases of single-family mortgages the GSEs made to meet those goals, drove lending to high risk borrowers. Using regression discontinuity analysis, I measure the effect of one of the goals, the Underserved Areas Goal (UAG), on the number of single-family mortgages purchased by the GSEs in targeted census tracts from 1996 to 2002. Focusing additionally on tracts that became UAG-eligible in 2005-2006, when the Department of Housing and Urban Development (HUD) began to determine eligibility using the 2000 Census, I measure the effect of the UAG on GSE purchases during peak years for the subprime mortgage market. The first approach reveals a statistically insignificant UAG effect of 0 to 3 percent, while the second approach measures a significant 2.5 to 5 percent effect. The results suggest a small UAG effect and challenge the view that the AHGs made the GSEs a key player in the subprime lending boom by causing them to supply substantially more credit to high risk borrowers than they otherwise would have supplied.

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I. Introduction

The subprime mortgage crisis that began in 2007 has drawn attention to the role played by Fannie Mae and Freddie Mac, the Government-Sponsored Enterprises (GSEs), in its causes. Chartered by Congress during the Great Depression, Fannie Mae was the first of the GSEs to come into existence. Charged with increasing the liquidity of mortgage credit and providing stability in the secondary market for residential mortgages, Fannie evolved over the years from an institution that only purchased FHA-insured mortgages to one that also purchased conforming and conventional, non-federally insured mortgages. By acquiring mortgages and, starting in 1968, securitizing them into mortgage-backed securities (MBS), Fannie could transfer default risk from the originator's books onto its own and in the process encourage further mortgage lending.

Though the federal government removed its explicit guarantee of Fannie's debt in 1954, fully privatized the GSE in 1968, and consequently took it off its balance sheet, the GSE still retained what has become known as an *implied* government guarantee of its debt. Because of Fannie's historical ties to the government, exemption from state and local taxes, low capital requirements, and ability to borrow directly from the Treasury, investors perceived that the government would stand behind it in the event of financial trouble. In 1970, Freddie was created with the same special treatment and implied guarantee. As a result, both GSEs could issue corporate bonds at considerably lower cost than competing private financial institutions.[1] The MBS that they packaged, guaranteed, and sold to investors also benefited from the implied government guarantee. Capital regulations encouraged banks to sell their mortgages to the GSEs and buy them back as MBS. While the banks were required to have a minimum of 4 cents of capital for every dollar worth of mortgages on their balance sheets, they would need to have only 1.6 cents of capital on hand for every dollar worth of agency MBS in their possession (Acharya et al., 2011). In turn, the GSEs were required to hold a minimum of 45 basis points of capital against credit risk, so that mortgages securitized by the GSEs would have a total

[1] Passmore, Sherlund, and Burgess (2005) estimate a 42 basis point GSE advantage on long-term debt and a 13 basis point advantage on short-term debt. Ambrose and Warga (2002) find the GSE advantage to be between 25 and 29 basis points over "AA" banking sector bonds; between 43 and 47 basis points over "A" bonds; and between 76 and 80 basis points over "BBB" bonds. The implied guarantee has allowed market participants to consider GSE debt as risk-free as Treasury securities (Acharya et al., 2011).

of 2.05 percent of capital backing them as opposed to the 4 percent worth of capital that would be backing whole loans held on bank books (Hancock et al., 2006). Though this rule freed up cash that both the banks and the GSEs could use, it also increased leverage throughout the financial sector.

With cheap borrowing costs and such incentives, the GSEs were able to grow from firms that owned or guaranteed 7.1 percent of residential mortgage debt outstanding in 1981 to 25.7 percent in 1990 and 46.3 percent in 2003. Although GSE market share dropped to 38.8 percent by 2006, possibly because of crowding out by the private sector, today the agencies are back to holding over 46 percent of residential mortgage debt outstanding (see Fig. 1).

Following a massive increase in mortgage defaults beginning in late 2006, credit losses ultimately forced the GSEs into government conservatorship in September, 2008. As market participants had correctly assumed, not only would GSE obligations be backed by the government, but the GSEs would also be permitted to continue operations while in conservatorship. The bailout has so far cost taxpayers over $150 billion, with Congressional Budget Office (CBO) estimates suggesting that the figure could more than double by 2019 (CBO, 2010).

The typical mortgage guaranteed or purchased by the GSEs has evolved over time. Initially, the GSEs focused on 30-year fixed-rate mortgages with loan-to-value (LTV) ratios of 80 percent or less and originated to borrowers with prime credit histories. Later, they introduced LTV programs of 95, 97, and eventually 100 percent, and programs for borrowers with blemished credit records. Numerous scholars have argued that the AHGs, introduced by the Federal Housing Enterprises Financial Safety and Soundness Act (GSE Act) of 1992, were the cause of this shift.

The AHGs consist of three goals, established with the intention of encouraging the GSEs to achieve their mission of increasing access to residential mortgage credit and providing assistance to the secondary market for these mortgages. Each goal stipulates that a certain percentage of dwelling units financed by each GSE's purchases has to meet established criteria. To qualify towards the "Low and Moderate-Income Goal" (LMG), a dwelling unit has to be owned by a borrower(s) whose income is below the area median

family income.[2] To qualify for a "Special Affordable Goal" (SAG), a dwelling unit has to be owned by a borrower(s) whose income is below 60 percent of the area median family income, or who has an income that is below 80 percent of the area median and resides in a census tract with median family income below 80 percent of the area median. Finally, a dwelling unit will count towards the "Underserved Areas Goal" (UAG) if it is located in a census tract where the median family income is less than 90 percent of the area median family income, or if it is located in a tract with median family income less than 120 percent of the area median and where at least 30 percent of the population is a minority.

GSE performance on these goals was measured annually by the Department of Housing and Urban Development (HUD), which up until 2008 was the agencies' prudential and mission regulator.[3] Since the goals focused on the dwelling units financed by GSE purchases, the GSEs could get different amounts of goal "credit" for different kinds of purchases. A single-family mortgage purchase could help finance 1 to 4 dwelling units and thus yield 1 to 4 goal credits; multi-family mortgage purchases could yield 5 or more credits; and purchases of Real Estate Mortgage Investment Conduits (REMICs), which included private-label MBS, could yield as many credits as the number of qualifying dwelling units financed by the mortgage pool.[4] By design, a purchase could count toward more than one goal, so that, for example, a mortgage to a very low-income borrower living in an underserved area could count toward all three goals. Additionally, goal definitions of low income, moderate income, and underserved areas were more closely tied with criteria used by HUD for its other housing programs than with definitions of U.S. poverty line income.[5]

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[2] "Area" can be a Metropolitan Statistical Area (MSA) or a rural area. For the LMG, the area median family income is estimated annually by HUD.

[3] The Office of Federal Housing Enterprise Oversight (OFHEO), housed within HUD, was the prudential regulator until it was replaced by the independent Federal Housing Finance Agency (FHFA) in 2008.

[4] The 1995 Final Rule, submitted by HUD and put into effect in 1996, allowed REMIC purchases to count toward the goals "as long as the underlying mortgages or mortgage-backed securities were not previously purchased or issued by the GSEs or otherwise would result in double counting" (Federal Register, 1995).

[5] The Community Reinvestment Act (CRA) and the Community Development Block Grant (CDBG) housing programs each require people to live in a census tract with median income of 80 percent or less than the area median income in order to qualify for their support. Other government programs, such as the Low-Income Housing Tax Credit (LIHTC), require the tract median income to be 60 percent or less than the area median income. According to HUD's former Assistant Secretary for Housing John Weicher, the poverty line median household income for a family of four has been between 40 and 45 percent of the national median household income since 1993 (Weicher, 2010).

