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Essays on Mortgage Finance and Housing Markets

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Essays on Mortgage Finance and Housing Markets

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DISSERTATION

Presented to the Faculty of the Graduate School of

The University of Texas at Austin

in Partial Fulfillment

of the Requirements

for the Degree of

DOCTOR OF PHILOSOPHY

THE UNIVERSITY OF TEXAS AT AUSTIN

May 2015

Dedicated to my wife Bernardita, to my sons Sebastian and Benjamin, and to my parents Eduardo and Maria Veronica.

Acknowledgments

I am grateful to Andres Almazan, Aydogan Alti, Fernando Anjos, Jonathan Cohn, Greg Hallman, Sam Kruger, Tim Landvoigt, Zack Liu, Richard Lowery, Jordan Nickerson, Sheridan Titman, Parth Venkat, Adam Winegar, the seminar participants at the University of Texas at Austin, Carlos Carvalho (Committee Member), Cesare Fracassi (Committee Member), Jay Hartzell (Committee Member), Laura Starks (Committee Member) and especially John Griffin (Chair) for their useful comments and guidance.

Essays on Mortgage Finance and Housing Markets

Publication No. ____________

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The University of Texas at Austin, 2015

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I first study the effects of additional loan modifications on loan losses during the recent financial crisis. Despite loan modification being widely discussed as an alternative to foreclosure, little research has focused on quantifying its effect on loan performance. By exploiting plausible exogenous variation in the incentives to modify securitized non-agency loans, I find that an additional modification reduces loan losses by 34.5% relative to the average loss. Consistent with theory, modifications are especially beneficial when borrowers are less likely to return to a current status without help and when foreclosure losses are higher. Modification types that grant greater concessions to borrowers are the most effective for minimizing losses. Overall, additional modifications prevent borrower foreclosure while simultaneously benefiting investors.

I then study the relation between originators that misreported mortgages and house price movements. ZIP codes with high concentrations of

misreporting originators experienced a 75% larger relative increase in house prices from 2003 to 2006 and a 90% larger relative decrease from 2007 to 2012 compared to other ZIPs. Six causality related tests suggest that high fractions of bad originators in a ZIP result in larger price swings. In areas of elastic land supply, ZIPs with bad originators are associated with a building boom and a subsequent price bust that is much more severe than in similar ZIPs without bad originators. Originators with high misreporting seemed to have both given credit to borrowers of a higher stated risk and further understated the borrowers' true risk. Overall, the findings suggest that there are settings where questionable business practices can lead to large distortionary effects.

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Chapter 1

When are Modifications of Securitized Loans Beneficial to Investors?

1.1 Introduction

Financial economists have long studied debt renegotiation in the context of corporate default.[1] With the recent securitization crisis and the collapse of the housing market, part of this attention has shifted toward the renegotiation of mortgage loans (i.e., loan modifications). Several academics and policymakers advocated for reforms to incentivize modifications because they blamed the low loan modification rates for exacerbating the waves of foreclosures that occurred during the financial crisis (e.g., Posner and Zingales (2009), Mayer, Morrison, and Piskorski (2009a), Congressional Oversight Panel (2009)).[2] However, others note that due to the asymmetric information inherent in the mortgage market, loan modifications are not necessarily beneficial for the loan holder (Adelino, Gerardi, and Willen (2013a)). Of partic-

[1]For example, early work by Gilson, John, and Lang (1990) and Asquith, Gertner, and Scharfstein (1994) study the outcome of debt restructuring following payment default and Beneish and Press (1993) study the costs of debt renegotiation following covenant violations.

[2]The rationale is that foreclosures can be prevented by changing the terms (e.g., the principal, the interest rate, the amortization period) of a distressed loan and then reinstating it. This always benefits the borrower, while it can also benefit the loan holder if the prevented loan losses outweigh the modification costs.

ular concern are the contract frictions of the non-agency securitized market, in which most of the foreclosures occurred.[3] This paper examines how loan modifications in the non-agency securitized market affect loan losses.

Evaluating the impact of loan modification on loan losses is challenging for multiple reasons. First, the impact of modification on losses is not constant across loans: Some loans may benefit from modifications while others may not. Thus, the average effect of modification on loan losses as captured by the standard regression framework is not very informative. What is of interest is the effect of modification on the *marginal* loan, which is the effect an additional modification would have on the next loan that would be selected for modification. Second, the decision to modify a loan is endogenous. Loan modifications are not randomly assigned and are likely to be determined by dimensions beyond what is accounted for in the data. For example, the modification decision may be correlated with an unobserved measure of loan quality that also affects loan losses, potentially causing a bias.

I address the challenges in measuring the effect of loan modification on loan losses using a quasi-natural experiment. I exploit a shock to modification incentives that affected a subset of the non-agency loans in my sample. In August 2008, Fannie Mae and Freddie Mac began paying servicers, who manage

[3]Foreclosure rates of privately securitized loans were quite high compared to other types of loans. By 2007, privately securitized mortgages made up 55% of foreclosure initiations (Mortgage Bankers Association (June 2007)) despite being roughly 20% of all mortgages (Goodman et al. (2008)).

loss mitigation decisions, an *incentive fee* for each successful modification.[4] Although I am interested in studying loans from the non-agency market and not from the government-sponsored enterprise (GSE) market, I take advantage of the fact that some servicers operate in both markets ("both-market servicers") while other servicers only operate in the non-agency market ("non-agency-only servicers"). The *incentive fee* made modifications in the GSE market more attractive to both-market servicers relative to modifications in the non-agency market (which offered no incentive fee). This provides a plausible source of exogenous variation in non-agency modifications of the servicers who operate in both markets that can be used to identify the effect of modification on the losses of the marginal loan.

Advocates for additional modifications argue that securitization distorts the modification incentives of servicers. First, servicers do not necessarily benefit more from modifications than from other actions available to them (e.g., foreclosures). Second, pooling and servicing agreements (PSAs), which set rules that govern privately securitized loans, do not provide precise guidelines to servicers, and may even limit modifications. Third, the seniority ordering of the various tranches inherent to securitized products can also affect modifications, because different investors could benefit from different servicing

[4] The incentive fee approximately covered servicers' modification expenses. In December 2008, these incentive fees were further formalized by the "Streamline Modification Program," a joint effort of Fannie Mae, Freddie Mac, the Federal Housing Finance Agency (FHFA), and the U.S. Department of the Treasury. The incentive fee was replaced by the Home Affordable Modification Program (HAMP) in March 2009. My sample starts in August 2007 and ends in February 2009.

decisions. However, while a successful modification avoids foreclosure and the subsequent destruction of property value,[5] it is not obvious that additional modifications are in the best interests of investors. First, if a loan becomes delinquent again shortly after modification, investors could suffer larger losses than if they had not modified the loan at all. In particular, if house prices are declining, losses associated with redefaulted loans could be much larger than the losses associated with foreclosure without modification (thus, servicers face "redefault risk"). Second, it is also possible that a delinquent borrower can return to a current status without help, in which case any concession to the borrower would be unnecessary (thus, servicers face "self-cure risk"). Third, modification might also encourage opportunistic behavior from other borrowers who may default with the intention of extracting benefits from servicers (thus, servicers face "moral hazard").[6] Ultimately, whether the effect of modification on loan losses is economically important is an empirical question, as is the question of when additional modifications are beneficial.

A difference-in-difference estimation in a large set of non-agency loans that became seriously delinquent or were modified ("distressed") shows that both-market servicers responded to the incentive fee by modifying their non-agency loans 5.7% less than non-agency-only servicers. This is equal to 50.9% of the average modification rate of 11.2%, showing that the incentive fee is a

[5]For example, foreclosed properties tend to lose value due to poor maintenance (Madar, Been, and Armstrong (2009), Campbell, Giglio, and Pathak (2011)).

[6]For a rigourous treatment of redefault and self-cure risks see Adelino, Gerardi, and Willen (2013a).

relevant instrumental variable (IV). This result is consistent with i) modifications being costly and labor intensive, ii) servicers being capacity constrained, and iii) servicers being unable or unwilling to increase capacity. In addition, for the incentive fee to be a valid instrument, it also must satisfy the exclusion restriction condition. I argue that because the introduction of the incentive fee in the GSE market is a direct incentive to modify, it should have had no effect on loan losses in the non-agency market (i.e., a separate market), except through the modifications of both-market servicers.

Using the incentive fee in an IV regression framework, I find that modification prevents losses by 13.9%, which is a sizable 34.5% relative to the average loan loss of 40.3%. This suggests that modifications of non-agency loans have an important economic effect on the margin. This result is robust to different subsamples and verified using a matching estimation procedure based on ZIP code, month of distress, and propensity score. The benefits of modification are especially important in areas with relatively larger housing price decreases and relatively larger unemployment increases, where borrowers are less likely to return to current without help (low self-cure risk), and where foreclosure losses are higher. Even in the ZIP codes with low self-cure rates and large losses associated with foreclosures, more than 40% of the modified loans do not redefault within three years, suggesting that modifications prevent future loan losses by helping avoid foreclosure.[7] Also, modification

[7]My empirical design takes into account redefault and self-cure risks, but abstracts from the potential moral hazard problem mentioned above, which can add additional costs to

types that grant greater concessions to borrowers are the most effective for minimizing losses.

One potential concern is that the previous results are driven by observations from areas where house prices recovered quickly after the real estate bust. A modification may appear helpful because a loss on the house was avoided, but the loss avoidance may actually have been caused by a house price rebound (through a gain of equity) and not the modification. I study house price rebounds in the ZIP codes with the largest housing price declines (where modifications are especially beneficial) and find the strongest effect of modification on loan loss prevention in the areas with no rebound. This confirms that the marginal benefits of modifications are not mechanically driven by observations from areas where house prices recovered quickly, but instead derive from modifications that prevent future foreclosures.

These results raise questions about implications for the economy. Ignoring general equilibrium considerations, a conservative back-of-the-envelope calculation indicates that during the past crisis, an increase of 10% in modifications[8] could have helped more than 66,000 distressed borrowers avoid delinquency and keep their homes while benefiting investors at the same time. Furthermore, these benefits could have been even more important in light of the negative spillover effects of foreclosures on house prices in a given neigh-

modification (as shown by Mayer, Morrison, Piskorski, and Gupta (2014)). I discuss this in Section 2.6.

[8]This increase is slightly above the difference in modification rates between non-agency-only servicers and both-market servicers after the incentive fee.

borhood, and the fact that this estimation only considers the non-agency spectrum.

Prior research convincingly argues that loan modifications could be a tool for mitigating damage from the recent foreclosure crisis (e.g., Posner and Zingales (2009), Mayer, Morrison, and Piskorski (2009a)). While this research is primarily theoretical, we currently lack direct empirical evidence quantifying the benefits of additional modifications. Piskorski, Seru, and Vig (2010), Agarwal et al. (2011), and Kruger (2014) show that securitization impedes loan modifications, a result that could be interpreted as evidence that servicers of securitized loans may modify too infrequently. On the other hand, Adelino, Gerardi, and Willen (2013a) do not find economically important differences in mortgage renegotiation between securitized loans and loans kept by the lenders (portfolio loans). My results draw no conclusions regarding differences in modification rates between securitized loans and portfolio loans. Rather, I use the introduction of the incentive fee to show that the effect of loan modification on losses is economically important, which is also consistent with servicers modifying too infrequently.

This paper also relates to the literature concerning the effect of the servicer on loan performance. Haughwout, Okah, and Tracy (2009) and Quercia and Ding (2009) show that loan redefault rates depend on the type of modification chosen by the servicer. Demiroglu and James (2012) show that originator-servicer affiliation affects residential mortgage-backed security (RMBS) performance. Gan and Mayer (2007) study commercial mortgage-backed securities

(CMBS) and show that when holding the junior tranche, the servicer delays liquidation and the security has higher delinquency rates. Like this prior research, my results show that servicing has an important impact on securitized loan performance.

Finally, this paper also relates to studies that evaluate recent policy interventions in the mortgage market. Agarwal et al. (2013) show that although HAMP increased loan modifications, its effect was weaker than expected largely due to differences in servicer response. Although my main focus is on quantifying the effects of additional modifications on loan losses and not on evaluating policy, my results also have policy implications by suggesting that the intervention of the GSE market may have negatively affected the non-agency market: Therefore, my results show that policymakers should be cautious of the unintended consequences that may result from other interconnected markets.

1.2 Background and empirical framework

1.2.1 The servicing industry

The servicer is the entity responsible for the collection of interest and principal payments of mortgage loans. If the loan is securitized, the servicer remits the payments to a trust that holds the mortgages. The trust later distributes the money to investors. Broadly speaking, if a borrower becomes delinquent, the servicer can i) wait to see if the loan self-cures without taking any action, ii) foreclose the loan, or iii) work with the borrower to help him or her become current (e.g., modify). In a loan modification, the servicer can choose to restructure one or more features of the loan (e.g., the principal, the interest rate, the amortization period), waive penalties and fees, or capitalize the interest and fees. Servicer actions differ in costs. Payment processing is the cheapest because it can be highly automatized and is subject to economies of scale. However, servicer costs increase during economic downturns, when delinquencies tend to increase significantly. The foreclosure process can still be automatized to some degree, though it is more costly than managing payments.[9] In contrast, loan modifications are more discretionary and require more labor, which makes them relatively more costly.[10]

Servicers deal with different types of mortgage loans, such as portfolio loans (retained by the lender) or securitized loans (sold by the lender). This study focuses on securitized loans. Securitized mortgages are classified into two main groups, depending on the issuer of the security. Loans included in RMBS and issued by investment banks are non-agency loans (or private-label loans). In contrast, agency (or GSE) loans are those in RMBS issued by government-sponsored enterprises such as Fannie Mae or Freddie Mac. The non-agency and the GSE mortgage markets differ not only in size but also in loan characteristics.[11] They also differ in the incentives that servicers face.

[9] An extreme example of this is the robo-signing scandal of 2010.

[10] Levitin and Twomey (2011) note that the modification costs for servicers range from $500 to $1,000.

[11] According to Goodman et al. (2008), the total size of the non-agency market by mid-2007 was $2.12 trillion, while the GSE market was $4.15 trillion. In terms of loan characteristics, non-agency loans in general are either larger than GSE loans, or have lower expected

Guidelines on servicer actions regarding the mortgages in the trust of a non-agency RMBS can be found in the Pooling and Servicing Agreement (PSA), a document generally prepared by the sponsor of the deal and filed with the Securities and Exchange Commission (SEC). The general guideline for the servicer is to manage the loan as if it were its own, which means it should maximize the net present value for the investor. Yet, PSAs are often vague with respect to the specific actions servicers should take if a loan becomes delinquent. On the other hand, the guidelines for servicers in the GSE market are set directly by the guarantors of the securities, Fannie Mae and Freddie Mac. Unlike PSAs, Fannie Mae's and Freddie Mac's guidelines are considerably more explicit. Additionally, Fannie Mae and Freddie Mac frequently update and clarify their guidelines for servicers.

Some servicers specialize in servicing only non-agency loans (non-agency-only servicers), while other servicers focus on GSE loans or a mixture of both loan types (both-market servicers). Table 1.1 documents this fact among 22 of the 23 servicers in my sample of non-agency loans as of year-end 2007.[12] Of the servicers listed in the table, 36% focus mainly on subprime (mostly non-agency securitized) loans, while the remaining 64% operate in both the GSE and the non-agency markets. My main empirical strategy exploits this heterogeneity.

quality than GSE loans, which conform to Fannie Mae's and Freddie Mac's guidelines.

[12] The missing servicer, MetLife Home Loans, was founded in 2008.

1.2.2 A change in modification incentives

In August 2008, Fannie Mae and Freddie Mac began paying servicers an incentive fee of $700 and $800, respectively, for each successful modification, in an attempt to provide incentives for servicers to pursue alternatives to foreclosure.[13] This incentive fee made modifications in the GSE market relatively more attractive to both-market servicers than modifications in the non-agency market, which offered no incentive fee at the time. To the extent that both-market servicers lacked the capacity to handle the increasing demand for modifications, the incentive fee provides a plausible source of exogenous variation in modification rates of both-market servicers, which can be used to identify the effect of modifications on loan losses. In this paper, I use the introduction of this incentive fee as an instrument for modification to estimate the causal effect of modification on the losses of the marginal loan (i.e., the next loan that would be selected for modification). I discuss the validity of the introduction of the incentive fee as an instrumental variable in detail in Section 1.4.

1.2.3 Main identification strategy

The main objective of this paper is to evaluate the impact of loan modification on loan losses. Consequently, the baseline regression of interest

[13] Later, in December 2008, these incentive fees were further formalized by the "Streamline Modification Program," a joint effort of Fannie Mae, Freddie Mac, the Federal Housing Finance Agency (FHFA), and the U.S. Department of the Treasury. An $800 incentive fee was offered to servicers for each successful modification. The fee was paid upon the completion of the modification, after a trial period.

is of the form

$$Y_i = \alpha_1 + \beta_1 Mod_i + X_i' \Gamma_1 + \epsilon_{1i} \tag{1.1}$$

where Y_i represents loan i's losses, Mod_i is an indicator for modification, and X_i is a vector of loan-level characteristics and fixed effects. More specifically, in this paper, net losses are defined as losses minus recoveries, divided by the outstanding principal amount when the loan became distressed (60+ days delinquent or modified). Losses of modified loans incorporate any concessions made to the borrower. I divide by the principal outstanding to capture the loss for the RMBS investor. The indicator Mod_i takes the value of one (1) if the loan was modified within six months from becoming distressed, and zero (0) if it was not (e.g., no action was taken, the loan was foreclosed, or the loan was modified *after* six months). The cases in which the servicer does not take any action are included to account for self-cures. In terms of fixed effects, I include Core Based Statistical Area (CBSA)-month of origination fixed effects in an attempt to control for unobservable quality and local economic conditions at the time of origination. The month of loan distress and servicer fixed effects are also included to control for aggregate economic conditions and time-invariant unobservable characteristics of servicers. Additionally, estimates are calculated from a sample of distressed loans in an attempt to further mitigate possible unobservable differences across loans.

However, estimating this baseline regression is unlikely to be very informative. First, the ordinary least-squares (OLS) estimate of β_1 essentially captures the average difference in loan losses between modified and non-modified loans. If the effect of loan modification on loan losses is not constant across loans, which is likely to be the case, then the OLS estimate of β_1 cannot be interpreted as the effect of modification on the losses of the marginal loan. Second, the decision to modify a loan is endogenous. Loan modifications are not randomly assigned and are likely to be decided based on dimensions beyond what is accounted for in the data. If the decision to modify a loan is correlated with unobserved characteristics (captured in the residual) that explain loan losses (e.g., an unobserved measure of loan quality), then the estimate of the coefficient β_1 is likely to be biased. Furthermore, it is not possible to forecast the direction of the bias, since it will depend on the correlation between the omitted variable and the rest of the explanatory variables in the regression, which are not necessarily obvious.[14]

To overcome the two concerns described above, I follow a two-stage least squares/IV approach (2SLS/IV). More specifically, I use the introduction of the incentive fee in the GSE market as an instrument for modifications. The first-stage regression is

$$Mod_i = \alpha_2 + \beta_2 BothMarkets \times AfterFee_i + X_i' \Gamma_2 + \epsilon_{2i} \tag{1.2}$$

[14] An example of this may be the correlation between a loan-level control (such as the property occupancy status) and borrower quality. Though investors may have an incentive to default strategically if prices drop dramatically and they lose the equity on the house, investors also tend to have higher income, so they may be better payers than property occupants.

where the instrumental variable is $BothMarkets \times AfterFee_i$, the interaction of $BothMarkets_i$ (a dummy variable that takes the value of one (1) if the servicer managing the loan is a both-market servicer, and zero (0) otherwise) and $AfterFee_i$ (a dummy variable that takes the value of one (1) if the loan became distressed after the incentive fee in the GSE market was introduced, and zero (0) otherwise).[15,16] The second-stage regression is

$$Y_i = \alpha_3 + \beta_3 \widehat{Mod}_i + X_i' \Gamma_3 + \epsilon_{3i} \tag{1.3}$$

where $\widehat{Mod}_i$ are the fitted values from Equation 1.2. The coefficient β_3 is consistent, provided that $BothMarkets \times AfterFee_i$ is a valid instrument. Moreover, because only a subset of loans is affected by the instrumental variable, the IV estimate of β_3 captures the Local Average Treatment Effect (LATE) of loan modification on loan losses. As I show in Section 1.4, the introduction of the incentive fee prevented some modifications of loans serviced by both-market servicers. Consequently, under the additional requirement that the instrumental variable affects the affected loans in the same way, the estimate of β_3 captures the effect that an additional modification would have on the marginal loan, which is the effect on the next loan that would be selected for modification.[17] Under the null hypothesis that an additional modification

[15] Although the dependent variable Mod_i is a binary variable, the first-stage regression fits a linear probability model since using a probit or a logit model could result in inconsistent estimates (Angrist and Pischke (2009)).

[16] Note that because X_i' includes servicer and month of distress fixed effects, the variables $BothMarkets_i$ and $AfterFee_i$ are not necessary in the regression.

[17] More formally, in the terms of the treatment effects literature, I estimate an IV with

does not affect the losses of the marginal loan, β_3 should not be statistically distinguishable from zero. Alternatively, if additional modifications are beneficial to RMBS investors, then β_3 should be statistically negative. Also, under the assumption that servicers first modify those loans that will benefit the most from modification, a negative β_3 would also be consistent with servicers modifying too infrequently.[18]

1.3 Data and sample

The primary source of data in this study comes from Lewtan's ABSNet Loan. This database provides detailed information on loans that back U.S. non-agency RMBS. Lewtan collects and cleans loan-level data reported in RMBS servicer and/or trustee tapes and covers more than 90% of the non-agency market. The database includes variables that describe each securitized loan at the time of origination, including the loan amount, credit score, combined loan-to-value ratio (CLTV), interest rate, level of documentation, existence of a prepayment penalty, and other descriptive variables. This database also contains the identity of the servicer, the monthly history of payments, foreclosure dates, loss information, and modification information (i.e., dates, type and amounts forgiven). Though sometimes reported by the servicer or

heterogeneous treatment effects. The LATE captures the effect of modification prevention on *compliers* (the loans that would have been modified had the incentive fee not existed).

[18] If servicers modify optimally (from the perspective of the RMBS investor), then the effect of an additional modification on loan losses should be zero (β_3 not distinguishable from zero).

the trustee, some of Lewtan's modification information is derived from changes to the mortgage contract. This helps ensure consistency across servicers and across time.[19]

I study loans in RMBS deals from vintages between 2000 and 2007 (when most RMBS issuances anteceding the financial crisis occurred) that became 60 days or more past due or were modified between August 2007 and February 2009.[20] This study does not focus on loans that became distressed after February 2009, since HAMP was announced in that month and implemented shortly after. Therefore, my window of analysis should only be affected by the incentive fee and should be free from the influence of HAMP.[21] The data also includes loss information up to September 2012. Ending the sample in February 2009 provides a window longer than three years to allow losses to materialize following servicer actions, so losses should not be affected by right censoring. Additionally, since my empirical strategy depends on identifying who makes the modification decision when a loan becomes distressed, I focus on loans with servicer information.[22] I also impose some additional restrictions

[19]In my sample, 66.8% of the modifications are self-reported, with the remaining 33.2% being implied. For a detailed discussion on these contract-change algorithms, see Adelino, Gerardi, and Willen (2013a).

[20]Specifically, I consider first-time delinquencies or modifications.

[21]Although my measure of modification considers loans modified within six months of the loan becoming distressed, the effect of HAMP on the measure should be negligible. It is a well-known fact that HAMP had a slow start. Indeed, the number of permanent modifications under HAMP in 2009 totaled 66,465, which is equivalent to 12.7% of the 521,630 permanent modifications under HAMP in 2010 (Inside Mortgage Finance (2012))

[22]During the years surrounding my analysis there was considerable merger activity in the servicing industry. A list with the relevant events to determine servicer identity at the time in which the loan becomes distressed can be found in Appendix A.

on the underlying loans. I consider first-lien loans that originated between 2000 and 2007. I omit Federal Housing Administration (FHA) and Veterans Affairs (VA) loans, which include guarantees from the government that may affect servicer behavior. I also omit negative amortization loans, loans smaller than $30,000, loans with loan-to-value (LTV) over 103%, and loans of multi-unit properties. Finally, I require the variables used as controls to be non-missing, and I focus on loans serviced by the 23 most frequent servicers in my sample. The final sample includes slightly less than one million loans.[23]

1.3.1 Sample description

Table 1.2 describes the loan sample by servicer type (both-market servicers and non-agency-only servicers) for different sub periods (full, pre-incentive fee and post-incentive fee). Several facts can be observed. First, the number of loans in distress serviced by both-market servicers is 2.9 (744,334/254,733) times larger than the number of distressed loans serviced by non-agency-only servicers, reflecting the fact that both-market servicers tend to have a larger market share in the non-agency market. Second, the characteristics of the loans across servicer types differ. Loans serviced by non-agency-only servicers appear to be of lower quality, on average (e.g., lower credit score, higher interest rates, a larger fraction of adjustable-rate loans), confirming the

[23]A total of 1.63 million loans became distressed between August 2007 and February 2009. After applying the filters described above, 1.02 million loans remain. Finally, I also drop the loans from small non-agency-only servicers (those with less than 5,000 loans). The final sample has 999,067 distressed loans.

importance of controlling for loan characteristics in the empirical analyses. Third, during the pre-incentive fee period, the difference in modification rates between non-agency-only servicers and both-market servicers is 1.4% (9.6%-8.2%). This difference increases to 9.5% during the post-incentive period. The relative increase in modifications by non-agency-only servicers is also accompanied by a relative increase in more aggressive modifications (larger proportions of multiple attribute modifications and principal reductions). Fourth, losses following unsuccessful modifications during the pre-incentive fee period are 3.1% (40.2%-37.1%) larger for both-market servicers, suggesting that non-agency-only servicers may have had slightly better expertise at modifying. This difference drops to 1.2% in the post-incentive fee period.

1.4 The incentive fee as an instrument

This section explores the effects of the incentive fee on the non-agency mortgage market. I argue that by introducing the incentive fee in the GSE market, Fannie Mae and Freddie Mac made modifications in the GSE market relatively more attractive than modifications in the non-agency market to both-market servicers, since the non-agency market had no incentive fee at the time. These servicers were likely to have capacity constraints at the time, since servicers lack incentives to have excess modification capacity and delinquencies were increasing abnormally. They responded by conducting fewer modifications in the non-agency market relative to their modifications in the GSE market, when compared to the pre-incentive fee period. Therefore, the incentive fee generated variation in modification rates of both-market servicers that is arguably independent from any potentially unobservable characteristics of the loans serviced by them. Moreover, the introduction of the incentive fee in the GSE market was a direct incentive to modify, so it is unlikely to have had any effect on loan performance in a separate and different market such as the non-agency market, except through the modifications of both-market servicers.

Although the available data do not allow for observation of the disaggregated modification behavior of both-market servicers in the GSE market, the non-agency-only servicers are a suitable control group in the non-agency market who were unaffected by the incentive fee. Therefore, it is possible to test the effect of the introduction of the incentive fee on the modification rates in the non-agency market of both-market servicers and non-agency-only servicers through a difference-in-differences (DD) framework, which is essentially the first-stage regression of the IV estimation.

I start by analyzing modification rates graphically. Figure 1.1 shows the monthly modification rates of the two groups of servicers around the time the incentive fee was introduced. During the period anteceding the incentive fee (delimited by the vertical line), when aggregate delinquencies in the U.S. (represented by the black line) were relatively lower, both types of servicers show similarly lower modification rates (defined as the number of non-agency loans modified within six months as a fraction of all non-agency loans in distress). Even though modification rates began to increase rapidly after November 2007 (consistent with servicing becoming a focus of attention for regulators and the media due to increasing delinquencies), the modification rates of both types of servicers move together. However, both modification rates diverge after the incentive fee is introduced, with both-market servicers exhibiting a significantly lower modification rate than their counterparts.

One concern that arises from Figure 1.1 is that modification rates begin to diverge in April 2008, four months before the incentive fee was introduced. This is partially due to the fact that the modification rates capture modifications completed within six months of the loan becoming distressed. The gray shaded area in the figure delimits the months in which modification rates were affected by the incentive fee. Before this, there is no indication that the trends are not parallel.[24]

Table 1.3 shows the result discussed above more formally through a DD estimation. The dependent variable is the modification indicator. The coefficient of interest is the one associated with the variable $BothMarkets \times AfterFee$. Recall that this is the interaction of $BothMarkets$ (a dummy variable that takes the value of one (1) if the servicer managing the loan is a both-market servicer, and zero (0) otherwise) and $AfterFee$ (a dummy variable that takes the value of one (1) if the loan became distressed after the incentive fee in the GSE market was introduced, and zero (0) otherwise). The

[24]In Appendix B, I show several of the figures in this paper in five to six-year windows and confirm that modification rates of both-market servicers and non-agency-only servicers moved parallel for a long period anteceding the incentive fee.

set of controls includes loan-level information at the time of origination such as credit score, CLTV, and interest rate. These controls also include indicators of whether the loan has an adjustable or fixed rate, has low/no documentation or full documentation, and whether it has a prepayment penalty. Another control is whether the borrower self-reported the property as owner-occupied, or as an investment/second home. The regression also controls for the unpaid principal balance at the time of the loan becoming distressed and includes CBSA-month of origination, servicer, and month of distress fixed effects. In particular, servicer fixed effects are important to control for time-invariant unobservable characteristics of servicers. Finally, standard errors are clustered by the Combined Statistical Area (CSA) to account for correlation within economically-tied geographic areas.[25]

Column 1 of Table 1.3 shows that after the introduction of the incentive fee, the relative difference between modification rates of both-market servicers and non-agency-only servicers increased by 5.7% on average (with non-agency-only servicers modifying proportionally more). This effect is statistically significant at the 1% level, and is equivalent to an increase of 50.9% relative to the mean modification rate of 11.2%. Column 2 shows the results of estimating the same regression as in Column 1 in a subsample of loans excluding loans that became distressed during the period from March 2008 to July

[25] CSAs are larger geographic areas than CBSAs. There are 124 CSAs and 939 CBSAs in the sample. While it is also possible that the regression residuals are correlated within servicers, clustering by servicer may result in biased standard errors due to the fact that there are only 23 servicers in the sample (Angrist and Pischke (2009)).

2008, which is demarcated by the gray area in Figure 1.1, when the incentive fee begins to affect modification rates. The effect of the incentive fee is even stronger, yielding a statistically significant coefficient of 6.4%. Appendix B further validates the previous estimates through several robustness tests and falsification tests. The coefficient associated with $BothMarkets \times AfterFee$ is economically and statistically significant when excluding Bank of America (the largest servicer) or California (the largest state) from the sample.[26] Additionally, the estimate drops to 1.0% under the false assumption that the incentive fee was introduced in January of 2008 (eight months earlier than the true date). Finally, Appendix B also shows that the economic effect of the incentive fee on the control variables used in the regression in Table 1.3 is minor.

One necessary requirement for the validity of my identification strategy is that servicers lacked the capacity to handle the increased demand for modifications. If both-market servicers had idle resources, it is possible that the differences in modification rates between both-market servicers and non-agency-only servicers is not due to the incentive fee causing a distortion of modification decisions of both-market servicers. This sample does not allow for a direct measure of capacity of the servicers, but the fact that most servicers were capacity-constrained and unable to handle the unexpectedly increased number of delinquencies has been widely discussed as a major concern

[26] Though in Table 1.1, Bank of America appears in sixth place in terms of market share, it became the largest servicer after acquiring Countrywide in July 2008.

during the real estate crisis (Cordell et al. (2009), Congressional Oversight Panel (2009), Wilse-Samson (2010)). In a speech in December 2008, in which he discussed the challenges in the real estate market, Chairman of the Federal Reserve Ben Bernanke explicitly stated:[27]

> "*... More generally, the sheer volume of delinquent loans has overwhelmed the capacity of many servicers, including portfolio lenders, to undertake effective modifications.*"

The capacity constraint of both-market servicers is not so easily resolved by hiring new employees. First, hiring a loan modification officer is not an expedited process. Labor markets have frictions, and modification officers must be trained and certified. Second, many of the servicers struggled financially during the crisis, which increased the difficulty of expanding capacity. Third, even if servicers are in good financial condition and if labor frictions are not present, it is not clear that servicers benefit from conducting non-agency modifications; therefore, servicers would not seek to hire more staff. Several studies argue that servicers are not incentivized to modify non-agency securitized loans, and they can profit more from foreclosures (e.g., Eggert (2007), Thompson (2011)). Given all these considerations, it is reasonable to believe that both-market servicers were capacity-constrained when the incentive fee was introduced, and that they most likely remained constrained–at least for the 8-month period I analyze following the incentive fee.

[27] Bernanke, Ben S. (December 4, 2008). Speech at the Federal Reserve System Conference on Housing and Mortgage Markets, Washington, D.C.

Column 3 of Table 1.3 provides additional evidence consisting in both-market servicers being capacity-constrained. Both-market servicers are divided into two groups based on the increase in delinquencies they experienced from the pre-incentive fee period to the post-incentive fee period. The variable $HighDelinquency$ is a dummy that takes the value of one (1) if the servicer belongs to the group with the larger increase in delinquencies, and zero (0) otherwise. The coefficient of -2.38% on the explanatory variable of interest ($HighDelinquency \times AfterFee$) indicates that, after the incentive fee, the both-market servicers that experienced the largest increase in non-agency delinquencies conducted fewer modifications (proportionally) than both-market servicers, who experienced the lowest increase in delinquencies compared to the pre-incentive fee difference.

