THESIS

AN ANALYSIS OF HOUSING VALUES AND NATIONAL FLOOD INSURANCE REFORM UNDER THE BIGGERT-WATERS ACT OF 2012

Submitted by

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ABSTRACT

AN ANALYSIS OF HOUSING VALUES AND NATIONAL FLOOD INSURANCE REFORM UNDER THE BIGGERT-WATERS ACT OF 2012

Previous research has shown that both flood risk and insurance premiums are capitalized in housing values. This paper examines the effect of National Flood Insurance Program reform implemented by the Biggert Waters Act of 2012 and the Homeowners Flood Insurance Affordability Act of 2014 on housing values over a three-and-a-half year time period. It is hypothesized that the effects of increasing flood insurance rates through the elimination of established subsidies was capitalized in home values resulting in a loss of value in areas where subsidies are maintained. The paper presents a hedonic price difference-in-difference OLS model which is then tested for flexibility to the policy period and robustness to the treatment group. The evidence indicates that (1) housing values trend differently for areas with subsidies than areas without and (2) that this effect is correlated with flood insurance reform periods and robust to the definition of the treatment group. I conclude that the Biggert-Waters Act had a negative impact on median home values for areas with subsidized policies.

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CHAPTER ONE

INTRODUCTION

Goals and Structure of the Study

This paper considers recent reform of the National Flood Insurance Program (NFIP) under the Biggert Waters Act of 2012 (BW12). The "Big Question" this paper aims to answer is whether changes in the administration of insurance premium subsidies had a measurable effect on residential housing markets across the United States. The NFIP has over 5.6 million policies, almost one for every twenty households across the U.S. With the implementation of BW12 in January and October 2013 the reform measures effectively increased the rates of property owners who received subsidized NFIP premiums. Whether subsidies were eliminated or substantially reduced and when these changes took place depended upon the type of property covered; however, all subsidized policies were affected by the reform. Later, in March 2014, these measures were scaled back due to popular recall of BW12. The passage of the Homeowners Flood Insurance Affordability Act of 2014 (HFIAA) renegotiated the terms of subsidy phase out period and reestablished some subsidies resulting in refunds to property owners who overpaid.

The main goal of the paper is to assess the difference in median home values between those zip codes directly impacted by the policy and those not directly impacted by analyzing home values in all time periods before, during and after implementation of BW12. This is done by applying a basic difference-in-differences model to a panel of median home values from January 2010 to May 2015. Anecdotal evidence found in places as diverse as local news media reports and official testimony at congressional hearings has implied that uncertainty surrounding both the expected ex-ante magnitude of these changes and the ex-post realization of increased insurance premiums slowed or stalled housing markets in areas with a substantial number of subsidized structures.[1] The economic rationality for an observable change in home values around reform implementation posits that as the expense of homeownership for a particular structure rises its transaction value will decrease by the same or similar amount in order to maintain a consistent net value of the property. An alternative but not mutually exclusive theory is that an increase in insurance premiums could lower housing values because it signals greater flood risk to the market. Either drop in property values could be restated as a reduction in land rents due to the increased internalization of flood risk. The primary aim of this paper is to determine if the increase in the effective NFIP flood insurance premium resulted in decreases in median home values according to this logic, though it may not be possible to tell which theory is the leading factor.

This paper is comprised of five chapters. The first chapter presents an introduction to the study, the National Flood Insurance Program and previous research. Chapter Two introduces the basic methodology. The data used in the study is presented in Chapter Three along with a general strategy for overcoming empirical estimation issues. Chapter Four presents results of the established model, several modifications and robustness tests of both the identified trend and to alternate causal explanations. The final chapter concludes the study by summarizing the findings and suggesting areas for future research.

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[1] See the testimony of Donna Smith in Insuring Our Future: Building a Flood Insurance Program That We Can Live With, Grow With and Prosper With: Hearings before the Committee on Appropriations Subcommittee on Homeland Security (Insuring Our Future, 2014)

The National Flood Insurance Program: From Past to Present

The National Flood Insurance Program (NFIP) was established in 1968 as a flood insurance program underwritten by the Federal Government and operated by the Federal Emergency Management Agency (FEMA). The NFIP is intended to support the provision of flood insurance in areas that private insurers are unwilling to offer coverage. The necessity of expanding flood insurance coverage became apparent after Hurricane Betsy triggered massive uninsured losses in the Gulf of Mexico in 1965. In order for home and business owners or renters to be eligible to receive flood insurance their communities must participate in the program. Participation requires communities to engage in flood mitigation and loss reduction efforts that meet or exceed FEMA requirements.[2] In theory this leads to a win-win situation whereby communities reduce the risk of catastrophic flooding, insurers lessen their exposure to covered losses and the Federal Government reduces the need to rely on taxpayers for emergency assistance. Today, policies are administered either directly by FEMA or by property and casualty insurers who participate in the "Write Your Own" Program.[3]

Subsidies were not a prevalent part of the original NFIP but were expanded a few years after its inception. The intent was that by lessoning the financial impact on property owners the program would be more appealing to community members. These subsidies were not means tested and instead were offered to the owner of any structure that was already standing when it was drawn into a federally designated flood zone by a new federal Flood Insurance Rate Map

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[2] According to the Government Accountability Office such efforts include "diverting the flow of water through well-designed channels and retaining walls, or by containing the water through ponds" (GAO, 2014). It is also worth noting the existence of the Community Rating System, which is a voluntary incentive program offered to communities that exceed the minimum requirements of the NFIP, also offers reduced rates but these are not considered to be subsidized.

[3] The Write Your Own program allows private insurers to issue policies underwritten by the Federal Government in exchange for an expense allowance.

(FIRM). The expectation at the time was that the stock of subsidized properties would decrease as structures reached the end of their useful life; however, this has taken longer than expected and nearly a quarter of property owners with NFIP policies still receive subsidized insurance premiums (American Rivers, 2011).[4]

A second round of widespread uninsured losses after Hurricane Agnes in 1972 demonstrated that the first formulation of the NFIP had failed to adequately provide sufficient flood insurance coverage. In 1975 the federal government began requiring participation in the NFIP to receive federal disaster assistance and federally backed mortgages in flood plains to incentivize communities to participate in the program. These provisions stand today and flood insurance is required for any structure located in a high risk area with a federally backed mortgage. Structures in low risk areas are typically not required to carry flood insurance, although lenders can require flood insurance at their discretion. Because private insurers are typically unwilling to underwrite their own flood risk, alternatives to FEMA underwritten policies are difficult to locate.[5] For FEMA underwritten policies, calculation of insurance premiums is complex and depends on several factors including the type of structure insured, the choice of content coverage and deductibles, as well as the designated base flood elevation. Properties that continue to receive a subsidy are a diverse mixture of pre-FIRM residences, businesses, non-primary residences and properties that have experienced severe repetitive loses as well as any structure grandfathered into a new flood zone. Pre-FIRM properties are all properties that have not been sold, significantly remodeled or rated with elevation data since the

[4] According to the Government Accountability Office, "as communities were mapped and joined NFIP, new subsidized policies were added. While the percentage of subsidized policies has decreased since the program was established, the number of these policies has stayed fairly constant" (GAO, 2013).
[5] A search turned up only two companies underwriting flood insurance. Policies are available that are underwritten by Lloyds of London and Lexington Insurance Company.

NFIP was reformed in 1974. [6] All subsidized properties were affected in some way by the reduction in subsidies under BW12. Some nonsubsidized policies saw their rates reduced as true risk was reassessed.

In addition to offering subsidized premiums, the NFIP calculation of rates is only meant to cover the average historical loss year. That the NFIP was never designed to deal with extreme events without assistance has brought insurance reform back into the political arena. Both of these practices resulted in NFIP premiums that are generally lower than if they were structured to insure against the true risk levels associated with flood hazards. This has left the program vulnerable to natural variation in the severity of extreme events.

The extensive destruction caused by Hurricanes Katrina, Rita and Wilma in 2005 resulted in the NFIP facing a deficit of over $19 billion; a shortfall the program is unlikely ever to recover without a taxpayer bailout (Holladay and Schwartz 2010). Compounding this issue is that FEMA has excluded 2005 as an outlier in the calculation of the average historical loss year moving forward to avoid popular outcry from what would be large across the board increases. However, a solution was needed to avoid reliance on taxpayer support for Katrina and future catastrophes and to maintain the viability of the NFIP. The first signs of a serious movement for flood insurance reform surfaced in the summer of 2011 when the Senate banking committee drafted legislation which authorized the phase-out of NFIP subsidies and a bill was passed by the House of Representatives calling for reform in a 406-22 vote. [7]

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[6] Some of the most common mitigation strategies for residential buildings that would be considered a significant remodeling effort include "elevating a building to or above the area's base flood elevation, relocating the building to an area of lower flood risk, or demolishing the building and turning the property into green space" (GAO, 2014).

[7] See "Senate Banking Committee NFIP Bill Forgives Debt, Reforms FEMA Role" on PropertyCasualty360 available at http://www.propertycasualty360.com/2011/07/18/senate-banking-committee-nfip-bill-forgives-debt-r and published on July 18, 2011 and "Special report: Irene wallops flood insurance program" available at http://www.reuters.com/article/2011/08/31/us-storm-irene-flood-idUSTRE77T5M620110831

In order to place the burden of flood damages on those bearing flood risk Congress passed the Biggert-Waters Act of 2012 on July 6. Among other provisions the law mandated the elimination of subsidies in order to bring all insurance policies in line with actuarially defined "true-risk" rates. The implementation of full-risk rates began in January of 2013 with single-family non-principal residences and was extended to all other subsidized policies in October 2013. True-risk rates were charged either immediately upon the realization of certain triggers or phased in at an annual increase of 25% until full risk rates were reached, depending on the type of structure and policy held. Table 1.1 describes each policy type and the effect of BW12 on its insurance premium.

Table 1.1: Summary of Policy Types and Impact of Biggert Waters

Policy Code	Policy/Property Description	Effect of BW12	Effective Date
A	Any property sold and policies lapsed or new since enactment	Full risk rated on renewal	10/1/2013
B	Single-family non-principal residences	25% increase in premium rates each year until premiums reflect full risk rates	1/1/2013
C	Business non-residential	25% increase in premium rates each year until premiums reflect full risk rates	10/1/2013
D	SRL Pre-FIRM subsidized	25% increase in premium rates each year until premiums reflect full risk rates	10/1/2013
E	Single-family or condo unit principal residences	Full risk rated on sale, lapse, or SRL	10/1/2013
G	Two to four family	Full risk rated on sale, lapse, or SRL	10/1/2013
H	Five or more family	Full risk rated on sale, lapse, or SRL	10/1/2013
I	Condominium building	Full risk rated on sale, lapse, or SRL	10/1/2013
F	Non-pre-FIRM SRL	Non-subsidized	N/A
K	All others	Non-subsidized	N/A

The effect of this policy was estimated to increase the aggregate premium across all NFIP policies by 50% to 75%, while keeping unaffected policies unchanged (Hayes and Neal, 2011). However, since the roughly 1.1 million subsidized policies affected by the law make up only

from Reuters. A search of Google will turn up many more news articles from this period with the earliest indications of significant reform beginning in March 2011.

20% of the program, the burden of this substantial increase is heavily concentrated on a small proportion of policies. Based on the 2012 statistics this constitutes a net increase in premiums earned of $1,670,667,881 to $2,506,001,822, or an average increase of up to $2,278 in annual premium for each subsidized policy.[8] Given the substantial variation in policy coverage options and risk exposure, reported increases in annual premiums of up to $35,000 are not entirely unbelievable.

On January 1, 2013 the new rates began to be levied on the roughly 1.1 million policies affected by the new law. Initially, only homeowners with subsidized insurance rates on non-primary residences. On October 1, 2013 the provisions covering all subsidized properties took effect. Shortly afterwards the states that saw the greatest impact began experiencing political backlash. In the summer of 2013 Maxine Waters, one of the laws namesakes, spoke against the dramatic rate increase and soon after, along with twenty-six other congressional leaders, wrote to Congress in protest of the way BW12 had been implemented and called for changes to the bill.[9] Louisiana led efforts to have the provisions of BW12 modified or recalled. On January 30, 2014 the Senate passed a bill to delay certain flood insurance rate hikes and on March 21 the Homeowners Flood Insurance Affordability Act of 2014 (HFIAA) was signed into law by President Barak Obama. A summary of important flood insurance reform events is given in Table 1.2. The new legislation reduced the rate at which premiums would reach full-risk pricing but did not eliminate the move to full risk rates. Under HFIAA annual rate increases are capped

[8] These estimates are based on the 2012 Total Earned Premium of $3,342,335,762 (FEMA, 2015) and the findings of Hayes and Neal (2011).
[9] See "Waters vows action to avert 'unaffordable' premium hikes blamed on flood insurance bill" available at http://www.nola.com/politics/index.ssf/2013/05/co-author_of_flood_insurance_a.html and "Congressional letter asks FEMA to administratively block huge flood insurance hikes" available at http://www.nola.com/politics/index.ssf/2013/07/congressional_letter_asks_fema.html published in May and July of 2013, respectively by the by NOLA Media Group.

at 5% - 15% of the full risk premium and an individual annual cap of 18% was imposed. HFIAA also removed the sale of a property as a trigger for subsidy loss due in part to reports that it had frozen real estate markets in flood hazard areas.[10] While properties that fall into this category are exempt from rate increases under HFIAA, they initially faced the same provisions as all subsidized properties under BW12 so are included in the study. Furthermore, it is not unreasonable that they could expect to face some sort of rate increases in the future. In general, policy makers are likely to continue to pursue flood insurance reform due to increases in coastal development and changes in flood risk associated with climate change, making reform an area ripe for continuing study.

[10] Some of the most prominent reports were from Pinellas County, Florida.

Table 1.2: Summary of Major Events

Date	Event
May 1957	American Insurance Association Study states that private industry cannot provide flood insurance because only those at highest risk buy it
September 1965	Hurricane Betsy prompts the establishment of the NFIP in 1968.
December 1974	Prompted by Hurricane Agnes Congress makes flood insurance mandatory for certain federal programs
July 1983	FEMA begins allowing private insurers to write flood insurance policies underwritten by the Federal Government.
August 29, 2005	Hurricane Katrina makes landfall in Louisiana, catalyzing serious debate on reforming the National Flood Insurance Program.
March 16, 2006	Congress begins an effort to investigate the National Flood Insurance Program with a series of Flood Insurance Reform and Modernization Acts beginning with H.R.4973
April 22, 2010	With the beginning of the great recession flood insurance is left off the congressional legislative effort until the introduction of H.R.5114 - Flood Insurance Reform Priorities Act
July 2011	The Senate and the House each move in support of flood insurance reform. • The Senate Banking Committee approves FEMA debt forgiveness and mandates phase out of subsidies • The House votes 406-22 in favor of flood insurance reform.
April 16, 2012	The Biggert-Waters Act of 2012 is introduced (H.R. 4348)
July 6, 2012	The Biggert-Waters Act of 2012 is signed into law • Reauthorized the NFIP for 5-years • Introduced rate and map making reform measures
January 1, 2013	Premiums are increased 25 percent each year until reaching full-risk rates for: • Non-primary residences
March 19, 2013	Congress begins to reassess the reform measures implemented under Biggert-Waters when the Flood Insurance Premium Relief Act (H.R.1267) and Flood Mitigation Expense Relief Act (H.R.1268) are introduced.
June 2013	The U.S. House of Representatives passes a bill that would delay rate hikes 281-146[11]
October 1, 2013	Premiums are increased 25 percent each year until reaching full-risk rates for: • Severe Repetitive Loss properties • Properties with cumulative paid flood losses exceeding fair market value • Businesses/non-residential buildings Full-risk rates take effect upon renewal for: • Property purchased on or after July 6, 2012 • New policies effective on or after July 6, 2012 • Lapsed policies reinstated on or after October 4, 2012
October 29, 2013	The bill that would later become the Homeowner Flood Insurance Affordability Act of 2014 is introduced (H.R.3370).
March 21, 2014	Homeowners Flood Insurance Affordability Act of 2014 signed into law.

A Review of Prior Research

Evidence for housing price differentials for properties located around hazards and the role of insurance premiums in housing markets has been acknowledged in the literature since at least the mid-1980's (MacDonald, et al., 1990; MacDonald, Murdoch and White, 1987), while more recent research has continued to shed light on the subject. Bin, Kruse and Landry (2008) regress the log of home values on housing attributes and an indicator for flood risk using a pooled first-order spatial hedonic model and find evidence that flood risk information is conveyed to buyers in the coastal housing market through insurance premiums regardless of whether flood insurance is purchased or not. Their analysis suggests that housing markets adjust for flood risk signaled through flood insurance premiums on a neighborhood level, rather than by the characteristics of the individual structure. An implication of this is that changes in insurance premiums will affect entire neighborhoods, regardless of which structures are actually insured. The authors also indicate that pre- and post-FIRM properties are not valued differently, again suggesting that neighborhood level flood insurance signals overwhelm the structure-specific influence of age, the primary determinant of pre- and post-FIRM attributes. These results lend support to the idea that changes in flood insurance premiums may be capitalized by local housing markets and that a change in rates may lead to a measurable change in home values and indirectly support the idea that the effect of insurance changes on home values can be adequately measured on the neighborhood or zip code level as put forth in this paper. I expand on these findings by looking at differences in the capitalization of flood insurance between zip codes before, during and after the insurance premium price changes associated with BW12. Evidence of a change in value will support the finding that the insurance premiums have an impact on the market value of a property; though the mechanism for this could be the change in the cost of ownership itself, to the new level of risk it signals to the market or both.

Prior research has established a connection between flood risk or flood events and decreases in housing values. Daniel, Florax and Rietveld (2009) conduct a meta-analysis of studies across ten states in which they show that “an increase in the probability of flood risk of 0.01 in a year is associated with a difference in transaction price of an otherwise similar house of -0.6%” but does not identify whether the risk or insurance was the causal factor. In another study Bin and Landry (2013) identify temporary but significant decreases in housing values due to flood risk perception. Using a difference-in-difference framework they show increases in risk premiums for homes sold in and around the flood plains of Pitt County, North Carolina after Hurricane Fran in 1996 and Hurricane Floyd in 1999. They find that the value for flood-zoned properties decreased by 5.7% to 8.8% after Hurricane Fran and 8.8% to 13% after Hurricane Floyd with an overall decrease of 6% to 20%, depending on model specification, while the risk premium itself diminishes over time and dissipates completely after six years. Most properties in the study area received little to no damage and robustness checks revealed that results were due to risk perceptions rather than significantly damaged properties.

