Title

Adapting to a More Volatile Climate: Essays in Individual, Firm, and Government Response

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Publication Date

2024

Peer reviewed|Thesis/dissertation

Adapting to a More Volatile Climate: Essays in Individual, Firm, and Government Response

By

MADELINE TURLAND
DISSERTATION

Submitted in partial satisfaction of the requirements for the degree of

DOCTOR OF PHILOSOPHY

in

Agricultural and Resource Economics

in the

OFFICE OF GRADUATE STUDIES

of the

UNIVERSITY OF CALIFORNIA

DAVIS

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2024

Abstract

Climate change is significantly amplifying the frequency and intensity of natural disasters, transforming them from sporadic events into persistent crises. Extreme heat, hurricanes, tornadoes, wildfires, floods, droughts, and other disasters are not only more common but also more devastating, leading to destruction of ecosystems, human casualties, and substantial economic losses. By analyzing past responses and predicting future trends, economists can help policymakers develop strategies that not only mitigate the financial toll of natural disasters but also enhance the resilience of vulnerable populations. This dissertation explores the intricate dynamics between natural disasters, insurance markets, and climate change, focusing on the state of California. It is structured into three chapters, each examining a distinct aspect of how risk and regulatory responses shape economic behavior and market outcomes in the context of increasing environmental volatility.

Chapter 1 investigates the influence of natural disaster risk on migration decisions and demographic sorting based on income and risk preferences, focusing on individual responses to a changing climate. One of the most effective ways to reduce individual exposure to natural disaster risk is to migrate away from risky areas. However, as people migrate away from risk, an opportunity opens for other people to migrate towards it. I study who chooses to move in both directions in rural California, which has experienced a rapid escalation of wildfire risk in recent years. Utilizing a novel measure of wildfire risk derived from California's residual homeowners insurance market, I distinguish the impacts of wildfire risk from other unobserved variables. The analysis reveals that increased wildfire risk prompts a population reshuffling, with lower-income and less risk-averse individuals more likely to migrate into high-risk areas. These findings have significant policy implications, highlighting the challenges of disaster preparedness and recovery among vulnerable populations.

Chapter 2 examines the consequences of regulatory interventions for firms in the disaster insurance market amid escalating climate risks. In California, increasing risk of wildfire is fueling an insurance crisis; in May 2023, the largest and fourth largest insurance firms (State Farm and Allstate) suspended all new underwriting activity related to homes and businesses citing increasing

wildfire risk. In response, the government implemented a moratorium on insurance non-renewals in zip codes that experience an emergency wildfire for one year following the emergency declaration in an attempt to protect customers and reduce reliance on the insurer of last resort. I present a theoretical model of this adversely selected market with an insurer of last resort and empirically evaluate the non-renewal moratoriums. Through a difference-in-differences methodology, I find that while the moratoriums temporarily reduced insurer-initiated non-renewals, their effects were short-lived, and there was no significant impact on participation in the state's insurer of last resort. These results highlight the limitations of regulatory measures in maintaining market stability under severe climate pressures.

Chapter 3 explores the role of government in facilitating climate change adaptation in California's groundwater and surface water markets. Climate models project that in California, droughts and floods will become more frequent and severe and year to year variability in precipitation will increase. In places where water rights have been established, water markets play a critical role in allocating water over space and can smooth climate risk by redistributing water to users that value it the most. While efforts have been made to improve water trading across space, market design has paid relatively less attention to the question of *when* scarce water resources should be allocated. This chapter examines the effect of water storage constraints on price dynamics in California's surface and groundwater markets, using transactions level water transfer data from 2010 to 2022. I examine recent market activity, including trading volumes and locations, and analyze the spatial and temporal variability in water prices. I find that surface water markets exhibit significant price fluctuations tied to precipitation changes due to limited storage capacity, whereas groundwater markets maintain stable prices, unaffected by such variability. These findings highlight the potential for conjunctive management of surface and groundwater to stabilize water prices and enhance economic welfare by leveraging California's substantial groundwater storage capacity. This study emphasizes the importance of integrated water management strategies to mitigate the economic impacts of climate change on water resources.

Collectively, this dissertation provides critical insights into the economic and policy challenges posed by natural disasters and climate change, emphasizing the need for adaptive strategies to manage risk and support vulnerable populations.

Acknowledgements

To my advisor Katrina, thank you for your unwavering support, encouragement, and guidance. You showed me how to navigate the complex world of academics with your “take no shit” attitude and set an example to aspire to. I am also grateful to my other committee members, Colin, Jens, and Pierre, for your insightful feedback, constructive criticism, and dedication to excellence.

To my classmates who have accompanied me on this academic journey, thank you for your emotional support, collaborative spirit, and for infusing joy into this PhD experience. I look forward to many years of continued friendship.

To my family, thank you for teaching me what hardwork, perseverance, and love look like.

I extend my heartfelt appreciation to Laurie Warren, Arnon Erba, Cathy Harrison, and Tiffany Denman for your help, always with a smile. I am indebted to UC Davis for providing me with the resources, facilities, and opportunities essential for conducting this research.

Finally, I am thankful to Devin for undertaking this adventure with me. Your dedication and work ethic is something I aspire to. Your unwavering love, encouragement, and understanding has sustained me through the challenges of this doctoral study, and I know will continue to do so for years to come. I love you.

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1 Running from Wildfires: the Role of Risk Preferences in Natural Disaster Sorting

Abstract

One of the most effective ways to reduce the risk of experiencing a natural disaster is also one of the most obvious: relocate to an area where natural disasters are less likely to occur. But, as more people make the decision to relocate out of risky areas, an opportunity opens for others to migrate in. This chapter examines the impacts of wildfire risk on the decision to migrate and tests for sorting on incomes and risk preferences. I develop a new measure of wildfire risk by exploiting the existence of a residual market for homeowners insurance in California, and construct an exposure instrument to distinguish the impacts of wildfire risk from unobserved variables. I test for changing risk preferences by examining risk reduction behaviors for risks that remain constant when wildfire risk changes: automobile liability insurance purchases. Results suggest that an increase in wildfire risk is associated with a mild reshuffling of the population where lower income and less risk averse people disproportionately migrate into risky areas. These results have important implications for policy design; less risk averse people are less likely to follow evacuation orders, and lower income people have fewer resources to recover following a disaster.

1.1 Introduction

Climate change is increasing the frequency and severity of natural disasters, consequently resulting in mounting losses incurred from such events (Intergovernmental Panel on Climate Change, 2022; Williams et al., 2019). One of the most effective ways to reduce exposure to natural disaster risk, and thus reduce the costs of climate change, is to migrate to an area where natural disasters are less likely to occur. However, as more people make the decision to relocate out of risky areas, an opportunity opens for others to migrate in. Those who subject themselves to elevated risk levels will bear a disproportionately larger share of the costs of climate change, experiencing more frequent and sizable losses as a result. Understanding who these people are sheds light on social and economic disparities, encourages proactive measures to mitigate climate risks, and allows policymakers and communities to allocate resources more effectively. Existing literature highlights sorting on incomes, race, ethnicity, and other socio-demographic indicators in response to floods, hurricanes, and extreme temperatures (Sheldon and Zhan, 2022; Fan et al., 2016), as well as in response to changes in risk levels for these events (Bakkensen and Ma, 2020; Fan and Bakkensen, 2022). Still unknown is the migratory response in rural and agricultural communities, which tend to be less exposed to flooding and hurricanes than their more densely populated coastal counterparts, and the extent to which risk preferences impact this response.

This chapter evaluates sorting on risk preferences and incomes in response to changes in wildfire risk in California. Wildfires are the fastest growing economic climate risk, with more than 150 billion USD in damages predicted in the United States for 2020-2029 – almost triple the amount from 2010-2019 (NOAA, 2020; FSF, 2021; Kearns et al., 2022). A major contributing factor to this trend is people exposing themselves to higher wildfire risk by choosing to live in wildfire risky areas.[1] Unlike flooding and hurricane risk, rural and agricultural communities are disproportionately impacted by escalating wildfire risk because of their proximity to wildland areas. In California, the 8 largest, 12 of the 16 most destructive, and the single deadliest wildfire in recorded history have happened since 2017 (CalFire, 2022).

I begin by constructing a simple conceptual framework in which individual utility is decreasing in wildfire risk and increasing and concave in income. I assume that house prices are negatively related to wildfire risk,[2] providing the mechanism for two intuitive predictions: an increase in wildfire risk leads to sorting on incomes and risk preferences, with lower income and less risk averse

[1]The wildland urban interface (WUI) is the area where houses and wildland vegetation meet or intermingle, and where wildfire problems are most pronounced. It is also the fastest growing land-use type in the United States. From 1990-2010, the WUI grew by 41% in the number of houses and by 33% in land area (Radeloff et al., 2018). Burke et al. (2021) estimate that nearly 50 million homes are currently in the WUI, and the number is increasing by 1 million every 3 years.

[2]It is well documented that house values are negatively related to natural disaster risk, including the risk of flooding, earthquakes, and wildfires (Bakkensen and Barrage, 2022; Koo and Liang, 2022).

people choosing to expose themselves to increased risk. While these theoretical predictions are straightforward, bringing credible empirical evidence to test them is not. The challenges are three-fold: first, finding a granular measure of wildfire risk that varies spatially and temporally, second, measuring risk preferences which are inherently unobservable, and third, accounting for potential omitted variable bias.

I construct a wildfire risk panel dataset for California using observed behavior of insurance companies. Insurers have an incentive to accurately estimate risk levels in order to remain competitive and solvent, and to keep this information private.[3] Risk estimates should theoretically be revealed in premiums paid, but strict regulation in California prevents insurers from charging rates that fully reflect their expectations of risk. However, insurers can select which customers they offer policies to, and simply refuse to insure anyone whose risk level exceeds the threshold needed to remain profitable. Risky customers that can't secure a private insurance policy must purchase from California's insurer of last resort, the California Fair Access to Insurance Requirement (FAIR) Plan. The FAIR Plan is mandated to provide basic fire insurance to people that are not able to find coverage on the traditional market because their risk level is too high. The size of the FAIR Plan in any local market represents the wedge between insurers' wildfire risk estimates and the regulated price. Because price regulation generally remains constant, FAIR Plan market share will reflect the aggregate risk estimates of the private insurance market in each zip code and year, and therefore I use it as my measure of wildfire risk.

The second challenge is measuring risk preferences, which are inherently unobservable. I measure risk preferences by looking at changes in voluntary, mitigating behavior for risks unrelated to wildfires: observed automobile liability insurance purchases. Because driving risk is independent of wildfire risk, if a change in wildfire risk induces a change in car insurance purchasing behavior and capacity to mitigate risks remains the same, then underlying risk preferences must have changed. This method of measuring risk preferences relies on risk preferences begin stable over time and consistent for similar types of risks, which economic research generally supports

[3] Risk estimates that are too low will result in premiums that are not sufficient to cover damages and risk estimates that are too high will lead the company to lose market share to competitors that can more accurately estimate risks.

(Soane and Chmiel, 2005; Einav et al., 2012).

The third challenge is eliminating the possibility that an omitted variable is causing bias in the empirical estimation. The main endogeneity concern is that local differences in the propensity to undertake private mitigation behaviors, such as clearing defensible space, could be correlated with FAIR Plan market share *and* are likely correlated with incomes and risk preferences. Insurance companies could be more likely to offer policies to homes that are better protected thus reducing FAIR Plan market share, and at the same time, wealthier and more risk averse individuals are more likely to protect their homes. I instrument for wildfire risk with an exposure instrument that draws from the shift-share literature (Borusyak et al., 2022; Goldsmith-Pinkham et al., 2020). I interact an exogenous cross-sectional measure of wildfire risk with aggregate, annual changes in wildfire risk. The idea is that areas with higher baseline wildfire risk are more likely to experience larger effects from an aggregate change in wildfire risk. Because baseline wildfire risk is unrelated to private risk mitigation behavior and local variation in risk mitigation behavior is purged by aggregation, this instrument isolates the portion of FAIR Plan market share in each zip code that is driven by wildfire risk. In my estimation I additionally include zip code and county-by-year fixed effects to control for a wide range of factors that vary spatially and/or temporally such as amenity values and rules surrounding insurance pricing.

I find that a one standard deviation increase in wildfire risk is associated with an overall population decline of 4% and a 20% increase in the number of in-migrants. Further, I can distinguish these migrants by income group; I find that an increase in wildfire risk increases low-income in-migration and decreases high-income in-migration. These results suggest an increase in wildfire risk causes a reshuffling of the population, with more people migrating out of a risky area than migrating in, and lower income people moving towards wildfire risk. These results are consistent with findings from prior work, including Bakkensen and Ma (2020) who find that low income people are more likely to migrate into areas with high flood risk.

I also find that increases in wildfire risk are associated with a shift towards a less risk averse population. A one standard deviation increase in wildfire risk corresponds to a 21% increase in the proportion of car insurance policies that exceed basic requirements, and the size of this effect is not impacted by the inclusion of income controls. This is consistent with Bakkensen and Barrage (2022) who establish that individuals sort on risk preferences in response to flood risk, but builds on it by using observational rather than survey data.

Finally, I provide preliminary evidence that sorting on incomes and risk preferences are caused by changes in housing costs. Because people are generally risk averse, there must be something other than wildfire risk that causes lower income and less risk averse individuals to migrate towards risk. I expect that lower housing costs act as this draw. I empirically test if increases in wildfire risk are associated with decreases in housing costs, and find that a one standard deviation increase in wildfire risk reduces typical housing values by $13.5 thousand, or 4.5% on average.

This chapter has three contributions. First, I study sorting on incomes and risk preferences in rural and agricultural communities in response to wildfire risk. An emerging literature studies sorting on natural disasters and natural disaster risk (Bakkensen and Barrage, 2022; Bakkensen and Ma, 2020; Fan and Bakkensen, 2022; Sheldon and Zhan, 2022; Fan et al., 2016), but these studies tend to focus on the response to floods and hurricanes, which disproportionately impact coastal communities. Wildfires and their impacts on rural communities remain understudied, even as associated damages grow more quickly than damages from other natural disasters (NOAA, 2020; FSF, 2021; Kearns et al., 2022). This literature also tends to ignore the role of risk preferences in the decision to migrate, with the exception of Bakkensen and Barrage (2022) who use survey data that ask hypothetical questions to elicit risk preferences. Understanding who exposes themselves to natural disaster risk is critical to understand recovery capacity and for achieving environmental justice goals. More broadly, this chapter contributes to the large environmental migration literature that studies human migration in response to environmental dis-amenities such as extreme temperature, precipitation volatility, and air pollution (Mueller et al., 2014; Bohra-Mishra et al., 2014; Gao et al., 2023; Bayer et al., 2009).

Second, I develop a new method to measure changes in risk preferences that does not depend on experimental or survey data. Risk preferences are often ignored because they are unobservable, but they are an important input into the decision making process, and heterogeneity in these preferences can contribute to observing a wide range of mitigating behavior. Economists traditionally measure risk preferences with experiments, surveys, or detailed insurance data where individuals choose between a set of lotteries (Barseghyan et al., 2018; Bakkensen and Barrage, 2022; Andreoni and Sprenger, 2012). Obtaining these types of data is onerous, and therefore these studies can only be carried out in limited settings. Much more accessible is aggregate data, which has been used to estimate risk preferences in limited settings including horse racing (Chiappori et al., 2010; Gandhi and Serrano-Padial, 2015). I make a few standard assumptions about household utility which allows me to measure changes in risk preferences using aggregate, observational data on household insurance purchases.

Finally, this work fits into a broader literature on the economic costs of climate change and climate adaptation (Smith et al., 2006; Gandhi et al., 2022; Kousky, 2014; Barreca et al., 2016; Diaz and Moore, 2017; Kousky, 2019; Botzen et al., 2019; Kahn, 2021; Sastry, 2021). Wildfires have been understudied in this literature, and are different from other disasters because private mitigation behavior and public fire fighting effort can dramatically impact damages. I contribute to a better understanding of the distributional costs of climate change; numerous studies document how disadvantaged communities are exposed to more pollution and bear a disproportionate share of the costs associated with climate change (Banzhaf et al., 2019; Hajat et al., 2015; Mendelsohn and Dinar, 2009). I also contribute to measuring the costs of climate change though the hedonic method.

This chapter proceeds as follows: section 2 provides relevant background on wildfire risk and the California insurance market, section 3 outlines the conceptual framework, section 4 describes the data, section 5 puts forth the empirical strategy, the results are in section 6, and section 7 concludes.

1.2 Institutional Background

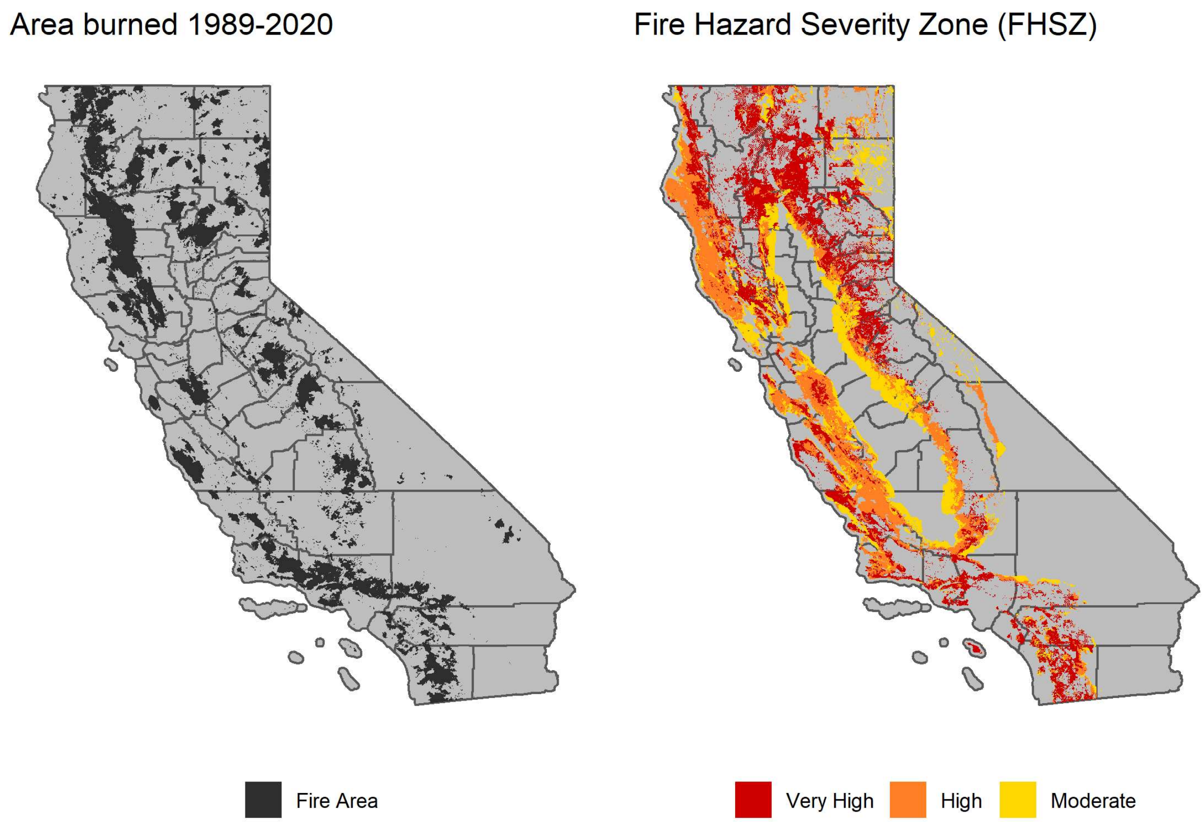
1.2.1 Wildfire risk in California

In California, a transition to a more arid climate combined with decades of fire suppression policy is causing more frequent and larger wildfires (Schweizer. et al., 2019). These impacts are heavily felt in rural areas: from 2000 to 2020 the burned area was over three times greater for rural compared to urban regions (Masri et al., 2021). In addition, development in high fire risk areas puts more structures at risk, making these fires more devastating. From 2005 to 2020, wildfires destroyed 89,210 structures, with 2017, 2018, and 2020 accounting for 62% of those losses (Barrett, 2020). The 8 largest, 12 of the 16 most destructive, and the deadliest wildfire in California recorded history happened since 2017 (CalFire, 2022). Moving forward, wildfire risk in California is expected continue increasing.

Structures at highest risk for wildfire damage are located in fire hazard severity zones (FHSZ), which were created by the Government of California and represent conditions as of 2007-2011.[4] Figure 1 shows the total area burned from 1989 to 2022 and FHSZ designations. Wildfire risk is concentrated in mountainous or hilly regions, and most fires burn in areas of low population density. The area burned appears to track the FHSZ map well with some discrepancies, likely caused by spatial variation in fire-fighting effort. In this chapter, I restrict my estimation sample to zip codes that are at least 25% contained within a FHSZ.

[4]A preliminary update to the FHSZ map was released in December 2022, but has not yet been formally adopted. Classification of a FHSZ is based on a combination of how a fire will behave and the probability of flames and embers threatening buildings. Each area gets a score for flame length, embers, and the likelihood of the area burning. The elements that determine the FHSZ designation are vegetation (fire hazard considers the potential vegetation over a 30-50 year time horizon), topography (fire typically burns more quickly and intensely up steep slopes), climate (fire moves faster and is more intense under hot, dry, and windy conditions), crown fire potential (under extreme conditions, fires burn to the top of trees and tall brush), ember production and movement (burning embers, known as firebrands, spread fire ahead of the flame front and can ignite buildings up to a mile away from the main fire), and fire history (past fire occurrence in an area over several decades).

Figure 1: California Wildfires and Fire Hazard Severity Zones



Note: Wildfire boundaries include all timber fires 10 acres or greater, brush fires 30 acres or greater, and grass fires 300 acres or greater. The FHSZ map was created from 2007 and reflects wildfire risk at that time.

Source: Constructed using data from CAL FIRE.

To reduce exposure to wildfire risk, individuals can purchase insurance or engage in home hardening activities (Meldrum et al., 2019; Brenkert-Smith et al., 2012). The goal of home hardening is to reduce the chance of damage during a wildfire and can include clearing defensible space, building with ignition and fire resistant materials, and covering vent openings. Constructing a new home to optimum wildfire resistance can increase costs by $18,200-$27,100 compared to constructing a new home that just meets current building regulations (Barrett et al., 2022). Home hardening can be so effective at reducing wildfire risk that it can impact whether or not insurers choose to offer coverage to a property. In California, a new regulation that forces insurers to provide discounts to homeowners that engage in home hardening activities was passed in 2022 (CDI, 2022).

1.2.2 Regulation of Homeowners' Insurance

Insurance is an important and widely used tool to mitigate potential financial damages from a wide range of risks. General homeowner policies usually cover losses from theft and vandalism, storms (e.g., hail damage), and wildfires and smoke. Most mortgage lenders require homeowners to purchase insurance, which contributes to a high uptake of homeowners insurance.[5] Losses from other natural disasters, such as floods and earthquakes, are usually not included in a general homeowners policy.

Most jurisdictions require that insurance rates be approved by a regulator before they can be implemented. In general, insurers justify rates using specific attributes of risk that predict loss, including catastrophe modeling. Catastrophe modeling allows insurers to evaluate and manage catastrophe risk from perils ranging from earthquakes and hurricanes to floods and wildfires, and is the most accurate, stable, and flexible way to predict expected losses. However, in California, the use of catastrophe modeling to justify rates is prohibited.

