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Evidence of Government Subsidy on Mortgage Rate and Default: Revisited

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Abstract

I empirically evaluate the subsidized default insurance policy (implemented through the guarantee for government-sponsored enterprises) in the U.S. mortgage market. First, I find that the subsidy raised mortgage interest rates for loans eligible for the subsidy (conforming loans), which is contrary to conventional wisdom. I do so by applying regression discontinuity designs and using the exogenous variation generated by a mandate of the U.S. Congress. My strategy circumvents the endogeneity problem in conventional studies. Second, using various time-to-default models, I find that the subsidy raised the mortgage default probabilities of all conforming loans. The paper has important policy implications on financial regulation and financial stability: I caution regulators against interpreting the observed jumbo-conforming spread as an indication that the subsidy necessarily lowers mortgage rates and benefits conforming borrowers; highlights the adverse impact of the subsidy on financial stability; and calls for deeper housing finance reforms in the U.S. beyond the Dodd-Frank Wall Street Reform and Consumer Protection Act.

Keywords

U.S. mortgage, government-sponsored enterprises, default, regression discontinuity designs, duration models

On September 6, 2008, the U.S. federal government committed to invest as much as $188 billion through 2009 to keep government-sponsored enterprises (GSEs) solvent. This amounted to about 1.3% of U.S. GDP in 2008. Indeed, as *The New York Times* (9/08/2008) wrote, it was ''the biggest and costliest government bailout ever.'' As summarized by Frame, Wall, and White (2013), this bailout sparked many debates about the role of the federal government in the housing finance system, with opinions ranging from no role at all to insuring against all credit losses. To shed light on these debates, it is important to understand the effects of the subsidy to the housing finance system introduced through the government's backing of the housing GSEs.

In simple terms, the subsidy works as follows. Mortgage originators as a whole (consolidated to a single entity, referred to as ''bank'') provide loans to borrowers in the primary mortgage market. If the loan amount is below the pre-specified conforming loan limit (CLL) (being a ''conforming loan''), then it is eligible to receive the GSEs' default insurance, so that in the event the borrower defaults, the GSEs will fully compensate the bank for any credit loss (although not all eligible loans actually receive the default insurance, every conforming loan has a positive probability of being insured by the GSEs ex ante). For example, in 2006 and 2007, this limit was $417,000 in all states in the U.S.

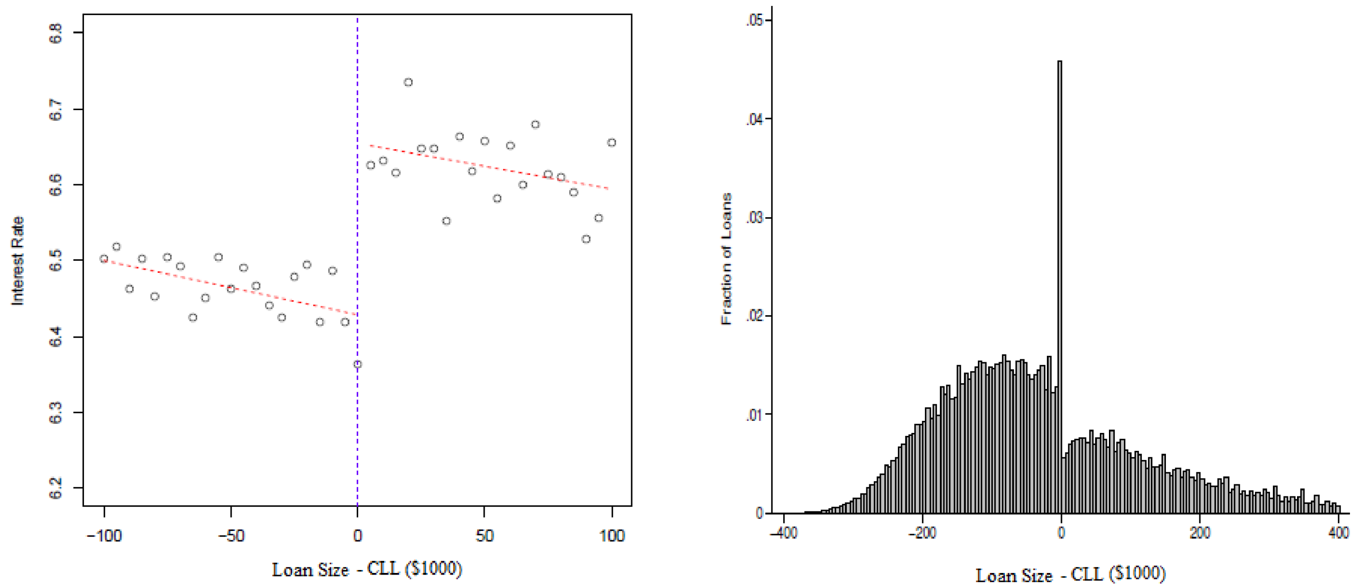
(except Alaska, Hawaii, Guam, and the U.S. Virgin Islands). If the loan amount is above the CLL (being a ''jumbo loan''), then it is ineligible for the insurance. Hence, the subsidy is effectively a default insurance program provided by the GSEs to the bank, but only for smaller loans. Note that for simplicity, I do not consider other criteria for conforming loans such as the credit score (hence, I do not distinguish ''jumbo loans'' from ''non-conforming loans''); in reality, the size limit criterion is enforced much more strictly than other criteria. This default insurance is subsidized (underpriced) for two reasons. On the one hand, the GSEs enjoy an implicit government guarantee: should the GSEs become insolvent due to mortgage credit losses, the federal government will step in and bail them out, which is what happened in 2008. This implicit government guarantee increases investors' confidence in the GSEs and enables the GSEs to borrow at a lower cost from the financial market, which constitutes a benefit to the GSEs. On the other hand, the competition between the two major GSEs (Fannie Mae and Freddie Mac) implies that they will undercut each other when pricing the default insurance, passing through some of the subsidy benefit to the bank in the form of underpriced default insurance. The focus of my paper is on the primary mortgage market between the borrowers and the bank, with the understanding that the bank receives a subsidy in the form of underpriced default insurance. In practice, GSEs' subsidized default insurance is more complicated, and it is done through GSEs' securitization. That is, GSEs purchase mortgages loans from the bank in the secondary market, issue mortgage-backed securities (MBSs) against a pool of mortgages, and provide default insurance on these MBS instead. For details, see Guttentag (2010, p. 138) and Acharya, Richardson, Van Nieuwerburgh, and White (2011, pp. 13 and 187).

This subsidized default insurance is likely to have important effects on the equilibrium contract terms offered by the bank to borrowers. In particular, it is likely to have differential effects on conforming loans and jumbo loans. Panel (a) of Exhibit 1, extracted from Figure 1 in DeFusco and Paciorek (2014), shows the observed interest rate schedule with respect to loan size for fixed-rate loans originated in 2006. Panel (a) shows that the interest rate schedule displays a clear discontinuity at the CLL, with average interest rates on loans just above the CLL being approximately 20 bps higher than those on loans just below. Panel (b) is a histogram of the loan size, which shows that the frequency of the loan size bunches exactly at the CLL, and drops substantially just above the CLL. It is the discontinuity in Panel (a) that is cited as supportive empirical evidence for the conventional wisdom that the subsidized default insurance decreased the interest rates of conforming loans.[1] However, my paper shows that this conventional wisdom does not hold, and that the subsidy raised mortgage interest rates of conforming loans.

The first and main contribution of this paper is to apply regression discontinuity designs to estimate the causal effect of GSEs' subsidized default insurance on mortgage interest rates. Doing so can correct for the endogeneity problem in conventional studies. The endogeneity problem arises because the subsidy policy is implemented in terms of the loan size, which is endogenously chosen by the borrower and thus is affected by the subsidy policy. Hence, it is possible that the subsidy has increased the interest rates of both the conforming and jumbo loans, but increased the jumbo loans by a larger amount so that we can still observe the jumbo-conforming interest rate spread (see Exhibit 2).

