Household Take-up of Subsidized Insurance*

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Abstract

We examine the reactions of a highly subsidized group of policyholders in the U.S. National Flood Insurance Program to an increase in their insurance premiums. Using difference-in-differences estimation on a panel of over two million policy-year observations, we find that about one fourth of the policyholders cancel their insurance coverage even though it is still cheaper than a fair rate. We examine potential explanations of our results and find evidence of a behavioral response: consumer non-renewals closely correspond to periods of negative media coverage of the reform. We also show tentative evidence that the reaction to the reform increased the likelihood of households experiencing financial hardship. Our findings add to research on public policy design and offer insights into household insurance demand.

Keywords: Public Programs · Public Policy Design · Insurance Choices

JEL Classifications: D81 · G22 · H31 · H53

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1 Introduction

Low-income households and other vulnerable groups often benefit from participation in public insurance programs. Such programs cover, among others, the health risks of diabetics, the flood risks of residents in coastal communities, and the longevity risks of low-skilled workers. They also typically feature substantial subsidies for those covered – enrollees only have to pay a fraction of the actuarial costs of their risk (Finkelstein et al., 2019). It is thus surprising that many people choose not to participate in these programs. Empirical research on consumers' insurance decisions shows across a variety of risks, that households often have a willingness to pay below their expected losses and may not react sensibly to price changes (Bütler and Teppa, 2007; Chalmers and Reuter, 2012; Browne et al., 2015; Finkelstein et al., 2019; Wagner, 2020). While there are some indications as to why this low willingness to pay persists, the exact mechanism is still unclear. Uncovering it and getting a clearer picture of when and how households participate in public programs thus has important implications for designing effective policies.

Low participation in public insurance programs with partial subsidies can, in principle, be explained by adverse selection (Einav and Finkelstein, 2011). However, neither Finkelstein et al. (2019) in health insurance, nor Wagner (2020) in flood insurance find sufficient evidence of adverse selection to explain a meaningful part of the gap. The underestimation of expected losses has often been discussed as a potential alternative explanation (Browne et al., 2015; Finkelstein et al., 2019; Wagner, 2020) but there is no empirical study which can directly link this explanation to the underinsurance phenomenon.[1] In this study, we examine the insurance decisions of a vulnerable group in the U.S. National Flood Insurance Program (NFIP) and a reform that reduces the premium subsidies offered to this group, while still keeping prices below actuarially fair levels. Policyholders are informed that their premium remains cheaper than a fair rate and thus underestimation of subjective losses by rational decision-makers is not sufficient to explain lacking demand for subsidized policies.

Using detailed policy-level panel data and a difference-in-differences analysis, we analyze the impact of reduced partial subsidies on flood insurance demand. We find strong demand reactions to decreases in the subsidy, observing three empirical patterns. Even though participation in the insurance program is still profitable, about 27% of policyholders cancel their insurance policy in reaction to the reform. This strong reaction on the extensive margin is not mirrored on the intensive margin, where the reform only had small and delayed effects. Lastly, the demand reactions

[1] Spinnewijn (2015) finds strong evidence for overly optimistic beliefs about expected losses in unemployment insurance. This finding motivates further research on the issue, but cannot establish a connection to underinsurance itself, because the analyzed unemployment insurance market is mandatory.

have a curious time pattern in that cancellations do not co-move with the price changes caused by the reform. For example, despite ongoing price increases until the end of our time series – 6 years after the reform – cancellations are concentrated in the 21 months immediately following the reform's passage, including before the reform had any effect on premiums (i.e., before it was implemented). Merging our policy-level data with real estate transaction data on the level of the ZIP code, we further show that regions with affected properties experienced an increased number of distress home sales after the reform. This indicates that the averse demand reaction to the reform increased the likelihood of financial hardship for the policyholders.

Our empirical setting is the U.S. market for residential flood insurance. The NFIP is the provider of almost all residential flood insurance in the U.S. and has a policy goal that at-risk populations are covered against severe losses.[2] Toward this goal, the NFIP frequently subsidizes insurance for older and riskier properties. The largest subsidies accrue to “severe repetitive loss properties” (SRL properties), a set of homes which account for only 1% of policies but make up 25–30% of total claims payments in the NFIP (GAO, 2010). Before the reform, SRL properties properties paid, at most, 35–40% of the actuarially fair premium for their insurance (GAO, 2008). The NFIP has operated at an increasingly worsening deficit since Hurricane Katrina and currently has an outstanding debt of $20.5bn (FEMA, 2018b). Out of concern that the NFIP had become too generous, Congress passed the Biggert-Waters Flood Insurance Reform Act of 2012 (Biggert-Waters), which authorizes rate increases on some NFIP policyholders to “ensure the fiscal soundness of the program” (FEMA, 2019). Beginning 1 October 2013, Biggert-Waters increased the premiums of SRL properties by 25% each year until the actuarially fair premium for the SRL property is reached. We examine in what way Biggert-Waters affected the insurance demand of SRL property owners. Our data are a panel of both SRL and non-SRL properties, and have more than two million policy-year observations from 2010 to 2018. Before the reform, a group of non-SRL properties was charged identical premium rates as a group of SRL properties, but their rates diverge after the reform. We employ difference-in-differences estimations using the group of non-SRL properties as a control group.

Our findings are not easily explained. As in previous studies of adverse selection, some selection is detectable in the market, but the effect is too small to explain the full demand reaction. Different from previous analyses, however, insureds were informed about prices being below expected costs. When announcing the price increase to the policyholders, the NFIP explicitly in-

[2] Households are pervasively underinsured against catastrophes: over 70% of disaster losses in the last decade were uninsured (Swiss Re, 2018). The U.S. Federal Emergency Management Agency (FEMA), which manages the NFIP, describes this policy goal as follows: “FEMA is committed to closing the insurance gap across the nation [...] One of [its strategic] goals is creating a culture of preparedness, which includes an ambitious `moonshot' to double the number of properties covered by flood insurance by 2022. Plain and simple, we need more insured survivors.” (FEMA, 2018a).

formed the affected group that even after the price increases, they were still paying significantly less than their actuarially fair rate. Decision-makers thus had all the information necessary to estimate expected losses correctly. We thus consider other potential explanations. Using census data on income, we find no evidence indicative of liquidity constraints playing a role as was suggested by previous literature (Casaburi and Willis, 2018; Ericson and Sydnor, 2018; Rampini and Viswanathan, 2019). Such constraints are also inconsistent with our results on extensive and intensive margin reactions.[3] Finkelstein et al. (2019) suggest cheaper repair markets without insurance as an explanation for lacking coverage. We argue that large differences are unlikely to appear in housing construction. While such effects likely play a role in health insurance, they are much less probable in flood insurance. [4] Matching additional data on the mitigation status of affected properties in 2018 to our panel data, we also find that mitigation as a substitute to insurance, as suggested by Ehrlich and Becker (1972), cannot fully explain the decision-makers' behavior. This is supported by the fact that mitigation is a very expensive substitute to financial risk management through the NFIP. The most likely explanation for our findings is that irrespective of the official communication from the NFIP, decision-makers still hold incorrect risk perceptions and that these misperceptions are induced by public officials' communications and media. This would be in line with the observations by Chalmers and Reuter (2012) regarding annuity demand and investor sentiment. We provide anecdotal and time series evidence for this claim, but cannot make any conclusive causal statements.

Our results contribute to the discussion of how to improve households' financial decisions. We highlight the fact that even when the benefits of participation are communicated explicitly, decision-makers might still prefer not to take part in the program, particularly when public sentiment for the program is negative. This is in line with the finding of Chalmers and Reuter (2012) who show that positive investor sentiment for the stock market can make retirees opt out of annuities with a risk-free rate of return larger than 10%. It is also consistent with previous work on both insurance decisions and other financial decision-making (Bhattacharya et al., 2012; Ragin et al., 2021) that shows that the mere provision of information is often insufficient for helping consumers make better financial decisions. Our findings thus raise implications for how to design effective communication of the consequences of participating in public insurance programs. We show that the simple verbal communication of facts seems insufficient to alter behavior. Research

[3]See Finkelstein et al. (2019) for a similar argument against a strong effect of liquidity constraints in their study of health insurance choices.

[4]A related explanation for low insurance demand is charity hazard (Browne and Hoyt, 2000) which proposes a crowding out of insurance demand due to public relief funds. We argue that this is an unlikely explanation of the lacking insurance demand, mostly because public relief funds are limited by law and even more limited in practice and because empirical evidence for charity hazard is scarce.

on private markets for financial products, however, highlights that more involved forms of communication such clearly laying out financial consequences (Bertrand and Morse, 2011) or even experience sampling of possible outcomes (Kaufmann et al., 2013) can be more effective.

We also contribute to the general literature on insurance demand and why it might be lacking for certain products and populations. Low demand for insurance policies, even when they are subsidized, has been documented in several markets and different potential explanations have been put forward as recounted above. Our findings are in line with the findings of Finkelstein et al. (2019) and Wagner (2020) that adverse selection is not the main driver of this empirical pattern. Our analysis does not rule out the potential explanation of incorrect risk perceptions, but it does show that the issue is more nuanced. Rather than simply having an incorrect perception of the risk which they face, as, for example, documented by Spinnewijn (2015) for unemployment insurance, decision-makers go further and mistrust officially communicated information on their exposure. This changes the focus of how to increase insurance take-up from mere information provision, to actively convincing households of the benefits of purchasing insurance.

Lastly, we contribute to the literature on the influence of media coverage on financial decisions. It is well known that stock prices can be influenced by how much media coverage the firm receives (Tetlock, 2007; Engelberg and Parsons, 2011; Hillert et al., 2014). For insurance products, it has been shown that media coverage of the risk in question can increase demand (Gallagher, 2014). We are, however, the first to consider media coverage of a public insurance program and its effects on participation in it. Our results highlight how important such coverage can be and that the success of public programs for vulnerable groups may depend on it.

The rest of the paper proceeds as follows. In the next section, we introduce our institutional setting and the main data we analyze. Section three shows our results regarding insurance demand on the extensive and the intensive margin. The fourth section discusses possible explanations for our findings. The last section provides evidence on incidences of financial hardship in geographic regions with SRL properties, discusses possible implications, and concludes.

2 Background

2.1 Institutional details

Congress established the NFIP in 1968 due to a lack of flood insurance offerings in the private insurance market (Michel-Kerjan, 2010). Today, standard homeowners insurance continues to exclude flood risk and the NFIP provides 95% of residential flood insurance in the U.S., insuring over 4 million households a year (Kousky et al., 2018).