Similarly, a study of Alachua County, Florida by Harrison, Smersh and Schwartz (2001) find that homes located in a flood zone sell for less than homes located outside of flood zones. Significantly, the price differential within the flood zone is less than the present value of future flood insurance premiums. An implication of this finding is that changes in the expected value of future flood insurance premiums may be partially muted in the housing market. As a result of partial capitalization in the housing market observed changes may be less than the amount of the premium increase. It follows that home values in areas with the largest price increases will be most affected while areas with relatively small changes may not show measurable effects of the flood insurance reform.

Prior research has also explored the socioeconomic factors that contribute to flood vulnerability and NFIP participation with a general consensus that flood risk affects especially poor and especially rich counties due to the trade-off between amenities and flood risk.[12] This distribution leads to a possible disproportionate effect of insurance reform and affordability concerns depending on who holds the subsidized policies in the NFIP program (Kousky et al. 2013). Additionally, research has suggested that state-level analysis of the insurance program can "hide important local differences," implying that investigation of the effects of BW12 on the zip code level is a meaningful pursuit (Michel-Kerjan, 2010). Although this paper does not attempt to address the question of social equity, these findings motivate this study as they suggest that flood insurance reform could have substantial unintended consequences that have not been adequately addressed in the literature.

[12] See Masozera et al. (2007), Sarmiento and Miller (2006) and Holladay and Schwartz (2010).

CHAPTER TWO

METHODOLOGY

Identification Strategy

The basic mechanism by which an increase in flood insurance premiums realized through the reduction of NFIP subsidies may lead to changes in the median home value in a neighborhood is captured by the general hedonic model $p_{it} = f(\boldsymbol{l_{it}}, \boldsymbol{s_{it}}, \boldsymbol{m_{it}})$, where p_{it} is the median value of a home in neighborhood *i* in time *t*, $\boldsymbol{l_{it}}$ is a matrix of associated neighborhood specific location characteristics, $\boldsymbol{s_{it}}$ represents the structural characteristics of the median home in that area and $\boldsymbol{m_{it}}$ are market characteristics (Tu, 2005). The NFIP characteristics of a neighborhood, such as the number of subsidies, belong to $\boldsymbol{l_{it}}$ while the policy that governs the subsidies may be considered a market characteristic.

The identification strategy for this analysis consists of utilizing dummy or indicator variables to estimate different effects of variables on median home values for zip codes belonging to different groups. This is known as a difference-in-differences model. In the simplest terms the group identifier d_{it} is regressed on *y* so that $p_{it} = \beta_0 + \beta_1 d_{it} + \boldsymbol{l_{it}}'\boldsymbol{\tau} + \boldsymbol{s_{it}}'\boldsymbol{\pi} + \boldsymbol{m_{it}}'\boldsymbol{\vartheta} + \varepsilon_{it}$ where $d_{it} = 1$ indicates an estimate of p_{it} from neighborhood *i* which belongs to some group d in time *t* and $d_{it} = 0$ indicates the neighborhood is not a member of the group in that period. The average effect on p_{it} for a non-member is just β_0 while β_1 reveals the average difference in this value that membership imparts. Thus the equation yields two conditional estimates for y_{it} depending upon whether the neighborhood is a member of the group in time *t*: $E(p_{it}|d_{it} = 0) = \beta_0 + \boldsymbol{l_{it}}'\boldsymbol{\tau} + \boldsymbol{s_{it}}'\boldsymbol{\pi} + \boldsymbol{m_{it}}'\boldsymbol{\vartheta}$ when group membership is false and $E(p_{it}|d_{it} = 1) = \beta_0 + \beta_1 + \boldsymbol{l_{it}}'\boldsymbol{\tau} + \boldsymbol{s_{it}}'\boldsymbol{\pi} + \boldsymbol{m_{it}}'\boldsymbol{\vartheta}$ when membership to the group is true.

Subtracting the former conditional expectation from the latter gives β_1; the difference in the conditional expectation of p_{it} for positive group membership. Where the characteristic defining group membership is the presence of some condition the group is known as the treatment group. Since the total effect on the treatment group is relative to the total effect on the non-treatment group, the latter is referred to as the reference group when $d_{it} = 0$.

The basic model begins by identifying two types of neighborhoods represented by the indicator Sub_i. Zip codes that are directly affected by the BW12 provisions on subsidized policies have subsidized NFIP policies in force and are represented by a positive indication, $Sub_i = 1$, and zip codes that are not directly affected have some number of NFIP policies in force, but none are subsidized, are identified by $Sub_i = 0$. The basic model also begins by distinguishing three time periods that an estimate can come from. The first two policy periods are denoted with indicator variables. An estimate from the period between when BW12 was passed to its repeal is indicated by $BW_t = 1$ and the post repeal period is indicated by $Post_t = 1$. The base period is the reference period, where both BW_t and $Post_t$ are zero. Since all zip codes are always in the same time period these do not need to be distinguished by zip code. This model hypothesizes that some effect on housing markets occurred after passage and all the way through to repeal and is represented:

(Eq. 2.1)

$$p_{it} = \beta_0 + \beta_1 Sub_i + \beta_2 BW_t + \beta_3 Post_t + \beta_4 (BW_t * Sub_i) + \beta_5 (Post_t * Sub_i) + \boldsymbol{X_{itk}}'\boldsymbol{\gamma} + \varepsilon_{it}$$

where $\boldsymbol{X_{itk}}$ is a matrix containing the zip code attributes $\boldsymbol{l_{it}}$, $\boldsymbol{s_{it}}$ and $\boldsymbol{m_{it}}$.

In this framework β_2 and β_3 give the average difference in median home value during the treatment window and after the treatment period, respectively. The β_4 coefficient allows the marginal change in median home values to be different for zip codes directly affected by the treatment from passage to repeal. This is expected to be negative if removing subsidies affected housing markets with subsidies different than those without. After BW12 is repealed the change in median home values for areas with subsidies, represented by β_5, is expected to be less than β_4 if HFIAA was seen as a benefit to subsidized areas. This model does not control for estimation issues arising from panel data dimensions, which are discussed in the following section.

CHAPTER THREE

DATA AND CONTEXT

This section describes the three main types of data used in the study, real estate data, flood insurance data and zip code characteristics. This study uses the series of recent flood insurance reforms of the Biggert-Waters Act and the Homeowners Flood Insurance Affordability Act to study the effect of flood insurance premiums on median home values. The primary question up for investigation is whether a measurable change in property values is detectable before, during or after BW12 due to the changes in flood insurance subsidy administration. Data on home values is obtained from the Zillow Group while flood insurance information is provided by FEMA. This section also presents some of the short comings of the data and the assumptions necessary to utilize it for the study.

Real Estate Data

The dependent variable for the analysis is the median value of homes in a neighborhood. Data for the dependent variable comes from a collection of monthly time series of estimated median home values for a panel of zip codes. The values are estimated with proprietary statistical techniques by the Zillow Group and are accessible on their Real Estate Research website. The series is known as the Zillow Home Value Index (ZHVI) and is available at several geographic resolutions and for a variety of market segments. I utilize the ZHVI on the zip code level covering all home categories including single family residences, co-ops and condominiums from January 2010 through May 2015. Zillow property value estimates in real estate research have appeared as both dependent and independent variables in peer reviewed and scholarly publications.[13] The ZHVI itself has also been the subject of study. While accuracy has been shown to vary there is a general consensus that it provides reasonably accurate estimates for an aggregate analysis of property values.[14] Hagerty (2007) in particular found a 7.8% median margin of error in Zillow estimates that was not systematically under or over estimated. The accuracy of Zillow estimates was also shown to differ for outliers and less densely populated areas. Gelman et al. (2011) find user submitted information on Zillow improves completeness but is not always accurate.

The ZHVI provides estimates of median home value in order to provide data that is not influenced by the characteristics of the particular homes sold in a given period. This validates the assumption that median structural characteristics can be eliminated by mean-differencing. By tracking full-value, arms-length sales that are not foreclosure resales and applying machine learning techniques the ZHVI aims to provide consistent home value estimates. The index is estimated using the same set of homes in each period, thus keeping the sales mix constant across time, while allowing characteristics to vary across zip codes. Important housing attributes that are considered in the model include physical facts about the home and land such as structure type and number of rooms, prior sale transactions, tax assessment information and geographic location. As an estimate, this data is subject to estimation error; however, Zillow assures that there is minimal systematic error. In other words, the "error is just as likely to be above the actual sale price of a home as below" (Zillow Real Estate Research, 2014). Other modifications

[13] See Kay et al. (2014) and Huang and Tang (2012) for examples of peer reviewed literature and Garriga (2013), Qiao et al., Stratmann (2013), Frey et al. (2013), Morris-Levenson (2014), Keating (2014), Cronqvist and Yonker (2009) and Guerrieri et al. (2013) for scholarly articles.

[14] See Kay et al. (2014) and Frey et al. (2013) for comparisons of Zillow data to actual sales in regression analysis; Hagerty (2007), Hollas et al. (2010) and MacDonald (2006) for accuracy of Zillow estimates; and Ma and Swinton (2012), Kim and Goldsmith (2009) and Clapp and Giaccotto (1992) for the use of assessed values in general.

of the data include the application of a five-term Henderson Moving Average Filter to reduce "noise" and seasonal adjustments. The net result is a median home price index comparable across both space and time. A graph of the average median home value across all zip codes reveals a strong upward trend in median home values is clearly visible beginning in March 2012 (Figure 3.1). Prior to that date median home values had been decreasing. The graph also includes the average change in median home values, revealing that the increase in median home values slowed considerably after August 2013, though the marginal growth rate remained positive. It is apparent from this graph that the trend in median home values over the course of this study changes over time.

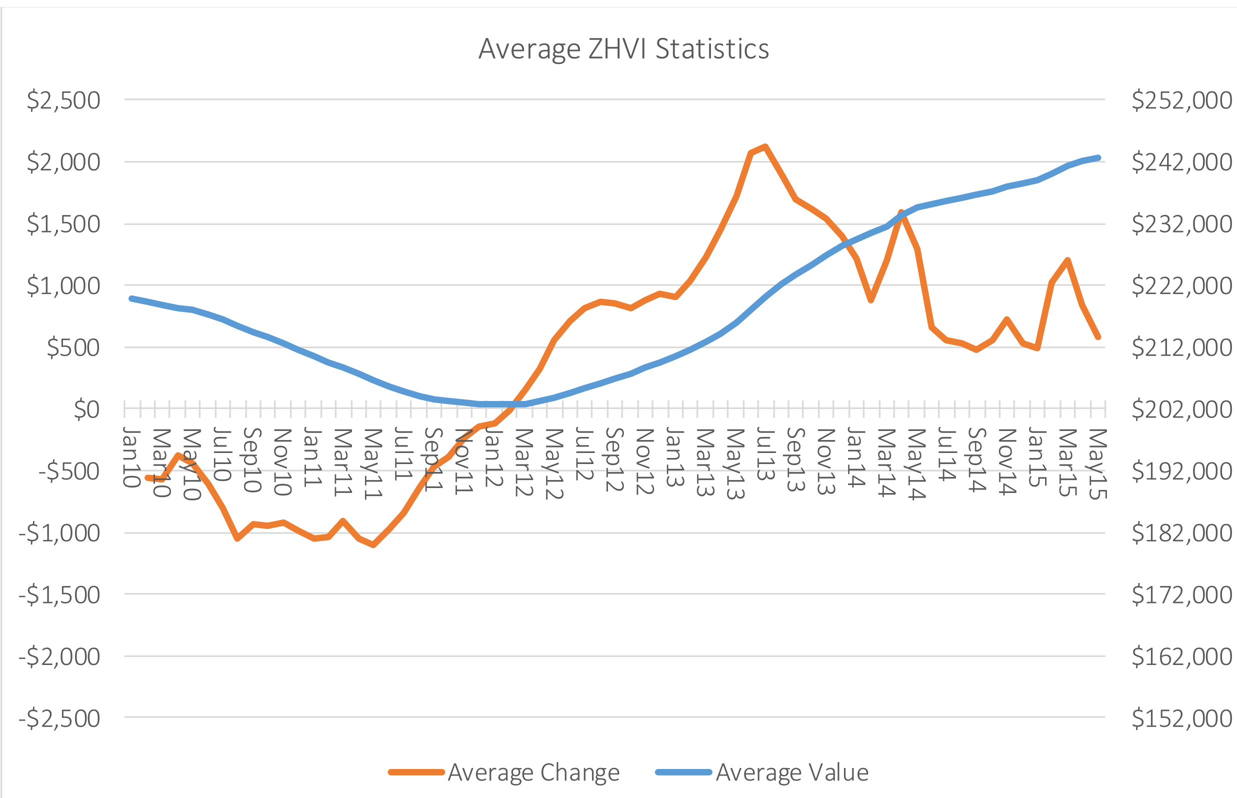

Figure 3.1: Average Median Home Value and Average Change in Value across All Zip Codes

Overall, Zillow provides data for 24,460 zip codes while NFIP data is available for 33,144 including American territories such as Guam, Puerto Rico, the U.S. Virgin Islands and American Samoa. The total viable overlap of the two datasets is for 12,091 zip codes. Overall, much of the geographic area of the United States is not covered by this study but over 76% of the continental U.S. housing market is covered by population.[15] The study covers primarily metropolitan areas (Figure 3.2). As a result generalizations to rural areas may not be made with the same confidence as to metro areas.

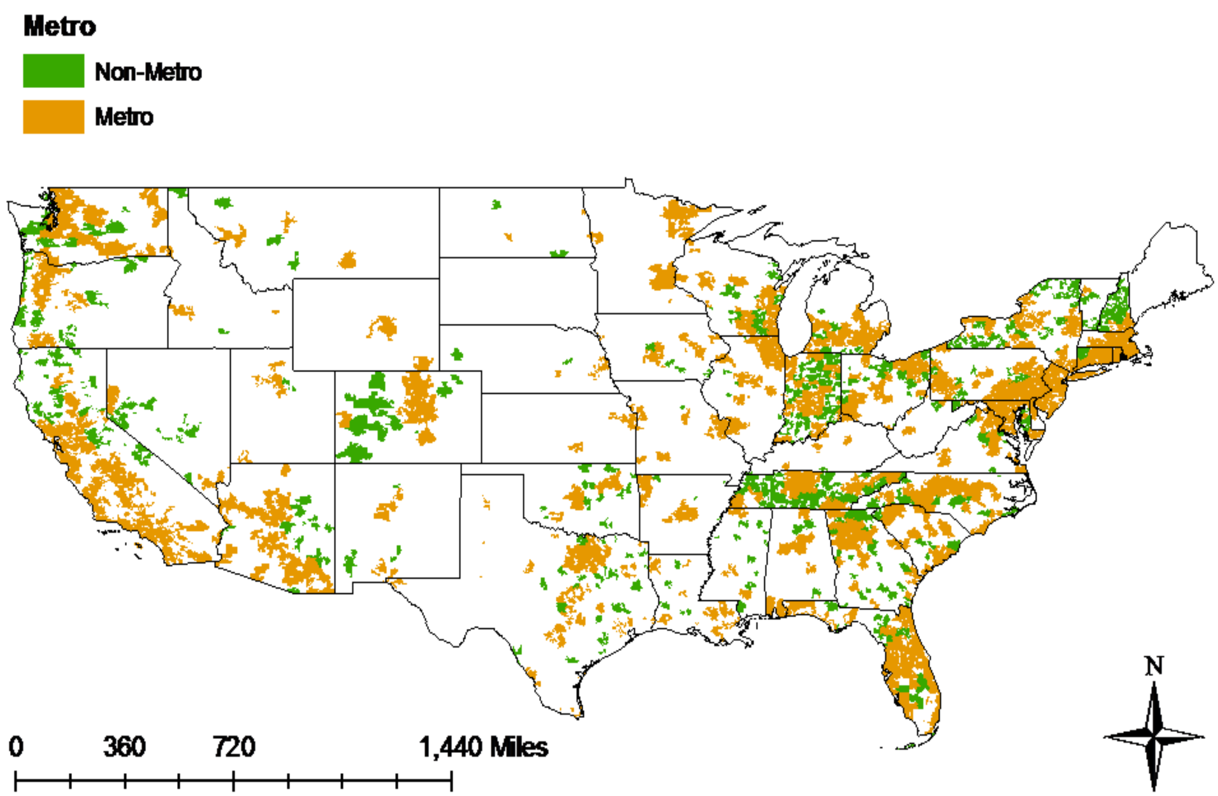

Figure 3.2: Study Scope by Metro and Non-Metro Distinction

[15] Based on the 2013 American Community Survey population estimates.