The main data set I use consists of the 30-year fixed-rate mortgage loan-level data collected by U.S. mortgage lenders under the Home Mortgage Disclosure Act (HMDA). My

Exhibit 1. Interest Rate-loan Size Plot (2006) and Loan Size Histogram

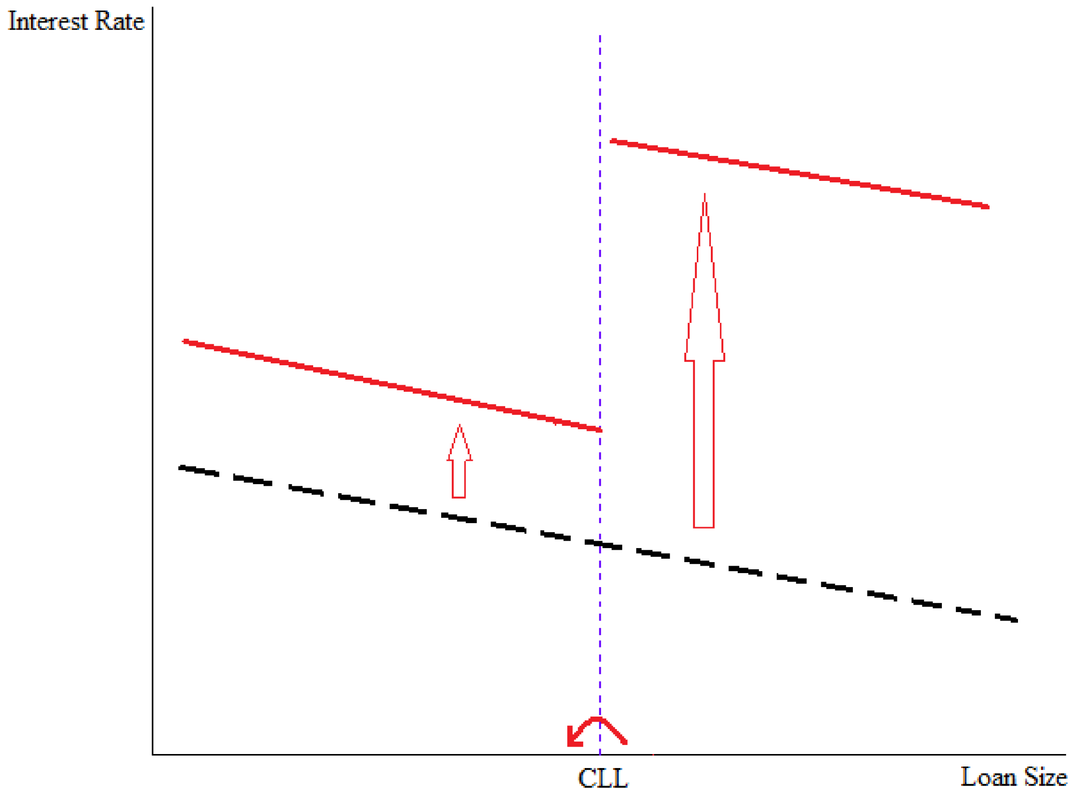


(a) Mean Interest Rate by Loan Size (2006)

(b) Loan Size Density

In Panel (a), each dot represents the mean interest rate in 2006 within a given $5,000 bin relative to the CLL ($417,000). The dashed lines are predicted values from a regression on the binned data. Panel (b) shows the fraction of all loans that are in any given $5,000 bin relative to the conforming limit. Data in Panel (b) are pooled across years and the sample includes all transactions in the primary DataQuick sample that fall within $400,000 of the conforming limit.

Exhibit 2. Illustration of Endogeneity



The dotted line represents the counterfactual schedule of the equilibrium interest rate and loan size without the subsidy/intervention. The introduction of the subsidy may have increased the interest rates both to the left and to the right of the subsidy eligibility cutoff (conforming loan limit, CLL), but increased the right by a larger amount. The arrow on the horizontal axis represents the potential self-selection by borrowers. Note that the slopes of the lines in this figure are for illustration; more precise representations of the actual equilibrium are available upon request.

identification strategy exploits the variation in interest rates generated by the GSEs' special affordable goal (SAG) mandated by U.S. Congress (one of the three ''mission goals''). Specifically, the SAG requires that at least a certain percentage of mortgage loans insured by GSEs should be taken by borrowers with incomes below 60% of the median income of its metropolitan statistical area (MSA) (or non-metropolitan county). Note that in practice, there is another criterion for the SAG, which is based on the census-tract income; to get cleaner results, I restrict my sample to loans that satisfy the borrower-income-based criterion.

My identification strategy can be summarized as follows. On the one hand, even if a mortgage loan falls below the loan size limit and is eligible to receive the subsidized default insurance (being a conforming loan), it does not necessarily get insured ex post. In fact, data show that only about 15% of the conforming loans actually get insured and receive the subsidy. On the other hand, the SAG implies that among the conforming loans, those taken by borrowers with incomes just below 60% of their MSA's median income will have a discontinuously higher ex ante probability to receive the subsidized default insurance than those with incomes just above. It is this higher ex ante subsidy probability that is the treatment in my empirical study. Since borrowers of GSE-insured loans are unlikely to be able to manipulate their incomes and since the 60% income cutoff is arguably an exogenous rule, borrowers just above and just below the 60% income cutoff are comparable (even in terms of the unobservable quality) except for the discontinuity in the ex ante probability of receiving the subsidized default insurance. Hence, any discontinuous change in the interest rate at the 60% cutoff must be attributed to the discontinuous change in the ex ante subsidy probability, and thus to the subsidy itself. Note that although some recent studies show evidence of mortgage borrowers misreporting incomes, this phenomenon mainly exists in the non-GSE insured mortgages and low-documentation mortgages.

Using this strategy, I find that loans with a discontinuously higher ex ante subsidy probability also have a discontinuously higher interest rate than those with a lower probability, even after controlling for borrower characteristics such as income and lien status. The difference is about 14 bps on average, which amounts to about 4% of the sample average (17 bps using data with income ratios between 59% and 61%). These effects seem to be economically small, but since the SAG eligibility is far from capturing the full effect of the GSEs' subsidized default insurance itself (it only measures the ex ante probability to receive that subsidy), the actual effect of the subsidy may still be economically large. These results imply that the subsidized default insurance has raised the mortgage interest rates of conforming loans, which is contrary to conventional wisdom. However, these results can be explained through the bank's moral hazard, as in the theoretical paper by Zhao (2018), which features asymmetric information between borrowers and the bank.

To better understand the empirical strategy, consider two borrowers from the same area. Suppose both borrowers take conforming loans, so both of them are eligible to receive the subsidy. Furthermore, suppose the first borrower has an income equal to 59.9% of the area's median income, and the second borrower has an income equal to 60.1%. These two borrowers are so similar to each other that their fundamental characteristics such as the credit scores and all other unobservable characteristics are also very similar. The only significant difference (i.e., discontinuous change in the underlying characteristic) is that the 59.9% borrower has a discontinuously higher ex ante probability to receive the default insurance subsidy than the 60.1% borrower. If the 59.9% borrower also has a discontinuously higher interest rate (as is the case in this paper), then it must be caused by the discontinuously higher subsidy probability. This is the main idea of regression discontinuity designs, as documented in Lee and Lemieux (2010), etc.[2]

As the second contribution of my paper, I also empirically show that the subsidy raises the mortgage default probabilities of all conforming loans. I do so by applying various time-to-default models (including exponential, Weibull, lognormal, and Cox models) to the single-family loan-level data first released by Freddie Mac in 2013. I find that all else being equal, being more likely to receive the government's mortgage subsidy shortens the conforming mortgage's time to default (i.e., raises the mortgage's default risk). These results are consistent with Acharya, Richardson, Van Nieuwerburgh, and White (2011), and further highlight the adverse impact of the subsidy that is identified in my first set of results.

The first question I examine (i.e., the effect of subsidy on mortgage rate) is related to the vast literature on estimating the size of the jumbo-conforming spread and the interpretation of the spread. This includes Cotterman and Pearce (1996), Ambrose, Buttimer, and Thibodeau (2001), Naranjo and Toevs (2002), Passmore, Sparks, and Ingpen (2002), and Ambrose, LaCour-Little, and Sanders (2004). McKenzie (2002) provides a summary. The estimates in these studies vary substantially, from as low as a few bps to as high as 60 bps. These authors generally regress the interest rate on a ''jumbo'' dummy under a particular parametric form, and interpret the coefficient of the jumbo dummy as the jumbo-conforming spread. Exceptions include Sherlund (2008), who non-parametrically estimates the effect of loan size and loan-to-value ratio on the interest rate while assuming the jumbo dummy enters linearly.

Moreover, in most of these studies, the estimated jumbo-conforming spread is interpreted as a proxy for the reduction in mortgage interest rates due to the GSE subsidy. One of the few exceptions is Passmore (2005), who points out that GSE shareholders and/or mortgage originators may capture some or all of the subsidy and do not pass it on to homeowners. My results are in line with those of Passmore (2005), but go one step further: the subsidy not only fails to be passed on to homeowners, but also hurts them due to its very presence.

As shown theoretically by Burgess, Passmore, and Sherlund (2005), the estimated jumbo-conforming spread is only a coarse measure of the GSEs' influence on mortgage rates. The reason emphasized by the authors is that mortgage rates depend on many factors other than the GSEs' subsidized default insurance, including the funding cost and the spreads needed to compensate for the credit, prepayment, and maturity mismatch risks of the mortgage. Each of these factors could be priced differently for jumbo versus conforming mortgages, thereby affecting the jumbo-conforming spread. Therefore, the authors conduct a second-step regression, which is to regress the jumbo-conforming spread (estimated using the traditional regression approach) on a measure of GSEs' funding advantage and the aforementioned factors. The authors then interpret the coefficient of the GSEs' funding advantage in the second-step regression as a more precise measure of GSEs' (causal) effect on mortgage rates.

As Kaufman (2014) points out, these studies are vulnerable to selection bias and sorting bias by borrowers, i.e., borrowers of higher quality (who would receive lower interest rates anyway) may have sorted into conforming loans, which causes bias in the estimate of the jumbo-conforming spread in the aforementioned studies. Sherlund (2008) attempts to address these biases. He uses geographic location to control for unobserved borrower characteristics, although assuming loans similar to each other in terms of loan size, loan-to-value ratio or geographic location might also be similar in other unobservable borrower characteristics. Kaufman (2014) instruments for a loan's conforming status using a discontinuous function of the home appraisal value, and finds that GSE purchase/insurance has lowered interest rates by about 10 bps over the period from 2003 to 2007. Adelino, Schoar, and Severino (2014) and Fuster and Vickery (2015) use similar instruments for conforming status.