The NFIP designs its insurance contracts, determines premium rates, and bears the claims risk. The Federal Emergency Management Agency (FEMA) administers the NFIP and develops and maintains flood insurance rate maps (FIRM), which identify flood zones. These flood maps influence building codes. For example, codes require new homes that are built in flood zones to be elevated. Homes built before local flood maps were developed (pre-FIRM homes) tend to be at greater risk as they often were not constructed with flooding in mind. The program has tended to offer preferential rates to pre-FIRM homes. It calculates their premium rates using methods that are less risk sensitive than the rating used for newer (post-FIRM) homes. For example, a property's elevation significantly influences the premium rates of post-FIRM homes but not pre-FIRM homes. The largest subsidies accrue to SRL properties. SRL properties are a specially designated group of properties that flood frequently. Using claims records beginning in 1978, the NFIP defines SRL properties as those with four or more claims payments of more than $5,000 each within a ten-year period.

On 6 July 2012, President Obama signed the Biggert-Waters Flood Insurance Reform Act into law, which began phasing out premium subsidies for pre-FIRM, SRL properties starting 1 October 2013. Biggert-Waters increased the premium rates on these properties by about 25% each year until the actuarially fair premium is reached. [5] Although the reform did not immediately take effect and pre-FIRM SRL and non-SRL properties in our sample were charged identical premium rates for another 15 monthes, they differed in their expectations about future premium increases after the passage of the law.

On 1 October 2012, after Biggert-Waters was signed into law but before SRL and non-SRL properties were charged different premium rates, both SRL and non-SRL property owners faced an identical premium rate increase. At the same time and unrelated to Biggert-Waters the NFIP required agents to re-underwrite SRL properties (e.g., update home characteristics, take photos of the front and rear) for policies that renew on or after 1 October 2012.

From 1 October 2013 on, Biggert-Waters implemented premium rate discrimination between SRL and non-SRL properties by phasing out SRL property owners' subsidies. Beginning 12 August 2013, about six weeks in advance of the first policy renewal with an increased premium, SRL property holders were officially informed about the changes to their flood insurance policy. From this date on, any policyholder got detailed information on the rate increases through renewal letters sent out to arrive 45 days before policy expiration. Among this information were two pieces

[5]The reform also removed subsidies for other properties. However, these properties were either dropped from our sample of analysis or the changes were reversed before Biggert-Waters came into effect.

critical for our identification. Policyholders were informed that 1) they had been paying a subsidized rate up until this point and 2) even though their premiums would increase, they would never exceed the actuarially fair rate. Specifically, the letter states:[6]

> "The Biggert-Waters Flood Insurance Reform Act of 2012, phases out subsidized rates for certain properties, including Severe Repetitive Loss (SRL) properties. Our records indicate that your building is an SRL property, and you have been paying a subsidized rate.
>
> Starting October 1, 2013 (...) your premium must increase 25 percent each year until it reaches the full-risk rate. The enclosed renewal bill reflects the statutorily-required 25 percent increase."

2.2 Data and descriptive statistics

The main data used in this study are policy-level panel data, including both SRL and non-SRL properties from 2009 to 2018. They show the insurance choices of each policyholder by year. Our data also include claims data from 1978 to 2012, which we match to the policy-level panel. Data include residential flood insurance policies on privately-owned properties in the NFIP. They provide policyholders' coverage limit and deductible choices, home characteristics (e.g., number of floors, whether the home has an elevation certificate, obstruction, etc.), location characteristics (zip code, community, etc.), and other pricing relevant variables. The data also include an indicator designating that a policy covers an SRL properties. For our primary analyses we examine the renewal and coverage choices of policies that were in force in 2009, the year our data begin, and track these policies over time. For example, these analyses do not include new policies that enter the program in 2010.

The data include over 5 million policies in 2009. Table 1 provides the steps to restrict the data to the group of SRL and non-SRL properties that were charged identical premium rates before Biggert-Waters but different rates afterward. We call properties in this subset the "Restricted Sample." The Restricted Sample is properties that are single-family homes that serve as a primary residence, are located in A flood zones, and have a pre-FIRM rating. Properties located in A flood zones (A, AE, A1–A30, AO, AH) lie in the non-coastal areas, which are not vulnerable to wave damage from storm surge, but are estimated to have at least a one percent annual probability of flooding. In total, the Restricted Sample includes 476,627 unique policies, 2,050 of which were

[6] See Appendix A for the full letter.

designated as an SRL property in 2009. We follow all policies in the 2009 cohort of the restricted sample until 2018, which provides us with more than 2 million policy-year observations to analyze their insurance choices.

Table 1: Sample selection process

Step	Description	Policies
0	All policies in 2009	5,002,696
1	Keep if single family home	4,245,726
2	Keep if primary residence	3,586,375
3	Keep if flood zone is A, AE, A1–A30, AO, AH	1,936,661
4	Keep if rated as a pre-FIRM property	476,627
Restricted Sample		476,627

Notes The table describes the steps of the sample selection process and outlines the number of policies kept with each data cleaning step.

Table 2 shows summary statistics on insurance choices and home and policy characteristics for SRL and non-SRL properties in the Restricted Sample in the year 2009. We first look at insurance choices. SRL and non-SRL properties exhibit similar building coverage ($140,373 versus $142,840). SRL property owners are, however, more likely to purchase contents coverage. In terms of deductibles, the NFIP provides default options. The policyholder can choose a deductible equal, below or above this default deductible. In 2009, the default deductible in the Restricted Sample was $1000. SRL and non-SRL property owners did not markedly differ in their deductible choices with the majority of both choosing the default deductible of $1000.

The table also provides information on home and policy characteristics, which we use as control variables in our empirical analyses. SRL and non-SRL properties exhibit similar elevation and construction characteristics, show similar CRS discounts, home age, and policy age. Further, we observe similar mean income on the ZIP code level among SRL and non-SRL property owners.

2.3 Effect of Biggert-Waters on premiums

The NFIP prices pre-FIRM properties using a relatively simple process. Coverage levels for building and contents coverage are multiplied by a factor with the first $60,000 of building coverage and the first $25,000 of contents coverage having different prices than the coverage exceeding those values. The sum of the resulting products is adjusted by four more factors reflecting the deductible choice, compliance with building standards after a loss, the community's risk rating

Table 2: Summary statistics in 2009

	SRL properties (2,050)			Non-SRL properties (474,577)		
	Mean	St. Dev.	Median	Mean	St. Dev.	Median
Coverage Choices						
Building Coverage	140,373	76,796	130,500	142,840	77,843	135,700
Contents Coverage	31,076	28,282	23,500	20,331	30,715	0
Has Contents Coverage	0.84			0.47		
Default Deductible ($1000)	0.64			0.53		
Deductible > $1000	0.17			0.27		
Home and Policy Characteristics						
CRS Score	0.92	0.81	0.90	0.92	0.86	0.95
No. Floors	1.54	0.66	1	1.49	0.66	1
Home Age	47	16	42	49	18	45
Income (ZIP code)	74,513	33,840	66,881	71,323	30,772	63,139
Policy Age	3.73	3.17	3	5.94	5.66	4
Has Elevation Certi cate	0.13			0.10		
Is Elevated	0.19			0.15		
Is Mobile Home	0.01			0.02		
Has Obstruction	0.20			0.23		

Notes Summary statistics for SRL and non-SRL properties in the Restricted Sample in 2009 are displayed. Table App.1 in Appendix B provides additional information on these variables.

and an administrative fee.[7] Summary statistics for the resulting annual premiums in the scal year 2009, separated by SRL and non-SRL properties are given in Table 3. We can see that SRL properties had about 25% higher premiums than other properties, but that the distributions look comparable otherwise. Because both types of properties were charged the same premium rates in 2009, these differences in premiums resulted from different choices in coverage limits or differences in the properties, such as the community they were located in, but not from the way the premiums were calculated.

Using the premiums and information on previous claims, we estimate the loading factor of each insurance policy. That is, we calculate the ratio of premiums to expected claims payments. For this, we estimate a frequency-severity loss model using claims-level, panel data between 2001 and 2012 (the year before the introduction of Biggert-Waters). Details on the procedure are given in Appendix D. Table 3 shows the results of this model for the 2009 scal year, describing both the annual probability of a claim as well as the expected value of the loss given a claim. Interestingly, SRL and non-SRL properties have claims of similar severity. However, SRL properties are much more likely to have a claim, with the annual claim probability being about seven times larger than for non-SRL properties.

The loading factor divides the policy-level premium by the expected loss (the probability of a

[7] For more details on the pricing process, see Appendix C.

Table 3: Premiums and risk

	Mean	St. Dev.	Percentiles p10	p50	p90
SRL Properties					
Premiums	1,119	648	369	994	2,081
P(Claim)	0.14	0.08	0.03	0.12	0.24
E[Loss j Claim]	40,338	18,549	20,045	35,917	71,798
Loading Factor	0.49	2.81	0.06	0.19	0.89
Non-SRL Properties					
Premiums	902	564	286	772	1,714
P(Claim)	0.02	0.01	0.00	0.02	0.04
E[Loss j Claim]	35,552	19,442	16,129	31,970	59,036
Loading Factor	3.94	17.50	0.34	1.49	7.71

Note: The table gives the summary statistics of the annual premiums as well as the estimated expected losses in the scal year 2009. Premiums are observed in the data and are calculated according to the procedure laid out in Appendix C. The other values are the results of a frequency-severity loss model, the speci cs of which are described in Appendix D.

claim multiplied by the expect loss given a claim). As described above, SRL and non-SRL policies differ signi cantly in their risk. This also translates to strong differences in their loading. The median loading factor for SRL policies is 0.19, indicating large subsidies for a signi cant portion of SRL property owners. In contrast, the median loading factor for non-SRL policies is larger than 1, indicating above actuarially fair insurance rates. Figure 1 displays a histogram of loading factors for SRL and non-SRL properties 2009. 91% of SRL and 37% of non-SRL exhibit loading factors smaller than one, indicating subsidized insurance premiums. This aligns with the motivation of the Biggert-Waters act that almost all SRL properties have actuarially advantageous insurance coverage.