As the mission regulator, HUD was also responsible for periodically revising the goals. From 1996 to 2008, it consistently ratcheted them up in a way that saw the LMG increase by 40 percent, the SAG increase by 125 percent, and the UAG increase by 85 percent (see Table 1). The fact that this pattern overlaps with the timeline of the growth of the housing bubble has fueled arguments that the AHGs are at least in part to blame for the mortgage crisis (Wallison, 2001; Roberts, 2010). Substantiated by the fact that the goals targeted low income individuals, by the large share of residential mortgage debt outstanding on GSE books in the lead-up to the crisis, and by the potential for moral hazard induced by implied government backing, this argument seems plausible.

If the AHGs did cause the GSEs to fund substantially more risky loans than they otherwise would have, the AHGs could have contributed to the mortgage credit bubble of 2002-2007 that ultimately led to the financial crisis. As mentioned earlier, there are three channels through which the GSEs could have responded to the AHGs: single-family mortgage purchases, multi-family mortgage purchases, and REMIC purchases. The analysis that follows looks only at the single-family mortgage channel, in large part because the data available on multi-family and REMIC channels is currently inadequate for a thorough causal analysis of the effect of the goals on those channels.

The rest of this paper is structured as follows. Section 2 provides an overview of existing literature and Section 3 describes the data and methodology employed in this paper. Section 4 reports results and Section 5 concludes.

II. Literature Review

Existing literature has tackled the effects of the goals on the supply of mortgage credit to the poor and underserved in several different ways. Focusing on mortgage originations at the Metropolitan Statistical Area (MSA) level in order to control for spatial variation in local economic risk, Ambrose and Pennington-Cross (2000) find some evidence that the GSEs purchase more loans from areas experiencing less economic risk and fewer loans in MSAs with a high percentage of loans being originated in underserved census tracts. They caution that by using Home Mortgage Disclosure Act (HMDA) data they are only able to analyze non-seasoned, single-family GSE purchases, which may

cause them to underestimate the strength of GSE purchasing activity in their areas of interest.[6]

By doing their research at the MSA level, Ambrose and Pennington-Cross reveal the general weakness of the data that is currently available. Unable to attain data on mortgage market characteristics at the tract level, Ambrose and Thibodeau (2004) also use MSA-level data to estimate supply and demand for mortgage credit. In measuring the effect of the AHGs on supply, the authors find mixed results. Although the effect is positive and significant over the period 1996 to 1999, when analyzed annually it appears that the effect was significant in 1998 but insignificant in 1996, 1997, and 1999. The authors do, however, find that an increase in GSE purchases of seasoned mortgages in an MSA results in an unambiguous increase in the total number of mortgages originated in the MSA.

More recent literature has found a way to circumvent the lack of specific data at the census tract level when trying to analyze the effect of the AHGs on mortgage market outcomes. These studies note the difficulty of isolating the AHG effect when working with all underserved tracts. Programs like the 1977 Community Reinvestment Act (CRA), which oversees loan origination activity among FDIC-insured depository institutions and encourages lending in tracts with tract-to-MSA-income ratios (TM) of 0.80 or below, confound the AHG effect on mortgage supply in tracts below this threshold.[7] Since the LMG targets individuals and the SAG also has a component that targets tracts with TM of 0.80 or below, it is difficult to isolate the effects of these two goals at the tract level. One criterion of the UAG, however, specifically targets tracts with a TM of 0.90 or below, allowing studies to isolate its effect by focusing on tracts whose

[6] Seasoned mortgages, which are not included in the HMDA data set, are those that were purchased by the GSEs in the year after their origination. An additional concern is that HMDA does not capture loans originated, sold first to a non-GSE entity, and then to one of the GSEs in the same year (Avery, et al., 2011).

[7] The Low-Income Housing Tax Credit (LIHTC), which requires a TM of at most 0.60, and the Community Development Block Grant (CDBG) Program, which requires a TM of at most 0.80, also make it difficult to isolate the AHG effect.

TM is between 0.80 and 0.90 and comparing them with those above the TM = 0.90 cutoff.[8]

Using tract-level 1990 Census and 2000 Census data for controls and tract-level HMDA data on GSE purchases to measure the "intensity" of GSE activity in each tract, An et al. (2007) estimate the UAG effect on housing market outcomes from 1990 to 2000. They compare changes in the homeownership rate, the vacancy rate, and in median home values in tracts just below the TM cutoff (TM $\leq$ 0.90) with those just above the TM cutoff (TM > 0.90). Although their first-stage estimates suggest that "tracts targeted under the GSE affordable goals were little different from untargeted tracts with respect to housing market outcomes during the 1990s," their second-stage estimates, controlling for potential endogeneity of GSE mortgage purchasing activity (e.g., the GSEs might be purchasing more in tracts that have increasing home prices, homeownership rates, etc.), indicate that the UAG increased home prices and homeownership rates.

An et al. (2007), in addition to several other papers on this topic (Bostic and Gabriel, (2006); Gabriel and Rosenthal, (2008); An and Bostic, (2008)), attempts to decrease the potential for omitted variable bias by using regression discontinuity analysis to compare tracts with TM 5 to 10 percentage points below the cutoff to tracts with TM 5 to 10 percentage points above the cutoff. However, none of these studies control for relative income within these ranges, assuming, for example, that a tract with TM of 0.80 is the same in the effect of people's income on GSE purchases as a tract with TM of 0.88, and potentially biasing their estimates of the UAG effect downward. An et al. (2007)'s first-stage estimates, which find that GSE market share is 12.8 percent lower in treatment versus control tracts, suggest that this downward bias is in fact present in these studies. Bostic and Gabriel (2006) also find generally negative and mostly statistically insignificant effects of the UAG on GSE purchasing intensity in California. Gabriel and Rosenthal (2008) report negative and statistically insignificant estimates and argue that GSE purchases crowd-out non-GSE purchases. An and Bostic (2008) conclude that credit supply and homeownership is "effectively unchanged" by the UAG. They posit that by

[8] Recall that mortgage purchases can count towards more than one goal for the GSEs, suggesting that an analysis of the UAG effect on tracts with TM between 0.80 and 0.90 will also capture some of the effects of the other goals.

improving lending conditions for those who would otherwise have to take out an FHA-insured loan with a high mortgage rate, the UAG left the FHA with the most risky borrowers, forced it to introduce stricter underwriting requirements, and decreased the number of FHA-insured mortgages originated. Thus, on net, they find credit supply in targeted tracts to be unchanged.

Bhutta (2010) resolves this downward bias concern by controlling for each tract's TM and focusing his analysis of the UAG effect on tracts within 5 percentage points and 2 percentage points of the cutoff. Through a similar regression discontinuity analysis as performed by the studies above, Bhutta finds a 4 percent increase in GSE purchases in targeted tracts in the period 1997 to 2002 as a result of the UAG. Since previous work tried to assess the effect of the UAG not only on the number of GSE purchases in targeted tracts but also on changes in home prices, homeownership, and vacancy rates, among other things, it had to use detailed Census data that was only available at the tract level on a decennial basis. By focusing purely on GSE purchasing activity, Bhutta is able to go a step further. Measuring the change in GSE purchases for tracts that were not targeted in 2001-02 (when the GSEs still used 1990 Census criteria in determining which tracts met the goals and which did not) but targeted in 2005-2006 (when the GSEs began using 2000 Census criteria to determine eligibility), Bhutta is able to estimate the UAG effect in 2005-2006. He finds that the goal generated an almost 6 percent increase in GSE purchases for tracts that were just above the cutoff in 2001-2002 and just below in 2005-2006. Additionally, he finds that for every percentage point increase in a tract's 2001-2002 TM for switching tracts this effect diminishes by 0.5 percent. In other words, less stable tracts, tracts that experienced larger declines in TM from the 1990 Census to the 2000 Census, experienced smaller increases in GSE purchases as a result of the goals than relatively stable tracts that had a small drop in TM.