Although the instrumental variable approach in Kaufman (2014) can help address the sorting bias and thus identify the effect of the subsidy on the interest rates of conforming loans relative to jumbo loans, it cannot identify the effect of the subsidy on conforming loans relative to the no-subsidy case. To identify that effect using a reduced-form study, one would need the pre-subsidy data. Given such data, one could use the variation in the interest rates (of conforming loans) before and after the introduction of the subsidy program. Unfortunately, such pre-subsidy data are not available because the subsidy was introduced in 1938 for Fannie Mae and 1970 for Freddie Mac; therefore, a theoretical model is needed to identify that effect. Of course, variation in the probability of receiving the subsidy generated by the mandate of Congress can be used to provide indirect evidence on the effect of the subsidy on conforming loans. That is the empirical strategy used in this paper, as noted earlier.

Bhutta (2012) and Moulton (2014) also use this strategy to study GSEs' mission goals, although they focus on the effect on GSE insurance (purchases) and volume of loan originations, rather than on interest rates. Bhutta (2012) finds that the underserved areas goal (UAG) has a positive but limited effect on GSE purchases and the mortgage origination volume. Moulton (2014) studies all three mission goals (SAG; UAG; low- and moderate-income goal, LMIG). He finds that the SAG increased GSE purchases but had no effect on mortgage lending (loan volumes and fraction of high-price loans), and that the UAG or LMIG did not alter GSE purchases or mortgage lending. These results are consistent with my empirical findings to the extent that the subsidy policy does not benefit consumers as much as intended.

The second question I examine is related to the literature on mortgage default. Elul et al. (2010), among others, discuss the modeling of the default decision using the ‘‘double trigger’’ framework: First, ‘‘illiquidity default,’’ i.e., the borrower would like to repay, but is not able to because of the low income received in that period, as in Gerardi, Shapiro, and Willen (2007), etc. Second, ‘‘strategic default,’’ i.e., the borrower chooses to default even if he is able to repay, as in the option approach by Deng, Quigley, and Van Order (2000), etc. Evidence by Bhutta, Dokko, and Shan (2010) and Gerardi, Herkenho, Ohanian, and Willen (2018) indicate that the ‘‘illiquidity default’’ played a more important role in the recent crisis. Using survey data, Guiso, Sapienza, and Zingales (2013) find that the propensity of ‘‘strategic default’’ is affected by both pecuniary and non-pecuniary factors (such as views about fairness and morality), and that exposure to other people who strategically defaulted increases the propensity to default strategically. But Gerardi, Herkenho, Ohanian, and Willen (2018) emphasize the important interaction between the two sources of default.

The rest of the paper is organized as follows. I present the analysis of the subsidy on mortgage rate. I then analyze mortgage default. The paper closes with concluding remarks.

Evidence on Mortgage Rate

Testable Hypotheses

To fix ideas, this subsection presents two hypotheses to be tested empirically. Regarding the effect on mortgage rate, I have:

Hypothesis 1: The introduction of the GSE subsidy program increases the equilibrium interest rate of any strictly conforming loan.

Regarding the effect on mortgage default, I have:

Hypothesis 2: The introduction of the GSE subsidy program increases the default probability of any strictly conforming loan, hence the aggregate default rate in the mortgage market.

The intuition of these hypotheses is provided in a companion theoretical paper by Zhao (2018), which involves the bank's moral hazard problem under asymmetric information. These two hypotheses are closely related to each other, in that Hypothesis 2 further highlights the adverse impact of the subsidy suggested by Hypothesis 1.

Data and Summary Statistics

The primary data source for testing Hypothesis 1 is the loan application-level data provided by U.S. mortgage lenders under the HMDA. The HMDA requires lenders to disclose information about the geographic location and other characteristics of the mortgage loans to facilitate enforcement of the fair lending laws. The lenders currently covered by the HMDA account for approximately 80% of all home lending nationwide, so the HMDA data set provides a representative picture of most home lending in the U.S. Since 1990, HMDA has required covered lenders to provide individual-level mortgage application information instead of the census-tract aggregate information in each calendar year (Avery, Brevoort, and Canner, 2007). The vast majority of these are 30-year fixed-rate loans.

Before explaining the data cleaning procedure, I would like to make several remarks on the data set. First, although the HMDA data set does not contain information on the contract interest rate, it does report the spread between the annual percentage rate (APR) and the applicable Treasury yield for the high-price loans since 2004. More precisely, the spread is reported for first-lien loans with spreads equal to or greater than 3 percentage points, and for second-lien loans with spreads equal to or greater than 5 percentage points. Since the APR also reflects the ''points'' and other fees paid by the borrower, it is a more accurate measure of the borrowing cost than the interest rate itself, and may be more suitable for the study of consumer welfare.

Second, the focus of the empirical study is to examine the change (rather than the level) of the interest rate caused by the subsidy. Assuming that the subsidy does not affect the risk-free rate (which seems a plausible assumption given that the risk-free rate is determined by many other macroeconomic factors), the change of the spread is equal to that of the interest rate, so the unavailability of the data on interest rate levels does not affect the identification of the effect of the subsidy.

Third, although the HMDA data set does not provide the borrower's credit score, this does not cause a bias in the estimation as long as the regression discontinuity design is valid. The reason is that the credit scores (and all unobservable characteristics) of borrowers who are sufficiently close to the 60% income cutoff must change in a smooth way, and thus cannot explain the discontinuous change in interest rates across the 60% income cutoff (indeed, non-mortgage loans such as credit card lendings and auto loans do not differentially target borrowers based on the same 60% income limit). In addition, the HMDA data set does provide information on whether a loan is insured by GSEs. This allows me to restrict the sample to loans that are ex post insured by GSEs, which can make the loans more comparable.

One may wonder how I can identify the effect of the subsidy if I only use the loans that have received the subsidy (i.e., being insured by GSEs). Regarding this, the crucial point is that my identification is achieved by using the ex ante different probabilities of receiving the subsidy (being insured by GSEs). Even though banks generally originate a loan knowing ex ante whether the loan will be eligible for the GSE subsidy (by running the loans through the GSEs' automated underwriting software), it is always uncertain whether that loan will actually be insured by GSEs ex post. Indeed, as explained above, only about 15% of the conforming loans actually get insured and receive the subsidy.

This implies that even among the loans that have received the GSE subsidy ex post, the bank understands there are still two distinctive groups at the time of setting the interest rates: one group has a discontinuously higher ex ante probability to receive the subsidy (the group with borrower incomes below the 60% cutoff), and the other group has a lower ex ante probability. If I can show that the first group also has a discontinuously higher interest rate, then it follows that the GSE subsidy has raised the mortgage interest rate. And by using loans that were actually insured by GSEs ex post, I can better control for the unobservables, while still achieving the identification. Note that other mortgage market features, such as prepayment and the possibility of taking piggyback loans, are unlikely to affect my identification because these features do not display a discontinuity exactly at the 60% income cutoff.

My sample period is from 2004 to 2007. I chose this period because the U.S. adopted a different definition of MSA in 2004, and because the bailout of GSEs in 2008 may have had a systematic impact on mortgage lending. I also merge the HMDA data set with the MSA median income data released by the U.S. Department of Housing and Urban Development to determine each loan's SAG status.

To focus on loans insured by the GSEs, I further clean the data by: (1) dropping loans larger than the CLL; (2) dropping loans insured by the Federal Housing Administration, or guaranteed by the Veterans Administration; (3) dropping refinance loans and home improvement loans, and keeping home-purchase loans only; (4) dropping denied and withdrawn applications, and keeping originated loans only; (5) following Bhutta (2012), dropping loans in Alaska, Hawaii, Guam, and the U.S. Virgin Islands; (6) dropping loans for which the sex, race, and ethnicity information is not provided by the borrower; and (7) dropping loans for which the spread information is not available.[3] The data cleaning yields a total of 1,712,419 observations.

The summary statistics of the cleaned data are presented in Exhibit 3. From 2004 to 2007, the average interest rate spread of all loans in the sample is about 5.37%. This is a relatively high spread, but one reason is that it is based on the APR, which is higher than the contract interest rate. The average loan size is about $133,000, and the average fraction of loans eligible for the SAG is 7.65%.