Instead of catastrophe modelling, insurers must use at least the last 20 years of observed loss history to justify rate changes. This is especially problematic for risks that may change quickly such as wildfire risk, and has resulted in many situations where the regulated price lies below the actuarially fair premium. From 2003-2022 (the past 20 years) on average approximately 1 million acres per year burned, but from 2017-2022 (the past 5 years) on average approximately 1.8 million acres per year burned. This highlights how a 20-year average loss history does not accurately measure expectations about current losses. Recent large losses and strict price regulation cast doubt on the continued ability of insurance companies to absorb fire-related losses (Issler et al., 2020).[6]

Although insurers cannot use catastrophe modeling to justify rates, they can use it to select

[5] According to the National Association of Insurance Commissioners, about 90% of homeowners have insurance.
[6] Insurers lost almost $25 billion from the 2017 and 2018 wildfire seasons.

which risks they want to insure. For example, if wildfire risk for a customer increases, an insurer may not be allowed to increase the premium charged, but they will be allowed to drop the policy. In 2019, insurers in California dropped 235,274 homeowner policies, a 61% increase from 2018 (California Department of Insurance, 2021), with most dropped policies coming from areas of moderate to high fire risk (Bikales, 2020). This strategy can allow an insurer to remain profitable under changing wildfire risks and restrictive price regulation, but leaves homeowners with fewer insurance providers to purchase from. If a homeowner cannot find insurance on the traditional market because they are deemed too risky, they can turn to the insurer of last resort in California, the Fair Access to Insurance Requirement (FAIR) Plan.

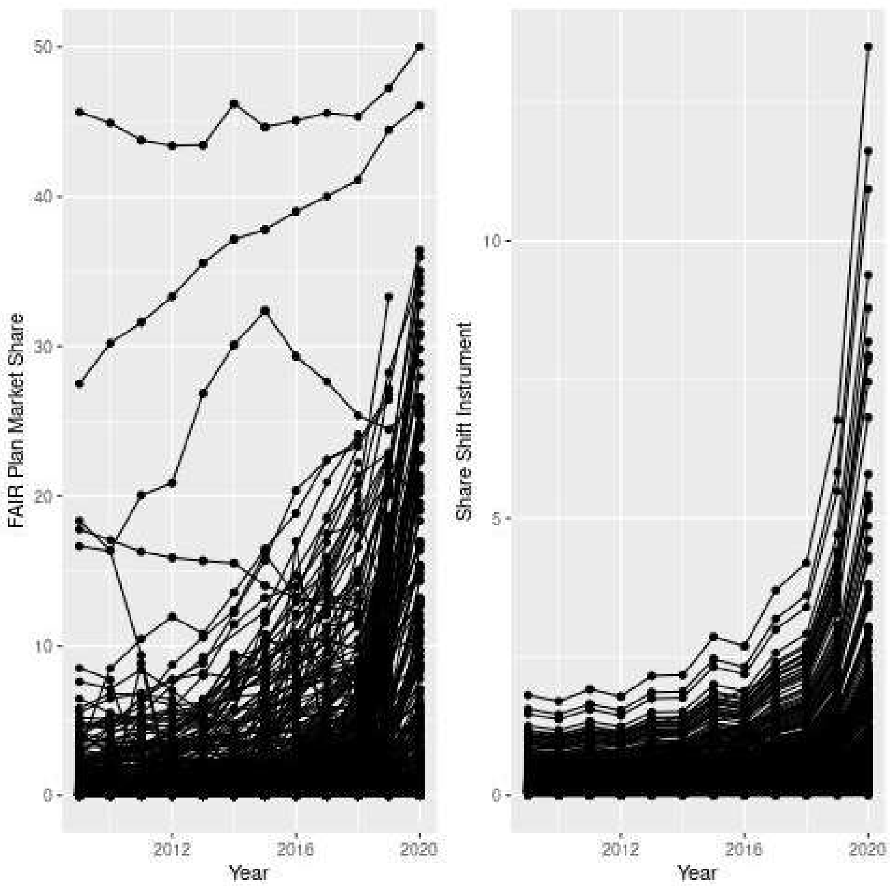
1.2.3 The California FAIR Plan

The California FAIR Plan was established as the insurer of last resort in August 1968 following the riots and fires of the 1960s. Its purpose is to provide temporary, basic fire insurance when traditional insurance is not available. FAIR Plan insurance is generally more expensive and provides less coverage than traditional insurance; it only provides coverage for wildfire, internal explosion, and smoke, and there is a maximum coverage limit that is binding for many homeowners.[7] The FAIR Plan is mandated to operate at zero economic profits, and receives no government funding.

Californians have had to increasingly rely on FAIR Plan coverage in wildfire risky areas as wildfire risk has increased and insurers are unwilling to cover them. Figure 2 shows how FAIR Plan market share has evolved over time in each zip code in my estimation sample. The left panel shows the observed FAIR Plan market share and the right panel shows the constructed exposure instrument used in the estimation. In most zip codes, FAIR Plan market share increased from 2009-2020, with the largest increases coming in 2019 and 2020.

[7] Anecdotally there are reports of people paying 2-3 times as much for FAIR Plan insurance than they were paying for traditional insurance.

Figure 2: FAIR Plan Market Share by Zip Code



Description: This chart shows FAIR Plan market share for every zip code that is in the estimation sample (at least 25% contained in a FHSZ by land area). The left panel shows the observed FAIR Plan market share and the right panel shows the constructed exposure instrument used in the estimation.

1.3 Conceptual Framework

In this section, I develop a simple theoretical model of how changes in wildfire risk impact household utility that results in two testable hypotheses. Households maximize utility by choosing between locations that experience differential shocks in wildfire risk. For simplicity, consider two locations in which all amenities other than wildfire risk remain constant over time; $l = 0$ has low and constant wildfire risk while $l = 1$ experiences positive shocks to wildfire risk. Assume that all households are risk averse or risk neutral, and that the utility of household i choosing location l in year t is,

$$U_{ilt} = V^*_{ilt}(X_i, r_{lt}, Y_{ilt}) + \varepsilon_{ilt}, \tag{1}$$

where, $V^*_{ilt}(X_i, r_{lt}, Y_{ilt})$ is the highest utility household i can achieve with choice l in year t and depends on household characteristics (X_i), wildfire risk level (r_{lt}), and disposable income (Y_{ilt}), and ε_{ilt} is an independently and identically distributed error term. I assume V^*_{ilt} is increasing and concave in disposable income (Y_{ilt}), and is decreasing in wildfire risk (r_{lt}), as show in equation 2,

$$\frac{\partial V^*_{ilt}}{\partial Y_{ilt}} > 0, \qquad \frac{\partial V^{*\,2}_{ilt}}{\partial^2 Y_{ilt}} < 0, \qquad \frac{\partial V^*_{ilt}}{\partial r_{lt}} < 0. \tag{2}$$

I model disposable income as a function of total household income (I_i) and housing costs in location l and year t $(h_{lt}(r_{lt}))$,

$$Y_{ilt} = I_i - h_{lt}(r_{lt}). \tag{3}$$

Housing is a composite good made up of housing and location characteristics, including risk levels for potential disasters. Therefore, housing costs in location l reflect the local residents' willingness to pay to live there. Increases in risk will reduce the expected value of living in location l, and therefore all households will experience a decrease in utility following an increase in risk. I expect this decrease in utility to be reflected in housing costs, and that housing costs in location l and year t will fall if wildfire risk increases in location l and year t,

$$\frac{\partial h_{lt}}{\partial r_{lt}} < 0 \tag{4}$$

The size of the relationship in equation 4 will depend on general equilibrium effects, but can be treated as exogenous to the individual household.

Equation 5 shows how utility for household i in location l and year t changes when wildfire risk changes. I decompose the change into the amenity effect, which directly measures the change in utility from an increase in risk, and the income effect, which measures the change in utility arising from a change in disposable incomes resulting from a change in housing costs. These effects work in opposite directions; an increase in wildfire risk causes a negative amenity effect and a positive income effect (through the channel of reduced housing costs).

$$\frac{\partial U_{ilt}}{\partial r_{lt}} = \underbrace{\frac{\partial V^*_{ilt}}{\partial r_{lt}}}_{\text{amenity effect} < 0} + \underbrace{\frac{\partial V^*_{ilt}}{\partial Y_{ilt}} * \frac{\partial Y_{ilt}}{\partial h_{lt}} * \frac{\partial h_{lt}}{\partial r_{lt}}}_{\text{income effect} > 0} \tag{5}$$

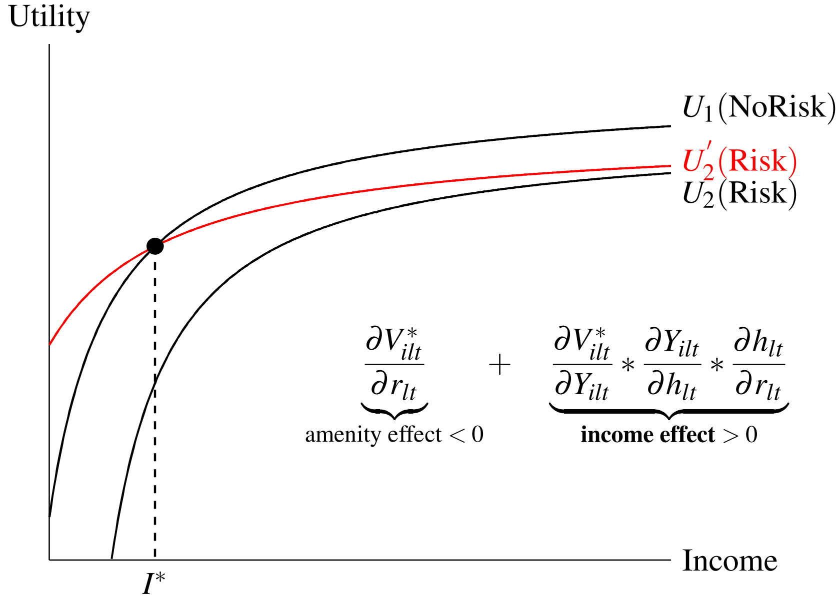
Because the income and amenity effects work in opposite directions, heterogeneity in the size of the effects will impact the migration decisions of households.

Income heterogeneity

I assume that the amenity effect is independent of income levels. That is, household income is not associated with the size of the amenity effect. Therefore, income heterogeneity will only impact the income effect. Because utility is concave in incomes, higher income households will experience a smaller utility gain from the same increase in income than lower income households. Therefore, the income effect will be greater for lower income households. This yields the first hypothesis, illustrated by Figure 3.

> **Hypothesis 1:** Lower income households are less likely to migrate away from and more likely to migrate towards areas that experience increases in wildfire risk.

Figure 3: Income Effect



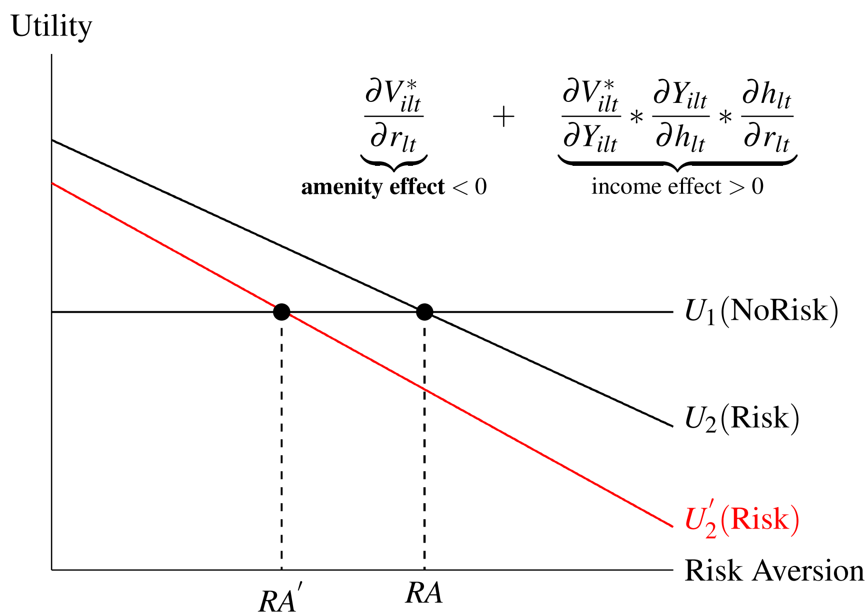
Description: U_1 is the utility function for a representative individual in a location with no risk and U_2 is the utility for an individual in a location with positive wildfire risk. Both curves are concave representing diminishing returns to income and U_2 is less than U_1 at every point because of risk aversion. Assume the risky location experiences a shock that increases the risk level faced, shifting utility to U_2'. Utility in the risk location increases because housing costs decrease and therefore disposable income increases. The increase in utility is largest for the lowest income individuals reflecting the higher marginal value of money for these individuals. Before the increase in wildfire risk, at all incomes this individual will choose to live in the location with no wildfire risk. After the risk change, if income is below I^* this individual will choose to live in the risky location. As risk increases, I expect the people choosing to live in a risky area to be lower income.

Risk Preference Heterogeneity

Risk preferences measure how much a household cares about risk. In this model they are captured by the amenity effect. I restrict the amenity effect to be negative and allow it to vary by household, but assume the distribution is independent of income. Households that are more risk averse will have a larger amenity effect (more negative) than households that are relatively less risk averse, holding income constant. Therefore, more risk averse households will experience a larger drop in utility from an increase in wildfire risk. This yields hypothesis 2, illustrated by Figure 4.

Hypothesis 2: Less risk averse households are less likely to migrate away from and more likely to migrate towards areas that experience increases in wildfire risk.

Figure 4: Amenity Effect



Description: U_1 is the utility function for a representative individual in a location with no risk. Because there is no risk, the level of risk aversion does not impact utility. U_2 is the utility for a representative individual in a location with positive wildfire risk. Because I assume individuals are risk averse, this curve is downward sloping. Assume the risky location experiences a shock that increases the risk level faced, shifting utility to $U_2^{'}$. Before the increase in wildfire risk, People with risk aversion below RA will choose to live in the risky area and others will choose to live in the safe area. After the change in wildfire risk, RA moves to $RA^{'}$ and the most risk averse people living in the risky area choose to migrate to the safe area, decreasing the average level of risk aversion in the risky location. Increases in risk should cause the population living in the risky area to be less risk averse.

In the empirical portion of this chapter I test hypotheses 1 and 2, and show evidence that housing costs fall in response to an increase in wildfire risk.

1.4 Data

The primary data comprise annual zip code migration and population, income, car liability insurance purchases, FAIR Plan market share, wildfire risk, and home values spanning 2009-2020.

I include zip codes that are at least 25% contained within a FHSZ because these are the regions where wildfire problems are most relevant. Summary statistics are shown in Table 1.

Table 1: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
FP Mktshr	11,044	1.5	4.0	0.0	0.02	0.3	1.1	50.0
Population	7,506	12,714.9	17,930.7	0	700	3,311	18,953	99,293
Movers	7,466	1,696.3	2,664.2	0.0	60.7	420.6	2,469.9	26,854.8
Local Movers	7,466	1,014.5	1,684.5	0.0	25.6	215.5	1,297.3	15,181.3
Mov<10	7,112	254.7	672.9	0.0	8.1	64.0	305.3	12,204.9
10<Mov<15	7,068	123.8	217.9	0.0	0.0	36.0	156.3	2,853.5
15<Mov<25	7,160	167.0	280.9	0.0	3.0	44.5	214.1	3,268.8
25<Mov<35	7,098	134.1	213.6	0.0	0.0	35.2	175.3	2,155.6
35<Mov<50	7,068	136.0	204.2	0.0	0.0	37.9	194.0	1,251.3
50<Mov<65	6,896	105.4	159.4	0.0	0.0	26.0	153.6	1,126.6
65<Mov<75	6,412	50.1	77.6	0.0	0.0	13.0	69.9	556.0
75<Mov	7,096	220.3	382.4	0.0	0.0	40.1	283.9	4,015.4
Proportion BL	11,042	11.7	5.3	0.0	8.2	10.9	14.7	43.9
House Value	4,234	301,676.4	248,199.7	29,253.8	150,834.4	227,545.4	361,099.6	2,342,286.0

FP Mktshr is FAIR Plan market share from the California Department of Insurance (CDI).
Population is the 5-year population estimate from the American Community Survey (ACS).
Movers is the 5-year estimate for total in-migration from the ACS.
Movers (county) is the 5-year estimate for in-migration originating from the same county from the ACS.
Mov<10 is the 5-year estimate for in-migration of people with less than $10,000.
10<Mov<15 is the 5-year estimate for in-migration of people with incomes between, $10,000 and $15,000.
15<Mov<25 is the 5-year estimate for in-migration of people with incomes between $15,000 and $25,000.
25<Mov<35 is the 5-year estimate for in-migration of people with incomes between $25,000 and $35,000.
35<Mov<50 is the 5-year estimate for in-migration of people with incomes between $35,000 and $50,000.
50<Mov<65 is the 5-year estimate for in-migration of people with incomes between $50,000 and $65,000.
65<Mov<75 is the 5-year estimate for in-migration of people with incomes between $65,000 and $75,000.
75<Mov is the 5-year estimate for in-migration of people with incomes greater than $75,000.
Proportion BL is the proportion of automobile insurance policies that are basic limits, from the CDI.
House Value comes from the Zillow Home Value Index (ZHVI) and reflects the typical value for homes in the 35th to 65th percentiles for a calendar year.

Migration and population data come from the American Community Survey (ACS). Zip code-year level data points are five year estimates; they encompass all survey responses for five years including and following the year indicated.[8] I use the number of movers, local movers, and movers in different income groups to measure migration.[9] This data covers 2011 to 2021, with some zip

[8]For example, the number of movers in 2009 represents everyone who responded to the survey from 2009-2013. One-year estimates that encompass survey responses for the year indicated are available at the county level. This does not provide the geographic granularity needed for my analysis.

[9]A mover is someone who changed addresses less than one year before they answered the survey and a local mover is someone whose previous address was in the same county as their current address. Movers are grouped into 8 income groups; income < \$10 000, \$10 000 < income < \$15 000, \$15 000 < income < \$25 000, \$25 000 < income < \$35 000, \$35 000 < income < \$50 000, \$50 000 < income < \$65 000, \$65 000 < income < \$75 000, and \$75 000

code-years missing due to confidentiality. On average, there are 12,714 people in a zip code and 13% of the population moves each year. 60% of movers are local and 68% of have income less than $50,000 per year.

Car liability insurance data come from the Survey on Auto Liability (SAL) from the CDI, spanning 2008 to 2021. I construct the proportion of policies that are 'basic limits' to measure risk preferences. 'Basic limits' policies meet the minimum coverage requirement for automobile insurance and 'above basic limits' policies exceed the minimum coverage requirements for automobile insurance. I focus on liability coverage because the amount purchased shouldn't depend on the value of the vehicle owned and I use bodily injury coverage because it is required by the state. The proportion of drivers that purchase 'basic limits' policies ranges from 0% to 44% with an average of 12%.

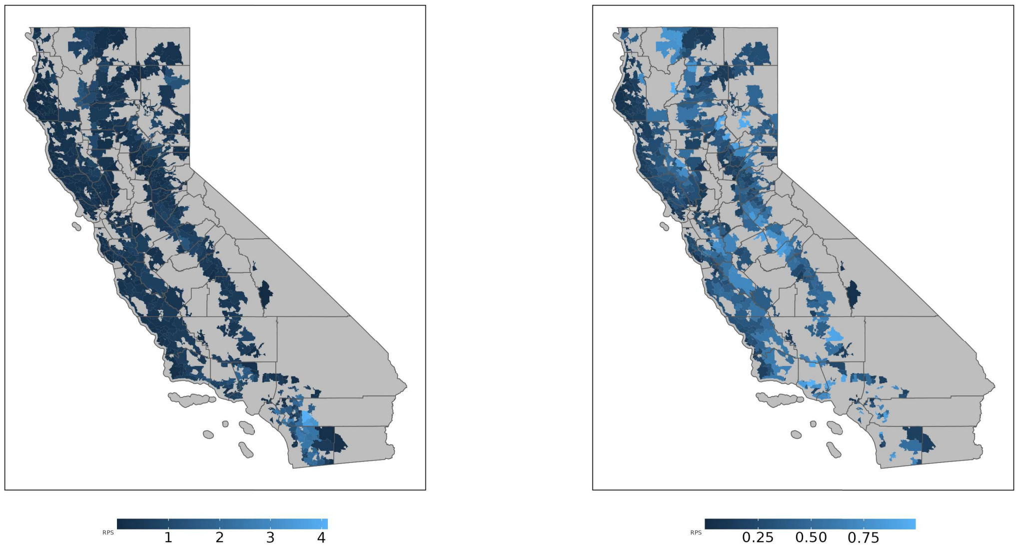
FAIR Plan market share comes from the Community Service Statement (CSS) from the CDI, which reports the number of exposure units (policy months) of coverage at the zip code-year level for each insurance company including the FAIR Plan. I use this data to calculate the FAIR Plan market share. The FAIR Plan represents a small market share on average (1.5%), but in a very few number of zip codes it can range to 50%. However, in more than 75% of zip codes, FAIR plan market share is less than 1.1%.

Wildfire risk data come from the Risk to Potential Structures (RPS) data set, published by the Forest Service Research Data Archive (Scott et al., 2023). These data integrate wildfire likelihood and intensity with generalized consequences to a home on every 30m by 30m pixel for the United States. For every place on the landscape it poses the hypothetical question, "What would be the risk to a house if one existed here?" I aggregate to the zip code level by averaging the values of each pixel located within each zip code boundary. This data represent a snapshot of wildfire conditions at the end of 2014. Figure 5 shows the RPS data aggregated to the zip code level for all zip codes in my estimation sample. A small number of zip codes have a high RPS value which makes it difficult to see the cross sectional variation. The right panel shows the RPS values for all

$<$ income.

zip codes in my estimation with an RPS less than 1. The RPS values represent the probability that a fire capable of causing damage to building burns each year.

Figure 5: Risk to Potential Structures (RPS)



Description: This map shows the average RPS values for each zip code in the estimation sample (at least 25% contained in a FHSZ by land area). The left panel shows the entire estimation sample, and the right panel shows only zip codes with an RPS value less than 1. RPS values represent the probability that a property experiences damage. RPS represents the percent chance that a fire capable of causing damage to a building burns in 2014.

Home value data come from the Zillow Home Value Index (ZHVI). This represents the typical home value for a region and is calculated as a weighted average of homes in the 35th to 65th percentile range. The data are reported at the zip code month level, and I average over each calendar year to obtain an annual estimate. This data span 2009-2020, with some missing zip code years. The typical home value is \$301,676 but varies over zip codes ranging from just under \$30,000 to over \$2 million.

I exclude from my data set any zip code directly impacted by a moratorium on cancellations and non-renewals in the year it was impacted and any following years because the moratorium distorts the ability of insurers to adjust who they offer insurance to and therefore will disrupt the ability of FAIR Plan market share to reflect wildfire risk.[10] This only impacts some zip codes for

[10]In 2018, the California legislature passed Senate Bill 824 that prohibits insurance companies from cancelling or

2018, 2019, and 2020.

1.5 Empirical Strategy

This section sets forth an empirical strategy to test if households sort on income and risk preferences in response to changing wildfire risk. I devise a plan to address three challenges: (1) measuring wildfire risk, (2) measuring risk preferences, and (3) identification.

1.5.1 Challenge 1: Measuring Wildfire Risk

I use FAIR Plan market to measure wildfire risk. Strict price regulation restricts insurers ability to price changing wildfire risk, but insurers can select which risks to take on. FAIR Plan market share is the proportion of the market that traditional insurers have refused to insure due to high wildfire risk. While an insurer can drop a policy for a wide range of reasons, the FAIR Plan only covers losses from fire, internal explosion, and smoke damage, so the only reason to buy it is to insure from wildfire risk.