Bhutta's important work, however, has several limitations. His use of 1994-1996 as the pre-UAG control years is likely biasing his results downward. Although the goals did not go into effect until 1996, HUD set a preliminary UAG at 30 percent as early as 1993 (see Table 1). Moreover, my analysis of single-family GSE purchases from 1993 to 1995 shows that Fannie went from having 20.3 percent of its purchases meet the UAG in 1993 to 25.7 percent in 1995, while Freddie had a smaller increase from 19.4 percent in

1993 to 22.0 percent in 1995 (see Fig. 2). Assuming that mortgages that meet the UAG differ from the types of mortgages the GSEs normally want to accumulate (otherwise there would be no point to the goal!), these trends suggest that the GSEs were not thinking of the 30 percent UAG as purely preliminary. Thus, using 1993-95 or even 1993-94 as control years would help produce a more precise measurement of the UAG effect.

The biggest limitation in Bhutta's paper is the nature of the data that he uses. By employing HMDA data, like all of the other scholars mentioned above, he is unable to take GSE purchases of seasoned mortgages and mortgages not directly sold to the GSEs into account when carrying out his analyses. He thus observes fewer mortgages than the GSEs actually made and, depending on the distribution of these uncounted mortgages among the targeted and untargeted tracts, his results could be either biased upward or downward.

III. Data, Methodologies, Results

Publicly available data on GSE purchasing activity makes it difficult to assess the extent to which the GSEs met their goals. As mentioned earlier, a goal would be achieved if a certain share of all *dwelling units* financed by GSE purchases met the necessary criteria. HMDA data used by most of the existing literature denotes which single-family and multi-family mortgages were purchased by the GSEs, but does not provide the number of dwelling units financed by each mortgage. Data available through HUD's GSE Public Use Data Base is similarly limited, but unlike HMDA data it includes GSE purchases of seasoned mortgages and mortgages the GSEs do not purchase directly from originators.[9]

The absence of data on dwelling units thus forces any analysis on the effect of the goals on GSE purchasing activity to make simplifying assumptions. In the analysis that follows, a single-family mortgage is assumed to consist of 1 dwelling unit. Although the analysis here is focused on single-family mortgages, it is worth noting that a similar

[9] Both data sets include data on home purchase and refinancing mortgages.

simplification would be more difficult to make for multi-family mortgages, since the variance in dwelling units financed is much larger.

Official HUD calculations suggest that the GSEs met or exceeded goal targets through 2007 with only one exception, when Freddie Mac fell short of the UAG in 2002 by 90 loans (Weicher, 2010; see Fig. 2).[10] This trend, however, does not necessitate a causal relationship between the goals and GSE purchasing activity. Other factors, from other governmental housing programs to an endogenous increase in originations in census tracts targeted by the goals, could have been driving the GSEs to purchase a larger share of products that qualified for the goals. Other factors may have brought high-risk borrowers into the mortgage market by perversely altering GSE purchasing activity or by working through non-GSE channels all together.

To see if a causal link between the goals and GSE purchasing activity does indeed exist, the analysis that follows employs HUD's GSE Public Use Data Base and its census tract-level data on all single-family GSE-purchased mortgages from 1993 to 2006. Similar to Bhutta (2010), the analysis is divided into two approaches, both of which estimate the UAG effect and, as is custom in the literature, use it as a proxy for the effect of the goals as a whole on GSE mortgage purchases. Here, the focus is solely on the first part of the UAG, where a tract is deemed eligible if its TM $\leq$ 0.90. The first approach measures the GSE effect for targeted tracts over the period 1996-2002. The second approach measures the UAG effect on tracts that switch from being untargeted in 2001-02 to being targeted in 2005-06. 1990 Census and 2000 Census data is used to control for tract and MSA-level characteristics.[11]

[10] Although Fig. 2 does not show data for the SAG, the GSEs met or exceeded it through 2007 without exception.

[11] GSE Public Use data was purchased from HUD using Stanford UAL Small Grant #4769. Decennial Census data was purchased from the National Technical Information Service (NTIS) of the U.S. Department of Commerce using the same UAL grant.

A. The UAG Effect, 1996-2002

I compiled a tract-level dataset that merges data on all single-family GSE purchases with 1990 Census data.[12] Table 2 reports summary statistics on tract-level controls. As in previous literature, I exclude non-MSA census tracts for reliability reasons and because it is not always possible to map a rural purchase to a specific tract. I also exclude observations based on the following exclusion criteria:

- Observations where either the state, county, MSA, or tract identifier is missing
- Observations for which state, county, and tract identifiers in the GSE dataset did not match precisely with 1990 Census state, county, and tract codes
- Observation where the borrower was listed as younger than 18 years old
- Tracts in Hawaii and Alaska in order to avoid potentially idiosyncratic housing markets
- Tracts formed between 1993 and 1999 so as to compare only tracts with unchanging geographical characteristics
- Tracts that are split between MSAs (only in New England states)
- Tracts with missing data on median income
- Tracts that in 1990 had fewer than 100 housing units
- Tracts that in 1990 had zero specified owner-occupied units
- Tracts that in 1990 had more than 30% of the population living in group quarters (military barracks, dormitories, detention centers, etc.)
- Tracts with an extremely high (>5) or low (<0.1) number of GSE purchases as a share of owner-occupied units (Bhutta (2010) excludes tracts where the number of *originations* as a share of owner-occupied units is >10 and <0.2. Since I only have data on GSE-purchased mortgages, I use the rule of thumb that the GSEs hold about half of all mortgage debt on their books in determining my extremely high and extremely low thresholds above).

When first delineated, census tracts are usually 2,500 to 8,000 persons in size and are designed to be "homogeneous with respect to population characteristics, economic status, and living conditions" (U.S. Census, 2000). There are no restrictions on their spatial size, however, as long as they do not cross county lines.

As Table 2 shows, there is a virtually identical number of tracts and MSAs below and above the TM = 0.90 cutoff, whether we use the ± 0.05 or the ± 0.02 bandwidth around the cutoff. When it comes to the number of GSE purchases, however, tracts with $0.90 < TM \leq 0.95$ have a statistically significantly higher number of purchases per year

[12] The period of analysis begins in 1996 when the AHGs were put into effect, and ends in 2002 because 2000 Census data on tracts was not released until 2003 and the HUD continued to use 1990 Census data to determine tract eligibility through 2002.

than tracts with $0.85 \leq TM < 0.90$. When we use a ± 0.02 bandwidth around the cutoff, the number of purchases per year in the two groups is not statistically different. This characteristic of the data suggests that it is important to control for income when using a ± 0.05 bandwidth around the cutoff, but is not necessary to do so when using a ± 0.02 bandwidth.