Exhibit 3 also presents the summary statistics for the GSE-insured and non-insured groups, respectively. As shown, the means of most variables significantly differ from each other

Exhibit 3. Summary Statistics for Both GSE-insured and Non-insured Loans

	(1)		(2)		(3)	
	All		GSE-insured		Non-insured	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Spread	5.371	1.557	3.754	0.865	5.475	1.534
b	133.012	97.690	151.477	82.838	131.820	98.452
SAG	0.077	0.266	0.164	0.370	0.071	0.257
Income	87.473	54.960	71.150	47.948	88.525	55.216
IncomeRatio	1.492	0.921	1.227	0.822	1.509	0.924
FirstLien	0.669	0.471	0.996	0.060	0.647	0.478
Male	0.645	0.479	0.662	0.473	0.644	0.479
Hispanic	0.237	0.425	0.162	0.368	0.242	0.428
Black	0.167	0.373	0.162	0.368	0.168	0.374
Tract-to-MSA	1.128	0.273	1.104	0.259	1.130	0.274

Notes: In (1), the number of observations is 1,712,419; in (2), the number of observations is 103,758; in (3), the number of observations is 1,608,661. b = loan size; IncomeRatio = borrower's income/MSA's median income; Tract-to-MSA = census tract's median income/MSA's median income.

in the two groups. The two-sample *t*-test also confirms this result. For example, the mean spread of the non-insured group is larger than that of the insured group by about 46% (1.72 percentage points). In addition, almost all of the GSE-insured loans are first-lien loans, whereas 64.67% of the non-insured loans are first-lien loans. The average incomes in these two groups also differ substantially, with the GSE-insured group being poorer than the non-insured group. These make economic sense, given that GSEs try to pick less risky loans while still satisfying their mission of serving relatively poor borrowers. These substantial differences between the two groups make it necessary to limit the sample to GSE-insured loans only. Doing so can also mitigate the bias due to the omitted variables in the HMDA data.

The summary statistics for the GSE-insured loans are presented in Exhibit 4. To facilitate the discussions in the statistical analyses, I also present the summary statistics of loans around 5%, 2%, and 1% of the 60% income cutoff. Note that although the average spread around the cutoff is quantitatively similar to that of the entire group of insured loans, the average loan size and income are much smaller than those of the entire group.

Empirical Results on Mortgage Rate

The empirical model used to test Hypothesis 1 is as follows:

$$Spread_i = \delta_0 + \delta_1 SAG_i + \delta_2 X_i + \nu_i, \quad (1)$$

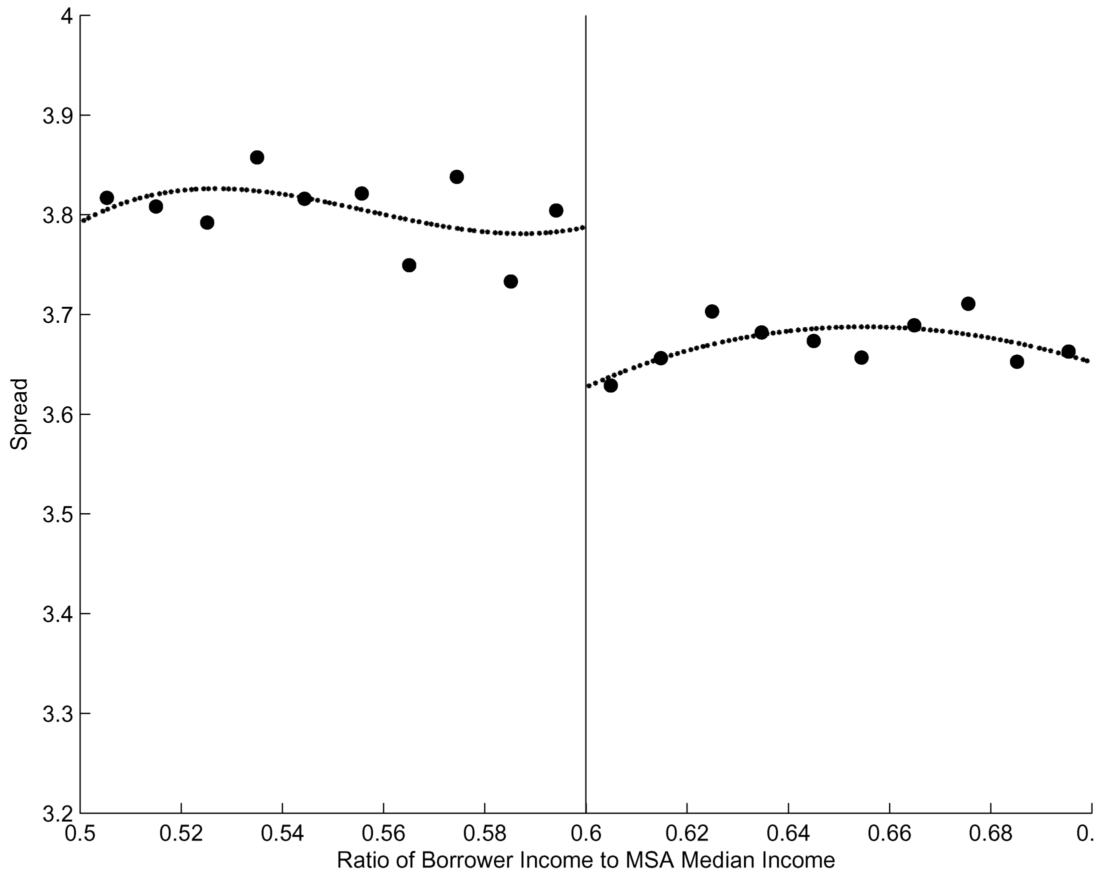
where $Spread_i$ is the difference between the interest rate of loan i and the risk-free rate; SAG_i is a dummy variable indicating whether loan i is eligible for the SAG, and it equals 1 if the ratio of borrower i's income to its MSA's median income falls below 60% (and 0 otherwise); X_i is a set of control variables, which includes borrower i's income level,

Exhibit 4. Summary Statistics for GSE-insured Loans

	(1)		(2)		(3)		(4)	
	All		0.55 ≤ IncomeRatio ≤ 0.65		0.58 ≤ IncomeRatio ≤ 0.62		0.59 ≤ IncomeRatio ≤ 0.61	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Spread	3.754	0.865	3.756	0.857	3.741	0.828	3.722	0.800
b	151.477	82.838	108.808	44.827	110.289	44.605	108.906	45.140
SAG	0.164	0.370	0.485	0.500	0.499	0.500	0.515	0.500
Income	71.150	47.948	35.985	5.892	36.030	5.764	35.880	5.694
IncomeRatio	1.227	0.822	0.601	0.029	0.600	0.011	0.600	0.006
FirstLien	0.996	0.060	0.998	0.041	0.999	0.038	0.999	0.033
Male	0.662	0.473	0.598	0.490	0.592	0.492	0.596	0.491
Hispanic	0.162	0.368	0.147	0.355	0.143	0.350	0.148	0.355
Black	0.162	0.368	0.186	0.389	0.193	0.395	0.185	0.389
Tract-to-MSA	1.104	0.259	1.040	0.194	1.041	0.194	1.048	0.202

Notes: In (1), the number of observations is 103,758; in (2), the number of observations is 8,872; in (3), the number of observations is 3,513; in (4), the number of observations is 1,845.

Exhibit 5. Discontinuity in Interest Rate Spread at the SAG Cutoff (2006)



income ratio (to its MSA's median income), lien status, gender, ethnicity, and race; and ν_i is the error term. Hypothesis 1 is equivalent to $\delta_1 > 0$.[4]

I begin with a graphical analysis of the SAG's effect at the 60% income ratio cutoff, using 2006 as an example. See Exhibit 5, which shows the average values of the outcome variable (the interest rate spread) for different values of the assignment variable (the ratio of borrower income to MSA median income). In addition, I fit the data non-parametrically on either side of the cutoff. As illustrated in Exhibit 5, the discontinuity in the fitted lines at the income ratio cutoff provides initial evidence of a SAG-induced discontinuous increase in the interest rate spread in 2006. The following statistical analysis confirms such a discontinuity.

Specifically, I use regression discontinuity (RD) designs to estimate the effect of being eligible for the SAG on mortgage interest rates. Although unobservable characteristics pose a challenge for the identification in general, a valid RD design can still identify the causal effect of the treatment on the outcome variable, as noted earlier. In particular, for this study, the treatment is having a higher ex ante priority for the subsidized default insurance, the outcome variable is the interest rate spread, and the assignment variable is the ratio of the borrower's income over the MSA's median income.

Following Imbens and Lemieux (2008), I use a local linear regression in the RD design. Specifically, I control for the assignment variable on either side of the cutoff, captured by the interaction term between the assignment variable and the SAG dummy. As discussed later, all other covariates change continuously at the cutoff in my study, so controlling for the assignment variable should be sufficient to identify the causal effect. However, including a set of controls provides a robustness check and also reduces the variance, as noted by Lee and Lemieux (2010).

The regression results with various specifications are presented in Exhibit 6. All specifications control for year fixed effects. Column (1) presents the results of global regressions using all observations, and columns (3)–(8) present the results using observations close to the cutoff. For example, in the regression for column (5), I use loans with borrower incomes falling between 58% and 62% of their MSA's median income. The closer to the cutoff, the more reliable the RD's result, provided that there are enough observations and that the RD design remains valid. In the regressions for columns (3)–(8), I control for MSA fixed effects, where standard errors are clustered at the MSA-level. In the regressions for all columns, I control for the income ratio (the assignment variable), and in all the regressions for the even-numbered columns, I also control for the income level (in addition to the income ratio) in order to mitigate the omitted variable bias. As shown in Exhibit 6, the coefficients of the SAG are very similar with and without the income level, confirming the validity of the RD design.