1.5.2 Challenge 2: Measuring Risk Preferences

I measure risk preferences by examining how changes in wildfire risk impact automobile liability insurance purchases. After controlling for zip code and year fixed effects, driving risk will be unrelated to wildfire risk, so, any changes in mitigating behaviors for risks unrelated to wildfires indicates a change in risk preferences.

I assume that each person has a quantifiable risk preference, and that their risk reduction behavior is consistent for the financial risks that come from wildfire and from driving. This assumption is consistent with empirical evidence from the literature; individual risk preferences appear to be persistent and moderately stable over time (Soane and Chmiel, 2010), and individuals are more consistent in their risk preferences across related domains (such as different types of insurance)

refusing to renew a policy because of wildfire risk in any zip code either impacted by, or adjacent to, a wildfire that was declared a disaster by the state government (CDI, 2023). Each moratorium lasts one year, and begins on the day the disaster is declared.

than across unrelated domains (such as personal finance and health) (Einav et al., 2012; Soane and Chmiel, 2005). I further assume there is no moral hazard and that risk preferences are independent of suite of car insurance policies available for purchase (Barseghyan et al., 2018).

In economic literature, risk preferences are commonly elicited through experiments where participants choose between a set of lotteries, or by observing individual insurance purchases. Aggregate horse race betting data has been used extensively to measure risk preferences, but it is less common to use aggregate insurance data (Barseghyan et al., 2018). In this chapter, I estimate changes in risk preferences rather than levels, and therefore aggregate data suffices.

1.5.3 Econometric Model

I estimate the impacts of wildfire risk on population, migration, incomes, and risk preferences using a two-way panel fixed effects model,

$$Y_{it} = \beta r_{it} + \phi_i + \psi_{ct} + \varepsilon_{it}. \tag{6}$$

Zip codes are indexed by i, years are indexed by t, Y_{it} is the outcome of interest, r_{it} is wildfire risk level (measured by FAIR Plan market share), ϕ_i are zip code fixed effects that control for unobserved variation that is constant over time, ψ_{ct} are county-by-year fixed effects that control for unobserved variation that is constant within a county but changes over time, and ε_{it} are unobservables. I cluster standard errors at the zip code level. I use a range of dependent variables to estimate my effects: total population, in-migration, local in-migration, in-migration by income group, and risk preferences (measured by the proportion of automobile insurance policies that are 'basic limits'). Additionally I use typical house values as a dependent variable to test the mechanism that sorting on incomes and risk preferences is caused by lower house values falling in risk areas. β retrieves the change in Y_{it} for a one percentage point increase in FAIR Plan market share.

1.5.4 Challenge 3: Identification

Threats to identification

The main threat to identification is omitted variable bias. If there is an omitted variable that is related to FAIR Plan market share and also related to an outcome variable, the estimated coefficients will be biased. For example, defensive expenditures are negatively correlated with FAIR Plan market share and also likely to be related to an outcome variable. As defensive expenditures increase, private insurers will be more likely to offer a policy thereby reducing FAIR Plan market share, holding all else constant. Defensive expenditures are also likely to be related to incomes; people with higher incomes are more likely to protect their homes because they can afford to do so.

Defensive expenditures are not the only possible omitted variable that could cause bias, for example, amenity values are correlated with wildfire risk and incomes. The wide range of outcome variables I use means that there is a greater potential for at least one of them to be correlated with an unobserved variable that is also related to wildfire risk. It is clear that some outcome variables will suffer from this problem (such as risk preferences and incomes as illustrated above), but less clear others will (such as population and migration flows). To overcome this potential identification concern, I use an exposure instrument.

An exposure instrument for wildfire risk

To circumvent potential omitted variable bias, I construct an exposure instrument for wildfire risk that draws from the shift-share literature (Bartik, 1987). Shift-share instruments are typically constructed by interacting starting local industry employment shares (constant over time) with aggregate industry shocks (constant over location), and then summing across industries. This is done to avoid bias caused by omitted variables such as local productivity. The idea is that localities with higher exposure to a certain industry (a higher beginning local industry employment share) will experience a larger effect from a common shock to that industry. Goldsmith-Pinkham et al.

(2020) demonstrate that identification using a shift-share instrument comes from independence of the starting local industry shares and the outcome variables, while Borusyak et al. (2021) show identification can also be achieved if aggregate shocks are independent across industries.

I construct my exposure instrument, Z_{it}, in equation 7 by using statewide changes in FAIR Plan market share (FP_t) as aggregate shocks and a baseline measure of wildfire risk, RPS_i, as my local industry shares. The idea is that zip codes with a higher baseline wildfire risk will be more exposed to aggregate shocks in wildfire risk. This differs from traditional shift-share instruments because the local industry shares do not sum to one, and I rely on a single aggregate shock rather than multiple, independent shocks.

$$Z_{it} = RPS_i * FP_t \tag{7}$$

The identifying assumption is that Z_{it} must not impact the outcome variables through any path other than FAIR Plan market share. The main confounders I am worried about are local differences in how FAIR Plan market share reflects wildfire risk. These same local variations are purged when I aggregate FAIR Plan market share to the state level. In addition, zip code and county-by-year fixed effects control for a wide range of cross-sectional and temporal variation.

Despite this, it is still possible a violation may occur. A violation will occur if zip code characteristics that affect outcomes (and vary with aggregate shocks) are also systematically correlated with zip code wildfire risk. For example, I may be concerned that building codes could be correlated with 2014 wildfire risk, and aggregate shocks to wildfire risk could disproportionately impact these building codes in a pattern related to baseline wildfire risk. However, in California, building codes are determined at the state level. There are stricter building codes in Fire Hazard Severity Zones, but this designation doesn't change over time, so it will be absorbed by zip code fixed effects.

To evaluate the strength of my instrument, I run the first stage regression given by,

$$r_{it} = \beta_1 Z_{it} + \phi_i + \psi_{ct} + \varepsilon_{it}, \tag{8}$$

where notation is consistent with equations 6 and 7. This instrument is strong; the R-squared value of the first stage is 0.71, the F statistic is 24.72, and the t-statistic on the coefficient for the instrument (β_1 in equation 8) is 3.33.

1.6 Estimation Results and Discussion

In this section, I report and discuss the estimation results from equation 6 for a variety of outcome variables to determine the effects of wildfire risk on population, migration, incomes, and risk preferences. The coefficients are interpreted as the effects from a one percentage point increase in FAIR Plan market share. When comparing wildfire risk in 2014 to FAIR Plan market share in 2014, a one standard deviation increase in wildfire risk (as measured by risk to potential structures) is related to a 10.25 percentage point increase in FAIR Plan market share. Therefore, the impact of a one standard deviation increase in 2014 cross sectional wildfire risk on population, migration, incomes, and risk preferences, is equal to the estimated coefficients multiplied by 10.25.

1.6.1 Do people migrate in response to changes in wildfire risk?

The results showing population and migration responses to changes in FAIR Plan market share are shown in Table 3. I use county-by-year fixed effects and show the naive specifications (columns 1-3) and the instrumental variable specifications (columns 4-6). The naive estimates show that a one percentage point increase in FAIR Plan market share today results in changes over the next five years of, a 49 person drop in population, a 33 person increase in the number of movers, and a 34 person increase in the number of local movers. If I inflate these estimates to correspond to a one standard deviation increase in wildfire risk, the population will decline by 507 people, the number of in-migrants will increase by 333, and the number of local in-migrants will increase by 347. These number correspond to an average decrease in population of 4%, an increase in the number of movers of 20%, and an increase in the number of local movers of 34%.

The instrumental variable estimates are similar to the naive estimates, but imprecisely estimated. The direction is consistent for population, movers, and local movers, and the size is consistent for movers and local movers, indicating there is no omitted variables biasing these results. The population estimate may suffer from omitted variable bias caused by defensive expenditures. People are more likely to undertake defensive expenditures if the population is low because they cannot rely on community firefighting efforts to protect their homes. Defensive expenditures are also likely to be negatively correlated with FAIR Plan market share, resulting in a positive omitted variable bias.

These results are consistent with a population reshuffle following a change in wildfire risk, with more people migrating out of a risky area than migrating in. In the following sections I investigate how incomes and risk preferences are related to these migration patterns.

Table 2: Total Population and Migration

	Dependent variable:					
	Population	Movers	Local Movers	Population	Movers	Local Movers
	(1)	(2)	(3)	(4)	(5)	(6)
FP Mktshr	-49.45^{**}	32.51^{***}	33.83^{***}			
	(20.20)	(8.08)	(7.72)			
FP Mktshr (IV)				-168.37	31.61	23.50
				(130.62)	(66.19)	(54.48)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No	No
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,506	7,466	7,466	7,506	7,466	7,466
R^2	1.00	0.99	0.98	1.00	0.99	0.98

Note: *p<0.1; **p<0.05; ***p<0.01

1.6.2 Do incomes change in response to changes in wildfire risk?

In this section, I decompose migrants into different incomes groups. The results are shown in Table 3. I classify low income movers as migrants with incomes less than \$25,000 and high income movers as migrants with incomes more than \$65,000. Using the 2SLS specification, I find that a one percentage point increase in FAIR Plan market share increases the number of low-income migrants by 41.5 and decreases the number of high-income migrants by 58. If I inflate these estimates to correspond to a one standard deviation increase in wildfire risk, the number of low income movers increases by 425 (or 78% on average) and the number of high income movers decreases by 591 (or 219% on average).

These results indicate that as wildfire risk increases, in-migrants shift towards being low-income. This means that wildfire risk is concentrating on people with the fewest resources to recover following a disaster. This finding is consistent with Bakkensen and Ma (2020) who find clear evidence that low income residents are more likely to move into high risk flood zones, Strobl (2011) who find that wealthier people migrate out of places hit by a hurricane, and Boustan et al. (2020) who find that out-migration increases following severe disasters, and that incomes fall.

Table 3: Migration by Income Group

	Income<\$25,000	Income>\$65,000	Income<\$25,000	Income>\$65,000
	(1)	(2)	(3)	(4)
FP Mktshr	26.56^{***}	-17.55^{***}		
	(3.36)	(3.12)		
FP Mktshr (IV)			41.50^{*}	-57.64^{**}
			(22.31)	(21.74)
Zipcode FE	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes
Observations	7.466	7.466	7.466	7.466
R^2	0.73	0.97	0.73	0.97

Note: *p<0.1; **p<0.05; ***p<0.01

1.6.3 Do risk preferences change in response to changes in wildfire risk?

I estimate risk preference sorting over wildfire risk by using the proportion of car insurance policies that are 'basic limits' as my dependent variable. The estimation results are shown in Table 4. County-by-year fixed effects over fit the model, so I use year fixed effects instead. Column (5) restricts the sample to zip codes with at least 40% of tax filings with gross income less than

$25,000 (approximately 25% of the data), and column (6) restricts the sample to zip codes with at least 7% of tax filings with gross income more than $200,000 (approximately 25% of the data). Income controls are included as the proportion of people in each income category (excluding the category with incomes greater than $200,000) in columns 2 and 4.

Table 4: Risk Preferences

	Dependent variable:					
	Proportion BL					
	(1)	(2)	(3)	(4)	(5)	(6)
FP Mktshr	0.03*	0.04***				
	(0.02)	(0.01)				
Inc<25		0.18***		0.12**		
		(0.03)		(0.05)		
25<Inc<50		0.15***		0.11***		
		(0.02)		(0.04)		
50<Inc<75		0.15***		0.11***		
		(0.02)		(0.04)		
75<Inc<100		0.15***		0.11**		
		(0.02)		(0.04)		
100<Inc<200		0.15***		0.10**		
		(0.02)		(0.04)		
FP Mktshr (IV)			0.24*	0.24	0.24	0.15*
			(0.13)	(0.16)	(0.24)	(0.08)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	No	No	No	No	No	No
Observations	11,042	8,885	11,042	8,885	2,462	2,110
R^2	0.92	0.97	0.91	0.96	0.95	0.98

Note: *p<0.1; **p<0.05; ***p<0.01

The empirical challenge in estimating the impact of wildfire risk on risk preferences is twofold. Incomes are a bad control variable because they are also causally impacted by changes in wildfire risk (Cinelli et al., 2022), but, excluding incomes could result in omitted variable bias that exaggerates the coefficient if incomes also impact the decision to purchase basic or above basic limits car insurance.[11] First, I recognize that although wildfire risk impacts the migration decisions of low income and high income people differently, these effects are economically small. Therefore I expect the omitted variable bias caused by excluding incomes will be small and unimportant. This is exactly what I find; the coefficients on FAIR Plan market share are not biased by excluding income controls. My preferred specification (column 3) shows that a one percentage point increase in FAIR Plan market share corresponds to a 0.24 percentage point increase in the proportion of car insurance policies that are basic limits (or approximately a 2% increase). Said differently, a one standard deviation increase in wildfire risk causes a 21% increase in the proportion of car insurance policies that are basic limits. This suggests an increase in wildfire risk induces the population to be less risk averse. I assume this is caused by a population reshuffle, because risk preferences are relatively stable over time (Einav et al., 2012).

Second, to reduce the potential for bias coming from changes in income, I restrict the estimation sample to the poorest zip codes (column 5) and the richest zip codes (column 6). Restricting the sample to include zip codes in a narrow income band reduces the possible impacts of FAIR Plan market share on income, and therefore reduces the bias. It also shows heterogeneity in sorting on risk preferences by income group; I cannot statistically detect an effect of FAIR Plan market share on the proportion of policies that are basic limits in the low income group, but I can in the high income group.

These results are one of the first attempts to quantify sorting on wildfire risk with observational data. Bakkensen and Barrage (2022) analyze the question of risk preference sorting on flood risk with a door-to-door survey, but ask hypothetical questions that are difficult to answer accurately. This chapter uses observational data, but assumes that individuals and insurance companies have

[11]Incomes are negatively related to wildfire risk and I expect them to be negatively correlated with the proportion of policies that are basic limits. I also anticipate finding a positive impact of wildfire risk on the proportion of policies that are basic limits. Therefore, omitting incomes could cause my estimate to be exaggerated.

the same perceptions of risk, and that those perceptions are correct. Future research will refine the empirical method and expand this method to other settings.

1.6.4 Mechanism: House Values

The vast majority of people are not risk seeking, and therefore are not drawn to wildfire risky areas by the wildfire risk itself. In fact, if all else is equal, most people would never choose to migrate into a risky area. There must be an additional factor that causes them to move. I hypothesize that this is lower housing costs. I empirically test if typical house values are lower in areas of higher wildfire risk, and the results are shown in Table 5. As expected, the coefficients in all of the estimated models are negative, indicating that housing costs are inversely related to wildfire risk. This provides preliminary evidence that housing costs are the driver of the sorting results.

Table 5: Typical House Values

	Dependent variable:	
	house_value	
	(1)	(2)
FP Mktshr	-415.53	
	(1,112.36)	
FP Mktshr (IV)		$-13{,}531.07^{*}$
		(7,059.02)
Zipcode FE	Yes	Yes
Year FE	Yes	Yes
County-Year FE	No	No
Observations	4,234	4,234
R^2	0.94	0.93

Note: *p<0.1; **p<0.05; ***p<0.01

1.7 Conclusions

This chapter measures sorting on wildfire risk in incomes and risk preferences. I develop a conceptual model that predicts lower income and less risk averse people migrate into risky areas. I empirically test these predictions with an exposure research design that draws from the shift-share literature. I also develop a new way to measure wildfire risk and risk preferences that varies by zip code over time.

Taken collectively, the results from the estimation tell a story that as wildfire risk increases in an area, there is a reshuffling of the population, with lower income and less risk averse people migrating in, potentially caused by lower housing costs. These results are consistent with prior studies that analyze sorting on natural disaster risk.

Sorting on incomes has important implications for policy makers. If indeed lower income people migrate towards and higher income people migrate away from risky areas, then natural disaster risk is concentrating on people with the fewest resources to recover following a disaster. This increases the need for recovery assistance from government and non-governmental organizations. Furthermore, sorting on risk preferences indicates that it may become increasing difficult to incentivize people to undertake private risk mitigation behavior. Less risk averse individuals are more difficult to incentivize to undertake private risk mitigation behaviors, and therefore, government programs designed to help homeowners take action may need to become more aggressive.

1.8 Appendix A: Additional Results Tables

Table 6: Total Population and Migration Results with Year Fixed Effects

	Dependent variable:					
	Population	Movers	Local Movers	Population	Movers	Local Movers
	(1)	(2)	(3)	(4)	(5)	(6)
FP Mktshr	-43.04^{***}	24.14^{***}	20.78^{***}			
	(11.59)	(5.12)	(4.35)			
FP Mktshr (IV)				198.06	-34.59	-63.08
				(122.76)	(52.75)	(47.95)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	No	No	No	No	No	No
Observations	7,506	7,466	7,466	7,506	7,466	7,466
R^2	1.00	0.99	0.98	1.00	0.99	0.98

Note: *p<0.1; **p<0.05; ***p<0.01

Table 7: In-Migration by Disaggregated Income Group: Year Fixed Effects

	Dependent variable:							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FP Mktshr	12.26^{***}	5.40^{***}	6.10^{***}	0.43	0.66	0.40	-1.09^{**}	-11.14^{***}
	(2.33)	(1.00)	(1.12)	(0.72)	(0.64)	(0.76)	(0.48)	(1.60)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	No	No	No	No	No	No	No	No
Observations	7,112	7,068	7,160	7,098	7,068	6,896	6,412	7,096
R^2	0.97	0.95	0.95	0.95	0.95	0.94	0.86	0.96

Dependent Variables: (1) Movers with income < \$10,000 *p<0.1; **p<0.05; ***p<0.01
(2) Movers with \$10,000 < income < \$15,000
(3) Movers with \$15,000 < income < \$25,000
(4) Movers with \$25,000 < income < \$35,000
(5) Movers with \$35,000 < income < \$50,000
(6) Movers with \$50,000 < income < \$65,000
(7) Movers with \$65,000 < income < \$75,000
(8) Movers with income > \$75,000

Table 8: In-Migration by Disaggregated Income Group: County-by-Year Fixed Effects

	Dependent variable:							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FP Mktshr	15.31***	8.06***	7.69***	0.63	0.67	−0.07	−1.39*	−11.89***
	(2.85)	(1.91)	(1.98)	(1.02)	(1.04)	(1.21)	(0.75)	(2.38)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No	No	No	No
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,112	7,068	7,160	7,098	7,068	6,896	6,412	7,096
R^2	0.97	0.95	0.96	0.96	0.95	0.94	0.87	0.97

Dependent Variables: (1) Movers with income < \$10,000 — *p<0.1; **p<0.05; ***p<0.01
(2) Movers with \$10,000 < income < \$15,000
(3) Movers with \$15,000 < income < \$25,000
(4) Movers with \$25,000 < income < \$35,000
(5) Movers with \$35,000 < income < \$50,000
(6) Movers with \$50,000 < income < \$65,000
(7) Movers with \$65,000 < income < \$75,000
(8) Movers with income > \$75,000

Table 9: In-Migration by Disaggregated Income Group: Year Fixed Effects, 2SLS

	Dependent variable:							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FP Mktshr (IV)	−12.65	−21.23*	−5.48	0.09	−0.94	0.27	−5.75	12.85
	(14.50)	(11.81)	(9.22)	(5.87)	(7.18)	(6.32)	(4.66)	(14.43)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	No	No	No	No	No	No	No	No
Observations	7,112	7,068	7,160	7,098	7,068	6,896	6,412	7,096
R^2	0.97	0.94	0.95	0.95	0.95	0.94	0.86	0.96

Dependent Variables: (1) Movers with income < \$10,000 — *p<0.1; **p<0.05; ***p<0.01
(2) Movers with \$10,000 < income < \$15,000
(3) Movers with \$15,000 < income < \$25,000
(4) Movers with \$25,000 < income < \$35,000
(5) Movers with \$35,000 < income < \$50,000
(6) Movers with \$50,000 < income < \$65,000
(7) Movers with \$65,000 < income < \$75,000
(8) Movers with income > \$75,000

Table 10: In-Migration by Disaggregated Income Group: County-by-Year Fixed Effects, 2SLS

	Dependent variable:							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FP Mktshr (IV)	12.97	−15.82	29.99**	−5.45	11.68	−0.33	−13.50*	−37.63**
	(18.28)	(12.92)	(13.28)	(9.42)	(10.87)	(9.06)	(7.02)	(16.64)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No	No	No	No
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,112	7,068	7,160	7,098	7,068	6,896	6,412	7,096
R^2	0.97	0.95	0.95	0.96	0.95	0.94	0.87	0.97

Dependent Variables: (1) Movers with income < \$10,000 — *p<0.1; **p<0.05; ***p<0.01
(2) Movers with \$10,000 < income < \$15,000
(3) Movers with \$15,000 < income < \$25,000
(4) Movers with \$25,000 < income < \$35,000
(5) Movers with \$35,000 < income < \$50,000
(6) Movers with \$50,000 < income < \$65,000
(7) Movers with \$65,000 < income < \$75,000
(8) Movers with income > \$75,000

Table 11: Risk Preferences, County-by-Year Fixed Effects

	Dependent variable:					
	Proportion BL					
	(1)	(2)	(3)	(4)	(5)	(6)
FP Mktshr	0.02	0.02				
	(0.02)	(0.02)				
Inc<25		0.14^{***}		0.12^{***}		
		(0.03)		(0.04)		
25<Inc<50		0.12^{***}		0.10^{***}		
		(0.03)		(0.03)		
50<Inc<75		0.11^{***}		0.09^{***}		
		(0.03)		(0.03)		
75<Inc<100		0.10^{***}		0.08^{**}		
		(0.03)		(0.03)		
100<Inc<200		0.10^{***}		0.08^{***}		
		(0.03)		(0.03)		
FP Mktshr (IV)			0.11	0.10	−0.07	0.11^{***}
			(0.08)	(0.08)	(0.12)	(0.04)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No	No
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,042	8,885	11,042	8,885	2,462	2,110
R^2	0.93	0.97	0.93	0.97	0.97	0.99

Note: *p<0.1; **p<0.05; ***p<0.01

Table 12: Typical House Values, County-by-Year Fixed Effects

	Dependent variable:	
	house_value	
	(1)	(2)
FP Mktshr	−153.14	
	(1,051.01)	
FP Mktshr (IV)		−3,287.18
		(4,259.56)
Zipcode FE	Yes	Yes
Year FE	No	No
County-Year FE	Yes	Yes
Observations	4,234	4,234
R^2	0.98	0.98

Note: *p<0.1; **p<0.05; ***p<0.01

2 Climate Change and the Regulation of a Crashing Insurance Market

(with Reid Taylor and Joakim Weill)

Abstract

As insurers seek to limit exposure to catastrophic losses, homeowners are increasingly unable to find insurance in the private market, forced to turn towards state-sponsored residual risk pools known as "insurers of last resort". In this chapter, we examine how regulation and market structure can lead to market unraveling when firms face rapidly increasing risk due to climate change. We first present a simple model of an adversely selected disaster insurance market to investigate how price regulation and increasing climate costs impact private markets in the presence of an insurer of last resort. We then empirically study the California non-renewal moratoriums, a first-of-its-kind regulation aimed at stymieing the retreat of insurance companies from high wildfire risk areas by forcing insurers to supply insurance to current customers following disasters in 2019 and 2020. Using quasi-random geographic variation in regulatory borders and a difference-in-differences identification strategy, we find that the moratoriums successfully reduced company-initiated non-renewals in the short run. However, the effects only lasted for one year, with insurers dropping policies as soon as the moratorium lapsed. Additionally, the moratoriums had no discernible effect on participation in the State's insurer of last resort.