Methodology

The discontinuity in the applicability of the UAG at TM = 0.90 allows for a regression discontinuity analysis that compares the number of GSE purchases in tracts treated and untreated by the UAG. In its simplest form, the tract-level regression that we have to consider is as follows:

$$Y_i = \alpha + \beta D_i + \varepsilon_i \quad (1)$$

where Y_i is the log number of GSE purchases in tract i, $D_i = \mathbf{1}[TM_i \leq 0.90]$ is the dummy variable that differentiates treated from untreated tracts, and ε_i is the error term that captures the unmeasured effects on GSE purchasing activity. This specification assumes that, at the limit, ε is the same for tracts just below and just above the cutoff. Formally, we have:

$$\lim_{h \to 0} \{E[\varepsilon_i | 0.90 - h \leq TM_i \leq 0.90] - E[\varepsilon_i | 0.90 < TM_i \leq 0.90 + h]\} = 0 \quad (2)$$

where E is the expectation operator. Thus, if (2) holds, the coefficient β on the treatment dummy D_i captures the UAG treatment.[13]

Several issues arise, however, with this simple estimation method. First, as studies like An et al. (2007) and Bhutta (2010) show, the number of originations in a tract is positively correlated with the TM of the tract, suggesting that our estimates of β in (1) will be downward biased as long as we do not control for TM. Additionally, looking at all tracts below the cutoff, in particular those with $TM \leq 0.80$, will also confound the interpretation of β because other governmental housing programs will apply. To overcome these shortcomings and improve on the estimation methods of the preceding

[13] Although self-selection into the treatment group is often a concern for regression discontinuity analyses (McCrary, 2008), given that each tract's TM for 1996-2002 was determined by the 1990 Census, before the introduction of the goals with the GSE Act of 1992, it is very unlikely that self-selection bias is present in this case.

literature, I run my regressions for $h = 0.02$ and $h = 0.05$ rather than $h = 0.10$ or greater, and in the case of h=0.05 I also control for the relative TM.

Another problem with (1) is that it does not include tract-level covariates that capture significant differences between the two tract groups in housing and demographic characteristics. I include these covariates in some of my regressions (see Table 3) and also control for the number of GSE purchases in each tract prior to the introduction of the goals. As discussed earlier, my analysis of single-family GSE purchases prior to the introduction of the goals suggests that Bhutta's use of 1994-1996 as the pre-treatment years is likely biasing his estimate of β downward. To mitigate this potential bias, I use the log number of GSE purchases in 1993-1995 as the control for pre-treatment GSE activity.[14]

I run three sets of regressions as part of this analysis. In the first set, I estimate (3) and (4) which augment (1) with relevant controls. (3) introduces $\mathbf{X}_i$, a vector that includes the log number of owner-occupied units and the log number of total housing units. (4) adds $\mathbf{Y}_i$, a vector that includes all other housing and demographic covariates.

$$Y_i = \alpha + \beta D_i + \gamma \mathbf{X}_i + \varepsilon'_i \quad (3)$$

$$Y_i = \alpha + \beta D_i + \gamma \mathbf{X}_i + \delta \mathbf{Y}_i + \varepsilon''_i \quad (4)$$

In addition, I introduce $D^*_i = \mathbf{1}[(TM_i \leq 0.90) \vee (TM_i \leq 1.20 \wedge M_i \geq 0.30)]$ where M_i is the minority share of the population living in tract i.[15] Thus, the coefficient β in regression (5) estimates the effect of the UAG based on both of its eligibility criteria.

$$Y_i = \alpha + \beta D^*_i + \gamma \mathbf{X}_i + \delta \mathbf{Y}_i + \varepsilon'''_i \quad (5)$$

The second set, typified by (6), includes $TM'_i = TM_i - 0.90$ and the interaction variable $(TM'_i)*(D_i)$ which together provide an important control for TM differences relative to the cutoff.

[14] Although using 1994-5 instead of 1993-5 as the control years yields a higher coefficient on the log number of GSE purchases in those years, it has virtually no effect on the coefficient on D_i.

[15] African American, Hispanic, Native American are examples of minority classifications. A tract with only African American residents, for example, will have minority share $M_i = 1$.

$$Y_i = \alpha + \beta D_i + \gamma TM'_i + \delta(TM'_i)*(D_i) + \eta \mathbf{X_i} + \theta \mathbf{Y_i} + \varepsilon^{IV}{}_i \quad (6)$$

The third set narrows the analysis down to $h = 0.02$ and includes a two-stage estimation procedure. Following Bhutta (2010), I use the first criterion of the UAG, $D_i = \mathbf{1}[TM_i \leq 0.90]$, as an instrument for UAG eligibility.[16,17] Since satisfying this criterion is clearly highly correlated with being UAG eligible and, controlling for TM, plausibly affects the number of GSE mortgage purchases in a given tract only through the UAG, it is arguably a good instrumental variable for this analysis.[18] The first and second stage regressions are described in (7) and (8), respectively. In all regressions, I control for MSA effects by clustering standard errors at MSA-level.

$$D^{*}{}'_i = \alpha + \beta D_i + \gamma \mathbf{X_i} + \delta \mathbf{Y_i} + \varepsilon^{V}{}_i \quad (7)$$

$$Y_i = \alpha + \beta D^{*}{}'_i + \gamma \mathbf{X_i} + \delta \mathbf{Y_i} + \varepsilon^{VI}{}_i \quad (8)$$

Results

Table 3 reports coefficient estimates for the regressions described above, with the log number of GSE purchases for 1996-2002 as the dependent variable. Column 1 reports the coefficient on the treatment dummy when the bandwidth around the cutoff is $h = 0.05$ and only tract size controls are employed. The estimate indicates that tracts below the cutoff see almost 8 percent fewer GSE purchases than tracts above the cutoff, which is in line with the negative estimate on D_i reported by An et al. (2007) and others.

Including other tract controls, column 2 shows untreated tracts with still about 1.5 percent more GSE purchases than treated tracts, although this estimate is no longer statistically significant. As discussed earlier, these results are most likely due to the fact that we are missing a control for income. Results in column 3 provide another reason why

[16] Bhutta argues that D_i is "plausibly exogenous given TM," while $D^{*}{}'_i$ is endogenous, although he does not specify his reasons for the endogeneity concern. From an omitted variable bias perspective, it is possible that surges in immigration of minorities to neighborhoods with minority levels just under 30 percent of the population are both making tracts eligible for the UAG and increasing the number of GSE purchases in those tracts purely through an increase in the demand for housing. In this sense, D_i is plausibly exogenous, controlling for TM, because it is harder to imagine surges in movements of relatively poor non-minorities into neighborhoods that are just above the TM = 0.90 cutoff.

[17] Bhutta also calculates the UAG effect using minority share of population = 0.30 as a discontinuity and finds "no substantive difference across the cutoff."

[18] The first UAG criterion is a very strong instrument, with a first-stage t-statistic of 75.99.

using D_i is better than using the true treatment dummy variable, one that also includes tracts with minority share of population at or above 30 percent as long as their TM is less than or equal to 1.20. Combining these two eligibility criteria weakens the strength of the TM = 0.90 discontinuity by including treated tracts between TM = 0.90 and TM = 0.95. Given that the difference in the number of GSE purchases in treated and untreated tracts was negative in our earlier estimates, we would expect it to be even more negative now due to a UAG-induced increase in the number of GSE purchases in the tracts we considered untreated before. The effect measured in column 3 is indeed lower by about 0.6 percent than the estimate in column 2, confirming our hypothesis.

The regression in column 4 maintains an $h = 0.05$ bandwidth and excludes all controls except for relative income. Although Bhutta records an increase in the coefficient on D_i to 0.0337 as a result of the introduction of income into the regression, my calculations suggest a virtually identical, negative and statistically insignificant, coefficient to the one I estimate with all of the other controls in column 2. Only when the other controls are back in the regression in column 5 does the coefficient on the dummy variable change substantially and become positive, implying that the income control and the other housing and demographic tract controls are all important to explaining variation in GSE purchasing activity. The coefficient is very close to zero, however, and not statistically different from it, suggesting a negligible UAG effect on GSE purchases.

The regression represented in column 6 includes the log number of GSE purchases for 1993-1995 in each tract as a control for past GSE purchasing behavior. Although expected to eliminate the downward bias on the coefficient on D_i in Bhutta's regressions, the inclusion of this control shows the UAG effect at 1.4 percent versus Bhutta's 3 percent. Something else must clearly be at play in order for my estimate to be lower. Though Bhutta speculates that his estimate of the UAG effect would be even bigger if he were to include seasoned mortgage purchases in his analysis, he does not consider the possibility that these purchases would be not be more heavily concentrated in targeted tracts. The fact that my data set includes these purchases, coupled with my lower UAG effect estimate, suggests that these purchases are distributed either in the same way as other GSE purchases or are more heavily concentrated in untargeted tracts.