Previous to Biggert-Waters, SRL and non-SRL properties faced the same premium rates. This changed in October 2013 when the reform took effect and the factors with which the coverage choices were multiplied started increasing more strongly for SRL properties than they did for non-SRL properties.[8] The goal of the reform was to increase premiums by about 25% each year until they reached an actuarially fair level. Note that even with such an annual increase, fair insurance premiums took some time to reach. The median SRL property had a loading factor of 0.19 before the reform, indicating that over 7 years of 25% annual increases were necessary to reach a value of 1.

Figure 2 shows the development of the mean premium for the subset of 2009 properties that

[8]Detailed trajectories of the premium rate increases for both types of properties are given in Tables App.2 and App.3 in Appendix C.

Figure 1: Histogram of loading factors in 2009 (own estimates)

Notes: The distribution of loading factors for SRL and non-SRL properties in 2009 is displayed. 91% of SRL and 37% of non-SRL exhibit loading factors smaller than one, indicating subsidized insurance premiums. Appendix D provides additional information on our estimation of loading factors.

continued to insure at the end of the panel dataset. Comparing the graphs for SRL and non-SRL policyholders shows that mean premiums exhibit signi cant deviances, as re ected in the 99% con dence intervals, starting the implementation of Biggert-Waters in 2013-Q4. At the end of the panel data set, in 2018, the mean premium for SRL properties was $3,771, about 88% higher than the mean premium for non-SRL properties.

3 In uence of Biggert Water on insurance decisions

3.1 Identi cation and estimation

We analyze the effect of Biggert-Waters on renewal decisions and coverage choices of SRL properties using data from scal years 2010 to 2018 including over 2 million policy- scal year observations. We focus on scal years, which start on October 1st of the previous calendar year because premium changes in the NFIP tend to coincide with them. We consider the implementation of Biggert-Waters a natural experiment as it created a plausibly exogenous source of variation in insurance prices by increasing insurance premiums for pre-FIRM SRL property owners (treatment group) but not pre-FIRM non-SRL property owners (control group). [9]

[9] Biggert-Waters appears to be a good candidate for such a natural experiment. It was enacted in July 2012, a relatively quiet period in terms of ood claims, which was roughly 3 months before Hurricane Sandy occurred. The part

Figure 2: Premium development between 2009 and 2018

Notes:Development of the mean annual premium for the subset of 2009 properties that continued to insure at the end of the panel dataset is displayed. For the sake of comparability, building coverage is xed to $140,000 and contents coverage is xed to $20,000.

Our main empirical speci cation is a difference-in-differences estimation. This estimation strategy leverages the panel structure of our data. First, we observe the insurance choices of each policyholder across years and so can leverage time series variation. Second, we observe both the treatment and the control group and so can leverage cross-sectional variation. The combination allows us to construct a counterfactual regarding what insurance choices SRL property owners would have made had the reform not occurred.

Using this counterfactual to estimate the treatment effect relies on two assumptions. First, we assume that, after the inclusion of controls, the responses of non-SRL properties capture common trends in the data. This rst assumption indicates that if the reform had not occurred, the average change in renewal rates for the SRL properties following the reform would have been the same as the average change in renewal rates for non-SRL properties, after accounting for model controls. For example, if a macroeconomic downturn caused non-SRL properties not to renew following

of the law pertaining to SRL properties was but one element of a large law that affected NFIP policyholders outside of our study. Thus, its passages does not appear to have been precipitated by underlying changes to SRL properties that might interfere with our estimation of the reform's effect.

the reform, SRL properties would have similarly decided not to renew. [10] Second, we assume that Biggert-Waters did not coincide with an additional change that might account for differences between SRL and non-SRL properties. We have been unable to identify changes concurrent with Biggert-Waters that affected only SRL properties or only non-SRL properties.

The empirical test for the effect of the reform on renewal decisions and coverage choices is an event study regression model. The dependent variable y for policyholder i at time t is a function of the treatment group indicator ($I(SRL_i)$), indicators $I(t = FY)$ for each fiscal year between 2010 and 2018, and their interactions:

$$y_{it} = \alpha I(SRL_i) + \sum_{j=2010;j\neq 2012}^{2018} \beta_j I(t = j) \times I(SRL_i) + X_{it}^{0}\gamma + FE_{zip \times year} + \varepsilon_{it} \tag{1}$$

Here, X_{it} is a vector of control variables, which account for characteristics of the home including its elevation relative to the flood plain, the number of floors, whether it is a mobile home, the age of the home, the age of the policy, and the NFIP's rating of the actions that the community has taken to reduce its flood risk (see Table 2 and Table App.1 in Appendix B for a description of each control variable). The estimation uses the fiscal year 2012 as the reference year such that the β_j are the estimates of Biggert-Waters' accumulated treatment effects in fiscal year j compared to that reference year.

As is indicated in Equation (1), our preferred specification includes fixed effects on the zip-code $\times$ year level. This estimation strategy allows for variable time fixed effects, but does not include individual fixed effects which are otherwise common in difference-in-differences estimators. When considering renewal decisions, the data structure differs from typical panel data. The dependent variable for a property is always 1 until the insurance policy is not renewed when the dependent variable becomes 0 for one year and the property subsequently is dropped from the data. With individual fixed effects, difference-in-differences estimations compare the average difference in the dependent variable before a treatment (in t_0) and after a treatment (in t_1) between the treated group (indicated s) and the control group (indicated c). They thus evaluate the expression $\frac{1}{n_s}\sum_{i=1}^{n_s}(y_{i;t_1;s} - y_{i;t_0;s}) - \frac{1}{n_c}\sum_{i=1}^{n_c}(y_{i;t_1;c} - y_{i;t_0;c})$. Only observations which are in the data both in t_0 and in t_1 are considered in the evaluation. In our data, this implies that all considered pre-treatment observations $y_{i;t_0}$ are equal to one. This reduces the above expression to

[10] Figure 3 shows the pre-reform renewal rates of the SRL and non-SRL properties and suggests support for the parallel trends assumption. While difference-in-differences estimation requires a common trend between treatment and controls, it does not require that the treatment and control groups are identical pre-treatment (Angrist and Pischke, 2008, Chapter 5), and we have reason to believe that SRL and non-SRL properties were not as the SRL properties had been previously identified as a vulnerable group by the NFIP.

$\frac{1}{n_s}\sum_{i=1}^{n_s} y_{i,t-1,s} - \frac{1}{n_c}\sum_{i=1}^{n_c} y_{i,t-1,c}$, removing the correction of pre-treatment differences from the estimator. With individual fixed effects, the estimation would thus only be identified correctly if the pre-treatment observations of SRL properties and non-SRL properties were identical on average. Crucially, this not only has to be true for the observed dependent variable (where the condition is trivial), but also for the unobserved variable which determines the observable renewal decision. Using a broader level fixed effect on the zip-code removes this problematic identifying assumption.

3.2 Renewal decisions

We show the results of estimating equation (1) with the renewal decision as the dependent variable in Table 4. Column (3) shows the result of our preferred specification which includes the full vector of control variables and zip-code by year fixed effects. To make the effects of including control variables and the fixed effects structure transparent, we also show alternative specifications in the other columns of the table. Column (1) includes no control variables and only includes fiscal-year fixed effects, which are necessary for the difference-in-differences estimation. Column (2) adds control variables and fixed effects on the zip-code level, but does not let these fixed effects vary over time. All columns show standard errors, heteroscedasticity-robust and clustered on the level of the individual property, in parentheses.

Despite the differences in the empirical specifications, all three columns paint a consistent picture. Previous to the fiscal year 2012, which serves as the reference time period in our estimation, renewal behavior did not differ in a statistically significant manner between SRL and non-SRL properties. For fiscal years 2013 and 2014, policyholders of SRL properties were significantly less likely to renew their insurance policy. The effects are not only highly statistically significant, but also carry strong economic implications at 17 and 18 percentage points, respectively. Overall, about one fourth of SRL property owners decided to stop insuring in response to the reform in the two years after its passage.[11] After the immediate effects in the fiscal years 2013 and 2014, the reform does not seem to have any additional effects on renewal behavior. For the remaining fiscal years 2015 through 2018, renewal rates do not differ between SRL and non-SRL properties.

The timing of the renewal behavior reactions is noteworthy on several accounts. While the decline in renewal probability coincides with the ratification of Biggert-Waters, the first effect ap-

[11] We calculate this 27% effect as follows. We assume that SRL properties would have renewed at the rate of non-SRL properties if not for the reform. Let p_{FY_13} and p_{FY_14} represent the renewal rates of non-SRL properties in fiscal year 2013 and fiscal year 2014, respectively. Also let $\hat{bw}_{FY\_13}$ and $\hat{bw}_{FY\_14}$ represent the estimated effect of Biggert-Waters on SRL properties in Column (3) of Table 4. Then, we calculate the total effect of the reform on SRL property renewals as a reduction of $p_{FY\_13} \cdot p_{FY\_14} - (p_{FY\_13} - \hat{bw}_{FY\_13}) \cdot (p_{FY\_14} - \hat{bw}_{FY\_14}) = 0.81 \cdot 0.77 - (0.81 - 0.17) \cdot (0.77 - 0.22) = 0.27$.