2.1 Introduction

Natural disasters pose substantial financial risks to households, firms, and communities, highlighting the urgent need for well-functioning insurance markets. However, escalating costs associated with disasters, compounded by spatially correlated and potentially catastrophic losses, present new challenges for insurers. In a majority of US states, "insurers of last resort" provide coverage to households unable to purchase insurance elsewhere. Initially meant to temporarily insure only the riskiest properties, state-run insurers of last resort now hold substantial market share in many places across the United States. This presents a puzzle for insurance market regulators: why are insurers of last resort growing, and which policies can help prevent private insurance markets from unraveling?

In this chapter, we examine how price regulation can result in the unraveling of the private insurance market, and thus full reliance on the insurer of last resort, when insurance firms face rapidly increasing risk due to climate change. We begin by constructing a conceptual model of a natural disaster insurance market featuring price regulation, a residual risk pool serviced by the insurer of last resort, and a private ("voluntary") market in which firms can use information about the marginal cost of consumers to choose which properties to insure. We show how the presence of a residual market is necessary to guarantee full market coverage when regulation constrains price below the expected average cost. When firms observe the underlying risk profile of consumers, they choose not to insure those that have expected costs above the regulated price, resulting in a bifurcated market as a portion of consumers receive coverage in the residual risk pool.

Our framework rationalizes the recent dynamics observed in the California homeowners insurance market, the largest homeowners insurance market in the country. Following consecutive record-setting wildfire seasons in 2017 and 2018, insurers in California refused to renew more than 200,000 homeowner's insurance policies primarily in areas of high wildfire risk (Bikales, 2020), a 61% increase from prior years, citing restrictive price-setting regulations (California Department of Insurance, 2021). At the same time, take-up of policies offered through the state's insurer of last resort, the California FAIR plan, spiked in the same areas. This is consistent with private firms reducing their exposure by ceding the highest risk policies to the insurer of last resort as cost increases outpaced increases in the regulated price of premiums. The model predicts that if prices are not allowed to reflect climate costs, then adjustments must occur through the insurer of last resort.

We then study a first-of-its-kind policy that California implemented to counter the explosive growth in its insurer of last resort: the non-renewal moratorium, which forces insurers to continue supplying insurance for at least one year to certain homeowners. The regulation impacts zip codes located near state-declared 'state of emergency' wildfires for one year following the emergency declaration, first in 2019 and then in 2020. We exploit the quasi-random variation generated by the regulatory boundaries of the moratoriums to study the causal impact of the policy on insurance market outcomes. Our treatment group includes zip codes adjacent to but not directly impacted by the wildfires. This avoids confounding the treatment effect of the moratorium with the direct effects of the fire, which are likely correlated with our outcomes of interest. Our control group comprises zip codes directly adjacent to those impacted by the regulation. These areas offer a credible counterfactual, as they closely resemble the treatment zip codes and were not subject to the moratorium: the quasi-random occurrence of wildfire ignition sites and zip code boundaries suggests their exemption from regulation was purely coincidental. In robustness tests, we show that results are similar with an alternative control group that uses nearest-neighbor matching between treatment zip codes and observably similar but untreated zip codes in the rest of the state.

We find that the moratoriums successfully increase insurance supply by decreasing company-initiated non-renewals while they are active, with no evidence that firms are able to avoid the regulation by forcing out customers using other methods. However, this effect is short-lived; firms increased non-renewals by 72% to 96% as soon as the year-long moratoriums ended. Additionally, we estimate that the moratorium had no discernible impact on slowing the transition of policies from the voluntary market into the FAIR plan. While the regulation restricted non-renewals of currently insured customers, it had no effect on firms refusing to insure new customers. These results highlight that the California moratorium only acted as a short-term band-aid, and that deeper changes of the rate-setting guidelines are required to avoid the unraveling of the market.

This chapter contributes to a growing literature on natural disaster insurance markets (Kunreuther, 1996, 2001; Kousky, 2011; Born and Klimaszewski-Blettner, 2013; Knowles and Kunreuther, 2014; Oh et al., 2023; Kousky, 2022; Marcoux and Wagner, 2024). Due to both the historical importance of flood losses and the lack of publicly available data on other homeowner insurance policies, work in this area largely focuses on flood insurance (Gallagher, 2014; Bradt et al., 2021; Wagner, 2022; Mulder, 2022; Weill, 2023). Two recent exceptions are Sastry et al. (2023), who focus on wind damage in Florida, and Boomhower et al. (2023), who examine the impacts of wildfires on the Californian homeowner insurance market. Boomhower et al. (2023) investigates the issue of wildfire risk estimation for insurers, while we focus on the interaction between private insurance markets and the state insurer of last resort, with an application to the California non-renewal moratoriums.

This chapter also contributes to the literature on insurance regulation, both theoretically and empirically. We develop a model of adverse selection in a segmented market under price regulation and rapidly changing risk that extends the canonical model of Einav et al. (2010), which has been used broadly to study adverse selection in insurance and lending markets (Spinnewijn, 2017; Cabral and Cullen, 2019; Boyer et al., 2020).[12] A majority of states are now operating "residual markets" or an "insurer of last resort" (Kousky, 2011). We provide a simple framework to investigate how these markets interact with private markets. This chapter also shows how regulating both price and risk selection, as is the case with the California non-renewal moratoriums, can lead to long-run firm exit and unravelling of the private market. Our work expands on the natural disaster insurance regulation literature (Born and Viscusi, 2006; Born and Klimaszewski-Blettner, 2013; Oh et al., 2023) and the smaller set of studies focused on regulatory effectiveness and efficiency in California's insurance market (Liao et al., 2022).

Finally, this work fits into a broader literature on climate adaptation (Barreca et al., 2016; Diaz and Moore, 2017; Kousky, 2019; Botzen et al., 2019; Kahn, 2021; Sastry, 2021). We first contribute to a large literature that focuses on housing markets and climate change; numerous studies document how insurance pricing and access impact the real estate market (Nyce et al., 2015; Issler et al., 2024) and how natural disaster risk can impact mortgage repayment (Biswas et al., 2023; Xudong et al., 2024). We also add to the small but growing literature on firm level adaptation to climate change (Prankatz and Schiller, 2021; Gu and Hale, 2022; Castro-Vincenzi,

[12] See Einav and Finkelstein (2023) for a review of literature that makes use of the framework from Einav et al. (2010).

2022; Bilal and Rossi-Hansberg, 2023).

This chapter proceeds as follows. Section 2 provides background on the California Moratoriums while section 3 introduces our conceptual model. Section 4 presents the data used, section 5 introduces the econometric framework and section 6 presents the results. Section 7 concludes.

2.2 Institutional Background

2.2.1 Insurance Markets

The price of an insurance policy is set before any potential losses are incurred.[13] This implies that insurers' profitability depends on both accurate projections of expected losses, and premiums that reflect these projections. Theoretically, if firms were unconstrained in their ability to calculate and set policy premiums, they would be able to offer a price for all risks. However, regulations have emerged with the dual goals of protecting customers from rates that are unfairly discriminatory and unreasonable, and ensuring premiums are sufficient to guarantee solvency. In some cases, regulation can lead premiums to diverge from expected costs through both suppressing premium growth and limiting the firm's ability to accurately incorporate cost forecasts. We focus our discussion on the main distortions rate regulation introduces to natural disaster risk pricing in the California homeowner's insurance market, which generalize to a varying degree to other state markets.

Before new insurance rates can be implemented, insurers must obtain prior approval of rates with the state Department of Insurance. This administrative process is cumbersome, and frequently lasts more than 12 months (Oh et al., 2023). The specifics of the rate approval process vary widely between states. For instance, in California, regulators face three specific regulations which suppress premium growth in practice. First, overall rate increases of 7% or higher (calculated over the entire insurer portfolio) are subject to in-depth public scrutiny at the unrecoverable cost of the insurer (California Ballot Propositions and Initiatives, 1988). This regulation has resulted in an

[13] Most property and casualty lines of insurance follow experience rating whereby premiums can be adjusted for losses incurred in previous contract periods. However, some policies use retrospective rating, which settles the final premium amount due at the end of the period and takes into account losses from that same period. Retrospective rating is generally reserved for worker's compensation and commercial policies.

effective rate increase cap as most rate increase filings are below this threshold, with significant bunching at 6.9% (Boomhower et al., 2023).[14] Second, California regulation requires the overall rate for natural disasters, known as a catastrophe load, to be justified by historical averages of losses over at least the past 20 years (California Code of Regulations, 2024).[15] Until 2023, insurers in California were not allowed to incorporate forward-looking catastrophe models or other means of forecasting as justification for higher catastrophe loads, exacerbating premium inadequacy when past loss experience does not reflect future expectations. Finally, California restricts firms from passing reinsurance costs through to consumer premiums. Recent industry literature has highlighted greatly increasing reinsurance premiums as climate risks increase, with reinsurance companies not subject to the same regulatory oversight as consumer facing insurance companies. This further drives the difference between the cost firms incur and the premium they are able to charge the customer.

State regulation also routinely specifies which observable home and homeowner characteristics are permissible in the underwriting and rating processes. While the classic case of adverse selection relies on consumers having private information unobserved by the firm, adverse selection can also arise from regulation restricting the set of permissible characteristics used for pricing. Thus, even after conditioning on the permissible observables, consumers that are offered the same premium can still vary in their expected costs. Regulation in California in late 2022 made two changes to the underwriting process: first, firms were forced to incorporate defensible space characteristics into their rating plan, and second, any use of catastrophe or risk scoring to underwrite or create rate differentials had to be filed with the state. The setting of rate differentials is distinct from catastrophe load calculations, which, as previously stated, have never been permitted to integrate catastrophe models. The second regulation change presents a hurdle to firms as they were given the option of publicly filing proprietary and confidential models (some contracted through

[14] Because the 7% regulatory constraint is calculated over the full portfolio of the insurer, the premiums can still increase faster than 7% for some homeowners.

[15] Insurers can opt to weigh certain years more than others in the premium calculations. However, actuarial convention and the regulator can push back on nonuniform weighting schemes, especially if firms are over-weighting certain years to increase expected costs.

3rd party companies) or to cease their use.

2.2.2 California Moratoriums

In response to large losses from record breaking wildfires in 2017 and 2018, insurance companies began to withdraw from high wildfire-risk areas. A standard, one-year insurance policy can typically only be cancelled mid-term by the insurer due to lack of payment or material fraud on behalf of the insured. However, an insurer is able to non-renew (not offer a subsequent contract) for a wider range of reasons, including changing beliefs about the probability of a claim. In an attempt to stymie the retreat of insurance companies from high-risk locations, the California legislature passed Senate Bill 824 in 2018. This bill prohibits insurance companies from non-renewing a policy because of wildfire risk in any zip code either directly impacted by, or adjacent to, a wildfire that was declared a state of emergency by the state government. The commissioner of the department of insurance cited the bill as giving, "millions of Californians breathing room and hits the pause button on insurance non-renewals while people recover."[16] The regulation impacts firms by limiting their ability to geographically diversify and to drop policies which are likely otherwise unprofitable given the firm's rating plan.

Each moratorium lasts one year from the date of disaster declaration. For the years examined in our study, the earliest start date for a moratorium is August 18 and the latest start date is November 18. We refer to the moratoriums by yearly cohorts. The collection of non-renewal moratoriums initiated following the 2019 fire season is the "2020 Moratorium", while those initiated after the 2020 fires is referred to as the "2021 Moratorium".

Due to the stochastic nature of wildfires, and specifically wildfire perimeters, zip codes located near each other can be differentially impacted by the moratorium despite being observably similar. Additionally, high risk areas in other parts of the state that have not yet experienced a fire post-legislation are not covered by the moratoriums, despite being similar. The quasi-random nature of the initial coverage of the moratorium, coupled with the lack of lead time and anticipation for firms,

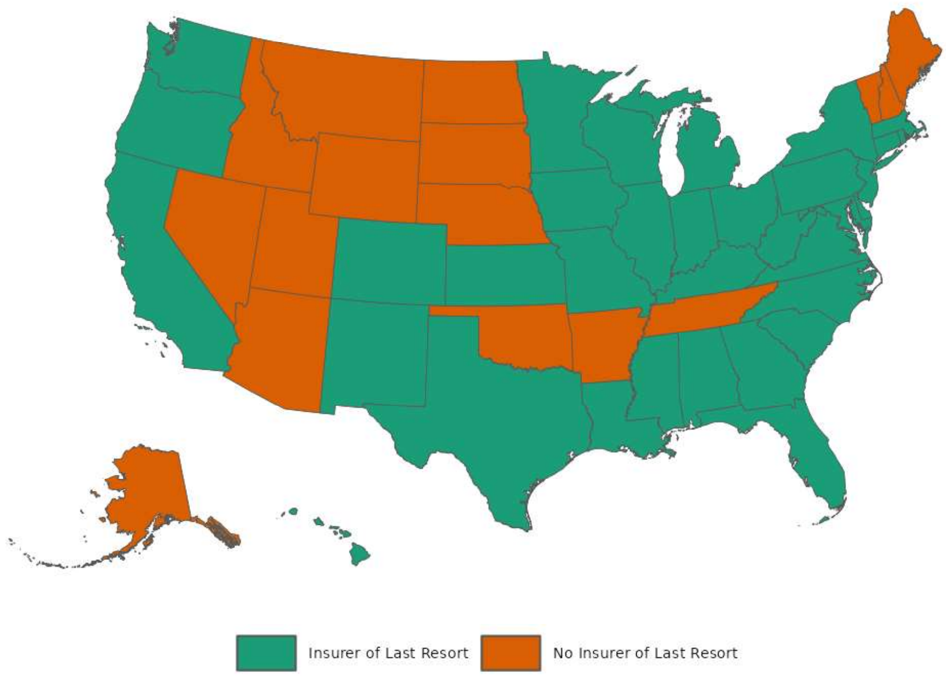
[16] See https://www.insurance.ca.gov/01-consumers/140-catastrophes/MandatoryOneYearMoratoriumNonRenewals.cfm.

forms the basis of our identification strategy to identify the causal impacts of the moratoriums on various insurance and consumer outcomes.

2.2.3 Insurers of Last Resort

Residual markets, or “insurers of last resort”, are state-run or state-sponsored plans that sell coverage for properties considered very high risk and unable to find insurance in the voluntary market. Insurers of last resort vary between states; Fair Access to Insurance Requirements (FAIR) plans were established in twenty-six states, the District of Columbia, and Puerto Rico in 1968 following the riots and bushfires of the 1960s. Dwyer (1978) offers an illuminating discussion of the establishment of FAIR Plans. Some states have “wind pools” or “beach plans”, which cover specific perils and limited geographies. These plans are typically more expensive than policies in the voluntary market and their coverage varies by state; see Kousky (2011) for a discussion of ten state-sponsored disaster insurance programs. Today, 34 states and the District of Columbia have an insurer of last resort (shown on Figure 6) – Colorado became the latest state to establish a FAIR Plan in 2023 in response to increasing wildfire losses.

Figure 6: Insurers of last resort in the United States



The California FAIR Plan issues policies on behalf of all companies licensed to write property and casualty insurance in California. Each member company participates in the profits, losses, and expenses in direct proportion to its market share in the state and thus are invested in the financial stability of the FAIR Plan. Its purpose is to provide temporary, basic fire insurance when traditional insurance is not available. It is designed to act as an emergency safety net while homeowners search for insurance in the traditional (or voluntary) market. California FAIR Plan policies are typically more expensive than the voluntary market, have a maximum coverage amount of $3 million, and require customers to obtain an additional Difference in Coverage (DIC) policy in order to replicate the coverages offered in a standard homeowner's insurance policy. Despite these differences, FAIR Plan and voluntary insurance policies are substitutable in the market because both can satisfy the requirements of mortgages lenders for homeowner's insurance.

2.3 Conceptual Model

This section presents a general, but simple, framework to clarify the role of the residual insurance market in a market with adverse selection. We begin by characterizing supply and demand in the presence of price regulation, and use a graphical approach to illustrate how the market equilibrium changes with increasing wildfire risk.

Our framework closely follows Einav et al. (2010); consumers make a discrete purchase decision for a homogeneous full-coverage insurance policy, which they buy at the lowest price available from profit-maximizing firms competing in the market. Consumers purchase their policy from either the private ("**voluntary**") market or the **residual** market.

We focus on two distinct pricing regulations. First, conditional on a set of permissible property characteristics $\{c_i\}$, the regulator sets a fixed price $\tilde{P}$ in the voluntary market. Second, in the residual market, prices can adjust freely but the regulator imposes a zero-profit condition.

The pricing constraint imposed in the voluntary market captures features found in state-level regulation of homeowner insurance rates.[17] In California, the Department of Insurance limits the characteristics $\{c_i\}$ that insurers can use to determine premiums. In particular, estimates of risk from catastrophe models cannot currently be used to set household-level insurance premiums (California Code of Regulations, 2023).[18] Insurers in California determine rates through a complex rate-filing process, in which requested premium increases averaged over the insurer portfolio must stay below 7% to avoid a costly public hearing.[19]

Most property-level characteristics that impact expected losses and that are observable to homeowners (such as location, building materials, number of floors, etc.) are readily observable to insurers. This stands in contrast to health or auto insurance markets, where consumers typically have private information about their expected losses. However, regulations that restrict the observable characteristics, c_i, permitted for rate-making prevent insurers from achieving perfect

[17]For example, California, Hawaii, Maryland, Massachusetts, Michigan, Nevada, Oregon and Utah prohibit the use of credit scores to determine home insurance rates.

[18]Relaxing premium regulation to allow for catastrophe modeling is an active debate in California (Watkins and Lee, 2022; State of California Department of Insurance, 2023).

[19]Additional details regarding insurance rate regulation in California are discussed in Boomhower et al. (2023).

price discrimination, resulting in what Finkelstein and Poterba (2014) call "asymmetrically used information". Imperfect price discrimination manifests itself as consumers with the same permissible characteristics being charged the same price, *despite* otherwise observable differences in their expected costs. This information asymmetry results in adverse selection, characterized by consumers with the highest expected costs also having the highest willingness to pay resulting in downward sloping marginal and average cost curves Einav et al. (2010). For simplicity, we assume that demand is higher than average cost at every point, implying that at actuarially fair prices, every consumer prefers to purchase insurance than to go without.[20]

Although firms do not have the freedom to set prices, they control which consumers to serve at the regulated price, and thus quantities in the voluntary market. Insurers can observe the marginal cost curve and decide not to offer insurance contracts to certain properties. However, the California moratoriums implemented in 2020 and 2021 directly eliminated this decision-making variable for insurers for one year.

2.3.1 Market Segmentation

In the graphical analysis that follows we depict one tranche of the market where all consumers have the same set of permissible characteristics and are charged the same premium, but vary in their expected losses. In Figure 7 panel (a), we consider the case where the regulator imposes an exogenous price $\hat{P}$ below the average cost curve at every point. While all consumers would opt to buy insurance at this price, insuring the entire market (Q^{max}) would lead to negative expected profits for firms.

In panel (b), firms use their knowledge about the marginal cost curve to select which consumers they offer coverage to. They choose to offer coverage only to consumers that are profitable, such that $\hat{P} \geq MC$. This results in only a portion of the market receiving insurance coverage from the voluntary market, consumers from Q^R to Q^{max}. The remainder of the market (Q^0 to Q^R) is forced to purchase from the residual market. The zero-profit condition imposed on the residual

[20]According to the National Association of Insurance Commissioners, about 90% of homeowners have insurance, largely due to the requirement to buy insurance to obtain a mortgage.

market results in price being set at the average costs over the customers that purchase from the residual insurer: $P^R = AC^R(Q^R)$. By construction, all customers ceded from the voluntary market will purchase a policy from the residual insurer because their willingness-to-pay is greater than the average cost curve at every point.[21] Positive profits (shown in blue) in the short run are possible in the voluntary market because prices are fixed at the regulated level and the costs of market entry are non-trivial.[22]

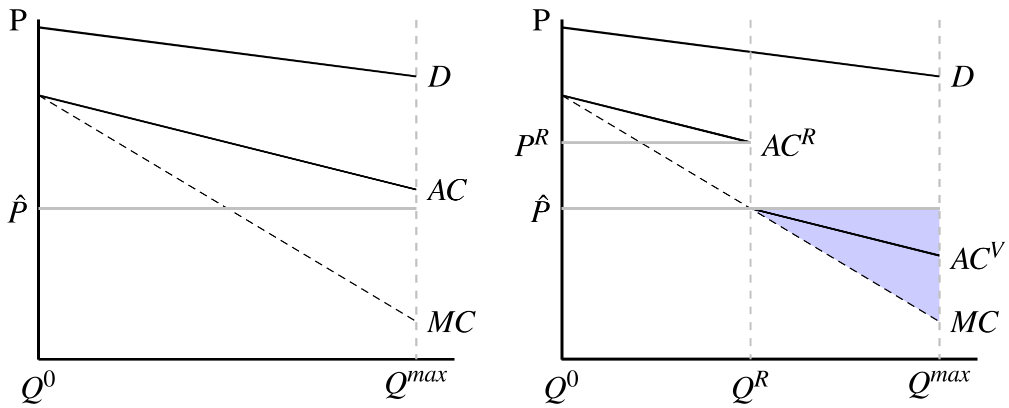
This stylized example highlights the crucial role played by the residual market; it ensures that all consumers can purchase an insurance contract *despite* the price regulation. To see this, note than in the absence of the residual market, consumers with marginal costs above the regulated price cannot buy insurance *regardless of how much they are willing to pay* as no firm would be willing to insure them. In contrast, in the absence of regulation, competitive firms would perfectly discriminate and charge each consumer a price equal to their marginal cost, ensuring insurance availability. The residual market thus allows price-suppressing regulation to be sustainable in the voluntary market.

Relative to the perfect price discrimination benchmark, where the price for each customer is equal to their marginal cost, the scenario with a residual market and price regulation entails clear distributional consequences. All consumers in the voluntary market are charged more than their marginal costs, with the lowest risk consumers paying the highest markups. Consumers buying in the residual market are charged an average cost necessarily greater than both the regulated price of the voluntary market ($\hat{P}$) and the average cost pooled across the total market, as the risk pooling is concentrated on only the highest risk consumers. Within the residual market, the riskiest consumers are charged less than their marginal cost, while the least risky consumers are charged more than their marginal cost.

[21] In an alternate scenario, if the demand curve were steep enough, consumers that are marginally ceded from the voluntary market will not purchase from the residual market as the pooled price is higher than their willingness-to-pay.

[22] Characterization of the long-run, dynamic nature of rate requests and the role profits play in future negotiation with the regulators is beyond the scope of this chapter.