Columns 7-9 reduce the bandwidth around the cutoff to $h = 0.02$ in order to capture the difference in GSE purchases for tracts that just qualified to those that just did not. Including all controls except for the control for GSE purchases for 1993-1995, we see an estimate on D_i that is negative but very small and not statistically different from zero. The estimate in column 8, with the control for GSE purchases included, shows 1.1 percent more purchases in treated tracts than in untreated ones, while Bhutta reports a 3.3 percent difference in favor of treated tracts.[19]

Employing the two-stage estimation procedure to correct for potential endogeneity bias, we get an estimate of a 2.7 percent UAG-effect, although it again is not statistically different from zero. Bhutta, on the other hand, reports about a 4 percent UAG-effect that is significant at the 5 percent level. If the control I use for GSE purchases from 1993 to 1995 does in fact reduce the negative bias in Bhutta's estimates, then the fact that my estimates are lower than his again suggests that my analysis is capturing purchases for which Bhutta does not account and which are not more heavily concentrated in treated tracts. This once again supports the theory that the seasoned mortgages and mortgages not directly sold to the GSEs that I am able to include in my analysis are distributed either in the same way as other GSE purchases or are more heavily concentrated in untargeted tracts.

B. The UAG Effect, 2005-2006

All of the existing literature on the goals, except for Bhutta (2010), does not go past the year 2000 in its analyses. It focuses on 2000 because that is the year when the Census released new figures on median home values, number of owner-occupied homes, total number of housing units, etc. at the tract level, giving authors an opportunity to see what impact 4 years of GSE activity had on housing market outcomes.

Focusing purely on changes in the number of GSE purchases, I estimate the UAG effect in 2005-2006 by taking advantage of the fact that HUD began to use 2000 Census

[19] Even though I control for income and minority shares and do the analysis with an $h = 0.02$ radius around the cutoff, my results could still be biased downward because some high minority tracts with $TM \geq 0.90$ that I consider untreated are actually treated. After deleting observations above the cutoff that meet the minority share ≥ 0.30 requirement, I redo the estimates reported in columns 4-8. These new estimates are virtually identical to the ones reported and stay insignificantly different from zero.

data in 2005 to assess how well the GSEs were doing on the goals. This is an especially important analysis because 2005 and 2006, as was noted earlier, were the peak years for the subprime mortgage market, with about 20 percent of all mortgage originations classified as subprime (FRBSF, 2007). Estimating the UAG effect on GSE purchases in these years will thus allow for an investigation of the role of the goals in the growth of the subprime market.

Methodology

I approach the problem through a time-series analysis (as opposed to the cross-sectional analysis I execute for 1996-2002) and estimate the UAG effect for tracts that were ineligible in 2001-2002 (tracts with $TM_{old} > 0.90$) but eligible in 2005-2006 ($TM_{new} \leq 0.90$) as a result of HUD's switch from the 1990 Census to the 2000 Census.[20] Additionally, this approach will allow me to compare the number of GSE purchases in tracts whose TM falls dramatically and into the UAG-eligible range with the number of GSE purchases in tracts that became UAG-eligible from only a small fall in TM. The comparison should shed light on how well the GSEs serve tracts in serious economic decline versus relatively stable tracts.

Table 4 displays the summary statistics for the controls employed in this analysis, separating them out for switchers (for which $\Delta D_i = 1$ if $TM_{i,\,new} \leq 0.90 \mid TM_{i,\,old} > 0.90$) and non-switchers (for which $\Delta D_i = 0$ if $TM_{i,\,new} > 0.90 \mid TM_{i,\,old} > 0.90$). As we can see from the table, only about 8.5 percent (1,408/(1,408+14,989)) of UAG-ineligible tracts in 2001-2002 ended up becoming UAG-eligible in 2005-2006. Those that switched did so by having their TM fall by an average of 17 percent, while those that did not switch saw their TM rise by a negligible amount. Although the number of purchases made in each type of tract per year fell from 2001-2002 to 2005-2006, the non-switchers had more purchases in each time period.

I apply the same exclusion criteria to the data as for the 1996-2002 analysis, but with the following changes:

[20] I prefer to use 2001-2002 as the pre-treatment years for these "switcher" tracts. Although HUD assessed GSE goal performance using 1990 Census data through 2004, 2000 Census data was already available and could have been affecting GSE behavior in 2003 and 2004.

- Tracts formed after 2002 and before 2005 are excluded
- Tracts with an extremely high (>3) or low (<0.06) number of GSE purchases as a share of owner-occupied units are excluded (I modify these restrictions proportionally, based on the fact that before we were dealing with 7 years of purchases – 1996 to 2002 – and now we only have 2 years of purchases)
- Tracts with $TM_{old} \leq 1.20$ and minority share of population in 1990 ≥ 0.30 are excluded in order to avoid counting tracts treated in 2001-2002 as switchers
- Tracts whose 1990 Census population size changed by more than 10 percent in the 2000 Census are also excluded[21].

I carry out the analysis using three regressions, with ΔY_i, the difference in the log number of GSE purchases in 2005-2006 from the log number of GSE purchases in 2001-2002, as the dependent variable.[22] (9) estimates the UAG-effect using a large bandwidth *h* around the $TM_{new} = 0.90$ cutoff, while (10) narrows the bandwidth to $h = 0.05$ and introduces a control vector $\mathbf{X_i}$. $\mathbf{X_i}$ includes the log median home value in 2000, the log number of owner-occupied units in 2000, the log total number of housing units in 2000, the proportion of the population that is black, the proportion of the population that is Hispanic, and TM_{old}.

$$\Delta Y_i = \alpha + \beta \Delta D_i + \varepsilon'_i \quad (9)$$

$$\Delta Y_i = \alpha + \beta \Delta D_i + \gamma \mathbf{X_i} + \varepsilon''_i \quad (10)$$

(11) introduces $TM'_{i,\,old} = TM_{i,\text{old}} - 0.90$ and an interaction variable $(TM'_{i,\,old})^* (\Delta D_i)$, while keeping all of the controls and maintaining bandwidth $h = 0.05$.[23] Just as in the earlier analysis, all regressions control for MSA effects by clustering standard errors at MSA-level.

$$\Delta Y_i = \alpha + \beta \Delta D_i + \gamma \mathbf{X_i} + \delta TM'_{i,\,old} + (TM'_{i,\,old})^* (\Delta D_i) + \varepsilon'''_i \quad (11)$$

Results

Table 5 reports all of the important coefficient estimates for this analysis. Column 1, with the bandwidth $h = 0.20$ and only the switcher dummy variable included in the regression, shows tracts switching into eligibility having 11.6 percent more GSE purchases than tracts that do not switch. Although the estimate is highly significant, the

[21] I use the Census Population-based Tract Relationship File to determine the exact mapping of a 1990 Census tract to its 2000 Census equivalent and to determine which tracts to exclude.
[22] This specification gives us the percentage change in GSE purchases.
[23] To avoid collinearity with $TM'_{i,\,old}$, the control TM_{old} is not included in vector $\mathbf{X_i}$ in regression (11).

large bandwidth and the low R-squared make it difficult to interpret the estimate as purely the UAG-effect. Column 2 includes tract-level housing and demographic controls and reduces the bandwidth to $h = 0.05$. The result is a much lower estimate of the UAG-effect, at about 2.6 percent, suggesting that tract characteristics have a large impact on the intensity of GSE purchasing activity. Column 3 also introduces an income control which measures just how strong the relative income fall into eligibility was for each tract. This regression allows us to conclude that the effect of just barely switching into eligibility is about a 5 percent increase in the number of GSE purchases.