Flood Insurance Data

Data on the National Flood Insurance Programs policies in force comes from a cross-sectional dataset provided by FEMA.[16] The dataset includes counts of total NFIP policies by zip code and total subsidized and unsubsidized policies for the month BW12 was passed. Subsidized policies are further classified by subsidy type allowing for the identification of how and when it would be affected by BW12. Only zip codes with data on both median home values from Zillow and flood insurance policies from FEMA were included in the study. A complete breakdown of how the zip codes are categorized is presented in Table 3.1.[17]

Table 3.1: Number of Zip Codes by Type Included in the Study

Policy Type	Policy Definition[18]	Zip Codes
Subsidized non-primary, business and SRL 25% increase until true risk	B + C + D	3,657
Subsidized condo and multi-family - keep subsidy until sale, lapse, severe repeated flooding	G + H + I	3,889
Subsidized other, keep subsidy until trigger	A + E	3,159
Total subsidized policies (affected by BW12)	A + B + C + D + E + G + H + I	9,825
Total non-subsidized (not affected by BW12)	F + K	2,266
Total NFIP policies in force[19]	Z + O + P	12,091

While the majority of NFIP policies are concentrated in Florida, Louisiana, Texas and New Jersey, dividing the total number of policies in force by the total number of housing units in an area makes it apparent that the program is prevalent throughout the country and not just in coastal areas. Figure 3.4 illustrates the division by quantile. Areas in the top 20% of policies per

16 While some policy data with a time series component is available on the FEMA website detail needed for this study is not. A Freedom of Information Act request may be able to locate the desired information but was not feasible given the time constraints.
17 Only 2.5% of the data covered zip codes that did not participate in the NFIP at all, so they are excluded from the regression analysis as described in the methodology section. Also, 133 zip codes affected in January but not October are excluded so as not to have to control their influence in October. Since the 6,067 areas affected in January are the exact same type removing these 133 shouldn't affect the estimate. Additionally, 150 zip codes are removed because of missing data on median home values.
18 These categories refer to the definitions presented in Table 1.1.
19 Multiple policy types are present in some zip codes so that the total subsidized is less than the sum of its parts.

housing unit are scattered about the continental U.S. from Florida to Nebraska to Washington State. As previous studies have suggested, changes to the program are neither an exclusively coastal nor an exclusively metro issue. It is also apparent that the subsidies are distributed widely. Areas with the highest proportion of NFIP subsidized are all over the U.S., with some of the highest rates in the Great Lakes area (Figure 3.4).[20] Florida has low rates of subsidies, despite having large numbers of subsidized policies in force, because of large numbers of NFIP policies in general. Simply having a large number of policies does not mean a low subsidy rate though, and vise versa; the correlation between these two measures is just -4%.

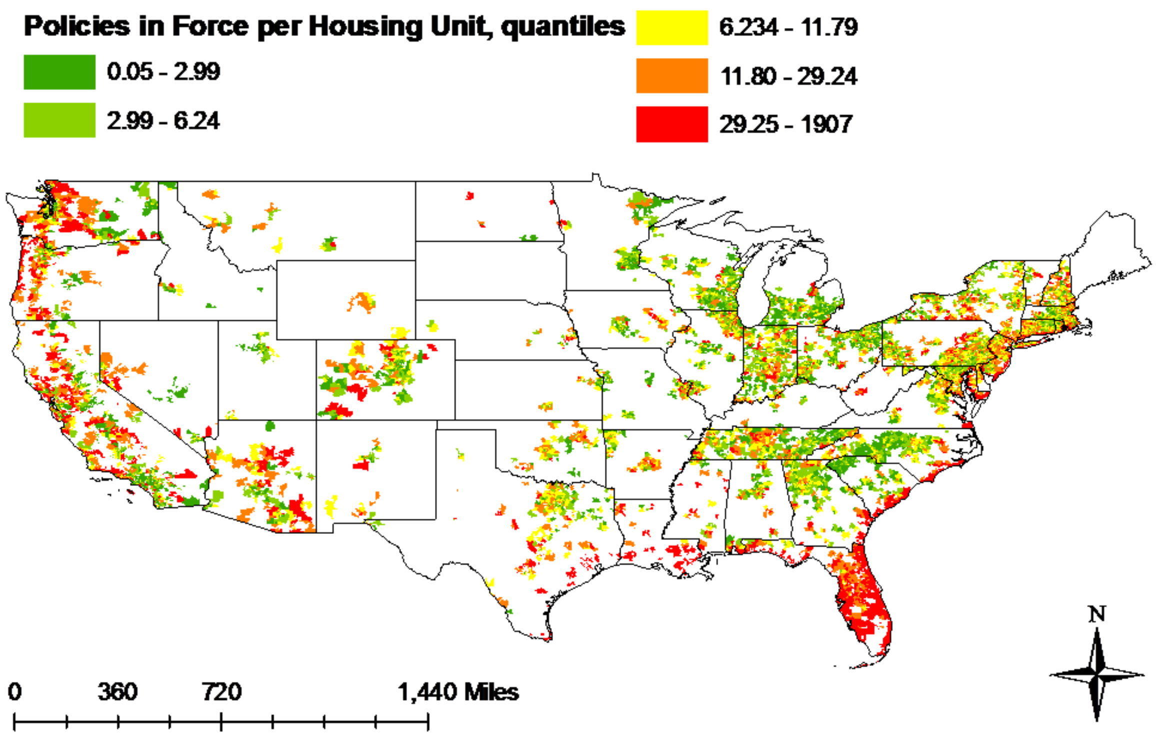

Figure 3.3: Policies in Force per 1,000 Housing Units

[20] Note that this includes business policies under the National Flood Insurance Program so that the number of policies per housing unit could be greater than one.

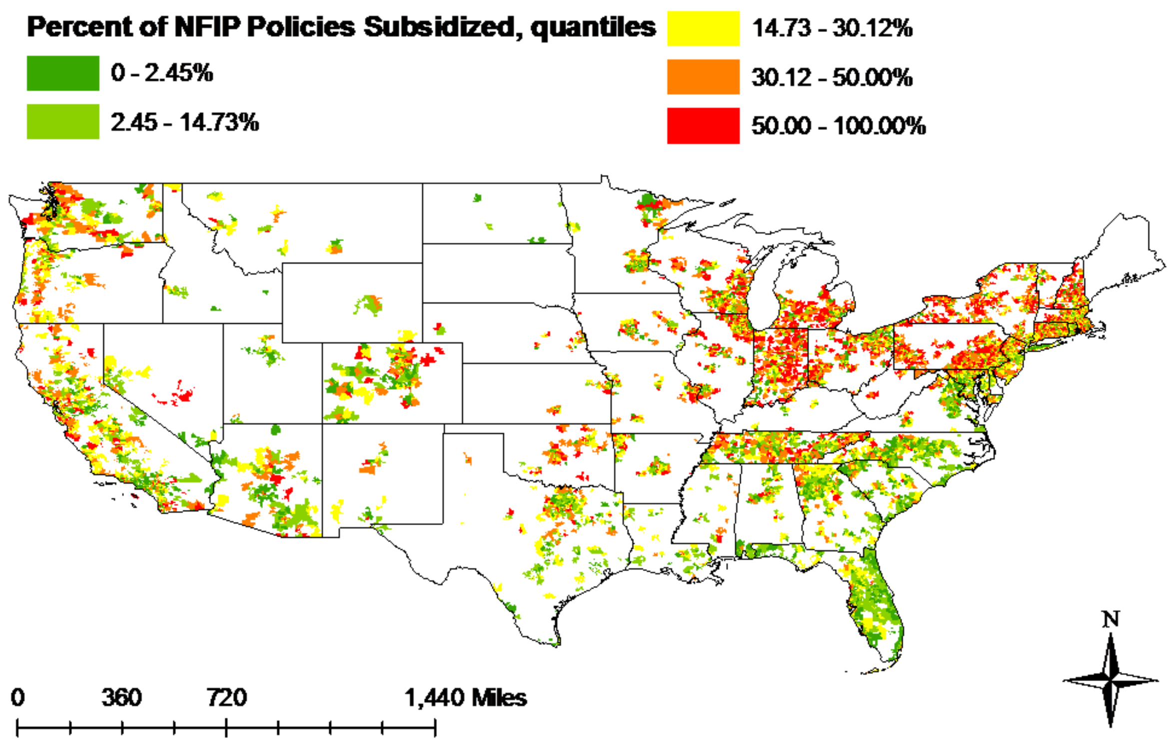

Figure 3.4: Percent of Policies in Force with Subsidy

The use of a cross-sectional dataset in a time series analysis requires some potentially significant assumptions. If an unobserved change in policy data is correlated with explanatory variables, such as the treatment periods, and the value of a median home this would be captured by the error leading to a form of endogeneity bias caused by the omitted values of the NFIP series. Omitted variables may also lead to inefficient OLS estimation. The hypothesis of this study is that the number of policies is correlated with median home values and it is expected that the BW12 reforms impacted the values. For the impact of the omitted variables not to be significant it must be assumed that the change in policy counts is not correlated with any of the included variables. This does not seem to be the case. Figure 3.5 shows how total policies in force have changed since 1990. The data indicate that policies in force increased until around 2008. Between 2008 and 2014 the number of NFIP policies in force was relatively steady however a decrease can be seen around the time of BW12. It is reasonable to assume that property owners maintaining voluntary NFIP policies may have dropped coverage in response to the policy. Thus the limited temporal dimension of the data likely induces an omitted variable problem.

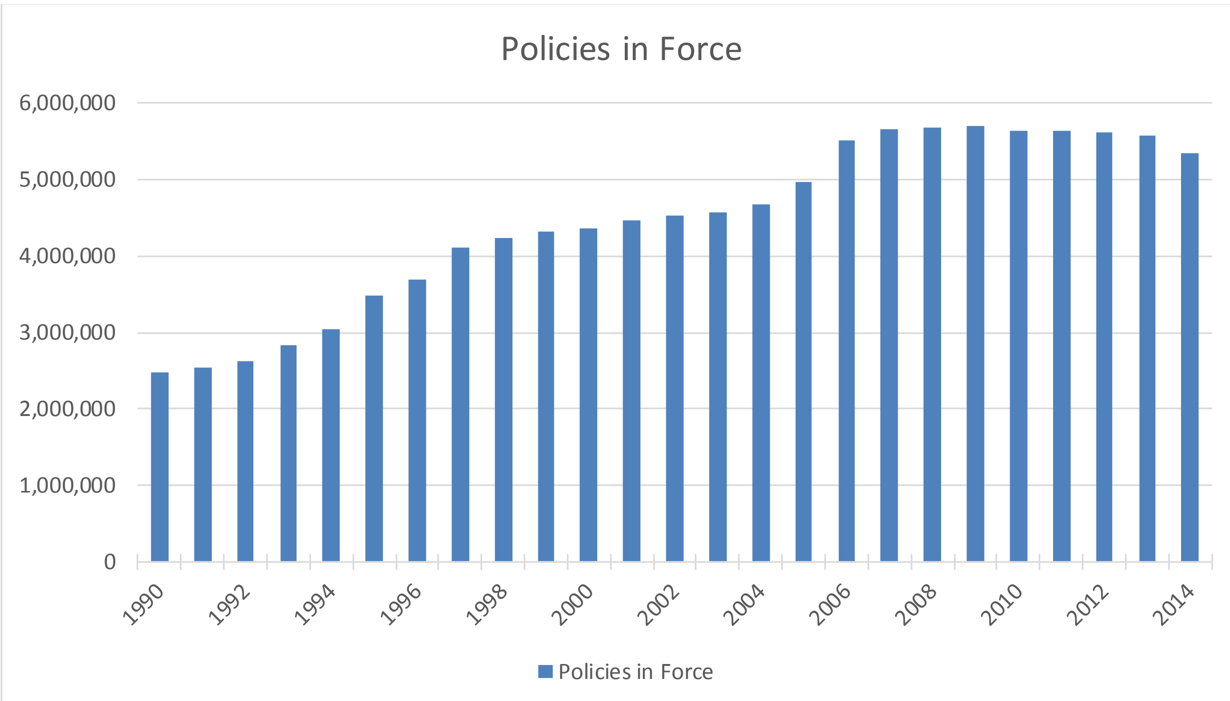

Figure 3.5: Total Policies in Force over Time

It is assumed that a causal link exists between the reduction in policies in force and the BW12 reform leading to some omitted variable bias: as owners of subsidized structures realized the extent to which the loss of the subsidy would increase their premium some of those policy holders would elect to discontinue coverage. While this is an empirical question the data to assess this is not available. Addressing omitted variable bias requires the inclusion of the variable or a closely related instrument and none was found. The question is now: how serious is this problem? It seems unlikely that whole communities would have elected to leave the program entirely. I make the assumption that no zip codes changed group membership over the course of the study due to BW12 so that no bias exists when using indicators to represent zip code group membership.[21] When using continuous measures of the number of policies in force the unobserved change over time the reduction is due to mainly to those with subsidized NFIP program dropping their policies. In this case the bias is expected to lead to underestimation of the effect of the treatment periods on median home values because of negative correlation between the number of subsidies and median home values during the treatment period. It is also expected to be relatively small; from 2011 to 2014 the total change in policies in force was roughly 5% with most of this change taking place from 2013 to 2014. While this is certainly not an insignificant change, it is not extremely large.

Additional Data

It is assumed that certain location specific variables do not change significantly over the course of the study; however, the five year time period is long enough that this may not be accurate for some of these variables. Factors such as actual flood risk exposure, demographic composition, quality of schools, and access to natural amenities and political representation can change over time. Time invariant differences between zip codes can be captured with zip code specific fixed effects for variables that are time invariant by adding a zip code specific intercept

[21] The boundaries of Flood Insurance Rate Maps could change over the study period and impact subsidies within a community but changes are made infrequently and new subsidized policy creation is assumed to be minimal. In general, map revisions are small and encompass areas considerably less than a zip code. Additionally, one of the provisions of BW12 was to reassess and update FIRMs with new technology since so many were outdated and this process takes time. After reassessing flood risk the maps have to be drawn up and presented to the community for public debate and questioning. A 90-day appeal period is mandatory. Additional changes to premium rates under BW12 occur upon remapping, but the provision calling for these premium rate changes was not to be implemented until the latter half of 2014.

α_i to the model, which may or may not be observed, and an associated individual specific error term v_i, which is not. The general form of the model in equation 2.1 now becomes:

(Eq. 3.1)

$$p_{it} = \alpha_i + \beta_0 + \beta_1 Sub_i + \beta_2 BW_t + \beta_3 Post_t + \beta_4(BW_t * Sub_i) + \beta_5(Post_t * Sub_i) + \boldsymbol{X'_{itk}\gamma} + v_i + \varepsilon_{it}$$

where ε_{it} is assumed to be an idiosyncratic observation-specific zero-mean random-error term. The total error term $v_i + \varepsilon_{it}$ is subject to the OLS assumption that model errors are uncorrelated with the regressors for unbiased estimation of model parameters. Thus it is critically important that the unobserved individual error v_i is uncorrelated with any variable in the model. To remedy this problem a fixed-effects model applying mean differencing is utilized to remove the heterogeneity effect and its associated error from the model. By doing so the time invariant heterogeneity parameters $(\alpha_i - \overline{\alpha_i})$, $(v_i - \overline{v_i})$ and $(\beta_1 - \overline{\beta_1})$, drop out of the equation.

Additional information on relevant neighborhood specific location characteristics and market characteristics are collected from various sources. The inclusion of structural characteristics of the median home are not necessary because Zillow holds this constant. Location specific attributes are both demographic and geographic. The latter attributes of interest include whether or not the zip code is coastal and the nature of flood risk in that area. While the flood zone ratings are generally available they must be accessed by individual community and stitched together. Since flood zones do not correspond to zip codes this was beyond the scope of available resources. Miles of rivers and stream, acres of lake and a coastal dummy are used to proxy flood risk. These measures provide insight into the type and quantity of risk faced. Since relatively large areas are able to contain larger bodies of water and more miles of river total square miles is included to control for the size of a zip code.

Demographic variables of interest fall into four broad categories: race, income, poverty and educational measures. Monthly series for these variables are not available. Local demographic attributes of each zip code can be obtained on the zip code level from the U.S. Census Bureau 5-year American Community Survey for 2013. However, this dataset only provides one cross-sectional estimate for the entire study period. As a result time variant location characteristics are not included in this study. Additionally, they cannot be assumed to be uncorrelated with the regressors because they include time so omitted variable bias may be present if they determine median home values. Whether certain attributes vary significantly over the course of the study period is not clear but is an empirical issue. In short panel series characteristics like the quality of education, demographic composition and political representation can be thought of as constant. The longer the study the less this assumption may hold. It is expected that any bias is small for several reasons including the medium length study period. Because time trends will be accounted for general changes in location characteristics or how they affect median home values is important only in so far as they differ between zip codes. While this is likely, it is less hazardous to assume that changes differ between groups of zip codes. Participation in the NFIP program is voluntary so the study only compares areas that participate in the program. While what areas have and do not have subsidies are not necessarily random there is no direct selection into the group so correlation between group membership and unobserved location characteristics may not be large.[22] Finally, the collinearity of time variant

[22]Receiving subsidies was not a voluntary option selected by communities but depended on when the area began the program and how recently homes have been drawn into a new flood plain. An indirect selection bias based on factors that contributed to early participation may be present, but for many communities receiving subsidies the initial FIRM would have been adopted in the 1970's and 1980's so time variant factors that would have contributed to initial participation would likely be different today. A characteristic such as flood risk exposure may lead to early participation. It's variability over time is not clear. The drawing of new or adjusted flood maps is assumed not to be related to time variant location characteristics.

and time invariant location characteristics or any other included variable would diminish this bias. Ultimately none of these arguments refute the presence of endogeneity due to location characteristics that change over time and future research may include some measures of these.

Market characteristics are captured by the monthly average of the S&P500 Index. Additional market conditions common for all areas are captured by a monthly fixed effect θ_t which is a month specific intercept that measures the common difference in median home values across all zip codes in month t. To address unobserved differences over time within each zip code cluster robust standard errors are reported for all results when available. The robust standard errors relax the assumption that observations within a group are independent and allows limited autocorrelation of errors within each area.

A visual inspection of average median housing values in Figure 3.1 clearly shows the presence of a general upward trend over time. Tests reveal that the series is deterministic, or has a unit root, meaning that there is a nearly one-to-one relationship between $ZHVI_{it}$ and $ZHVI_{i,t-1}$. A unit root results in inefficient estimation of the model and the solution to this problem is to difference the series by its previous value.[23] While a visible trend remains in the first differenced trend line the difference is found to be stationary.