Figure 7: Baseline Market



(a) No Segmentation (b) Presence of a Residual Market

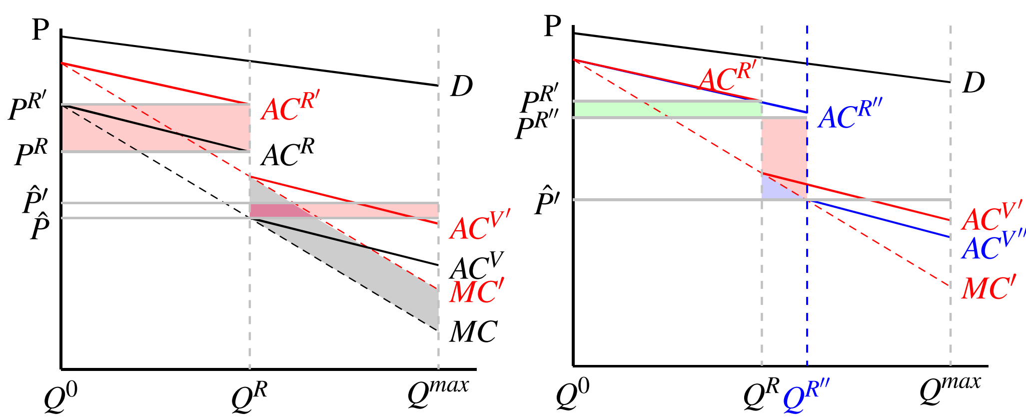
2.3.2 Increasing perceptions of wildfire risk

Consider what happens when the industry experiences an extreme weather event (or series of events) that causes insurers to update their risk perceptions. We distinguish between actual risks (which we assume remain constant) and perceptions of risk (which are impacted by recent events). We assume that when perceptions change, they shift closer to the true risk levels. In Figure 8, MC' and AC' represent the increase in perceived insurance costs following a particularly bad series of extreme events. For simplicity we assume these curves are a parallel shift in expected costs for each consumer.

We focus on what happens when the regulated price adjusts more slowly than perceived costs increase, as is the case in our setting. That is, the regulated price increases from $\hat{P}$ to $\hat{P}'$, with $\hat{P}' - \hat{P} < AC' - AC$. Such situations can occur due to the length of the rate-filing process, or due to specific regulatory constraints. In contrast, because the price in the residual market is not constrained by the regulated price, price adjusts to the average cost over customers already insured in the residual market, $P^{R'} = AC^{R'}(Q^R)$, keeping profits equal to zero in the residual market.

Panel (a) of Figure 8 coincides with the market under the California non-renewal moratoriums when firms are not allowed to adjust which consumers they serve in the voluntary market (Q^R is held constant). As premiums increase, consumers suffer a reduction in wealth represented by the red rectangle in the residual market and the red and purple areas in the voluntary market. Firms in the voluntary market expect higher costs, shown by the grey and purple areas, which is only partially offset by the increase in the regulated price. In this situation, firms lose money on consumers with a new marginal cost greater than the new regulated price, and would choose not insure these properties absent the moratorium. If the perceived increase in costs is high enough that the average cost over the remaining consumers is above the regulated price, firms will exit the voluntary market in the long run and the market will collapse.

Figure 8: Market With Expected Cost Increase



(a) No Residual Market Adjustment (b) Residual Market Adjustment

Panel (b) depicts the market if firms are able to adjust their portfolio given the new regulated price and cost curves, meaning that Q^R is allowed to adjust. In relation to our empirical study, this coincides with the market at the end of the non-renewal moratorium. Following the increase in risk perceptions, the voluntary market cedes any consumers who have a new marginal cost higher than the new regulated price, which are between Q^R and $Q^{R''}$, where $Q^{R''}$ is determined by the intersection of the new marginal cost curve and the new regulated price. Because we consider a demand curve that is always above the residual market's average cost curve, the consumers dropped from the voluntary market will purchase insurance in the residual market. Given the residual market operates as a non-profit, and that the consumers dropped from the voluntary market have lower marginal costs than those already participating in the residual market, the residual market price drops to $P^{F''}$.

The costs of allowing adjustment from Q^R to $Q^{R''}$ are entirely born by the group of consumers forced out of the voluntary market as a result of the adjustment. These consumers lose the red and blue areas in Figure 8 (b) due to the higher price $P^{F''}$. The firms capture the blue portion of the welfare loss due to the reduction in expected losses. Customers already in the residual market experience a benefit, shown by the green rectangle, because the addition of lower-risk consumers to the risk-sharing pool reduces the price.

In sum, this model generates four simple predictions in relation to our empirical study of non-renewal moratoriums under changing risks: (i) premiums increase in both the residual market and voluntary market, (ii) consumers in the voluntary market are not ceded to the residual market when the moratorium is active, leading to short-run losses for firms, (iii) the residual market share increases when the moratoriums become inactive, and (iv) holding costs constant, residual market price decreases when the moratoriums become inactive. We test these predictions in the following section and assess how the characteristics of consumers in the residual market and voluntary market changed following the moratoriums.

2.4 Data

2.4.1 Insurance Data

We obtain homeowner's insurance data from the California Department of Insurance (DOI). These data are a combination of three separate products: the Community Service Statement (CSS), the Personal Property Exposure (PPE), and the Residential Property Experience (RPE). The CSS contains information on earned exposures, earned premiums, number of policies, and average premium at the company-zip code-year level for all insurance companies licensed to operate in California from 2009 to 2020. Importantly, the California FAIR plan reports data in the CSS alongside companies in the voluntary market which allows us to calculate a zip code level FAIR plan market share. The PPE survey reports the amount of coverage provided, number of units insured, and deductible amounts at the company-zip code-year level from 2009 to 2021. All companies writing more than $5 million in insurance in California are required to report. Lastly, the RPE data set reports the number of new, renewed, and non-renewed policies at the zip code-year level. Importantly, we observe whether the decision to non-renew the policy was initiated by the insurer or by the customer. The RPE is reported yearly from 2015 to 2021.

2.4.2 Wildfire Risk

We use the Risk to Potential Structures (RPS) data from the US Forest Service to construct zip code level measures of wildfire risk.[23] The RPS relates both the probability of a fire occurring as well as the expected intensity of a potential fire, asking the question, ”What would be the relative risk to a house being located on this pixel?” Thus, the measure does not rely on the current presence of a building in order to assess the risk. This allows for an insurance relevant wildfire risk measure to be calculated even in sparsely populated portions of the state, and comparison between currently inhabited and not yet inhabited locations. We calculate the zip code level average RPS by calculating the mean across the RPS values for each 30 meter pixel located within the boundary of the zip code. We also calculate the standard deviation of the RPS values within a zip code to capture the variability of fire risk within a zip code. The RPS is time invariant and represents a snapshot of wildfire conditions modeled in 2014. In reality, wildfire risk can change over time following drought conditions and recent wildfire activity. Additionally, the RPS data do not account for changes or variation in home construction types, which is an important way homeowners can manage wildfire risk.

2.4.3 Wildfire Boundaries

We use geolocated fire perimeters from the California Department of Forestry and Fire Protection (CalFire)'s Fire and Rescue Assessment Program (FRAP) to identify the location of wildfires

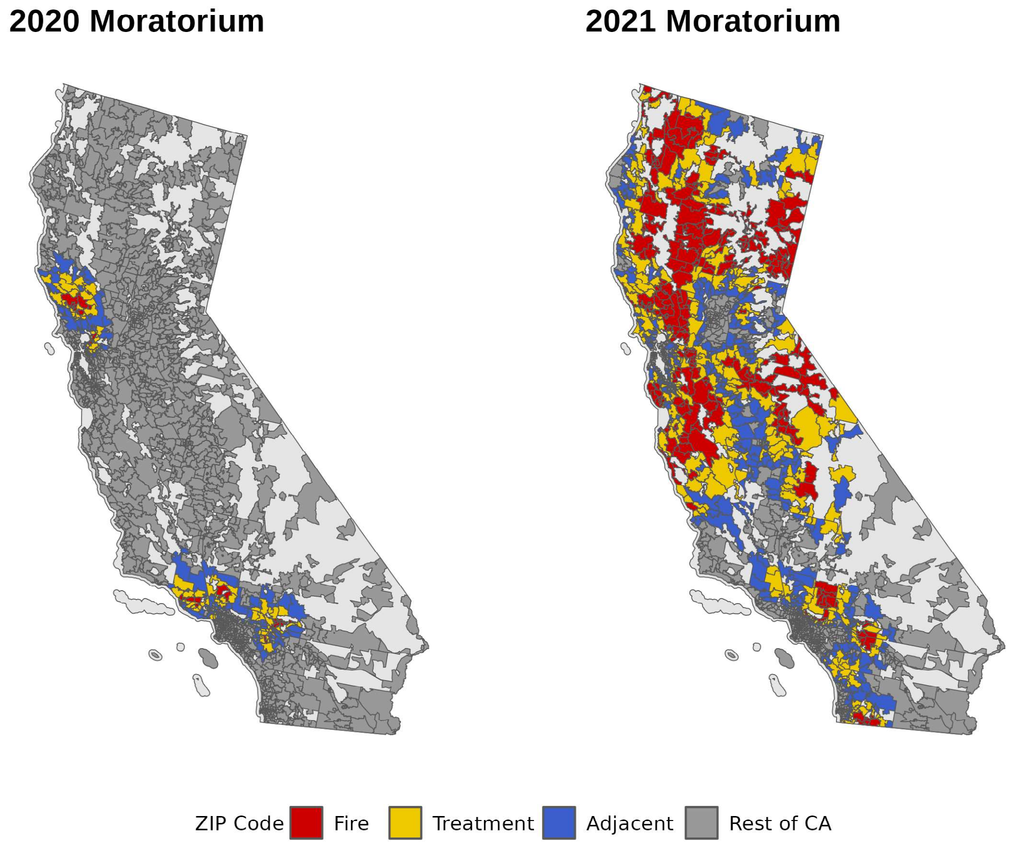
[23]Formally, zip codes are not geographic in nature, but yet relate a collection of mail routes. The census thus created Zip Code Tabulation Areas (ZCTA) which are geographic representations of zip codes. We use ZCTAs to construct all geographic level data to match the level of observation of our insurance data, but use the more common term “zip code” in the rest of the text.

during our sample period. The fire perimeters are developed by CalFire jointly with the US Forest Service, the Bureau of Land Management, and the National Park Service, and the Fish and Wildlife Service covering both public and private lands in California. Data on the location, area covered, cause of the fire, and the responding agency are available. We exclude prescribed fires from our dataset. Wildfires occur in both Northern and Southern California, largely concentrated in the foothill and mountainous areas along both the coastal and Sierra Nevada ranges.

2.4.4 Non-Renewal Moratorium Status

We identify zip codes subject to a non-renewal moratorium in 2020 or 2021 using data from the office of the California Insurance Commissioner. We classify zip codes into 4 categories. 'Fire' zip codes are those that were included in the moratorium because they directly experienced a fire that was declared a disaster. 'Treatment' zip codes are included in the moratorium by regulation due to being adjacent to zip codes which burned, but did not directly experience the fire causing the disaster declaration. These zip codes form the basis of our identification strategy discussed in the following sections. 'Adjacent' zip codes are zip codes that are not included in the moratorium but share a border with a zip code covered under a moratorium. 'Rest of State' encompasses all other unimpacted zip codes. The Department of Insurance reports all zip codes subject to a moratorium without distinction, thus we spatially merge the fire perimeter data to differentiate the 'Fire' and 'Treatment' zip codes. Figure 9 depicts the various moratorium classifications for the state of California in 2020 and 2021, separately.

Figure 9: Zip Code Classifications



2.4.5 Descriptive Statistics

Table 13 presents summary statistics for the dataset broken out by the 2020 moratorium classification. As expected, fire zip codes were also the riskiest, *ex ante*, as measured by RPS, but indistinguishable from nearby treatment and adjacent zip codes while zip codes in the 'Rest of State' category have a notably lower wildfire risk level. This supports the assumption that while areas prone to wildfire are not random distributed, the precise location of wildfire events and the interaction with zip code boundaries is essentially random. While areas impacted by fires and the moratorium have higher FAIR Plan market shares, only 3% of the market is served by the FAIR plan on average in wildfire impacted zip codes. Over the entire sampling period, there does not appear to be any trends associated with the number of new policies, renewals, or customer or company initiated non-renewals.

Table 13: Summary Statistics by Zip Code Classification (2020 Moratorium)

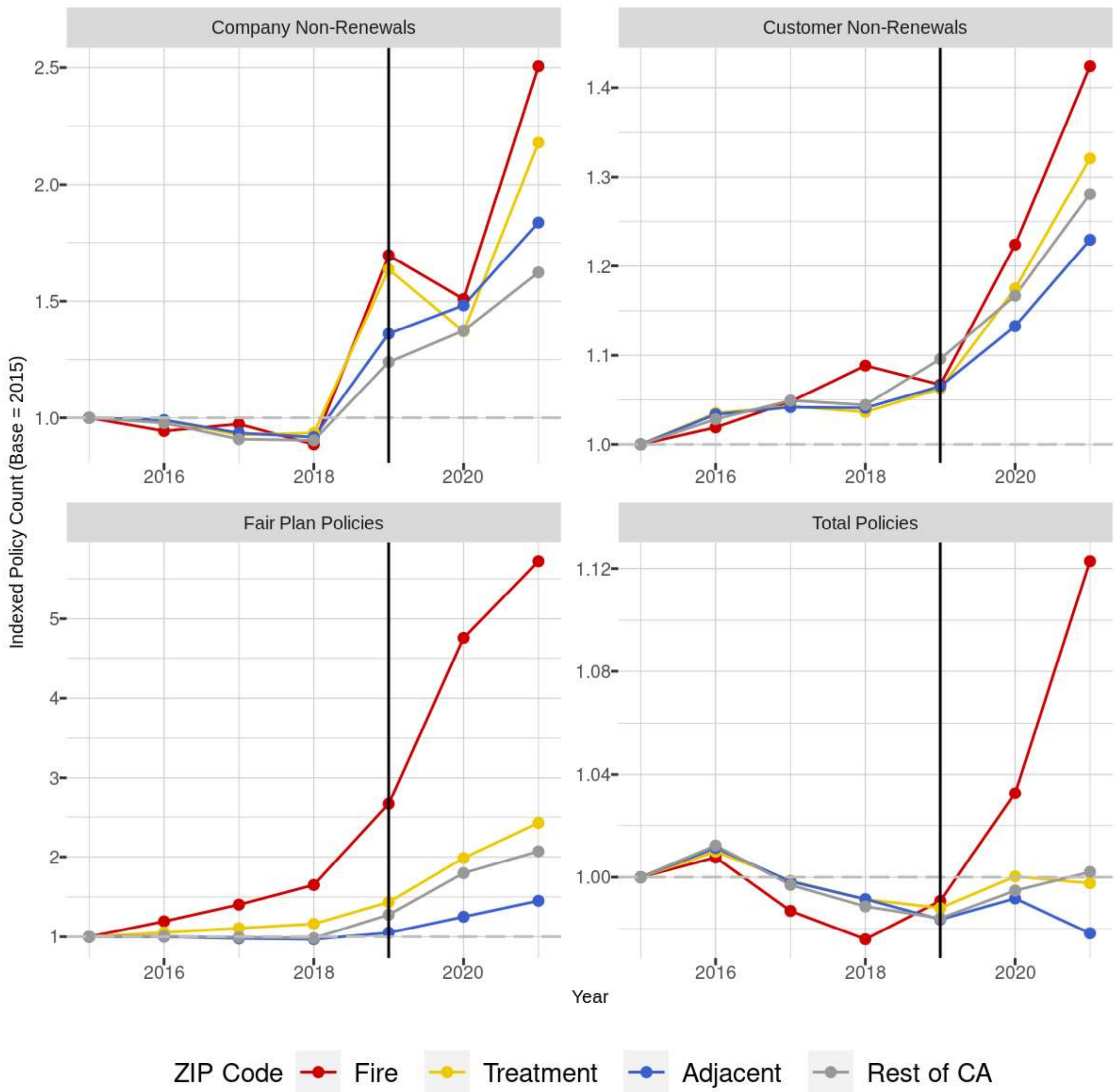
Zip code Classification	Fire		Treatment		Adjacent		Rest of State	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Premium (dollars)	1049.5	388.4	1099.2	579.8	1085.1	677.0	966.3	495.2
New Policies (count)	517.9	513.5	848.6	653.5	790.6	710.8	511.8	577.5
Renewals (count)	4262.1	3924.9	6309.0	4303.0	5672.2	4639.0	3995.7	4171.0
Customer Nonrenewals (count)	404.7	385.7	622.9	481.1	594.5	540.4	382.0	433.4
Company Nonrenewals (count)	101.2	100.3	168.0	128.3	143.5	123.1	95.3	102.9
FAIR Plan Market Share	0.03	0.05	0.02	0.04	0.02	0.05	0.02	0.04
RPS	0.5	0.6	0.5	0.6	0.4	0.5	0.2	0.4

Figures 10 and 11 show the evolution of the number of company-initiated non-renewals, customer-initiated non-renewals, new policies, and renewals by zip code classification status separately for the 2020 moratorium and 2021 moratorium. Statistics are indexed relative to their level in 2015.

For the 2020 moratorium, 'Fire' and 'treatment' zip codes both saw a decrease in the number of company-initiated non-renewals in the year they were covered by the moratorium, consistent with the moratorium being a binding constraint on firm behavior. However, we also see a large reversal the year the moratorium was lifted for these zip codes. While only preliminary, this suggests that the moratorium was only successful at altering firm behavior in the short term. A potential concern in our setting is that firms are able to circumvent the moratorium by forcing-out customers through making their product less attractive in an effort to have them cancel, thereby disguising a company-initiated non-renewal as a customer-initiated non-renewal. This would result in the moratorium seeming more effective than it actually is. While we note here that customer-initiated non-renewals increased the most in 'fire' zip codes, we return to this question later in our results using a causal framework, showing that this is likely not a concern.

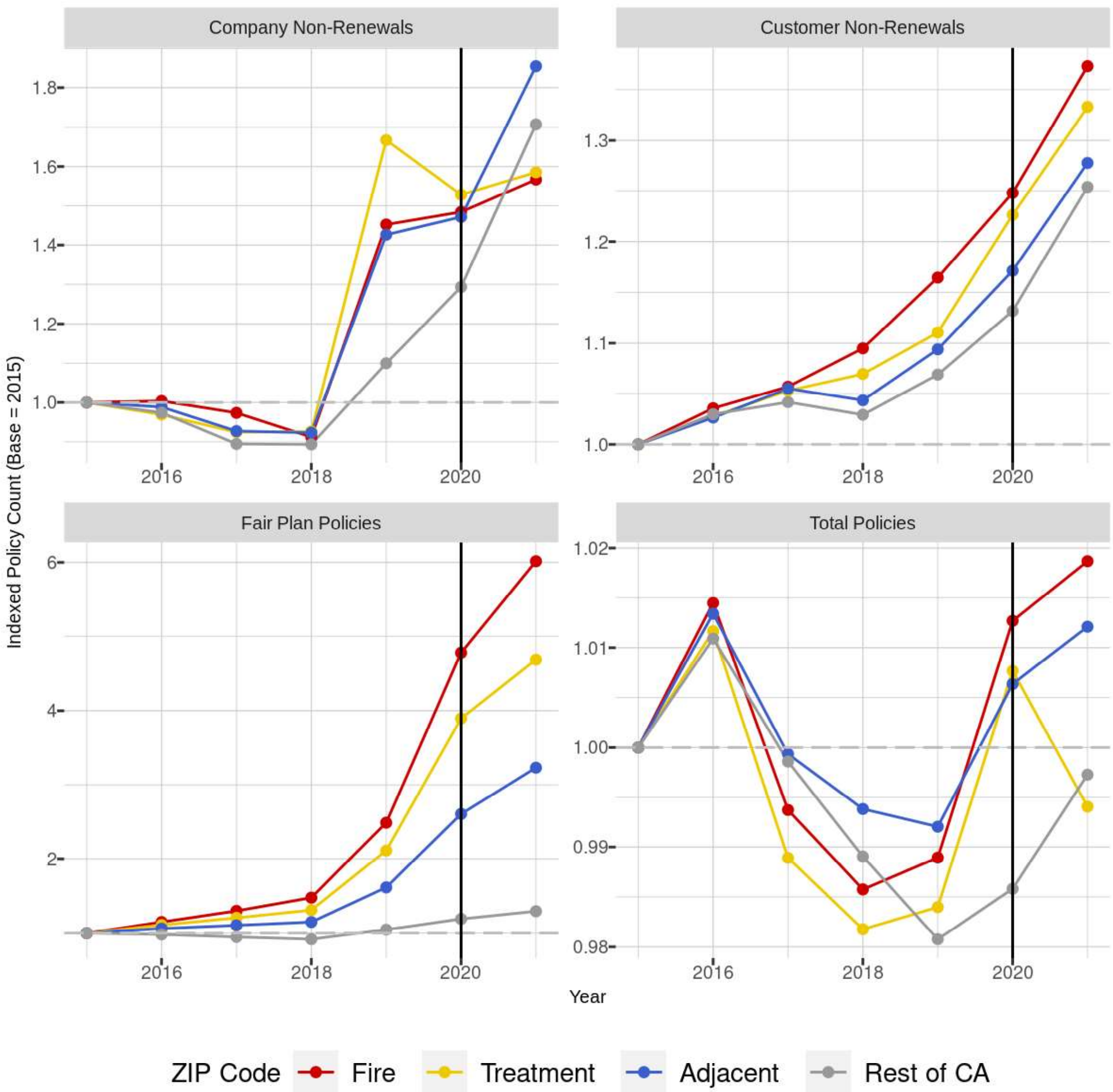
For both the 2020 2021 moratorium designations, we see a clear increase in both FAIR plan policies and the total number of policies in the year after the fire. This is consistent with evidence that large disasters can drive *ex post* demand for insurance, as well as the narrative that firms are

Figure 10: Statistics by 2020 Moratorium Classification



Notes: Zip codes are broken out by moratorium classifications. 'Fire' zip codes were directly impacted by a wildfire in 2019 and covered by the non-renewal moratorium in 2020. 'Treatment' zip codes were covered by the non-renewal moratorium in 2020 but did not experience a wildfire in 2019. 'Adjacent' zip codes share a border with zip codes covered by the non-renewal moratorium in 2020. 'Rest of State' zip codes are the remaining zip codes not covered by the moratorium.

Figure 11: Statistics by 2021 Moratorium Classification



Notes: Zip codes are broken out by moratorium classifications. 'Fire' zip codes were directly impacted by a wildfire in 2020 and covered by the non-renewal moratorium in 2021. 'Treatment' zip codes were covered by the non-renewal moratorium in 2021 but did not experience a wildfire in 2020. 'Adjacent' zip codes share a border with zip codes covered by the non-renewal moratorium in 2020. 'Rest of State' zip codes are the remaining zip codes not covered by the moratorium.

accelerating their retreat from the highest risk zip codes.

Taken together, this descriptive evidence suggests that the moratorium may have had significant impacts on the market by reducing non-renewals in the short-term, but that these reductions were concentrated to just the year of the moratorium coverage. In our next section we formalize the assumptions needed to identify the causal impacts of the moratoriums.

2.5 Methods

The stochastic nature of wildfires and the unique geographic coverage of the California non-renewal moratorium allow for a difference-in-differences specification to recover causal estimates of the policy's impacts. We make use of the sharp geographic border discontinuity between neighboring zip codes, comparing treated zip codes covered by the moratorium to those zip codes located just outside the borders, before and after the policy change.

The non-renewal moratorium covers policies in zip codes that experienced a state declared disaster fire and their immediate neighboring zip codes. Identification in our model requires that no other changes, contemporaneous with the policy, could explain the observed changes in the outcome variables. As such, we omit zip codes that are directly impacted by a disaster fire from treatment as they experience housing supply shocks, receive disaster relief funding, and are impacted by other unobserved factors perfectly correlated with the timing of the moratorium.