The previous evidence suggests that the incentive fee prevented non-agency modifications of both-market servicers; however, it is still possible that both-market servicers modify less in the post-incentive fee period because they face different risks. For example, if the distressed borrowers from both-market servicers are more likely to self-cure, then the pattern shown in Figure 1.1 can still be optimal. To explore this possibility, I plot the self-cure rates of both-market servicers and non-agency-only servicers in Figure 1.2. Though self-cure rates of both-market servicers are consistently higher, which is consistent with these servicers modifying less, the incentive fee has no apparent effect on the relative difference in self-cure rates among the two servicer types.

Finally, if modification affects loan performance, the difference in losses of non-agency loans between the two types of servicers should also respond to the introduction of the incentive fee. More specifically, if modifications are beneficial, given that the introduction of the incentive fee seemed to have impeded modifications of both-market servicers in the non-agency market, both-market servicers should experience a relative increase in loan losses compared to non-agency-only servicers after the incentive fee. Table 1.4 shows that this is the case.[28] Servicers that operate in both-markets experience a relative increase of 0.93% in loan losses after the introduction of the incentive fee when compared to non-agency-only servicers. This result is confirmed in Column 2 in a subsample of loans that excludes loans that became distressed during the period from March 2008 to July 2008 (indicated by the gray area in Figure 1.1). Additionally, the Figure 1.3 shows the previous result graphically. Non-agency-only servicers show greater losses than both-market servicers in the subset of loans that became distressed before the incentive fee, but the difference virtually disappears afterwards.[29]

[28]Note that the specification in Table 1.4 is essentially the reduced form regression deriving from the IV regression that uses the incentive fee as an instrument. This, in turn, is the same specification as Table 1.3, with the only difference that the dependent variable is the loan loss.

[29]Appendix B shows the losses by servicer type in a five-year window around the incentive fee. Although some convergence among losses of both servicer types can be seen before my sample period, losses of both-market servicers and non-agency-only servicers tend to move parallel before the incentive fee in my sample (as shown by Figure 1.3).

1.5 Are mortgage modifications beneficial?

This section examines whether loan modification has an impact on the losses of the non-agency loans in the sample. I start by showing the baseline linear regression. Then I use the introduction of the incentive fee in an IV framework as described in Section 1.2.

1.5.1 Baseline regression (OLS)

I start by estimating OLS regressions of loan losses on a modification indicator, loan-level controls, and fixed effects, as specified by Equation 1.1. As before, I cluster standard errors by CSA. Though this regression may be subject to endogeneity problems and it is not very informative, it provides a benchmark to contrast with the IV estimation. Table 1.5 shows the results for different subperiods. Column 1 shows that modified loans experience lower losses than non-modified loans, on average, from August 2007 to February 2009. Specifically, on average, a modification saves investors 6.1% (15.1% relative to the average loss of 40.3%) of the principal amount outstanding at the time the loan became distressed. Columns 2 and 3 show that, though the average effect of modification on loan losses is significant in both the pre-incentive fee period and the post-incentive fee period, it is 4.4 % stronger in the post-incentive fee period.

1.5.2 IV estimation

As shown in the previous section, the introduction of the incentive fee generated plausible exogenous variation in modification rates of both-market servicers. I exploit this variation through the IV framework described in Section 1.2, making a slight change to the main specification compared to the regressions estimated above. For ease of estimation, I include both CBSA and month of origination fixed effects separately instead of including CBSA-month of origination fixed effects (interacted).[30] Appendix B shows that the baseline OLS estimation yields virtually the same coefficients when this change in the set of fixed effects is introduced. The same is true for the DD estimation shown in the previous section, as shown by the first-stage regression below.

Table 1.6 shows the results of the IV estimation for the full sample of loans and for a subsample of loans that excludes loans that became distressed during the period from March 2008 to July 2008, when the incentive fee begins to affect modification rates. Columns 1 and 2 show the first-stage regressions. The variable $BothMarkets \times AfterFee$ strongly explains the modification indicator in both samples, showing that the instrument satisfies the relevance restriction.[31] Additionally, both F-statistics are significantly above the threshold of 10, indicating that the instrument is strong (Bound, Jaeger, and Baker

[30]Including CBSA-month of origination fixed effects is equivalent to including 34,667 dummy variables, which makes the IV estimation considerably more complex.

[31]Note that, as discussed in the previous paragraph, the coefficient estimate on $BothMarkets \times AfterFee$ in Column 1 of Table 1.6 (which is comparable to Column 1 of Table 1.3) is 5.76% which, compared to 5.72%, is a very similar value.

(1995), Staiger and Stock (1997)).

Columns 3 and 4 of Table 1.6 show the causal effect of modification on the losses of the marginal loan. The coefficient on the modification indicator in the full sample is -13.9% (statistically significant at the 1% level), meaning that, on average, modifying a non-modified loan that would have likely been modified without the incentive fee reduces losses by 13.9% (the LATE). This is equivalent to a 34.5% decrease relative to the mean loss of 40.3%, which is an economically significant amount. This result is confirmed in the subsample in Column 4, which shows a coefficient of -11.9%.[32] Also, both the bias and the lack of informativeness of the OLS estimate are confirmed by the fact that the coefficient of -13.9% on the modification indicator in Column 3 of Table 1.6 is more than twice as large (in absolute value) than the coefficient of -6.1% on the modification indicator in Column 1 of Table 1.5. This justifies the empirical strategy undertaken in this paper.

1.6 The channel and validation of the main results

The previous section shows that modification has an important causal effect on the losses of the marginal loan. However, the results do not speak to why or under which conditions modifications are most helpful. In this section I investigate the channel through which modifications affect outcomes and

[32] Additionally, in Appendix B, I show that the results in Table 1.6 are robust to the exclusion of loans serviced by Bank of America (the largest servicer) and loans that originated in California (the largest state).

provide additional insight on the effect of modification on loan losses.

1.6.1 The channel

As mentioned in the introduction, several factors can affect the modification decision and its success. When a loan is modified, investors can suffer greater losses if the borrower redefaults shortly after modification. Additionally, it is also possible that a delinquent borrower can self-cure (i.e., return to current without help), so any concession to the borrower would be unnecessary. Because house price movements can impact the previous factors, I analyze the effect of modification in loan samples based on the short-run house price movements in the ZIP codes of the loans.

I compute ZIP code-level house price returns using Zillow, an online real estate database.[33] Each loan is assigned a return based on the ZIP code of the loan's underlying property. Returns are computed from the month in which the loan became distressed to the bottom price of the index in 2009. Then, the loans in the sample are divided into three groups based on their house price returns. The first group corresponds to loans with returns equal or greater than zero (no house price drop). The second and third groups are based on the median return of the remaining loans. The second group contains loans that experienced a small house price drop, while the third group contains

[33] Zillow provides ZIP code-level house price indices based on median home values. A description of their methodology can be found on the Zillow website: `http://www.zillow.com/research/zhvi-methodology-6032`. Of the loans in the sample, 92.9% are from ZIP codes with Zillow coverage.

the loans that experienced a large house price drop.

Table 1.7 shows the result of the IV estimation for each group of loans.[34] The geographic distribution of the loan sample shows significant variation in house price returns. The average return in the group of loans in the large house price drop area is -34.6%, which is 3.7 times larger than the average return of -9.3% from the group of loans in the small house price drop group. Also, very few loans (only 21,616) are in the group that experienced no house price drop. Modifications are especially impactful in the set of loans that experienced the largest short-term drop in house prices following the month of distress, with a coefficient of -34.53% on the modification indicator; however, there is no statistically significant difference between the coefficient of -34.53% and the coefficient of -7.16% in the sample of loans with a small house price drop.[35] This result can be understood by an analysis of self-cure rates, redefault rates, and foreclosure costs. The set of loans with large house price drops shows an average self-cure rate of 13.4%, considerably lower than the self-cure rates of the other two groups, which are 22.3% for the small house price drop group and 27.5% for the no-drop group. In addition, the areas with the largest house price drops also show a much larger average loss in the case of foreclosure.

[34]The first-stage regressions (i.e., those that show the relevance restriction is met) are available in Appendix B. Additionally, I also provide the results of the IV estimation using an alternative set of house price returns computed from March 2009 to March 2010 (to ensure that returns do not affect the modification decision) and find the same results as in Table 1.7.

[35]The 95% confidence intervals of both of the coefficients on the modification indicator in Columns 2 and 3 of Table 1.7 overlap substantially, indicating that both estimates do not statistically differ from one another.

Foreclosed loans in areas with a large house price drop show average losses of 68.3%, much larger than the losses of 51.1% and 49.0% shown in the other two area groups. Redefault rates, on the other hand, do not appear to differ between groups to the same extent. The redefault rates of loans in the group with a large house price drop show a 3% difference from the loans in the group with a small house price drop.

Overall, Table 1.7 shows that additional modifications are helpful when self-cure risk is low and when foreclosure costs are high. The fact that redefault rates do not vary significantly across groups, and the fact that more than 40% of the modified loans do not redefault suggests that modifications help prevent loan losses by avoiding foreclosure and subsequent liquidation.

1.6.2 Additional validation of the results

1.6.2.1 Housing price rebounds

A possible concern with the results presented previously could be that they are driven by areas where house prices experienced a strong recovery after the house price bust. Consider, for example, a borrower who decides to default because of the negative equity in a house that obtains a loan modification, and assume that this modification does not really improve the situation of the borrower. If the house price continues to decrease or remains flat, the borrower may decide to redefault. If, on the contrary, the house price experiences a large increase, the borrower may continue to pay the mortgage because he or she would now have equity in the house. In this case, the modification may appear to be helpful because the loss on the house was avoided, but the rebound in housing prices actually caused the benefit, not the modification. However, it is also important to note that house price rebounds may also help delinquent mortgages to self-cure. Therefore, it is not always clear how the IV estimates are affected by geographic house price swings.

To address the concern above, I compute ZIP code-level house price rebounds. The rebound is the cumulative return of index from the time it reached its bottom level in 2009 to its level in September 2012, when no additional loan losses can be observed.[36] Within the group that exhibited a large house price drop, as shown in Table 1.7, I classify the loans into three groups based on their house price rebound and then repeat the IV estimation for each group. The first group consists of the loans in areas that did not experience a rebound in house prices (i.e., prices continued to drop). The second and third groups are based on the median rebound of the remaining loans: The second group contains the loans that experienced a small house price rebound while the third group contains the loans that experienced a large house price rebound.

Table 1.8 shows the results of the IV estimation. The coefficients on the modification indicator are large in all areas (although not statistically significant in the group of loans that experienced a small rebound). The effect is

[36]The bottom level is defined as the lower value of the index before January 2010. Consequently, ZIP codes that continue to experience a decline in house prices may have a "negative" rebound.

stronger (with a significant coefficient of -37.6% on the modification indicator) in the group of loans in which house prices continue decreasing, indicating that the results shown in the previous table are not explained mechanically by house price rebounds. Both self-cure rates and redefault rates do not vary considerably across house price rebound groups, though in the case of foreclosure, loan losses are around 6% greater in the areas where prices continued to decrease. When we consider the fact that 40.0% of the modified loans do not redefault, these results once again support the idea that modifications help prevent losses because they help avoid future redefaults and foreclosures.

1.6.2.2 Unemployment increases

Unemployment is another important factor that may affect the success of a modification. Areas that exhibit increases in unemployment should show lower self-cure rates and higher redefault rates. Therefore, it is unclear how changes in unemployment rates may impact the effect of modifications on loan losses. To investigate this, I divide the loan sample based on the change in unemployment rates in the Metropolitan Statistical Area (MSA) where the underlying property of the loan is located. I divide the sample into two groups based on the median increase in unemployment from the month the loan became distressed to the highest value of the index in 2009.[37] Table 1.9 shows the results of repeating the IV estimation in the two groups described

[37]Unemployment data comes from the Bureau of Labor Statistics. All MSAs in the sample experienced an increase in unemployment

above.

Modifications are considerably more helpful in preventing loan losses in areas that experienced a strong increase in unemployment rates soon after the loan became distressed. As hypothesized, self-cure rates in these areas are lower and redefaults are larger. However, it is not clear whether self-cure risk dominates redefault risk in this case, because the areas with the largest increase in unemployment rates also show significantly larger house price declines. The results in Table 1.9 are consistent with the results in Table 1.8, thus providing reassurance about the validity of the previous results.

Finally, Appendix B shows the results of the IV estimation based on unemployment levels at the time the loan became distressed, and on the average household income of the ZIP code in 2006 (income data comes from the IRS 2006 SOI database). Additional modifications prove to be more helpful in preventing loan losses in areas with lower unemployment and higher income.

1.6.2.3 The moral hazard problem

One possible limitation of my results is that these tests do not account for the fact that increasing modifications may potentially create a moral hazard problem. If borrowers believe that obtaining a modification is easy, they may default strategically with the intention of extracting some benefit from servicers even though they did not have any actual difficulty in meeting the mortgage payments. Mayer, Morrison, Piskorski, and Gupta (2014) show that default rates increased after Countrywide agreed with the government to offer modifications to seriously delinquent borrowers, which indicates that moral hazard can potentially reduce the benefits of modification.

Figure 1.4 shows the number of distressed loans from non-agency-only servicers relative to the number of distressed loans from both-market servicers in my sample. The figure shows that the number of distressed loans by the two types of servicers in the sample does not relatively change significantly after the incentive fee; therefore, non-agency-only servicers do not experience an abnormal increase in defaults after the incentive fee, showing that my empirical tests should not be affected by strategic behavior. It is unlikely that borrowers understood the dynamics of the setting I exploit in this paper as easily as they could understand a well-publicized settlement such as the Countrywide settlement. However, when interpreting my results, it is important to consider that it is still possible that the benefits of modifications could be reduced in a scenario in which modifications increase significantly and borrowers are aware of the increase.[38]

1.6.2.4 Matching estimation

In this section, I combine the introduction of the incentive fee with a matching methodology as a complement to the IV regressions. To the extent that the incentive fee in the GSE market impeded some modifications of

[38]The average modification concession in my sample is 1% of the outstanding balance, so it is unlikely that this potential moral hazard problem explains a significant portion of the IV coefficient of 13.9%. However, the effect could be more substantial if the expected modifications are mostly principal reductions.

both-market servicers in the non-agency market, it is possible to find a counterfactual to a modified loan from a non-agency-only servicer by matching it to a similar loan from both-market servicers–a loan that arguably would have been modified had the incentive fee not existed. This experimental design provides two very similar loans from different servicers, in which one was modified and the other was not modified due to an exogenous reason (i.e., the incentive fee). A schematization of this empirical strategy is depicted in Figure 1.5. In the period that follows the incentive fee, I match non-agency loans that were modified by non-agency-only servicers (the treated group) with non-agency loans that were not modified by both-market servicers (the control group). I then compare their losses through the average treatment effect on the treated.[39]

Of course, the validity of the test relies heavily on the matching procedure used to find the modified loan's counterfactual. For this reason, when computing the propensity score used to match the loans, a complete set of loan-level characteristics and fixed effects are included. In addition, besides matching the loans by propensity score, I also match by ZIP code and by the month in which the loans became distressed, in an attempt to control for unobservable geographical and timing factors. Additionally, I repeat the estimation using CBSA instead of ZIP code to obtain a higher matching rate.

[39] I omit loans that were modified between 7 and 12 months from the control group. The use of a propensity score tends to match a high proportion of loans modified shortly after the six-month threshold. By using this filter, the proportion of late modifications (after six months) in the matched group is virtually the same as the proportion of late modifications in the full sample.

The results of the previously described test are shown in Table 1.10. Panel A shows the logit regressions used to estimate the propensity scores. Panel B shows the average treatment effect on the treated (ATT), both for the matching that uses ZIP code and for the matching that uses CBSA (which achieves a higher matching rate). As shown by Column 1, modified loans yield losses that are 9.8% lower than non-modified loans, on average. The ATT is slightly lower when matching by CBSA (-8.4%). Finally, in Column 2, matched loans are restricted to the quartile with the largest propensity scores. Even with this tighter matching criterion, the ATTs continue to be sizeable and significant. Overall, the matching results confirm that modifications have an effect on loan losses.

1.6.3 Are certain types of modifications better than others?

For the sake of completeness, this subsection analyzes the average effects of the different types of modifications on loan losses. Specifically, the data allow for distinguishing between modifications of multiple attributes, principal reductions, interest rate reductions, and capitalizations. I estimate an OLS regression using the subset of modified loans. The specification is similar to the specification in Table 1.5, but instead of the modification indicator, I include indicators for whether the modification was a principal reduction or an interest rate reduction, or whether multiple attributes were changed. Therefore, the coefficient on each estimator is interpreted as the relative average effect of the modification type relative to capitalizations.[40]

The coefficient estimates of the previously described regression are displayed in Figure 1.6. Modifications in which multiple attributes are changed and/or the principal is reduced are the most effective on average in terms of preventing future losses. Interest rate reductions follow, being about 4.0% more helpful than capitalizations. In Appendix B, I confirm the same ordering using the matching methodology applied above.[41] These findings are consistent with previous studies, which show that modifications that grant a larger concession to the borrower experience lower redefault rates (Haughwout, Okah, and Tracy (2009), Quercia and Ding (2009)).

1.7 Implications for the aggregate economy and the effects of policy

So far, I have focused on the effects of modification on loan performance. This section discusses the implications of the results presented above for the aggregate economy (i.e., for RMBS investors and households). I then briefly discuss the fact that it appears the incentive fee had a distortionary effect on the modification decisions of both-market servicers, beyond thinking of the incentive fee only from an identification strategy standpoint.

[40] The 1,357 modifications in which the type is unidentified are dropped from the sample used in the regression.

[41] ATTs are -14.0%, -11.9%, -6.1%, and -3.3% for modifications in which multiple attributes were changed, for principal reductions, for interest rate reductions, and for capitalizations, respectively.

1.7.1 Implications for the aggregate economy

As mentioned above, a prevention of 13.9% in losses is equivalent to saving 34.5% relative to the average loan loss of 40.3%. However, the impact of this loss prevention on the RMBS market and the aggregate economy is still unanswered. Abstracting from general equilibrium considerations,[42] a back-of-the-envelope calculation can help estimate economic magnitudes.

First, let us consider an RMBS delinquency rate of 40% and an increase of 10% in modifications, which is slightly above the difference in modification rates between non-agency-only servicers and both-market servicers after the incentive fee. This 10% increase would imply preventing losses of RMBS for a total value of 0.56% (40%×10%×13.9%) of the deal. This is important because this effect is almost half of the average RMBS equity tranche, which is roughly 1.2% (Begley and Purnanandam (2013)).

Second, and more importantly, given that more than 40% of the modified loans in the sample do not redefault,[43] an increase of 10% in modifications would imply more than 4% (40%×10%) more successful modifications. Because approximately 55% of defaults stemmed from non-agency loans during the crisis, and because more than 3 million mortgages were in default at one time, 10% more modifications could have helped 66,000 (3M×55%×4%) borrowers avoid delinquency and the potential loss of their homes. If we consider

[42]It is possible that a significant increase in modifications has an effect on the mean values of some of the variables I will consider in the estimations.

[43]This is true at least before September 2012, which provides a significant amount of time for modified loans to redefault.

that about 65% of modified loans do not redefault within a year, and we use this figure as a benchmark for successful modifications, the effects would be significantly greater. Furthermore, the previous magnitudes are a lower bound not only because I use conservative figures in my estimations, but also because these estimations do not consider the negative spillover effects of foreclosures in neighborhoods (Campbell, Giglio, and Pathak (2011)) and only consider the non-agency spectrum.

Overall, these results suggest that additional modifications would have achieved the intended goal of preventing foreclosures. Furthermore, modifications allow people to remain in their homes while benefiting loan holders. These results have important implications for current policy discussions (Posner and Zingales (2009), Mayer, Morrison, and Piskorski (2009a)).

1.7.2 Unintended effects of policy: GSE intervention and non-agency modifications

In this paper, I show that the introduction of an incentive fee for modifications in the GSE market negatively affected modifications of both-market servicers, who appear to have redirected resources toward the GSE market. The incentive fee was further formalized by the Streamline Modification Program, a joint effort of Fannie Mae, Freddie Mac, the Federal Housing Finance Agency, and the U.S. Department of the Treasury. Thus, it follows that both GSEs and regulators intervened in the GSE market. Although I use the introduction of the incentive fee in the GSE market as an instrument in my empirical framework, concerns regarding possible unintended effects of policy should be raised by the fact that this intervention in the GSE market distorted the incentives to modify loans in the non-agency market of the subset of servicers affected by the policy.

The incentive fee was introduced with the intention of helping to prevent foreclosures and liquidations of loans in deals guaranteed by Fannie Mae and Freddie Mac. I cannot evaluate whether the intervention was positive or negative in terms of aggregate losses and social welfare because I do not observe GSE loan performance. However, based on my findings, the policy had a negative spillover effect by preventing modifications that would have been helpful on average, ultimately causing greater losses in the non-agency market.

1.8 Conclusion

After examining how loan modifications affect loan losses in a sample of nearly one million non-agency securitized loans that became distressed between August 2007 and February 2009, I find that additional loan modifications significantly help reduce loan losses by 34.5% relative to the average loss of 40.5%. Modifications are especially helpful in preventing future loan losses in areas where, in relative terms, housing prices decrease and unemployment increases: These areas have low self-cure risk and higher losses in the case of foreclosure. The benefits of modifications are not explained mechanically by house price rebounds; they are explained by their effect on avoiding future foreclosures and subsequent liquidations. In addition, I find that the most effective modification types grant greater concessions to borrowers.

Leaving general equilibrium considerations aside, a simple calculation indicates that an increase of 10% in modifications, which is slightly above the difference in modification rates between non-agency-only servicers and both-market servicers after the incentive fee, could potentially have helped tens of thousands of distressed households keep their homes, while also benefiting the loan holders, on average.

Overall, my results show that more modifications of non-agency loans are desirable and, in times of large increases in delinquencies, servicers tend to modify too infrequently. Modifications prevent foreclosures, a fact that directly supports recent advocates who argue that loan modifications should be used as a tool to mitigate damage from the recent foreclosure crisis.

My findings also suggest caution for policy makers, as an incentive meant to help GSE loans led to fewer modifications of non-agency loans. Paradoxically, the non-agency loan market–which is mostly subprime–was precisely the market of the highest concern for regulators and the government during the crisis, and the market from which most of the alarm originated.

Chapter 2

Did Dubious Mortgage Origination Practices Distort House Prices?

Note: This chapter is joint work with John Griffin.

2.1 Introduction

Are the costs of misrepresentation large or small? Are these costs localized, or do they affect prices more broadly? Leff (1964), Lui (1985), and Acemoglu and Verdier (2000) argue that corruption is not necessarily problematic. On the other hand, Shleifer and Vishny (1993) argue that corruption is more costly than just the amount paid for the corrupt activity because corruption has distortionary economic effects.[1] Akerlof and Romer (1993) also demonstrate how financial corruption can cause aggregate price distortions that are much larger than the amounts gained from the original activity.[2] They point to the U.S. savings and loans crisis as an example where develop-

[1]Empirical studies have largely found that corruption is harmful to economic growth due to channels such as a reduction in innovation and foreign direct investment (Mauro (1995), Wei (2000), Reinikka and Svensson (2004)). For a more detailed discussion of corruption and its effects, see Bardhan (1997) and Svensson (2005).

[2]They argue that government guarantees or other financial frictions can lead to a setting where the normal economics of maximizing value can be replaced by managers maximizing extractable value or "looting."

ers and bankers extracted rents from thrifts through non-recourse construction loans that allowed the developers and bankers to book high short-run accounting profits on building projects with negative expected returns. However, the combined activity had the unintentional effect of amplifying a commercial real estate building boom and an ultimate bust that was exceedingly costly to tax payers. In a similar vein, we ask whether questionable origination practices led to any distortions in the recent 2002-2011 real estate boom and bust.

Misreporting a few features on a lender's application seems harmless enough; however, the process could extend credit to a borrower who may have little financial wherewithal or desire to repay. What if misreporting was not isolated, but instead driven by origination practices that differed widely across loan originators with different geographic coverage? ZIP codes that contained a high presence of originators with questionable origination practices may have experienced relatively more undeserved loans than ZIP codes with better loan origination standards. This excess credit may have led to increased housing demand causing a rise in prices when excessive credit was applied and a decrease in prices when the credit was removed. This is the central explanation we test.

Our paper builds on influential work by Mian and Sufi (2009), Mayer and Pence (2009), and Pavlov and Wachter (2011) that links residential real estate prices and valuations to supply-side credit. Mian and Sufi (2009) show that subprime ZIP codes experienced a large increase in credit from 2002 to 2005 unrelated to income growth. This increase in credit can be traced to the rise of securitization (Nadauld and Sherlund (2013)), indicating support for a supply-based explanation for the 2002 to 2006 house price bubble. Mian and Sufi note that additional research is required to better understand the economics behind the credit supply mechanism. We fill this void by examining the relationship between cross-sectional housing price movements and cross-sectional variation in origination practices.

There is substantial evidence that the "originate-to-distribute" model led to "lax screening" and poor quality loans (Keys, Mukherjee, Seru, and Vig (2010), Purnanandam (2011), and Keys, Seru, and Vig (2012)). However, there is also growing awareness that the problem may have moved beyond a lack of careful monitoring to actively pushing loans that did not meet underwriting standards. Jiang, Nelson, and Vytlacil (2013), Carrillo (2011), Ben-David (2011), Garmaise (2013), and Haughwout, Lee, Tracy, and Van der Klaauw (2011) all document various aspects of mortgage misreporting at certain banks or in certain geographic areas. Piskorski, Seru, and Witkin (2015) and Griffin and Maturana (2014) detect large-scale second-lien misreporting that is later associated with significantly higher probabilities of default. They find that second-lien misreporting varies widely across originators. This wide variation in misreporting practices across originators lays the empirical groundwork to ask if bad origination practices simply led to isolated losses or if they were linked to regional distortions in home prices.

In particular, one can contrast two main views about the role of securitization in the housing crisis that both build upon the existing literature and are generally consistent with the originate-to-distribute view. First, securitization may have been subject to low standards across the board with little variation across originators. If this is the case, then ZIP codes with larger increases in supply-side credit due to securitization should exhibit the largest increases and decreases in housing prices in the boom and bust cycles, respectively. Second, rather than securitization giving out excess credit in a consistent manner, certain originators which engaged in rampant misreporting may have given much more credit to unqualified borrowers. If these bad origination practices concentrated in certain ZIP codes, the excess credit could have led to an increased demand for housing from uncreditworthy borrowers who ultimately would default. These practices could have led to substantial price distortions.

We test these two views while also noting that the two explanations are not necessarily mutually exclusive. To examine the first view, we measure the fraction of loans securitized in a ZIP Code. For the second explanation, each year, we classify originators in the highest tercile of second-lien misreporting in Griffin and Maturana (2014) as the 'worst' originators. We measure the fraction of all transactions each year within a ZIP code by the worst, medium, and best originators.[3] We first document that ZIP codes with high fractions of securitized loans exhibited a larger rise in house prices from 2003 to 2006 and a larger decrease from 2007 to 2012. However, we find that this correlation is

[3] Even the originators with lower levels of second-lien misreporting exhibited some small amounts of second-lien misreporting. Measurement problems should reduce the power of our tests and understate the impact of misreporting.

weaker than that between bad origination activity in a ZIP code during 2003 to 2006 and home price increase and subsequent 2007 to 2012 price decrease. This relation between the worst origination activity in a ZIP code and subsequent 2003 to 2006 house price increases and 2007 to 2012 decreases holds within Metropolitan Statistical Areas (MSAs) and after controls for income levels and income growth. The relation also holds for ZIP codes in the top 25% of the income distribution, indicating that the effect is not confined to subprime ZIP codes. Overall, the 858 ZIP codes with the highest fraction of worst originators' market share increased 75% faster (63% relative to 36%) over the 2003 to 2006 boom relative to the 4,318 ZIP codes with a lower presence of bad originators. Conversely, from 2007 to 2012, these same ZIP codes with a high presence of worst originators experienced a decrease nearly twice as large as the other ZIPs (40% relative to 21%).

The strong relation between house prices and bad origination practices in the ZIPs need not be causal. For example, originators with bad practices may simply be more aggressive at expanding into areas of rapidly increasing prices, or they may have expanded into more inelastic areas that experienced larger increases. To investigate the causality we take several approaches.

First, as instruments for the market share of the worst originators between 2003 and 2006, we use the market share of the worst originators and the number of worst originators present in a ZIP code, both in 2002, which is prior to our period of examination. The main intuition is that lenders had a certain geographical presence before the advent of massive securitization. Those with bad practices expanded operations most rapidly through their original locations. A two-stage regression procedure confirms the distortive effect of bad origination practices on house prices. An increase of 5% in the market share by the worst originators increases housing returns by 6.4% in the 2003 to 2006 boom and decreases housing returns by 11.6% in the bust.

Second, bad originators may have expanded operations by targeting areas of unmet loan demand by uncredit-worthy borrowers. We use ZIP Code level loan rejection rates from the Home Mortgage Disclosure Act from 1996 to 1999 to predict the expansion of bad origination activity. We find that this instrument strongly predicts worst originator market share in 2003 to 2006. The second-stage regression confirms the distortive effect of bad origination practices on house prices.

Third, we use anti-predatory law changes between 2004 and 2005 as an exogenous source of variation that restricts the lending activity of the worst originators. During the boom years, ZIP codes in states that passed anti-predatory laws experienced a 9.6% annual lower home price increase relative to states with no law change.

Fourth, if the extension of credit by worst originators was causal, then the supply of credit should precede price peaks. Although there is considerable variation in ZIP code level price peaks, the peak of credit by the worst originators precedes the house price peak in over 90% of our 858 ZIPs with large market share of worst originators.

Fifth, originators with bad practices may simply be more aggressive at expanding into areas of rapidly increasing prices. We test this by matching the 2003 to 2006 run-up of house prices of ZIP codes with a large market share of worst originators with ZIP codes with a low presence of the worst originators within the same MSA. The ZIP codes with high concentrations of the worst originators exhibit a 15% larger bust from 2007 to 2012 even after controlling for income and income growth. This is consistent with the worst originators issuing credit in an irresponsible manner, which caused a distortive effect on house prices.

Sixth, an alternative explanation is that the worst originators rationally expanded credit into ZIP codes with high elasticity of housing supply, and that these ZIP codes were more sensitive to housing swings. We test this in two ways. First, the effect of the worst origination activity should lead to larger price swings in areas of inelastic land supply. We find this result. Second, in elastic ZIP codes, instead of excess credit leading to large increases in house prices, it led to large increases in new housing construction over 2004 to 2006 that was seemingly unwarranted since housing prices crashed in the bust. Consistent with building to meet more legitimate demand, elastic ZIP codes with a low presence of the worst originators had only a minor burst in housing prices compared to the large correction in the ZIP codes with a high presence of the worst originators. This evidence in elastic ZIP codes is consistent with loose credit from the worst originators creating excess housing supply that led to an excess building boom, followed by a subsequent price decline.

We then turn to examine the channel through which the worst originators affected prices. First, bad originators could extend credit to uncreditworthy borrowers by originating loans to borrowers with a higher stated risk profile. Second, these worst originators could be poorer at screening their applicants. Third, through the underreporting of applicant risk, they could be granting loans to applicants with a risk profile that is even worse than stated. We find evidence for the first and third explanation but not for the second. Our set of worst originators do issue loans which have a much higher expected delinquency percentage at the time of issuance, even controlling for stated loan attributes. Yet, the interest rates that worst originators charged were stronger predictors of future default than for better originators, indicating that they were seemingly better at screening loan applicants than their peers. Finally, we find that the originators who engaged in second-lien misreporting may have engaged in full-doc loan misreporting as well.[4] Thus, originators who engaged in large amounts of second-lien misreporting had bad practices in the sense that they gave credit out to borrowers with a higher stated risk profile while also underreporting the true risk profile of their borrowers.

We also study the fact that bad originators may have expanded operations by targeting areas of unmet loan demand. We use ZIP Code level loan

[4]The loans these originators reported as full documentation defaulted at a higher rate than from other originators even after controlling for other loan attributes and a ZIP code-level propensity score loan matching. These same loans ended up having missing debt-to-income information over 99% of the time.

rejection rates from the Home Mortgage Disclosure Act from 1996 to 1999 to predict the expansion of the origination activity of the different types of loan originators. We find that only the origination activity by the worst originators relates to unmet demand.

Finally, for illustrative purposes, we compare the costs of inflation by comparing the activity in the 858 ZIPs with high presence of bad origination activity to those in the 4,318 ZIP codes with a low presence of bad origination activity. We find that the ZIP codes with bad origination activity had $656 billion in excessive transaction values from 2003 to 2012 and over $1 trillion in excess market value at the peak. These back-of-the-envelope calculations indicate substantial potential effects of bad origination activity, but more work is needed to assess precise magnitudes.