Table 4: Regression results: Renewal decisions of the 2009 policy cohort between 2010 and 2018

	Dependent variable:		
	P(Renew)		
	(1)	(2)	(3)
I(t = 2010) x I(SRL)	0.004	0.005	0.01
	(0.01)	(0.01)	(0.01)
I(t = 2011) x I(SRL)	0.02	0.02	0.01
	(0.01)	(0.01)	(0.01)
I(t = 2013) x I(SRL)	0.16	0.16	0.17
	(0.02)	(0.02)	(0.02)
I(t = 2014) x I(SRL)	0.17	0.17	0.18
	(0.02)	(0.02)	(0.02)
I(t = 2015) x I(SRL)	0.03	0.04	0.02
	(0.02)	(0.02)	(0.02)
I(t = 2016) x I(SRL)	0.01	0.03	0.01
	(0.02)	(0.02)	(0.02)
I(t = 2017) x I(SRL)	0.03	0.01	0.02
	(0.02)	(0.03)	(0.03)
I(t = 2018) x I(SRL)	0.04	0.01	0.04
	(0.03)	(0.03)	(0.03)
I(SRL)	0.01	0.001	0.01
	(0.01)	(0.02)	(0.02)
Controls	No	Yes	Yes
ZIP FE	No	Yes	No
Year FE	Yes	Yes	No
ZIP x Year FE	No	No	Yes
Clustered SE	Policyholder	Policyholder	Policyholder
R-Sq	0.0103	0.0518	0.1184
R-Sq (Within)	0.0002	0.0066	0.0085
Observations	2,068,727	2,024,035	2,024,035

Notes Results of estimating Equation (1) with the renewal indicator as the dependent variable. Fiscal year 2012 is the reference category. The columns differ by the control variables and xed effects included in the estimation as indicated in the lower panel of the table. Standard errors, heteroscedasticity-robust and clustered by policyholder, are in parentheses.

pears before the actual implementation of the reform. The rate increase preceding the scal year 2013 applied similarly to SRL and non-SRL properties (see Table App.2). Yet, we nd that it affected their renewal decisions differently in the sense that SRL properties reacted much more negatively to it. Further, while we see a strong initial reaction to rate increases due to Biggert-Waters in scal year 2014, we see no difference in renewal rates due to the reform in later scal years. This result is not easy to interpret. On the one hand, premium rates for SRL properties continued to increase compared to those of non-SRL properties over time (see Figure 2). On the other hand, excess non-renewals of SRL properties in scal years 2013 and 2014 could have led to a selected sample of remaining SRL properties in later years which make the difference-in-differences results less interpretable. For the discussion of potential mechanisms, we thus focus on the two years immediately after the reform and delay discussion of the later years in our data to Section 5. When combining the annual effects, we see that the reform resulted in a persistent effect. As we explore in detail in Appendix E, the share of SRL properties of the 2009 cohort which remain insured is lower than that of the non-SRL properties from the scal year 2013 on and throughout the entire observation period.

We complement the analysis in Table 4 with an event study approach that examines quarterly renewal rates instead of annual renewal rates. We implement this approach by replacing the scal year dummies in equation 1 by quarter-year dummies. The 2012-Q2 dummy, representing the last quarter before the passing of Biggert-Waters, is the omitted category. Figure 3 plots the coef cients and their 99% con dence intervals. Renewal rates are not statistically different for the nine quarters before the rati cation of the reform. The event study model, thus, provides general support for the parallel trends assumptions. Figure 3 further shows that the reductions in renewal rates begin with the rst rate increase after the rati cation of the reform (2012-Q4) and persist until after the implementation of Biggert-Waters (2013-Q4). Starting 2014-Q3, renewal rates are no longer statistically different from pre-reform levels.

3.3 Coverage choices

In addition to demand adjustments on the extensive margin in the form of renewal rates, we also analyze adjustments on the intensive margin. For this, we consider the effect of Biggert-Waters on coverage choices of SRL and non-SRL properties. To give all policyholders' decisions equal weight, we normalize the dependent variable and de ne it as the ratio of building coverage and

Figure 3: Renewal decisions between 2010 and 2018: Event study coefficients

Notes: Figure is based on the estimation of the equation $y_{it} = I(SRL_i) + \sum_{j=2010Q1; j \neq 2012Q2}^{2018Q3} {}_j I(t=j) I(SRL_i) + X_{it}' + FE_{zip \times quarter} + \varepsilon_{it}$, with the renewal indicator as the dependent variable, t as a quarterly indicator and X_{it} as the vector of the same control variables used in the annual analysis reported in Table 4. The figure reports the ${}_j$ coefficients and their 95% confidence interval over time. Standard errors are heteroscedasticity robust and clustered by policyholder. Vertical lines indicate the relevant events of the reform.

building coverage chosen in 2009:

$$coverageChoice_{it} := \frac{buildingCoverage_{it}}{buildingCoverage_{i,2009}}. \tag{2}$$

An insurance coverage decision encompasses both an upper limit and a deductible. However, since most policyholders choose the default deductible, we focus our analysis on the limit.

We consider a quarterly event study with the coverage level as the dependent variable. Results of an estimation with annual treatment dummies are reported and discussed in Appendix F. The results of the quarterly event study are reported graphically in Figure 4. Neither the strong extensive margin reaction to the not-Biggert-Waters related premium increases in October 2012, nor those after the first Biggert-Waters induced premium increases in October 2013 are mirrored on the intensive margin. While the quarterly coefficient estimates in the fiscal years 2013 and 2014 are somewhat more variable than they are in previous years, they do not predominantly show negative values and are not statistically significantly different from zero. Coefficient estimates only become consistently negative from the fiscal year 2016 onward. While the quarterly coefficients are not consistently statistically significant, results of an annual event study reveal statistically significant, negative coefficients for the fiscal years 2017 and 2018 (see Appendix F).

Figure 4: Coverage Limits between 2010 and 2018: Event study coefficients

Notes: Figure is based on the estimation of the equation $y_{it} = \alpha I(SRL_i) + \sum_{j=2010Q1; j\neq 2012Q2}^{2018Q3} \beta_j I(t=j) I(SRL_i) + X_{it}'\gamma + FE_{zip \times quarter} + \varepsilon_{it}$, with the coverage level as the dependent variable, t as a quarterly indicator and X_{it} as the vector of the same control variables used in the renewal analysis of Section 3.2. The figure reports the β_j coefficients and their 95% confidence interval over time. Standard errors are heteroscedasticity robust and clustered by policyholder. Vertical lines indicate the relevant events of the reform.

In the coverage analysis, coefficients in the later years after the reform have to be interpreted cautiously. Tracking a cohort of insurance policyholders over time creates a selected sample in later years of the analysis because some property owners decide not to renew. Unobservable home and policy characteristics at the beginning of our observation period can differ from those at the end of our observation period. Parallel trends of the entire cohort before Biggert-Waters are thus not a good test for identification of the difference-in-differences estimator for the SRL properties remaining in the late periods of our data. One way to avoid this selection process is to limit our analysis to those properties which do renew in every year until the the fiscal year we want to analyze. We show in Online Appendix F that the parallel trends assumptions seems to hold for each analyzed subsample. Thus, while we thus cannot exclude sample selection effects completely, the main results from Figure 4 are not affected by them.

4 Potential mechanisms

Our difference-in-differences estimations identify three major consequences of the Biggert-Waters reform on the insurance demand of SRL property owners. On the extensive margin, we see a strong immediate reaction to the reform. For two years after the passage of the reform, SRL property owners renew their policy less often than the owners of non-SRL properties. This reaction is not mirrored on the intensive margin. For the years immediately following the reform, we see no or only a very small effect on the coverage choices of SRL property owners. Statistically signi cant reactions on the intensive margin can only be detected in the last two years of our observation period, 4 years after the passage and 3 years after the implementation of the reform. The last major empirical nding is that the timing of the demand reactions follows an unexpected pattern, as is illustrated in Figure 5. Half of the effect on the extensive margin can be observed after the passage of the reform, but before it was implemented and premium rates actually started to differ between SRL and non-SRL properties. Later premium rate increases after 2014 did not have any detectable effect on extensive margin decisions.

We also document how all owners of SRL properties were informed that the rates they were currently paying were subsidized and that the reform's rate increases would stop once actuarially fair premiums had been reached. This excludes rational (subjective expected utility) policyholders with incorrect risk perception as a potential explanation of our ndings. Such decision makers can have subjective loss probabilities divergent from their objective counterparts. However, they also need to incorporate new information on loss probabilities, such as the letter sent by the NFIP, and act accordingly. In the remainder of this section, we will thus discuss other mechanisms as potential explanations of our three empirical ndings.

4.1 Adverse selection

If the insurance provider's information suggests that premiums are better than actuarially fair, then the canonical explanation of low insurance demand is that a subgroup of policyholders have private information about their risk which suggests that their insurance premium after the rate increase is not actuarially fair. This mechanism would thus attribute excess cancellations of SRL

Figure 5: Renewals and Prices

Notes: This figure overlays the treatment effects plot shown in Figure 3 with the evolution of prices shown in Figure 2. Prices are relative to the the reference period Q2 2012. The vertical line marks Q3 2012, the quarter when the reform was enacted.

property holders to adverse selection. While we know that the aggregate premiums collected from SRL properties before Biggert-Waters was not sufficient to cover the aggregate expected losses of these properties, it is possible that for some of the SRL properties premiums actually exceeded expected losses.

Although it is not a good explanation for the timing of the observed demand reactions, adverse selection could explain two of our three major findings. Specifically, it is fathomable that adverse selection causes demand reactions to premium increases and that these reactions are (under certain conditions) concentrated on the extensive margin. To analyze whether this explanation has merit, we conduct two empirical tests. In the first, we use our loading factor estimation (as reported in Figure 1) to consider whether our estimate of a property's risk influences the decision to renew a policy. For this, we repeat our main analysis, but add an interaction of the difference-in-differences variable with our calculated loading factors. Since higher loading factors imply fewer (or no) subsidies, adverse selection would predict these interaction terms to have a negative coefficient. However, as we can see in Table 5, the estimated coefficients are very small and of mixed signs.

In 2014, the year with the largest effect in the initial analysis, the estimated coef cient is negative and signi cant, but very small in magnitude, particularly when considering that the inter-decile range of the loading factor for SRL properties is 0.83, as can be seen in Table 3.

We also report an analysis of adverse selection inspired by Einav et al. (2010). They observe that when private information guides the insurance choices of policyholders, then higher coverage levels need to be associated with higher ex-post loss levels. This idea can be transferred to our empirical setting. When private information guides renewal decisions, policyholders with smaller expected cost would be the rst to cancel their insurance policies when premium rates are increased. After every round of premium increases, the remaining pool of insured properties should thus have higher average expected cost than the pools of previous years. Table 6 lists the average expected cost, calculated with loss data until 2009, of all SRL properties for each scal year. We use expected costs based on our loading factor estimations for two reasons. First, ood insurance losses are lumpy over time and thus annual comparisons of ex-post loss realizations are not informative. This is particularly the case here, because Hurricane Sandy occurred only a few weeks after Biggert-Waters was passed. Second, our data regarding losses after 2010 cannot fully be matched to our insurance demand data, such that selection issues can appear when comparing loss ratios across time. Using expected costs based on our loading factor estimations circumvents both these issues.