However, looking at the coefficient on $(TM'_{old})*(\Delta D)$, we can also see that a unit increase in TM_{old}, a tract's pre-switch relative income, results in more than 20 percent fewer mortgage purchases. In other words, the UAG-effect dissipates by 0.2074 of a percent for every hundredth of a unit that a switching tract's TM_{old} was above 0.90, so that for a switcher whose TM_{old} was about 1.15, the UAG-effect is zero. As we can see from Table 4, the average switcher began with $TM_{old} = 1.0$, suggesting that the UAG-effect for the average switching tract is about $4.98 – (20.74)*(1.0-0.90) = 4.98 – 2.074 = 2.91$ percent.

Bhutta (2010) reports similar estimates for the regressions in columns 1 and 2 above, but we differ in our estimates in column 3. Bhutta measures a 5.8 percent increase in the number of GSE purchases from 2001-2002 to 2005-2006 for switching tracts and dissipation of the UAG-effect at only 0.5 percent per unit increase in TM_{old}. As with the UAG analysis for 1996-2002, one plausible explanation for this result is that my control group, the non-switchers, is seeing a larger percentage increase in the number of GSE purchases from 2001-2002 to 2005-2006 than the tracts in Bhutta's control group. Although my estimate of the UAG effect in the 1996-2002 analysis differs from the estimate I derive here, both analyses suggest that Bhutta's estimates are upward biased.[24] My ability to capture GSE purchases of seasoned mortgages and mortgages not directly sold to them likely allows me to have a more precise estimate of the UAG effect.

This analysis also reveals that the GSEs purchased more in tracts whose TM stayed relatively stable than in tracts whose TM dropped significantly, implying that the

[24] Recall that my estimates for the 1996-2002 period showed a UAG-effect between 0 and 3 percent (see columns 7-9 in Table 3), whereas here the estimate is between 2 and 5 percent.

UAG did not push the GSEs into tracts in serious economic decline. This conclusion is especially important given the fact that 2005-2006 saw the peak of the subprime mortgage market and that the goals have often been blamed for getting the GSEs deep into this market. If we consider high income variability as a proxy for high default risk and subprime status, this analysis gives reason to believe that the UAG was not responsible for the increased involvement of the GSEs in the subprime market, at least not through single-family mortgage purchases.

IV. Conclusion

The mortgage crisis prompted intense scrutiny of the GSEs with a particular focus on their mortgage purchases since the introduction of the AHGs in 1996. While the future of the GSEs and any governmental role in the housing finance system is now in question, it is plausible that the government may continue to encourage lending to low-income borrowers or borrowers in low-income areas. It is thus important to understand how to effectively target such lending. Studying the AHGs is a valuable and timely step in this direction.

Utilizing data currently made available through HUD's GSE Public Use Database and data from the 1990 and 2000 Decennial Censuses, I estimate the effect of one of the goals, the UAG, on the number of single-family mortgages purchased by the GSEs in poor and underserved neighborhoods from 1996 to 2002. I find a small and statistically insignificant effect: the UAG increased single-family mortgage purchases by 0 to 3 percent. Taking advantage of a change in the way HUD assesses GSE goal performance, I measure the UAG effect in 2005-2006 and find it to be between 2.5 and 5 percent. My estimates in both of these analyses are lower than those in Bhutta (2010) most likely because I am able to include purchases of seasoned mortgages and mortgages not directly sold to the GSEs in my analysis. Previous literature has not been able to assess the distribution of GSE purchases not captured by the HMDA dataset; using a new dataset, I conclude that these purchases are not more heavily concentrated in targeted tracts but instead are either distributed in a similar fashion to purchases contained in HMDA or are more heavily concentrated in untargeted tracts.

Since the UAG had such a small effect on GSE purchases in poor and underserved areas at the peak of the subprime mortgage market, I conclude that the goal and the GSEs' single-family mortgage purchases in these areas were not the drivers of the subprime market. Thinking about the UAG as a proxy for the AHGs as a whole, as is custom in the literature, I posit that single-family mortgage purchases made by the GSEs in response to the goals were not responsible for driving the increase in the number of high-risk borrowers in the mortgage market prior to the crisis.

Further research is required to understand the role played by the goals in spurring GSE multi-family mortgage purchases and REMIC investments, as well as the roles of these products in the growth and collapse of the housing market. Although data for such research should exist, much of it has so far not been made available by HUD and the FHFA. Other ways by which the GSEs could have influenced the growth of the subprime market should also be explored, including the effect of the growth in the size of the GSEs' balance sheets since the 1990s. Additional work should continue on understanding the potential effects of non-GSE factors, such as the perception that house prices can only go up, on subprime lending in the lead-up to the crisis.

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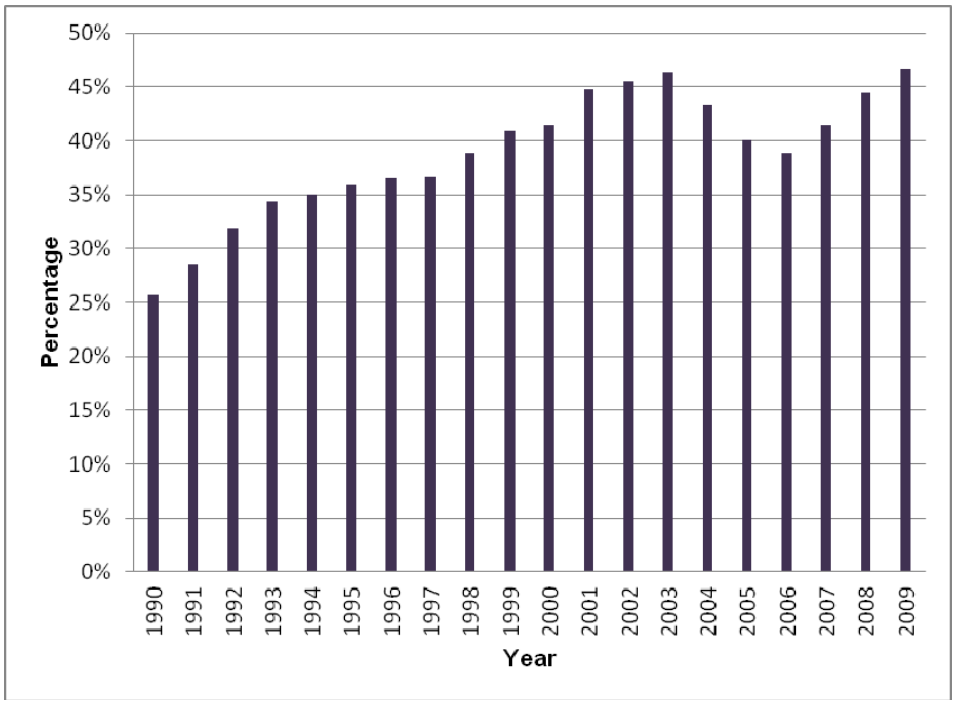
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VI. Appendix

Figure 1: Percentage of Total U.S. Residential Mortgage Debt Outstanding Owned by the GSEs



Source: Federal Housing Finance Agency (FHFA).