The key variable of interest in the regressions for Exhibit 6 is the SAG dummy. Columns (5)–(6) show that being eligible for the SAG has a statistically significant effect on the interest rate spread, and it raises the spread (and the interest rate) by 11 bps, amounting to about 2.9% of the average spread in the corresponding sample. Using loans that are even closer to the cutoff (for borrowers with incomes between 59% and 61% of their MSA median income), the results in columns (7)–(8) show that the SAG eligibility raises the spread by 17 bps, amounting to about 4.6% of the average spread in the corresponding sample. These effects are economically small, but since the SAG eligibility is far from capturing the full effect of the GSEs' subsidized default insurance itself (SAG only measures the probability of receiving that subsidy), the actual effect of the subsidy may still be economically large. The results in these four columns establish the key empirical results of the paper: having a higher subsidy probability raises the interest rate spread of conforming loans, as well as the interest rate level itself (assuming the risk-free rate is unaffected). This result confirms Hypothesis 1, and contrasts with conventional wisdom.

Note that although the SAG eligibility decreases the spread in the global regressions and has no effect in the 5% regressions, these effects are likely driven by the fact that it is hard to correctly identify the economically small effect of the SAG using observations sufficiently far from the cutoff. Thus, I can mainly rely on columns (5)–(8) when interpreting the results.

Other covariates in the regressions provide a way to double check my empirical strategy. As expected, the status of being a second-lien loan significantly increases the spread, both statistically and economically. In addition, the effects of the gender and almost all race

Exhibit 6. RD Design Results for the SAG

Spread	(1) Global	(2) Global with Income	(3) 5% with MSA	(4) 5% with MSA & Income	(5) 2% with MSA	(6) 2% with MSA & Income	(7) 1% with MSA	(8) 1% with MSA & Income
SAG	−0.0308**	−0.0332***	0.0279	0.0271	0.1078*	0.1082*	0.1719*	0.1659*
	(0.012)	(0.007)	(0.450)	(0.465)	(0.074)	(0.072)	(0.062)	(0.077)
Income		−0.0019***		−0.0132		0.0038		0.0414
		(0.000)		(0.245)		(0.846)		(0.176)
Second-lien	2.4030***	2.4140***	3.5169***	3.5154***	3.6244***	3.6222***	3.3437***	3.2749***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	−0.0101*	−0.0106*	−0.0139	−0.0140	−0.0372	−0.0372	−0.0536	−0.0535
	(0.070)	(0.058)	(0.507)	(0.505)	(0.213)	(0.213)	(0.257)	(0.259)
Not Hispanic or Latino	0.0826***	0.0942***	−0.0100	−0.0109	0.0314	0.0316	0.0108	0.0144
	(0.000)	(0.000)	(0.713)	(0.688)	(0.472)	(0.468)	(0.841)	(0.788)
Asian	−0.1568***	−0.1506***	0.0466	0.0454	0.0299	0.0302	−0.1085	−0.1038
	(0.000)	(0.000)	(0.719)	(0.726)	(0.839)	(0.838)	(0.672)	(0.686)
Black	0.2011***	0.2002***	0.3293***	0.3289***	0.3039**	0.3043**	0.1032	0.1071
	(0.000)	(0.000)	(0.006)	(0.006)	(0.020)	(0.020)	(0.645)	(0.633)
Native Hawaiian or Other Pacific Islander	−0.0297	−0.0305	0.2680	0.2681	−0.1196	−0.1191	−0.3927	−0.3807
	(0.533)	(0.523)	(0.239)	(0.237)	(0.620)	(0.622)	(0.159)	(0.173)
White	−0.0243	−0.0268	0.0588	0.0588	−0.0113	−0.0109	−0.1522	−0.1483
	(0.424)	(0.378)	(0.582)	(0.581)	(0.922)	(0.924)	(0.463)	(0.475)
IncomeRatioPrim_L	−0.0196	0.0980	−0.0861	0.6816	1.3762	1.1703	16.5731	13.9400
	(0.816)	(0.248)	(0.929)	(0.542)	(0.701)	(0.753)	(0.278)	(0.380)
IncomeRatioPrim_R	0.0057	0.1075***	1.6552*	2.4353**	6.0874*	5.8920	−2.5920	−6.7756
	(0.138)	(0.000)	(0.061)	(0.025)	(0.089)	(0.134)	(0.829)	(0.583)
R^2	0.066	0.067	0.104	0.104	0.170	0.170	0.208	0.210

Notes: In (1) and (2), the number of observations is 103,758; in (3) and (4), the number of observations is 8,872; in (4) and (5), the number of observations is 3,513; in (7) and (8), the number of observations is 1,845. *P*-values are in parentheses; Standard errors are clustered at the MSA level. SAG = Special Affordable Goal. IncomeRatioPrim = IncomeRatio − 0.60. IncomeRatioPrim_L = IncomeRatioPrim ∗ SAG. IncomeRatioPrim_R = IncomeRatioPrim ∗ (1-SAG). The baseline case of the dummy variable for race is American Indian or Alaska Native.

* $p < 0.1$.

** $p < 0.05$.

*** $p < 0.01$.

dummies are statistically insignificant. This makes economic sense given that the very intention of the HMDA is to prevent gender and racial discrimination and to enforce fair lending.[5]

Validity Testing of the RD Designs

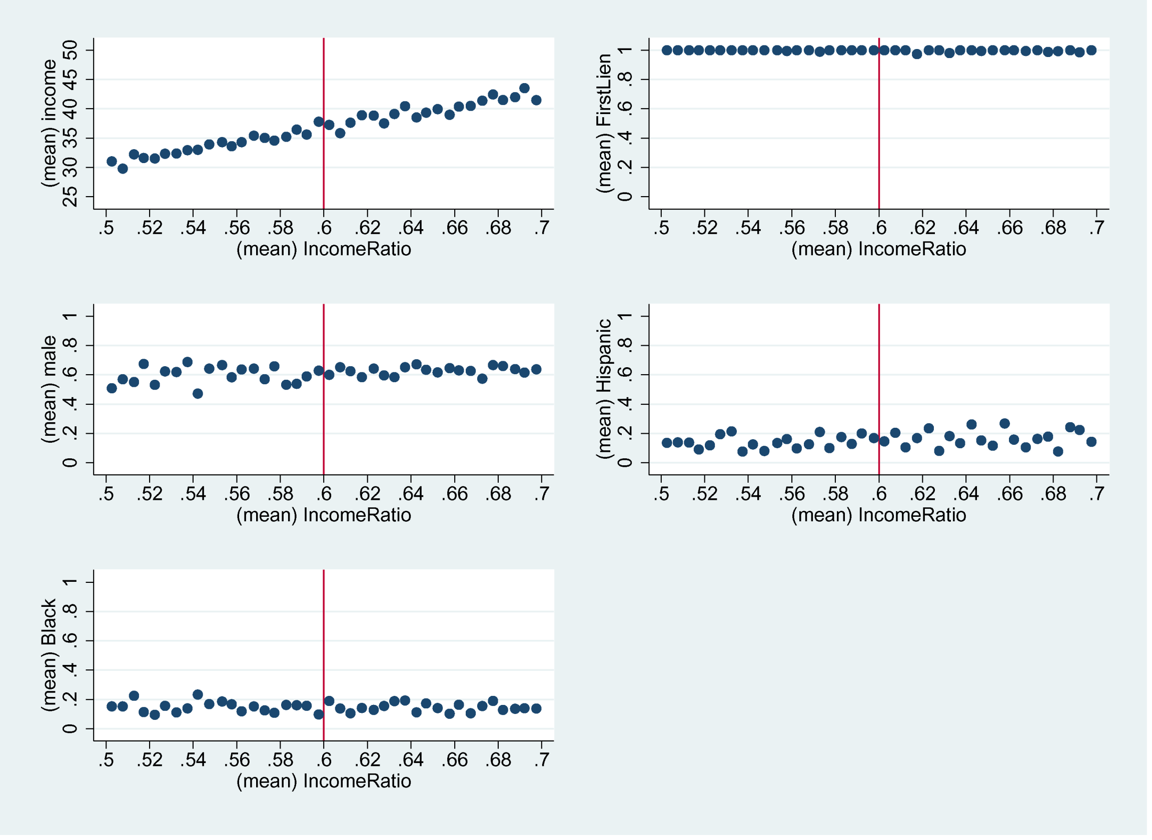
Two conditions are needed for the RD designs to be valid. First, the subjects should have an imprecise control over the assignment variable; otherwise the design suffers from a self-selection problem. In my case, this self-selection problem means that borrowers manipulate (i.e., mis-report) their incomes to be just below or above the median income of their MSA, depending on whether the offered equilibrium interest rates for loans below the income cutoff are lower or higher than those above. However, such a mis-reporting is unlikely, given that the borrowers of GSE-insured mortgage loans are required to submit verifiable income documents when applying for these loans.

Some recent studies show evidence of mortgage borrowers misreporting incomes (e.g., LaCour-Little and Yang, 2013; Jiang, Nelson, and Vytlacil, 2014; Ambrose, Conklin, and Yoshida, 2016; Mian and Sufi, 2017). However, as pointed out by these studies, this phenomenon mainly exists in the non-GSE insured mortgages and low-documentation mortgages. For example, Mian and Sufi (2017) find that fraud was most prevalent ''among mortgages originated from 2002 to 2005 sold for non-GSE securitization.'' Ambrose, Conklin, and Yoshida (2016) find that ''the majority of adverse selection and income falsification is confined to a specific borrower group,'' which consists of borrowers who originated low-documentation loans but could have easily originated full-documentation mortgages instead. Moreover, the same study warns against policies that are based on the assumption of income falsification in other borrower groups and that are designed to eliminate activities associated with excesses in mortgage originations. Given that I only consider the loans actually insured by GSEs and that GSEs have strict rules regarding income documentation, misreporting is unlikely to be a concern in my case. These arguments support the validity of the RD designs in terms of the first condition.