Overall, our evidence supports the hypothesis that bad origination practices had a distorting effect on house prices. Nevertheless, we are not trying to distinguish the precise extent to which all of the increase in credit from originators which engaged in misreporting is due to fraud or simply lax (but legal) standards. Our goal is not to examine all causes for the housing price bubble as surveyed by Mayer, Pence, and Sherlund (2009b) or Levitin and Wachter (2012).[5] Our evidence is difficult to reconcile with arguments that the cri-

[5] While our focus is on cross-sectional differences, Hubbard and Mayer (2009) find that interest rates may have played a sizeable role in the aggregate housing increases whereas Glaeser et al. (2010) and Adelino, Schoar, and Severino (2013b) find a small effect. Coleman, LaCour-Little, and Vandell (2008) and Demyanyk and Van Hemert (2011) find deteriorating underwriting standards.

sis was not driven by problems in securitization incentives (Gorton (2008) and Gorton (2009)), or that origination practices did not drive house prices (Foote, Gerardi, and Willen (2012)).

2.2. Hypotheses

Housing prices respond to a shift in the demand curve (Herring and Wachter (2000), Hubbard and Mayer (2009)). As lenders loosen credit standards, those who could not previously qualify to purchase a house are able to. Additionally, borrowers who qualified for smaller loans can now afford larger ones. If lenders allow purchasers with little or no equity to borrow large amounts of credit, then there could be a large shift in the demand for housing. The magnitude of the demand shift will depend on what fraction of new borrowers, who were previously credit constrained, are given access to credit. Thus, our tests follow a similar rationale as those done by Mian and Sufi (2009) and Pavlov and Wachter (2011); when the supply of credit is extended by lowering underwriting standards, the demand curve for housing shifts outward, and housing prices increase. However, originators may vary in regards to the extent they affect demand. Originators who were willing to loan to uncreditworthy borrowers may shift the demand curve more than originators who screen borrowers to meet certain stated minimum standards. Once an originator is willing to misreport whether a borrower has money down or income above a threshold, the loan may be issued to a borrower with little ability to repay the loan.[6] This could occur not only for non-agency loans, but also for agency loans to the extent that the misreporting is undetected by the federal agency. In contrast, an originator who is securitizing loans but is not misreporting may not lend to borrowers below standards. Additionally, Ben-David (2014) shows that higher leverage buyers paid 3.4% too much for the house, providing a more immediate channel for origination practices to affect house prices since misreported second-lien loans are typically of extremely high combined loan-to-value. This reasoning leads to our first hypotheses:

Hypothesis 1.1: *ZIP codes with a larger fraction of originators with bad practices will experience more rapid house price increases during periods of credit expansion.*

Hypothesis 1.2: *ZIP codes with a larger fraction of originators with bad practices will experience larger price decreases during the period of credit contraction.*

Alternatively, it may be that lending standards were low across the board and that all originators gave out credit indiscriminately to uncreditworthy borrowers. If this is the case, then house prices should purely be related to the fraction of loans in the ZIP that are securitized. House prices should

[6] It is well documented that underwriters who securitized loans through RMBS did perform due diligence and thus likely understood that the loans they were acquiring were worse than stated. If the underwriter paid the appropriately lower price for the origination on the misreported loan, then it would eliminate the incentive to misreport. However, if the underwriter also profits from misreported loans in RMBS and purchases the loans at less than the required misreporting discount, then originators would be incentivized to continue to misreport.

not be related to the misreporting practices of the originators.

Hypothesis 1.1A and 1.2A: *The misreporting practices of the originator will have little or no relation to house price increases or decreases.*

Saiz (2010) and Glaeser, Gyourko, and Saiz (2008) show that the elasticity of the available supply of land has a large effect on housing prices. In inelastic ZIP codes, prices may increase quite rapidly with increases in housing price demand, whereas in ZIP codes with an elastic supply of available land, price increases will be short-lived and followed by new construction. Hence, if bad originators lead to an increase in housing, we expect the price distortion effects of bad credit should be greatest in areas of inelastic land supply.

Hypothesis 2: *ZIP codes in areas of inelastic land supply should experience the most rapid increase and decrease in housing prices in response to bad credit practices.*

Glaeser, Gyourko, and Saiz (2008) show prices are never more than 10% to 15% above production costs in areas of elastic supply. In elastic areas when originators with bad practices extend credit, excess credit should not lead to larger price increases since new housing can be built. However, it does lead to overbuilding, and when credit is removed, house prices will experience large busts not typically associated with areas of high elasticity.

Hypothesis 3: *In areas of elastic land supply, bad origination practices will lead to large price decreases during the period of credit contraction.*

Hypothesis 3A: *In areas of elastic land supply, bad origination prac-*

tices have no relation to prices during the period of credit contraction.

We now turn to our data and measurement of bad origination practices.

2.3. Data, Measures, and Sample

In this section we discuss our data sources, the measure of bad origination practices, the construction of our empirical measures, and sample selection.

2.3.1. Data

The data used in this study is from a number of reputable sources. Property transaction information is obtained from DataQuick, securitized loan information from Lewtan's ABSNet Loan, ZIP code-level house price indices from Zillow, ZIP code-level demographics from the Decennial Census 2000, and ZIP code-level household income information from the IRS.

DataQuick is one of the main providers of real estate transaction information recorded by county assessors. Specifically, we use DataQuick's History File, which provides the transfer date, location, the type of property transaction, and the names of the originators involved. Lewtan, on the other hand, compiles and cleans information from servicer/trustee reports of non-agency RMBS deals (similar to the information available to RMBS investors in Prospectus Supplements at issuance). From ABSNet, we use fields including the origination date, type of transaction, originator name, and ZIP code associated to the loan. We obtain house price indices from Zillow, an online real estate database, for 12,614 ZIP codes from 2003 to 2012. Mian and Sufi (2009) report that the Fiserv's Case Shiller Weiss indices have a correlation of 0.91 with overlapping ZIP codes in Zillow, yet Zillow's coverage is much broader.[7] Lastly, we obtain demographic and income data for controls for our empirical tests from the 2000 Decennial Census and the IRS SOI. Specifically, from the Census we obtain ZIP code-level variables such as the population, the number of houses, and the housing vacancy rate. From the IRS files, we obtain the average household income per ZIP code in 2001 and the change in average household income between 2001 and 2006. We also use the Home Mortgage Disclosure Act (HMDA) data set. This data set contains detailed information about loan applications and the actions that followed the applications (i.e., whether the loan was originated along with the reason in the case that the origination failed). Using Census track information the HMDA data is mapped to our ZIP Codes for approximately 70% of our sample.

2.3.2. Originator Practices

We use the measure of unreported second-lien loans in Griffin and Maturana (2014) as a proxy of mortgage originators' bad practices. This indica-

[7]The Zillow Home Value Index (ZHVI) is a time series of median home values. Zillow home values, called "Zestimates," are calculated by a proprietary automated valuation model that accounts for both recent nearby transaction data and home attributes. This model generates Zestimates for both recently sold homes and homes that have not seen a recent transaction. Within a ZIP code, the ZHVI is generated by calculating the median Zestimate. The raw median is then adjusted for seasonality and systematic residual error, among other filters. A detailed description of the methodology can be found on the Zillow website: http://www.zillow.com/research/zhvi-methodology-6032.

tor essentially compares the data from servicer/trustee reports (in ABSNet) with the corresponding property transactions from the county deed records (in DataQuick). Although Griffin and Maturana examine three types of misreporting, second-lien misreporting is strongly related to the originator whereas owner occupancy misreporting and appraisal overstatements are not. They find that more than 13% of the first-lien loans originated between 2002 and 2007 that were reported as not having a second lien in the RMBS records did have a second lien issued on the same day in the county-level transaction records. Piskorski, Seru, and Witkin (2015) also find extremely similar levels of second-lien misreporting using entirely different data sources and methodologies. They also find that misreporting varies widely across states, suggesting substantial cross-sectional variation for analysis. Griffin and Maturana find that the unreported second-lien indicator varies significantly across the set of the largest 25 mortgage originators in their sample. They show that delinquencies by the originator are strongly related to second-lien misreporting levels, even after controlling for the three types of loan level misreporting. This suggests that the originators with high levels of second-lien misreporting may have engaged in other bad practices which led to losses. Thus, we use originator second-lien misreporting because this type of misreporting seems correlated with other bad practices including but not limited to mortgage misrepresentation practices.[8] Nevertheless, in the last section we will further investigate

[8]As an independent verification of this conjecture, WMC mortgage, which had the highest level of second-lien misreporting rate in Griffin and Maturana's sample, is reported to be under criminal investigation by the FBI and the US Department of Justice. The

the potential problems with these originators.

Each year, we classify the same 25 loan originators with more than 2.2 million loans in Griffin and Maturana (2014) into three groups based on the cumulative fraction of misreported loans they issued. Specifically, we use the amount of cumulative second-lien misreporting of each originator in year $t-1$ to rank the originators in year t.[9] We refer to the originators in the tercile with the highest misreporting as the *worst originators* and to the originators in the tercile with the lowest misreporting as the *best originators*.[10] Some of the originators with medium or low levels of second-lien misreporting have been reported to have engaged in questionable loan practices. For this reason we call them 'medium' or 'best' but also note that they may have additional types of misreporting/fraud beyond the second-lien misreporting. To the extent that our benchmark for better practices still contains some bad origination practices, our empirical tests using this benchmark will likely understate the extent to which bad origination practices affected house prices. The amount of misreporting per tercile per year is presented in Appendix B and shows that indeed the best tercile has trivial levels of second-lien misreporting compared to the worst originators.

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accusations against WMC include rampant practices of falsifying loan documents in many dimensions and sidelining employees who repeatedly reported some of the falsifications they had seen (Hudson and Reckard (2012)).

[9]Since we have the unreported second-lien indicator for the period 2002 to 2007, our rank of originators starts from 2003 (using 2002 data). Also, beginning in 2008 we hold the ranking fixed for the following years.

[10]We also refer to the originators in the second tercile as *medium originators*. If the originators are not in the set of 25 originators we call these *unranked originators*.

2.3.3. Empirical Framework

As we are interested in the negative spillover effects of loan originators' practices, we use DataQuick to create a set of ZIP code-level measures to capture the importance of each type of originator in the mortgage market at each ZIP code. We use purchase transactions and not refinances since we are interested in the transactions that influence market prices. The use of the county deed records is important to expand the set of mortgages beyond those securitized in non-agency RMBS. This set includes agency mortgages and privately held mortgages, though we suspect that mortgages privately held by the worst originators are negligible since these originators are known for their role in securitizing mortgages. To capture their relative importance, we divide the number of loans for purchase issued by each type of originator (worst, medium, best, and not ranked) in each ZIP code by the total amount of purchase transactions in the ZIP code with originator information over the ranking period. Likewise, we construct measures of the fraction securitized by each type of originator. For this, we take the number of purchase loans issued by each type of originator in each ZIP code from the securitized (non-agency) loans in ABSNet and divide it by the total number of purchases per ZIP code in DataQuick.

2.3.4. Sample Selection

To guarantee the accuracy of our measures and empirical tests, we impose some restrictions on the ZIP codes sample. First, since our main measures heavily rely on the identification of loan originators, we only use loans with non-missing originator names, and we drop ZIP codes where the originator name coverage is less than 25% as some counties may not commonly report originator names. Of the 69.9 million purchase transactions (from 2003 to 2012) recorded in DataQuick, 24.7 million or 25.5% have non-missing lender names.[11] Second, because we want accurate measures, we require ZIP codes to show more than 500 purchase transactions during the period 2003 to 2006. Third, we drop ZIP codes where the proportion of securitized (ABSNet) loans to county level (DataQuick) loans is in the highest 2.5%, as those extreme values are likely due to a relatively lower coverage of the DataQuick database. Finally, since most of our specifications will rely on identification within the MSA, we drop MSAs with less than 15 ZIP codes remaining after applying the first three filters explained above. A total of 5,176 ZIP codes remain which compares favorably to the Mian and Sufi (2009) house price sample of slightly over 3,000 ZIP codes.[12] The 5,176 ZIP code sample has lender name coverage for 42.5% of the observations. We only use these loans with lender name coverage to generate our measures and tests.

[11]The states with larger coverage are New Hampshire, Montana, and the District of Columbia, with 72%, 59%, and 57% coverage, respectively. On the other hand, South Dakota and Vermont have virtually no coverage.

[12]DataQuick shows 18,909 ZIP codes with purchase transactions with originator names during 2003 to 2012. After dropping the ZIP codes with low originator name coverage 11,096 ZIP codes remain. An additional 3,861 ZIP codes do not comply with the minimum requirement of the number of transactions, leaving 7,235 ZIP codes. Then, 1,069 ZIP codes are lost after merging the sample with Zillow and dropping ZIP codes with high values of *fraction securitized*, or missing values for the controls. Finally, 990 ZIP codes are dropped because they are in MSAs with less than 15 ZIP codes.

Descriptive statistics for the ZIP code-level measures and controls are shown in Table 1.2. As mentioned before, we focus on the top 25 lenders ranked by non-agency securitization and classify them into three groups (worst, medium, and best) based on the amount of second-lien misreporting they exhibit. On average, the worst originators were responsible for 5.6% of loans between 2003 and 2006[13] while the medium and best originators have 17.3% and 11.1% respectively of the market of loans with originator names reported in the 5,176 ZIPs. The worst, medium, and best originators combined account for 34% of the loan originations between 2003 and 2006 with the remaining 66% being from originators who are not among the top 25 non-agency originators. Furthermore, the three types of ranked originators (top 25) account for 92.4% of privately securitized loans over the period from 2003 to 2006 (14.5% of the 15.7%). These non-agency securitizers also sold loans to agency deals. We find that 12 of the 25 non-agency securitizers did business with government-sponsored enterprises (GSE).[14]

[13] Appendix B shows that *worst originators' market share* (from 2003 to 2006) varies considerably across ZIP codes.

[14] Fannie Mae makes public a subset of the loans they have acquired since 2000. In the 7.3 million loans in the Fannie Mae sample data that were originated between 2003 and 2007, we confirm the presence of five of our top 25 non-agency originators in the agency market (Bank of America, Chase, GMAC-RFC, SunTrust, and Wells Fargo). However, it seems likely that at least a subset of the remaining 20 originators were also involved with agency deals because Fannie Mae simply lists the lender name as "Other" for 21.6% of the loans. We investigate this further by conducting an online search for agency MBS prospectuses that linked our remaining 20 originators with GSEs and confirm that at least an additional seven lenders (Argent, BNC, Countrywide, First Franklin, Fremont, New Century, and WMC) sold loans to agency MBS as well.

2.4. Bad Origination Activity and House Prices

Our main goal in this section is to test whether home prices are related to origination activity of misreporters (**Hypothesis 1.1 and 1.2**) or simply the amount of securitization (**Hypothesis 1.1A and 1.2A**). We start this section by visually inspecting if the presence of the worst originators is related to extreme positive (negative) house returns during the boom (bust). We divide ZIP codes into two groups: those where the average market share of the worst originators during the third quarter of 2004 and the second quarter of 2006 exceeds 10% are in the first group, and the remaining ZIP codes are in the second group.[15] The objective is to compare the house price movements during the boom and the bust of both groups and determine if the group with the highest worst originators' market share experienced a higher increase on prices during the boom, followed by a more violent crash of prices during the bust. Figure 2.1 shows the progression of house prices: the ZIP codes with the highest worst originators' market share went up by 63% during the 2003 to 2006 boom, whereas the ZIP codes with the best originators only experienced a 36% run-up from 2003 to 2006. This 27% difference in absolute terms amounts to a 75% (27%/36%) relatively larger increase in house prices in the ZIP codes with the worst originators relative to the other ZIPs. Conversely, from 2007 to 2012, ZIP codes with a high presence of worst originators experienced a

[15]The sizes of these groups are of 858 and 4,318 ZIP codes, respectively. Appendix B shows that there is indeed considerable variation in the worst originators' market share during the 2003 to 2006 period. The worst originators' market share of both groups rapidly decreases towards zero during 2007, as most of the worst originators went bankrupt or lost considerable business.

40% decrease as compared to a 21% decrease for ZIP codes with the best originators, a 19% absolute difference or a 90% (19%/21%) higher relative decrease.

We look at this result from a geographic perspective and show that the 57% of ZIP codes with large house price increases and 61% of ZIP codes with large house price decreases have a high market share of worst originators.[16] As additional motivation for our analysis, in Appendix B, we show that there exists a strong positive relation between securitization and ZIP-code house returns. However, the simple univariate relation is eight to ten times stronger when using the fraction securitized by lenders with bad origination practices rather than the total fraction securitized.[17]

We turn now to a more formal framework. In Panel A of Table 2.2, we present OLS regressions where ZIP code house price return is the dependent variable and the market shares of the three different types of originators are the main explanatory variables of interest. We include several controls. To account for the relation between securitization and house returns we include the fraction of loans securitized at the ZIP code during the period 2003 to 2006. We also control for demographic characteristics that might be related to cross-sectional differences in house returns across ZIP codes such as the ZIP

[16]These results are found in Appendix B. The findings suggest that loan origination by the worst originators is correlated with extreme house price fluctuations during the boom and bust periods and that the effect is particularly strong in the West Coast. This emphasizes the importance of our use of MSA controls in all of our main results.

[17]In Appendix B, we confirm this result in a multivariate regression framework.

codes' population, number of house units, and vacancy rate (all in the year 2000). Controls are included for average household income in 2001 and average household income changes from 2001 to 2006 (also at the ZIP), as well as MSA fixed effects. Standard errors are heteroskedasticity-robust and clustered at the MSA level. Column 3 confirms the strong relation between the worst originators' market share and house returns during the boom. Also, the effect of the worst originators is the most important among the three types of originators. The coefficient of 1.235 on *worst originators' market share* (which is statistically significant at the 1% level) implies that an increase of 5% (which is less than the difference between the median *worst originators market share* and the 90^{th} percentile) in loan origination activity in a ZIP code by the worst originators during 2003 to 2006 increased prices 6.18% on average during the boom. Similarly, during the bust, an increase of 5% in the worst originators' market share during 2003 to 2006 implied a decrease of 7.10% in house prices during the bust (Column 6). As in the boom, the relevance of the worst originators surpasses that of the other types of originators. Loan issuances by originators with the worst practices are strongly related to house price distortions. Interestingly, the fraction securitized does not enter significantly as a determinant of the housing price run-up during the boom with the inclusion of worst originators' market share. The coefficient does enter significantly in the bust, though the slope is about 1/7 of that on the market share due to worst originators.

Given that Mian and Sufi (2009) show that house price distortions were concentrated in subprime ZIP codes, we now examine if our results only hold in subprime ZIP codes. Panel B of Table 2.2 shows the same specifications in Panel A for the set of ZIP codes in the highest quartile based on household income during 2001. The effect of *worst originators' market share* is strongly related to house returns in the boom and bust. A 5% change in the market share of the worst originators is associated to an average increase of 4.25% in house return. This relation is strong both statistically and economically, but the slope is slightly lower than in the full sample. In the bust, the coefficient remains unchanged.

While Mian and Sufi (2009) use the percentage of borrowers in a ZIP code with credit score under 660 as of 1996 as their measure of 'subprime' ZIP code, we use income since we lack credit score data for the whole ZIP code and the two should be highly correlated. However, as a check, we compute a measure of subprime ZIP code based on the credit scores of the securitized loans in ABSNet (using the whole database of approximately 20 million loans) and obtain similar results (see Appendix B). Overall, we find little support for home prices being related to just securitization (**Hypothesis 1.1A and 1.2A**) but considerable support for ZIP Code level variation being related to bad origination practices (**Hypothesis 1.1 and 1.2**).

2.5. Did Bad Origination Cause House Price Distortions?

Although we find a strong relation between house prices and the percentage market share of the worst originators, the relation may not be causal. The worst originators may have followed more aggressive business strategies in entering ZIP codes with increasing house prices. A second and related possibility is that the worst originators aggressively entered the ZIP codes where they expected prices to go up during the boom because of tight supply. Third, there could be some other omitted variable that drove both bad origination and house price movements. We take a variety of approaches to investigate if the relationship between bad origination practices and house price distortions is causal. First, we use the market share of the worst originators and the number of worst originators in a ZIP code both in 2002 as instruments for *worst originators' market share* over the 2003 to 2006 period. Second, we explain the expansion of worst originator market share through a measure of unmet credit demand at the ZIP code from 1996 to 1999. Third, we use changes in state-level anti-predatory lending laws as a quasi-natural experiment that affects the amount of bad loan origination. Fourth, we analyze the cross-sectional variation in price peaks across ZIP codes to examine if supply peaks from worst or best originators anticipate ZIP code level price peaks. Fifth, to address whether worst originators were targeting areas of rapid price appreciation, we match house price returns over the boom period and compare returns during the bust for a set of ZIP codes with a high activity of the worst originators and a set of ZIP codes with low worst originator activity. Sixth, we use the elasticity of housing supply from Saiz (2010) as a proxy for expectations about future house appreciation. We focus on the effect of worst originator activity on house prices in ZIP codes in the most elastic MSAs.

2.5.1. Instrumenting For Worst Originator Activity

We would like to find an instrumental variable (IV) related to *worst originators' market share* that does not affect house prices directly. In this subsection, we use a set of plausible instruments based on the idea that originators spread their operations first through their existing operations, and also by moving to areas with untapped credit demand. First, we use the market share of the worst originators and the number of worst originators present in a ZIP code, both in 2002, to account for originators geographical locations prior to the crisis. Subsequently, we use the aggregate ZIP-code level loan application rejection rates from 1996 to 1999 as a proxy for unmet loan demand.

2.5.1.1. Worst Originator Presence

We start by using the market share of the worst originators and number of worst originators present in a ZIP code in 2002 as this instrument for *worst originators' market share*. The idea of the instrument is that originators with dubious practices were located in particular locations across the country. As securitization grew in importance, these originators expanded to new areas, but the original locals experienced a wave of credit which allowed them to increase their market share in these areas. 2002 is the year before we examine any home price movements.[18] As shown in the bottom panel of Table 2.3, the instrument satisfies the relevance restriction. Both instruments predict *worst originators' market share* from 2003 to 2006. The partial correlation is positive and highly significant, and the F-statistic is also always higher than the threshold of 10, both with and without control variables, indicating that there is no problem of weak instruments (Bound, Jaeger, and Baker (1995); Staiger and Stock (1997)). Intuitively, the exclusion restriction should also be satisfied. There is no apparent reason why the regional presence of certain bad lenders should affect house prices in the future through something other than the credit channel.

We follow the typical two-stage procedure and estimate the IV regression using 2SLS. The results are displayed in Table 2.3. The fitted value of *worst originators' market share* from the first stage regression positively affected house returns during the period from 2003 to 2006, while the effect is negative during the period from 2007 to 2012. The IV coefficients on *worst originators' market share* are 1.272 in the boom and -2.309 in the bust, which are slightly larger than the OLS coefficients of 1.235 and -1.420. More specifically, an increase of 5% in the market share by the worst originators increases

[18]More specifically, in each ZIP code during 2002, we count the number of loan originations by lenders that consistently ranked among the highest tercile of second-lien misreporting from 2003 to 2008 (i.e., Fieldstone, First Franklin, Fremont, GreenPoint, and WMC). For an originator to be considered as being 'present' in a county, it had to originate at least 300 loans in the county during 2002. Appendix B shows the frequency with which each lender ranked in each tercile of second-lien misreporting between 2003 and 2008.

housing returns by 6.4% in the 2003 to 2006 boom and decreases housing returns by 11.6% in the bust. Interestingly, neither medium nor best originator activity has a significant distortionary effect on house returns in this IV setting.

2.5.1.2. Prior Loan Application Rejection Rates

We now use the aggregate ZIP-code level loan application rejection rate in the period from 1996 to 1999 as an instrument for *worst originators' market share*. The idea behind this instrument is that the loan application rejection rate is a proxy for unmet loan demand from borrowers. Loan application rejection rates indicate that the demand for housing is more likely to be unsatisfied. If worst originators expand by granting credit to uncredit worthy borrowers then the previous loan application rejection rate will be positively related with *worst originators' market share* over the subsequent 2003-2006 period in which their activity expands. The effect of loan application rejection rates on future housing prices would then be through a future increase in market share, plausibly satisfying the exclusion restriction. It is reasonable to expect that loan rejection rates should be related to house prices through the credit channel.

For each ZIP code, we sum the number of rejected applications between 1996 and 1999 as captured by the HMDA records and then divide them by the total number of applications received during the period.

As before, we estimate an IV regression using 2SLS. The first stage coefficient is shown at the bottom panel of Table 2.4. The partial effect of the instrument on *worst originators' market share* is positive and strongly significant and therefore the instrument satisfies the relevance restriction. Additionally, the F-statistic is also considerably higher than the threshold of 10 for the two specifications (with and without ZIP code-level controls) indicating that the instrument has strong predictive power.

The results of the second stage (where housing returns are regressed on the fitted values of *worst originators' market share* from the first stage plus controls) are shown in Table 2.4. The coefficients on *worst originators' market share* are several times larger than the coefficients of the OLS regressions. Therefore, we are more comfortable relying on the strong causal inference from the regression than on the exact coefficient magnitudes. As with the previous instrument, neither medium nor best originator activity has a significant effect on house returns.

Overall, the instrumental variable regressions indicate that worst originator activity had a causal effect on house prices during the periods from 2003 to 2006 and from 2007 to 2012.

2.5.2. Anti-Predatory Law Changes

An alternative path to examining if bad origination activity has an effect on house prices is to focus on a quasi-exogenous event that reduced bad quality loan originations, and therefore, loan originations by the worst originators. In this section, we use anti-predatory law (APLs) as a quasi-natural experiment to analyze the effect of loan supply by the worst originators on house price movements. Bostic et al. (2008) find that APLs reduce subprime loan originations, especially when the APLs are more restrictive. These law changes should have also led to relatively less origination of loans by originators with bad standards. Indeed, we find such a relation in Appendix B. Hence, we compare house price movements in states that passed APLs between 2004 and 2005 with house price fluctuations in states with no anti-predatory laws.[19]

Figure 2.2 shows the house price movements of the ZIP codes that suffered a law change and the ZIP codes in the benchmark (no APL). Both sets of ZIP codes experience extremely similar house price increases during the two-year period anteceding the law change. However, after the law change, house prices of ZIP codes in the first group continue to increase at a much lower rate than the ZIP codes in the second group. This finding is consistent with APLs preventing some bad quality loan originations which would have otherwise occurred, and hence reducing upward pressure on home prices.[20]

Table 2.5 shows the previous result more formally. We regress house price returns on a *Post Law* dummy and a set of controls and quarter fixed effects.[21] The negative coefficient on the law dummy variable of -0.024 (and$t$-statistic of 4.31) means that ZIP codes in states that passed APLs had a 2.4%

[19]The set of states that implemented APLs in 2004 and 2005 are New Mexico (Q1 of 2004), South Carolina(Q1 of 2004), Massachusetts (Q3 of 2004), Indiana (Q1 of 2005), and Wisconsin (Q1 of 2005). The set of states with no APLs are Arizona, Delaware, New Hampshire, Montana, Oregon, Washington, and Tennessee.

[20]In Appendix B we also plot the results for the three different quarters in which the law changes occur and find that house prices significantly diverge in two of the three quarters (Q1 and Q3 of 2004).

[21]Standard errors are heteroskedasticity-robust and clustered by CBSA.

slower quarterly (9.6% annually) home price increase than in states with no APLs. The table also shows a negative effect of the law changes on worst originator loan supply (Column 2, coefficient of -2.1% and t-statistic of 3.97) which is consistent with the law change being the channel for reducing the origination supply.[22] Columns 3 and 4 of Table 2.5 show that the effects are considerably stronger for the subsample of ZIP codes with worst originator loan supply above the median level. The relative increase of house prices and worst originator loan supply of ZIP codes with law changes are 3.5% and 5.1% lower, respectively, than ZIP codes with no APLs.

2.5.3. The timing of Supply and Price Peaks

If loan origination had a causal effect on house prices during the bust period, we would expect loan supply to have peaked before house prices. Furthermore, if bad credit by the worst originators had a more important effect on the excessive house price fluctuations during the financial crisis than credit of better quality, then we should see loan origination supply by the worst originators generally peaking before house prices and significantly more frequently than the loan supply by the best originators. In contrast, if prices were driven by the best originators initiating credit, then the expansion or contraction of their activity in a ZIP code should anticipate ZIP code level price peaks.

[22] We construct a measure of worst originators' loan supply by dividing the number of loan originations by the worst originators each quarter by the total amount of loans granted by the worst originators from 2003 to 2009. To put the variable on a quarterly basis values are then scaled by multiplying the variable by 28 (the number of quarters between 2003 and 2009).

To ensure the adequate presence of bad originators, we focus on the subset of ZIP codes where the average market share of the worst originators exceeded 10% between the third quarter of 2004 and the second quarter of 2006. Interestingly, there is considerable variation in house price peaks between 2005 and 2007 with 18.5 % of price peaks occurring in 2005, 43.8 % in 2006, and 31.3% in 2007 (as shown in Appendix B) that allows us to examine if this cross-section of peaks is related to supply peaks.

Figure 2.3 shows the quarterly zip code-loan supply by the worst and the best originators along with ZIP code house price movements during the four year window around the house price peak. Panel A consolidates all peak-years. While both types of supply initially increase together with house prices and decrease before the house price peak, loan supply by the worst originators decreases rapidly before the supply by the best originators. Panels B through D show that this pattern is consistent within each peak-year. In particular, each peak year displays the pattern that supply by bad originators peaked two to three quarters prior to prices. Note that for ZIP codes that peaked in 2005 and 2006, the supply of loans by the best originators is only slightly below its peak level six to eight quarters after the price peak even though prices in the ZIP have fallen 15%.

To test for the reliability of these patterns, Table 2.6 presents a test of difference in proportions. More specifically, for the 858 ZIPs with a large presence of worst originators, we compare if the proportion of ZIP codes where loan supply by bad originators peaks before house prices is significantly larger than the proportion of ZIP codes where loan supply by the best originators peaks before house prices. Loan supply by the worst originators peaks before house prices in 90% of the 804 ZIP codes (of the 858) where house prices peaked between 2005 and 2007. Furthermore, this proportion is 24.3% larger in absolute terms (36.8% larger in relative terms) than the proportion of the best originators and is strongly significant (z-statistic of 11.7). Once again, this result is consistent across different peak-years. Loan supply by the worst originators anticipates price peaks more than the loan supply by the best originators, suggesting that supply by bad originators played a leading role in house price fluctuations.

2.5.4. Were the Worst Originators Simply Chasing House Returns?

The worst originators might have simply been chasing large house price returns and quickly entering booming ZIP codes. Therefore, the greater drop in house prices for ZIP codes with worst originator activity could simply be due to mean reversion. Alternatively, the worst originators may have been issuing undeserved credit which increased the supply of loans and amount of credit, leading to house price dislocations. The two explanations have different implications with respect to matching ZIPs with large worst originator activity with ZIPs with low presence of the worst originators based on house price increases. Suppose the worst originators were simply targeting ZIP codes which experienced a large house price increase, then if one matched ZIP codes with similar level of home price appreciation during the boom, one would expect a similar bust as the home prices reverted to their pre-bubble expectation. In contrast, if the worst originators were giving out undeserved credit unrelated to fundamentals, then one would expect these ZIP codes to crash to lower levels than ZIP codes where more of the credit may have been warranted.

To examine these hypotheses, we take the 858 ZIP codes where the worst originators had an average market share of more than 10% between the third quarter of 2004 and the second quarter of 2006, and match each of them to a ZIP code in the same MSA with the most similar housing returns from 2003 to 2006 and where the worst originators have a market share lower than 5%.

House prices go down at the same time that the majority of the worst originators went bankrupt unexpectedly (gray area in Figure 2.4).[23] Panel A of Figure 2.4 compares the house price movements of the worst originator and the matched group price movements and finds that, consistent with the matching construction, the two groups have almost identical price run-up during the boom. The dashed lines represent the 95% confidence interval and show that the decrease in house price is significantly stronger for the group of ZIP codes with the worst originator activity. For the worst misreporting group, the home prices decrease 39.4% whereas for the matching ZIP code within the same MSA with a lower presence of the bad originators, home prices decrease 23.5%. Thus, even though the two ZIP codes increase the same amount from

[23]While it can be argued that an originator's bankruptcy could have been expected, the time of the event was arguably unexpected.

2003 to 2006, ZIP codes with bad originators experience a 15.9% larger drop in housing prices from 2007 to 2012. The results are consistent with the hypothesis that loans issued by the worst originators exhibited a distorting effect on house prices.

In Panel B of Figure 2.4, due to concerns that originators might have self-selected to ZIP codes (or that individuals might have self-selected to originators) within small areas, we repeat the previous exercise but the match is done among ZIP codes of different MSAs. The findings are similar; the ZIP codes with the worst originators experienced the largest drops in house values after 2007.

One concern is that the differences in the bust could be due to differences in characteristics, like the average income between the two groups. Hence, in Table 2.7, we test the result shown in Panel A of Figure 2.4 more formally by controlling for differences in the population, income, and growth in income in the ZIP codes. Here, we estimate the following difference-in-difference regression:

$$\begin{aligned} return = \beta_0 + \beta_1(worst\ orig. > 10\% \times post2006) \\ + \beta_2(worst\ orig. > 10\%) + \beta_3 post2006 + X\Gamma + \epsilon, \quad (2.1) \end{aligned}$$

where *return* is a vector with house returns,[24] *worst orig. >10%* is an indicator that identifies the 858 ZIP codes in the first group graphed in Panel A of Figure

[24]Specifically, this vector has 2 returns per ZIP code, one for the boom and one for the bust.