We can see in Table 6 that expected cost change only slightly over the scal years. While the average price of the insurance policies almost doubles from 2012 until 2018, the expected cost increase by a mere 3.7%. If adverse selection was the sole driver behind our result, then costs would have had to increase by at least 28%.[12] Both of the analyses reported here thus do not point towards adverse selection as a main driver of our results. They are, however, not without their weaknesses. Both analyses rely on our loading factor estimates for measuring the risk of an insured property. Our loading factors differ from those calculated by the NFIP and thus using them to detect adverse selection is a worthwhile exercise. Nevertheless, our estimates are based on the data observable by the NFIP. Thus, if there are crucial unobservable risk factors which are known to the policyholders, adverse selection could still be a possible explanation for the observed demand reaction in the scal year 2014. The timing of the observed demand reactions, however, makes this unlikely. First, adverse selection cannot explain the excess non-renewals by

[12]We arrive at this number for the limiting case of risk neutral policyholders. In this case, policyholders will cancel their policy if their expected cost exceeds their price. We observe 25% excess calculations, spread equally across scal years 2013 and 2014. The policyholders cancelling in 2013 (2014) would have a maximum expected cost of 7.1 (8.1). This implies that in order to have an average cost of 48.5 in 2012, the other 75% of properties would have to have average expected cost of 62.1, which is 28% higher than 48.5. This is a lower bound on the cost increase, because we assume risk neutrality and that policyholders' expected cots are at the upper bound of the price increase.

Table 5: Regression results: Loads

	Dependent variable:		
	P(Renew)		
	(1)	(2)	(3)
I(t = 2010) x I(SRL) x Load	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)
I(t = 2011) x I(SRL) x Load	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
I(t = 2013) x I(SRL) x Load	0.001	0.001	0.003
	(0.001)	(0.001)	(0.001)
I(t = 2014) x I(SRL) x Load	0.005	0.005	0.004
	(0.001)	(0.001)	(0.001)
I(t = 2015) x I(SRL) x Load	0.02	0.02	0.02
	(0.02)	(0.02)	(0.02)
I(t = 2016) x I(SRL) x Load	0.03	0.03	0.03
	(0.01)	(0.01)	(0.01)
I(t = 2017) x I(SRL) x Load	0.01	0.01	0.01
	(0.01)	(0.01)	(0.02)
I(t = 2018) x I(SRL) x Load	0.0005	0.001	0.01
	(0.01)	(0.01)	(0.01)
I(SRL) x Load	0.002	0.002	0.001
	(0.0005)	(0.001)	(0.001)
I(SRL)	0.05	0.01	0.01
	(0.03)	(0.03)	(0.03)
Controls	No	Yes	Yes
ZIP FE	No	Yes	No
Year FE	Yes	Yes	No
ZIP x Year FE	No	No	Yes
Clustered SE	Policyholder	Policyholder	Policyholder
R-Sq	0.0089	0.0518	0.1184
R-Sq (Within)	0.0002	0.0066	0.0084
Observations	2,031,043	2,024,035	2,024,035

Notes Results of estimating Equation (1) with the renewal indicator as the dependent variable and an added interaction with the loading factor. Fiscal year 2012 is the reference category. The columns differ by the control variables and xed effects included in the estimation as indicated in the lower panel of the table. Standard errors, heteroscedasticity-robust and clustered by policyholder, are in parentheses.

Table 6: Average Expected Cost by Year for SRL Properties

Year	Price	Expected Cost
2012	6.8	48.5
2013	7.1	47.3
2014	8.1	49.0
2015	9.0	50.4
2016	10.3	50.6
2017	11.4	50.3
2018	13.0	50.3

Note: Expected cost is the expected payment per $1,000 of insurance coverage, in $2012.

SRL property owners *before* the implementation of Biggert Waters. Any demand reaction due to adverse selection should be observed for both SRL and non-SRL properties and should thus not influence our difference-in-differences estimate. Second, if adverse selection was the main driver behind the renewal behavior of SRL property owners, then it would be unlikely that we see no reactions to the rate increases after fiscal year 2014. As we can see in Figure 5, premiums for SRL properties continue to rise when compared to those for non-SRL properties. Yet, our estimations show only little reaction to these later increases.

4.2 Incorrect risk perception due to media coverage

Because private information seems to play a minor role in the insurance choices of SRL policyholders, we return to incorrect subjective risk perception as a possible mechanism. This explanation is evoked commonly for flood insurance choices (Wagner, 2020; Gallagher, 2014). In our setting, we initially excluded subjective loss expectations as a possible explanation because policyholders were informed by the NFIP that they were paying subsidized rates and that this would still be true after the premium increases. However, it is possible that policyholders did not trust this official communication. If they rather thought that the current rate level adequately reflected their risk of a flood loss, then premium rate increases could lead policyholders to cancel their policy. Media coverage of the Biggert-Waters reform likely plays a role in this mechanism. Official communication from the NFIP about Biggert-Waters reached policyholders 45 days before their renewal decision in fiscal year 2014. However, we already see demand reactions to the Biggert-Waters reform in fiscal year 2013. If media coverage of the reform preceded the official communication and made SRL property owners think that the upcoming rate increases were making flood insurance coverage disadvantageous for them, then the pattern we observe in the data could emerge.

We analyze this potential explanation by doing a structured search of media outlets through Google News for the terms “Biggert-Waters" and “Flood insurance Reform Act of 2012" between 2010 and 2016. After eliminating duplicates from the data, our search rendered 185 articles from various sources. Media coverage is split between regional and national general audience outlets (such as al.com or the New Your Times) and outlets targeted at insurance professionals, such as the Insurance Journal. While the former reaches policyholders directly, information from the latter can reach them through their insurance agents.

Articles indeed seem to enforce the view that Biggert-Waters could lead to problems for SRL property owners. Some descriptions of the consequences were relatively neutral, like that of the New York Times in November of 2012:

> “FEMA, as a result of this year's legislation, has the authority to raise premiums by as much as 25 percent per year over the next ve years. The increases will be imposed mostly on [...] properties that repeatedly ood, but whose owners have paid far below market insurance rates."

Other coverage detailed the potential negative consequences in more dramatic terms. The Boston Globe cites one policyholder in September 2013 saying

> “I could be looking at ood insurance costing me $10,000 or more a year, [...] I don't know too many people who can come up with that kind of money."

The negative sentiment was further ampli ed by the public statements made by regional activists and politicians. Michael Hecht, president and chief executive of the Greater New Orleans Inc. Regional Economic Alliance, is quoted[13] that Biggert-Waters

> “almost amounts to an illegal taking where somebody stands to lose their most important asset."

And Senator Robert Menendez (D-NJ) is quoted in the Insurance Journal[14] that his constituents came to him

> “in tears, expressing horror stories of skyrocketing ood insurance premiums that threatened to force them from their homes."

[13]https://www.propublica.org/article/without-a- nal-map-new-york-rebuilds-on-uncertain-ground
[14]https://www.insurancejournal.com/news/national/2014/03/24/324217.htm

While it is hard to make causal claims about the influence of media coverage on the perception of the Biggert-Waters premium rate increases, we can inspect the time series patterns of the amount of media coverage of the analyzed reform and of policyholder renewal behavior to gain an impression of how well the two patterns align. Figure 6 shows both time series on a quarterly basis and scaled into the [0,1] and [-1,0] interval, respectively. We can see that the cancellations closely mirror media coverage with what appears to be a small time lag. Applying a Granger test of causality supports the idea that media coverage causes cancellations ($F = 5.4$, $p < 0.01$).

Figure 6: Quarterly time series of media coverage and renewal decisions

Notes: The figure plots the renewal rate for SRL properties, the median price paid by SRL properties per $1,000 of coverage, and media coverage of the Biggert-Waters reform. Media coverage counts the total number of media articles on Google News containing the words “Biggert-Waters" and/or “Flood Insurance Reform Act of 2012" each quarter. Values are scaled based on their range in the time series. For example, the counts for media coverage range from 0 to 33 across quarters, which take the values 0 and 1 in the figure. The vertical line marks Q3 2012, the quarter when the reform was enacted.

While neither the quotes from the media coverage or politicians, nor the time series data allow us to make definitive causal statements about whether the media coverage has caused the policyholder reaction to Biggert-Waters, the mechanism could explain all the empirical observations made in Section 3. If policyholders thought that premiums are now strongly unfavorable to them, they will cancel their insurance coverage outright rather than adjust it on the intensive margin. Also, the puzzling timing of excess cancellations could be due to the media coverage which started with the passage of the law and not with its implementation and became much more muted after the fiscal year 2014.

4.3 Liquidity constraints

Another motive for lacking demand for actuarially fair or even subsidized insurance has recently been proposed by Ericson and Sydnor (2018). Among other aspects, they consider ex-ante liquidity constraints. If households have insufficient income to cover increasing insurance premiums and are unable to borrow against current assets or future income, then it might be rational for them to forego purchasing full insurance even if the premiums are actuarially fair or subsidized. [15]

We consider the validity of liquidity constraints as a potential explanation for our empirical findings by analyzing the effect of income on the reaction to Biggert-Waters. Higher income households react less to changes in liquidity (Olafsson and Pagel, 2018; Gross et al., 2021) and thus should have less extensive demand reactions to Biggert-Waters. Our empirical analysis is similar to the one regarding the loading factor in Section 4.1. The difference between our specifications there and the analysis here is that we only have information on household income on the zip-code level. Thus, our analysis is limited to documenting heterogeneous behavior on this geographical aggregate. As we can see in Table 7, coefficient estimates of the interaction are small, mixed in sign and, at best, marginally significant. The empirical analysis thus provides no indication that liquidity constraints play a role in the response of SRL policyholders to Biggert-Waters.

In addition to the lack of indicative results in Table 7, liquidity constraints also have a hard time explaining the complete empirical pattern observed in our data. If they were the driving force behind demand reactions to Biggert-Waters, then policyholders should have adjusted their demand on the intensive margin rather than on the extensive margin (see Finkelstein et al., 2019, for a similar argument). Yet, we see an initial reaction to the reform which seems to be concentrated fully on the extensive margin. Further, liquidity constraints could only explain the asymmetric renewal behavior between SRL and non-SRL properties in the fiscal year 2013 if SRL property owners were more liquidity constrained than non-SRL property holders. However, descriptive statistics on 2009 coverage levels, the average number of floors of the house and zip-level income suggest no such discrepancy. Liquidity constraints are thus an unlikely explanation for our findings.

[15] To a certain extent, the argument is related to that of Cook et al. (1977) if one considers the house as an irreplaceable good given the financial situation of the household. This is corroborated further by the fact that after catastrophic flood losses many policyholders choose to pay off their mortgages and relocate rather than to rebuild their house (Gallagher and Hartley, 2017).