The second condition is that the baseline covariates other than the assignment variable (such as the income level) should have smooth distributions across the cutoff, so that I can attribute the discontinuity in the outcome variable to that in the treatment status. One way to check this is to simply plot the bin means of the baseline covariates and visually check whether the bin mean displays a discontinuity at the cutoff. As Exhibit 7 makes clear, all covariates are distributed smoothly across the cutoff, including the average income, the fraction of first-lien loans, the fraction of loans to male borrowers, etc.

An alternative and more rigorous way to test the second condition is to do a separate local linear regression for each baseline covariate, replacing the outcome variable by the covariate in the local linear regression. As noted by Lee and Lemieux (2010), with multiple covariates, it is useful to combine the multiple and separate tests into a single test statistic. The authors suggest running a seemingly unrelated regression (SUR) for each covariate, and then performing a chi-square test for all the coefficients of the treatment dummy being zero. I follow the suggestion, and the chi-square statistic in my case turns out to be 2.07, with a p-value of 0.9132. Hence, I cannot reject the null hypothesis that all the

Exhibit 7. Validity Testing for the SAG: Balanced Distributions of Covariates



coefficients of the treatment dummy are zero. In sum, the RD designs I conduct have passed the validity tests.

Robustness Check

This subsection presents a range of robustness checks. First, I conduct some falsification tests to rule out the possibility that the discontinuity results for the SAG are spurious. Specifically, since Exhibit 5 seems to contain another discontinuity at the income ratio of 57%, I conduct the RD designs at this income ratio, as well as at 63% to account for the symmetry. The regression results for these falsification tests are presented in Exhibits 8 and 9. As the two exhibits make clear, SAG is insignificant in all local regressions (columns (3)–(8)), in both the 57% and 63% cases). These results confirm that the discontinuous jump of the interest rate at the 60% cutoff is unlikely to be generated by spurious results, but rather by the underlying subsidy mechanism associated with the 60% cutoff.

Second, I conduct some robustness checks using a different data set. Since the HMDA data only contain the spread instead of the interest rate itself, one may wonder if the results with SAG are just driven by this measurement error. To examine this, I use a random sample extracted from another data set, which contains the interest rate information. This is the single-family loan-level (SFLL) data released by Freddie Mac in 2013 and periodically updated. Note that I do not use the SFLL data for the main result while testing Hypothesis 1, because it does not have the data for borrower income. Instead, I impute the borrower income information from the ''back-end'' debt-to-income (DTI) ratio.

I also examine the period from 2004 to 2007, and I follow a similar data cleaning procedure as for the HDMA data where possible. In addition, I keep loans with FICO scores above 620 to make the loans more comparable, since 620 is said to be another conforming criterion used by GSEs. The ultimate sample used in the robustness check contains 47,961 observations.

The SFLL data set does not provide geographic information needed to determine the SAG status of a loan. As a result, I impute the borrower's income and study the effect of the ''low- and moderate-income goal (LMIG),'' which is another mission goal mandated by Congress. Specifically, the goal requires that at least a certain percentage of mortgage loans purchased by GSEs should be for borrowers with incomes below 100% of the median income of its MSA (or non-metropolitan county). Similar with the case of the SAG, I apply the RD designs and compare interest rates around the 100% income ratio cutoff.

The results for the LMIG with various specifications are presented in Exhibit 10. Across all columns, as expected, both the FICO score and the income level have significantly negative effects on the interest rate, and the LTV has a significantly positive effect. For example, a one-point increase in the FICO score lowers the interest rate by about 0.06 bps. However, again contrary to conventional wisdom, being eligible for the LMIG (and having a higher priority for the subsidized default insurance) has a statistically positive effect on the interest rate. The magnitude is 20–25 bps, amounting to 3%–4% of the average interest rate in the corresponding sample.

Exhibit 8. Falsification Test Results for the SAG at 57%

Spread	(1) Global	(2) Global with Income	(3) 5% with MSA	(4) 5% with MSA & Income	(5) 2% with MSA	(6) 2% with MSA & Income	(7) 1% with MSA	(8) 1% with MSA & Income
SAG	−0.0207 (0.114)	−0.0233* (0.075)	−0.0145 (0.728)	−0.0141 (0.734)	−0.0069 (0.918)	−0.0047 (0.944)	−0.0570 (0.605)	−0.0546 (0.624)
Income		−0.0019*** (0.000)		−0.0245** (0.048)		−0.0318 (0.277)		−0.0202 (0.691)
Second-lien	2.4035*** (0.000)	2.4145*** (0.000)	3.8372*** (0.000)	3.8469*** (0.000)	4.2639*** (0.001)	4.2564*** (0.001)	3.7318** (0.013)	3.7267** (0.013)
Female	−0.0104* (0.062)	−0.0109* (0.051)	−0.0487** (0.020)	−0.0487** (0.020)	−0.0213 (0.544)	−0.0213 (0.545)	−0.0541 (0.307)	−0.0541 (0.308)
Not Hispanic or Latino	0.0825*** (0.000)	0.0941*** (0.000)	−0.0003 (0.993)	−0.0023 (0.949)	−0.0104 (0.828)	−0.0123 (0.795)	−0.0706 (0.302)	−0.0700 (0.304)
Asian	−0.1563*** (0.000)	−0.1501*** (0.000)	0.0429 (0.702)	0.0399 (0.723)	−0.0035 (0.987)	−0.0087 (0.968)	−0.0610 (0.849)	−0.0641 (0.842)
Black	0.2012*** (0.000)	0.2004*** (0.000)	0.2812*** (0.001)	0.2779*** (0.001)	0.2790 (0.106)	0.2788 (0.105)	0.2244 (0.358)	0.2288 (0.354)
Native Hawaiian or Other Pacific Islander	−0.0295 (0.536)	−0.0302 (0.526)	0.2278 (0.121)	0.2258 (0.124)	0.3736 (0.163)	0.3775 (0.155)	0.4931 (0.222)	0.4947 (0.225)
White	−0.0242 (0.426)	−0.0266 (0.381)	0.0263 (0.744)	0.0230 (0.775)	0.0753 (0.631)	0.0765 (0.624)	0.1361 (0.552)	0.1400 (0.545)
Not Hispanic or Latino	0.0825*** (0.000)	0.0941*** (0.000)	−0.0003 (0.993)	−0.0023 (0.949)	−0.0104 (0.828)	−0.0123 (0.795)	−0.0706 (0.302)	−0.0700 (0.304)
IncomeRatioPrim_L	0.0305 (0.763)	0.1485 (0.145)	−1.2367 (0.177)	0.2511 (0.841)	−2.0000 (0.585)	−0.1194 (0.978)	−14.0361 (0.348)	−12.8680 (0.407)
IncomeRatioPrim_R	0.0070* (0.065)	0.1085*** (0.000)	−0.3891 (0.702)	1.0627 (0.430)	2.9499 (0.515)	4.9946 (0.305)	11.8213 (0.362)	13.3020 (0.341)
R^2	0.066	0.067	0.103	0.103	0.148	0.148	0.200	0.200

Notes: In (1) and (2), the number of observations is 103,758; in (3) and (4), the number of observations is 8,448; in (5) and (6), the number of observations is 3,349; in (7) and (8), the number of observations is 1,721. *P*-values are in parentheses. SAG = 1 if the loan is eligible for the SAG. IncomeRatioPrim = IncomeRatio − 0.60. IncomeRatioPrim_L = IncomeRatioPrim ∗ SAG. IncomeRatioPrim_R = IncomeRatioPrim ∗ (1 − SAG). The baseline case of the dummy variable for race is American Indian or Alaska Native.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