An additional dimension not unique to panel data is space. To assess how the two areas differ within a difference-in-differences framework the assumption that the housing markets in each group are independent and that housing trends are similar is required. In regards to the issue of independence this may not always be the case. Because zip codes tend to be small housing

[23] Most previous literature using Zillow estimates has used a log transformation; however, these studies often pool values from different years or consider a very short time period. See Kayet al. (2014), Stratmann (2013), Fret et al. (2013), Keating (2014) for log transformations. Morris-Levenson (2014) use the change in city level ZHVI as a dependent variable while Morris-Levenson (2014) uses the change in ZHVI, but as a dependent variable. See Appendix I: Stationarity for a discussion on this result.

values in one zip code may have direct or indirect effects on the housing values of nearby areas. Theoretically, spatial autocorrelation in housing markets may arise from two sources: common neighborhood characteristics that cast influence across subject boundaries and spatial spillover of housing prices between neighbors (Can, 1992). For example, zip codes may share the same school district, county services and local amenities that would exert a similar influence on median home values in separate but nearby locations. Additionally, nearby areas could serve as substitutes for one another resulting in a convergence of value across borders.[24] Correcting for spatial lags or spatial errors can lead to improvements in parameter estimation over standard OLS; a few studies that have done so have favored the spatial error model more often than not.[25] Data descriptions and summaries for all variables are presented in Table 3.2 and Table 3.3, respectively.

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[24] If the nature of these spillovers are correlated with group membership this would introduce an omitted variable problem. Therefore in the absence of spatial econometrics it is necessary to assume spillovers are present for both groups and do not depend on group membership.

[25] See Kay et al. (2014), Frey et al. (2013) for spatial error models, Tu (2005) for spatial lag and Feng and Humphreys (2008) for comparison of OLS, MLE and spatial 2SLS.

Table 3.2: Summary of Variable Definitions

VARIABLES	DESCRIPTION
Dependent Variables	
$ZHVI_{it}$	Zillow estimated median home values for zip code *i* in time *t.*
$\Delta ZHVI_{it}$	The first-difference of $ZHVI_{it}$
Dependent Variables	
PIF_i	Total number of NFIP policies in force in zip code *i* in in July 2013.
O_i	Total number of subsidized NFIP policies in force in zip code *i* in in July 2013.
Sub_i	Indicator equal to one if zip code *i* had subsidized policies in force in July, 2013; 0 otherwise.
$Pass_t$	Indicator equal to one if the estimate is from the pre-enactment phase of Biggert Waters: July, 2012 to January, 2013; 0 otherwise.
BW_t	Indicator equal to one if the estimate is from the entire Biggert-Waters period from passage to repeal: July, 2012 to April, 2014; 0 otherwise.
Act_t	Indicator equal to one if the estimate is from the first enactment period of Biggert-Waters provisions: January 2013 to March 2014; 0 otherwise.
$Post_t$	Indicator equal to one if the estimate is from the post repeal period of Biggert-Waters, also the time the Homeowners Flood Insurance Affordability Act was in place: April, 2014 to May, 2015; 0 otherwise.
SP_t	The average value of the S&P500 Index for month *t.*
THU_i	Total housing units in zip code *i* in the 2013 5-year American Community Survey.
$Year_i$	Median year built for the homes in zip code *i.*
$Coastal_i$	Indicator equal to one if zip code *I* is coastal, zero otherwise.
$MilesRS_i$	Miles of rivers and streams in zip code *i*
$AreaLand_i$	Land area of zip code *I* in square miles
$Metro_i$	Indicator equal to one if zip code *i* was considered a metro area by the 2013 Rural Urban Influence Continuum; 0 otherwise.

Table 3.3: Summary of Variable Statistics

VARIABLES	N	Mean	Std.Dev.	Min	Max
$ZHVI_{it}$	12,091	218,547	193,665	26,000	5.300e+06
$\Delta ZHVI_{it}$	12,091	368.2	2,757	-68,300	154,000
PIF_i	12,091	346.42	1290	1	28,834
O_i	12,091	64.24	318.1	0	10,194
Sub_i	12,091	0.813	0.390	0	1
SP_t	12,091	1,525	321.7	1,097	2,112
THU_i	12,091	8,045	6,424	46	40,274
$Year_i$	12,081	1974	15.41	1939	2007
$Coastal_i$	12,091	0.1552	0.3621	0	1
$MilesRS_i$	12,091	13.79	27.82	0	687.78
$AreaLand_i$	12,091	48.02	91.93	0.0712	2,047
$Metro_i$	773,824	0.881	0.323	0	1

CHAPTER FOUR

RESULTS

This study aims to address whether flood insurance reform, specifically the Biggert-Waters Act of 2012, had a measurable impact on housing values. This section presents a series of models and results to test the robustness of each model to alternate specifications.[26] The first section addresses Research Question 1 which attempts to identify if a difference in trend between areas with and without subsidies exists. The first subsection of section one presents two models using indicator variables for membership into policy participation groups and treatment periods; the second presents a more flexible analysis of the treatment period; subsection three accounts for robustness to an alternate first-order dependent lag model; subsection four accounts for first-order autocorrelation of the error process; finally, subsection five accounts for spatial correlation of the errors. The second question concerns whether this effect is unique to the definition of the treatment group or if alternate specifications produce similar results. The first section of section two randomly specifies membership to the treatment group, the second removes the four states affects the most by the real estate collapse, a coastal distinction is drawn third, the fourth subsection considers the median age of the housing stock and finally results are presented for low, mid and high quantiles of subsidy measures.

Research Question 1: Is there a difference in median home values between areas with and without subsidies associated with changes in the National Flood Insurance Program?

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[26] A similar approach of using multiple specifications to check robustness is taken by Tu (2005), Kay et al. (2014), Dye et al. (2014), Frey et al. (2013) and Huang and Tang (2012).

Basic Models

So far this study has identified two types of zip codes, areas that participate in the NFIP with and without subsidized policies in force, and three time periods, before passage, from passage to repeal, and after repeal. This section provides results for this model as well as an alternate specification of the time periods. In each model the reference group are areas that have no subsidized NFIP policies in force. This group serves as control group for the effect of the policy because it is not directly affected by the new conditions governing subsidized rates. The treatment group are zip codes that contain some number of subsidized NFIP policies; they are directly affected by BW12s subsidy provisions. The model that has been presented so far is reproduced in its entirety as Equation 4.1 and will be referred to as Model I.

(Eq. 4.1)

$$\Delta p_{it} = \alpha_i + \theta_t + \beta_0 + \beta_1 Sub_i + \beta_2 BW_t + \beta_3 Post_t + \beta_4 (BW_t * Sub_i) + \beta_5 (Post_t * Sub_i) + \boldsymbol{X'_{itk}\gamma} + v_i + \varepsilon_{it}$$

Model I expresses the change in the median estimated home value of zip code *i* in month *t* as a function of policy variables, treatment periods, location specific characteristics, market conditions and monthly (θ_t) and individual (α_i) fixed effects. The time invariant variables, noted by the absence of a *t* subscript, will drop out during mean-differenced estimation. The coefficients of interest are β_4 and β_5. The former measures the average difference in the change in median home values for zip codes that had subsidized policies in force in July 2013 during the period from passage in that month to repeal in April 2014.[27] If the provisions on subsidies in BW12 had an effect on median home values this coefficient is expected to be significant. Specifically, it is expected to be negative. This would reflect that the change in median home

[27] Repeal was officially on March 21, 2014.

values over this period was on average less than that in areas without subsidies. For β_5 the expectation is less clear. If it is negative it would reflect that subsidized areas still had a rate of change in median home values below that of areas without subsidies. It is expected that the elimination of BW12 is a benefit, but it is not clear that that the Homeowners Flood Insurance Affordability Act is also a benefit or simply less of a bad thing. If the latter this coefficient will still be negative but should be greater than β_4, if the former it may be insignificant or postivie.

Model II is represented by equation 4.2 and expands on Model I by allowing the period before enactment but after passage to be uniquely defined. This is done by dividing BW_t into two parts. The first part is the period from passage to enactment $(Pass_i)$ and the second part is the entire period that BW12 was in effect (Act_t).

(Eq. 4.2)

$$\Delta p_{it} = \alpha_i + \theta_t + \beta_0 + \beta_1 Sub_i + \beta_2 Pass_i + \beta_3 Act_t + \beta_4 Post_t + \beta_5 (Pre_t * Sub_i) + \beta_6 (Act_t * Sub_i) + \beta_7 (Post_t * Sub_i) + \boldsymbol{X'_{itk}\gamma} + v_i + \varepsilon_{it}$$

The coefficient on the interaction between Sub_i and Act_t has a similar interpretation to its interaction with BW_t in Model I except that it specifically defines the period in which the BW12 provisions are in place. The coefficient on the $Pass_i$ interaction is expected to be negative if the effects of the BW12 provisions were anticipated in areas with subsidies in place prior to enactment and insignificant otherwise.

Results for Models I and II and are presented in Table 4.1. Before examining the coefficients of interest it is worth nothing that the coefficients on the treatment period are positive and significant.[28] The positive sign indicates an increase in the change in median home

[28] To test the robustness of the models to the specification of the time effect these models were run with continuous time variables instead of fixed effects. Polynomials up to order five were included. All time trends were significant and the treatment periods remained significant in each of the new specifications although the coefficients reduced in magnitude. The estimates and standard errors on the time period policy interaction

values over these periods. Their interaction with the subsidy group indicator is consistent in all models. Each one shows that the change median home values in was less in every period, including the pre-enactment and post-repeal phases. The differences between all treatment and group interactions is significant in both models with p-values of 0.0000. This confirms that the change in median home values for subsidizes areas is significantly less than those without subsidies in each treatment period. The greatest difference is seen in Model II when the whole enactment period is taken together.

Table 4.1: Difference in Changes in Median Home Values, Results for Models I and II

VARIABLES	Model I	Model II
Policy Period		
BW_t	2,582***	
$Pass_t$		1,774***
Act_t		3,040***
$Post_t$	1,257***	1, 572***
Policy Period Subsidy Interaction		
$BW_t * Sub_i$	-668.5***	
$Pre_t * Sub_i$		-584.5***
$Act_t * Sub_i$		-702.1***
$Post_t * Sub_i$	-344.0***	-344.0***
Constant	-372.5***	-372.5***
Observations	773,824	773,824
R-squared	0.141	0.141
Number of Zip Code	12,091	12,091

Robust standard errors *** p<0.01, ** p<0.05, * p<0.1
Note: The dependent variable is Δp_{it}. Individual and monthly fixed effects and dSP_t are included but not reported.

The coefficients represent the average monthly difference in the change in median home values for members of the group with subsidies from those without over the respective time

parameters did not change, indicating the policy effect is robust to the specification of the time trend. Overall, the continuous time models explained slightly less of the variation with R-squares from 0.118 to 0.134.

period. In each model the largest effects are seen during the treatment window but Model II makes it clear that the effect was largest in the enactment period rather than the passage to enactment phase. The change in median home values during enactment was approximately $700 less per month in areas with subsidies. Median home prices grew $585 slower per month before enactment and $344 slower after repeal. For the entire period from passage to repeal home values grew about $670 slower per month. These findings are consistent across models I and II.

Robustness of the Trend: Monthly Treatment Periods

In this section I expand the subsidy time period interaction term by interacting Sub_i with an indicator for each individual month after the July 2012 passage of BW12 to see how the change in median home values actually differs for each month.[29] This gives insight into the data by adding flexibility to the model and reveals the difference trend between areas with and without subsidies relative to the base period before BW12 was passed. Interacting Sub_i with the monthly indicator M_t produces the Equation 4.3, referred to as Model III:

(Eq. 4.3)

$$\Delta p_{it} = \alpha_i + \theta_t + \beta_0 + \beta_1 Sub_i + \beta_2 BW_t + \beta_3 Post_t + \sum_{t=30}^{64} \boldsymbol{\delta}(M_t * Sub_i) + \boldsymbol{X}'_{\boldsymbol{itk}}\boldsymbol{\gamma} + v_i + \varepsilon_{it}$$

The hypothesis is that the difference in trends between areas with and without subsidies over the reference period will become significant sometime around the time BW12 is passed. If it occurs around July 2012 this would suggest the effects were anticipated around the passage of the act. If it occurs before July 2012 then there is evidence that the effect of the act was anticipated even before it was passed. Similarly, if the difference peaks when the enactment

[29] Dye et al. (2014) takes a similar approach to checking model results. They replace a post-2008 dummy with yearly interactions to confirm that the year the change took place was the one associated with the dummy variable.

takes place this would indicate an immediate adjustment regardless of whether it was also anticipated. It is expected that the entire effect of BW12 was not anticipated and that there was some adjustment when the provisions took effect. However, the use of a moving average by Zillow may hide this structural break. It is also thought that the difference between areas will decrease when BW12 is repealed. Again, if this result is expected this may occur before March 2014. On the other hand, if the HFIAA is not seen as a significant improvement over BW12 the change in trend around this time may be minimal. When interpreting these results it is important to recall that correlation does not equal causation. No amount of correlation of the expected effects with the expected months can definitively show causation. The less they align the more doubt is cast on a causal relationship. Unless results are profoundly inconsistent with prior expectations or supremely aligned with them this model will always be open to interpretation. However, it provides as detailed look at the data at hand as is possible under the assumption that the model is well specified. Results are presented in Table 4.2. The coefficients on the treatment group time period interaction (δ) are displayed in Figure 4.1 along with their 95% confidence intervals. These coefficients represent the difference in the change in median home value growth between subsidized and non-subsidized areas; therefore, the difference from non-subsidized areas is represented by the x-axis.

Table 4.2: Difference in Changes in Median Home Values, Model III

VARIABLES		VARIABLES	
BW_t	1,558***	**Oct13 (Second Enactment)**	-494.6***
$Post_t$	1,875***	Nov13	-348.4***
		Dec13	-323.6***
Policy Interaction (δ):		Jan14	-328.6***
		Feb14	-287.1***
Jul12 (Passage)	-514.4***	**Mar14 (Repeal)**	-278.0***
Aug12	-476.7***	Apr14	-169.5
Sep12	-561.7***	May14	-106.5
Oct12	-611.4***	**Jun14 (Peak)**	-85.97
Nov12	-612.4***	Jul14	-89.98
Dec12	-600.5***	Aug14	-109.0
Jan13 (First Enactment)	-685.5***	Sep14	-134.8**
Feb13	-838.7***	Oct14	-225.2***
Mar13	-996.2***	Nov14	-430.6***
Apr13	-1,044***	Dec14	-468.3***
May13	-1,045***	Jan15	-425.4***
Jun13 (Trough)	-1,096***	Feb15	-495.5***
Jul13	-999.2***	Mar15	-580.2***
Aug13	-787.4***	Apr15	-640.2***
Sep13	-653.6***	May15	-551.4***
Observations	773,824		
Number of Zip Codes	12,091	Constant	-337.3***
R-squared	0.143	dSP_t	2.045***

Robust standard errors *** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variable is Δp_{it}. Individual and monthly fixed effects are included but not reported.

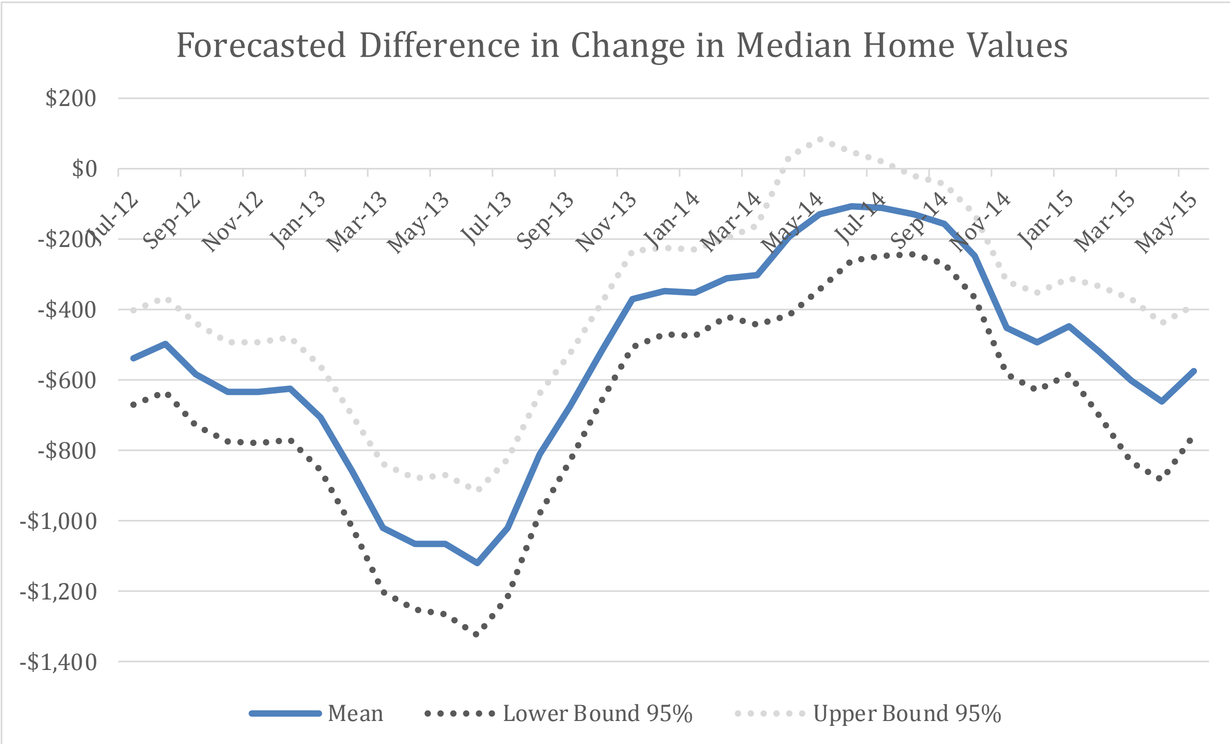

Figure 4.1: Forecasted Difference in Change in Median Home Values for Subsidized Areas, Model III

It is impossible to say for certain that BW12 is responsible for the difference in trends between areas with subsidies and without subsidies, but the evidence is compelling. The difference in the change in monthly median home values peaks after BW12 is passed and occurs between the two enactment periods. This effect rapidly diminishes and become insignificant around the time BW12 is repealed. Oddly, it becomes significant again towards the end of the study period. It is also significant before BW12 is passed. It is still not clear that this correlation is not coincidence. Model IV extends this analysis into the past for a more complete picture of how home values correlate with NFIP reform over the course of the study. This model is given in Equation 4.4 and results are provided visually in Figure 4.2. Note the only difference from Equation 4.3 is the extension of the summation back to $t = 2$.