2.4, *post*2006 is a dummy variable that takes a value of one if the date of the return corresponds to the year 2007 or later, and zero otherwise, and X is the same set of control variables used in the specifications in Table 2.2. Since we are interested in comparing the magnitudes of the price decrease between the two groups, the parameter of interest is β_1. Column 2 in Table 2.7 shows that after most of the worst originators went bankrupt, the house prices of the group of ZIP codes decreased an additional 17.3% on average compared to the ZIP codes with lower activity of the worst originators.

Overall, ZIP codes where the worst originators had more business show a larger price decrease even when the ZIP code is matched to require a similar house price increase than the ZIP code where the worst originators had less business. This effect is not explained by income, income growth, MSA fixed effects, or other controls. The results are inconsistent with the notion that the relation between worst origination market share and home prices is due to strong trend chasing. The results are consistent with the hypothesis that prices fell more in ZIP codes with larger fractions of worst originators because these originators doled out unwarranted credit, which had a distortive effect on home prices.

2.5.5. Are the Price Distortions by Bad Originators Explained by Increased Price Expectations?

We analyze the possibility that the worst originators entered the markets where they expected house prices to go up. Following Mian and Sufi

(2009), we use elasticity of housing supply from Saiz (2010) as a proxy for house appreciation expectations.[25] The elasticity measure is a topologically-based measure that gauges elasticity by surrounding geographic constraints. Glaeser, Gyourko, and Saiz (2008) show that house prices fluctuate much more in inelastic MSAs.[26] Hence, expectations of future house price increases by originators are expected to be higher in areas where housing supply is relatively inelastic. Thus, it is in these MSAs where we expect an increase in credit supply to have a larger effect on prices. If the previous results are driven by the worst originators aggressively forecasting house prices by issuing loans in ZIP codes with inelastic supply, the expansion of worst origination market share should be confined to those areas with tight elasticity. Following Mian and Sufi (2009), we examine the top and bottom 50% and 25% of MSAs. We examine whether the worst originator market share increases were confined to MSAs where the housing supply was inelastic. We find that, though level of activity was lower in elastic MSAs, worst originators increased their market share both in inelastic and elastic MSAs.[27]

Since home prices should be more sensitive to credit in inelastic MSAs, we expect to see a stronger relation between worst origination credit expansion and house prices in inelastic MSAs as discussed in **Hypothesis 2**. The

[25]Table VI of Saiz (2010) reports the elasticity of housing supply for the 1970 to 2000 period for 95 metro areas with a population over 500,000. We match 65 of these with our sample, which includes 90.2% of the ZIP codes.

[26]We confirm this result in Appendix B.

[27]Appendix B shows the worst originators' market share in both elastic (blue solid circles) and inelastic (black hollow circles) MSAs. Panel A shows the result when we split the MSAs in half, and Panel B shows the result when we select the extreme quartiles.

expansion of credit should have little effect on the run-up in prices in elastic MSAs since increases in prices will predominately be due to increases in construction costs and limited with respect to land supply. However, as discussed in **Hypothesis 3**, these areas could experience a considerable decrease in house prices during the crash if the expansion of credit to unqualified borrowers led to an increase in housing supply that was not supported by income and population growth.

In Table 2.8, we estimate our main specifications for the bust for elastic and inelastic ZIP codes (based on the top and bottom 50% and 25% of MSAs). For the elastic MSAs, a 5% increase in loan issuances by the worst originators explains an economically large decrease of 9.05% in house returns on average (column 1). The results are slightly stronger in the top 25% of ZIP codes (column 2), indicating that bad origination activity was associated with a considerable bust in elastic MSAs. In columns 3 and 4 (inelastic MSAs) the same coefficients are negative and significant (-1.268 and -1.265), indicating that only the worst origination market share during the run-up (not medium or good) is predictive of a bust in inelastic MSAs).[28]

As the worst originators expanded to very elastic areas, we further analyze the subset of ZIP codes in the 25% more elastic MSAs. Figure 2.5 shows that in the elastic ZIP codes with high share of the worst originators the excess credit led to a large increase in new housing construction in between

[28] In Appendix B we present the same regressions for the boom period.

2004 and 2006 that was seemingly unwarranted since housing prices crashed in the bust. Elastic ZIP codes with a low presence of the worst originators had only a minor burst in prices and ended up with house prices in 2012 around 20% above those in the ZIP codes with a high presence of the worst originators. This explanation is consistent with loose credit from bad originators creating excess housing supply that led to an excess building boom, followed by a subsequent price decline.

In summary, the worst originators did not seem to solely target areas where land supply was constrained, as there are substantial increases in worst origination credit in elastic areas. Moreover, the increase in credit in areas of elastic supply led to unwarranted housing and a subsequent crash in home prices even in areas that experienced little run-up due to the increase in credit.

The fact that the bad origination activity is related to crashes in areas of elastic land supply indicates that the relation between bad origination and crashes is not due to bad originators solely concentrating in areas of tight land supply. More generally, our five tests to address various aspects of potential endogeneity suggest that bad origination practices did cause economically large house prices fluctuations.

2.6. The Channel

The previous section establishes that bad origination practices seem to have caused large home price fluctuations, but the precise mechanism still needs to be further explored. We seek to learn more about the differences in the lending practices of the originators who engaged in second-lien misreporting. First, bad originators could extend credit to uncreditworthy borrowers by originating loans to borrowers with a higher risk of default and stating their attributes correctly. Second, the poor loan performance could also suggest that certain lenders were poor at screening their applicants. Third, the understating of borrower information itself could be extending credit to uncreditworthy applicants. We examine these possibilities in several ways. We look at the predicted probability of delinquency of the loans issued by the worst and the best originators using the stated loan attributes. We also examine if the interest rates charged by bad originators indicate poor screening abilities of bad originators and if misreporting by bad originators was confined to the second-lien channel, or if there is any evidence of any other forms of misreporting.

Finally, we examine whether the lending activity of the different types of originators in a ZIP code relates to the previous unmet demand in the area.

2.6.1. Loan Quality

We assess the credit risk of the loans at the point of origination to see if the loans by bad originators had similar or higher probabilities of default. We base our estimates on the stated loan properties. We have detailed borrower and loan characteristics for the set of loans in non-agency securitized products (ABSNet). We fit a logit model using all first-lien loans originated before 2001, where the dependent variable is a dummy that takes the value of one if the loan became seriously delinquent (90+ days) before 2002, and zero otherwise. The set of explanatory variables includes credit score, combined loan-to-value ratio, interest rate, the log of the loan amount, and dummy variables for level of documentation (low/no-doc or full-doc), self-reported occupancy status, refinance, and the existence of a prepayment penalty. We then use the estimated coefficients in combination with the loan characteristics of the securitized loans originated during 2003 or later to obtain expected probabilities of delinquency. This approach to estimate delinquency probabilities is similar to the one used by Ashcraft, Goldsmith-Pinkham, and Vickery (2010). We assess this behavior across ZIP codes with a high or low presence of the bad originators since bad originators may be lending in riskier regions.

Panel A of Figure 2.6 shows that the worst originators securitized loans that were significantly worse than the loans securitized by the best originators in terms of average ex-ante probability of delinquency. This occurs across all types of ZIP codes.

One possibility is that the competition with bad originators caused the best originators to issue riskier loans in ZIP codes where there is a high presence of bad originators. This does not appear to be the case. The best originators do not issue loans with a higher foreclosure frequency in ZIP codes where bad originators have a high presence. Originators with high misreporting are also not issuing riskier loans in the ZIP codes where they have the largest presence. This suggests that the effect of bad originators is predominately due to their higher market share where they issue loans with a higher ex-ante default probability.

It is interesting to note whether good and bad originators engaged in second-lien misreporting in the areas in which bad originators had a high market share. Panel B of Figure 2.6 shows that the worst originators do have high levels of second-lien misreporting in the ZIP codes where they have the highest presence; almost half of their originations exhibit second-lien misreporting (on purchase transactions) in the ZIP codes where they have the highest market share. However, bad originators still misreport around 35% of their loans in the ZIP codes where they have little business. In contrast, the best originators present significantly lower (and much more stable) levels of second-lien misreporting. This indicates that the misreporting practice was not primarily a problem only for certain loan officers or branch locations but related to business practice and culture within the loan originating firms.

2.6.2. Do worst originators misreport in other dimensions or were they poor at loan screening?

Originators who misreported on second-lien loans issued loans of lower quality based on stated loan attributes. But if stated loan attributes are incorrect, the loans could be even riskier than recorded. It is not clear if our separation of originators by second-lien misreporting is an issue related solely to second-lien misreporting or a symptom of other forms of misreporting. We obviously do not have access to internal bank sources such as documentation and debt coverage to verify such information. However, we can investigate the predictive value of loan attributes for default. If an attribute was incorrectly reported, then this would decrease their ability to predict future default.

We estimate an OLS regression where the dependent variable is an indicator for delinquency (90+ days) and the explanatory variables are a set of loan-level characteristics. Since we seek to capture any difference in the explanatory power of the loan-level controls across different type of lenders, we include the interaction of a dummy variable for worst lender with each one of the controls. The coefficient estimates are shown in Column 1 of Table 2.9. We find that combined loan-to-value ratio and the level of documentation are strong predictors of default in general, but their explanatory power is weaker for loan originators with high levels of second-lien misreporting.

We also wish to understand if bad originators were poor at screening borrowers, or if they understood that certain borrowers were of higher risk but lent to them anyway. If an originator sought to maximize short-term profits, they would lend to a risky borrower at a high rate, but then underreport some of the loan's risky features when they resold it. In this case, the loan's interest rate would still be a good predictor of future default. Yet, if bad originators simply did a worse job of screening borrowers, the interest rate would be a less accurate predictor of default for bad originators. In Column 2 of Table 2.9 we regress delinquency on the interest rate at the time of loan origination. Overall, the interest rate strongly forecasts default, but it is a significantly better forecaster for the worst originators. In Column 3 with the other control variables, we again find that interest rates are a much stronger predictor of default for bad originators. These results suggest that the worst originators were not poor at gauging risk or tricked by borrowers, but that they charged higher interest rates for loans that were indeed considerably more risky. Additionally, even after controlling for interest rates, the explanatory power of combined LTV and full-doc indicators are less for bad originators.

Although these findings are consistent with potential misrepresentation of these loan features, it is also possible that the low predictive power of certain borrower information is due to some other differences in the types of loans originated by bad originators. The loans may have substantially different features that make the comparison of loan-to-value ratio and documentation level problematic. To investigate this possibility, we take a propensity score matching approach where for each loan issued by a bad originator, we find another loan issued by a good originator in the same ZIP code-year that also has similar propensity score.[29]

In Columns 4 to 6 of Table 2.9 we repeat the analysis discussed above in the matched sample of loans. We again find that combined LTV and the full-

[29] To compute the propensity score, we estimate a logit regression where the dependent variable is a dummy that takes the value of one if the loan was issued by one of the worst originators and takes the value of zero if the loan was issued by one of the best originators, and the explanatory variables are combined LTV, credit score, interest rate, the log of the loan amount, and indicators for low-doc, non-owner occupied property, arm loan, and the existence of a prepayment penalty. Also, we impose a maximum distance between propensity scores of 1%. We are able to impose such a tight criteria because there are many more loans from the better originators, and we match with replacement up to a maximum of five times. We find a match for 81% (86,822 out of 107,338) of the loans by the worst lenders. A comparison of the matched loans from both groups is shown in Appendix B confirming that they have similar characteristics.

documentation indicator are significantly weaker predictors of delinquency for worst originators which again raises the possibility of further misreporting.[30]

The fact that combined LTVs are not good predictors of delinquency is to be expected given the second-lien misrepresentation documented by Piskorski, Seru, and Witkin (2015) and Griffin and Maturana (2014). However, the fact that the same originators who engage in second-lien misreporting have a lower forecasting power of the full-documentation indicator raises the possibility of additional misreporting. [31] To investigate this we look at whether there are any differences between good and bad originators in the reporting of debt-to-income for full-documentation loans. Interestingly, good and bad originators have approximately the same percentage (43.5% for good and 43.1% for bad) of loans self-declared as full-docs. Yet, of the loans classified as full-doc, 16.9% have missing debt-to-income for good banks while this percentage is 99.6% for bad banks (only 168 full-doc loans issued by the worst originators have non-missing debt-to-income). These differences indicate either a lack of disclosure which is more concentrated for bad originators or that the bad originators misrepresented low or no-doc loans as full documentation with the intention of making them appear less risky.[32]

[30]In addition, we estimate regressions of delinquency on loan characteristics for both matched groups (best and worst) separately in Appendix B. Loan characteristics explain a much larger proportion of the variation in delinquencies in the sample of loans from worst originators (differences in R-squared is 10% in absolute terms or 40% in relative terms).

[31]Indeed, the recent JP Morgan statement of facts states that loans securitized by JP Morgan were missing key pieces of documentation, including income. The report also identified excessive debt-to-income. http://www.justice.gov/iso/opa/resources/94320131119151031990622.pdf.

[32]Griffin and Maturana (2014) found that owner occupancy misreporting was primarily

In summary, although we cannot fully rule out other explanations, we find that the originators who engaged in second-lien misreporting have considerably more full-documentation loans that are missing a key piece of documentation raising the possibility that these originators engaged in other forms of misreporting beyond second-lien. We do not find evidence that bad originators did a poor job of classifying risky borrowers since the interest rates they charged were actually better predictors of future default than the interest rates charged by originators with lower levels of misreporting. Hence, we find that the 'worst originators' were primarily 'bad' in their misreporting practices and in giving out credit to more risky borrowers. These two are intuitively related since an originator who can misreport key loan features can give more credit to riskier borrowers than an originator who correctly reports.

2.6.3. Unmet Demand

We conclude this section by examining whether the market share of the different types of originators in 2003 to 2006 is relates to previous unmet demand. Table 2.10 shows the results of an OLS estimation where the dependent variable of the regressions are the market shares of the different types of originators between 2003 and 2006, and the independent variable of interest is the ZIP code-level loan rejection rates (unmet demand) between 1996 and 1999. Of the three types of market share (i.e., worst, medium, and best),

on behalf of occupants, and appraisal misreporting was primarily a misreporting from appraisers. Consistent with this finding, Appendix B reports owner occupancy misreporting and appraisal misreporting across good and bad originators and finds little differences.

only the one corresponding to the worst originators is positively related to unmet demand. This result is consistent with the worst originators expanding by granting credit to uncredit worthy borrowers in areas where demand for housing was not satisfied.

2.7. How Large were the Price Dislocations due to Bad Practices?

In the previous sections, we show that ZIP codes with a high presence of loan originators with bad lending practices experienced significant house price distortions: prices rose quickly and excessively during the boom period and later crashed more violently during the bust period than prices in ZIP codes with low presence of the worst lenders. Intuitively, given the importance of the housing market to the US economy (19% of the GDP in 2005), the effect of bad origination practices seems economically important. In this section we attempt to measure the aggregate economic distortions of bad practices to the affected households. We develop two measures (a transaction and an aggregate value measure) that capture this 'cost' in the 858 ZIP codes in our sample where the worst originators had a market share of at least 10%. We compare the increase in house prices in ZIPs with a high presence of the worst originators to ZIPs with a low presence of the worst originators as the benchmark. To the extent that our benchmark also had bad lending or not all ZIP codes largely affected by bad practices are included, our calculations likely understate the true importance of bad practices.

First, we measure the total excess transfer paid by buyers in each house purchase transaction during the boom and the bust. To the extent that prices in the ZIP codes with a high presence of the worst originators were unjustifiably high, each purchase will involve an unnecessary money transfer to the seller. Recall from Figure 2.1 that ZIP codes with the highest worst originators' market share appreciated at 63% whereas ZIP codes with a low presence of bad originators only appreciated 35% between 2003 to 2006. As a proxy for price inflation, we use the cumulative difference (starting from the end of the first quarter of 2003) between the Zillow's house price return of each affected ZIP code relative to the average house price return of the 4,318 ZIP codes with low presence of the worst lenders (the benchmark). As an example, let's suppose a house was sold during the fourth quarter of 2005, and that the ZIP code where the house is located experienced a 61% appreciation from the end of the first quarter of 2003 to the end of 2005. Given that the average return of the house prices in the benchmark was 34% during that same period, the cumulative return difference is 27%.[33] Because we lack house prices for the majority of purchase transactions, we use the Zillow index price in the ZIP as a proxy for the value. We realize the index is a median price and that the total would be more accurate if using mean values, but the use of medians is advantageous to avoid the affect of outliers. Thus, if the Zillow house price index in the ZIP code was, for example, $300,000 by the end of 2005, then the excess transfer

[33] To avoid double counting, when the cumulative house return of the bad ZIP code is lower than the cumulative house return of the benchmark, we fix the difference at 0%.

paid by the buyer was \$81,000 (\$300,000×27%). To compute the aggregate measure at the ZIP code level for the second quarter of 2005, we multiply the excess transfer of \$81,000 by the number of purchase transactions in the ZIP code that quarter. We only consider purchase transactions to capture the money transfers between borrowers.[34] We repeat this process for every quarter during 2003 to 2011 and sum all quarter aggregates to obtain a ZIP code level measure. Then, we aggregate across the 858 ZIP codes to obtain the aggregate cost of the price distortions. We find that these money transfers at higher prices were \$480.3 billion from 2003 to 2006 and a further \$176.1 billion at higher prices from 2007 to 2011, totaling \$656.4 billion during the 9 year period (2003 to 2011). Though not linear, this averages \$72.9 billion per year, which is equivalent to 0.53% of the average GDP of the US between 2003 and 2011.

Second, we measure the aggregate distortion on the stock of houses in the 858 ZIP codes with the highest presence of the worst originators by the end of 2006. Again, we rely on the cumulative difference in house price returns between the affected ZIP codes and the benchmark of the ZIP codes with low presence of the worst originators, from the end of the first quarter of 2003 to the end of the fourth quarter of 2006. To obtain the total stock of houses in each ZIP code, we sum all houses in the ZIP code with construction

[34]However, the cost of bad practices can be even larger if we consider that borrowers could have additionally extracted money from lenders through cash-out refinances. While DataQuick does not specifically flag cash-out refinances, we know that 54.2% of the transactions in the 858 ZIP codes were either cash-out or term/rate refinances.

dates between 1900 and 2006 (according to DataQuick). Under the assumption that all houses have the value of the index by the end of 2006, we can then obtain the price distortion of the ZIP code-house stock by multiplying the ZIP code index price by the house price stock and by the cumulative difference in house price returns. We find that the sum of the price inflation of the house stock in the 858 ZIP codes was of $1,098.9 billion the end of 2006. This price inflation is equivalent to 6% of the total stock of all the ZIP codes covered by DataQuick and Zillow, which account for $18.4 trillion as of the end of 2006.[35] Additionally, estimates of losses associated with MBS securities issued before the financial crisis are around $500 billion.[36] Thus, the distortionary costs of misreporting on prices may exceed the direct losses to investors.

Overall, for the 858 ZIP codes where the bad practices originated, we estimate substantial distortion on transactions and on the stock of houses. We hope the calculations give a sense of the potential costs of bad practices, but we also realize that estimating the total effect of bad practices is difficult since our benchmarks likely also contain bad practices, and moderately bad practices are present in other ZIP codes. Additionally, the relation between the originators and house prices may not be directly attributable to their bad practices. Nevertheless, we think these back-of-the-envelope calculations support Akerlof and Romer (1993) 's conjectures that the distortionary effects

[35] Appendix B shows histograms of frequencies for the main ZIP-code level variables involved in the cost calculations.

[36] See Financial Crisis Inquiry Commission (2011) and Greenlaw, Hatzius, Kashyap, and Shin (2008)

of misreporting can be large.

2.8. Conclusion

The process of underreporting key loan attributes can have the by-product of facilitating credit to borrowers who have little ability to repay. We find that fraction of originators who engaged in second-lien misreporting helps explain the 2003 to 2006 run-up of housing prices and its subsequent 2007 to 2012 collapse. The effect is strong even after controlling for credit due to securitization, income, income growth, and present even in the wealthiest ZIP codes, indicating that the effect is not merely a subprime phenomenon. Through two instrumental variable tests, and an exogenous law change, it appears that the bad originators did cause the price distortions through the issuance of excess credit through bad practices. There is no evidence to support the view that these effects are due to bad originators merely chasing prices or forecasting areas of highly inelastic housing to expand into.

It is interesting to ask why the credit from these misreporting mortgage originators had a much larger distorting influence on house prices. Since the interest rates these bad originators charged were useful predictors of default, it seems these originators were actually competent in their applicant screening. It appears that these lenders who engaged in second-lien misreporting not only gave credit to borrowers with a much higher stated risk profile, but also significantly underreported the true loan risk.

Our paper highlights that the unintended side-effect of mortgage misreporting may have had real direct distortionary costs on those who bought homes in ZIP codes with substantial misreporting. In fact, the distortionary effects caused by the bad practices of even a small fraction of originators may be even more costly than the direct losses suffered by MBS investors. These findings also suggest that actions of agents who facilitated misreporting jointly helped caused the real estate crisis and that these agents should not simply blame investor losses on market conditions. We are not intending to provide a comprehensive examination of all forces behind the home price expansion, but rather to document the importance of bad origination practices in this growing literature. Indeed, Akerlof and Romer (1993)'s conjecture that the distortions of prices through corrupt activity may spur further speculation seems to be an interesting avenue for future housing market research. Our findings support the idea that misreporting, a seemingly benign form of corruption, can have broad and unintended consequences, not just in developing markets, but also in the most open and transparent of markets. We hope our findings will spur additional debate and research on the role of trust and integrity in financial markets.

Tables and Figures

Table 1.1: Mortgage servicers at year-end 2007

This table characterizes 22 of the 23 servicers in the loan sample at year-end 2007 (the missing servicer, MetLife Home Loans, was founded on 2008). Volume figures (in billions of dollars) are from Inside Mortgage Finance (IMF). Subprime volume values marked with a * are not reported by IMF and are estimated from Moody's or S&P's servicer evaluation reports.

Servicer name	Servicer type	Total Volume	Total Mkt. share	Subprime Volume	Subprime Mkt. share
Countrywide	Both-market	$1,476	13.2%	$112	11.9%
Wells Fargo	Both-market	$1,473	13.2%	$51	5.4%
CitiMortgage	Both-market	$800	7.2%	$62	6.6%
JP Morgan	Both-market	$776	7.0%	$74	7.9%
WAMU	Both-market	$623	5.6%	$44	4.7%
Bank of America	Both-market	$517	4.6%	$10*	1.1%
RFC - GMAC	Both-market	$410	3.7%	$41	4.4%
IndyMac	Both-market	$198	1.8%	$4	0.4%
National City	Both-market	$188	1.7%	$1	0.1%
PHH Mortgage	Both-market	$159	1.4%	<$1	<0.1%
SunTrust	Both-market	$146	1.3%	<$1	<0.1%
Aurora (Lehman)	Both-market	$113	1.0%	$2*	0.2%
EMC (Bear)	Both-market	$89	0.8%	$22	2.3%
Ocwen	Non-agency	$53	0.5%	$53	5.6%
Option One	Non-agency	$48	0.4%	$48	5.1%
HomEq (Barclays)	Non-agency	$47	0.4%	$46	4.9%
Litton (Goldman)	Non-agency	$46	0.4%	$41	4.4%
Saxon (Morgan)	Non-agency	$34	0.3%	$34	3.6%
American Home	Non-agency	$30	0.3%	$30	3.2%
SPS (CSFB)	Non-agency	$29	0.3%	$21	2.2%
PNC	Both-market	$25	0.2%	$1*	0.1%
Carrington	Non-agency	$15	0.1%	$15	1.6%
	Total Outstanding	$11,150		$940	

Table 1.2: Data summary

This table describes the sample of mortgages that became distressed between August 2007 and February 2009, by servicer type. Data comes from Lewtan's ABSNet Loan. The sample consists of loans in RMBS deals from vintages between 2000 and 2007. All loans are first-lien loans originated between 2001 and 2007, with an original amount exceeding $30k and a LTV ratio at origination lower than 103%. FHA loans, VA loans, negative amortization loans, and loans of multi-unit properties are not included. Loans are categorized by the type of servicer holding the servicing rights at the time the loan became distressed (60+ days delinquent or modified). The first group, "both-market servicers," includes servicers which manage loans both from government-sponsored enterprises (GSEs) mortgage-backed securities and from non-agency mortgage-backed securities. The second group, "non-agency-only servicers," includes servicers which mostly manage non-agency loans. (d) denotes a dummy variable.

	Full period Aug07-Feb09		Before fee Aug07-Jul08		After fee Aug08-Feb09	
	Both	NA only	Both	NA only	Both	NA only
Number of loans in distress	744,334	254,733	465,324	172,862	279,010	81,871
Type of resolution						
Modification within 6 months (d)	10.3%	14.0%	8.2%	9.6%	13.7%	23.2%
Multiple attributes (d)	3.8%	7.6%	2.1%	4.7%	6.5%	13.7%
Principal reduction (d)	0.2%	0.4%	0.2%	0.3%	0.2%	0.6%
Rate reduction (d)	1.1%	1.9%	0.9%	1.5%	1.4%	2.7%
Capitalization (d)	5.1%	3.9%	4.9%	3.0%	5.5%	5.9%
Other (d)	0.1%	0.1%	0.2%	0.1%	0.1%	0.3%
Modification within 12 months (d)	16.6%	23.7%	14.6%	19.4%	20.0%	32.9%
Foreclosure initiation within 6 months (d)	34.2%	36.3%	38.0%	39.0%	28.0%	30.7%
Foreclosure initiation within 12 months (d)	45.9%	48.1%	49.7%	51.3%	39.6%	41.2%
Foreclosure completion within 24 months (d)	35.5%	35.9%	39.6%	40.1%	28.5%	27.2%
Loan characteristics						
Unpaid balance ($, mean)	266,679	235,139	259,284	236,728	279,013	231,782
Credit score at origination (mean)	656.1	629.5	649.9	627.4	666.5	633.9
Combined LTV at origination (%, mean)	86.1	85.8	86.7	86.2	85.1	85.1
Interest rate at origination (%, mean)	7.6	8.6	7.7	8.7	7.4	8.5
Adjustable (d)	69.8%	77.8%	73.2%	80.2%	64.2%	72.8%
Non-owner occupied (d)	15.4%	11.8%	14.9%	11.6%	16.1%	12.2%
Low/No-doc (d)	59.7%	49.4%	58.6%	50.1%	61.5%	48.0%
Prepayment penalty (d)	46.2%	67.1%	48.5%	67.2%	42.5%	66.8%
Resolution outcome						
Modification redefault (d)	51.1%	67.3%	54.2%	67.0%	48.0%	67.6%
Modification redefault within 1 year (d)	31.2%	43.9%	32.5%	43.9%	29.9%	44.0%
Pct. loss if modified within 6 months (%, mean)	33.3	30.7	40.2	37.1	26.4	25.2
Pct. loss if foreclosed within 6 months (%, mean)	60.8	62.8	63.5	65.5	54.8	55.5

Table 1.3: The effect of the incentive fee on modification rates

This table shows OLS estimates of regressions where the dependent variable is an indicator for whether a loan was modified within six months of becoming distressed. In columns 1 and 2 the explanatory variable of interest is *Both Markets*×*After Fee*, the interaction of *Both Markets* (a dummy variable that takes the value of one if the servicer managing the loan services loans both from government-sponsored enterprises (GSEs) mortgage-backed securities and from non-agency mortgage-backed securities, and zero otherwise) and *After Fee* (a dummy variable that takes the value of one if the loan became distressed after the incentive fee in the GSE market was introduced, and zero otherwise). In column 1 the regression is estimated using the full sample while in column 2 the period from February 2008 to July 2008 (when the modification rate starts being affected by the incentive fee) is excluded from the estimation. In column 3, only loans from both-market servicers are considered. Both-market servicers are divided into two groups based on the increase in delinquencies they experienced from the pre-incentive fee period to the post-incentive fee period. The explanatory variable of interest is *High Delinquency*×*After Fee*. *High Delinquency* is a dummy variable that takes the value one if the servicer belongs to the group with the larger increase in delinquencies, and zero otherwise. Loan-level controls and a variety of fixed effects are also included. Continuous control variables are standardized and the regression's intercept is not reported. All estimates are in percentage terms. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
	Full Period Aug07-Feb09	Excluding Mar08-Jul08	Both-market servicers only
Both Markets×After Fee	-5.72***	-6.37***	
	(-14.86)	(-15.06)	
High Delinquency×After Fee			-2.38***
			(-9.20)
Credit Score	-0.60***	-0.59***	-1.16***
	(-7.69)	(-9.46)	(-13.18)
CLTV	0.66***	0.68***	0.83***
	(6.36)	(6.39)	(7.72)
Interest Rate	3.77***	3.36***	2.57***
	(23.15)	(22.99)	(13.15)
Unpaid Balance	0.30***	0.23**	0.05
	(2.99)	(2.58)	(0.49)
Adjustable	4.33***	3.79***	4.23***
	(17.58)	(15.80)	(16.37)
Non-Owner Occupied	-4.78***	-4.37***	-4.29***
	(-35.38)	(-29.30)	(-34.97)
Low/No-Doc	-3.31***	-3.05***	-3.21***
	(-17.23)	(-15.92)	(-12.83)
Prepayment Penalty	4.32***	3.97***	4.61***
	(17.67)	(18.29)	(17.91)
CBSA×Origination month FE	Y	Y	Y
Servicer FE	Y	Y	Y
Distress month FE	Y	Y	Y
Observations	999,067	758,625	740,040
Adj. R^2	0.09	0.10	0.07

Table 1.4: Reduced form: the effect of the incentive fee on loan losses

This table shows OLS estimates of regressions where the dependent variable is the net loan loss, which is defined as losses minus recoveries, divided by the outstanding principal amount at the time of becoming distressed. Losses of modified loans incorporate any concessions made to the borrower. The explanatory variable of interest is *Both Markets*×*After Fee*, the interaction of *Both Markets* (a dummy variable that takes the value of one if the servicer managing the loan services loans both from government-sponsored enterprises (GSEs) mortgage-backed securities and from non-agency mortgage-backed securities, and zero otherwise) and *After Fee* (a dummy variable that takes the value of one if the loan became distressed after the incentive fee in the GSE market was introduced, and zero otherwise). Loan-level controls and a variety of fixed effects are also included. Continuous control variables are standardized and the regression's intercept is not reported. In column 1 the regression is estimated using the full sample while in column 2 the period from February 2008 to July 2008 (when the modification rate starts being affected by the incentive fee) is excluded from the estimation. All estimates are in percentage terms. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)
	Full Period Aug07-Feb09	Excluding Mar08-Jul08
Both Markets×After Fee	0.93***	0.90***
	(3.43)	(3.14)
Credit Score	1.57***	1.56***
	(9.82)	(9.47)
CLTV	5.22***	5.27***
	(17.79)	(17.35)
Interest Rate	-1.35***	-1.29***
	(-6.27)	(-6.08)
Unpaid Balance	-5.88***	-5.61***
	(-12.53)	(-11.97)
Adjustable	4.13***	4.16***
	(15.38)	(15.30)
Non-Owner Occupied	15.71***	16.01***
	(9.73)	(10.43)
Low/No-Doc	2.52***	2.50***
	(7.34)	(7.21)
Prepayment Penalty	0.95***	0.85***
	(3.96)	(3.74)
CBSA×Origination month FE	Y	Y
Servicer FE	Y	Y
Distress month FE	Y	Y
Observations	999,067	758,625
Adj. R^2	0.22	0.22

Table 1.5: OLS regressions of loan losses on modification

This table shows OLS estimates of regressions where the dependent variable is the net loan loss, which is defined as losses minus recoveries, divided by the outstanding principal amount of the loan at the time of becoming distressed. Losses of modified loans incorporate any concessions made to the borrower. *Modification* is an indicator that takes the value of one if the loan was modified within six months of becoming distressed, and zero otherwise. Loan-level controls and a variety of fixed effects are also included. Continuous control variables are standardized and the regression's intercept is not reported. The regressions are estimated using the full sample (column 1), the period before the incentive fee was implemented (column 2), and the period after the incentive fee was implemented (column 3). All estimates are in percentage terms. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
	Full Period Aug07-Feb09	Before fee Aug07-Jul08	After fee Aug08-Feb09
Modification	-6.08***	-3.77***	-8.20***
	(-15.70)	(-8.12)	(-17.60)
Credit Score	1.54***	1.25***	2.03***
	(9.72)	(6.23)	(12.19)
CLTV	5.26***	5.62***	4.71***
	(18.22)	(22.34)	(11.14)
Interest Rate	-1.12***	-1.01***	-1.54***
	(-5.32)	(-5.17)	(-5.24)
Unpaid Balance	-5.86***	-6.78***	-4.76***
	(-12.50)	(-12.44)	(-11.84)
Adjustable	4.39***	4.96***	3.55***
	(16.64)	(19.98)	(10.27)
Non-Owner Occupied	15.42***	14.85***	16.25***
	(9.64)	(8.22)	(12.39)
Low/No-Doc	2.32***	3.03***	1.10***
	(7.13)	(8.29)	(3.97)
Prepayment Penalty	1.21***	1.47***	0.79***
	(4.88)	(4.62)	(3.59)
CBSA$\times$Origination month FE	Y	Y	Y
Servicer FE	Y	Y	Y
Distress month FE	Y	Y	Y
Observations	999,067	638,186	360,881
Adj. R^2	0.22	0.22	0.21