Exhibit 9. Falsification Test Results for the SAG at 63%

Spread	(1) Global	(2) Global with Income	(3) 5% with MSA	(4) 5% with MSA & Income	(5) 2% with MSA	(6) 2% with MSA & Income	(7) 1% with MSA	(8) 1% with MSA & Income
SAG	−0.0456***	−0.0474***	−0.0152	−0.0154	−0.0021	0.0018	0.0379	0.0434
	(0.000)	(0.000)	(0.675)	(0.672)	(0.970)	(0.975)	(0.743)	(0.704)
Income		−0.0019***		0.0044		−0.0261		−0.0292
		(0.000)		(0.714)		(0.189)		(0.261)
Second-lien	2.4024***	2.4134***	3.2483***	3.2497***	3.3783***	3.3669***	3.6571***	3.6552***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	−0.0099*	−0.0103*	0.0063	0.0063	−0.0030	−0.0031	−0.0025	−0.0019
	(0.077)	(0.064)	(0.765)	(0.766)	(0.923)	(0.922)	(0.958)	(0.969)
Not Hispanic or Latino	0.0828***	0.0945***	−0.0106	−0.0104	−0.0227	−0.0239	−0.0273	−0.0257
	(0.000)	(0.000)	(0.687)	(0.693)	(0.597)	(0.578)	(0.600)	(0.623)
Asian	−0.1571***	−0.1509***	0.1178	0.1173	0.1448	0.1416	−0.2065	−0.1960
	(0.000)	(0.000)	(0.377)	(0.378)	(0.505)	(0.515)	(0.690)	(0.704)
Black	0.2012***	0.2004***	0.3995***	0.3991***	0.4441**	0.4425**	0.0698	0.0766
	(0.000)	(0.000)	(0.000)	(0.000)	(0.025)	(0.024)	(0.875)	(0.863)
Native Hawaiian or Other Pacific Islander	−0.0299	−0.0307	0.2549	0.2539	0.4550	0.4541	−0.1355	−0.1282
	(0.531)	(0.520)	(0.191)	(0.194)	(0.310)	(0.308)	(0.859)	(0.867)
White	−0.0240	−0.0265	0.0878	0.0872	0.0991	0.0976	−0.2608	−0.2538
	(0.429)	(0.383)	(0.374)	(0.378)	(0.584)	(0.587)	(0.552)	(0.562)
IncomeRatioPrim_L	−0.0731	0.0458	0.7539	0.4872	1.0995	2.9879	−4.5741	−4.9940
	(0.306)	(0.526)	(0.402)	(0.708)	(0.726)	(0.371)	(0.791)	(0.768)
IncomeRatioPrim_R	0.0036	0.1057***	0.5898	0.3239	−0.4953	1.0779	13.7251	17.9379
	(0.351)	(0.000)	(0.542)	(0.770)	(0.881)	(0.749)	(0.276)	(0.162)
R^2	0.066	0.067	0.117	0.117	0.163	0.164	0.205	0.206

Notes: In (1) and (2), the number of observations is 103,758; in (3) and (4), the number of observations is 9,088; in (5) and (6), the number of observations is 3,678; in (7) and (8), the number of observations is 1,835. *P*-values are in parentheses. SAG = 1 if the loan is eligible for the SAG. IncomeRatioPrim = IncomeRatio − 0.60. IncomeRatioPrim_L = IncomeRatioPrim ∗ SAG. IncomeRatioPrim_R = IncomeRatioPrim ∗ (1 − SAG). The baseline case of the dummy variable for race is American Indian or Alaska Native.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

Exhibit 10. RD Design Results for the LMIG

Interest Rate	(1) Global	(2) Global with Income	(3) 5%	(4) 5% with Income	(5) 3%	(6) 3% with Income	(7) 2%	(8) 2% with Income
LMIG	0.0020	0.0029	0.0979	0.1001	0.1984**	0.1992**	0.2562**	0.2524**
	(0.913)	(0.876)	(0.182)	(0.172)	(0.037)	(0.036)	(0.037)	(0.040)
FICO	−0.0005***	−0.0005***	−0.0005**	−0.0005**	−0.0008***	−0.0008***	−0.0006*	−0.0006*
	(0.000)	(0.000)	(0.022)	(0.028)	(0.005)	(0.006)	(0.081)	(0.084)
LTV	0.0043***	0.0043***	0.0059***	0.0057***	0.0061***	0.0060***	0.0071***	0.0070***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Income		−0.0024***		−0.0025*		−0.0016		−0.0024
		(0.000)		(0.096)		(0.431)		(0.363)
MSA_college	−0.0030***	−0.0020***	−0.0030*	−0.0011	−0.0048*	−0.0033	−0.0027	−0.0007
	(0.000)	(0.000)	(0.090)	(0.615)	(0.050)	(0.286)	(0.390)	(0.865)
IncomeRatioPrim_L	−0.2536***	−0.1219	8.6513	8.4932	11.0505	10.3582	63.4838	61.1125
	(0.006)	(0.200)	(0.345)	(0.354)	(0.586)	(0.610)	(0.103)	(0.117)
IncomeRatioPrim_R	0.2649	0.4023**	2.4529	3.0107	38.3895*	39.0203*	18.2800	17.1127
	(0.133)	(0.024)	(0.794)	(0.749)	(0.054)	(0.051)	(0.638)	(0.660)
IncomeRatioPrim2_L	−0.7142***	−0.7224***	272.7786	260.8051	101.5716	45.2966	7,230.8168	6,954.7399
	(0.002)	(0.001)	(0.524)	(0.542)	(0.948)	(0.977)	(0.109)	(0.124)
IncomeRatioPrim2_R	−0.6407	−0.6852	−251.6613	−265.9723	−3,185.1832**	−3,214.3421**	146.1488	397.0764
	(0.210)	(0.180)	(0.578)	(0.556)	(0.046)	(0.044)	(0.975)	(0.932)
IncomeRatioPrim3_L	−0.8173***	−0.8182***	2,547.9374	2,426.2178	−9,145.7807	−10,282.1672	243,913.5802	235,185.4578
	(0.000)	(0.000)	(0.652)	(0.667)	(0.789)	(0.764)	(0.100)	(0.114)
IncomeRatioPrim3_R	0.4564	0.5032	4,857.7864	4,997.9052	67,620.2224*	68,096.5327*	−71,979.5064	−81,964.9666
	(0.250)	(0.205)	(0.423)	(0.410)	(0.057)	(0.055)	(0.642)	(0.597)
R^2	0.375	0.375	0.354	0.356	0.359	0.359	0.363	0.364

Notes: In (1) and (2), the number of observations is 47,961; in (3) and (4), the number of observations is 1,343; in (5) and (6), the number of observations is 830; in (7) and (8), the number of observations is 566. LMIG = 1 if the borrower satisfies the criterion of the ''Low-and-Moderate-Income Goal,'' and 0 otherwise. IncomeRatio = borrower income/MSA median income. IncomeRatioPrim = IncomeRatio − 1. IncomeRatioPrim_L = IncomeRatioPrim ∗ LMIG. IncomeRatioPrim2_L = IncomeRatioPrim∧2 ∗ LMIG, and so on.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

Exhibit 11. Summary Statistics of the SFLL Random Sample (Originated in 2004–2007)

	Mean	Std. Dev.	Min.	Max.
Default Rate (%)	0.731	8.52	0.000	100.000
LMIG	0.924	0.265	0.000	1.000
FICO	730.362	53.82	446.000	850.000
Income	36.378	20.47	7.844	170.140

Notes: There are 2,559,329 observations. The number of observations excludes the observations for which one of the listed variables has missing data. Default is defined as being delinquent for three months or longer, following the convention in the literature. Income is imputed from the loan amount and the debt-to-income ratio. Unit: thousands of U.S. dollars.

Evidence on Mortgage Default

Data and Summary Statistics

In this section, I examine Hypothesis 2, which states that the subsidy increases the default probability of any strictly conforming loan. The data set I use is the random sample of the SFLL data released by Freddie Mac. This loan-level data set contains two parts: (1) loan information as of the date of the origination, such as the product type of the mortgage (fixed rate or adjustable rate), loan amount, debt-to-income ratio, and FICO score; and (2) monthly loan performance information for each loan recorded in the origination part, in particular, the monthly delinquency status and the survival time of the loan (number of months in which the loan remains ''active,'' i.e., non-defaulted) since the origination date.

I follow the same data-cleaning procedure as the data set used to produce Exhibit 10. For example, I also study loans originated between 2004 and 2007; and I focus on home-purchase and 30-year fixed-rate mortgages. After the data cleaning, the sample contains 2,559,329 observations.

The summary statistics of the key variables are presented in Exhibit 11. The exhibit shows that the average default rate in the sample is low (0.73%). This is not surprising, given that the loans are of high quality with an average FICO score of 730.

Results on Mortgage Default

While testing Hypothesis 2, I apply various duration or time-to-default models (including exponential, Weibull, lognormal, and Cox) to the SFLL data set. I use the following model to test Hypothesis 2 (in the accelerated failure-time form):

$$S_i = \beta_0 + \beta_1 Subsidy_i + \beta_2 BorrowerChar_i + \varepsilon_i, \tag{2}$$

where S_i is the survival time of loan i (i.e., time to default, in number of months). $Subsidy_i$ is the ''subsidy propensity'' of loan i, which is proxied by the $LMIG_i$ dummy variable described above, that is, if $LMIG_i = 1$, it indicates that loan i is ex ante more likely to receive the subsidy and thus has a higher subsidy propensity. $BorrowerChar_i$ is the vector of borrower characteristics of loan i, such as FICO score and income. Hypothesis 2 is equivalent to $\beta_1 < 0$ (for Cox models, the definition of the coefficient is different and the expected sign is opposite, as explained below).