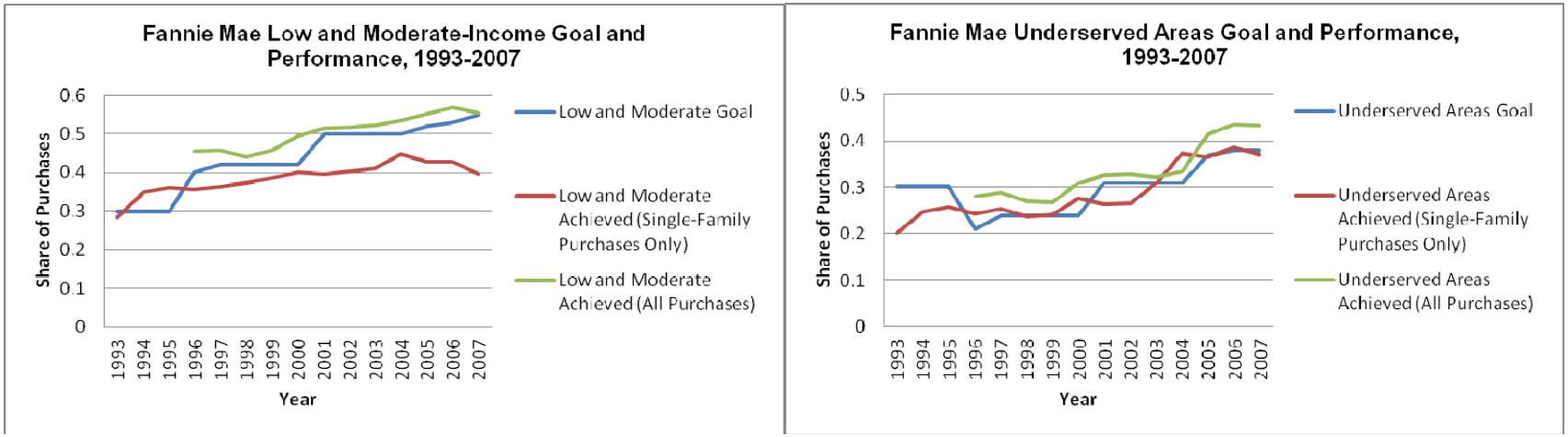
Table 1: GSE Affordable Housing Goals, 1993-2008

Year(s)	Low-Moderate Income	Special Affordable	Underserved Areas
1993-1995	30%	NA	30%
1996	40%	12%	21%
1997-2000	42%	14%	24%
2001-2004	50%	20%	31%
2005	52%	22%	37%
2006	53%	23%	38%
2007	55%	25%	38%
2008	56%	27%	39%

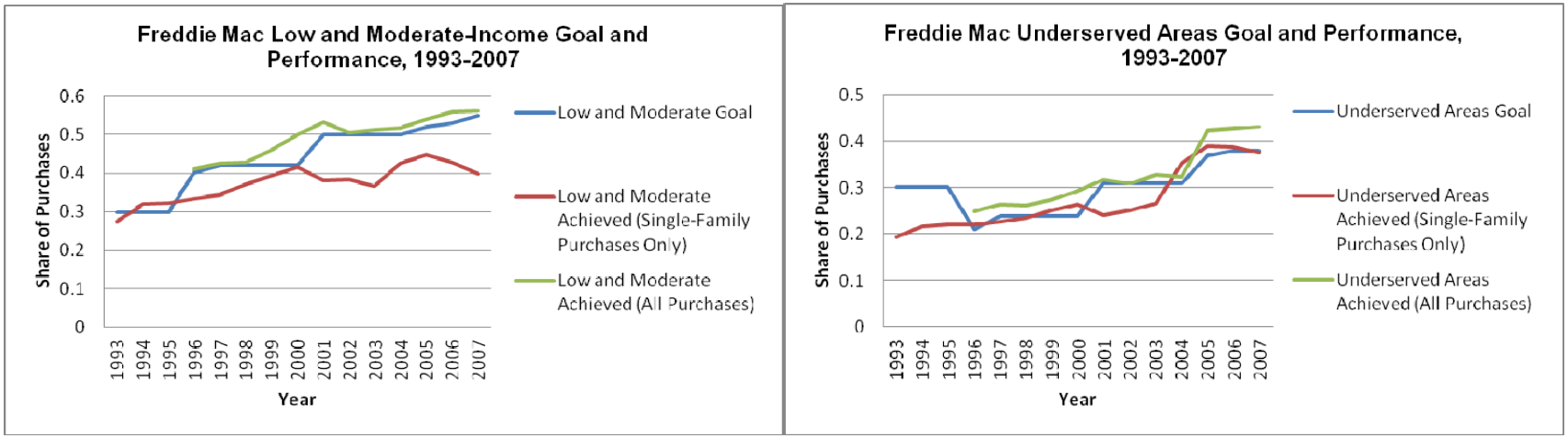
Source: Weicher, 2010.

Figure 2: Low and Moderate-Income Goal Performance and Underserved Areas Goal Performance, by GSE

Panel A: Fannie Mae



Panel B: Freddie Mac



Note: Panels show GSE performance on goals (1) using only single-family mortgage purchases and (2) using all purchases (single-family, multi-family, REMICs, etc.) for which the GSEs can attain goal credit. Data Sources: FHFA, 2010 and the GSE Public Use Database.

Table 2: Summary Statistics for Key GSE and 1990 Census Variables

	All Tracts		0.85≤TM≤0.90		0.90<TM≤0.95	
A. GSE Data Characteristics						
Number of Census Tracts	38,893		2,582		2,634	
Number of MSAs	908		653		658	
Number of Purchases (per tract per year, 1993-1995)	71.9	(89.7)	48.9***	(51.2)	57.6	(53.2)
Number of Purchases (per tract per year, 1996-2002)	102.9	(123.6)	80.9***	(79.5)	91.9	(88.2)
B. 1990 Census Tract Characteristics						
Population	4,459.2	(2,388.8)	4,428.6	(2,255.8)	4,501.1	(2,194.4)
Borrower Age	41.9	(11.2)	41.0	(11.0)	41.4	(11.3)
Proportion of Population Age 65+	0.13	(0.07)	0.141*	(0.08)	0.14	(0.07)
Proportion of Population Black	0.13	(0.24)	0.11***	(0.20)	0.08	(0.17)
Proportion of Population Hispanic	0.08	(0.16)	0.08***	(0.14)	0.07	(0.13)
Total Housing Units	1,811.3	(1,017.4)	1,845.4	(952.5)	1,857.6	(968.5)
Owner-Occupied Housing Units	1,068.3	(686.6)	1048.7***	(593.0)	1,106.5	(609.6)
Proportion of Detached Units	0.59	(0.28)	0.56***	(0.25)	0.60	(0.24)
Proportion of Multifamily Units	0.17	(0.21)	0.17	(0.20)	0.16	(0.19)
Proportion of Mobile Units	0.06	(0.10)	0.09**	(0.13)	0.08	(0.12)
Proportion of Units Built 1980-1989	0.18	(0.19)	0.16*	(0.16)	0.17	(0.16)
Proportion of Units Built 1940-1969	0.43	(0.23)	0.44	(0.22)	0.45	(0.22)
Proportion of Units Built Before 1940	0.20	(0.22)	0.22***	(0.22)	0.19	(0.21)
Proportion of Population in Group Quarters	0.01	(0.03)	0.01	(0.03)	0.01	(0.03)
Median Home Value	112,291.0	(86,878.5)	88040.8***	(58,229.2)	94,738.1	63,715.8