Table 7: Regression results: Income

	Dependent variable:		
	P(Renew)		
	(1)	(2)	(3)
I(t = 2010) x I(SRL) x ZIP Income	0.002	0.002	0.002
	(0.004)	(0.004)	(0.004)
I(t = 2011) x I(SRL) x ZIP Income	0.002	0.003	0.004
	(0.004)	(0.004)	(0.004)
I(t = 2013) x I(SRL) x ZIP Income	0.002	0.002	0.003
	(0.01)	(0.01)	(0.01)
I(t = 2014) x I(SRL) x ZIP Income	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)
I(t = 2015) x I(SRL) x ZIP Income	0.002	0.001	0.002
	(0.01)	(0.01)	(0.01)
I(t = 2016) x I(SRL) x ZIP Income	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)
I(t = 2017) x I(SRL) x ZIP Income	0.01	0.01	0.004
	(0.01)	(0.01)	(0.01)
I(t = 2018) x I(SRL) x ZIP Income	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)
I(SRL) x ZIP Income	0.004	0.002	0.002
	(0.003)	(0.003)	(0.003)
I(SRL)	0.01	0.04	0.02
	(0.06)	(0.06)	(0.06)
Controls	No	Yes	Yes
ZIP FE	No	Yes	No
Year FE	Yes	Yes	No
ZIP x Year FE	No	No	Yes
Clustered SE	Policyholder	Policyholder	Policyholder
R-Sq	0.0103	0.0519	0.1184
R-Sq (Within)	0.0002	0.0066	0.0085
Observations	2,060,751	2,024,035	2,024,035

Notes Results of estimating Equation (1) with the renewal indicator as the dependent variable and an added interaction with income on the ZIP level (in $1,000s). Fiscal year 2012 is the reference category. The columns differ by the control variables and xed effects included in the estimation as indicated in the lower panel of the table. Standard errors, heteroscedasticity-robust and clustered by policyholder, are in parentheses.

4.4 Mitigation

Risk management is a multifaceted activity and insurance is not the only way to manage flood risk. Homeowners can take several mitigation measures to increase their house's resilience towards floods. If these measures are priced into the insurance policy, they are often complements to flood insurance (Ehrlich and Becker, 1972). However, if rates are not adjusted, mitigation and insurance can be seen as substitutes to one another. Because insurance rates of pre-FIRM properties are set irrespective of mitigation measures, one possible explanation to our empirical findings is that SRL property owners stopped insuring their houses and instead increased their mitigation efforts. This could explain all three documented behavioral patterns. The passing of Biggert-Waters can change the discounted benefit/cost analysis of mitigation measures for SRL-properties and thus lead to insurance cancellations even before the law is in effect. Additionally, lack of adjustment in the premium rates could lead to predominantly extensive margin reactions by policyholders.

To test the potential explanation we link our main data with a second data set, resulting from a Freedom of Information Act data request. The data is a 2018 cross section of insured and uninsured properties which had been classified as SRL properties by the NFIP at some point in there existence. We were able to link 656 of these properties with properties from our 2009 cohort which were still insured in the 2012 fiscal year, the fiscal year before the reform showed any effects on SRL properties. In 2015, 462 of these 194 properties were still insured and 462 did not have coverage from the NFIP, such that they had canceled their insurance policy at some point in the fiscal years 2013 and 2014.

Table 8 shows the claim history and mitigation status for the insured and uninsured properties. The likelihood for mitigation is 16 percentage points higher in the uninsured group than in the insured group. Given that 27% of the SRL properties insured in 2012 cancelled their policy in response to Biggert-Waters, the choice to mitigate instead of insure can, at most, explain 42% of the cancellation.[16] However, this number has to be seen as a generous upper bound, because it is also likely that properties with mitigation measures in place before 2012 were more likely to cancel their insurance policy than unmitigated properties. In fact, if media induced risk misperceptions are the main driver for excess cancellations among SRL properties, as is argued in Section 4.2, then it is probable that owners of (partially) mitigated properties were particularly perceptible to this bias. Mitigation as a substitute for insurance can thus only explain part of the observed behavior.

[16] 27% cancellations due to Biggert-Waters is equivalent to 177 of the 656 analyzed properties. 16% of properties with additional mitigation of the 452 uninsured properties equals 74 properties. 74 of 177 properties are 42%.

Table 8: Statistics of SRL properties in scal year 2012 in dependence of their insurance status in scal year 2015

	Uninsured in FY 2015 (462)			Insured in FY 2015 (194)		
Claims Statistics (as of Oct. 2012)	Claims Payments	Tot. Payment	Tot. No. Losses	Claims Payments	Tot. Payment	Tot. No. Losses
Mean	47,374	219,633	6	48,385	231,343	7
St. Dev.	54,144	154,180	3	56,747	154,737	3
Pctl(1)	2,243	18,469	2	1,889	26,337	2
Pctl(25)	13,614	117,042	4	14,017	130,012	4
Median	30,725	190,621	6	30,253	207,695	6
Pctl(75)	59,035	284,488	7	60,669	291,172	8
Pctl(99)	277,069	801,688	18	314,954	713,883	21
Type of Mitigation (as of May 2018)						
Building acquired and demolished as part of a program			0.02			0.01
Building elevated to or above BFE			0.17			0.05
Building demolished but not acquired through a federal program			0.01			0.00
Building replaced by a new elevated/ oodproofed building			0.04			0.02
Building protected by a ood control/stormwater management project			0.00			0.00
None			0.76			0.92

Notes Claims and mitigation statistics of SRL properties in scal year (FY) 2012 are displayed. Properties that are insured in scal year 2015 are compared with properties that are uninsured in scal year 2015. Monetary amounts are converted into 2018 dollars.

4.5 Institutional Explanations

We also consider several potential institutional explanations. One is that the NFIP is not a monopolistic insurer and thus the SRL property owners could have simply switched to the private ood insurance market. This, however, is an unlikely explanation. Private insurers are generally better at pricing catastrophe insurance than the public system (see, e.g., Lin, 2020, for earthquake insurance). It is thus unlikely that properties with subsidized rates in the NFIP would be able to get cheaper insurance coverage on the private market. Since none of our analyses in Section 4.1 imply that the canceled insurance policies were predominantly unsubsidized, a crowding out of the public market by the private market is an unlikely explanation for the behavior we observe.

When nancing a home at risk of a ood through a federally backed mortgage lender, the home in question must be insured against this risk. One explanation for excess cancellations could thus be, that the passing of Biggert-Waters coincided with mortgages being paid off and thus the requirement for ood insurance expiring. This explanation, however, would require that in the scal years 2013 and 2014, mortgages of SRL properties were more likely to expire than those of non-SRL properties. While we have no data available to test this hypotheses, it seems unlikely that it is true. There is no reason to expect that the SRL property owners should have a higher propensity for paying of their mortgage in these two years than the owners of regular homes.

Finkelstein et al. (2019) determine the pricing of uncompensated care as the most likely explanation for their observation of lacking demand for subsidized health insurance. The argument is that decision-makers pay more money for care that is covered by insurance companies than for care which is paid for out-of-pocket. While such an explanation is likely in health care, where the source of funds determines the pricing, it is unlikely to play a role in our empirical setting. Health insurance usually features expense-based reimbursement which often bypasses the insured completely. In flood insurance policies, damages are often assessed by the insurance companies, but indemnities are paid out to the insured who then purchase replacement construction on the free market. Because indemnity-financed and out-of-pocket purchases are not distinguishable for the contractors, it is unlikely that the prices differ between the two.

A related explanation sometimes invoked for lacking demand for insurance against catastrophe risks is that of charity hazard (Browne and Hoyt, 2000). The argument is that households do not purchase insurance because they expect to get charitable donations from public relief funds in case of a large catastrophic loss. In general, this seems like an unlikely explanation because public relief funds are limited by law and even more limited in practice and because empirical evidence for charity hazard is scarce, particularly for extensive margin reactions (Kousky et al., 2018). In our specific case, the explanation is even less likely than in general, because by their very definition, SRL properties get flooded more often than most other properties. As such, it is not particularly likely for them that a loss event will coincide with a larger scale catastrophic event that would lead to charitable donations or the possibility to access public relief funds.

Lastly, it could be the case that Biggert-Waters decreased the value of SRL properties and that this decrease in home value lead owners to cancel their insurance policy. This explanation, however, also seems unlikely. While it is true that changes in insurance premiums can affect home values (Nyce et al., 2015), it is unclear whether sudden decreases in home value make homeowners cancel their insurance policy. In fact, research on house prices and the incidence of home fires suggest that policyholders might want to keep their coverage and hope for a loss such that the indemnity reimburses the construction value rather than the current value of the house (Eriksen and Carson, 2017).

5 Discussion and Conclusions

In this study we analyze demand reactions to a reform of the NFIP in which previously heavily subsidized policies faced premium increases which would eventually lead to actuarially fair premiums. We document that policyholders react strongly to these premium increases in the sense that many of them choose not to renew their policies. The timing of these reactions is peculiar. They are concentrated in a brief time window that starts before the reform takes effect and ends years before all of its implicated premium increases have taken effect. We argue that while there are multiple possible explanations for people refusing to purchase subsidized insurance, it seems most likely that media coverage and statements by of cials skewed the public's perception of the reform and lead to the strong and immediate reaction by policyholders.

The temporal pattern of the lacking renewals by SRL policyholders has another likely implication. Renewal behavior of the affected policyholders seems by and large indistinguishable from that of the unaffected policyholders after the 2014 scal year. Those who did not cancel their policies soon after the reform stayed in the program and bene ted from the still subsidized insurance plans until the end of our observation period. The behavioral frictions which caused excess cancellations thus apparently only applied to a subgroup of the policyholders. This observation gives further rise to the idea that some people are more able to understand nancial products and make decisions concerning them (Lusardi and Mitchell, 2014; Corgnet et al., 2018). Identifying those individuals with low nancial literacy and helping them make better decisions is one of the key challenges for future research in household nance.

Not purchasing subsidized insurance means that households forgo an opportunity to reduce their risk and earn money in expectation. While this is not bene cial to the household in general, it could be questioned how strong the effect on the household's welfare is. To answer this question, we consider the worst possible consequence of not having ood insurance: losing ones house. For this purpose, we access data on home transactions in the CoreLogic database. We use this data to compare the number of distress sales between ZIP codes which contained SRL properties and those which do not before and after the reform.