(Eq. 4.4)

$$\Delta p_{it} = \alpha_i + \theta_t + \beta_0 + \beta_1 Sub_i + \beta_2 BW_t + \beta_3 Post_t + \sum_{t=2}^{64} \boldsymbol{\delta}(M_t * Sub_i) + \boldsymbol{X'_{itk}\gamma} + v_i + \varepsilon_{it}$$

Figure 4.2 shows that this trend is generally robust to the definition of the reference period and that a significantly slower rate in the growth of median home values appears in areas with flood subsidized flood insurance policies and is highly correlated with flood insurance reform. Generally, changes in the rate that median home values are diverging align with congressional actions on flood insurance reform either immediately or within a month of the action. Median home values begin to fall around July 2011 when both houses of congress made official moves in favor of flood insurance reform that would eliminate subsidies. The difference in the change in median home values increases in April 2012 when BW12 is introduced and again in December 2013 just before the first provisions take effect. There is not much change around July 2012 when BW12 is passed but the growth in median home values remained below that for areas without subsidies. Median home values continued to diverge until June 2013 when significant pressure built to reassess BW12 and the House passed a bill that would delay further rate increases. The change in median home values for subsidized areas remained below that of non-subsidized areas until just after the HFIAA was passed in April 2014. Shortly thereafter the change in median home values began to diverge again. While this paper has focused on BW12 this could be due to HFIAA provisions that did not provide as much relief as expected.

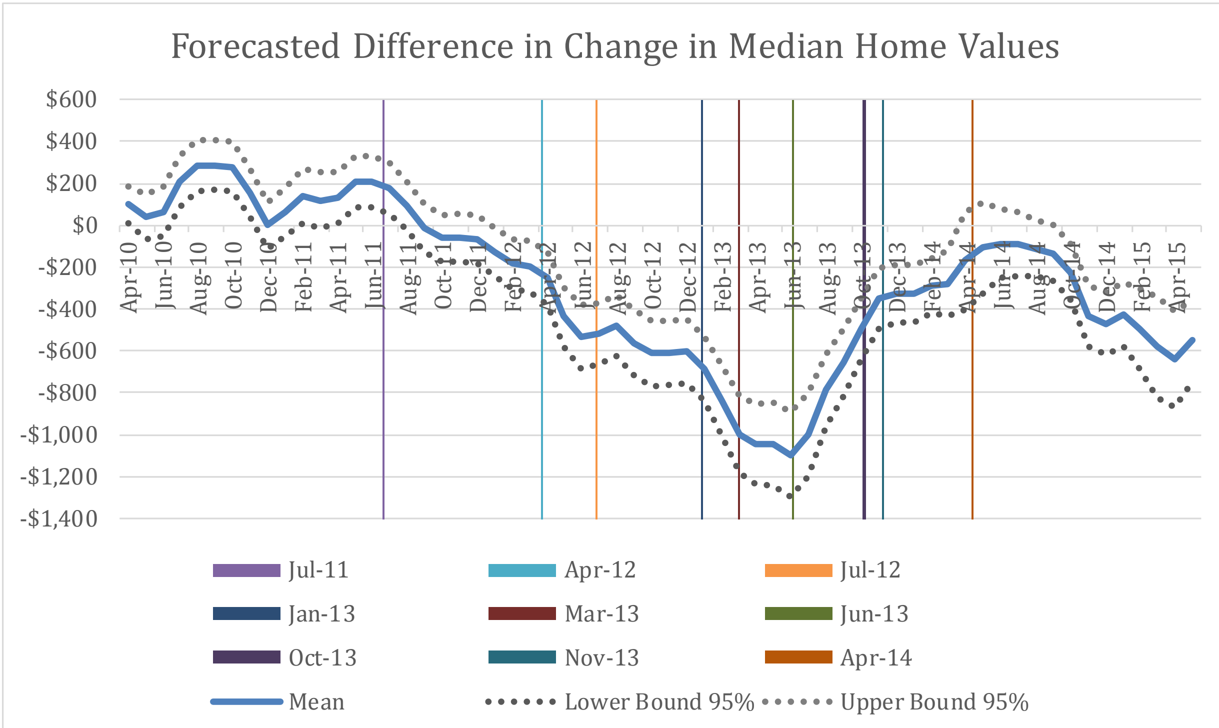

Figure 4.2: Forecasted Difference in Change in Median Home Values for Subsidized Areas, Model IV

These models point out that the time period distinctions put forth in Models I and II do not accurately align with the data, but nevertheless correlate well with flood insurance reform actions. From the results presented in Models III and IV it appears that rather than the dates of passage, enactment and repeal a smoother divergence begins around the time reform was first formally introduced and differences are more closely associated with congressional action on insurance reform than the enactment of BW12. Cumulatively, Models III and IV suggest a strong correlation between flood insurance reform and the change in median home values. Converting these results to average median home values gives a more intuitive picture of these results and illustrates the divergence in median home values from when BW12 was proposed to when the HFIAA was introduced (Figure 4.3). The argument that flood insurance reform measures are the causal force behind these changes seems to be reasonable but the possibility of spurious correlation or model specification error has not yet been entirely ruled out.

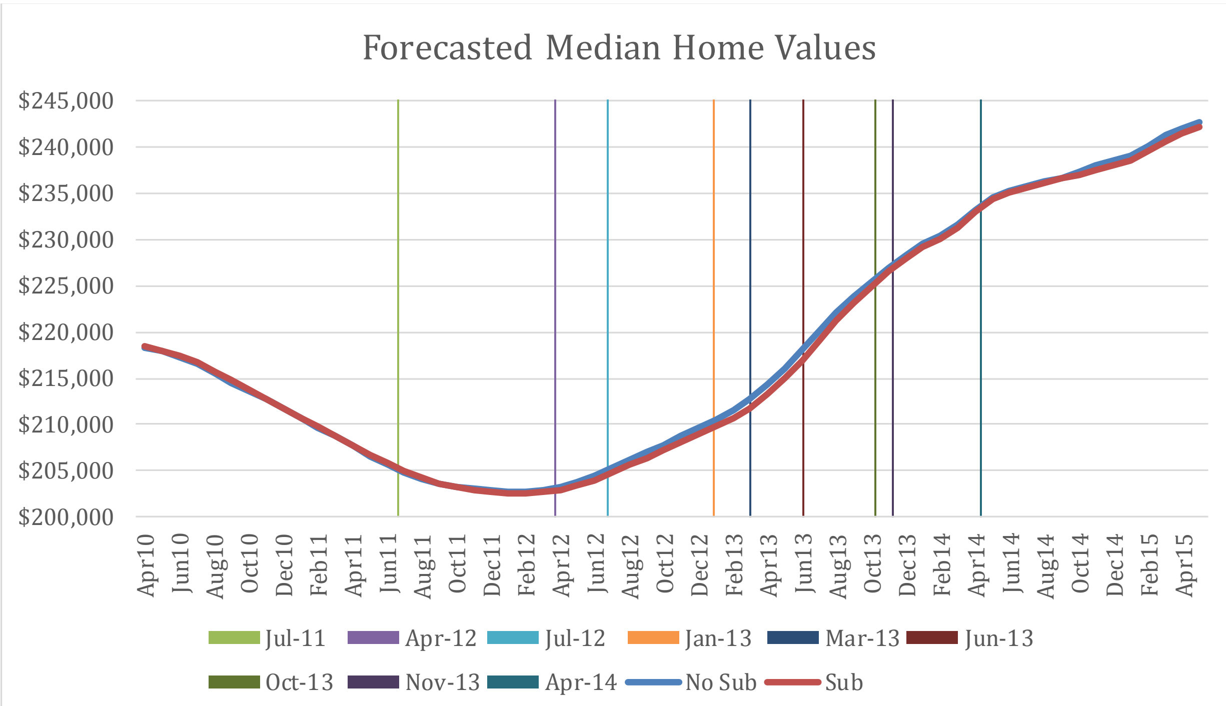

Figure 4.3: Forecasted Difference in Median Home Values, Model IV

Robustness of the Trend: AR(1) Dependent Lag Specification

This model is fit to a non-differenced dependent variable and instead includes a one time period lag as a regressor to test the robustness of the results to a similar functional form. This regression is implemented in Stata using the xtdpd command and utilizing the two-stage least squares Arellano-Bond estimator. [30] The first stage estimates an instrument for the dependent

[30] The Stata "xtdpd" command implements this Arellano-Bond two-step-least-squares estimation. More information on this test and the "xtabond" and "xtdpd" estimators can be found in StataCorp (2014) and Chapter 9 of Cameron and Trivedi (2010). Complete citations are given in References. A two-step Generalized Method of Moments procedure is also possible with the Arellano-Bond estimator. While GMM estimator is more efficient, this requires the additional assumption of homoscedasticity of the error term which cannot be made.

variable lag and the second stage controls for within group fixed effects by utilizing the first-differenced values of all variables, including the autoregressive IV, as instruments for the level equation. The equation actually estimated is first differenced; however, results are obtained for the empirical model, Model V, given by equation 4.5.

(Eq. 4.5)

$$p_{it} = \alpha_i + \theta_t + \beta_0 + \beta_1 y_{i,t-1} + \sum_{t=2}^{64} \delta_t (M_t * Sub_i) + \boldsymbol{X'_{itk}\gamma} + v_i + \varepsilon_{it}$$

In this framework the dependent variable lag is endogenous because the first-differenced lag is simultaneously determined with the first differenced error but if there is no serial correlation $y_{i,t-2}$ can serve as an alternative instrument. A test of serial correlation of the errors is calculated under the null hypothesis that there is no autocorrelation using the correlation between $\varepsilon_{i,t}$ and $\varepsilon_{i,t-k}$ where k is a specified number of autoregressive lags rejects this assumption. It is true by definition that correlation exists for $k = 1$ but correlation for $k \geq 2$ violates the no autocorrelation assumption. No autocorrelation is rejected for all $k < 7$ but fails to reject at $k = 7$ and $k = 8$ (Table 4.3). Thus an instrument is constructed using the more distant lags as well as every other regressor included in the model.

Table 4.3: Autocorrelation test result under null hypothesis of no autocorrelation

k	z	Prob>z
1	15.177	0.0000
2	-12.717	0.0000
3	-14.309	0.0000
4	2.2137	0.0269
5	-4.6025	0.0000
6	-3.0574	0.0022
7	1.0124	0.3114
8	-0.42644	0.6698

The model is assumed to show the same trend as the first differenced estimators depicted in Figure 4.3 above. Specifically, it is expected that the trends in subsidized and non-subsidized areas diverge in July 2011 when Congress began to seriously discuss flood insurance reform. The gap is expected to be greatest just before Maxine Waters and other representatives appealed to Congress for a reassessment of the policy. Finally, there should be no observed difference around the time BW12 was actually repealed. Figure 4.4 displays the results which do in fact show a similar trend to the one described in the monthly robustness test. This verifies that the observed correlation is not simply a byproduct of the way the model was specified. The Model V series begin to diverge in September 2011, peak in June 2013 and diminish in the period's immediately preceding repeal.

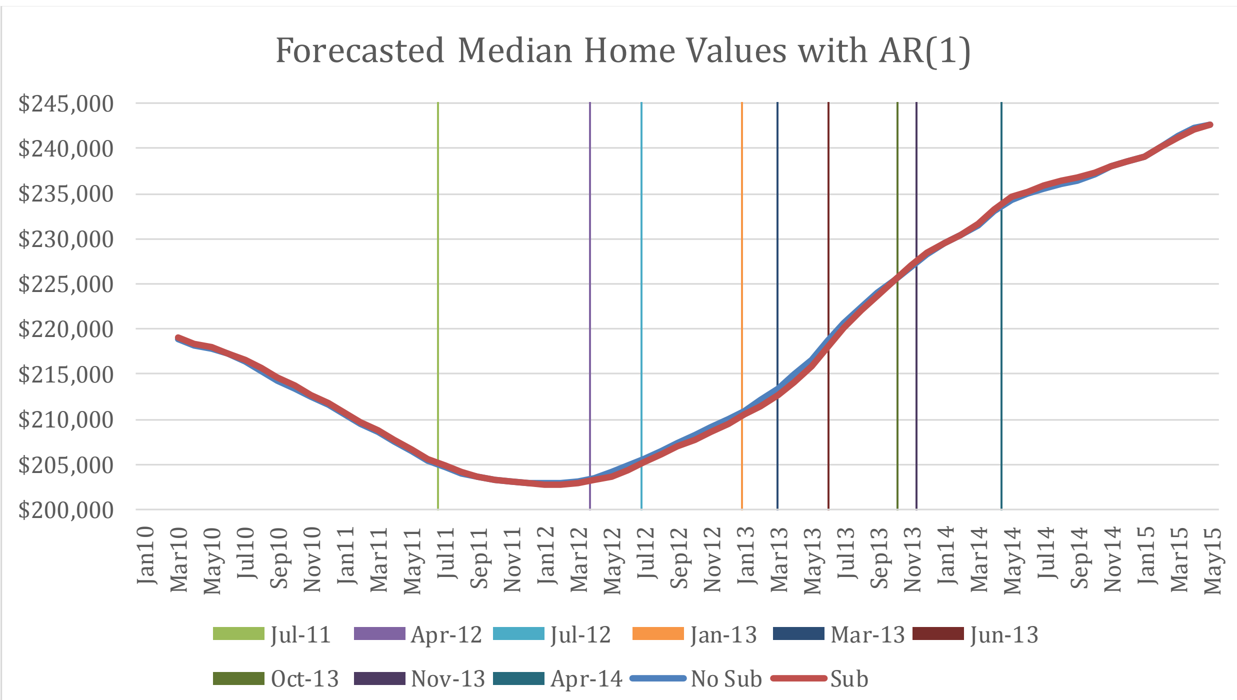

Figure 4.4: Forecasted Difference in Median Home Values, Model V

Robustness of the Trend: First-Order Error Autocorrelation

Formal tests of all five models indicate the presence of significant first-order autocorrelation.[31] This violates the Gauss Markov theorem and results in OLS no longer having the property of minimum variance. While still unbiased this could lead to misleading conclusions based on the standard t- and F-tests. Model II and Model IV are rerun with an AR(1) process to test robustness of the conclusions of the prior models.[32] Model II is presented in Table 4.4 with (a) and without (b) the inclusion of time period fixed effects, which seem to affect the estimation of the autoregressive model.

Table 4.4: AR(1) Model Results: Model II, II (a) and II (b)

VARIABLES	Model II	Model II (a)	Model II (b)
Policy Period			
$Pass_t$	1,774***	5.52e+08***	720.9***
Act_t	3,040***	5.52e +08***	1,212***
$Post_t$	1,257***	5.52e +08***	1,316***
Policy Period Subsidy Interaction			
$Pass_t * Sub_i$	-584.5***	-190.6***	-179.0***
$Act_t * Sub_i$	-702.1***	-397.4***	-382.0***
$Post_t * Sub_i$	-344.0***	-271.1***	-260.9***
Constant	-372.5***	-5.52e +08***	-140.3***
Observations	773,824	761,733	761,733
Number of Zip Codes	12,091	12,091	12,091

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: The dependent variable is Δp_{it}. Individual and monthly fixed effects are included but not reported.

The inclusion of the monthly fixed effects with the AR(1) process seems to cause problems with the underlying time trend. Excluding them results in a more intuitive model and

[31] The xtserial command in Stata which impliments the Wooldridgetest for serial correlation in panel data is applied with p-values of 0.0000 against the null of no first-order autocorrelation in all cases.
[32] Cluster robust standard errors are no longer reported.

has minimal effects on the interaction variables of interest. Compared to Model II the AR(1) models indicate a reduction in the magnitude of the change between periods. Theoretically the estimates of the previous models are unbiased and consistent and so it is somewhat surprising to observe these changes but it is possible that there is some general endogeneity in the model due to the moving average. Nevertheless, the results indicate a divergent trend peaking sometime between January 2013 and March 2014, which is consistent with the previous models. A graph of the AR(1) robustness check is presented in Figure 4.5.

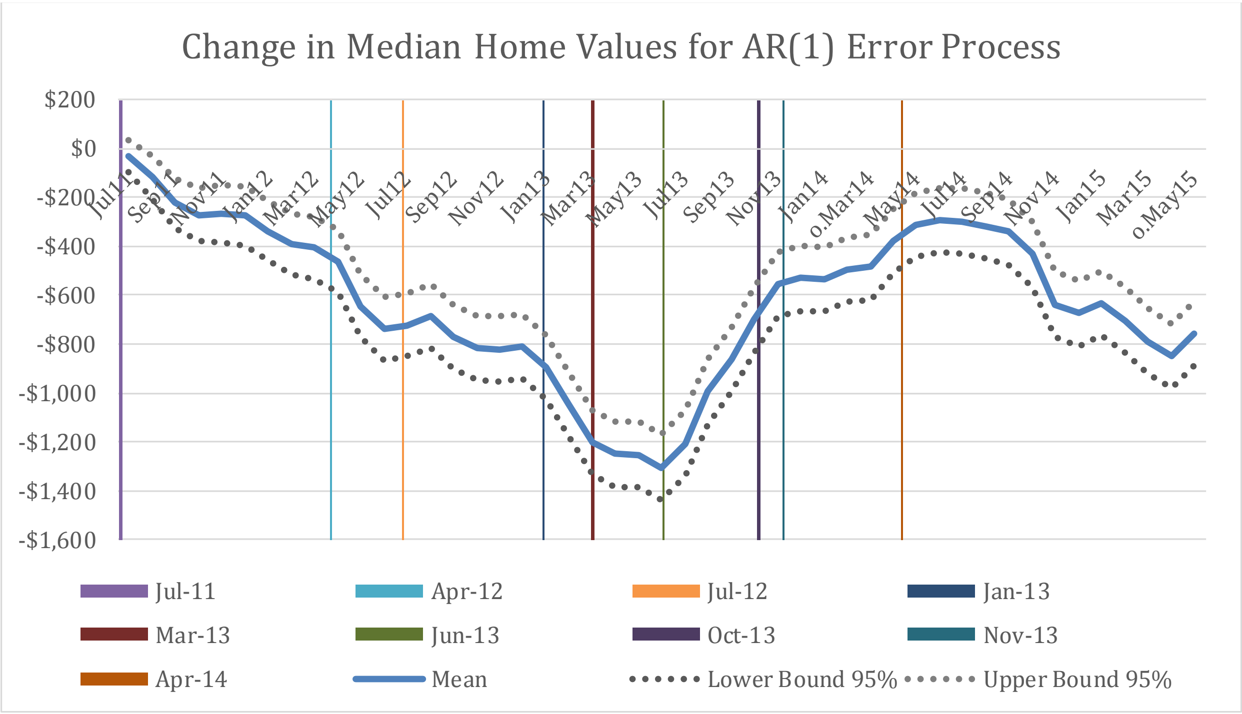

Figure 4.5: Difference in Change in Median Home Values for Subsidized Areas with AR(1) Error Process

The results clearly demonstrate that the trend is robust to the first-order AR(1) process for the model errors. Results indicate significant decreases over the reference period are slightly different but the relative differences between months is consistent. So far it is apparent that the observed trends are robust to several model formulations, reference periods and error processes.