	All Tracts		0.88≤TM≤0.90		0.90<TM≤0.92	
A. GSE Data Characteristics						
Number of Census Tracts	38,893		1,055		1,016	
Number of MSAs	908		457		460	
Number of Purchases (per tract per year, 1993-1995)	71.9	(89.7)	52.5	(54.6)	55.3	(48.7)
Number of Purchases (per tract per year, 1996-2002)	102.9	(123.6)	85.3	(83.6)	91.3	(88.9)
B. 1990 Census Tract Characteristics						
Population	4,459.2	(2,388.8)	4,562.7	(2,394.4)	4,555.7	(2,173.6)
Borrower Age	41.9	(11.2)	40.9	(11.0)	41.3	(11.3)
Proportion of Population Age 65+	0.13	(0.07)	0.14	(0.08)	0.14	(0.08)
Proportion of Population Black	0.13	(0.24)	0.10***	(0.19)	0.08	(0.16)
Proportion of Population Hispanic	0.08	(0.16)	0.08	(0.14)	0.07	(0.13)
Total Housing Units	1,811.3	(1,017.4)	1,893.0	(1,008.8)	1,898.4	(937.5)
Owner-Occupied Housing Units	1,068.3	(686.6)	1,090.9	(592.6)	1,115.0	(581.0)
Proportion of Detached Units	0.59	(0.28)	0.58	(0.25)	0.59	(0.24)
Proportion of Multifamily Units	0.17	(0.21)	0.16	(0.19)	0.16	(0.18)
Proportion of Mobile Units	0.06	(0.10)	0.09	(0.13)	0.09	(0.13)
Proportion of Units Built 1980-1989	0.18	(0.19)	0.16	(0.17)	0.17	(0.16)
Proportion of Units Built 1940-1969	0.43	(0.23)	0.45	(0.22)	0.44	(0.22)
Proportion of Units Built Before 1940	0.20	(0.22)	0.21	(0.22)	0.20	(0.21)
Proportion of Population in Group Quarters	0.01	(0.03)	0.01	(0.03)	0.01	(0.03)
Median Home Value	112,291.0	(86,878.5)	86,690.2	(56,236.4)	90,067.8	(59,413.9)

Note: Standard deviation is in parentheses for means. *p<0.10, **p<0.05, ***p<0.01, where the p-value is from test of differences of means (tracts with 0.85≤TM≤0.90 vs. tracts with 0.90<TM≤0.95; and tracts with 0.88≤TM≤0.90 vs. tracts with 0.90<TM≤0.92).

Table 3: Estimates of the UAG Effect, 1996-2002

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)[a]
	First Set: Without Income Controls			Second Set: With Income Controls			Third Set: Narrow Range		
Below Cutoff Dummy (D)	-0.0798***	-0.0153		-0.0143	0.0040	0.0142	-0.0026	0.0110	
	(0.0124)	(0.0101)		(0.0350)	(0.0192)	(0.0154)	(0.0168)	(0.0134)	
UAG-Targeted Dummy (D*)			-0.0219**						0.0274
			(0.0110)						(0.0199)
TM'				0.7273	0.5820	0.4900			
				(0.9060)	(0.4742)	(0.3863)			
(TM')*(D)				3.1089**	-0.3875	-0.8382			
				(1.2318)	(0.6603)	(0.5298)			
Log Number of GSE Purchases, 1993-1995						0.4822***		0.4963***	0.5793***
						(0.0148)		(0.0198)	(0.0220)
R-Squared	0.629	0.709	0.709	0.015	0.709	0.807	0.718	0.812	0.816
N (tracts)	5,215	5,199	5,199	5,215	5,199	5,187	2,067	2,063	2,063
Bandwith (h)[b]	0.05	0.05	0.05	0.05	0.05	0.05	0.02	0.02	0.02
Tract Size Controls[c]	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Other Tract Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes

*p<0.10, **p<0.05, ***p<0.01. Dependent variable is the log number of GSE purchases for 1996-2002. MSA cluster-robust standard errors are reported in parentheses.

[a] Estimates calculated using 2SLS regression with D as an instrument for the true treatment status dummy D*.

[b] Tracts with TM between $0.90 \pm h$ are included in regressions.

[c] Tract size controls include log owner-occupied units and log total housing units.

Table 4: Summary Statistics for Switcher Tracts and Non-Switcher Tracts, 2001-2002/2005-2006

	Switchers (ΔD=1)		Non-Switchers (ΔD=0)	
A. Data Characteristics				
Number of Census Tracts	1,408		14,989	
Change in Tract-MSA Income Ratio (TM) (2000 TM minus 1990 TM)	-0.171***	(0.136)	0.007	(0.203)
Number of Purchases (per tract per year, 2001-2002)	103.63***	(79.07)	184.70	(137.73)
Number of Purchases (per tract per year, 2005-2006)	87.40***	(68.73)	114.99	(95.40)
B. Tract Characteristics, 2000				
TM_{new}	0.834***	(0.064)	1.311	(0.393)
Population	3,948.53***	(1,735.11)	4,649.68	(2,062.04)
Proportion of Population Age 65+	0.16***	(0.08)	0.15	(0.07)
Proportion of Population Black	0.07***	(0.14)	0.05	(0.10)
Proportion of Population Hispanic	0.06***	(0.08)	0.05	(0.09)
Total Housing Units	1,756.40***	(820.43)	1,927.78	(903.91)
Owner-Occupied Housing Units	1039.77***	(547.29)	1,369.51	(626.23)
Proportion of Detached Units	0.54***	(0.26)	0.71	(0.22)
Proportion of Multifamily Units	0.18***	(0.19)	0.13	(0.17)
Proportion of Mobile Units	0.05***	(0.11)	0.03	(0.07)
Proportion of Units Built 1990-2000	0.08***	(0.09)	0.14	(0.13)
Proportion of Population in Group Quarters	0.03***	(0.07)	0.04	(0.09)
Median Home Value	111,468.50***	(60,338.54)	186,239.90	(132,283.60)
C. Tract Characteristics, 1990				
TM_{old}	1.006***	(0.115)	1.305	(0.372)
Population	3,788.97***	(1,546.68)	4,180.62	(1,646.78)
Proportion of Population Age 65+	0.16***	(0.09)	0.13	(0.07)
Proportion of Population Black	0.05***	(0.13)	0.03	(0.08)
Proportion of Population Hispanic	0.03	(0.05)	0.03	(0.05)
Total Housing Units	1,648.52**	(740.52)	1,695.62	(755.42)
Owner-Occupied Housing Units	976.73***	(484.21)	1,173.98	(483.09)
Proportion of Detached Units	0.54***	(0.26)	0.71	(0.23)
Proportion of Multifamily Units	0.18***	(0.19)	0.13	(0.17)
Proportion of Mobile Units	0.06***	(0.10)	0.05	(0.08)
Proportion of Units Built 1980-1989	0.13***	(0.15)	0.19	(0.18)
Proportion of Units Built 1940-1969	0.48***	(0.23)	0.44	(0.23)
Proportion of Units Built Before 1940	0.23***	(0.24)	0.16	(0.19)
Proportion of Population in Group Quarters	0.03	(0.07)	0.03	(0.08)
Median Home Value	87,182.54***	(53,312.05)	142,499.10	(102,104.20)

Note: *p<0.10, **p<0.05, ***p<0.01, where the p-value is from a test of differences of means between switchers and non-switchers.

Standard deviation is in parentheses.

Table 5: Estimates of the UAG Effect, 2001-2002/2005-2006

	(1)	(2)	(3)
	Change in Log Number of GSE Purchases		
Switcher Dummy (ΔD)	0.1157***	0.0258*	0.0498***
	(0.0119)	(0.0119)	(0.0176)
TM'_{old}			0.0381**
			0.0806
$(TM'_{old})*(\Delta D)$			-0.2074
			(0.1043)
R-Squared	0.029	0.182	0.183
N (tracts)	6,435	1,771	1,771
Bandwith (h)[a]	0.20	0.05	0.05
Tract Controls[b]	No	Yes	Yes

*p<0.10, **p<0.05, ***p<0.01. Dependent variable is the change in the log number of GSE purchases, 2005-2006 values minus 2001-2002 values. MSA cluster-robust standard errors are reported in parentheses.

a Tracts with TM between 0.90 ± h are included in regressions.

b Tract controls include log median home value in 2000, log total number of owner-occupied units in 2000, log total number of housing units in 2000, prop. Black, prop. Hispanic, and TM_{old} (not included in regression (3)).