Table 1.6: Instrumental variable regressions of loan losses on modification

This table shows the results of instrumental variable regressions estimated using the standard two-stage least squares (2SLS) procedure. Two samples of loans are used in the estimations: the full sample of loans and for a subsample that excludes loans that became distressed during the period from March 2008 to July 2008 (when modification rates begin being affected by the incentive fee). In the first stage (Columns 1 and 2) the incentive fee introduction is used as an instrument for modification. In the second stage (Columns 3 and 4) the fitted value from the first stage is the main explanatory variable. Loan-level controls and a variety of fixed effects are also included. Continuous control variables are standardized and the regression's intercept is not reported. All estimates are in percentage terms. Reported t-statistics (for the first stage) and z-statistics (for the second stage) in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
	First stage		Second stage	
	Full Period Aug07-Feb09	Excluding Mar08-Jul08	Full Period Aug07-Feb09	Excluding Mar08-Jul08
Modification			-13.92***	-11.91***
			(-2.95)	(-2.60)
Both Markets×After Fee	-5.76***	-6.42***		
	(-15.35)	(-15.80)		
Credit Score	-0.61***	-0.61***	1.51***	1.51***
	(-8.42)	(-10.14)	(9.33)	(9.02)
CLTV	0.67***	0.68***	5.26***	5.28***
	(6.53)	(6.75)	(18.01)	(17.31)
Interest Rate	3.72***	3.31***	-0.75***	-0.79***
	(23.00)	(23.13)	(-3.66)	(-4.14)
Unpaid Balance	0.33***	0.26***	-5.64***	-5.39***
	(3.23)	(2.90)	(-12.45)	(-11.99)
Adjustable	4.41***	3.85***	4.84***	4.70***
	(18.28)	(16.63)	(12.24)	(13.12)
Non-Owner Occupied	-4.78***	-4.35***	14.97***	15.42***
	(-34.95)	(-30.62)	(8.79)	(9.64)
Low/No-Doc	-3.23***	-2.94***	2.13***	2.22***
	(-16.10)	(-15.06)	(5.10)	(5.70)
Prepayment Penalty	4.23***	3.89***	1.66***	1.44***
	(17.36)	(17.83)	(4.87)	(4.55)
Origination month FE	Y	Y	Y	Y
CBSA FE	Y	Y	Y	Y
Servicer FE	Y	Y	Y	Y
Distress month FE	Y	Y	Y	Y
Observations	999,067	758,625	999,067	758,625
Adj. R^2	0.09	0.10	0.21	0.21
F-statistic	235.5	249.6		

Table 1.7: Instrumental variable regressions by housing price drop

This table shows the second stage of instrumental variable regressions estimated using the standard two-stage least squares (2SLS) procedure. The loans in the sample are divided into three groups based on their short-run house price returns. Column 1 shows the results of the IV estimation on the loans with returns equal or greater than zero (no house price drop). Two other groups are formed based on the median return of the remaining loans. Column 2 considers the loans that experienced a small house price drop while Column 3 considers the loans that experienced a large house price drop. In the first stage (in the Appendix) the incentive fee introduction is used as an instrument for modification. In the second stage the fitted value from the first stage is the main explanatory variable. Loan-level controls and a variety of fixed effects are also included. Continuous control variables are standardized and the regression's intercept is not reported. All estimates are in percentage terms. Reported z-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
	No housing price drop	Small housing price drop	Large housing price drop
Modification	-6.87	-7.16	-34.53***
	(-0.31)	(-1.18)	(-3.09)
Loan-level controls	Y	Y	Y
Origination month FE	Y	Y	Y
CBSA FE	Y	Y	Y
Servicer FE	Y	Y	Y
Distress month FE	Y	Y	Y
Observations	21,616	451,936	451,948
Adj. R^2	0.14	0.17	0.12
Mean price drop (%)	2.1	-9.3	-34.6
Mean price rebound (%)	0.0	-5.0	-6.3
Self-cure rate (%)	27.5	22.3	13.4
Redefault rate (%)	49.4	55.5	58.5
Loss rate if foreclosed (%)	49.0	51.1	68.3

Table 1.8: Instrumental variable regressions by housing price rebound

This table shows the second stage of instrumental variable regressions estimated using the standard two-stage least squares (2SLS) procedure. I rank the loans in the large house price drop group in Table 1.7 into three groups based on their ZIP code house price rebound (from the bottom of 2009 o September 2012) and then repeat the IV estimation for each group. Column 1 shows the results of the IV estimation on the loans in areas that did not experience a rebound in house prices (prices continued to drop). Two other groups are formed based on the median return of the remaining loans. Column 2 considers the loans that experienced a small house price rebound while Column 3 considers the loans that experienced a large house price rebound. In the first stage (in the Appendix) the incentive fee introduction is used as an instrument for modification. In the second stage the fitted value from the first stage is the main explanatory variable. Loan-level controls and a variety of fixed effects are also included. Continuous control variables are standardized and the regression's intercept is not reported. All estimates are in percentage terms. Reported z-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
	No housing rebound	Small housing rebound	Large housing rebound
Modification	-37.57***	-34.23	-31.20*
	(-3.10)	(-1.46)	(-1.75)
Loan-level controls	Y	Y	Y
Origination month FE	Y	Y	Y
CBSA FE	Y	Y	Y
Servicer FE	Y	Y	Y
Distress month FE	Y	Y	Y
Observations	305,441	73,309	73,178
Adj. R^2	0.11	0.13	0.14
Mean price drop (%)	-34.1	-35.6	-35.5
Mean price rebound (%)	-12.9	2.9	12.5
Self-cure rate (%)	13.9	12.8	11.6
Redefault rate (%)	58.3	58.5	59.6
Loss rate if foreclosed (%)	70.4	64.5	64.3

Table 1.9: Instrumental variable regressions by unemployment change

This table shows the second stage of instrumental variable regressions estimated using the standard two-stage least squares (2SLS) procedure. I divide the sample into two groups based on the median increase in unemployment from the month the loan became distressed to the highest value of the index in 2009. Column 1 considers the loans with a small increase in unemployment while Column 2 considers the loans with a large increase in unemployment. In the first stage (in the Appendix) the incentive fee introduction is used as an instrument for modification. In the second stage the fitted value from the first stage is the main explanatory variable. Loan-level controls and a variety of fixed effects are also included. Continuous control variables are standardized and the regression's intercept is not reported. All estimates are in percentage terms. Reported z-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)
	Small increase in unemployment	Large increase in unemployment
Modification	-6.48	-55.28*
	(-0.56)	(-1.89)
Loan-level controls	Y	Y
Origination month FE	Y	Y
CBSA FE	Y	Y
Servicer FE	Y	Y
Distress month FE	Y	Y
Observations	226,562	224,838
Adj. R^2	0.18	0.05
Mean unemployment (%)	4.5	4.7
Mean increase in unemp. (%)	3.8	6.4
Mean price drop (%)	-12.4	-32.3
Self-cure rate (%)	23.4	15.8
Redefault rate (%)	54.5	62.3
Loss rate if foreclosed (%)	53.9	64.4

Table 1.10: Matching analysis of loan losses

This table shows the results from a propensity score matching analysis that compares the loan losses of loans modified by servicers which only manage non-agency loans with the loan losses of loans that were not modified by servicers which manage both agency and non-agency loans. The matching is performed based on the month the loans became distressed, on ZIP code (or CBSA for a higher matching rate), and on propensity scores calculated using the logit regression in Panel A (which shows odd ratios and robust z-statistics in parentheses). Continuous explanatory variables are standardized and the regression's intercept is not reported. Matching is performed without replacement using the nearest neighbor technique (1-to-1). Also, a common support is imposed and the maximum difference between the propensity scores of the treated (modified) loans and the control (non-modified) loans is limited to 0.5% (0.1% when matching by CBSA). Since some loans have identical propensity scores, the sample is randomly sorted before matching. Panel B shows the average treatment effect on modifications (ATT), with robust t-statistics in parentheses. Column 1 shows the ATT for the whole sample and column 2 shows the ATT when restricting the loans to be in the quartile with the largest propensity scores. ***p<0.01, **p<0.05, *p<0.1.

Panel A: Determinants of modification

	Pr(Modification = 1)
Credit Score	0.71***
	(-28.21)
CLTV	1.01
	(0.58)
Interest Rate	1.99***
	(78.35)
Unpaid Balance	1.01
	(0.56)
ARM	2.02***
	(33.13)
Non-Owner Occupied	0.51***
	(-21.97)
Low/No-Doc	0.59***
	(-26.96)
Prepayment Penalty	3.14***
	(59.20)
Origination month FE	Y
Distress month FE	Y
CSA FE	Y
Observations	239,761
Pseudo R^2	0.23

Panel B: Treatment effects

	(1)	(2)
Differences in loss rates, modified minus not modified	All loans	High propensity score
Matched sample (by ZIP), ATT	-9.8%***	-7.1%***
t-statistic	(-9.67)	(-3.63)
Matching rate	13.3%	5.6%
Matched sample (by CBSA), ATT	-8.4%***	-7.2%***
t-statistic	(-15.97)	(-9.82)
Matching rate	44.9%	33.1%

Table 2.1: Descriptive statistics

This table shows descriptive statistics for the 5,176 zip codes in the sample. To obtain the final sample, ZIP codes where the originator name coverage is less than 25% are dropped, and zip codes are required to show more than 500 purchase transactions during the period 2003 to 2006. Additionally, zip codes with the highest 2.5% fraction of loans securitized are dropped. Finally, MSAs with less than 15 zip codes are dropped.

	Mean	p10	p50	p90
Worst Originators' Mkt. Share (03-06)	5.6	1.7	4.8	10.8
Medium Originators' Mkt. Share (03-06)	17.3	11.0	16.9	23.9
Best Originatos' Mkt. Share (03-06)	11.1	6.6	10.6	16.2
Unranked Originators' Mkt. Share (03-06)	66	51.4	67.4	77.6
Fraction Securitized (03-06)	15.7	7.2	14.7	26.1
Fraction Securitized by Worst Originators (03-06)	1.0	0.2	0.8	2.0
Fraction Securitized by Medium Originators (03-06)	6.8	2.9	6.4	11.3
Fraction Securitized by Best Originators (03-06)	6.7	2.8	6.2	11.4
Fraction Securitized by Unranked Originators (03-06)	1.2	0.4	1.0	2.2
Population (2000), th.$	24.6	6.4	21.7	46.3
Housing Units (2000), th.$	9.7	2.5	8.8	18.0
Housing Vacancy Rate (2000)	6.0	2.2	4.5	10.7
Average Household Income (2001), th.$	56.7	30.6	47.6	88.9
Change Average Household in Income (01-06), th.$	12.9	2.4	7.8	27.3
House Price Return (03-06)	44.8	10.0	39.0	86.9
House Price Return (07-12)	-21.5	-45.5	-20.2	-0.5
Number of Zip Codes	5,176			

Table 2.2: Effect of worst originator activity on house returns

This table shows OLS estimates for regressions where zip code price return is the dependent variable, on the zip code-level market share for various types of originators during the period from 2003 to 2006. The regressions include different combinations of demographic controls and MSA fixed effects. Columns 1 to 3 show the results for the boom period (2003-2006) and columns 4 to 6 show the results for the bust period (2007-2012). Panel A shows the regression results for all zip codes while Panel B includes only the zip codes in the highest income quartile in 2001. *t*-statistics are presented in parentheses. ***p<0.01, **p<0.05, *p<0.1.

Panel A: All Zip Codes						
	2003-2006			2007-2012		
Worst Originators' Mkt. Sh.	3.253***	1.743***	1.235***	-2.282***	-1.982***	-1.420***
	(29.34)	(4.47)	(2.91)	(-34.89)	(-5.10)	(-3.55)
Medium Originators' Mkt. Sh.	1.297***	-0.449***	-0.320**	-0.007	0.010	-0.017
	(14.68)	(-3.27)	(-2.60)	(-0.13)	(0.09)	(-0.18)
Best Originators' Mkt. Sh.	0.469***	-0.698*	-0.553*	-0.182***	0.782***	0.607**
	(4.36)	(-1.79)	(-1.67)	(-2.87)	(2.77)	(2.33)
Fraction Securitized			0.045			-0.222***
			(0.40)			(-3.10)
Population			0.005***			-0.002***
			(3.15)			(-3.54)
Housing Units			-0.011***			0.006***
			(-3.12)			(4.21)
Housing Vacancy Rate			0.658***			-0.167***
			(4.59)			(-4.02)
Average Household Income			-0.001***			0.000***
			(-2.67)			(5.07)
Δ in Avg. Household Income			0.001			0.000***
			(1.35)			(3.77)
Constant	-0.010	0.506***	0.479***	-0.066***	-0.192***	-0.195***
	(-0.73)	(10.07)	(11.90)	(-8.24)	(-4.43)	(-4.72)
MSA FE	N	Y	Y	N	Y	Y
SE Clustered by MSA	N	Y	Y	N	Y	Y
Observations	5,176	5,176	5,176	5,176	5,176	5,176
Adj. R-squared	0.28	0.80	0.81	0.23	0.75	0.76

Table 2.2 - **continued**

Panel B: High Income Zip Codes						
	2003-2006			2007-2012		
Worst Originators' Mkt. Sh.	1.930***	0.659**	0.850***	-1.474***	-1.495***	-1.420***
	(6.04)	(2.44)	(2.74)	(-7.07)	(-3.59)	(-3.75)
Medium Originators' Mkt. Sh.	1.278***	-0.107	-0.075	0.146	0.171	0.159
	(8.36)	(-0.82)	(-0.57)	(1.46)	(1.30)	(1.28)
Best Originators' Mkt. Sh.	-0.200	-0.242	-0.358**	-0.094	0.452***	0.351**
	(-1.13)	(-1.26)	(-2.42)	(-0.82)	(2.73)	(2.27)
Fraction Securitized			-0.066			-0.014
			(-1.08)			(-0.24)
Population			0.001			-0.001
			(0.37)			(-0.94)
Housing Units			-0.002			0.005**
			(-0.69)			(2.00)
Housing Vacancy Rate			0.311***			-0.179***
			(3.61)			(-3.47)
Δ in Avg. Household Income			0.000*			0.000***
			(1.78)			(3.02)
Constant	0.080***	0.389***	0.389***	-0.098***	-0.174***	-0.179***
	(3.67)	(10.40)	(10.39)	(-6.88)	(-6.47)	(-6.49)
MSA FE	N	Y	Y	N	Y	Y
SE Clustered by MSA	N	Y	Y	N	Y	Y
Observations	1,035	1,035	1,035	1,035	1,035	1,035
Adj. R-squared	0.20	0.82	0.83	0.05	0.70	0.72

Table 2.3: Effect of worst originator activity on house returns – IV

This table shows the results of a two-stage estimation procedure via 2SLS. First, the endogenous variable, *Worst Originators' Mkt. Share* is regressed on the market share of the worst originators in 2002 and the number of worst banks operating in the zip code in 2002 (the instruments), the market share of the best and medium originators, and a set of controls (depending on the specification). Then, zip code price returns are regressed on the zip code-level market share for various types of originators during the period from 2003 to 2006. In particular, *Worst Originators' Mkt. Share* is the fitted value of the regression in the first stage. The regressions include different combinations of demographic controls and MSA fixed effects. Columns 1 to 2 show the results for the boom period (2003-2006) and columns 3 to 4 show the results for the bust period (2007-2012). Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by MSA. The instruments' coefficients in the first stage regression, along with the F-statistic are shown in the bottom of the table. ***p<0.01, **p<0.05, *p<0.1.

	2003-2006		2007-2012	
Worst Originators' Mkt. Share	1.669***	1.272**	-2.693***	-2.309***
	(4.37)	(2.08)	(-6.42)	(-4.04)
Medium Originators' Mkt. Share	-0.503***	-0.389**	0.0949	0.0400
	(-3.25)	(-2.54)	(0.86)	(0.39)
Best Originators' Mkt. Share	-0.301***	-0.828**	0.581*	0.434
	(-2.98)	(-2.05)	(1.95)	(1.46)
Controls	N	Y	N	Y
MSA FE	Y	Y	Y	Y
Observations	4,034	4,034	4,034	4,034
Adj. R-squared	0.85	0.86	0.80	0.81
First Stage Coefficients				
Worst Originators' Mkt. Share in 2002	1.156**	0.777**		
	(2.57)	(2.23)		
Worst Originators' Presence in 2002	0.00387***	0.00179**		
	(3.65)	(2.04)		
Observations	4,034	4,034		
Adj. R-squared	0.727	0.804		
F-statistic	51.89	13.80		

Table 2.4: Effect of worst originator activity on house returns – IV 2

This table shows the results of a two-stage estimation procedure via 2SLS. First, the endogenous variable, *Worst Originators' Mkt. Share* is regressed on the loan application rejection rate from 1996 to 1999 (the instrument), the market share of the best and medium originators, and a set of controls (depending on the specification). Then, zip code price returns are regressed on the zip code-level market share for various types of originators during the period from 2003 to 2006. In particular, *Worst Originators' Mkt. Share* is the fitted value of the regression in the first stage. The regressions include different combinations of demographic controls and MSA fixed effects. Columns 1 to 2 show the results for the boom period (2003-2006) and columns 3 to 4 show the results for the bust period (2007-2012). Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by MSA. The instrument's coefficient in the first stage regression, along with the F-statistic are shown in the bottom of the table. ***p<0.01, **p<0.05, *p<0.1.

	2003-2006		2007-2012	
Worst Originators' Mkt. Share	3.909***	3.887***	-3.205***	-3.420***
	(6.03)	(3.39)	(-6.12)	(-3.88)
Medium Originators' Mkt. Share	-0.450***	-0.262*	0.0431	0.00942
	(-3.09)	(-1.75)	(0.42)	(0.11)
Best Originators' Mkt. Share	-0.388	-0.478	0.607	0.557
	(-0.72)	(-0.97)	(1.57)	(1.32)
Controls	N	Y	N	Y
MSA FE	Y	Y	Y	Y
Observations	3,939	3,939	3,939	3,939
Adj. R-squared	0.76	0.78	0.69	0.69
First Stage Coefficients				
Rejection Rate from 1996 to 1999	0.219***	0.110***		
	(6.46)	(5.51)		
Observations	3,939	3,939		
Adj. R-squared	0.66	0.764		
F-statistic	41.71	30.35		

Table 2.5: Effect of APLs on house price movements and loan supply by the worst originators

This table shows the effect of anti-predatory lending laws on house price movements and loan supply by the worst originators, during the boom period (2003-2006). In the first two columns, the zip codes included are in states that passed anti-predatory lending laws (APLs) between 2004 and 2005 or in states that did not pass any APLs before 2006. In the last two columns, the sample is restricted to the half of zip codes with the largest average loan supply by the worst originators. The variable *Post Law* takes the value of one after the quarter where an APL was passed, and zero otherwise. All regressions include quarter fixed effects. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by CBSA. ***p<0.01, **p<0.05, *p<0.1.

	All Zip Codes		High Worst Orig. Supply	
	House Returns	Supply	House Returns	Supply
Post Law	-0.024***	-0.021***	-0.035***	-0.051***
	(-4.31)	(-3.97)	(-7.03)	(-7.37)
Fraction Securitized	0.035	0.221***	0.017	0.226***
	(1.72)	(6.05)	(1.55)	(6.58)
Population	0.000	0.001*	0.000	0.001
	(0.24)	(1.88)	(1.56)	(1.17)
Housing Units	0.000	-0.002	-0.001*	-0.003
	(0.25)	(-1.62)	(-1.90)	(-1.24)
Housing Vacancy Rate	0.032*	0.043*	0.042**	-0.004
	(1.86)	(2.06)	(2.61)	(-0.16)
Average Household Income	-0.000	-0.000***	0.000	-0.000***
	(-1.55)	(-4.50)	(1.57)	(-5.92)
Constant	0.007	0.004	0.006	0.019**
	(1.17)	(0.96)	(0.84)	(2.61)
Quarter FE	Y	Y	Y	Y
Observations	17,162	17,000	8,710	8,880
Adj. R-squared	0.266	0.396	0.373	0.299

Table 2.6: Proportion peaks

This table compares the proportion of zip codes where loan supply by the worst originators peaked before the zip code house price with the proportion of zip codes where loan supply by the best originators peaked before the zip code house price. The z-statistic of a proportion test is reported in the last column.

	Number of zip codes	Pct. of zip codes where supply by worst originators peaked before house price peak	Pct. of zip codes where supply by best originators peaked before house price peak	Difference in proportions	z-statistic
All house price peak-years	804	90.0	65.8	24.3	11.72
House price peaks in 2005	159	80.5	69.8	10.7	2.21
House price peaks in 2006	376	87.0	62.2	24.7	7.79
House price peaks in 2007	269	100.0	68.4	31.6	10.05

Table 2.7: Relative house price drop difference between run-up matched ZIP codes

This table shows OLS estimates for the specification,

$$\begin{aligned} return = \beta_0 + \beta_1(worst\ orig.\ zips > 10\% \times post2006) + \\ \beta_2(worst\ orig.\ zips > 10\%) + \beta_3 post2006 + X\Gamma + \epsilon, \end{aligned}$$

where *return* is a vector with house returns, *worst orig. zips > 10%* is an indicator that identifies the 858 zip codes in the first group graphed in Panel A of Figure 2.4, *post2006* is a dummy variable that takes the value of one if the date of the price corresponds to the year 2007 or later, and zero otherwise, and X is a set of control variables that includes the fraction of loans securitized at the zip code level during the period 2003 to 2006, zip code population, number of house units, vacancy rate (all in the year 2000), average household income in 2001, and average household income changes from 2001 to 2006. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by MSA. ***p<0.01, **p<0.05, *p<0.1.

	2003-2012	
Worst Orig. Zips > 10% × Post2006	-0.144***	-0.146***
	(-6.74)	(-6.57)
Post2006	-0.680***	-0.681***
	(-12.53)	(-12.43)
Worst Orig. Zips > 10%	0.030***	-0.006
	(2.94)	(-0.57)
Fraction Securitized		0.657***
		(3.95)
Population		-0.002
		(-1.32)
Housing Units		0.007
		(1.68)
Housing Vacancy Rate		0.185
		(1.68)
Average Household Income		-0.001***
		(-3.21)
Δ in Avg. Household Income		0.002***
		(3.62)
Constant	0.477***	0.390***
	(9.50)	(7.03)
Observations	1,472	1,435
Adj. R-squared	0.78	0.79

Table 2.8: Effect of worst originator activity in elastic and inelastic ZIP codes during the bust

This table shows OLS estimates for regressions where zip code house price returns during the bust is the dependent variable, on the zip code-level market share for various types of originators during the period from 2003 to 2006, for different subsamples of zip codes based on housing supply elasticities from Saiz (2008). The regressions include different combinations of demographic controls and MSA fixed effects. Column 1 shows the estimates for the zip codes in MSAs in the most elastic half. Column 2 shows the regression for zip codes in MSAs in the most elastic quartile. Column 3 considers the most inelastic half, and column 4 the most inelastic quartile. Reported *t*-statistics in parentheses are heteroskedasticity-robust and clustered by MSA. ***p<0.01, **p<0.05, *p<0.1.

	Elastic MSAs		Inelastic MSAs	
	2007-2012		2007-2012	
	Top 50%	Top 25%	Bottom 50%	Bottom 25%
Worst Originators' Mkt. Share	-1.809***	-2.400***	-1.268***	-1.265**
	(-5.73)	(-7.24)	(-2.99)	(-2.78)
Medium Originators' Mkt. Share	-0.070	-0.161	0.076	0.042
	(-0.52)	(-0.88)	(0.56)	(0.23)
Best Originators' Mkt. Share	-0.078	-0.544***	0.903***	1.127***
	(-0.26)	(-3.38)	(3.49)	(4.88)
Fraction Securitized	-0.187	-0.177	-0.230***	-0.230**
	(-1.58)	(-1.61)	(-2.99)	(-2.58)
Population	-0.004***	0.000	-0.001***	-0.001**
	(-2.88)	(0.01)	(-2.98)	(-2.43)
Housing Units	0.010***	-0.001	0.005***	0.005***
	(2.79)	(-0.21)	(4.01)	(3.33)
Housing Vacancy Rate	0.017	-0.094	-0.237***	-0.237***
	(0.15)	(-0.63)	(-4.55)	(-3.60)
Average Household Income	0.001	0.001	0.000***	0.000*
	(1.59)	(1.29)	(4.63)	(2.07)
Δ in Avg. Household Income	0.000	-0.000	0.000***	0.000
	(0.58)	(-0.52)	(2.90)	(1.27)
Constant	-0.062	0.061**	-0.292***	-0.336***
	(-1.66)	(2.29)	(-6.02)	(-6.35)
MSA FE	Y	Y	Y	Y
Observations	1,796	633	2,871	2,111
Adj. R-squared	0.67	0.67	0.76	0.70

Table 2.9: Explanatory power of loan-level controls

This table shows OLS loan-level regressions where the dependent variable is an indicator for whether the loan became 90 days or more delinquent and the explanatory variables are a set of loan characteristics. We also include the interaction of a dummy variable for worst lender with each one of the controls and ZIP Code interacted with year of origination fixed effects. Columns 1 through 3 shows the results for the full sample of loans. Columns 4 through 6 show the results for a sample where, for each loan issued by a bad originator, we find another loan issued by a good originator in the same ZIP code-year that also has similar propensity score. To compute the propensity score, we estimate a logit regression where the dependent variable is a dummy that takes the value of one if the loan was issued by one of the worst originators and takes the value of zero if the loan was issued by one of the best originators, and the explanatory variables are combined LTV, credit score, interest rate, the log of the loan amount, and indicators for low-doc, non-owner occupied property, arm loan, and the existence of a prepayment penalty. Also, we impose a maximum distance between propensity scores of 1%. We are able to impose such a tight criteria because there are many more loans from the better originators and we match with replacement up to a maximum of five times. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by CBSA. ***p<0.01, **p<0.05, *p<0.1.

		All Loans			Matched Loans	
CLTV	0.630***		0.609***	0.785***		0.766***
	(38.86)		(33.52)	(32.96)		(31.67)
CLTV×Worst	-0.189***		-0.253***	-0.367***		-0.390***
	(-10.07)		(-13.83)	(-14.72)		(-14.82)
Full-Doc	-8.581***		-8.538***	-9.986***		-9.912***
	(-20.03)		(-20.30)	(-12.42)		(-12.47)
Full-Doc×Worst	1.377**		1.400**	2.090***		2.187***
	(2.14)		(2.32)	(3.58)		(3.77)
Interest Rate		2.688***	0.851***		3.103***	1.024***
		(7.42)	(5.11)		(9.67)	(5.83)
Interest Rate×Worst		1.445***	0.978***		1.066***	0.428***
		(15.52)	(9.47)		(11.33)	(2.83)
Non-owner Occupied	3.003***		2.900***	0.619		0.444
	(4.94)		(5.02)	(0.61)		(0.44)
Non-owner Occ.×Worst	-2.881***		-2.736***	-0.796		-0.600
	(-3.69)		(-3.42)	(-1.03)		(-0.76)
Credit Score	-0.154***		-0.145***	-0.168***		-0.157***
	(-26.69)		(-33.98)	(-18.34)		(-18.05)
Credit Score×Worst	-0.012**		0.003	0.007		0.011**
	(-2.14)		(0.52)	(1.46)		(2.16)
ln(Loan Amount)	3.628***		4.128***	5.458***		6.092***
	(9.32)		(9.79)	(7.24)		(7.71)
ln(Loan Amount)×Worst	2.781***		1.813***	2.961***		2.631***
	(9.48)		(6.48)	(8.68)		(7.23)
ARM	0.962***		1.391***	0.814		1.244**
	(4.05)		(5.51)	(1.30)		(2.00)
ARM×Worst	4.137***		3.603***	3.106***		3.341***
	(7.26)		(5.76)	(3.87)		(4.11)
Prepayment Penalty	6.798***		7.204***	10.207***		10.539***
	(21.72)		(23.55)	(12.69)		(13.00)
Prepayment Pen.×Worst	-2.218***		-2.516***	-4.921***		-4.912***
	(-3.82)		(-4.50)	(-5.83)		(-5.75)
Constant	41.852***	18.647***	25.054***	28.347***	32.294***	7.386
	(7.95)	(7.79)	(4.86)	(4.00)	(14.22)	(0.95)
ZIPxYear FE	Y	Y	Y	Y	Y	Y
Observations	932,236	932,236	932,236	173,644	173,644	173,644
Adj. R-squared	0.30	0.24	0.30	0.278	0.212	0.279

Table 2.10: Unmet demand and market share

This table shows the relation between the market share of the different types of originators and ZIP code-level loan rejection rates (unmet demand). Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by MSA. ***p<0.01, **p<0.05, *p<0.1.

	Worst Originators' Mkt. Share		Medium Originators' Mkt. Share		Best Originators' Mkt. Share	
Unmet Demand	0.161***	0.110***	-0.045***	-0.036*	-0.121***	-0.009
	(19.41)	(5.47)	(-3.76)	(-1.83)	(-13.10)	(-0.56)
Fraction Securitized		0.187***		0.126***		0.040**
		(10.57)		(5.94)		(2.07)
Population		0.000***		-0.000**		-0.000***
		(7.56)		(-2.49)		(-5.54)
Housing Units		-0.000***		0.000***		0.000***
		(-6.84)		(2.76)		(5.63)
Housing Vacancy Rate		0.000		-0.000		-0.000
		(1.17)		(-1.18)		(-0.36)
Avg. Household Income		-0.000***		0.000***		0.000***
		(-5.54)		(2.73)		(4.21)
Δ in Avg. Household Income		0.000		-0.000		0.000
		(0.82)		(-1.08)		(0.44)
Constant	0.030***	0.024***	0.179***	0.147***	0.131***	0.057***
	(20.26)	(6.26)	(83.42)	(30.98)	(79.54)	(15.64)
Observations	3,939	3,939	3,939	3,939	3,939	3,939
Adj. R-squared	0.09	0.76	0.00	0.65	0.04	0.65

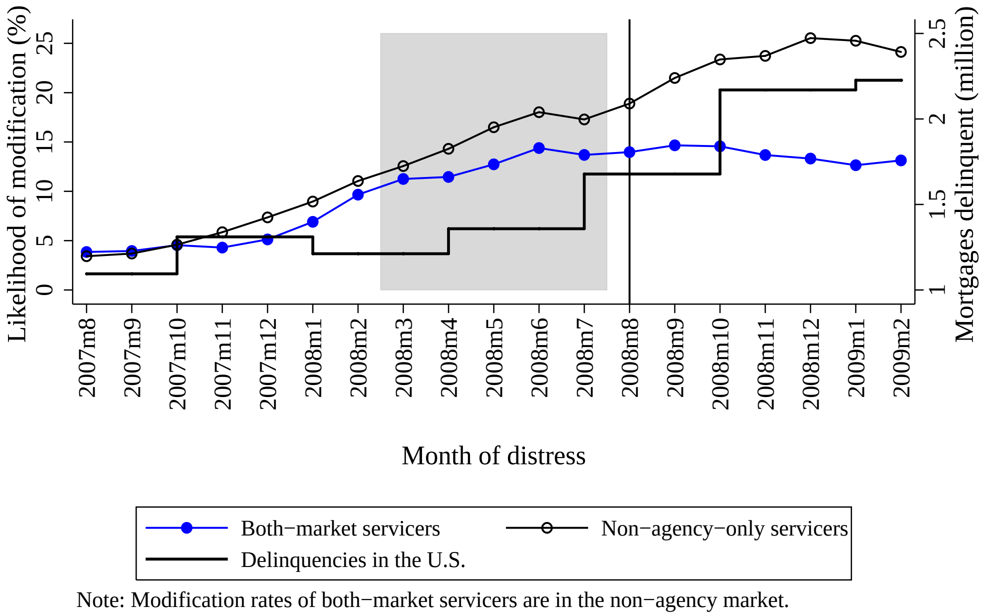


Figure 1.1: Non-agency loan modifications by servicer type

This figure shows the likelihood of modification within six months of the loans becoming distressed (60+ days delinquent or modified), by month. Loans are categorized by the type of servicer holding the servicing rights at the time the loan became distressed. The first group, "both-market servicers," includes servicers which manage loans both from government-sponsored enterprises (GSEs) mortgage-backed securities and from non-agency mortgage-backed securities. The second group, "non-agency-only servicers," includes servicers which mostly manage non-agency loans. The black line shows the total number of mortgages 60+ days delinquent in the U.S. (foreclosure initiations are not included). The vertical line indicates the month when Fannie Mae and Freddie Mac increased modification incentives in the GSE market. The gray area delimits when the modification rate starts being affected by the incentive fee. U.S.-level figures are calculated from the Mortgage Bankers Association's National Delinquency Survey.