Robustness of the Trend: Spatial Error Model

A spatial error model is now introduced for Model II to account for unobserved factors common across space that may determine housing values. An alternate model would be a spatial lag model in which nearby home values directly affect the values of the neighbors. While this may have some validity the motivation here is that commonalities in home values across space are due to common unobserved spatial characteristics such as access to the same public services, school systems, amenities or flood risk that are common across zip code boundaries.

A spatial weights matrix is constructed in GeoDa using a queen's contiguity. This results in an NxN matrix indicating whether each zip code i is adjacent to every other zip code j. Of the 12,091 zip codes the average number of neighbors is 4.7 with a maximum of 18 and 142 are islands. The spatial coefficient λ is significant and the coefficients are consistent with previous estimates. Results are presented with cluster robust standard errors in Table 4.5.

Table 4.5: Results for Model II with Spatial Error Component

VARIABLES	Model II	Model II SEM
Policy Period		
$Pass_t$	1,774***	1,645***
Act_t	3,040***	2,338***
$Post_t$	1,257***	1,442***
Policy Period Subsidy Interaction		
$Pass_t * Sub_i$	-584.5***	-308.5***
$Act_t * Sub_i$	-702.1***	-406.6***
$Post_t * Sub_i$	-344.0***	-174.9***
λ		0.0792***
σ_e^2		4.832e+06***
Constant	-372.5***	
Observations	773,824	773,824
R-squared	0.141	0.094
Number of Zip Codes	12,091	12,091

Robust standard errors *** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variable is Δp_{it}. Individual fixed effects are included but not reported. This model excludes dSP_t and monthly fixed effects due to limited computational resources.

Results from the spatial error model show that estimated differences between subsidized and non-subsidized areas are slightly less in all periods. Overall, the models above indicate that differences in changes in median home values between areas with and without subsidized NFIP policies are highly correlated with congressional action on flood insurance reform. Economic theory would support this explanation under well informed and fluid markets that reacted to new information as it is made available. While this may not always be realized so cleanly in practice the data series is generated by the proprietary statistical techniques of Zillow Research Group rather than pure economic processes. This study thus must distinguish itself slightly from analyzing data based solely on economic theory and instead acknowledge that it ultimately depends on which factors Zillow considers or how and when they come into play. Nevertheless, the results are consistent with economic theory. While a plausible competing explanation for the significant difference between areas with and without subsidies could be constructed the correlation is compelling even in the absence of proven causation. Given the consistent correlation of changes with flood related insurance events the burden seems to be placed on an alternate theory to offer a better explanation. A few will be considered in the next section.

Answer to Research Question 1: There is a difference in median home values in areas with and without subsidized National Flood Insurance Program policies. These changes are associated with Congressional action on NFIP reform measures.

Research Question 2: Is this difference robust to alternate causal explanations?

Robustness of the Treatment: Random Treatment Group Membership

To see whether the observed results could have been due to an arbitrary specification of the treatment group Model II (r) is rerun with a random set of zip codes designated as subsidized. This is accomplished by first assigning each zip code a random number from zero to one. Subsidized areas make up 81.26% of all observations and that ratio is maintained by designating all zip codes with a value greater than 0.8126 as non-subsidized and the remainder being subsidized. One sample is presented which has 2,242 “unsubsidized” zip codes and 9,849 “subsidized” zip codes. This is similar to the 2,266 and 9,825 true split. Results for Model II are presented in Table 4.6 and the forecast is presented in Figure 4.6.

Table 4.6: Model II Random Results for Random Treatment Group

VARIABLES	Model II	Random
Policy Period		
$Pass_t$	1,774***	1,266***
Act_t	3,040***	2,450***
$Post_t$	1,257***	976.5***
Policy Period Subsidy Interaction		
$Pass_t * Sub_i$	-584.5***	40.94
$Act_t * Sub_i$	-702.1***	23.36
$Post_t * Sub_i$	-344.0***	0.532
Constant	-372.5***	-372.5***
Observations	773,824	773,824
R-squared	0.141	0.139
Number of Zip Code	12,091	12,091

Robust standard errors *** p<0.01, ** p<0.05, * p<0.1
Note: The dependent variable is Δp_{it}. Individual and monthly fixed effects are included but not reported.

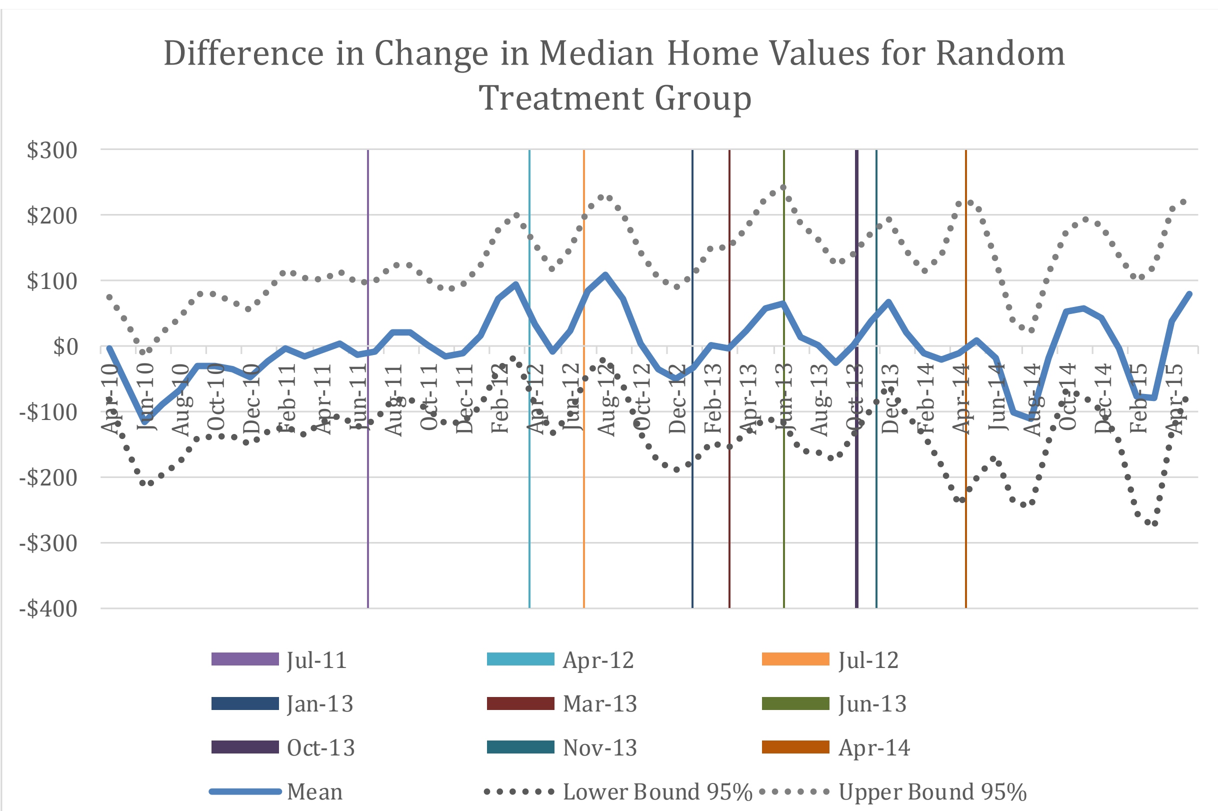

Figure 4.6: Difference in Change in Median Home Values for Radom Treatment Group

It is apparent in both presentations that no significant difference exists between subsidized and unsubsidized areas when these treatment groups are assigned randomly. This lends great support to the hypothesis that the observed trend, associated with flood insurance reform, is in fact due to the unique qualities of areas with subsidies and not some other factor that could be correlated with a random selection to subsidy group membership.

Robustness of the Treatment: Housing Recovery

Arizona, California, Florida and Nevada were hit hardest by the housing collapse and over 83% of zip codes in the four states are subsidized, accounting for roughly 20% of subsidized zip codes overall. Thus it is possible that this subset may be driving the results if they are recovering or not recovering differently from the collapse. If removing them from the sample eliminates the difference in subsidized areas then it can be said that this effect was overwhelmingly due to these states. By extension it may cast some doubt on whether the results were due to recovery from the collapse rather than the subsidized areas. If the trend is still visible than it is thought to be robust to the recovery of the housing market in hardest hit areas. This section presents results of Model II and an associated forecast from the subset of zip codes excluding these states as a robustness check. This model is represented by Equation 4.2 but is referred to as Model II ACFN when run on the subset. The Results are presented in Table 4.7 and Figure 4.7, respectively.

Table 4.7: Model II Results Excluding AZ, CA, FL and NV

VARIABLES	Model II	Minus ACFN
Policy Period		
$Pass_i$	1,774***	2,030***
Act_t	3,040***	2,991***
$Post_t$	1,257***	1,404***
Policy Period Subsidy Interaction		
$Pass_t * Sub_i$	-584.5***	-641.9***
$Act_t * Sub_i$	-702.1***	-791.6***
$Post_t * Sub_i$	-344.0***	-393.8***
Constant	-372.5***	-405.7***
Observations	773,824	625,984
R-squared	0.141	0.156
Number of Zip Code	12,091	9,781

Robust standard errors *** p<0.01, ** p<0.05, * p<0.1
Note: The dependent variable is Δp_{it}. Individual and monthly fixed effects are included but not reported.

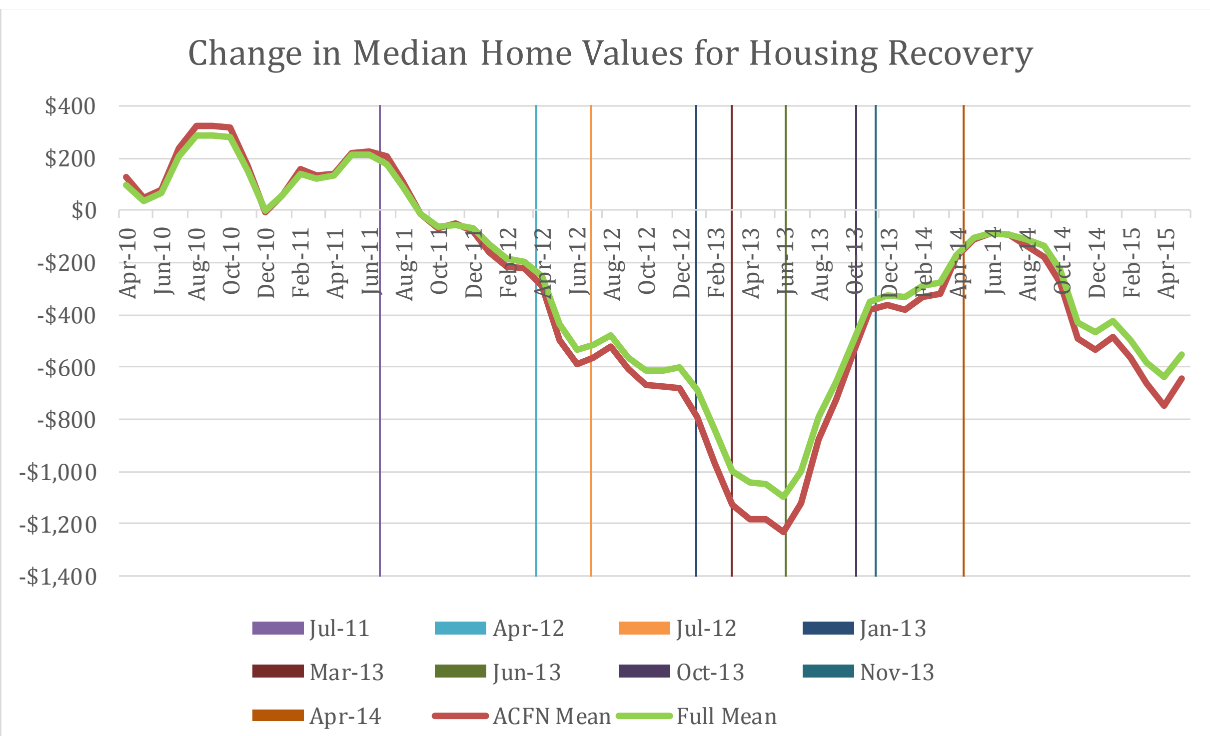

Figure 4.7: Change in Median Home Values for Subsidized Areas Excluding ACFN

Results of the former reveal that the average difference over the pre-passage reference period is about $50 to $90 greater in each treatment period for the subset excluding the four states. The model forecast shows that the trend is persistent and very similar to that of the full dataset. This may indicate that the excluded states are recovering faster than other areas which had previously muted the impact of the loss of subsidies in each treatment period. It is not clear from these models whether the changes seen here are due to the removal of the subsidized areas associated with the states or the change in composition of states recovering from the crisis; however, it is clear that the measured effect and overall trend are robust to the change in sample.

Robustness of the Treatment: Coastal vs Non-Coastal

Another way to divide the data is based on whether the zip code borders a coastal area or not. Coastal areas are exposed to storm surge and wave related risks with their own flood designation. As this leads to greater risk it may also lead to greater premium increases. Additionally, many reports of housing market trouble from BW12 came from coastal areas of Florida and Louisiana. Zip codes were designated as coastal if they were within two miles of the Atlantic, Gulf, Pacific or Great Lakes coast. It is expected that greater changes in median home values will be seen in coastal areas. Results, presented in Table 4.8 and Figure 4.8, confirm this hypothesis. Coastal areas saw an average change in median home values that was $1,335, $1,500 and $1,055 for post-passage, enactment and post-repeal phases, respectively. This is more than two times the effect seen in non-coastal areas. The forecast shows a similar pattern to that of the full dataset for each area but is much more pronounced in coastal areas. The divergence after the passage of HFIAA appears substantially larger in coastal areas than then in either the full or non-coastal dataset, indicating this may be a coastal phenomenon.

Table 4.8: Model II Results for Coastal and Non-Coastal Areas

VARIABLES	Model II	Coastal	Non-Coastal
Policy Period			
$Pass_i$	1,774***	3,294***	1,501***
Act_t	3,040***	4,828***	2,764***
$Post_t$	1,257***	2,153***	1,134***
Policy Period Subsidy Interaction			
$Pass_t * Sub_i$	-584.5***	-1,336***	-504.3***
$Act_t * Sub_i$	-702.1***	-1,506***	-614.3***
$Post_t * Sub_i$	-344.0***	-1,055***	-260.5***
Constant	-372.5***	-309.7***	-384.1***
Observations	773,824	120,128	653,696
R-squared	0.141	0.150	0.151
Number of Zip Code	12,091	1,877	10,214

Robust standard errors *** p<0.01, ** p<0.05, * p<0.1
Note: The dependent variable is Δp_{it}. Individual and monthly fixed effects are included but not reported.

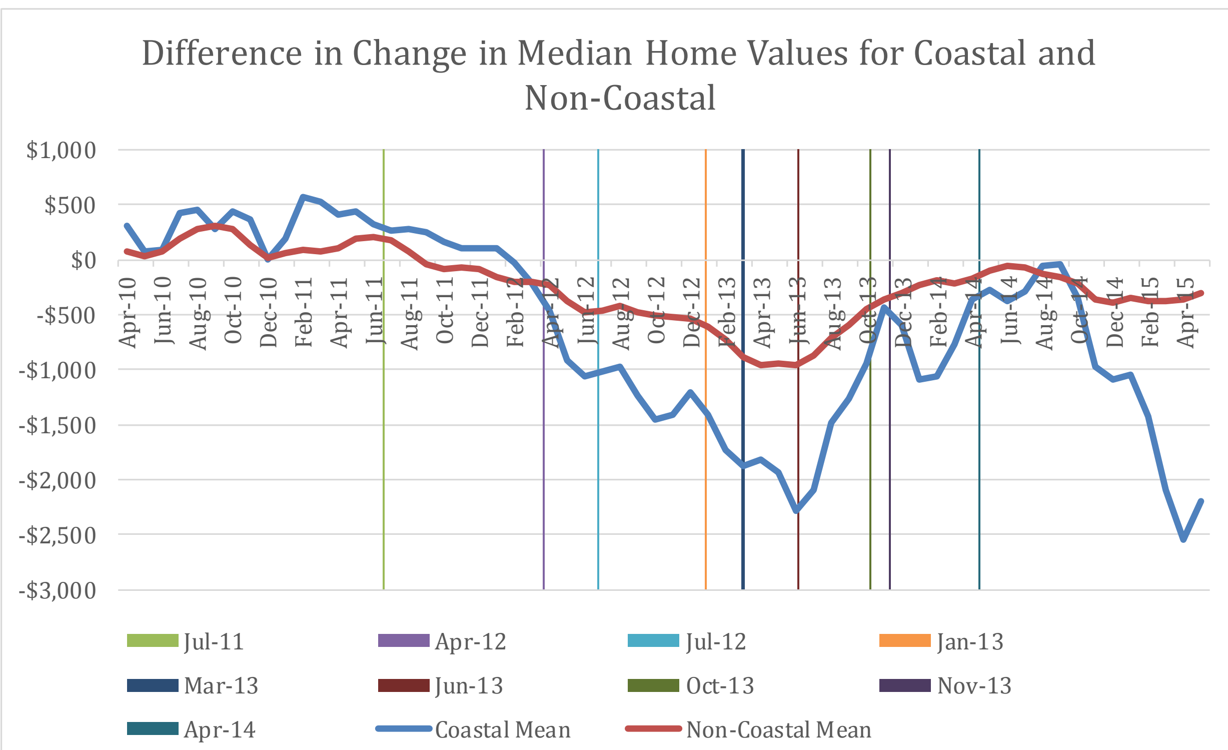

Figure 4.8: Difference in Change in Median Home Values for Subsidized Areas by Coastal Distinction

Robustness of the Treatment: Age of Housing Stock

The median age of the housing stock in a neighborhood could reflect any number of underlying processes. To see if results are robust to whether homes in an area are relatively new or relatively old the data is divided into two parts: whether the median home was built during or before 1974 or if they were built in 1975 or later. This division is chosen because it is the year that participation in the NFIP was made mandatory in order to receive federal disaster assistance or federally backed mortgages. Areas that had joined prior to this were more likely at risk than areas that joined after these incentives were added. While this distinction does not determine when a zip code joined they would have had to exist prior to 1974 for this to be a possibility. Additionally, new areas built into a flood zone should not have been eligible for subsidies. It is therefore expected that older areas will have seen a greater effect of NFIP reform. Many factors can complicate this but this division splits the qualities of the data roughly in half; the pre-1975 cohort contains 46% of all observations, 46% of subsidized areas and 58% of subsidized policies. Whether or not this is a good proxy for flood risk is uncertain and alternative explanations may exist but results give some insight into how the trend relates to the age of the housing stock. Results indicate that older areas were more heavily impacted as would be expected in relation to having greater flood risk (Table 4.9). Additionally, the trend is robust and visible in both groups though substantially more so in older areas (Figure 4.9).