Our estimating equation is,

$$y_{zt} = \alpha + \sum_{j=0}^{1} \beta_j T_z D_j + \sigma_z + \delta_t + \varepsilon_{zt}, \tag{9}$$

where, y_{zt} is the outcome of interest in zip code z in year t, T_z is the treatment indicator variable which takes a value of 1 if zip code z is impacted by a moratorium during the sample period, and D_t are post-period event time indicators taking a value of $D_j = 1$ for the year of the moratorium ($j = 0$) or the first year post treatment ($j = 1$). We include zip code fixed effects, σ_z, to control for time-invariant geographic heterogeneity correlated with wildfire risk and the insurance outcome variables, such as climate, elevation, slope, vegetation types, population density, and access to emergency services. We also include year fixed effects, δ_t, to account for common annual shocks across all units. This controls for unusually dry or hot seasons or macro-financial trends which impact the risk appetite of firms. We cluster model standard errors at the zip code level.

Identification of causal estimates from equation (9) relies on three main assumptions. First, the common trends assumption requires that outcome variables in both treatment and control areas should evolve along the same trend over time, and would have continued along a similar path absent the moratorium. We can provide supporting evidence for the assumption through estimation of the following event study analog of equation (9):

$$y_{zt} = \alpha + \sum_{j=-6}^{1} \beta_j D_{j(gj)} + \sigma_z + \delta_t + \varepsilon_{zt}, \tag{10}$$

where, y_{zt} is our outcome of interest and $D_{j(gt)}$ is matrix of indicator variables which take a value of one if the first year of the moratorium is j years away for zip codes in moratorium group g in year t. We would expect to estimate no statistical difference between the treatment and control groups in the pre-policy event-time coefficients.

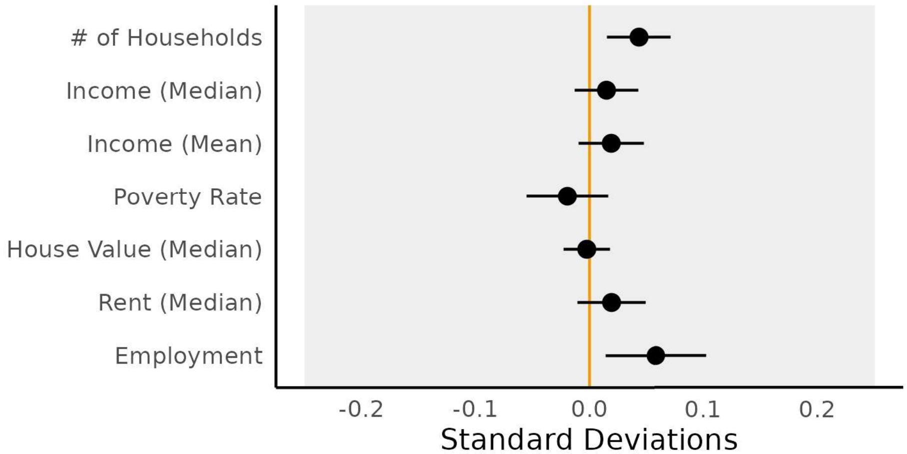
Secondly, recent methodological advances show that the TWFE model, as shown in equation (9), only yields consistent causal estimates of the average treatment effect on the treated when the treatment effects are homogeneous across groups (Goodman-Bacon, 2021; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfoeuille, 2020). The main concern is due to the staggered timing of the policy across zip codes, the two-way fixed-effects model uses all possible combinations of treatment and control comparisons, leading to earlier treated groups being used as controls for later treated groups, resulting in inconsistent estimates for the average treatment effect on the treated if effects are heterogeneous across cohorts.

There are two reasons why we would expect heterogeneous treatment effects in our setting. First, a non-renewal moratorium is novel to the California insurance market, meaning insurers may adapt over time in how they respond to the policy and the Department of Insurance may also adapt in their enforcement role. Second, due to the record-breaking 2020 wildfire season, the 2021 moratorium covered more territory than in 2020, leading to a potentially different response from the insurers as a larger share of their business was under compulsory supply.

We take several steps to address these concerns. We report our results using estimates for the event study models using the estimator from Sun and Abraham (2021) which delivers consistent estimates in the presence of heterogeneous effects and differential timing of treatment. Secondly, we estimate equation (9) separately for the 2020 and 2021 treatment cohorts selecting only never-treated control units from each cohort's neighboring zip codes, limiting the "forbidden comparisons".

Finally, because we use geographic borders to designate treatment, identification requires that the populations on either side of the border are homogeneous and that there is no selection into treatment or other feature of zip codes correlated with treatment. In order to limit the inherent differences between treatment and control groups, and to account for unobserved heterogeneity, we restrict the control group to zip codes that directly border a treated zip code , but do not experience the moratorium during our sample. We believe these zip codes represent a plausible counterfactual due to geographic proximity. In figure 12 we report the coefficients from cross-sectional baseline regressions using demographic and housing characteristics from the 2018 Census American Community Survey 5-year estimates at the zip code level. Results show that the treatment and adjacent zip codes for both the 2020 and 2021 moratorium are observably similar in the pre-treatment period. After controlling for observable factors, it is by random chance that these zip codes were not included in the moratorium boundaries because the location and size of disaster fires is as good as random each year. Importantly, as zip codes are not administrative boundaries, such as city or county borders, we would expect the unobserved heterogeneity to be smooth across the border. We also unaware of any increased funding or interventions implemented by jurisdictions in response to the fires that follow zip code designations or would apply to zip codes which were unimpacted by the fire perimeter.

Figure 12: Baseline Regressions



Notes: Plotted estimated coefficients and 95% confidence intervals are from cross sectional regressions at the zip code level of the respective outcome variable on an indicator for whether the zip code is treated. Observations are limited to our treatment and adjacent zip codes. Outcome variables have been standardized. Data is sourced from the 2018 Census American Community Survey's 5-year estimates.

It is plausible that the most geographically proximate zip codes follow different trends and that the most similar zip codes are actually located elsewhere in the state. For example, zip codes along the foothills of the central valley have very high fire risk, but border flat farm land which has near zero wildfire risk as measured in our data. In response, in addition to our main difference-in-difference specification, we refine the control group by using a nearest-neighbor matching approach, matching treated zip codes covered under the moratorium with zip codes from the 'rest-of-state' zip codes that do not border any treatment area, and are never treated or experience a disaster fire during our sample period. Following the synthetic control literature, we match based on pre-treatment period trends in outcome variables as well as our time invariant measures of average and variance of wildfire risk (RPS) at the zip code level.

An additional benefit of using the matching approach is that by choosing zip codes that do not directly neighbor the moratorium zip codes, we are able to test whether there are spillover effects from the treatment areas to the neighboring zip codes. The imposition of the moratorium disrupts a firm's ability to balance the geographic concentration of their portfolio by forcing supply in the moratorium zip codes. This may lead to increased departure of firms from the closest zip codes to avoid being too highly concentrated, biasing the estimates from our base DiD approach. Similar results between the base sample and our nearest-neighbor matched sample provides supporting evidence that there is no differential spillover between nearby and more distant zip codes.

2.6 Results

We begin our discussion of the impact of the non-renewal moratorium on insurance markets by estimating whether the regulation was in fact binding for firms. If the moratorium is binding, we would expect to see a sharp decrease in company-initiated non-renewals in treated zip codes compared to the control groups while the moratorium is in effect. We present event study results using the estimator from Sun and Abraham (2021) for the effect of the non-renewal moratorium on company-initiated non-renewals in Figure 13. Event time 0 represents the year the moratorium was in effect in the treatment zip code, while event time 1 represents the year after the moratorium has been lifted and the regulation is no longer in effect. Point estimates are shown along with 95% confidence intervals. Results from the specification using the adjacent neighboring zip codes as the control group are shown in panel (a). We find a large and statistically significant decrease in company-initiated non-renewals the year of treatment.

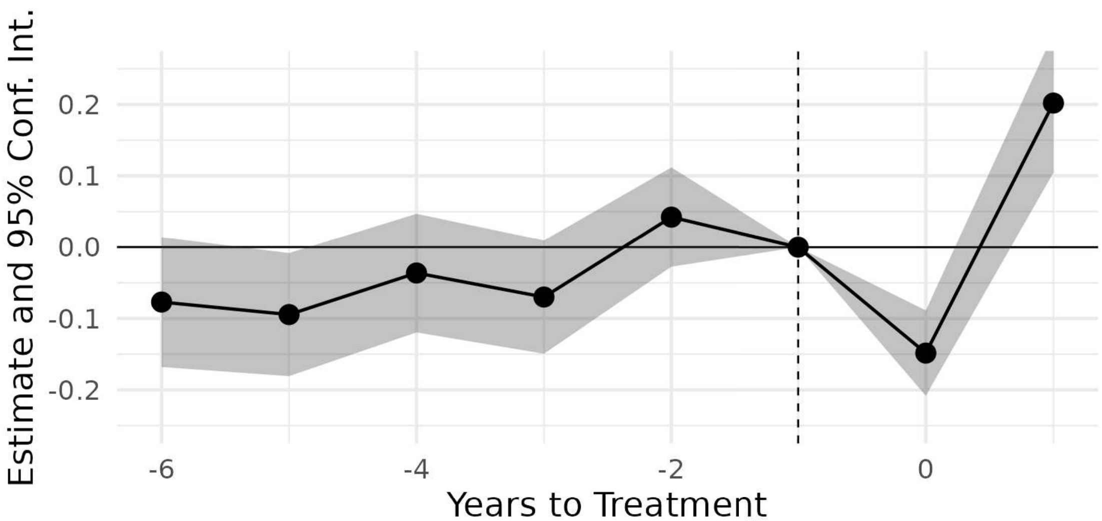
During the period of the moratorium, firms decrease their non-renewals by 15% compared to the control zip codes. Non-renewals do not completely disappear during the moratorium as firms are still able to non-renew policies for a variety of reasons, and only non-renewals that uniquely cite increased wildfire risk were restricted by the moratorium. The sharp decrease provides evidence that the regulation was binding and firms were not able to fully avoid the regulation.

However, the effect of the moratorium is short-lived as the decrease in non-renewals is quickly reversed the year after the moratorium is lifted. We estimate a large subsequent increase in non-renewals of 20% when compared to the adjacent control zip codes at event time 1. This is consistent with the narrative that firms not only simply delayed the non-renewal action to the following contract period, but accelerated their retreat from moratorium areas.

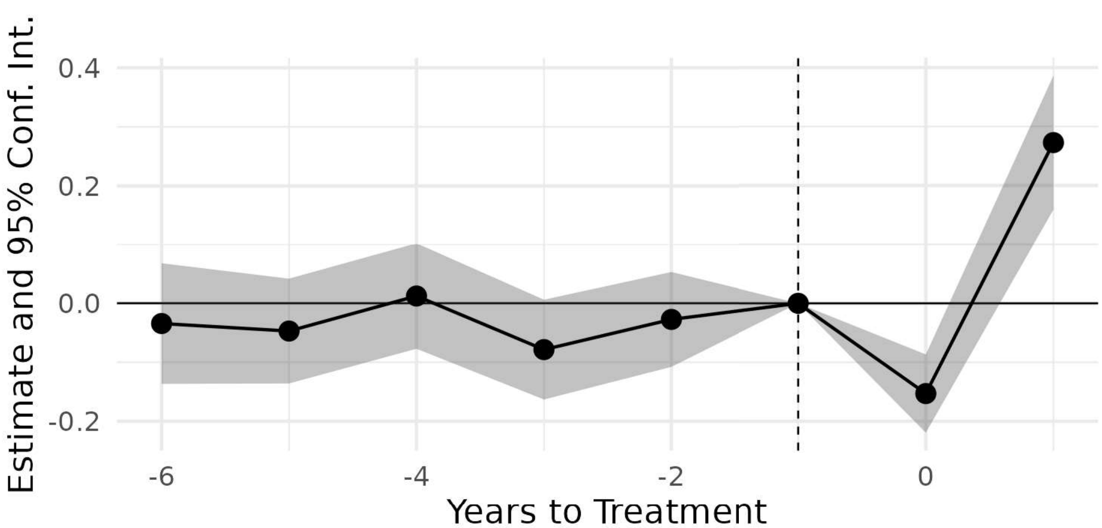
While the adjacent zip codes are located next to treatment zip codes covered by the moratorium, it is possible that these zip codes differ, not just in their levels, but also trends in insurance outcomes over time. Zip codes that burn may be systematically different than zip codes geographically close by, and therefore the best counterfactual setting may be located in other parts of the state. To address this, we also report estimates from a matched difference-in-differences model, where control zip codes are chosen through nearest-neighbor matching on average pre-treatment outcomes and zip code wildfire risk (RPS), shown in panel (b) of Figure 13. We restrict the pool of potential control zip codes to those that are not adjacent to treatment. We estimate a nearly identical decrease in non-renewals the year of the moratorium and a slightly larger post-policy increase the year the moratorium expires, with similar precision of estimates when compared to the specification using adjacent zip codes as control units.

Because the matched control group draws uniquely from non-adjacent zip codes, this specification allows for spatial spillover effects of treatment. Forcing firms to retain additional policies in treated areas that they would have otherwise non-renewed could lead firms to adjust their portfolio in adjacent zip codes in order to avoid being geographically concentrated in high risk areas. The similarity between the results using adjacent control zip codes and the matched control group suggests that spillovers are not a concern in our setting.

Figure 13: Effect of the Moratorium on Company-Initiated Non-Renewals



(a) Control group: Adjacent zip codes



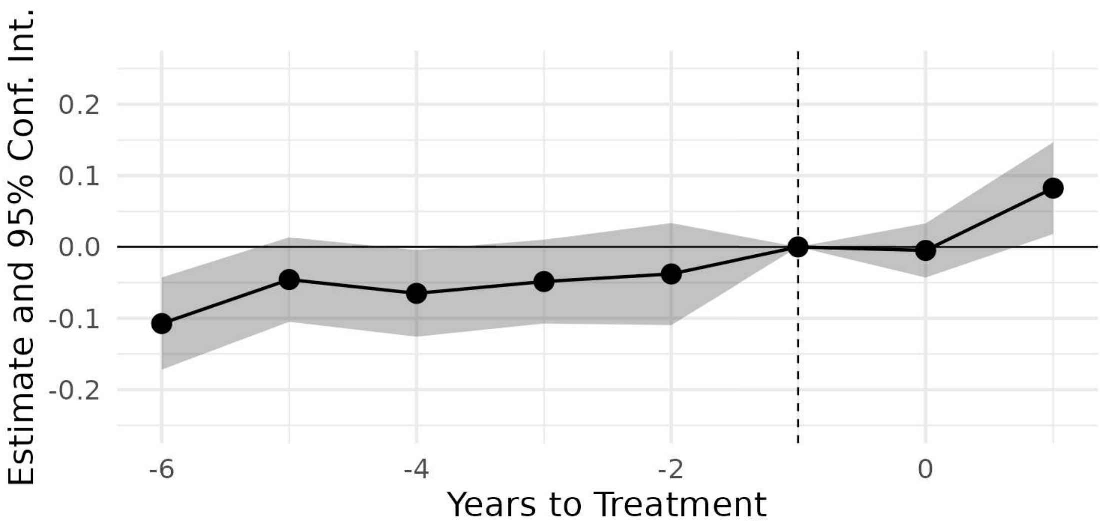
(b) Control group: NN-matched zip codes

Identification of causal effects in the event study framework relies on common pre-trends between treatment and control zip codes and that these trends would have continued in the absence of the policy. We can test the first part of this assumption using the pre-treatment estimated coefficients from the plots in Figure 13. For both control groups, pre-period coefficients include zero in the confidence-interval with no systematic trend, providing supporting evidence that the treatment and control groups were not on separate trends.

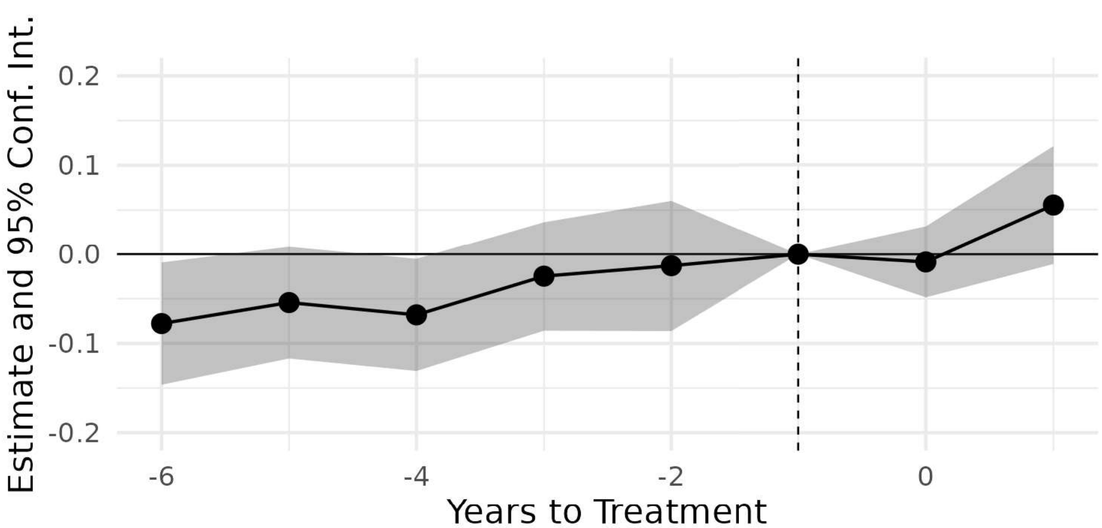
In Appendix Tables 16 and 17 we explore heterogeneity in the effect along the dimensions of wildfire risk and income. We estimate Equation (10) separately by RPS and income quartile. Results show the effect was largely contained to the highest wildfire risk zip codes. However, there is no evidence of heterogeneous effects by income quartile,

We now turn to the effect of the moratorium on non-renewals initiated by the customer. Figure 14 plots the estimated coefficients using both adjacent zip codes and the matched control zip codes in separate panels. Customer-initiated non-renewals were unaffected during the moratorium, but increased the year it was lifted. Coefficients on the pre-treatment years are precisely estimated and indistinguishable from zero, providing support that the common trends assumption holds in this setting. The lack of an effect the year of the moratorium provides further supporting evidence that the moratorium was binding for firms. While we cannot rule out that firms increased non-renewal activity for other reasons in response to the moratorium, this result shows that firms were not successful pushing away existing customers or manipulating their reports. If they had been able to do so, we would expect to see a positive coefficient, similar in magnitude, to the coefficient from the regression on company-initiated non-renewals in Figure 13 at event time 0.

Figure 14: Effect of the Moratorium on Customer-Initiated Non-Renewals



(a) Control group: Adjacent zip codes

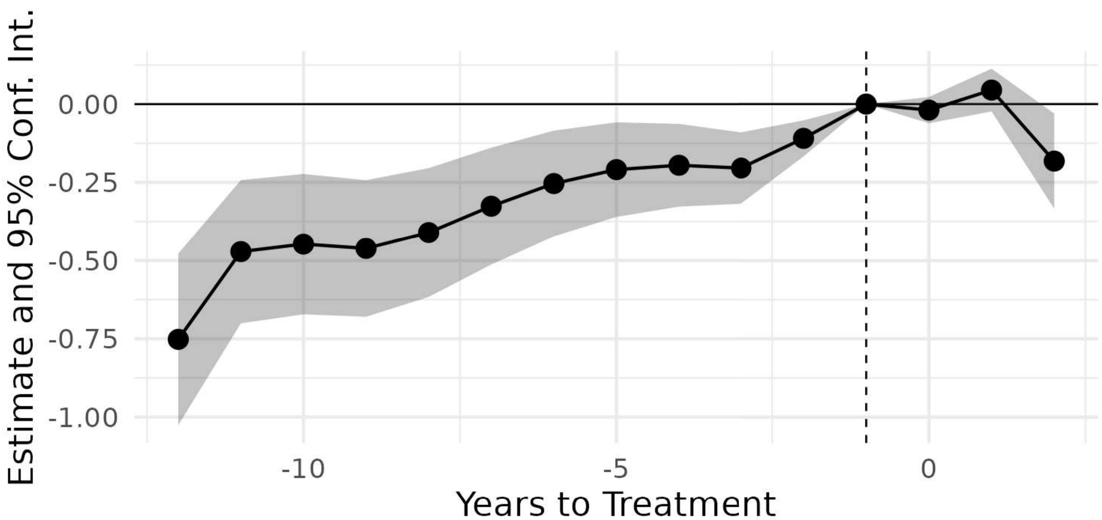


(b) Control group: NN-matched zip codes

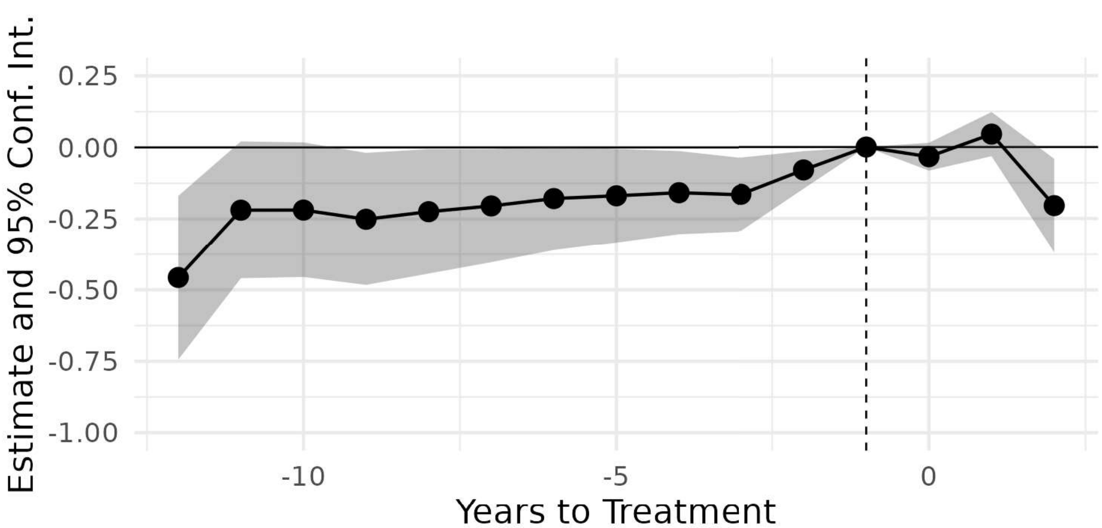
We provide the following explanation for the post-moratorium positive coefficient. Once firms were able to non-renew policies again, this created a salient shopping trigger for customers. Non-renewals have to be delivered in writing to the policyholder at least 75 days in advance of the expiration date of the policy (California Code, Insurance Code – INS § 678.1). Thus, the process of the insurers initiating non-renewal action likely drove increased customer-initiated non-renewals as customers shopped for new policies ahead of their contract expiry. Thus, the true effect of the moratorium being lifted represents a combination of the customer and company-initiated non-renewals in event time 1. We can think of the addition of the two coefficients as the upper-bound estimate of the combined effect of the moratorium on firm driven non-renewal activity.

We next turn to the efficacy of the moratorium in slowing the transition of policies from being insured by the voluntary market to being covered by the FAIR plan. From the descriptive evidence presented in the data section, we know that the number of policies insured by the FAIR plan in both treated and control areas is increasing during the period leading up to the moratorium as firms had already begun to limit their exposure in high risk areas. When using our econometric approach with FAIR plan policies as the outcome variable (shown in Figure 15), we do not detect any discernible impact of the moratorium on the market share of the FAIR Plan.

Figure 15: Effect on FAIR Plan Policies



(a) Control group: Adjacent zip codes



(b) Control group: NN-matched zip codes

Results using the adjacent control zip codes exhibit significant diverging pre-trends, which violate the assumption needed for causal interpretation of the regression coefficients. The results are consistent with the story that FAIR plan market share was increasing in the treated zip codes faster than in control zip codes prior to the moratorium. Yet, upon employing matched control units, we notice considerably milder pre-trends between treatment and control units. Nevertheless, we continue to estimate a precise null effect of the moratorium.