Figure 7 shows the result of a quarterly, ZIP-code level difference-in-differences event study analysis akin to those used for analyzing renewal and coverage level choices. The dependent variable is the share of total quarterly sales that are distress sales, the average of which is about 11% over the entire observation period. The gure shows that while the estimates are noisy, there seems to be a clear positive effect on the number of distress sales after the reform. For most quarters, the share of distress sales seems to increase by between 0.5 and 1 percentage points, which constitutes a relative increase between 5 and 9%. Because we are unable to link the sales record in the CoreLogic data to the individual properties in the NFIP data, we cannot establish a clear causal link between policy non-renewals and distress sales. However, we take Figure 7 as indicative evidence that the behavioral frictions associated with the reactions to Biggert-Waters increased the likelihood of nancial hardship for the affected policyholders.

Figure 7: Distress Sales, Share of Total Sales

Notes: Figure is based on the estimation of the equation $y_{kt} = \sum_{j=2009Q1; j \neq 2012Q2}^{2018Q3} \beta_j I(t = j) \cdot I(SRL_k) + FE_{zip} + FE_{quarter} + \epsilon_{kt}$, with t as a quarterly indicator. The dependent variable is the share of total quarterly sales in ZIP code k that are distress sales (short sales and foreclosure sales). The indicator $I(SRL_k)$ equals 1 for ZIP codes that have at least one insured SRL property in the year before the reform was enacted (Q3 2011 to Q2 2012). The figure reports the β_j coefficients and their 95% confidence interval over time. Standard errors are heteroscedasticity robust and clustered by ZIP code. The vertical line marks Q3 2012, the quarter when the reform was enacted.

Subsidized public insurance programs are a common feature of many social welfare states and participation in them can greatly benefit households. Particularly for low-income groups, such programs provide an important opportunity to manage risks which can pose an existential financial threat. In this paper, we show that when policy action affects the amount of subsidies, households can react by cancelling their participation in the program. This reaction blunts the effectiveness of subsidized public insurance programs as a policy tool and endangers the financial welfare of vulnerable households. Our results suggest that even if the official communication to households clarifies that participating in the program is still beneficial, households might still be influenced by other sources of information, such as the media and statements made by officials. It thus seems paramount to embed information for policyholders in a more holistic communication strategy and inform other stakeholders about the details of the changes made to the public insurance program. Further research is necessary to find optimal forms of communication with households and to determine the characteristics of those households which need to be informed in particular detail.

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Appendix A Renewal letter sent to for SRL property owners

Figure 8: Renewal letter sent to SRL properties starting August 2013

Notes: The gure displays the letter sent to SRL property owners 45 days before policy expiration to inform them about future rate increases.

Appendix B Additional information on home and policy characteristics

Table App.1: Control variables

Variable	Description
CRS Score	The community's score on the Community Rating System (CRS). The CRS is a voluntary program that rewards communities for taking actions to mitigate ood risk beyond minimum NFIP requirements. Community actions reduce policyholder premiums by up to 45%. The CRS score is the associated premium reduction, ranging from 0 (no mitigation) to 45 (maximum mitigation).
Elevation Certi cate	Home elevation is sometimes estimated by communities; however, homeowners can also contract an engineer or surveyor to evaluate their homes. This variable can take 12 values depending on who assessed the elevation and when.
Elevation Indicator	A building that has no basement and that has its lowest elevated oor raised above ground level by foundation walls, shear walls, posts, piers, pilings, or columns.
Floors	Number of oors in the home, taking four possible values: 1, 2, 3 or more, or split-level.
Home Age	Age of the home, in years.
Income (ZIP code)	Mean income on the ZIP code level.
Loading factor	Fraction of the home's insurance premium and its expected claims payments.
Mobile	Indicates whether the structure is a manufactured/mobile home.
Obstruction	Description for elevated buildings regarding the area and machinery attached to the building below the lowest oor. It takes 13 values, depending on the size of the area, whether it has permanent walls, and the presence/location of machinery (e.g., if it is elevated). We include dummy variables for these in our models.
Policy Age	Age of the insurance policy with respect to original new business year.

Notes NFIP (2014) provides additional information on these variables. Each of these variables occurs as control variable in our regression models in Section 3.2 and 3.3.

Appendix C Premium Calculation and Detailed Effects of Biggert-Waters on Premium Rates

In the NFIP, homeowners can separately choose building coverage $c^{(b)}$ and contents coverage $c^{(c)}$ up to a limit of \$250,000 for buildings and \$100,000 for contents. The premium p for NFIP's ood insurance policy depends on these insurance choices and is calculated as follows:

$$\text{Building:} \quad p^{(b)} = \min \quad c^{(b)}; 60000 \quad r_b^{(b)} + \max \quad c^{(b)} \quad 60000; 0 \quad r_a^{(b)} \tag{3}$$

$$\text{Contents:} \quad p^{(c)} = \min \quad c^{(c)}; 25000 \quad r_b^{(c)} + \max \quad c^{(c)} \quad 25000; 0 \quad r_a^{(c)} \tag{4}$$

$$\text{Total:} \quad p = \quad p^{(b)} + p^{(c)} \quad + icc \quad crs + f \tag{5}$$

In the above formula $r_b^{(b)}$ denotes the premium rate for building coverage limits up to \$60,000, called “basic building coverage”, and $r_a^{(b)}$ designates the premium rate for the additional coverage exceeding \$60,000. Similarly, $r_b^{(c)}$ denotes the premium rate for basic contents coverage until \$25,000 and $r_a^{(c)}$ for additional coverage exceeding \$25,000.

The sum of the premium for building $p^{(b)}$ and contents coverage $p^{(c)}$ is multiplied by a deductible factor . NFIP's rating manual provides these deductible factors. For example in January 2012, the minimum deductible option of \$1,000 had a $= 1{:}1$ while the maximum deductible option of \$5,000 had a $= 0{:}81$. The policy's premium further comprises a Community Rating System (crs) discount for the respective communities' engagement in mitigation measures, an administrative fee f, and a fee icc as a contribution to the increased cost of compliance program that helps policyholders bring their properties to current building standards after a loss occurrence. In 2010, the median crs discount, administrative fee f, and icc fee are 0.9, \$40, \$64 for the non-SRL properties and 1, \$40, \$64 for the SRL properties in the Restricted Sample. The difference between SRL and non-SRL properties in the median crs discount is due to differences in community af liations.

The Biggert-Waters Reform Act increased the premiums of the SRL property owners in the Restricted Sample. Speci cally, Biggert-Waters increased the building and contents coverage rates, $f\, r_b^{(b)}; r_a^{(b)}; r_b^{(c)}; r_a^{(c)} g$. Table App.2 shows the development of building premium rates, $r_b^{(b)}$ and $r_a^{(b)}$ over time for properties without a basement, which represents 75% of both SRL and non-SRL properties in the Restricted Sample. Table App.3 shows the corresponding data for properties with basements.

Previous to the implementation of Biggert-Waters, both types of properties had the same premium rates. For example in January 2013, all policyholders in the Restricted Sample with no basement paid $r_b^{(b)} = 0{:}0076$ and $r_a^{(b)} = 0{:}0077$ per dollar of coverage. Any increases in the premium rates, such as the one in October 2012, also applied to both types of properties. Following Biggert-

Table App.2: Development of building rates over time

Effective date	SRL properties		Non-SRL properties	
	$r_b^{(b)}$	$r_a^{(b)}$	$r_b^{(b)}$	$r_a^{(b)}$
2008-10-01	0.0076	0.0054	0.0076	0.0054
2009-10-01	0.0076	0.0057	0.0076	0.0057
2010-05-01	0.0076	0.0056	0.0076	0.0056
2010-10-01	0.0076	0.0060	0.0076	0.0060
2011-10-01	0.0076	0.0066	0.0076	0.0066
2012-10-01	0.0076	0.0077	0.0076	0.0077
Implementation of Biggert-Waters for SRL properties				
2013-10-01	0.0091	0.0092	0.0091	0.0077
2014-10-01	0.0091	0.0092	0.0085	0.0078
2015-04-01	0.0103	0.0105	0.0089	0.0081
2016-04-01	0.0129	0.0131	0.0094	0.0085
2017-04-01	0.0161	0.0164	0.0099	0.0090
2018-04-01	0.0201	0.0205	0.0104	0.0095

Notes The table displays the development of building rates in the Restricted Sample for properties without a basement. It shows the rate $r_b^{(b)}$ for basic building coverage until \$60,000 and the rate $r_a^{(b)}$ for additional building coverage above \$60,000 for both SRL and non-SRL properties between October 2008 and December 2018.

Waters, premium rates increased more strongly for SRL properties than for non-SRL properties. $r_b^{(b)}$ increased to $0{:}0091$and $r_a^{(b)}$ to $0{:}0092$for SRL properties versus $r_b^{(b)} = 0{:}0091$and $r_a^{(b)} = 0{:}0077$ for non-SRL properties. Thus, both SRL and non-SRL properties experienced a premium rate increase for the basic building coverage until \$60,000 but only SRL properties faced an increase for the additional building coverage above \$60,000. In subsequent years, due to Biggert-Waters, SRL properties experienced additional increases, approximately tripling their pre-reform rates, while rates for non-SRL properties changed only moderately, by comparison.

Table App.3: Building rates in dependence of different basement options

September 2013	SRL properties		Non-SRL properties	
	$r_b^{(b)}$	$r_a^{(b)}$	$r_b^{(b)}$	$r_a^{(b)}$
No Basement	0.0076	0.0077	0.0076	0.0077
With Basement	0.0081	0.0114	0.0081	0.0114
With Enclosure	0.0081	0.0137	0.0081	0.0137
Elevated on Crawlspace	0.0076	0.0077	0.0076	0.0077
Non-Elevated with Subgrade Crawlspace	0.0076	0.0077	0.0076	0.0077
Manufactured (Mobile) Home	0.0076	0.0077	0.0076	0.0077
October 2013	**SRL properties**		**Non-SRL properties**	
	$r_b^{(b)}$	$r_a^{(b)}$	$r_b^{(b)}$	$r_a^{(b)}$
No Basement	0.0091	0.0092	0.0091	0.0077
With Basement	0.0097	0.0136	0.0097	0.0114
With Enclosure	0.0097	0.0163	0.0097	0.0137
Elevated on Crawlspace	0.0091	0.0092	0.0091	0.0077
Non-Elevated with Subgrade Crawlspace	0.0091	0.0092	0.0091	0.0077
Manufactured (Mobile) Home	0.0091	0.0092	0.0091	0.0077

Notes The table displays the rate $r_b^{(b)}$ for basic building coverage until \$60,000 and the rate $r_a^{(b)}$ for additional building coverage above \$60,000 in the Restricted Sample for both SRL and non-SRL properties before (in September 2013) and after (in October 2013) the implementation of Biggert-Waters for SRL properties. These rates are dependent on the characteristics of the home's basement or crawlspace. 75% of both SRL and non-SRL properties in the Restricted Sample fall into the category "no basement". The building rates corresponding to this category are provided in Table App.2 for the time period between October 2008 and December 2018.