Table 4.9: Model II Results for Pre- and Post-1975 Median Homes Areas

VARIABLES	Model II	Pre-1975	Post-1975
Policy Period			
$Pass_i$	1,774***	1,992***	1,586***
Act_t	3,040***	3,443***	2,269***
$Post_t$	1,257***	1,401***	1,126***
Policy Period Subsidy Interaction			
$Pass_t * Sub_i$	-584.5***	-920.6***	-293.4***
$Act_t * Sub_i$	-702.1***	-1,135***	-322.2***
$Post_t * Sub_i$	-344.0***	-741.8***	5.127
Constant	-372.5***	-197.2***	-524.6***
Observations	773,824	359,680	413,504
R-squared	0.141	0.122	0.174
Number of Zip Code	12,091	5,620	6,461

Robust standard errors *** p<0.01, ** p<0.05, * p<0.1
Note: The dependent variable is Δp_{it}. Individual and monthly fixed effects are included but not reported.

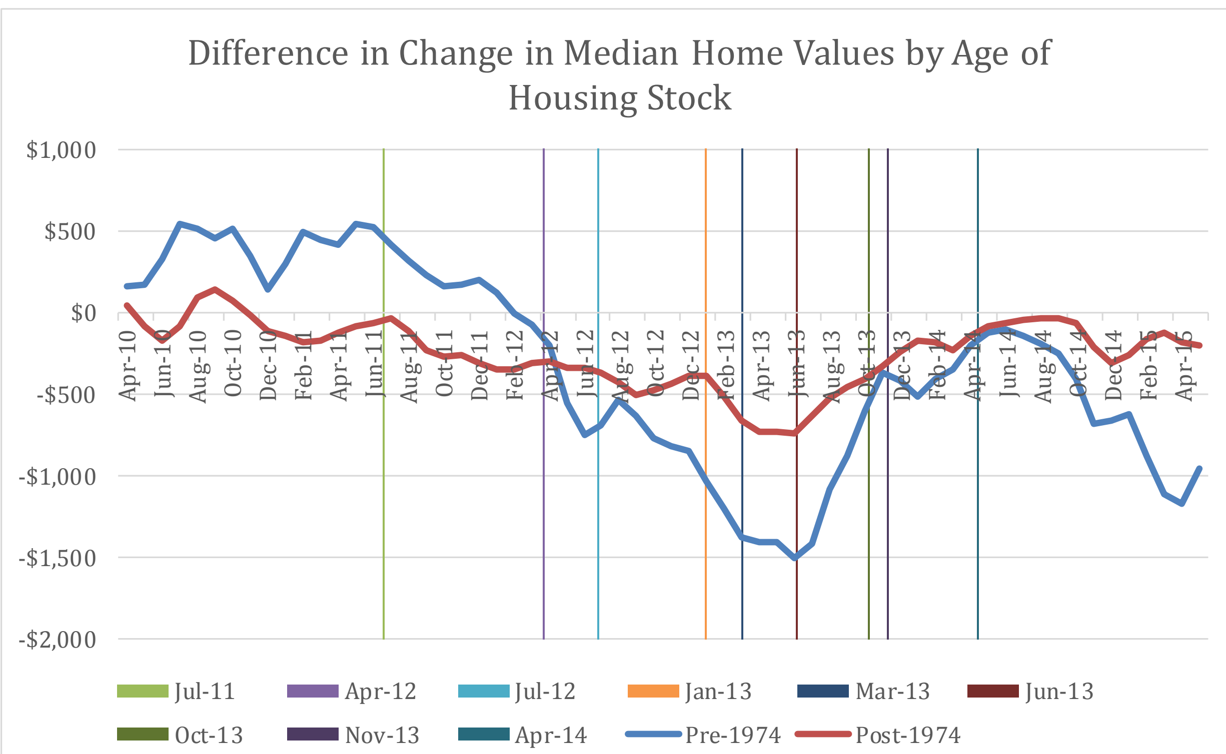

Figure 4.9: Difference in Change in Median Home Values for Subsidized Areas by Median Age

Robustness of the Treatment: Quantiles

While the above models help to qualify the nature of the difference in subsidized and non-subsidized areas nothing so far has indicated whether this depends on the number of subsidies present. The percent of NFIP policies that are subsidized ranges from 0% to 100%. Typically areas that are highly subsidized have few policies but there are 116 zip codes with over 1,000 subsidized policies and more than 90% are subsidized. Overall, the zip code with the most number of policies has almost 11,000 with almost 50% subsidized. To estimate the intensity of subsidy concentration Model II is run on the first, last and middle two quantiles of number of subsidized policies. It is expected that areas with more subsidized policies will see greater impacts from BW12 than areas with only a few policies as more of the housing market is affected by the policy. On the other hand, if areas with a large number of subsidized policies are also large in general this effect may be ambiguous and depend instead on the proportion of all homes that are subsidized.

Results are similar across the lowest three quartiles but show that the upper quartile of zip codes saw a reduced effect (Table 4.10). This is counter intuitive but it is possible that areas with large number of subsidized policies have a large number of homes in general and the analysis is rerun using the percent of housing units subsidized to account for this (Table 4.11). Results of the second set show that the greatest effect was in the middle two quartiles but that the upper quartile saw greater effects than the lower. While it is still odd that the largest effects aren't where the most policies are this is more intuitive than the previous result. A possible explanation for these results is that areas with the most policies are not the same areas as those facing the greatest premium changes. If this is the case then the number of policies is not the best measure of impact from NFIP reform and flood risk or another measure should be used instead.

Table 4.10: Quantile Results for Number of Subsidized Policies

VARIABLES	Model II	Low	Mid	High
Policy Period				
$Pass_i$	1,774***	1,652***	1,639***	1,779***
Act_t	3,040***	3,153***	3,066***	2,649***
$Post_t$	1,257***	1,256***	1,220***	1,203***
Policy Period Subsidy Interaction				
$Pass_t * Sub_i$	-584.5***	-682.6***	-638.5***	-377.9***
$Act_t * Sub_i$	-702.1***	-805.2***	-748.6***	-503.9***
$Post_t * Sub_i$	-344.0***	-502.3***	-358.9***	-145.5**
Constant	-372.5***	-321.2***	-347.2***	-344.9***
Observations	773,824	316,672	442,944	304,256
R-squared	0.141	0.151	0.146	0.155
Number of Zip Code	12,091	4,948	6,921	4,754

Robust standard errors *** p<0.01, ** p<0.05, * p<0.1
Note: The dependent variable is Δp_{it}. Individual and monthly fixed effects are included but not reported.

Table 4.11: Quantile Results for Percent of Policies Subsidized

VARIABLES	Model II	Low	Mid	High
Policy Period				
$Pass_i$	1,774***	1,794***	1,756***	1,775***
Act_t	3,040***	3,258***	2,606***	3,096***
$Post_t$	1,257***	1,224***	1,233***	1,216***
Policy Period Subsidy Interaction				
$Pass_t * Sub_i$	-584.5***	-418.1***	-738.6***	-438.5***
$Act_t * Sub_i$	-702.1***	-475.4***	-859.4***	-607.5***
$Post_t * Sub_i$	-344.0***	-298.7***	-443.2***	-190.5***
Constant	-372.5***	-308.1***	-335.5***	-373.8***
Observations	773,824	298,112	461,440	304,320
R-squared	0.141	0.175	0.149	0.133
Number of Zip Code	12,091	4,658	7,210	4,755

Robust standard errors *** p<0.01, ** p<0.05, * p<0.1
Note: The dependent variable is Δp_{it}. Individual and monthly fixed effects are included but not reported.

The graph of forecasted difference in the change in median home values (Figure 4.10) illustrates that while the areas with the most subsidized policies per housing unit did not see as much of an effect as those in the middle two quartiles the trends is similar throughout all three divisions. Nevertheless, areas with large numbers of subsidies recovered more than any other group indicating that it is possible that the majority of policies in high policy areas were expecting rate changes in October rather than January. Since the October effect seems to have been muted due to congressional talks on ending rate hikes areas with high numbers of subsidies bolstered by October policies may not have seen the same negative change in value as other areas. Since less than 15% of all policies were affected in January this could explain the unanticipated result.

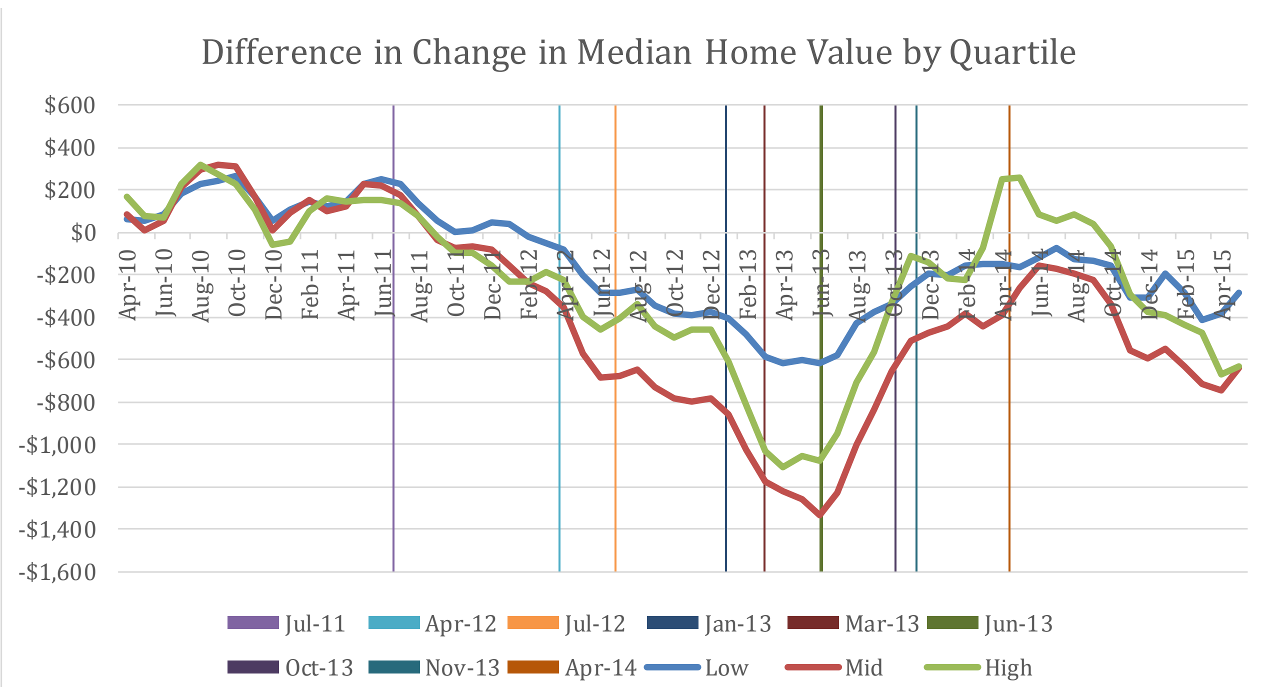

Figure 4.10: Difference in Change in Median Home Value for Percent Subsidized, Quartiles by Percent Subsidized

Another possible explanation may be that the types of risk and actual premium increases realized are not related to the number of policies present. It would be logical that the more policies there are the more the local housing market will internalize a change in NFIP policy; however, if the magnitude of the change is related to the actual change in risk premiums this may not be captured by quantiles on policy incidence. Looking at the map in Figure 3.4 reveals that the areas with the highest percent of subsidized NFIP policies tend to be inland around the Great Lakes and in the Midwestern region. If it is expected that coastal areas bear the majority of the rate increases then the highest quantile will not capture the greatest impacts. Overall it is reasonable to conclude that the trend is robust to the number of policies present as significant effects are clearly visible in each group.

Answer to Research Question 2: The observed trend is robust to several changes in sample and are consistent with some hypothesis regarding flood insurance reform as the casual factor. Additionally, the results are not robust to random selection of the treatment group.

CHAPTER FIVE

CONCLUSION

In this paper I examine the effects of the flood insurance reform on housing values. Specifically, I seek to identify whether there were differences in value trends between areas with and without zip codes and if so whether this was unique to subsidized areas. The analysis begins by estimating a simple model which allows for a difference in home values based on hypothesized treatment windows. The results suggest an effect; however, because the policy may have been anticipated and these effects may not have aligned with the defined policy windows a more flexible model was run. This model showed that the trend began in September 2011 and diverged until June 2013. The gap then closed until June 2014, where it began to diverge again. I found that changes in these periods correspond with congressional actions on flood insurance reform. In July 2011 Congress made its first official moves to eliminate subsidies, In June 2013 Congress as a whole was petitioned by some of its members to reconsider the Biggert-Waters Act of 2012, in April of 2014 the Homeowners Flood Insurance Affordability Act was passed. I estimate dependent lag, temporal error process and spatial error process models to test the robustness of the trend to model specification and alternate error processes. I find that the trend is consistently identified.

Concerned about the robustness of the results to alternate explanations I examine several subsets of the data. I first test the robustness of the results to the definition of the treatment group and find no significant effect in a random designation of subsidized areas. Next I examine the housing market recovery and find that the trend is present in areas excluding those hit hardest by the collapse. I find the results are robust to quantiles and align with expectations based on coastal and median age divisions. Overall, the evidence supports the conclusion that a significant difference in median home value growth in areas with subsidies associated with the Biggert-Waters Act of 2012 and is caused by the new flood insurance provisions regarding subsidies. In general, the effect on housing values is less than the $2,248 average increase in NFIP premium which is consistent with prior research that indicates risk and insurance rates are capitalized at a discount.

I have shown that flood insurance reform had negative impacts on the housing market; I now argue that this is an expected outcome from a justifiable economic adjustment. Assuring the continued existence of disaster insurance requires the distribution of the costs associated with disaster recovery to those undertaking the risk. The program was conceived out of necessity because private insurers were unwilling to offer flood insurance in high risk areas. Because losses from catastrophes can destroy the lives and livelihood of victims on large scales a system of risk sharing and guaranteed compensation is necessary to help restore social normality after a disaster. With climate change making significant weather events more likely reform of the current unsustainable system is increasingly critical to assure the long-term solvency of the NFIP. By eliminating subsidies the NFIP puts the burden of true risk on those who incur the risk rather than all who incur some level risk or the taxpayer in general. This makes the program become financially sustainable.

Additionally, the accurate communication of risk signals to the market is an economic improvement that can allow the markets to naturally respond. With less market distortion the NFIP could encourage additional risk mitigation in the areas it is most needed. This would lower the cost of a disaster for the victim, the taxpayer and the insurer. Since many flood areas are located in ecologically sensitive coastal, marsh and wetland areas internalizing the true risk of development can also prevent critical habitat loss (Holladay and Shwartz, 2010).

A decline in home values in areas that had not faced their true risk premiums is a natural consequence of the internalization of more accurate rates. While it may be painful for some the increased costs of risk taking is exactly the goal of flood insurance reform. If any substantial progress in reforming the program is to be made premiums will have to go up; nevertheless, care must be taken to assure that the distribution of these changes does not disproportionately burden those in society least capable of dealing with economic stress. Policies on single-family non-primary residences and those structures that have seen severe repetitive flooding could be looked at first. Loans that facilitate flood risk mitigation can help finance reductions in loss exposure from insurers and where tied to means tested vouchers and lowered insurance rates they can offset insurance payments for those in need (Kausky and Kunreuther, 2013).

The HFIAA is considered a band aid to the provisions of BW12 and a stepping stone to future changes in the NFIP. For all these reasons issues relating to flood insurance are likely to remain relevant for some time. Future research may examine additional alternate explanations to increase confidence in the robustness of the results. Additionally, this analysis makes only cursory attempt at characterizing which housing markets are most vulnerable. Of greater significance is whether the policy is regressive or progressive. Research has shown the concentration of NFIP policies in particularly poor and particularly wealthy areas. It is important for future policy reform to determine who bears the costs of potential changes.