Two points are worth noting regarding the empirical framework. Firstly, I do not use a period-by-period probit or logistic model with the dependent variable being a dummy variable indicating whether loan i defaults in month t. The reason is that, as Bajari, Chu, and Park (2011) point out, I essentially observe only one outcome for each loan, which is the time to default. The period-by-period probit or logistic model treats the status of the loan in each month as a separate observation, which artificially deflates the standard errors. By contrast, the time-to-default framework I use circumvents this problem by treating each loan as one observation. Secondly, I do not include loan characteristics (such as interest rate and loan size) as additional regressors. This is a more appropriate specification than the model with loan characteristics. To see this, suppose we do include loan characteristics and estimate the following equation (where $LoanChar_i$ is the vector of loan characteristics o loan i):

$$S_i = \alpha_0 + \alpha_1 Subsidy_i + \alpha_2 LoanChar_i + \alpha_3 BorrowerChar_i + \eta_i. \tag{3}$$

The problem with equation (3) is that in reality, loan characteristics are also endogenously chosen by the borrower, and thus we have to include the following equation in the estimation:

$$LoanChar_i = \gamma_0 + \gamma_1 Subsidy_i + \gamma_2 BorrowerChar_i + u_i. \tag{4}$$

Plugging equation (4) into equation (3), we get the following after some rearrangements:

$$S_i = (\alpha_0 + \gamma_0\alpha_2) + (\alpha_1 + \gamma_1\alpha_2)Subsidy_i + (\alpha_3 + \gamma_2\alpha_2 BorrowerChar_i + (\eta_i + u_i\alpha_2). \tag{5}$$

Equation (5) is equivalent to equation (2). In other words, equation (2) is the reduced form of the joint system consisting of equations (3) and (4), and thus captures the net effect of the subsidy on the time to default.

The results for the various duration models are presented in Exhibit 12. In the regressions for columns (2), (4), (6), and (8), I control for MSA fixed effects and use robust standard errors, so the interpretation is mainly based on the results in these columns. Across these columns, as expected, a mortgage loan with a higher FICO score will have a longer time to default, i.e., a lower default risk, ceteris paribus (the signs of all the coefficients in columns (7) and (8) need to be interpreted differently; see the note under Exhibit 12). Importantly, all the results in the exhibit indicate that being eligible for the LMIG (and having a higher priority for the subsidized default insurance) has a statistically negative effect on the mortgage's time to default (for the Cox model in columns (7) and (8), this corresponds to a statistically positive effect on the mortgage's hazard rate, i.e., default rate). In addition, the reported Akaike information criteria suggest that the most reliable model is the Weibull model with MSA fixed effects and robust standard errors (column (4)). These results suggest that the subsidy raises a conforming loan's default risk and makes the entire housing finance system more fragile, which confirm Hypothesis 2.

Exhibit 12. Effects of Subsidy on Mortgage Default

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Exponential	Exponential MSA & Robust	Weibull	Weibull MSA & Robust	Lognormal	Lognormal MSA & Robust	Cox	Cox MSA & Robust
LMIG	−0.2526***	−0.1546***	−0.0587***	−0.0365***	−0.0492***	−0.0273***	0.3326***	0.2055***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
FICO	0.0123***	0.0125***	0.0021***	0.0022***	0.0024***	0.0026***	−0.0118***	−0.0124***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Income	−0.0039***	0.0020***	−0.0012***	−0.0001	−0.0009***	0.0003***	0.0066***	0.0003
	(0.000)	(0.000)	(0.000)	(0.509)	(0.000)	(0.006)	(0.000)	(0.519)
Constant	0.6237***	0.3303**	3.7633***	3.6541***	3.7759***	3.6439***	—	—
	(0.000)	(0.025)	(0.000)	(0.000)	(0.000)	(0.000)		
			ln(p)	ln(p)	ln(sigma)	ln(sigma)		
Parameter	—	—	1.7344***	1.7288***	−0.7343***	−0.7511***	—	—
			(0.000)	(0.000)	(0.000)	(0.000)		
Chi^2	9,186	19,230	8,418	34,348	9,053	29,335	8,420	18,105
AIC	196,863	190,322	144,030	137,863	144,696	138,654	466,302	460,109

Notes: There are 2,559,329 observations. Unlike all other models in this table, a *positive* coefficient for the LMIG variable in the Cox model is consistent with Hypothesis 2. The reason is that the coefficients in the Cox model represent those in the hazard function rather than in the survival-time function (since the Cox model does not have an accelerated failure-time form); hence, a positive coefficient for LMIG in the Cox model means LMIG increases the default probability and decreases the survival time of the loan. Similar interpretations apply to the coefficients for other variables in the Cox model. *P*-values are in parentheses. LMIG = 1 if the borrower satisfies the criterion of the "Low-and-Moderate-Income Goal," and 0 otherwise. The p parameter is the shape parameter in the function $h_0(t) \equiv pt^{p-1}$, where $h_0(t)$ is the baseline hazard as in $h(t_j) = h_0(t)g(x_j)$ with t_j being the survival time and x_j being the covariates. "- - -" means "not applicable." The sigma parameter is the parameter in the survivor function $S(t_j) = 1 - \Phi[(log(t_j) - \mu_j)/\sigma]$, where $\Phi(.)$ is the standard Normal cumulative distribution function AIC = Akaike information criterion. In the regressions for columns (2), (4), (6), and (8), we control for MSA fixed effects and use robust standard errors.

* $p < 0.1$.

** $p < 0.05$.

*** $p < 0.01$.

Note that the estimated effects of income in columns (4) and (8) are insignificant, which may seem less intuitive. However, this can be explained by the multicollinearity between the FICO score and income. That is, most of the effect of the income variable is likely to have been captured by the FICO score, which makes the income variable itself insignificant.

Conclusion

In this paper, I empirically evaluate the effectiveness of the government subsidy done through the mortgage default insurance by the GSEs. I show that the supporting evidence for the conventional wisdom is subject to an endogeneity problem: the subsidy policy is implemented in terms of the loan size, which is endogenously chosen by the borrower and thus is affected by the subsidy policy. To circumvent this problem, I use the variation in interest rates generated by a mandate issued by U.S. Congress, the ''special affordable goal'' targeted at relatively poor borrowers. Since this mandate is implemented in terms of borrowers' incomes, which are unlikely to be manipulated by borrowers of GSE-insured loans, my empirical strategy provides a relatively clean way to identify the causal effect of the subsidy on interest rates. Using this strategy, I find that among the conforming loans, those with a discontinuously higher ex ante probability to receive the subsidy also have discontinuously higher interest rates, which implies that the subsidy has raised the mortgage interest rates of conforming loans.

I also study the effect of the subsidy on mortgage default. Applying various time-to-default models to another loan-level data set, I find that the subsidy raises mortgage default rates, which implies that the subsidy undermines financial stability. These results are robust to various model specifications and estimation approaches.

There are two avenues for future research. On the empirical front, future work can apply the empirical strategies I use to more detailed data so as to mitigate the measurement error problem. On the theoretical front, future work can build models to explain the non-conventional results I find, as well as to study the welfare implications of the subsidy. Zhao (2018) offers one example of such theoretical work.

Endnotes

[1] Note that with an underpriced default insurance scheme, the bank will optimally choose to over insure by taking excessive risk. There are two ways to do so. Firstly, the bank can take excessive risk through an extensive margin (i.e., by relaxing the lending criteria and extending mortgage loans to more borrowers). This is the margin studied by Keys, Mukherjee, Seru, and Vig (2010). Secondly, the bank can take excessive risk through an intensive margin, i.e., by raising the interest rate and/or leverage ratio of the same borrower (for the same borrower, a higher interest rate and/or a higher leverage ratio means a larger repayment burden and thus a higher default risk). It is the intensive margin that is the focus of this paper.

[2] One may argue that, ceteris paribus, lower-income borrowers (those below the 60% cutoff) will get higher interest rates than higher-income borrowers (those above) due to their

intrinsic risks, regardless of the probability of receiving the GSE subsidy. However, my empirical strategy compares borrowers who are so close to each other (around the 60% cutoff) that the difference in their intrinsic risks is very small. Such a small difference in the intrinsic risks would imply only a continuously (i.e., slightly) higher rate at the left of the 60% income cutoff, and cannot explain the discontinuously higher rate. The only underlying characteristic that has a discontinuous change across the 60% cutoff is the probability of receiving the GSE subsidy, so only this characteristic can be the cause for the discontinuously higher interest rate at the left of the 60% cutoff.

[3] After going through steps (1)–(6), there are about 17% of originated loans for which the spread information is available in 2004–2007. Hence, the empirical testing is based on a relatively small subset of loans, and thus the results support my model's prediction to the extent of the representativeness of the selected sample. However, given the role played by the high-risk loans in the recent financial crisis, it is important to study the pricing of the relatively risky portion of the mortgage market.

[4] Like the CAPM model (see Brealey, Myers, and Allen, 2013), my empirical model also addresses the spread (i.e., premium) between the return of an asset and the risk-free rate. The key difference is that the CAPM model focuses on the correlation between the spread and the expected risk premium on the market; whereas my empirical model attempts to examine the causality between the spread and the fundamental factors such as the GSE subsidy.

[5] The covariate that has an unexpected sign is the dummy variable Black: loans for Black borrowers are found to have a higher spread than those for all other races. However, I can still reconcile this unexpected sign as follows. On the one hand, I have included some variables that are not used by the bank when making the lending decision, such as race and gender. On the other hand, due to data restrictions, I have omitted some important variables such as the credit score. Since the credit score is likely to be correlated with race and gender, I have effectively used race and gender as a proxy for the omitted variable. Given that Black borrowers may have a lower credit score on average, the positive coefficient of the Black dummy may just reflect the effect of the omitted credit score. Indeed, as I move closer to the cutoff, the omitted variable problem becomes less severe (due to balanced distributions of the omitted variable on both sides of the cutoff), and thus the Black dummy becomes statistically insignificant, as columns (7)–(8) indicate.

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