Appendix D Estimation of loading factors

Using claims-level, panel data between 2001 and 2012, we estimate loading factors for each property. In the following, we provide the details of our frequency-severity loss estimation model. For both building and contents damage we separately fit a generalized linear model for the frequency and severity of claims in dependence of the SRL indicator, and several control variables (CRS score, elevation certificate, elevation indicator, home age, mobile, obstruction (see Table App.1 in Appendix B for further details), elevation difference to 100-year flood event, and state characteristics on the distance to the coast, precipitation, and share of water).

We employ a Poisson GLM with logarithmic link function for the frequency of building damages ($losses_b$) and contents damages ($losses_c$):

$$E[losses_j] = \exp(\quad SRL + \quad controls) + \varepsilon; \qquad j \in \{b; c\} \tag{6}$$

For the severity of building damages ($size_b$) and contents damages ($size_c$) we use a Gamma

GLM with logarithmic link function:

$$E[size_j] = \exp(\quad SRL + \quad controls) + \varepsilon; \qquad j \in \{b; c\} \tag{7}$$

Given the estimates for the frequency and severity of building and using the commonly made independence assumption of claims frequency and severity, we calculate a property's expected loss as follows:

$$expected\_loss = E[losses_b] \quad E[size_b] + E[losses_c] \quad E[size_c] \tag{8}$$

We additionally obtain the policyholder's expected payment by subtracting the property's deductibles for building ($deductible_b$) and contents coverage ($deductible_c$):

$$expected\_payment = indicator_b \quad E[losses_b] \quad \max(E[size_b] \quad deductible_b; 0) \tag{9}$$

$$+ \; indicator_c \quad E[losses_c] \quad \max(E[size_c] \quad deductible_c; 0) \tag{10}$$

Finally, we get loading factors by dividing the policy's premium by the policyholder's expected payment:

$$loading\_factor = premium/expected\_payment \tag{11}$$

Appendix E Share of Properties Remaining in the Sample

We analyze whether the effect of the reform is persistent over time. For this, we estimate Equation (1), but balance the panel in an artificial way. After the coverage for a property is not renewed, we use the last observation we have for the property and add it for all remaining periods, with the dependent variable set to 0. The coefficient on the interaction of the SRL indicator and each fiscal year fixed effect then estimates the difference in the share of properties from the 2009 cohort who are still insured between SRL and non-SRL properties. A negative value indicates that a larger proportion of SRL properties cancelled their insurance coverage than this was the case for non-SRL properties. The results of the estimation are given in Table App.4. We can see that the effect of Biggert-Waters is persistent over time. Even though we do not see a difference in renewal rates between the two types of properties after fiscal year 2014, even in fiscal year 2018, relatively fewer SRL properties than non-SRL properties remain in the data. At its maximum in fiscal year 2014, the difference between the two shares is 13 percentage points, but even in 2018 it is still 6 percentage points.

Figure 9 provides a graphical impression of the shares of properties remaining in the data. We plot the results from column (2) in Table App.4. This specification has annual fixed effects which are not differentiated by ZIP-cope and thus allows for an easy comparison between the non-SRL properties (for which the annual fixed effect shows the conditional mean) and the SRL properties (for which the conditional mean is the sum of the annual fixed effect and the corresponding coefficient in column (2) of Table App.4). The confidence interval around the conditional mean can be used to infer whether the share of remaining SRL properties is statistically significantly different from that of remaining non-SRL properties. We can see that the stronger relative decline in insurance coverage for SRL properties in the fiscal years 2013 and 2014 leads to a persistent effect over time and that the gap in insurance coverage between the two types of properties continues to be statistically significant throughout the entire observation period.

Table App.4: Regression results: Linear model on arti cially balanced panel for the 2009 policy cohort between 2010 and 2018

	Dependent variable:		
	P(Renew)		
	(1)	(2)	(3)
I(t = 2010) x I(SRL)	0.01	0.002	0.004
	(0.01)	(0.01)	(0.01)
I(t = 2011) x I(SRL)	0.01	0.01	0.004
	(0.01)	(0.01)	(0.01)
I(t = 2013) x I(SRL)	0.09	0.09	0.10
	(0.01)	(0.01)	(0.01)
I(t = 2014) x I(SRL)	0.12	0.13	0.13
	(0.01)	(0.01)	(0.01)
I(t = 2015) x I(SRL)	0.09	0.09	0.10
	(0.01)	(0.01)	(0.01)
I(t = 2016) x I(SRL)	0.08	0.08	0.08
	(0.01)	(0.01)	(0.01)
I(t = 2017) x I(SRL)	0.07	0.07	0.07
	(0.01)	(0.01)	(0.01)
I(t = 2018) x I(SRL)	0.06	0.06	0.06
	(0.01)	(0.01)	(0.01)
I(SRL)	0.02	0.01	0.02
	(0.01)	(0.02)	(0.02)
Controls	No	Yes	Yes
ZIP FE	No	Yes	No
Year FE	Yes	Yes	No
ZIP x Year FE	No	No	Yes
Clustered SE	Policyholder	Policyholder	Policyholder
R-Sq	0.1904	0.2823	0.3014
R-Sq (Within)	0.0002	0.0423	0.0449
Observations	4,183,950	4,081,292	4,081,292

Notes Estimation of equation (1) with an arti cially balanced panel. Dependent variable is an indicator variable which equals one when the policy is active in the given year. Fiscal year 2012 is the reference category. Standard errors, heteroscedasticity-robust and clustered by policyholder, are in parentheses.

Figure 9: Share of the 2009 cohort's properties still insured, conditional on observables and relative to scal year 2012

Notes: Event study coef cients and 95% con dence intervals are plotted. The 2012-Q2 dummy is the reference category. Renewal rates are not statistically different for the nine quarters before the rati cation of the reform. The gure further shows that the reductions in renewal rates begin with the rst rate increase after the rati cation of the reform (2012-Q4) and persist until after the implementation of Biggert-Waters (2013-Q4). Starting 2013-Q3, renewal rates are no longer statistically different from pre-reform levels.

Appendix F Further Analyses of Coverage Choices

F.1 Annual Event Study Estimation

Results of estimating equation (1) with the coverage choice as the dependent variable are given in Table App.5. Speci cations are similar to the analyses of the renewal decisions. Column (1) shows results with only annual xed effects. Column (2) includes the full vector of control variables and ZIP level xed effects. Column (3), which again shows the result of our preferred speci cation, includes control variables and zip-code year xed effects. All three columns show standard errors, heteroscedasticity-robust and clustered on the level of the individual property, in parentheses.

As before, the results between the three different speci cations are relatively consistent. We see no signi cant differences in the coverage choices between SRL properties and non-SRL properties in the scal years following the passage and the implementation of Biggert-Waters. Coef cients of the fiscal years 2013 through 2016 are negative, but not statistically significant. This shows that while policyholders had a strong immediate reaction to the reform on the extensive margin, they did not have a comparable reaction on the intensive margin. The coefficients do become both more negative and statistically significant for the fiscal years 2017 and 2018. This is a sign that policyholders of SRL properties do react to the later premium rate increases by reducing coverage on the intensive margin.

F.2 Analysis of Selection Effects

The coverage choice estimation is potentially vulnerable to sample selection bias, because it compares the insurance choices of the full cohort in 2010 with those of partial cohorts in later years after some policyholders already cancelled their policies. We thus repeat the analysis reported in Figure 4 in a way that the sample remains the same for each analyzed quarter. Specifically, Figure 10 graphically shows the quarterly event study results for eight cohorts, starting at the cohort which renews in both the fiscal years 2010 and 2011 and ending at the cohort which renews in all fiscal years between 2010 and 2018. Results of these analyses generally support the findings from Table App.5 and Figure 4. While some selection effects exist, coefficients for the early years after the reform show no reduction in demand on the intensive margin. Similarly consistent with the analysis in the main paper, coefficient estimates for quarters in the fiscal years 2017 and 2018 are mostly negative and decreasing over time.

Table App.5: Regression results: Coverage choices of the 2009 cohort between 2010 and 2018

	Dependent variable:		
	Coverage Limit		
	(1)	(2)	(3)
I(t = 2010) x I(SRL)	0.04	0.04	0.04
	(0.03)	(0.03)	(0.04)
I(t = 2011) x I(SRL)	0.03	0.03	0.04
	(0.03)	(0.03)	(0.04)
I(t = 2013) x I(SRL)	0.01	0.01	0.003
	(0.01)	(0.01)	(0.01)
I(t = 2014) x I(SRL)	0.04	0.04	0.04
	(0.03)	(0.03)	(0.04)
I(t = 2015) x I(SRL)	0.04	0.03	0.04
	(0.04)	(0.04)	(0.04)
I(t = 2016) x I(SRL)	0.06	0.05	0.06
	(0.04)	(0.04)	(0.04)
I(t = 2017) x I(SRL)	0.11	0.11	0.14
	(0.04)	(0.04)	(0.05)
I(t = 2018) x I(SRL)	0.11	0.10	0.15
	(0.04)	(0.04)	(0.06)
I(SRL)	0.04	0.07	0.08
	(0.03)	(0.07)	(0.07)
Controls	No	Yes	Yes
ZIP FE	No	Yes	No
Year FE	Yes	Yes	No
ZIP x Year FE	No	No	Yes
Clustered SE	Policyholder	Policyholder	Policyholder
R-Sq	0.0034	0.029	0.0502
R-Sq (Within)	0	0.0003	0.0003
Observations	2,057,470	2,013,545	2,013,545

Notes Results of estimating Equation (1) with the coverage limit divided by the policyholder's coverage limit in 2009 as the dependent variable. Fiscal year 2012 is the reference category. The columns differ by the control variables and xed effects included in the estimation as indicated in the lower panel of the table. Standard errors, heteroscedasticity-robust and clustered by policyholder, are in parentheses.