The moratorium eliminated one channel through which firms could reduce their exposure in high-risk areas. However, our results show that customers were rejected from the voluntary market and found insurance in the FAIR Plan at similar rates in both areas covered by the moratorium and control areas. This provides evidence that the policy was an ineffective tool at slowing the retreat of insurers from high risk areas, despite providing temporary reprieve for select customers. Firms were still able to reduce their exposure through not writing new policies for current residents who had their policy cancelled by another firm for non-protected reasons, or homeowners new to the zip code who were also not protected by the moratorium.

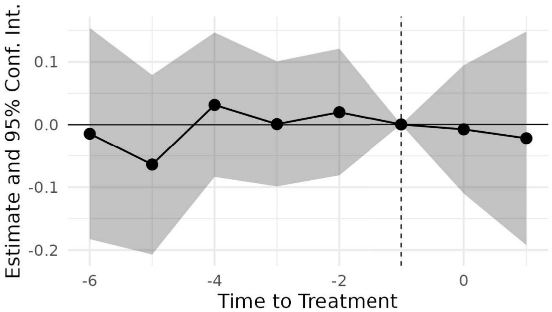
2.7 Conclusions

As climate change increases the risk of large scale natural disasters, well-functioning insurance markets will be necessary for consumers who rely on them often as the sole method of risk transfer. This chapter highlights how regulation and market structures that has traditionally been designed to benefit consumers by suppressing price levels can have large distortionary effects and lead to the unraveling of the market as prices and risk diverge. The California non-renewal moratorium is a unique policy tool the government implemented in an attempt to maintain a stable supply of homeowners insurance in the face of rapidly increasing wildfire risk. The moratoriums were effective in achieving this goal, but only in the short-term, and the strong rebound effect suggests that this policy is not an effective long term solution to correct market failures.

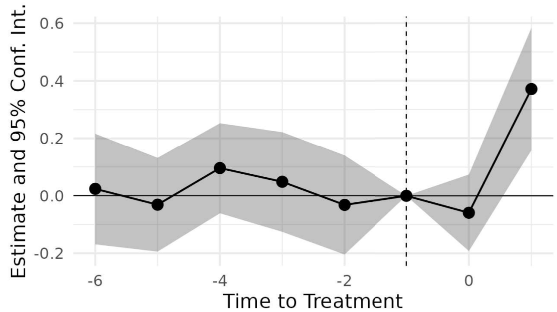
There is still a need for a permanent solution to this problem, which should include measures to reduce wildfire risk faced by households, both by adapting to wildfire risk and discouraging migration to high risk areas exacerbated by artificially low homeowner's rates. Regulating the industry in a way that allows firms to react to increased risk and earn reasonable profits can reduces the incentive for firms to retreat from the market and results in a functioning private market, a stated goal of the Department of Insurance.

2.8 Appendix B: Additional Figures

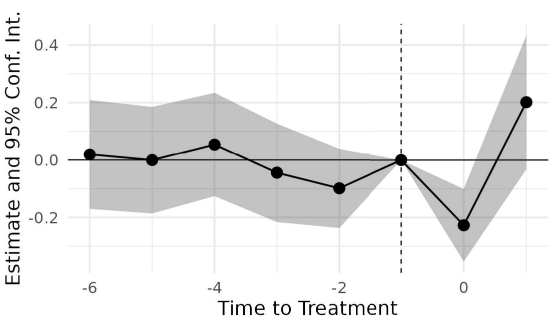
Figure 16: Effect of Moratorium on Company-Initiated Nonrenewals by RPS Quartile



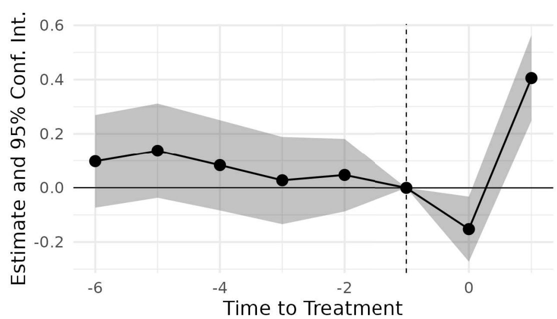
(a) 4th



(b) 3rd

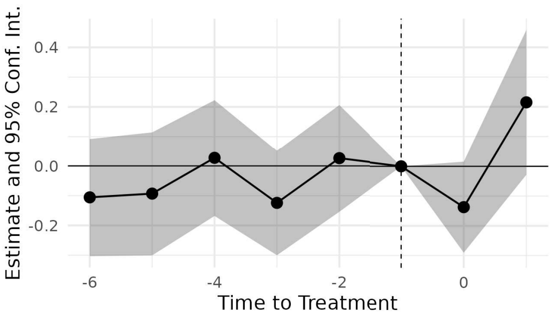


(c) 2nd

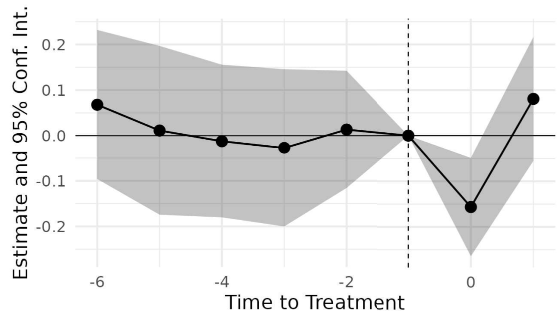


(d) 1st

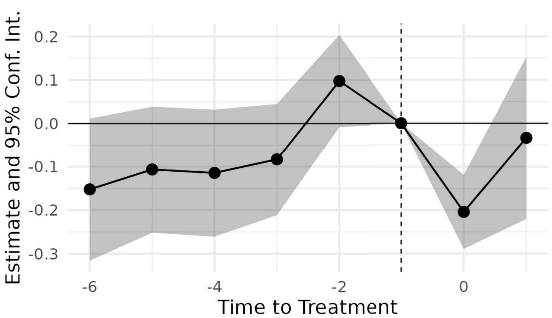
Figure 17: Effect of Moratorium on Company-Initiated Nonrenewals by Income Quartile



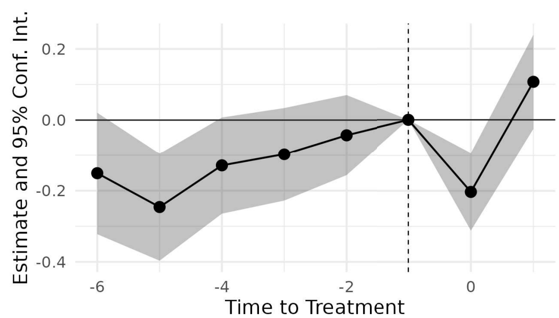
(a) 4th



(b) 3rd



(c) 2nd



(d) 1st

3 Water Markets and the Potential for Storage to Smooth Climate Risk

3.1 Main

Climate change will fundamentally alter California's already volatile agricultural water supplies. Scientists project that floods and droughts will become more frequent and extreme, and that year to year variability in precipitation will increase uncertainty in agricultural water availability (Kunkel et al., 2013; Swain et al., 2018; Water, 2020; CCA, 2024). In addition to increased uncertainty in precipitation, warming temperatures will directly affect the supply of and demand for water resources. Less precipitation will fall as snow; the snowpack will melt earlier; soils will become drier and evaporation will increase (Jessoe et al., 2018). Well-functioning water markets have the potential to mitigate the costs of climate change by transferring water from water rights holders to those with a higher economic value for water (Gonzales and Ajami, 2019; Anderson et al., 2019; Grafton et al., 2011; Chong and Sunding, 2006; Mendelsohn, 1994).

Formal water markets have been used to allocate water in a range of locations including California. A market for water describes the voluntary, compensated transfer of water across buyers and sellers in a decentralized system. Transfers have the potential to lead to efficiency gains by reallocating water from those who value it less to those who value it more (Howe et al., 1986; Colby, 1990; Mansur and Olmstead, 2012; Colby and Isaaks, 2018; Ferguson and Milgrom, 2023). One necessary condition for surface water markets is physical infrastructure that allows for the movement and conveyance of water across users. California has invested heavily in a vast and complex infrastructure made up of dams, canals, pipelines and rivers to transfer water from wet locations to relatively dry and high-demand urban and agricultural centers. Despite this network, water trading is underutilized relative to what economic theory predicts (Brewer et al., 2008; Donohew, 2008; Grafton et al., 2012; Regnacq et al., 2016; Colby and Isaaks, 2018). To date, the primary focus in the literature has been on spatial barriers that limit trading across geographies. Historically, transaction costs, regulatory hurdles and legal requirements have hampered water trades by making them economically infeasible (Hagerty, 2023; Ferguson and Milgrom, 2023; Leonard et al., 2019; Grafton et al., 2012; Loomis et al., 2003; Colby, 1990; Rafey, 2023; Payne and Smith, 2013; Satoh, 2015; Brennan, 2008; Brookshire et al., 2004).

While efforts have been made to improve water trading across space, market design has paid relatively less attention to the question of *when* scarce water resources should be allocated. In California, I hypothesize that much of the disparity in willingness to pay for water occurs over time. This is because volatility in precipitation occurs primarily over time, a phenomena that is only expected to become more severe with climate change (CCA, 2024). Large efficiency gains could be realized from banking water during times of abundance and borrowing it during times of drought (Brennan, 2008). Storing water from one year to the next can smooth water consumption and reduce the costs associated with droughts and floods (Ghosh, 2019; Ghosh et al., 2014; Gonzalez et al., 2020).

However, storage capacity constraints in California limit the amount of water that can be transferred from one year to the next. The Sierra snowpack operates as the state's largest reservoir but only offers storage from winter to spring. Reservoirs have historically provided the primary mode of inter-annual storage, but they also serve other, and sometimes competing, purposes including flood protection, power generation, environmental conservation, and recreation (Escriva-Bou et al., 2019). Climate change will further strain the storage capacity of existing reservoirs, as intertemporal water supplies become more variable (Ehsani et al., 2017). Underground aquifers offer an opportunity to expand surface water storage capacity. Water would enter aquifers through artificial recharge to be stored indefinitely and extracted at some point in the future (Zhang et al., 2020; Alam et al., 2020; Ulibarri et al., 2021).

This paper examines the effect of water storage constraints on price dynamics in California's surface and groundwater markets, using transactions level water transfer data from 2010 to 2022. Compared to past work, these data as described in the Methods section include recent historical droughts and deluges and are unique in that they distinguish between surface water and groundwater trades (Loomis et al., 2003; Brown, 2006; Brewer et al., 2008; Donohew, 2008). I begin by empirically characterizing recent market activity including trading volumes, trading locations, and the movement of water among various uses. Next, to gauge the effectiveness of these markets at redistributing water across locations, I evaluate spatial variability in water prices and between-region trading activity. I find substantial between-region trading activity in surface water markets suggesting some degree of spatial market integration, yet imperfectly correlated prices indicate the state surface water market is not efficient, supporting claims from earlier work (Hagerty, 2023; Colby, 1990).

I then assess temporal price variation in surface water and groundwater markets to determine how prices respond to precipitation and how storage impacts this response. I show that in markets with limited storage capacity, such as the surface water market, prices fluctuate over time and these movements are closely tied to precipitation shocks. Conversely, in markets with unconstrained storage capacity like groundwater markets, prices remain steady over time, unaffected by changes in precipitation. I explore depleted groundwater basins as a possible source for additional surface water storage. California groundwater basins can store at least 17 times as much water as all major California reservoirs combined (DWR, 2024), but are not widely used to store surface water. Conjunctive management of surface water and groundwater could stabilize surface water prices and improve economic welfare. The results from this study are a useful first step in valuing improved coordination of groundwater and surface water supplies, as a tool to reduce current costs in the allocation of water and climate change induced costs from increased variability in precipitation.

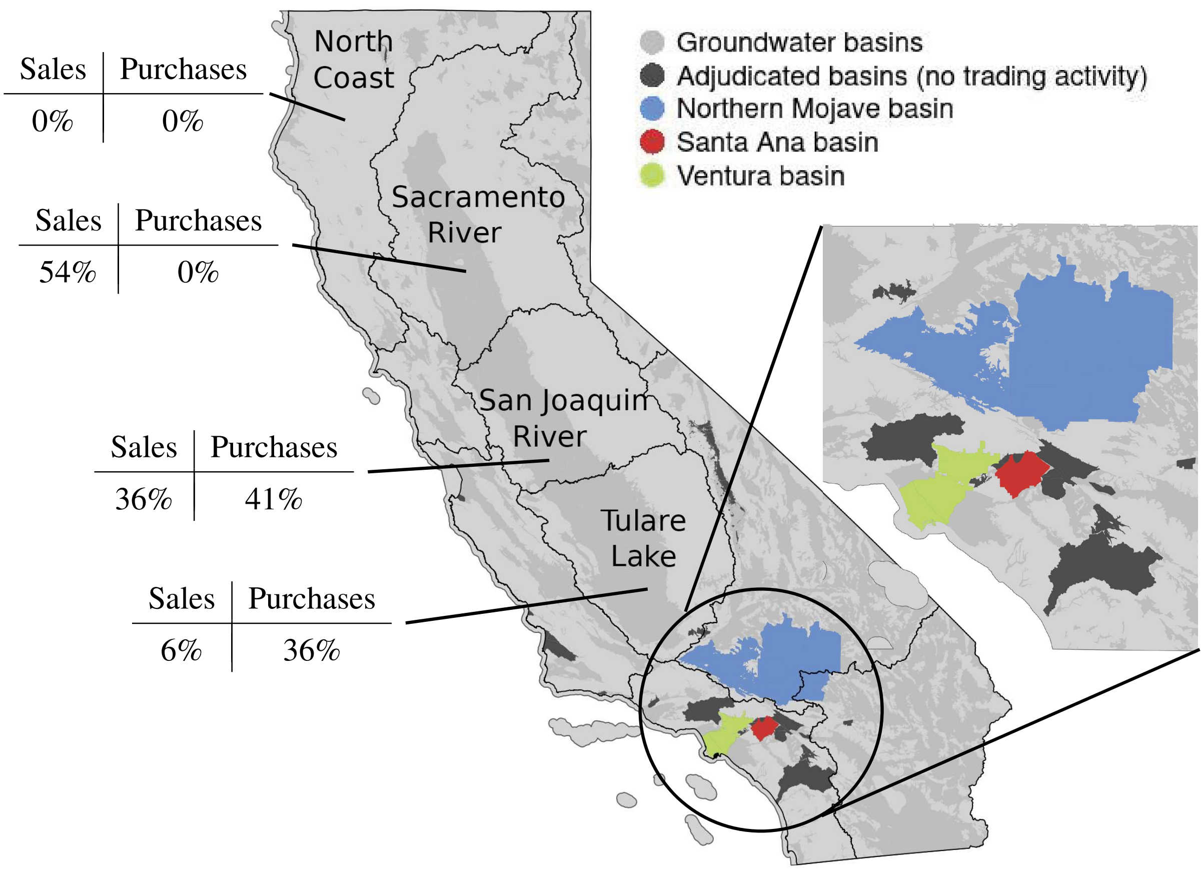
3.2 Trends in Market Activity

3.2.1 Surface Water Markets

Surface water trading is concentrated in four of California's nine hydrologic regions, shown in Figure 18: Tulare Lake, Sacramento River, San Joaquin River, and the North Coast. These four regions account for 89% and 81% of the volume of surface water sold and bought, respectively.

Sacramento River is the largest seller of water, accounting for 50% of the total volume sold and Tulare Lake is the largest buyer accounting for 42% of the total volume bought. As shown in Panel A of Table 14, surface water trading overwhelmingly involves agricultural water users, although some water is sold by municipal users and bought by environmental and municipal users.

Figure 18: California's Water Network



Notes: This map outlines California's nine hydrologic regions and groundwater basins. Hydrologic regions and basins with the most trading activity are labelled. Adjudicated areas that don't have significant trading activity are shown in dark gray. Percentages reflect the proportion of sales (purchases) with a buyer (seller) from a different region.

Table 14: Water Volume Transferred Between Uses

			Buyer Water Use				
			Agricultural	Environmental	Industrial	Municipal	**Total**
Seller Water Use	Agricultural	surface	64%	7.5%	0%	8.6%	**80.1%**
		ground	6.9%	0%	1.5%	12.1%	*20.4%*
	Environmental	surface	0.09%	0%	0%	0.7%	**0.8%**
		ground	0%	0%	0%	0%	*0%*
	Industrial	surface	0.01%	0%	0%	0.3%	**0.3%**
		ground	1.4%	0%	2.2%	8.2%	*11.8%*
	Municipal	surface	8.8%	0.6%	0%	9.8%	**19.2%**
		ground	2.3%	0%	5.7%	59.8%	*67.7%*
Total		surface	**72.9%**	**8.1%**	**0%**	**19.4%**	**100%**
		ground	*10.6%*	*0%*	*9.3%*	*80.1%*	*100%*

Note: This table shows the percent of trades that transfer water between uses for surface water and groundwater. The final column labelled 'Total' sums horizontally the percentages for surface water and groundwater separately, and represents the total percent of trades for each 'Seller Water Use' type. The final row labelled 'Total' does the same but vertically, and represents the total percent of trades for each 'Buyer Water Use' type. By construction the 'Total' column and rows each sum to 100% separately for surface water and groundwater.

Between 2010 and 2022, 1064 surface water trades occurred and accounted for 74% of the total volume of water traded in California.The distribution of trading volume is skewed right, with a few very large trades. The median transfer size is 887 acre-feet and the mean transfer size is 3177 acre-feet. In my sample, there are 206 sellers and 199 buyers, with the largest five sellers and buyers accounting for 25% and 35% of the total volume sold and bought. While this market is relatively concentrated, recent work suggests limited market power (Tomori et al., 2024).

3.2.2 Groundwater Markets

A necessary condition for the existence of a groundwater market are enforceable property rights, which, in California, are only established within adjudicated basins. Adjudicated basins are locations where courts have assigned property rights in response to disputes over legal access to the water. As of 2014, only 27 groundwater basins, mostly located in southern California, had been adjudicated. However, this may change with the implementation of California's Sustainable Groundwater Management Act (SGMA). This regulation requires all groundwater basins to meet sustainable groundwater targets by 2040, and adjudication may feature into many sustainability plans.

Figure 18 maps California's groundwater basins, adjudicated areas, and basins with significant trading activity. Three basins comprise the majority of groundwater trading with the Northern Mojave basin, Ventura-San Gabriel Coastal basin and Santa Ana basin making up 35%, 43% and 18% of groundwater trading, respectively. Panel B of Table 14 which summarizes the movement of water across uses highlights that majority of trading activity is across municipalities. Unlike surface water markets, environmental users do not participate.

Groundwater transactions are more frequent but smaller in trading volume than surface water transactions. From 2010 to 2022, 3165 groundwater trades occurred. Trading volume is right skewed, with a median trading volume of 73 AF and mean trading volume of 372. I observe 430 sellers and 250 buyers during this period, with the largest five sellers and buyers accounting fro 18% and 37% of sales and purchases, respectively. Recent work suggests that market power may be an issue in groundwater trading (Bruno and Sexton, 2020).

3.3 Spatial Integration and Efficiency

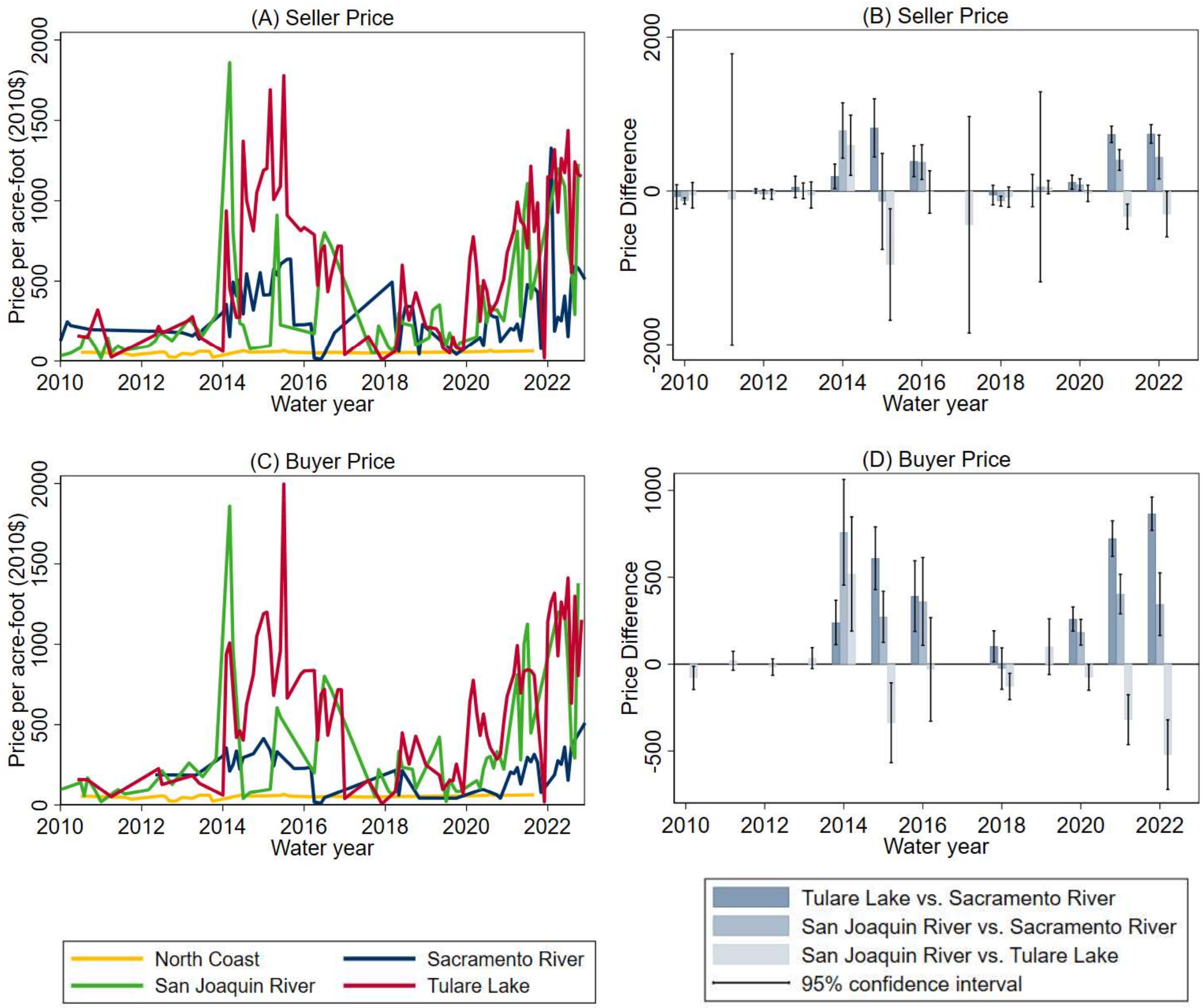
I examine the spatial functioning of water markets in California using the criteria of market integration and market efficiency. Integration describes the volume of trading activity across locations, and efficiency measures how well the market equates the marginal values for the water across locations Barrett (2001). In an efficient market with no frictions, a single market price will emerge and almost all transactions will occur at this price. However transaction costs, such as regulatory costs, can restrict trading and introduce distortions in prices across locations. I first assess inter-regional trading activity to measure the degree of market integration, and then check for spatial price differentials to analyze how efficiently this market reallocates water across space.