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APPENDIX I

STATIONARITY

Nonstationarity can enter into panel data by way of trends, cycles or random walks and can pose a serious obstacle to drawing conclusions from model results. Data that are non-stationary have means or variances that change over time, making hypothesis testing dubious because the asymptotic properties of the estimate are not the same at each point in time. This invalidates the traditional t- and F-distributions used for making hypothesis tests. Accordingly, the subject of nonstationarity has received considerable attention in the empirical literature on panel data.[33]

Stationarity is a problem if $y_{i,t-1}$ determines y_{it} in a one-to-one manner. This type of relationship is said to be deterministic as y_{it} will be equal to its value in the previous period plus a stochastic component. If a series has a unit root then shocks are not dissipated over time and results in there being no long term mean for the data. Time trends or drift can also be included in a determinist series or can cause a series to be nonstationary on their own. This is known as trend stationary and it does not lead to a deterministic relationship. The cure for trend stationary data is simply to correctly model the trend. Thus the correct solution to non-stationary data depends on its source and type.

In a simple framework the equation $y_{it} = \rho_i y_{i,t-1} + \boldsymbol{z}_{\boldsymbol{it}}'\boldsymbol{\gamma}_{\boldsymbol{i}} + \varepsilon_{it}$ can be estimated to test the relationship between y_{it} and $y_{i,t-1}$ where $\boldsymbol{z}_{\boldsymbol{it}}'$ is a matrix of panel specific characteristics such as fixed effects or time trend and the null hypothesis tests whether the lag coefficient $\rho_i = 1$

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33 A search of EconLit for the terms "unit root" and "nonstationary" with "panel data" turned up 873 and 171 results, respectively, as of October, 2015.

versus the alternative that $\rho_i < 1$ (StataCorp, 2014).[34] The null hypothesis is that all panels contain unit roots. The inclusion of $\boldsymbol{z}'_{it}$ variables tests the robustness of nonstationarity to different stochastic processes and identifies to what extent the best prediction of the value for y_{it} is y_{it-1}.[35] If the null hypothesis is not rejected there is a deterministic relationship and the series must be first-differenced by subtracting $y_{i,t-1}$ from y_{it}. If the null is initially rejected but the inclusion of $\boldsymbol{z}'_{it}$ characteristics fix the issue the series has an associated time trend that may be sufficient to make the data stationary, in this case differencing would not be required.

Since nonstationary relationship can take a number of forms several tests of the median home value series using various specifications of $\boldsymbol{z}'_{it}$ using the Im–Pesaran–Shin (IPS) test are estimated.[36] Each test indicated that the median home value series is highly non-stationary implicating a deterministic relationship rather than trend-stationarity. A shortfall of the IPS test is that it assumes the errors in the test are not serially correlated. Using the Aikaike's information criterion and the Augmented Dicky Fuller (ADF) test to select the correct autoregressive process reveals an average of 5.23 autoregressive lags across panels are appropriate.[37] Testing several specifications of $\boldsymbol{z}'_{it}$ using the ADF procedure with an AR(6) process rejects nonstationarity in some but not all of the cases.[38] First differencing and conducting a series of unit root tests on

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34 The actual model estimated is $\Delta y_{it} = \varphi_i y_{i,t-1} + \boldsymbol{z}'_{it}\boldsymbol{\gamma}_i + \varepsilon_{it}$ with the null hypothesis $\varphi_i = 0$ versus the alternative that $\varphi_i < 0$.

35 It is possible that more than one lag must be used to achieve stationary data.

36 Several specifications were needed because misspecification increases the chance of a Type I error and it is impossible to know the correct process.

37 The equation tested is fundamentally the same for both IPS and ADF tests, the difference being the assumptions on asymptotic behavior and behavior of the error term. IPS requires that N and T go to infinity sequentially while ADF only requires that T does with N finite. ADF is a better test for the data since N are zip codes it is more reasonable to assume that N is fixed while time goes to infinity but the former isn't necessarily wrong. By default the ADF regression includes an AR(1) lags, so only IPS could be used under the initial assumption of serially uncorrelated errors.

38 Because it is impossible to know which model was the correct specification of the nonstationary process several specifications were tested. Each set of tests was also performed using IPS, ADF and Phillips-Perron asympotics when applicable. Five of eleven models indicate the inclusion of an AR(6) process with trend is the source of the identified nonstationarity; however, since both misspecification and a moving average

$d.ZHVI_{it} = ZHVI_{it} - ZHVI_{i,t-1}$ rejected the null hypothesis of unit roots in all cases, strongly suggesting an I(1) deterministic trend. Model I would now be estimated on the first-differenced median home value: $\Delta ZHVI_{it} = \beta_0 + \beta_1 Sub_{it} + \beta_2 BW_t + \beta_3(BW_t * Sub_{it}) + \theta_t + \varepsilon_{it}$. Note that the monthly fixed effect, θ_t, is a more dynamic specification of its linear counterpart, θt, which would not vary for each month. As a result the model specification addresses the deterministic stationarity of $ZHVI_{it}$ by first differencing and the trends still present in $\Delta ZHVI_{it}$ by introducing time period fixed effects that do not constrain the trend to be linear or significant in each period.

component, which is known to be present, increase the chance of falsely rejecting the null of a unit root confidence is not easily placed in the inconsistent results (Gujarati, 2009). All fourteen specifications of the $\Delta ZHVI_{it}$ process rejected the null at p=0.0000. Thus it is reasonable to think this series is stationary as rejection of the null is robust to model specification. Conducting similar tests on the natural log of failed to reject the null of unit roots in four out of four cases and rejected the null with an average of 5.19 lags chosen by AIC.

APPENDIX II

FULL MODEL RESULTS

Table I: Full Results for Model I

VARIABLES		VARIABLES	
Policy Variables		Dec12	-755.9***
Sub_i	Omitted		(24.40)
	-	Jan13	-803.1***
BW_t	2,582***		(24.36)
	(63.30)	Feb13	-662.2***
$Post_t$	1,257***		(24.72)
	(53.35)	Mar13	-468.2***
			(23.06)
Policy Period Subsidy Interaction		Apr13	-236.4***
$BW_t * Sub_i$	-668.5***		(14.68)
	(61.84)	May13	Omitted
$Post_t * Sub_i$	-344.0***		-
	(57.84)	Jun13	430.8***
			(15.09)
Monthly Fixed Effects		Jul13	411.3***
Mar10	-14.16		(22.34)
	(17.84)	Aug13	237.1***
Apr10	-68.15***		(26.18)
	(22.41)	Sep13	13.57
May10	10.76		(26.78)
	(23.32)	Oct13	-74.44**
Jun10	-177.8***		(29.01)
	(24.98)	Nov13	-186.6***
Jul10	-420.0***		(31.00)
	(26.48)	Dec13	-277.5***
Aug10	-525.3***		(29.75)
	(26.73)	Jan14	-452.6***
Sep10	-589.1***		(30.25)
	(26.19)	Feb14	-776.1***
Oct10	-629.0***		(31.00)
	(26.88)	Mar14	-523.5***
Nov10	-573.9***		(35.70)
	(24.69)	Apr14	990.6***
Dec10	-624.1***		(39.22)
	(24.69)	May14	670.8***
Jan11	-717.1***		(37.00)
	(26.61)	Jun14	10.44
Feb11	-630.1***		(27.18)
	(28.18)	Jul14	-72.10***

Mar11	-518.9***		(24.22)
	(26.05)	Aug14	-61.02***
Apr11	-701.4***		(21.50)
	(25.03)	Sep14	-171.5***
May11	-742.7***		(17.79)
	(24.73)	Oct14	Omitted
Jun11	-562.3***		-
	(24.97)	Nov14	Omitted
Jul11	-495.9***		-
	(26.37)	Dec14	-84.66***
Aug11	-100.6***		(17.78)
	(26.97)	Jan15	-86.55***
Sep11	-79.36***		(23.21)
	(23.51)	Feb15	365.9***
Oct11	-52.64**		(26.10)
	(23.39)	Mar15	610.8***
Nov11	111.2***		(32.41)
	(23.97)	Apr15	215.6***
Dec11	215.1***		(30.35)
	(24.64)	May15	-46.84*
Jan12	196.9***		(26.82)
	(25.49)		
Feb12	300.8***	**Control Variables**	
	(25.07)	dSP_t	1.023***
Mar12	486.2***		(0.0882)
	(24.81)	$Metro_i$	Omitted
Apr12	705.0***		-
	(25.76)	$Year_i$	Omitted
May12	963.1***		-
	(27.44)	$Coastal_i$	Omitted
Jun12	1,102***		-
	(27.88)	$MilesRS_i$	Omitted
Jul12	-889.5***		-
	(26.48)	$AreaLand_i$	Omitted
Aug12	-837.4***		-
	(26.45)	THU_i	Omitted
Sep12	-856.6***		-
	(26.63)		
Oct12	-851.7***	Constant	-372.5***
	(26.76)		(19.25)
Nov12	-739.4***	Observations	773,824
	(25.91)	R-squared	0.141
		Number of Zip Codes	12,091

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: The dependent variable is Δp_{it} and 12,091 individual fixed effects are included but not reported.

Table II: Full Results for Model II

VARIABLES	
Policy Variables	
Sub_i	Omitted
	-
$Pass_t$	1,774***
	(63.53)
Act_t	3,040***
	(68.24)
$Post_t$	1,257***
	(53.35)
Policy Period Subsidy Interaction	
$Pass_t * Sub_i$	-584.5***
	(65.41)
$Act_t * Sub_i$	-702.1***
	(64.51)
$Post_t * Sub_i$	-344.0***
	(57.84)
Monthly Fixed Effects	
Mar10	-14.16
	(17.84)
Apr10	-68.15***
	(22.41)
May10	10.76
	(23.32)
Jun10	-177.8***
	(24.98)
Jul10	-420.0***
	(26.48)
Aug10	-525.3***
	(26.73)
Sep10	-589.1***
	(26.19)
Oct10	-629.0***
	(26.88)
Nov10	-573.9***
	(24.69)
Dec10	-624.1***
	(24.69)
Jan11	-717.1***
	(26.61)
Feb11	-630.1***
	(28.18)
Mar11	-518.9***
	(26.05)

VARIABLES	
Nov12	Omitted
	-
Dec12	-16.44
	(13.08)
Jan13	-1,234***
	(26.85)
Feb13	-1,093***
	(25.51)
Mar13	-899.0***
	(25.02)
Apr13	-667.2***
	(20.86)
May13	-430.8***
	(15.09)
Jun13	Omitted
	-
Jul13	-19.56
	(16.89)
Aug13	-193.7***
	(23.98)
Sep13	-417.3***
	(27.07)
Oct13	-505.3***
	(30.54)
Nov13	-617.4***
	(34.28)
Dec13	-708.3***
	(32.55)
Jan14	-883.5***
	(32.31)
Feb14	-1,207***
	(33.18)
Mar14	-954.4***
	(37.44)
Apr14	990.6***
	(39.22)
May14	670.8***
	(37.00)
Jun14	10.44
	(27.18)
Jul14	-72.10***
	(24.22)
Aug14	-61.02***
	(21.50)
Sep14	-171.5***

Apr11	-701.4***		(17.79)
	(25.03)	Oct14	Omitted
May11	-742.7***		-
	(24.73)	Nov14	Omitted
Jun11	-562.3***		-
	(24.97)	Dec14	-84.66***
Jul11	-495.9***		(17.78)
	(26.37)	Jan15	-86.55***
Aug11	-100.6***		(23.21)
	(26.97)	Feb15	365.9***
Sep11	-79.36***		(26.10)
	(23.51)	Mar15	610.8***
Oct11	-52.64**		(32.41)
	(23.39)	Apr15	215.6***
Nov11	111.2***		(30.35)
	(23.97)	May15	-46.84*
Dec11	215.1***		(26.82)
	(24.64)		
Jan12	196.9***	**Control Variables**	
	(25.49)	dSP_t	1.023***
Feb12	300.8***		(0.0882)
	(25.07)	$Metro_i$	Omitted
Mar12	486.2***		-
	(24.81)	$Year_i$	Omitted
Apr12	705.0***		-
	(25.76)	$Coastal_i$	Omitted
May12	963.1***		-
	(27.44)	$MilesRS_i$	Omitted
Jun12	1,102***		-
	(27.88)	$AreaLand_i$	Omitted
Jul12	-150.1***		-
	(21.22)	THU_i	Omitted
Aug12	-97.95***		-
	(21.51)		
Sep12	-117.2***	Constant	-372.5***
	(18.34)		(19.25)
Oct12	-112.3***	Observations	773,824
	(11.20)	R-squared	0.141
		Number of Zip Codes	12,091

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variable is Δp_{it} and 12,091 individual fixed effects are included but not reported.

Table III: Full Results for Model III

VARIABLES		VARIABLES	
Policy Variables		Jul10	209.6***
Sub_i	Omitted		(63.18)
	-	Aug10	287.3***
BW_t	2,782***		(62.21)
	(84.39)	Sep10	286.8***
$Post_t$	1,182***		(60.93)
	(62.50)	Oct10	278.6***
			(61.19)
Monthly Fixed Effects		Nov10	152.2***
Mar10	-113.6***		(58.60)
	(30.57)	Dec10	1.164
Apr10	-230.5***		(56.28)
	(49.60)	Jan11	62.40
May10	19.41		(58.75)
	(51.57)	Feb11	138.1**
Jun10	-224.5***		(67.19)
	(53.87)	Mar11	119.2*
Jul10	-621.8***		(68.43)
	(60.91)	Apr11	133.5**
Aug10	-801.6***		(61.91)
	(61.79)	May11	210.3***
Sep10	-892.9***		(62.64)
	(64.83)	Jun11	211.2***
Oct10	-941.2***		(62.14)
	(65.10)	Jul11	178.6***
Nov10	-760.7***		(61.02)
	(59.62)	Aug11	90.31
Dec10	-703.8***		(58.72)
	(58.67)	Sep11	-12.72
Jan11	-845.0***		(57.36)
	(61.16)	Oct11	-61.75
Feb11	-816.9***		(57.59)
	(69.97)	Nov11	-57.53
Mar11	-633.9***		(58.69)
	(65.50)	Dec11	-68.05
Apr11	-872.7***		(59.23)
	(62.90)	Jan12	-128.7**
May11	-955.8***		(59.32)
	(60.88)	Feb12	-183.9***
Jun11	-716.9***		(60.40)
	(57.88)	Mar12	-199.0***
Jul11	-715.0***		(62.33)
	(61.84)	Apr12	-253.7***
Aug11	-66.16		(65.29)
	(58.57)	May12	-436.9***
Sep11	-92.53*		(72.52)
	(53.01)	Jun12	-531.3***

Oct11	-71.72		(77.41)
	(56.09)	Jul12	-514.4***
Nov11	103.2*		(74.34)
	(56.88)	Aug12	-476.7***
Dec11	218.0***		(73.44)
	(56.98)	Sep12	-561.7***
Jan12	207.8***		(79.08)
	(60.03)	Oct12	-611.4***
Feb12	362.0***		(79.14)
	(58.81)	Nov12	-612.4***
Mar12	575.2***		(77.80)
	(58.02)	Dec12	-600.5***
Apr12	878.9***		(77.83)
	(58.85)	Jan13	-685.5***
May12	1,329***		(78.85)
	(68.20)	Feb13	-838.7***
Jun12	1,517***		(84.93)
	(70.31)	Mar13	-996.2***
Jul12	-1,287***		(94.02)
	(69.94)	Apr13	-1,044***
Aug12	-1,273***		(96.18)
	(70.59)	May13	-1,045***
Sep12	-1,220***		(99.82)
	(72.93)	Jun13	-1,096***
Oct12	-1,128***		(103.8)
	(72.76)	Jul13	-999.2***
Nov12	-976.2***		(99.92)
	(70.76)	Aug13	-787.4***
Dec12	-1,075***		(88.69)
	(65.58)	Sep13	-653.6***
Jan13	-1,084***		(81.21)
	(61.82)	Oct13	-494.6***
Feb13	-792.0***		(75.87)
	(57.58)	Nov13	-348.4***
Mar13	-476.8***		(74.95)
	(51.77)	Dec13	-323.6***
Apr13	-187.3***		(70.05)
	(32.38)	Jan14	-328.6***
May13	Omitted		(70.32)
	-	Feb14	-287.1***
Jun13	564.3***		(66.62)
	(42.98)	Mar14	-278.0***
Jul13	393.5***		(80.25)
	(59.91)	Apr14	-169.5
Aug13	96.80		(117.9)
	(68.69)	May14	-106.5
Sep13	-251.5***		(111.2)
	(71.27)	Jun14	-85.97
Oct13	-484.9***		(83.01)
	(73.38)	Jul14	-89.98
Nov13	-747.1***		(77.31)
	(80.08)	Aug14	-109.0
Dec13	-818.0***		(69.22)

	(74.28)	Sep14	-134.8**
Jan14	-979.2***		(68.18)
	(75.36)	Oct14	-225.2***
Feb14	-1,316***		(68.56)
	(80.25)	Nov14	-430.6***
Mar14	-1,124***		(74.24)
	(97.53)	Dec14	-468.3***
Apr14	887.4***		(77.12)
	(93.21)	Jan15	-425.4***
May14	491.1***		(78.34)
	(88.74)	Feb15	-495.5***
Jun14	-218.5***		(98.79)
	(72.17)	Mar15	-580.2***
Jul14	-265.8***		(122.4)
	(71.13)	Apr15	-640.2***
Aug14	-200.8***		(118.4)
	(57.57)	May15	-551.4***
Sep14	-334.5***		(100.7)
	(45.64)		
Oct14	Omitted	**Control Variables**	
	-	dSP_t	2.045***
Nov14	Omitted		(0.267)
	-	$Metro_i$	Omitted
Dec14	45.79		-
	(50.09)	$Year_i$	Omitted
Jan15	45.58		-
	(69.78)	$Coastal_i$	Omitted
Feb15	473.1***		-
	(75.80)	$MilesRS_i$	Omitted
Mar15	844.3***		-
	(106.3)	$AreaLand_i$	Omitted
Apr15	480.4***		-
	(101.4)	THU_i	Omitted
May15	143.6*		-
	(85.63)		
Monthly Subsidy Group Interaction			
Apr10	99.59**	Constant	-337.3***
	(44.05)		(20.72)
May10	36.95		
	(54.43)	Observations	773,824
Jun10	66.65	Number of Zip Codes	12,091
	(58.84)	R-squared	0.143

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: The dependent variable is Δp_{it} and 12,091 individual fixed effects are included but not reported.