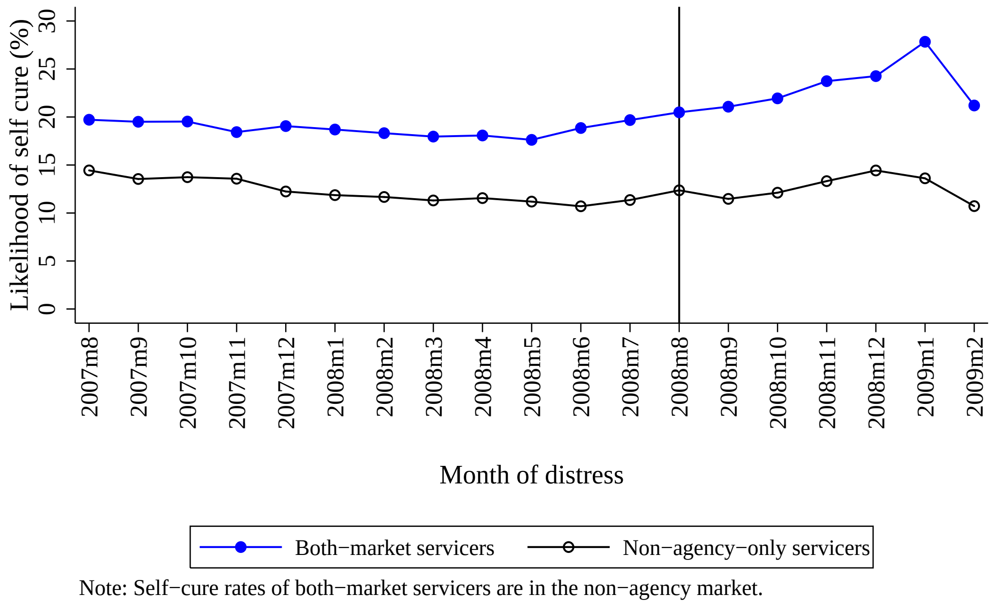
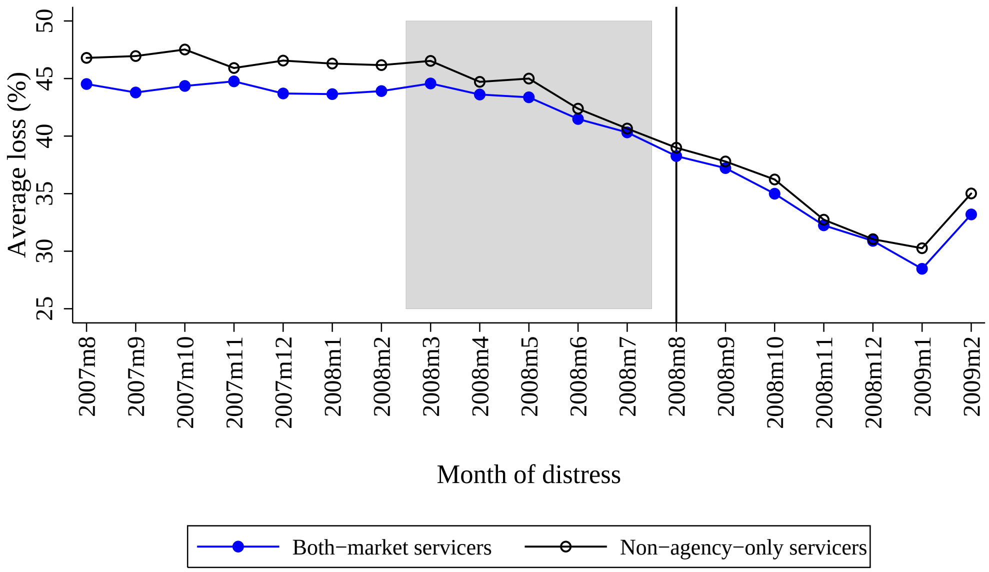


Figure 1.2: Non-agency self-cure rates by servicer type

This figure shows the likelihood of self-cure by month of distress. Loans are categorized by the type of servicer holding the servicing rights at the time the loan became distressed. The first group, "both-market servicers," includes servicers which manage loans both from government-sponsored enterprises (GSEs) mortgage-backed securities and from non-agency mortgage-backed securities. The second group, "non-agency-only servicers," includes servicers which mostly manage non-agency loans. The vertical line indicates the month when Fannie Mae and Freddie Mac increased modification incentives in the GSE market.



Note: Losses of both−market servicers are in the non−agency market.

Figure 1.3: Losses of non-agency loans by servicer type

This figure shows the average losses of the two servicer types in the loan sample, by month of distress (60+ days delinquent or modified). Loans are categorized by the type of servicer holding the servicing rights at the time the loan became distressed. The first group, "both-market servicers," includes servicers which manage loans both from government-sponsored enterprises (GSEs) mortgage-backed securities and from non-agency mortgage-backed securities. The second group, "non-agency-only servicers," includes servicers which mostly manage non-agency loans. The vertical line indicates the month when Fannie Mae and Freddie Mac increased modification incentives in the GSE market. The gray area delimits when the modification rate starts being affected by the incentive fee.

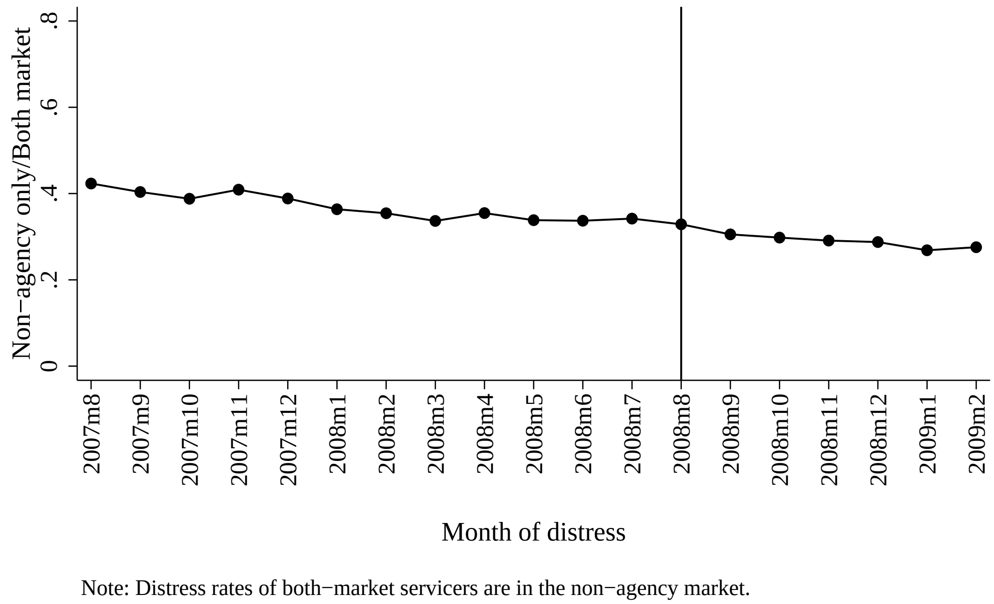


Figure 1.4: Relative difference in distressed loans across servicer types
This figure shows the ratio between the number of distressed loans in the sample by the two types of servicers. Each month the number of distressed loans from non-agency-only servicers is divided by the number of distressed loans from both-market servicers. The vertical line indicates the month when Fannie Mae and Freddie Mac increased modification incentives in the GSE market.

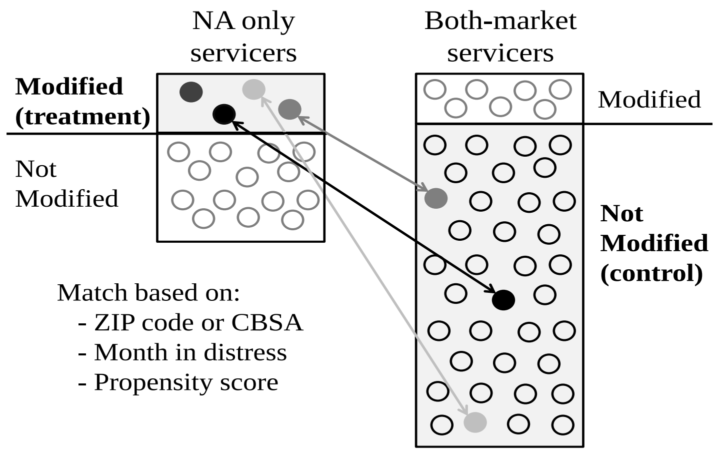


Figure 1.5: Matching strategy schematization

This figure shows a diagram illustrating the matching strategy. The intuition is to match a loan modified by non-agency-only servicers (treatment) with a very similar not-modified loan from both-market servicers (control) which arguably would have been modified had the incentive fee not existed, and compare their ex post losses. The matching is performed based on the month the loan became distressed, on ZIP code or CBSA, and on propensity scores based in a large set of loan characteristics.

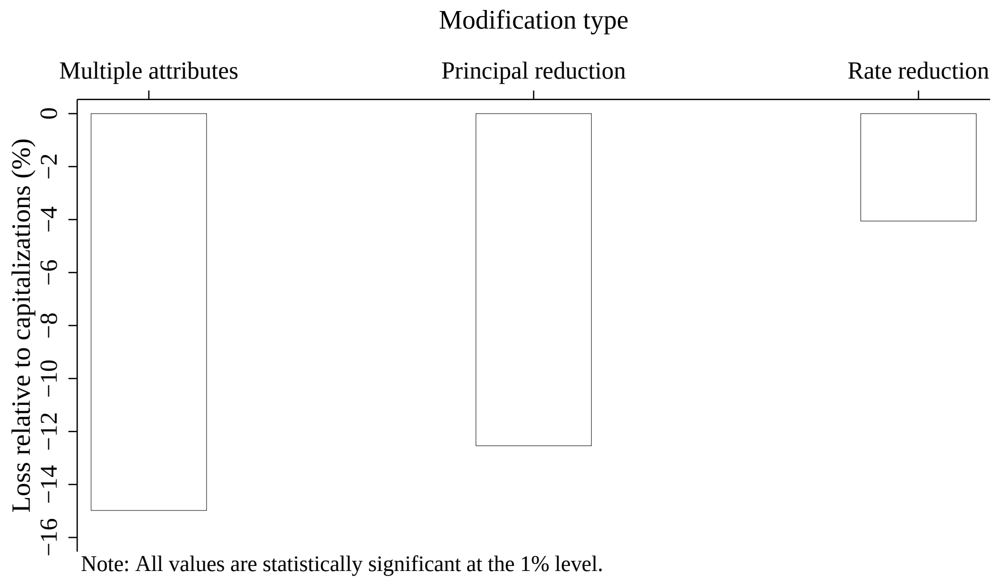


Figure 1.6: Effect of modification type on loan losses

This figure shows the effect of modification type on loan losses, relatively to the effect of capitalizations. An OLS regression where the dependent variable is the net loan loss and the explanatory variables of interest are three indicators for whether the modification (executed within six months from the loan becoming distressed) is a principal reduction, an interest rate reduction, and for whether more than one attribute was modified. Loan-level controls and CBSA-month of origination, servicer, and month of distress fixed effects are also included. All estimates are in percentage terms. Standard errors are heteroskedasticity-robust and clustered by CSA. The bars plot the coefficients associated with each modification type indicator.

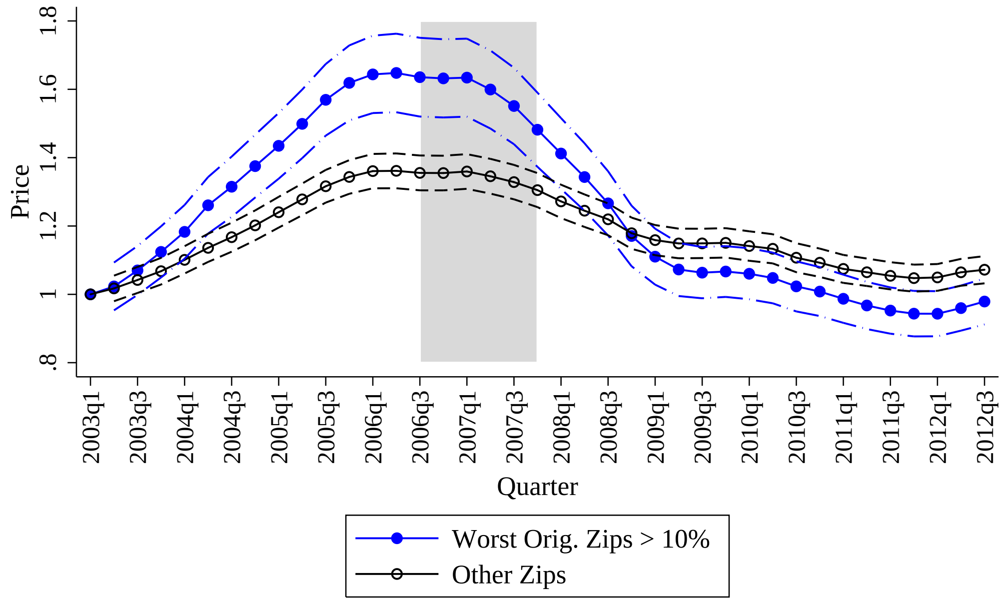


Figure 2.1: House price movements and worst originators' market share

This figure shows the relation between activity of the worst originators and house prices. Zip codes are divided into two groups: those where the average market share of the worst originators during the period 2004q3-2006q2 (highlighted by the yellow shaded area) exceeds 10% (blue solid circles) and the other remaining zip codes (black hollow circles). The gray shaded area highlights the period when most of the worst originators went bankrupt or lost considerable business. Dashed lines show the 95% confidence interval for house price.

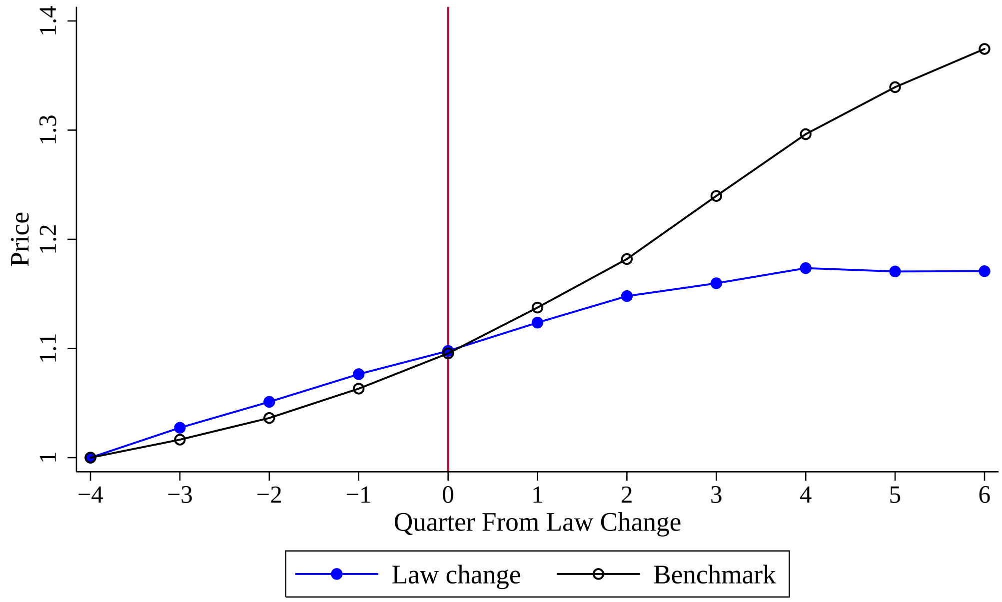


Figure 2.2: House price movements before and after APLs

This figure compares the house price movements of zip codes in states that passed anti-predatory lending laws (APLs) between 2004 and 2005 (blue circles) with the house price movements of a benchmark of zip codes in states that did not pass any APLs before 2006 (black hollow circles), before and after the law changes. The set of states that implemented APLs in 2004 and 2005 are New Mexico (Q1 of 2004), South Carolina(Q1 of 2004), Massachusetts (Q3 of 2004), Indiana (Q1 of 2005), and Wisconsin (Q1 of 2005). The set of states with no APLs are Arizona, Delaware, New Hampshire, Montana, Oregon, Washington, and Tennessee.

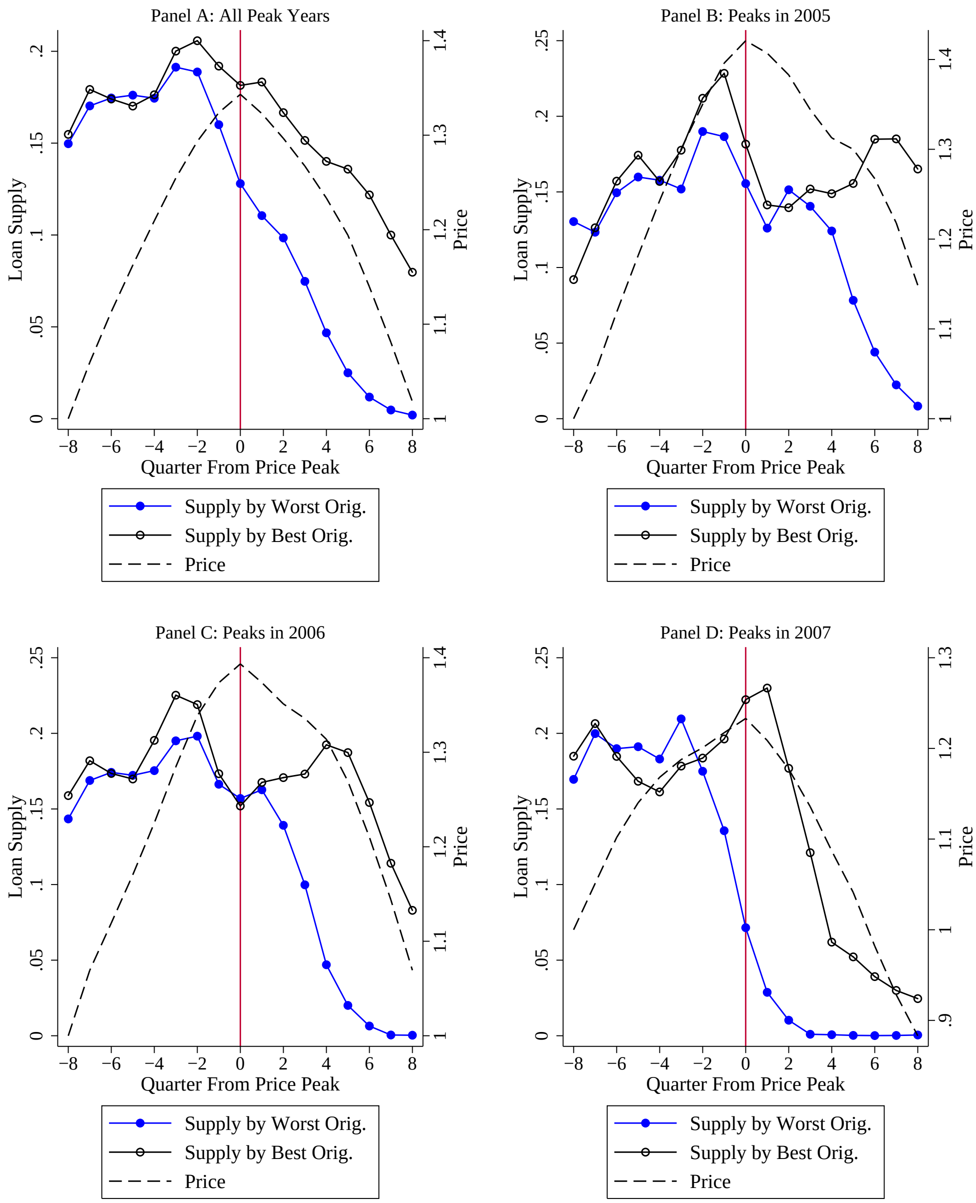


Figure 2.3: Loan supply and house price peaks

This figure shows the zip code-quarterly loan supply by the worst (blue circles) and best (black hollow circles) originators around zip code-house price peaks. Zip codes with an average market share of the worst originators during the period 2004q3-2006q2 exceeding 10% where house prices peaked between 2005 and 2007 are included. The dashed lines represent house price movements. Panel A shows all zip codes and Panels B, C, and D, show zip codes where house prices peaked in 2005, 2006, and 2007, respectively.

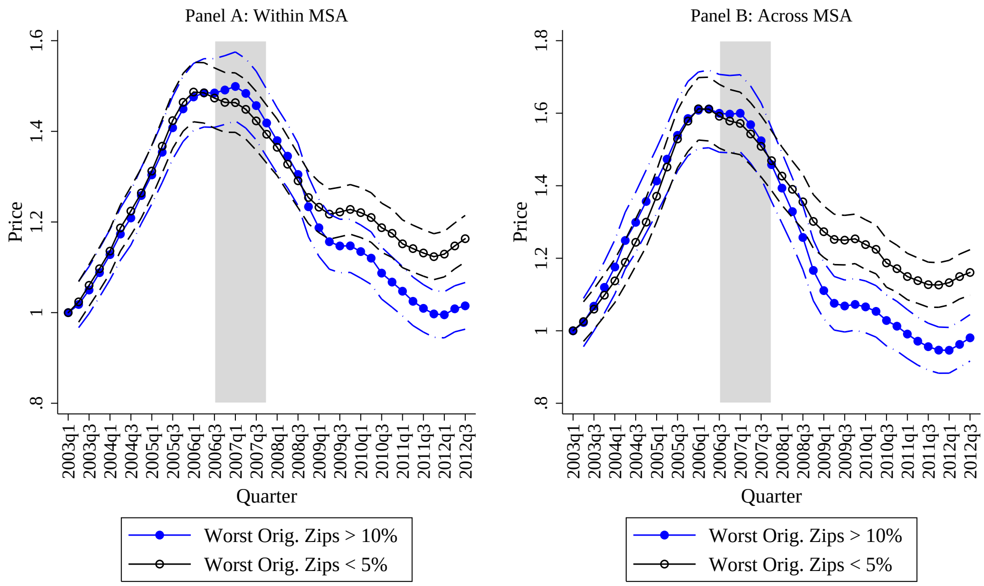


Figure 2.4: House price movements (run-up matching)

This figure compares the average house price movement of zip codes where the average market share of the worst originators during the period 2004q3-2006q2 exceeds 10% (blue solid circles) with the average house price movement of a group of zip codes that show an average market share of the worst originators below 5% during the same period (black hollow circles). The control group is constructed to match the house returns of the group with high activity of the worst originators during the run-up period as closely as possible (matching is done with replacement and ZIP codes are allowed to be matched a maximum of five times). In Panel A, zip codes in the control group are also required to be in the same MSA as the zip codes with high activity of the worst originators. In Panel B, matching is done across MSAs. The gray shaded area highlights the period when most of the worst originators went bankrupt or lost considerable business. Dashed lines represent the 95% confidence interval for house price.

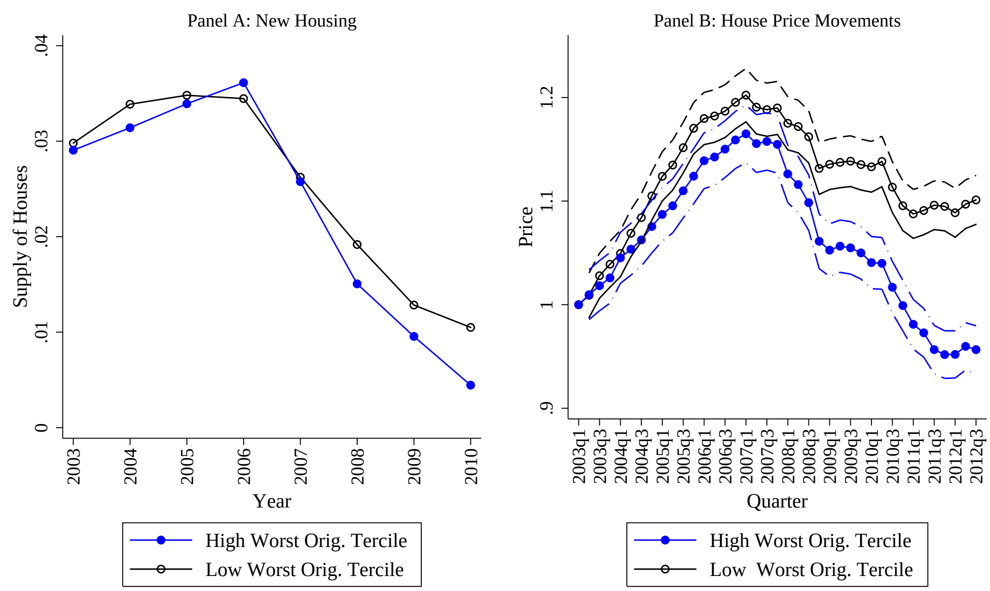


Figure 2.5: New houses and price movements in elastic ZIP codes

This figure shows the new housing supply and house price changes in elastic zip codes (zip codes in the 25% of more elastic MSAs). Panel A, shows the zip code-average of new house transactions as a fraction of total houses in 2002 for the zip codes in the highest tercile of worst originators' market share (blue circles) and the lowest tercile of worst originators' market share (black hollow circles). Panel B shows the average price changes for the same groups. Dashed lines represent the 95% confidence interval for house price.

Panel A

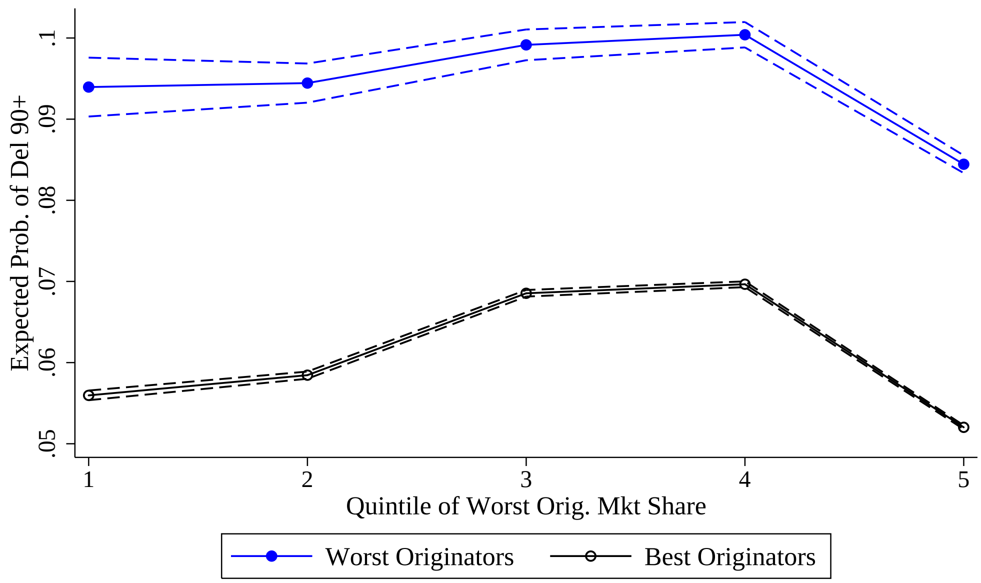
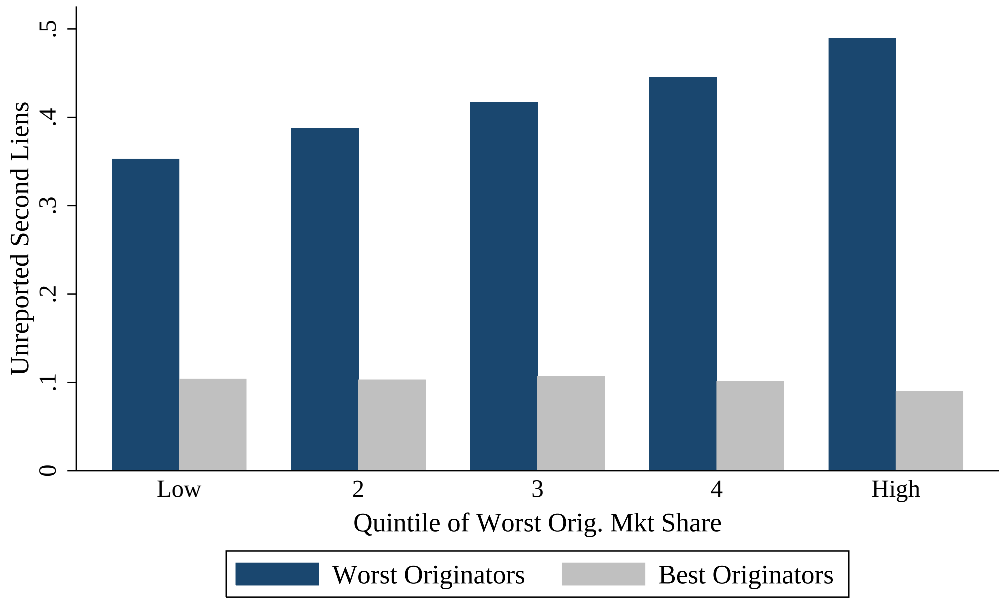


Figure 2.6: Worst and best originator quality comparison

This figure shows a comparison of the quality of the loans originated by the worst and best originators in our sample. Panel A shows the percentage of second-lien misreporting exhibited by loans issued by the worst and best originators, by worst originators' market share quintile. Panel B shows the average expected probability of delinquency (90+ days) exhibited by loans issued by the worst and best originators, by worst originators' market share quintile. The expected probability of delinquency is obtained by fitting a logit model at the beginning of 2002 using all first-lien loans originated before 2001 in ABSNet. More specifically, the dependent variable is a dummy that takes the value of one if the loan became delinquent before 2002, and zero otherwise. The set of explanatory variables includes credit score, combined loan-to-value ratio, interest rate, the log of the loan amount, and dummy variables for level of documentation (low/no-doc or full-doc), self-reported occupancy status, refinance, and the existence of a prepayment penalty.

Figure 2.6 - **continued**

Panel B



Appendices

Appendix A

Relevant events for determining the servicer when a loan becomes distressed

Jul 2006. Centex Home Equity becomes Nationstar Mortgage.

Dec 2006. Merrill Lynch acquires First Franklin (from National City).

May 2007. Carrington acquires servicing rights from New Century.

Sep 2007. CitiMortgage acquires ACM Mortgage Services (which also owns Argent and Ameriquest).

Apr 2008. JP Morgan acquires Bear Sterns (which also owns EMC Mortgage).

Apr 2008. American Home Mortgage acquires Option One.

Jul 2008. Bank of America acquires Countrywide.

Sep 2008. JP Morgan acquires Washington Mutual.

Sep 2008. PNC Mortgage acquires National City.

Sep 2008. Barclays acquires Lehman Brothers (Aurora Loan Services later transfers servicing rights to Nationstar on Jul 2012).

Oct 2008. Bank of America acquires Merrill Lynch.

Dec 2008. Bank of America acquires GreenPoint Mortgage.

Jan 2009. Wells Fargo acquires Wachovia.

Mar 2009. OneWest Bank acquires IndyMac.

Mar 2010. IBM acquires Wilshire Credit Corporation.

Sep 2010. Ocwen acquires HomeEq Servicing (from Barclays).

Sep 2011. Ocwen acquires Litton Loan Servicing (from Goldman Sachs).

Appendix B

Supplementary tables and figures

Table B.1: The effect of the incentive fee on modification rates - Robustness
This table shows the robustness of the results in Table 1.3. Columns 1 and 2 show the same regression than Column 1 of Table 1.3 excluding from the sample loans serviced by Bank of America (the largest servicer in the sample) and excluding the loans originated in California (the largest state). Column 3 shows the results of a falsification test where the incentive fee is assumed to start in January 2008 and the period in which the regression is estimated goes from August 2007 to July 2008 (just before the incentive fee was implemented). All estimates are in percentage terms. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
	Excluding BOA	Excluding California	Falsification test
Both Markets×After Fee	-6.05***	-5.98***	-1.00***
	(-14.49)	(-13.79)	(-4.48)
Credit Score	0.04	-0.51***	0.04
	(0.58)	(-5.88)	(0.42)
CLTV	0.24**	0.85***	0.37***
	(2.14)	(7.40)	(4.54)
Interest Rate	3.97***	3.49***	3.53***
	(25.63)	(25.07)	(18.85)
Unpaid Balance	0.67***	0.45***	0.59***
	(6.72)	(4.65)	(5.96)
Adjustable	3.00***	4.55***	3.02***
	(13.42)	(15.18)	(11.83)
Non-Owner Occupied	-4.79***	-4.88***	-3.76***
	(-31.07)	(-35.67)	(-23.91)
Low/No-Doc	-3.17***	-2.99***	-2.83***
	(-19.62)	(-12.99)	(-18.36)
Prepayment Penalty	3.32***	3.75***	3.14***
	(19.74)	(14.13)	(15.19)
CBSA×Origination month FE	Y	Y	Y
Servicer FE	Y	Y	Y
Distress month FE	Y	Y	Y
Observations	858,966	748,383	638,186
Adj. R^2	0.09	0.09	0.08

Table B.2: The effect of the incentive fee on modification rates - Additional falsification test

This table shows an additional falsification test to show the robustness of the results in Table 1.3. The difference-in-differences estimation in Table 1.3 is repeated using each original loan-level control variable as the dependent variable. The coefficient on the variable *Both Markets*×*After Fee* (i.e., the effect of the incentive fee) is reported (t-statistics are heteroskedasticity-robust and clustered by CSA).