3.3.1 Surface Water Markets

In the surface water market, trading across hydrologic regions accounts for 55% of the volume transferred and 31% of transactions. There is no market integration in the North Coast, as all sales and purchases are within the region. The remaining regions with trading activity exhibit some degree of integration, as shown in Figure 18. These regions - Sacramento River, San Joaquin River, and Tulare Lake - share some common features. They span the hot, dry and agriculturally productive central valley of California, receive water from shared state and/or federal water projects, and have high water demand driven by irrigated agriculture. Water tends to flow from north to south, as water is sold by users in the Sacramento River region to buyers in the more southern San Joaquin or Tulare Lake regions.

Water prices trend similarly in the three integrated regions but are disconnected from the North Coast, where prices are low and stable. This is shown in Panels A and C of Figure 19 which plot volume-weighted average monthly prices sellers receive and buyers pay by hydrologic region. In the three connected regions, a simple comparison of annual means indicates that in some years water prices significantly differ across regions while in others I fail to detect a difference. Panels B and D of Figure 19 report the estimated difference in means with 95% confidence intervals for pairwise comparisons between regions. Spatial differences may arise because of regulatory constraints on trading or transactions costs in moving surface water. While the surface water market does redistribute water across space, price differences across hydrologic regions indicate that this market is not efficient (Hagerty, 2023; Colby, 1990).

Figure 19: Regional Surface Water Prices and Mean Comparisons



Notes: Panels A and C show the volume-weighted average seller and buyer price at the monthly level. Panels B and D plot the differences in means between regions with 95% confidence intervals for each water year (October to September).

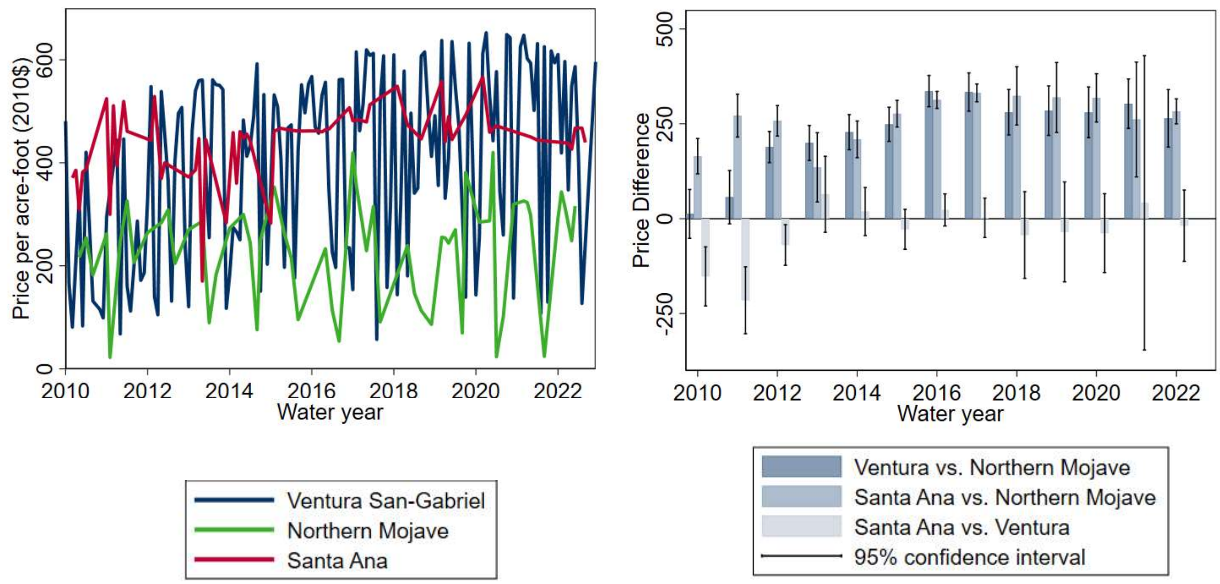
3.3.2 Groundwater Markets

In my data, groundwater trading activity appears to be limited to within basin trades. This indicates that markets in my data are not integrated, though outside of my sample across basin groundwater trading may be occurring.

As shown in Figure 20, I observe systematic spatial differences in groundwater prices that persist over time. This figure plots volume-weighted average monthly groundwater prices in the three basins with the most trading activity: the Northern Mojave, Santa Ana, and Ventura San-

Gabriel Coastal basins. Mean comparisons of annual price, shown in the right panel, indicate water is valued similarly in the Santa Ana and Ventura San-Gabriel Coastal basins, but less in the Northern Mojave basin. This indicates there could be gains to transferring water across basins.

Figure 20: Groundwater Prices and Mean Comparisons



Notes: The left panel shows the volume-weighted average groundwater price at the monthly level. The right panel plot the differences in means between basins with 95% confidence intervals for each water year (October to September).

3.4 Temporal efficiency

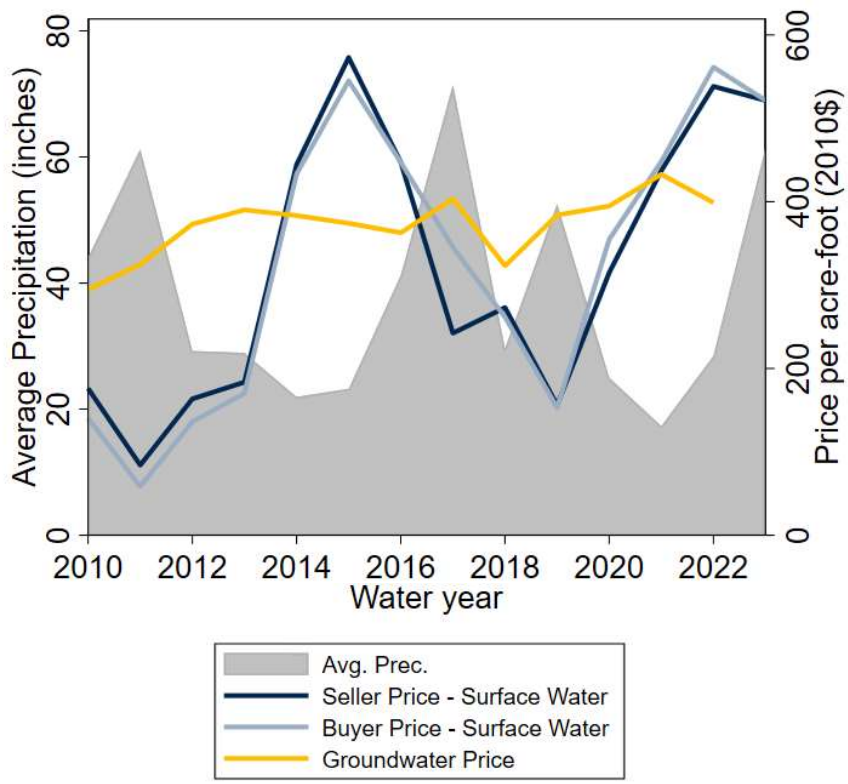
To analyze the extent to which markets allow users to smooth water consumption over time, I document how surface water and groundwater prices change over time. One indication that markets may not be efficient is if the marginal value of water, which is reflected in prices, changes between years. I then test the extent to which precipitation shocks drive this price volatility.

Missing from this analysis is an examination of market integration. In my sample, 99.5% of trades occur within a single year. While banking and borrowing is becoming more common in California's water market, qualitative evidence indicates that historically temporal trading accounts for a small share of trading activity (PPI, 2021).

3.4.1 Surface Water Markets

Figure 21 shows the volume-weighted surface water price buyers pay and sellers receive in each water year from 2010-2023, defined as October to September. Annual surface water prices vary widely; in 2015 the buyer price was 9 times higher than in 2011.

Figure 21: Water prices and precipitation over time



A primary factor explaining these fluctuations in price is precipitation, which is negatively correlated with prices as illustrated in Figure 21. Precipitation alone explains 22% of the variability in surface water prices and a causal examination, as described in the Methods section, reveals that a one inch increase in average precipitation causes prices to increase by \$9.54-\$11.18, as shown in columns (1) and (4) of Table 15. This is consistent with past work that finds water prices are higher during droughts (Pullen and Colby, 2008; Ghosh, 2019).

This precipitation-driven price volatility suggests the surface water market is unable to redistribute water over time and mitigate the effects of increasingly volatile surface water supplies.

Table 15: Price Response to Precipitation in the Surface Water Market

		Transaction Price in Seller Region			*Transaction Price in Buyer Region*		
		(1)	(2)	(3)	(4)	(5)	(6)
Precipitation in Seller Region	Overall effect	-9.54***		-25.53***			
		(2.80)		(6.99)			
	Sacramento River		-1.92				
			(2.13)				
	San Joaquin River		-10.34***				
			(3.84)				
	Tulare Lake		-21.21***				
			(5.99)				
	Precipitation X Capacity			1.71***			
				(0.62)			
Precipitation in Buyer Region	Overall effect				-11.18***		-20.28***
					(3.30)		(5.69)
	Sacramento River					1.61	
						(3.09)	
	San Joaquin River					-8.82**	
						(3.91)	
	Tulare Lake					-16.98***	
						(4.79)	
	Precipitation X Capacity						1.43**
							(0.56)
Controls	Seller Region	Yes	Yes	Yes			
	Buyer Region				Yes	Yes	Yes
	Seller Water Use	Yes	Yes	Yes	Yes	Yes	Yes
	Buyer Water Use	Yes	Yes	Yes	Yes	Yes	Yes
	Type of Water Right	Yes	Yes	Yes	Yes	Yes	Yes
	Observations	887	887	887	887	887	887
	R^2	0.25	0.29	0.29	0.23	0.26	0.26

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors are clustered by region and water year.

3.4.2 Groundwater Markets

Compared with surface water prices, groundwater prices are stable over time, as illustrated by Figure 21. From 2010-2022, the highest average price is only 47% larger than the lowest average price. Further, groundwater prices do not depend on changes in precipitation. I fail to detect any economically or statistically significant impact of precipitation on prices, show in Table 16. This is despite groundwater basins being susceptible to both aggregate and local variations in precipitation; natural recharge occurs as precipitations falls, and in regions where trading happens some surface water is imported each year to artificially recharge aquifers.

The resilience of prices to precipitation shocks indicates that this market efficiently distributes water over time, storing it during wet years for use during dry years.

Table 16: Price Response to Precipitation in the Groundwater Market

	Transaction Price	
	(1)	(2)
Precipitation	-0.17	
	(0.21)	
Precipitation		-0.42**
Northern Mojave		(0.19)
Precipitation		0.57
Santa Ana		(0.53)
Precipitation		0.19
Ventura San-Gabriel Coastal		(0.44)
Basin	Yes	Yes
Seller Water Use	Yes	Yes
Buyer Water Use	Yes	Yes
Subbasin	Yes	Yes
Observations	2,956	2,956
R^2	0.95	0.95

Note: *$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$*
Standard errors are clustered by basin and water year.

4 Storage to smooth markets

I reconcile the differences in price response to precipitation between surface water and groundwater markets by considering variations in their respective storage capacities. Storage is limited for surface water; total reservoir capacity is approximately equal to one's years supply for cities and farms (PPI, 2018). However, competing uses restrict the ability to utilize this capacity to transfer water from one year to the next (Escriva-Bou et al., 2019), and I find prices respond strongly to precipitation shocks. Conversely, groundwater storage capacity is vast; the state's usable groundwater storage is approximately 8-12 times larger than the combined surface water reservoir capacity DWR (2021). I find that prices do not respond to precipitation shocks in the groundwater market. More storage capacity reduces the exposure of water users to precipitation shocks by decoupling the quantity of water supplied each year from precipitation in that year.

I further explore the question of how storage capacity impacts exposure to precipitation shocks by comparing surface water price dynamics in regions with differing storage capacities; the Sacramento River region has 13 million acre-feet of storage capacity, the San Joaquin River region has 10 million acre-feet of storage capacity, and the Tulare Lake region has 2 million acre-feet of storage capacity (CDE, 2024). Results are reported in columns (2) and (5) or Table 15. Prices in the San Joaquin River and Tulare Lake regions are responsive to changes in precipitation, increasing by \$8.82-\$10.34 and \$16.98-\$21.21 in response to a one inch decrease in average precipitation, respectively. Prices in the Sacramento River region unresponsive to changes in precipitation; estimates are small and I cannot statistically differentiate them from zero. The Sacramento River region has 30% more reservoir capacity than the San Joaquin River region and seven times as much reservoir capacity as Tulare Lake region. The higher storage levels could play a role in the stability of prices through precipitation shocks.

I explicitly estimate the impact of storage capacity on price responsiveness to precipitation shocks by interacting precipitation with storage capacity in columns (3) and (6) of Table 15. Each million acre-feet of storage capacity tempers the price response to one additional inch of precipitation by \$1.43-\$1.71.

This finding is consistent with results from other commodity markets. When aggregate grain stocks are low, prices are highly sensitive to small supply and demand shocks, consistent with predictions from storage models (Wright, 2012). This plays out in periodic agricultural commodity booms and busts, characterized by broad and sharp movement of commodity prices in the same direction. There is no simple explanation of what causes these events, but they tend to coincide with low inventory and unpredictable changes in supply or demand (Carter et al., 2011). For example, from 2006-2008, world cereal prices increased by 92% and vegetable oil prices doubled (FAO, 2024). At the same time, the U.S. implemented the biofuel mandate which greatly increased the demand for agricultural commodities and global inventories declined (Timmer, 2010). Lower stock-to-use ratios (the ratio of inventory levels to consumption) are an indication of vulnerability to large price spikes even when the current price shows no cause for concern (Bobenrieth et al., 2013).

Improving California's surface water storage could help stabilize prices through increasingly large precipitation shocks driven by climate change. California can increase storage capacity by building new reservoirs, raising dams, removing sediment from existing reservoirs, and lowering water intakes. But, these options come with large fiscal, environmental, and social costs (McCartney, 2009; Ansar et al., 2014), and may only improve storage capacity a marginal amount, and this improvement may be temporary.

Integrating groundwater and surface water markets could provide a solution to temporal price volatility in surface water markets. Using artificial recharge to enhance the natural replenishment of groundwater resources can link surface water supplies with groundwater storage and allow water users to bank and borrow water over time thus reducing the costs of water-related disasters such as droughts. Economic research shows benefits to groundwater banking (Ghosh et al., 2014; Gonzalez et al., 2020; Karimov et al., 2010; Montilla-López et al., 2016), and in some places in California it is already happening. For example, the Kern County Water Bank, established October 1995, can store approximately 1.5 million acre-feet of water. Access to this water bank reduced the risk of prolonged drought and caused farmers to plant more high-value perennial crops rather than low-value annual crops (Arellano-Gonzalez and Moore, 2020).

The main challenge to extending groundwater banking in California revolves around water rights; banking and borrowing can only work if surface right holders maintain their ownership of water stored underground. The current absence of property rights in most of California's groundwater basins makes this impossible. In addition, many surface water rights are structured as 'use it or lose it,' and the choice to store water underground could result in a permanent loss of that water right. Establishing and designing a property rights system that allows for water banking is a critical first step to it's expansion in California.

The idea of improving water storage by using groundwater basins is not new in California. In March 2023 Governor Newsom issued an executive order to temporarily enable local water agencies and other water users to capture water from the latest round of storms to recharge state groundwater supplies without permits. While this is a good start, a more permanent and comprehensive policy is required.

4.1 Methods

4.1.1 Data

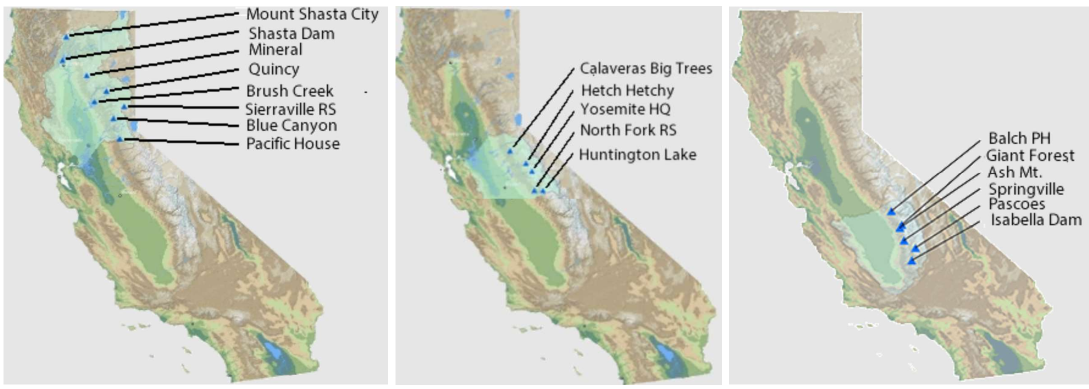
Water Trades

I use proprietary, transactions level data for water trades from 2010-2022 provided by West Water. These data includes the quantity traded, price, type of buyer and seller, whether the trade is temporary or permanent, location, and whether the trade occurred in the surface water or groundwater market. Additionally, surface water trades provide the location of the buyer and seller. These data do not represent the universe of water transfers, but are the best available at the transaction level. West Water collects and verifies data by contacting and visiting market participants. I limit my sample to transactions that are one-year leases (representing a temporary transfer of the right to extract water for one year) because these reflect current market conditions rather than expectations of future market conditions. These one-year leases make up 95% of the transactions in my dataset. I also deflate all prices to 2010$ using the consumer price index (CPI) from the U.S. Bureau of Labor Statistics. The CPI measures the price for all items in U.S. city average, all urban consumers, not seasonally adjusted.

Precipitation

Precipitation data comes from the California Data Exchange Center, measured in inches at monitoring stations throughout the state. Figure 22 shows the locations of stations used to measure precipitation in the Sacramento River, San Joaquin River, and Tulare Lake regions.

Figure 22: Stations used to measure precipitation

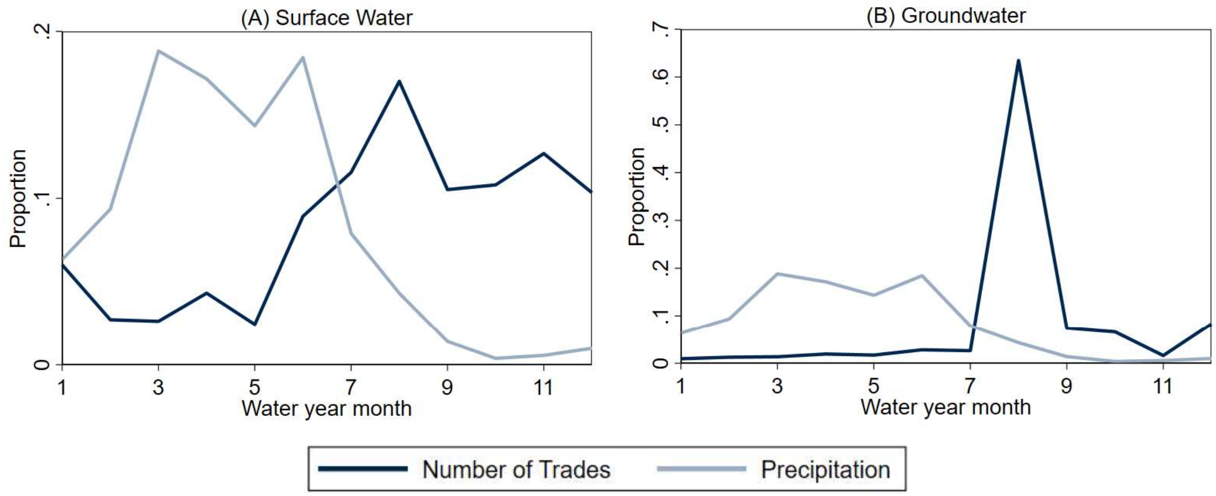


(a) Sacramento River (b) San Joaquin River (c) Tulare Lake

Source: California Data Exchange Center- Precipitation

I measure precipitation as the total amount accumulated during the water year in which the trade occurred. The distribution of precipitation and trading throughout the water year is shown in Figure 23. The bulk of annual precipitation happens from December to March, and although it is not uncommon for some precipitation to happen after this, it is uncommon for this precipitation to substantially impact water supplies for that year. Most water trading happens after this so for most transactions, market participants will have full information about water supplies for the current water year. I allow precipitation to vary by hydrologic region for surface water trades, but hold it constant over space for groundwater trades.

Figure 23: Distribution of precipitation and water trades by month



Notes: This figures plots the proportion of trades and precipitation for each water year month. Month 1 corresponds to October, the first month of the water year, and month 12 corresponds to September, the last month of the water year.

4.1.2 Analysis

Spatial Efficiency: Comparison of Means

I calculate the difference in mean water price between locations k and l in water year y with 95% confidence intervals according to,

$$\mu_{k,y} - \mu_{l,y} \pm t_{df,0.025}\sqrt{\frac{s_{k,y}}{n_{k,y}} + \frac{s_{l,y}}{n_{l,y}}}, \tag{11}$$

where, $\mu_{\{l,k\},y}$ is the volume-weighted mean water price, $s_{\{l,k\},y}$ is the standard deviation of water price, $n_{\{l,k\},y}$ is the number of transactions, and $t_{df,0.025}$ is the critical value from the Student T distribution with degrees of freedom $df = min\{n_{k-1,y}, n_{l-1,y}\}$ and size 0.025. Locations l and k are hydrologic regions in the surface water market and basins in the groundwater market. I use a Student-T distribution to accommodate smaller sample sizes, which range from 2 to 181.

Temporal Efficiency: Price Response to Precipitation

I estimate the impact of precipitation on water prices using an ordinary least squares regression model. In the surface water market I estimate,

$$P_{i,j,y} = \alpha_j + \gamma R_{j,y} + \mathbf{X}\beta + \varepsilon_{i,j,y}, \tag{12}$$

where, $P_{i,j,t}$ is the price for transaction i in location j and water year y, α_j are location fixed effects, $R_{j,y}$ is precipitation in inches in location j and water year y, $\mathbf{X}$ is a vector of control variables with coefficients β, and $\varepsilon_{i,j,y}$ are errors. Control variables include seller and buyer water use fixed effects, type of water right for surface water trades, and subbasin for groundwater trades. γ is interpreted as the change in water price induced by a one inch increase in average precipitation. Results from estimating this equation are shown in columns (1) and (4) of Table 15 and column (1) of Table 16.

In the surface water market, I provide two sets of results. The first uses precipitation and prices in the seller region (columns (1)-(3) of Table 15) and the second uses precipitation and prices in the buyer region (columns (4)-(6) of Table 15). In all specifications I use heteroscedastic robust standard errors clustered at the location and water year level. I also weight by the natural logarithm of volume transferred. This allows trades that move more water to have a larger impact on the estimated coefficients, but does not allow a small number of very large trades to overwhelm the estimation.

I further decompose the relationship differentiate the impacts of precipitation on prices by location,

$$P_{i,j,y} = \alpha_j + \sum_j \gamma_j R_{j,y} + \mathbf{X}\beta + \varepsilon_{i,j,y}, \tag{13}$$

where all variables are consistent with equation 12 except γ_j varies by location. Here, γ_j is interpreted as the price response in location j induced by a one inch increase in precipitation in location j. Results from estimating this equation are shown in columns (2) and (5) of Table 15 and column (2) of Table 16.

Finally, I estimate the impact of storage on the response to precipitation in the surface water market by including an interaction term between precipitation and storage capacity,

$$P_{i,j,y} = \alpha_j + \gamma R_{j,y} + \delta S_j R_{j,y} + \mathbf{X}\beta + \varepsilon_{i,j,y}. \tag{14}$$

Here, all variables are consistent with equation 12. S_j is the total storage capacity in hydrologic region j. δ is the change in the surface water price response associated with one million acre-feet of surface water storage capacity. The price response to one additional inch of precipitation is, $\gamma + \delta S_j$.

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