Dependent variable	Mean value of variable	Effect of the incentive fee	t-statistic	Effect relative to the mean (%)
Credit Score	649.34	1.37	3.39	0.2
CLTV (%)	86.03	0.13	1.35	0.2
Interest Rate (%)	7.82	0.03	2.10	0.4
Unpaid Balance ($)	258,637.40	4,534.14	4.94	1.8
Adjustable (%)	71.86	-3.01	-9.13	-4.2
Non-Owner Occupied (%)	14.44	-0.32	-1.47	-2.2
Low/No-Doc (%)	57.05	1.81	3.94	3.2
Prepayment Penalty (%)	51.53	-3.82	-6.25	-7.4

Table B.3: OLS regression of loan losses on modification with alternative set of fixed effects

This table shows OLS estimates of regressions similar to the ones in Table 1.5, but with a CBSA and month of origination fixed effects included separately (not interacted). The dependent variable is the net loan loss, which is defined as losses minus recoveries, divided by the outstanding principal amount at the time of becoming distressed. Losses of modified loans incorporate any concessions made to the borrower. *Modification* is an indicator that takes the value of one if the loan was modified within six months of becoming distressed, and zero otherwise. Continuous control variables are standardized and the regression's intercept is not reported. All estimates are in percentage terms. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
	Full Period Aug07-Feb09	Before fee Aug07-Jul08	After fee Aug08-Feb09
Modification	-5.97***	-3.70***	-8.11***
	(-15.75)	(-8.22)	(-19.28)
Credit Score	1.56***	1.26***	2.06***
	(10.32)	(6.63)	(13.60)
CLTV	5.21***	5.56***	4.62***
	(18.34)	(22.61)	(11.48)
Interest Rate	-1.04***	-0.93***	-1.37***
	(-5.13)	(-4.88)	(-5.12)
Unpaid Balance	-5.67***	-6.56***	-4.55***
	(-12.43)	(-12.21)	(-12.11)
Adjustable	4.49***	5.11***	3.56***
	(16.52)	(19.81)	(11.02)
Non-Owner Occupied	15.35***	14.80***	16.18***
	(9.75)	(8.48)	(12.13)
Low/No-Doc	2.39***	3.09***	1.13***
	(7.00)	(8.10)	(4.06)
Prepayment Penalty	1.32***	1.60***	0.93***
	(5.47)	(5.18)	(4.62)
CBSA FE	Y	Y	Y
Origination month FE	Y	Y	Y
Servicer FE	Y	Y	Y
Distress month FE	Y	Y	Y
Observations	999,067	638,186	360,881
Adj. R^2	0.21	0.21	0.20

Table B.4: Robustness tests for Table 1.6

This table shows the results of instrumental variable regressions estimated using the standard two-stage least squares (2SLS) procedure. Bank of America (the largest servicer) or California (the largest state) are omitted from the sample. In the first stage (Columns 1 and 2) the incentive fee introduction is used as an instrument for modification. In the second stage (Columns 3 and 4) the fitted value from the first stage is the main explanatory variable. Loan-level controls and a variety of fixed effects are also included. Continuous control variables are standardized and the regression's intercept is not reported. All estimates are in percentage terms. Reported t-statistics (for the first stage) and z-statistics (for the second stage) in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
	First stage		Second stage	
	Excluding BOA	Excluding California	Excluding BOA	Excluding California
Modification			-10.21**	-9.30*
			(-2.16)	(-1.83)
Both Markets×After Fee	-6.07***	-6.00***		
	(-15.07)	(-14.40)		
Credit Score	0.04	-0.53***	1.54***	1.65***
	(0.60)	(-6.58)	(10.03)	(8.75)
CLTV	0.24**	0.85***	5.08***	4.84***
	(2.24)	(7.66)	(18.89)	(16.31)
Interest Rate	3.92***	3.43***	-1.11***	-0.68***
	(25.74)	(26.18)	(-5.17)	(-2.88)
Unpaid Balance	0.69***	0.49***	-6.09***	-5.07***
	(6.82)	(4.99)	(-12.63)	(-10.59)
Adjustable	3.10***	4.64***	4.47***	4.49***
	(14.50)	(16.04)	(11.55)	(10.47)
Non-Owner Occupied	-4.76***	-4.87***	14.87***	16.21***
	(-31.57)	(-35.63)	(8.80)	(8.17)
Low/No-Doc	-3.11***	-2.89***	2.16***	2.08***
	(-18.61)	(-12.43)	(5.09)	(3.97)
Prepayment Penalty	3.22***	3.65***	0.99***	1.21***
	(18.83)	(14.32)	(3.40)	(2.99)
Origination month FE	Y	Y	Y	Y
CBSA FE	Y	Y	Y	Y
Servicer FE	Y	Y	Y	Y
Distress month FE	Y	Y	Y	Y
Observations	858,966	748,383	858,966	748,383
Adj. R^2	0.09	0.09	0.22	0.21
F-statistic	227.0	207.3		

Table B.5: First stage of Table 1.7

This table shows the first stage regressions of the second stage regressions shown in Table 1.7. The loans in the large house price drop group in Table 1.7 are ranked into three groups based on their ZIP code house price rebound (from the bottom of 2009 o September 2012). Column 1 shows the results of the IV estimation on the loans with returns equal or greater than zero (no house price drop). Two other groups are formed based on the median return of the remaining loans. Column 2 considers the loans that experienced a small house price drop while Column 3 considers the loans that experienced a large house price drop. All estimates are in percentage terms. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
	No housing price drop	Small housing price drop	Large housing price drop
Both Markets×After Fee	-6.19***	-5.76***	-4.59***
	(-6.71)	(-11.65)	(-20.34)
Credit Score	-0.69***	-0.95***	-0.29**
	(-2.66)	(-8.94)	(-2.41)
CLTV	1.14***	0.94***	0.26***
	(6.56)	(7.90)	(4.02)
Interest Rate	2.03***	3.73***	4.01***
	(5.15)	(19.63)	(23.55)
Unpaid Balance	0.12	0.26**	0.50***
	(0.51)	(2.42)	(3.88)
Adjustable	7.56***	4.79***	3.58***
	(9.76)	(16.36)	(12.39)
Non-Owner Occupied	-5.33***	-5.14***	-4.40***
	(-9.04)	(-32.41)	(-20.06)
Low/No-Doc	-1.57***	-3.23***	-3.51***
	(-3.08)	(-11.35)	(-16.34)
Prepayment Penalty	4.67***	4.85***	3.74***
	(9.26)	(11.52)	(13.86)
Origination month FE	Y	Y	Y
CBSA FE	Y	Y	Y
Servicer FE	Y	Y	Y
Distress month FE	Y	Y	Y
Observations	21,616	451,936	451,948
Adj. R^2	0.10	0.09	0.09
F-statistic	45.0	135.6	413.6

Table B.6: Robustness test for Table 1.7

This table shows the second stage of instrumental variable regressions estimated using the standard two-stage least squares (2SLS) procedure. The loans in the sample are divided into three groups based on their short-run house price returns computed using a different criteria than in Table 1.7. Here house price returns are computed from March 2009 to March 2010 (to ensure that returns do not affect the modification decision). Column 1 shows the results of the IV estimation on the loans with returns equal or greater than zero (no house price drop). Two other groups are formed based on the median return of the remaining loans. Column 2 considers the loans that experienced a small house price drop while Column 3 considers the loans that experienced a large house price drop. All estimates are in percentage terms. Reported z-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
	No housing price drop	Small housing price drop	Large housing price drop
Modification	-12.29	-11.63*	-22.28***
	(-1.40)	(-1.82)	(-2.83)
Loan-level controls	Y	Y	Y
Origination month FE	Y	Y	Y
CBSA FE	Y	Y	Y
Servicer FE	Y	Y	Y
Distress month FE	Y	Y	Y
Observations	272,732	326,939	327,616
Adj. R^2	0.18	0.21	0.17
First stage coefficient	-5.88***	-6.28***	-5.15***
	(-9.88)	(-11.73)	(-18.78)
F-statistic	97.6	137.6	352.9
Mean price drop (%)	4.0	-3.6	-16.0
Self-cure rate (%)	19.4	19.9	15.1
Redefault rate (%)	55.9	56.5	57.3
Loss rate if foreclosed (%)	49.4	58.3	72.2

Table B.7: First stage of Table 1.8

This table shows the first stage regressions of the second stage regressions shown in Table 1.8. The loans in the sample are divided into three groups based on their short-run house price returns. Column 1 shows the results of the IV estimation on the loans in areas that did not experience a rebound in house prices (prices continued to drop). Two other groups are formed based on the median return of the remaining loans. Column 2 considers the loans that experienced a small house price rebound while Column 3 considers the loans that experienced a large house price rebound. All estimates are in percentage terms. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)
	No housing price rebound	Small housing price rebound	Large housing price rebound
Both Markets×After Fee	-4.39***	-5.34***	-5.35***
	(-18.97)	(-7.88)	(-7.10)
Credit Score	-0.31**	-0.41*	-0.09
	(-2.18)	(-1.71)	(-0.91)
CLTV	0.34***	0.16	0.10
	(4.07)	(0.87)	(0.78)
Interest Rate	3.76***	4.61***	4.74***
	(17.76)	(20.10)	(24.86)
Unpaid Balance	0.59***	0.30**	0.36*
	(2.80)	(2.15)	(1.88)
Adjustable	3.83***	3.09***	2.95***
	(12.78)	(11.88)	(5.83)
Non-Owner Occupied	-4.55***	-4.17***	-3.42***
	(-17.93)	(-8.68)	(-12.27)
Low/No-Doc	-3.42***	-3.47***	-3.98***
	(-10.68)	(-11.34)	(-22.20)
Prepayment Penalty	3.84***	3.65***	3.70***
	(9.46)	(14.46)	(15.55)
Origination month FE	Y	Y	Y
CBSA FE	Y	Y	Y
Servicer FE	Y	Y	Y
Distress month FE	Y	Y	Y
Observations	305,441	73,309	73,178
Adj. R^2	0.09	0.09	0.09
F-statistic	360.0	62.0	50.4

Table B.8: IV regressions by unemployment and income levels at the time of distress

This table shows the second stage of instrumental variable regressions estimated using the standard two-stage least squares (2SLS) procedure. The loans in the sample are divided into two groups based based on unemployment levels at the time the loan became distressed, or on the average household income of the ZIP code in 2006. All estimates are in percentage terms. Reported z-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
	Low unemployment	High unemployment	Low income	High income
Modification	-35.73***	-8.22	-6.11	-13.78***
	(-2.91)	(-1.19)	(-0.91)	(-3.17)
Credit Score	1.46***	1.58***	1.93***	1.25***
	(7.51)	(7.97)	(8.49)	(7.84)
CLTV	5.46***	5.01***	5.92***	4.85***
	(17.26)	(11.07)	(14.03)	(22.69)
Interest Rate	0.45	-1.56***	-2.00***	-0.30
	(1.10)	(-4.52)	(-8.99)	(-1.09)
Unpaid Balance	-5.46***	-6.22***	-10.67***	-2.91***
	(-10.90)	(-14.98)	(-16.85)	(-8.71)
Adjustable	5.80***	4.30***	5.46***	4.16***
	(10.26)	(7.34)	(9.35)	(18.35)
Non-Owner Occupied	13.48***	16.17***	17.36***	9.28***
	(6.42)	(8.92)	(9.59)	(14.23)
Low/No-Doc	2.21***	1.64***	2.78***	1.91***
	(2.98)	(5.10)	(5.64)	(3.55)
Prepayment Penalty	2.24***	1.24***	0.56	1.75***
	(3.62)	(3.05)	(1.18)	(4.94)
Origination month FE	Y	Y	Y	Y
CBSA FE	Y	Y	Y	Y
Servicer FE	Y	Y	Y	Y
Distress month FE	Y	Y	Y	Y
Observations	466,260	452,758	499,469	499,466
Adj. R^2	0.15	0.23	0.22	0.20
First stage coefficient	-5.30***	-4.61***	-5.54***	-5.93***
	(-9.97)	(-10.41)	(-14.83)	(-13.06)
F-statistic	63.4	61.1	220.0	170.6
Mean income/unemployment	4.6	7.8	36,263	71,177

Table B.9: First stage of Table 1.9

This table shows the first stage regressions of the second stage regressions shown in Table 1.9. The sample is divided into two groups based on the median increase in unemployment from the month the loan became distressed to the highest value of the index in 2009. Column 1 considers the loans with a small increase in unemployment while Column 2 considers the loans with a large increase in unemployment. All estimates are in percentage terms. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by CSA. ***p<0.01, **p<0.05, *p<0.1.

	Small increase in unemployment	Large increase in unemployment
Both Markets×After Fee	-4.85***	-11.11***
	(-7.96)	(-7.82)
Credit Score	-0.22**	0.39***
	(-2.06)	(4.10)
CLTV	0.87***	0.07
	(13.58)	(1.28)
Interest Rate	3.12***	3.28***
	(11.29)	(18.66)
Unpaid Balance	0.63***	0.50***
	(6.87)	(6.71)
Adjustable	4.19***	1.33***
	(8.00)	(9.76)
Non-Owner Occupied	-4.59***	-2.91***
	(-23.09)	(-22.94)
Low/No-Doc	-2.66***	-2.66***
	(-5.08)	(-23.45)
Prepayment Penalty	3.25***	2.07***
	(8.55)	(10.27)
Origination month FE	Y	Y
CBSA FE	Y	Y
Servicer FE	Y	Y
Distress month FE	Y	Y
Observations	226,562	224,838
Adj. R^2	0.08	0.06
F-statistic	63.4	61.1

Table B.10: Matching analysis of loan losses

This table shows the results from a propensity score matching analysis that compares the loan losses of loans modified by servicers which only manage non-agency loans with the loan losses of loans that were not modified by servicers which manage both agency and non-agency loans, by modification type. The matching is performed based on the month the loans became distressed, on ZIP code (or CBSA for a higher matching rate), and on propensity scores calculated using the logit regression in Panel A of Table 1.10. Matching is performed without replacement using the nearest neighbor technique (1-to-1). Also, a common support is imposed and the maximum difference between the propensity scores of the treated (modified) loans and the control (non-modified) loans is limited to 0.5% (0.1% when matching by CBSA). Since some loans have identical propensity scores, the sample is randomly sorted before matching. The table shows the average treatment effect on modifications (ATT), with robust t-statistics in parentheses. Columns 1, 2, 3, and 4 show the ATT for modifications in which more than one attribute was modified, principal reductions, interest rate reductions, and capitalizations, respectively. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)	(3)	(4)
Differences in loss rates, modified minus not modified	Multiple attributes	Principal reduction	Interest rate reduction	Capitalization
Matched sample (by ZIP), ATT	-14%***	-11.9%**	-6.1%*	-3.3*
t-statistic	(-10.67)	(-2.18)	(-1.81)	(-1.74)
Matching rate	12.5%	13.8%	13.2%	16.0%
Matched sample (by CBSA), ATT	-12.8%***	-11.9%***	-0.2%	-3.0%***
t-statistic	(-19.11)	(-4.01)	(-0.09)	(-2.97)
Matching rate	42.9%	42.1%	44.1%	53.9%

Table B.11: Effect of securitization on house price returns (pooled regressions)
This table shows OLS estimates for regressions where zip code house price returns is the dependent variable, on the zip code-level of securitization and on the fraction of securitized loan originations by the various types of originators during the period from 2003 to 2006. Columns 1 and 2 show the results for the boom period (2003-2006), and columns 3 and 4 show the results for the bust period (2007-2012). *t*-statistics are presented in parentheses. ***p<0.01, **p<0.05, *p<0.1.

	2003-2006		2007-2012	
Fraction Securitized	1.44***		-0.79***	
	(25.62)		(-24.70)	
Fraction Securitized by Worst Originators		7.73***		-5.97***
		(12.81)		(-17.54)
Fraction Securitized by Medium Originators		1.86***		-0.54***
		(10.45)		(-5.37)
Fraction Securitized by Best Originators		0.17		-0.31***
		(0.94)		(-2.96)
Constant	0.22***	0.23***	-0.09***	-0.10***
	(23.12)	(24.36)	(-16.55)	(-18.18)
Observations	5,176	5,176	5,176	5,176
Adj. R-squared	0.11	0.13	0.11	0.14

Table B.12: Effect of securitization on house returns

This table shows OLS estimates for regressions where zip code house price returns is the dependent variable, on the zip code-level of securitization and on the fraction of securitized loan originations by the various types of originators during the period from 2003 to 2006. Columns 1 to 4 show the results for the boom period (2003-2006) and columns 5 to 8 show the results for the bust period (2007-2012). Columns 3, 4, 7 and 8 include demographic controls. All regressions have MSA fixed effects, and standard errors are clustered at the MSA level. *t*-statistics are presented in parentheses. ***p<0.01, **p<0.05, *p<0.1.

	2003-2006				2007-2012			
Fraction Securitized	0.181***		0.198***		-0.432***		-0.444***	
	(3.28)		(2.78)		(-5.58)		(-8.87)	
Fraction Securitized by Worst Originators		7.420***		5.130***		-5.309***		-2.699*
		(4.77)		(4.71)		(-3.15)		(-1.89)
Fraction Securitized by Medium Originators		-0.365*		-0.121		0.021		-0.216
		(-1.93)		(-0.75)		(0.12)		(-1.38)
Fraction Securitized by Best Originators		-0.586		-0.378		-0.070		-0.332
		(-1.54)		(-1.19)		(-0.24)		(-1.33)
Population			0.007***	0.006***			-0.004***	-0.004***
			(3.19)	(2.87)			(-5.14)	(-5.82)
Housing Units			-0.017***	-0.015***			0.011***	0.011***
			(-3.16)	(-2.95)			(5.40)	(5.96)
Housing Vacancy Rate			0.704***	0.658***			-0.209***	-0.187***
			(4.10)	(3.99)			(-4.14)	(-3.99)
Average Household Income			-0.001***	-0.001***			0.001***	0.001***
			(-3.32)	(-3.00)			(7.03)	(6.61)
Change in Avg. Household Income			0.000	0.001			0.001***	0.000***
			(0.78)	(1.20)			(2.85)	(3.32)
Constant	0.420***	0.438***	0.430***	0.432***	-0.147***	-0.159***	-0.196***	-0.197***
	(48.56)	(47.60)	(24.92)	(29.69)	(-12.14)	(-12.68)	(-15.03)	(-13.76)
MSA FE	Y	Y	Y	Y	Y	Y	Y	Y
SE Clustered by MSA	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5,176	5,176	5,176	5,176	5,176	5,176	5,176	5,176
Adj. R-squared	0.77	0.79	0.80	0.81	0.66	0.69	0.73	0.73

Table B.13: Effect of worst originator activity on house price returns (higher credit score)

This table shows OLS estimates for regressions where zip code house price returns is the dependent variable, on the zip code-level market share of the various types of originators during the period from 2003 to 2006. The zip codes in the regressions belong to the lowest quartile based on the fraction of loans securitized reported in ABSNet with credit scores under 660. The regressions include different combinations of demographic controls and MSA fixed effects. Columns 1 to 3 show the results for the boom period (2003-2006), and columns 4 to 6 show the results for the bust period (2007-2012). *t*-statistics are presented in parentheses. ***p<0.01, **p<0.05, *p<0.1.

	2003-2006			2007-2012		
Worst Originators' Mkt. Share	2.152***	1.412***	1.463***	-1.520***	-1.792***	-1.745***
	(9.01)	(4.26)	(4.38)	(-10.53)	(-3.41)	(-3.46)
Medium Originators' Mkt. Share	0.996***	-0.161	-0.067	0.033	0.144	0.170
	(6.74)	(-1.09)	(-0.46)	(0.37)	(0.86)	(0.89)
Best Originators' Mkt. Share	-0.268	0.005	0.057	0.145	0.533**	0.431**
	(-1.54)	(0.02)	(0.18)	(1.39)	(2.52)	(2.14)
Fraction Securitized			-0.106			-0.087
			(-1.25)			(-1.05)
Population			0.002			-0.001
			(1.15)			(-1.42)
Housing Units			-0.005			0.006**
			(-1.33)			(2.49)
Housing Vacancy Rate			0.435***			-0.208***
			(5.48)			(-4.19)
Average Household Income			-0.000*			0.000
			(-1.99)			(1.37)
Change in Avg. Household Income			0.000			0.000***
			(1.23)			(3.06)
Constant	0.159***	0.387***	0.381***	-0.104***	-0.168***	-0.178***
	(6.75)	(7.13)	(7.94)	(-7.28)	(-4.27)	(-4.20)
MSA FE	N	Y	Y	N	Y	Y
SE Clustered by MSA	N	Y	Y	N	Y	Y
Observations	1,036	1,036	1,036	1,036	1,036	1,036
Adj. R-squared	0.20	0.75	0.76	0.11	0.58	0.61

Table B.14: Effect of worst originator activity in elastic and inelastic ZIP codes during the boom

This table shows OLS estimates for regressions where zip code house price returns during the boom is the dependent variable, on the zip code-level market share of the various types of originators during the period from 2003 to 2006, for different subsamples of zip codes based on housing supply elasticities from Saiz (2008). The regressions include different combinations of demographic controls and MSA fixed effects. Column 1 shows the estimates for the zip codes in MSAs in the most elastic half. Column 2 shows the regression for zip codes in MSAs in the most elastic quartile. Column 3 considers the most inelastic half and column 4 the most inelastic quartile. Reported *t*-statistics in parentheses are heteroskedasticity-robust and clustered by MSA. ***p<0.01, **p<0.05, *p<0.1.

	Elastic MSAs		Inelastic MSAs	
	2003-2006		2003-2006	
	Top 50%	Top 25%	Bottom 50%	Bottom 25%
Worst Originators' Mkt. Share	0.924	-1.032**	1.093***	1.315***
	(0.80)	(-2.19)	(2.78)	(3.44)
Medium Originators' Mkt. Share	0.147	-0.209	-0.423***	-0.253**
	(0.77)	(-1.28)	(-4.74)	(-2.73)
Best Originators' Mkt. Share	0.256	0.185	-0.925**	-1.212***
	(1.25)	(0.80)	(-2.43)	(-2.95)
Fraction Securitized	0.009	0.024	0.094	0.109
	(0.08)	(0.16)	(0.65)	(0.61)
Population	0.001	0.003	0.005***	0.005***
	(0.77)	(1.37)	(3.92)	(3.87)
Housing Units	-0.004	-0.007	-0.012***	-0.011***
	(-1.36)	(-1.66)	(-3.29)	(-3.07)
Housing Vacancy Rate	0.870***	0.637***	0.631***	0.579***
	(2.81)	(4.06)	(4.03)	(3.52)
Average Household Income	-0.002***	-0.001***	-0.001***	-0.001***
	(-3.77)	(-4.12)	(-2.88)	(-3.83)
Change in Avg. Household Income	0.002***	0.002***	0.000	0.001**
	(5.48)	(4.49)	(0.66)	(2.28)
Constant	0.319***	0.267***	0.586***	0.616***
	(6.98)	(9.95)	(15.94)	(12.16)
MSA FE	Y	Y	Y	Y
SE Clustered by MSA	Y	Y	Y	Y
Observations	1,796	633	2,871	2,111
Adj. R-squared	0.80	0.67	0.82	0.80

Table B.15: Lender names and second-lien misreporting ranking frequencies

This table shows the number of years each of the 25 lenders in the sample entered the different terciles of second-lien misreporting.

	Tercile of second-lien misrep.		
Lender	1	2	3
Fieldstone	0	0	6
First Franklin	0	0	6
Fremont	0	0	6
GreenPoint	0	0	6
WMC	0	0	6
Aegis	0	2	4
Mortgage IT	1	2	3
Ownit	0	0	3
American Home	0	4	2
BNC	0	4	2
New Century	0	4	2
People's Choice	0	4	2
Argent	5	1	0
Bank of America	4	2	0
Chase	2	4	0
Countrywide	2	4	0
Downey	6	0	0
IMPAC	6	0	0
Indymac	6	0	0
National City	3	3	0
Option One	3	3	0
GMAC RFC	6	0	0
SunTrust	1	5	0
Washington Mutual	5	1	0
Wells Fargo	1	5	0

Table B.16: Loan characteristics by lender type (matched sample)

This table compares the characteristics of the loans issued by the worst originators with the characteristics of the loans issued by the best originators in the matched sample. For each loan issued by a bad originator, we find another loan issued by a good originator in the same ZIP code-year that also has similar propensity score. To compute the propensity score, we estimate a logit regression where the dependent variable is a dummy that takes the value of one if the loan was issued by one of the worst originators and takes the value of zero if the loan was issued by one of the best originators, and the explanatory variables are combined LTV, credit score, interest rate, the log of the loan amount, and indicators for low-doc, non-owner occupied property, arm loan, and the existence of a prepayment penalty. Also, we impose a maximum distance between propensity scores of 1%. We are able to impose such a tight criteria because there are many more loans from the better originators and we match with replacement up to a maximum of five times.

	Worst Lenders	Best Lenders
CLTV (perc.)	90.1	91.0
Full-Doc	42.0%	32.0%
Interest Rate (perc.)	7.30	7.28
Non-owner Occupied	17.6%	19.9%
Credit Score	677	686
Loan Amount	257,079	277,674
ARM	80.7%	78.5%
Prepayment Penalty	50.6%	50.6%
Delinquent 90+	63.2%	54.4%

Table B.17: Explanatory power of loan-level controls - separate subsamples

This table shows OLS loan-level regressions where the dependent variable is an indicator for whether the loan became 90 days or more delinquent and the explanatory variables are a set of loan characteristics. ZIP Code interacted with year of origination fixed effects are also included. Columns 1 shows the results for the loans by the worst originators while column 2 shows the results for the loans by the best originators. For each loan issued by a bad originator, we find another loan issued by a good originator in the same ZIP code-year that also has similar propensity score. To compute the propensity score, we estimate a logit regression where the dependent variable is a dummy that takes the value of one if the loan was issued by one of the worst originators and takes the value of zero if the loan was issued by one of the best originators, and the explanatory variables are combined LTV, credit score, interest rate, the log of the loan amount, and indicators for low-doc, non-owner occupied property, arm loan, and the existence of a prepayment penalty. Also, we impose a maximum distance between propensity scores of 1%. We are able to impose such a tight criteria because there are many more loans from the better originators and we match with replacement up to a maximum of five times. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered by CBSA. ***p<0.01, **p<0.05, *p<0.1.

	Delinquency 90+	
	Worst	Best
CLTV	0.351***	0.803***
	(16.19)	(28.98)
Full-Doc	-7.559***	-9.680***
	(-7.19)	(-11.16)
Interest Rate	1.635***	0.827***
	(8.03)	(4.34)
Non-owner Occupied	-0.656	0.167
	(-0.52)	(0.14)
Credit Score	-0.139***	-0.154***
	(-14.36)	(-14.90)
ln(Loan Amount)	9.221***	5.661***
	(8.41)	(6.29)
ARM	4.737***	2.137***
	(5.69)	(3.32)
Prepayment Penalty	5.598***	11.578***
	(6.93)	(13.51)
Constant	-2.725	7.008
	(-0.24)	(0.62)
ZIPxYear FE	Y	Y
Observations	86,822	86,822
Adj. R-squared	0.25	0.35

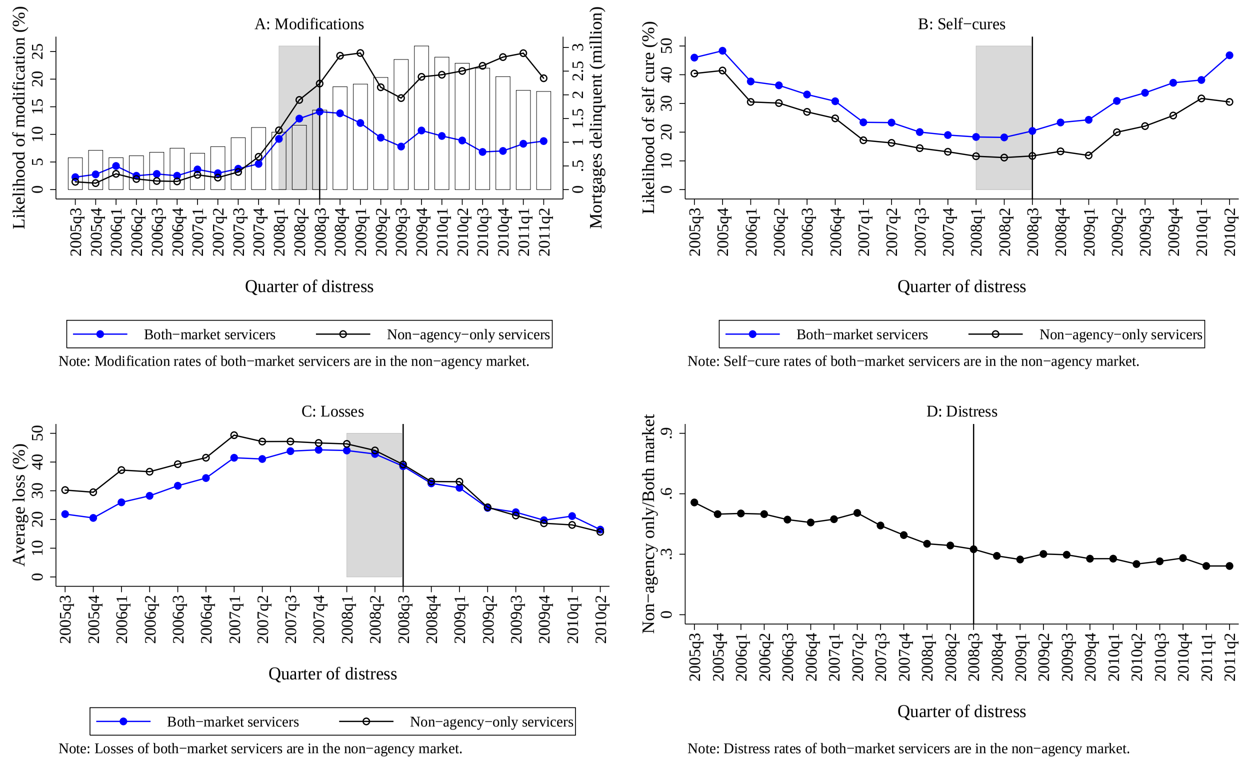


Figure B.1: Main figures in larger time windows

This figure shows several of the figures shown in the paper for a larger time window than the one analyzed. Panel A, B, and C show the modification rates, the self-cure rates, and the loan losses of both-market servicers and non-agency-only servicers, respectively. Panel D shows the ratio of the number of distressed loans between the two types of servicers.

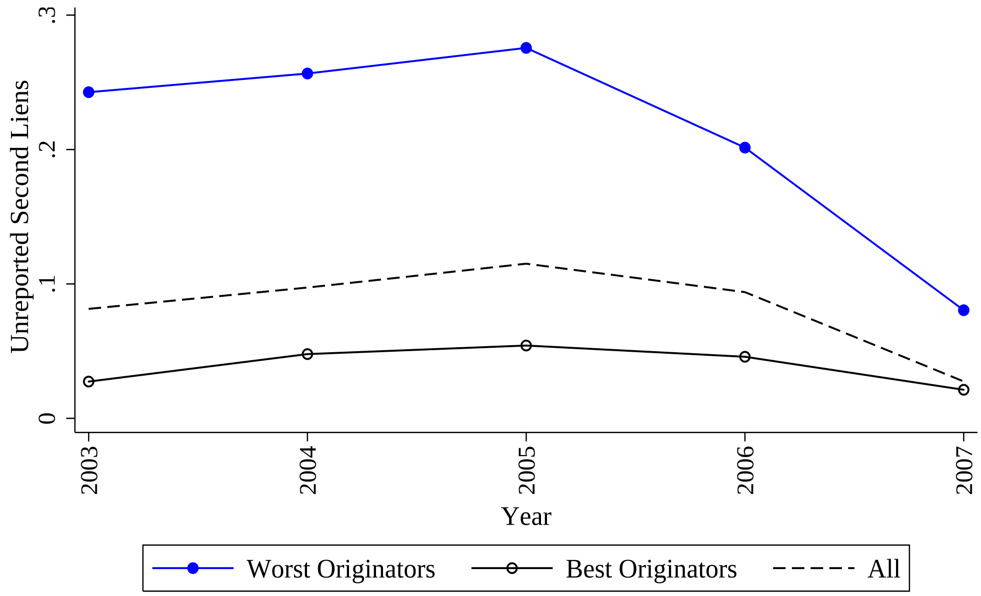


Figure B.2: Second-lien misreporting by originator tercile

This figure shows the yearly second-lien misreporting of the highest (blue solid circles) and lowest (black hollow circles) terciles of misreporting. The dash line shows the average for all the originators.

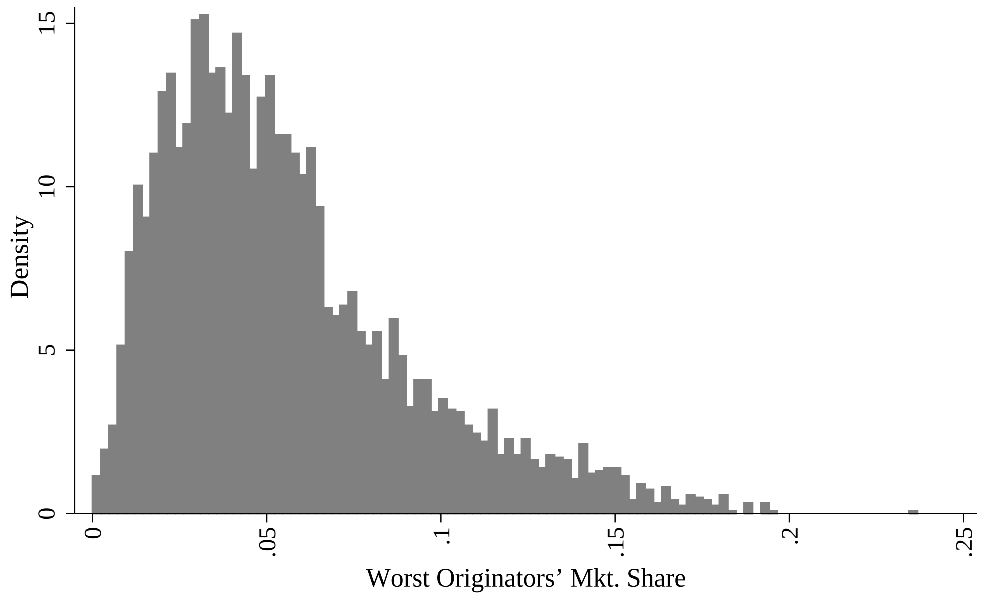


Figure B.3: Histogram of worst originators' market share

Each year the 25 loan originators in Griffin and Maturana (2014) are classified into 3 groups based on the cumulative fraction of loans they issued with second-lien misreporting. The amount of cumulative misreporting of each originator in year $t-1$ is used to rank the originators in year t. Originators in the tercile with the highest misreporting are referred to as the worst originators. This figure shows the histogram of frequencies of the *worst originators' market share* between 2003 and 2006.

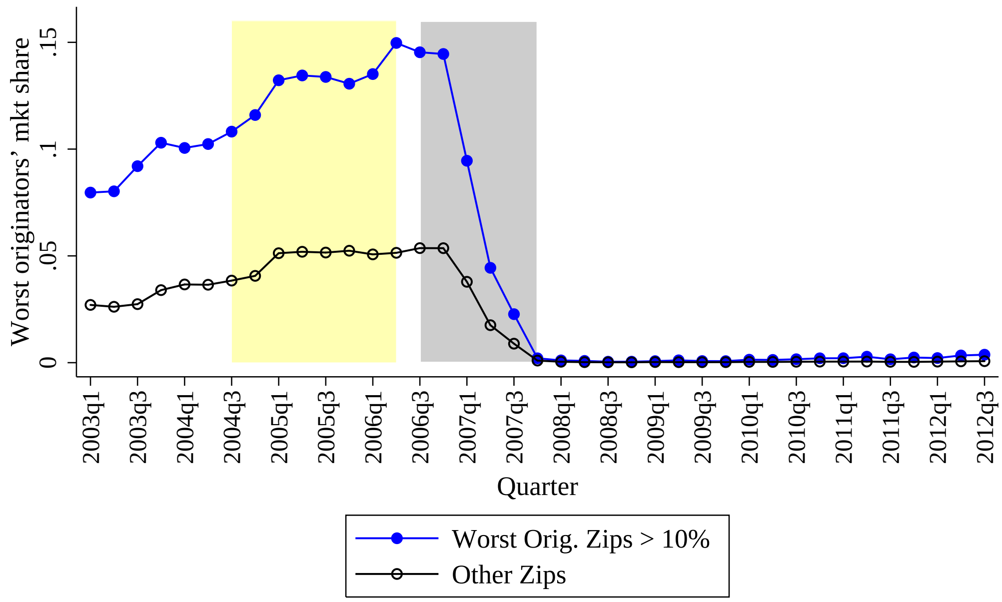


Figure B.4: Worst originators' market share

This figure shows the time-series of the average worst originators' market share for each group shown in Figure 2.1. Zip codes are divided into two groups: those where the average market share of the worst originators during the period 2004q3-2006q2 (highlighted by the yellow shaded area) exceeds 10% (blue solid circles) and the other remaining zip codes (black hollow circles). The gray shaded area highlights the period when most of the worst originators went bankrupt or lost considerable business.

A: 2003-2006

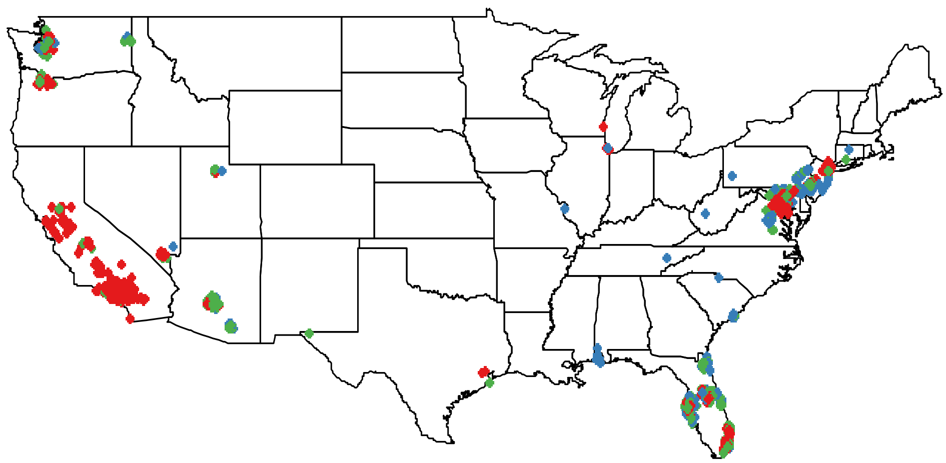
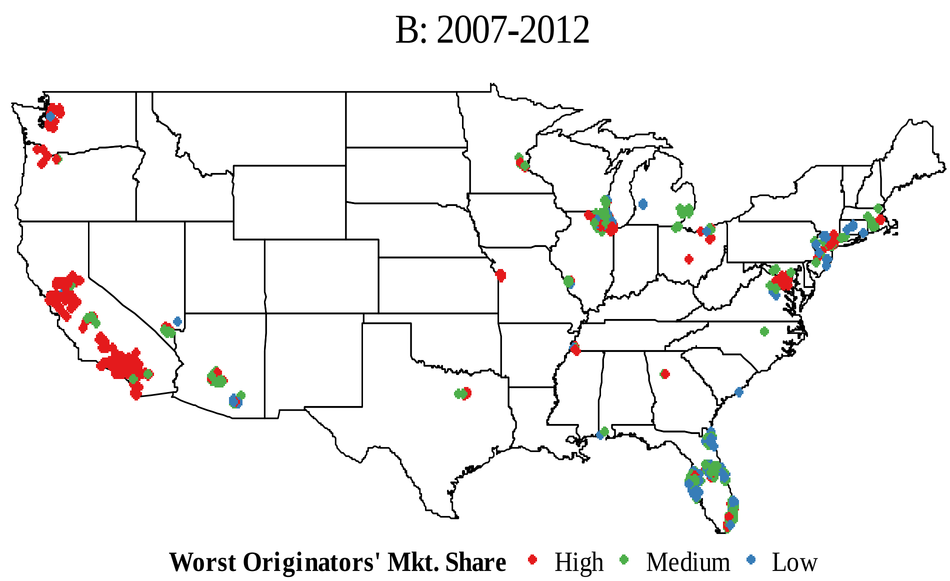


Figure B.5: Extreme house price movements and worst originators' market share

The above maps report the zip codes with the most extreme house returns during the boom and the bust, as well as the presence of bad originators within these zip codes. Zip codes are first divided into three equal terciles based on market share of the worst originators. Zip codes with the highest presence of the worst originators are in red, a moderate presence is in green, and the lowest presence is in blue. Additionally, zip codes are classified into four equal quartiles based on house price returns during the boom and the bust. In the boom, Panel A, only the zip codes in the highest quartile of house returns are displayed, representing the largest gains. Similarly, in the bust, Panel B, only the zip codes in the lowest quartile of house returns are displayed, representing the largest losses.

Figure B.5 - **continued**



Panel A: Securitization and house price return

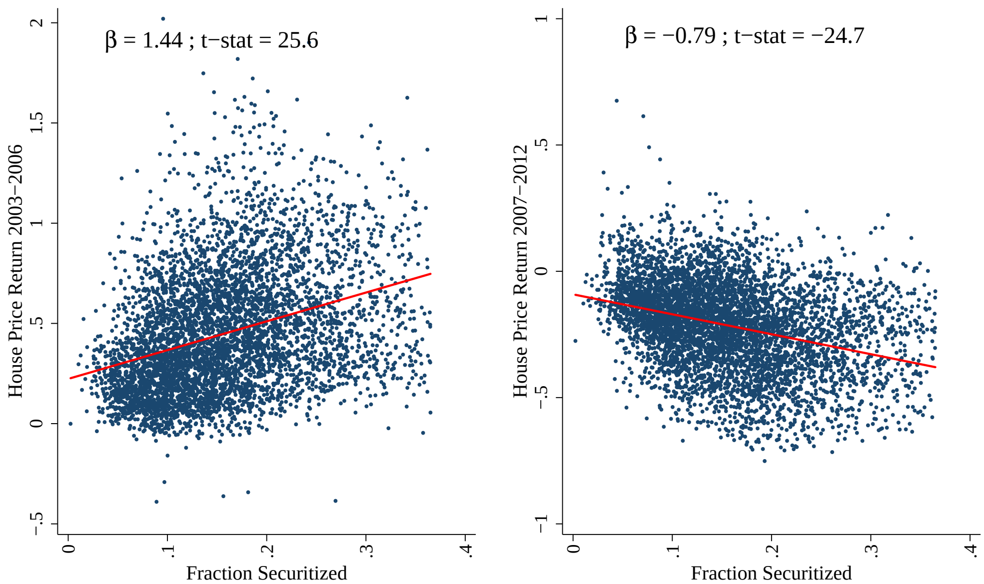
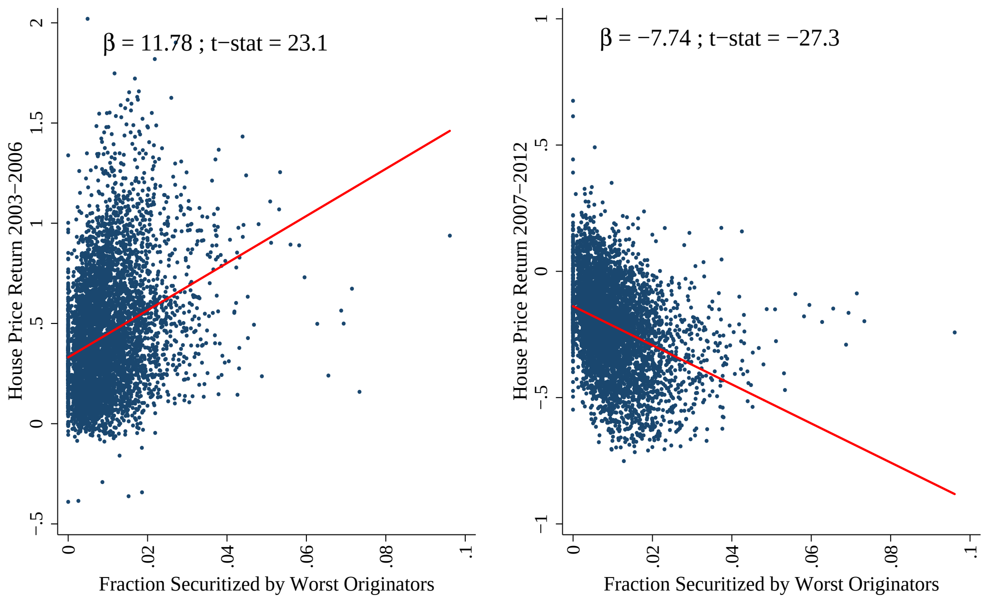


Figure B.6: Securitization and house returns

This figure shows the correlation of securitization activity with house price returns. Panel A shows the relation between the fraction of loans securitized in each zip code during the period 2003-2006 and the return of the corresponding zip code house price index for the 2003-2006 period (left graph), and for the 2007-2012 period (right graph). Panel B shows the relation between the fraction of loans securitized in each zip code during the period 2003-2006 by the worst tercile of originators based on second-lien misreporting and the return of the corresponding zip code house price index. The red lines fit pooled linear regressions. Coefficient estimates and t-statistics are presented at the top of each graph.

Figure B.6 - **continued**

Panel B: Securitization by worst originators and house price return



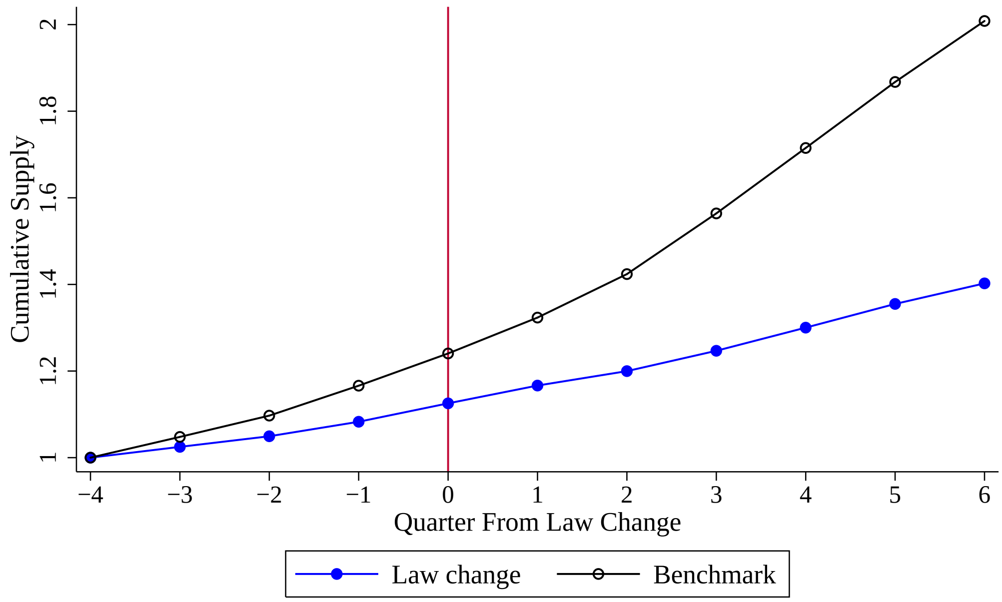


Figure B.7: Loan supply by the worst originators before and after APLs

This figure compares the cumulative loan supply by the worst originators of zip codes in states that passed anti-predatory lending laws (APLs) between 2004 and 2005 (blue circles) with the cumulative loan supply by the worst originators of a benchmark of zip codes in states that did not pass any APLs before 2006 (black hollow circles), before and after the law changes. The states in the first group are Indiana, Massachusetts, New Mexico, South Carolina, and Wisconsin. The states in the benchmark are Arizona, Delaware, New Hampshire, Montana, Oregon, Washington, and Tennessee.

Panel A: 2004Q1, New Mexico and South Carolina

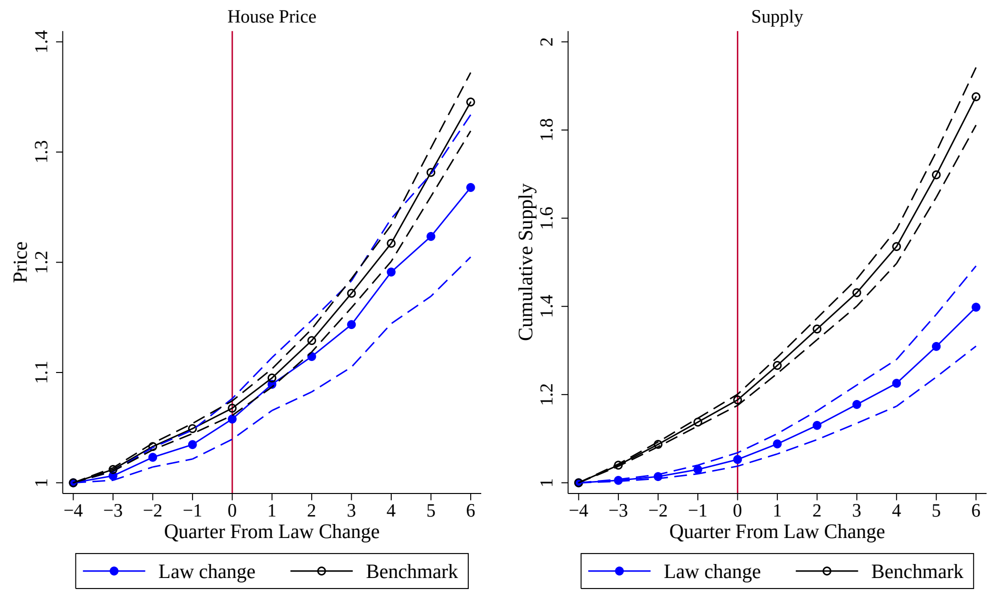
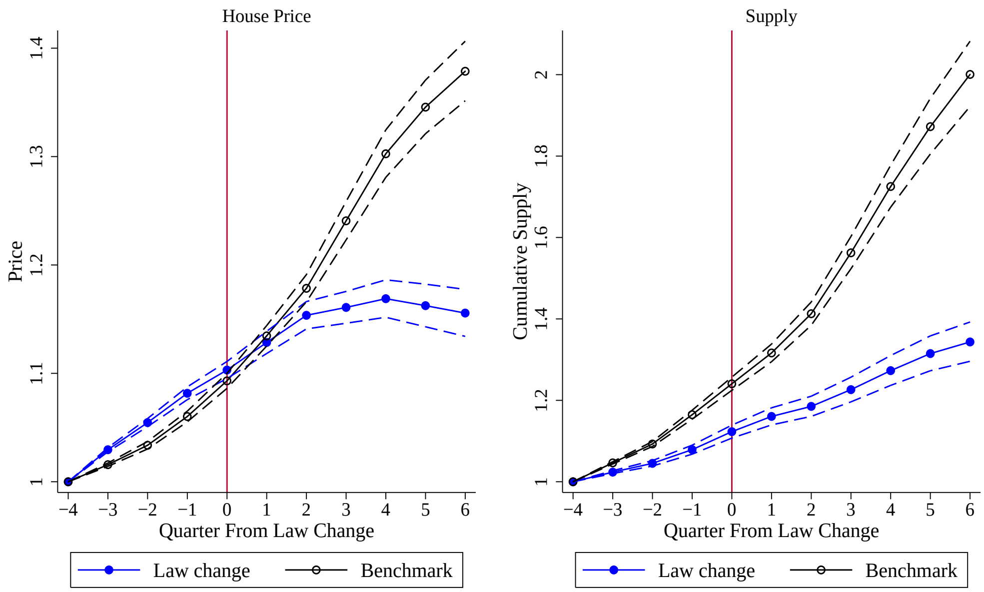


Figure B.8: Effect of APLs on house price movements and loan supply by the worst originators

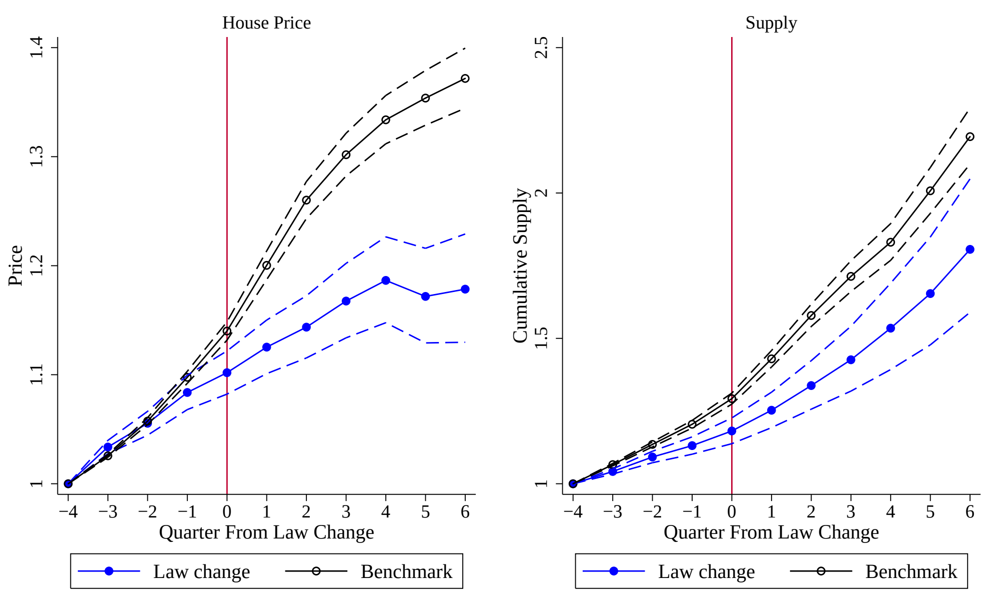
This figure compares the house price movements (on the left) and the cumulative loan supply by the worst originators (on the right) of zip codes in states that passed anti-predatory lending laws (APLs) between 2004 and 2005 (blue circles) with the house price movements and the cumulative loan supply by the worst originators of a benchmark of zip codes in states that did not pass any APLs before 2006 (black hollow circles). In each panel, zip codes share the same quarter when APLs were passed. In Panel A, the zip codes in the "Law Change" group are from New Mexico and South Carolina (APL in 2004Q1). In Panel B, the zip codes in the "Law Change" group are from Massachusetts (APL in 2004Q3). In Panel C, the zip codes in the "Law Change" group are from Indiana and Wisconsin (APL in 2005Q1). Dashed lines delimit the 95% confidence interval.

Figure B.8 - **continued**

Panel B: 2004Q3, Massachusetts



Panel C: 2005Q1, Indiana and Wisconsin



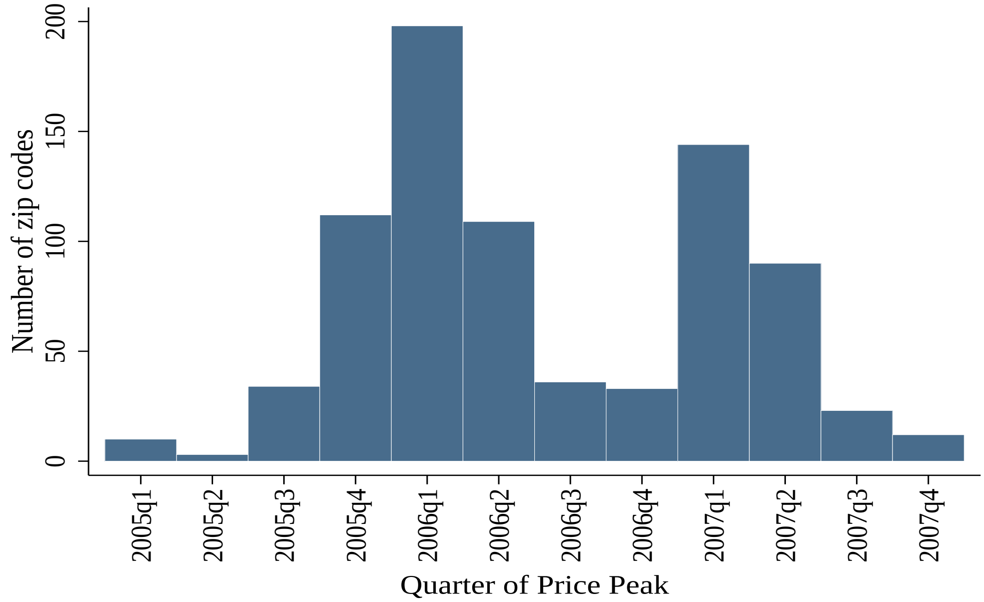


Figure B.9: Frequency histogram of house price peaks

This figure shows the frequency histogram for the house price peaks of the zip codes considered in Figure 2.3 and Table 2.6.

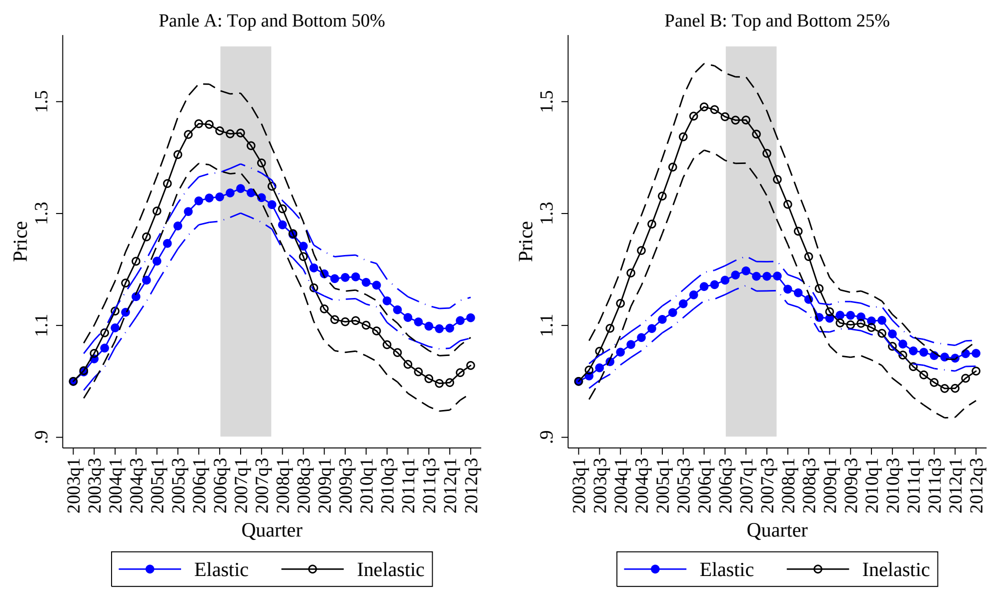


Figure B.10: House price movements in elastic and inelastic ZIP codes

This figure shows the average price movements of zip codes in MSAs with high housing supply elasticity (blue circles) and with low housing supply elasticity (hollow black circles). Panel A splits the sample in half. Panel B considers the zip codes in the extreme quartiles of MSA housing supply elasticity. The dashed lines represent the 95% confidence interval.

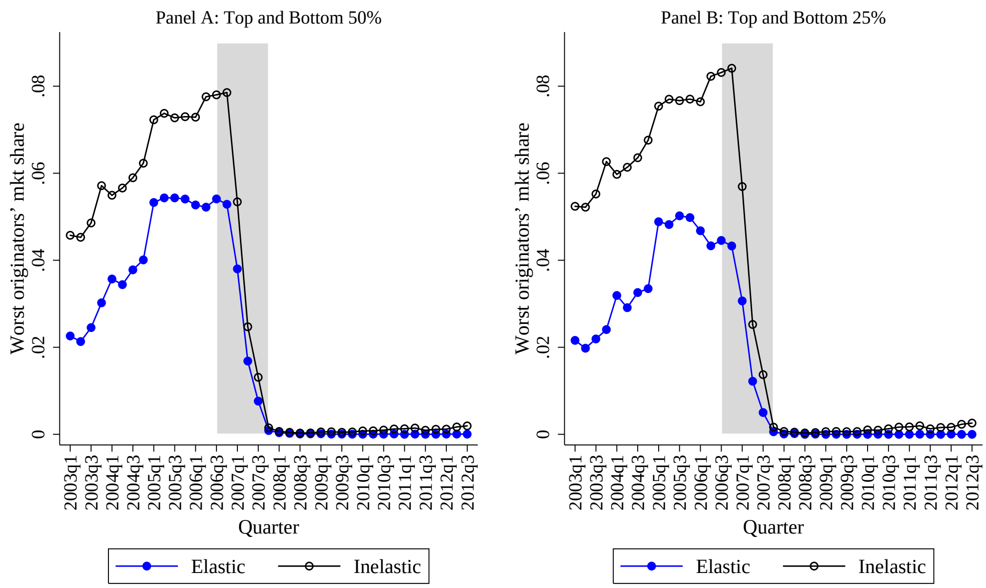


Figure B.11: Worst originator activity of elastic and inelastic ZIP codes

This figure shows the evolution of the worst originators' market share of zip codes in MSAs with high housing supply elasticity (blue solid circles) and with low housing supply elasticity (black hollow circles). Panel A compares the top half of MSAs against the bottom half based on housing supply elasticity. Panel B considers the zip codes in the extreme quartiles.

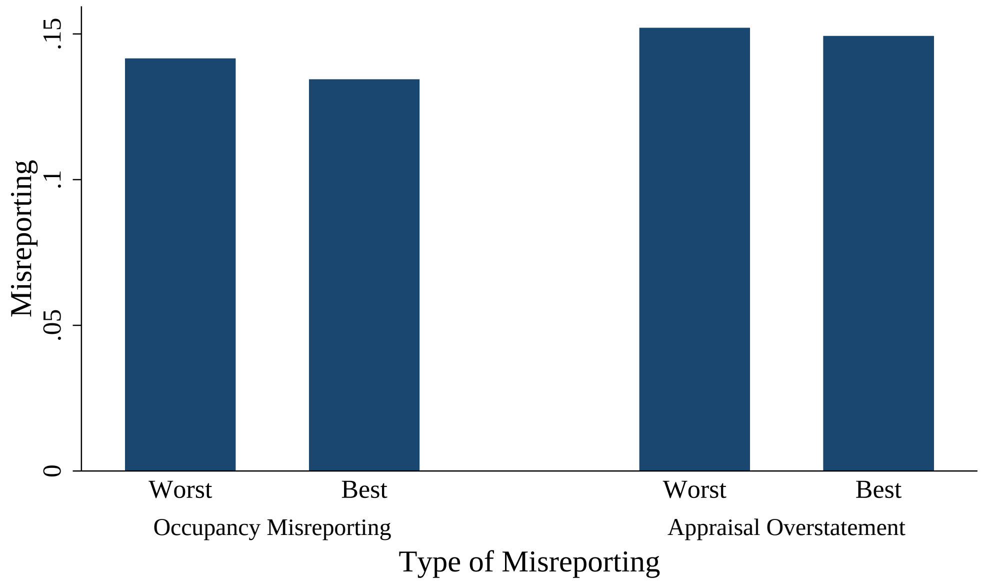


Figure B.12: Occupancy misreporting and appraisal overstatements by lender type

This figure shows the amounts of occupancy misreporting and appraisal overstatements by lender type (worst and best). The two misreporting indicators are defined in Griffin and Maturana (2014).

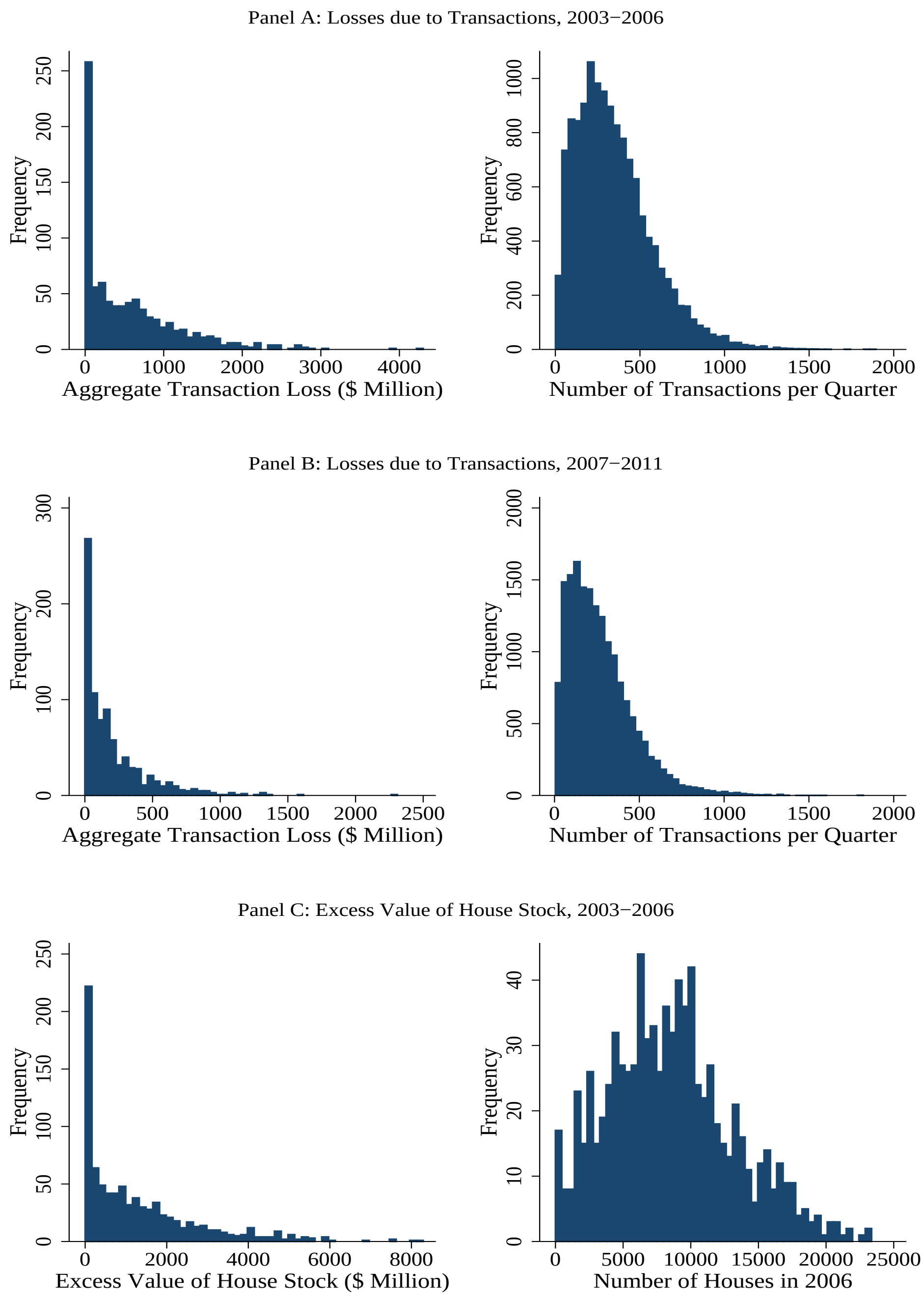


Figure B.13: Histograms of cost estimates components

This figure shows histograms of ZIP code-level values of different components involved on the computation of the transaction losses (Panels A and B) and the excess value of houses (Panel C) in the ZIP codes with larger share of bad originators.

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Vita

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This dissertation was typeset with LaTeX[†] by the author.

[†]LaTeX is a document preparation system developed by Leslie Lamport as a special version of Donald Knuth's TeX Program.