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Essays in Environmental Economics: Exploring the Effects of Extreme Events

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy
in Economics

by

Victoria Lea Wang

2025

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ABSTRACT OF THE DISSERTATION

Essays in Environmental Economics: Exploring the Effects of Extreme Events

by

Victoria Lea Wang

Doctor of Philosophy in Economics

University of California, Los Angeles, 2025

Professor Adriana Lleras-Muney, Chair

Wildfires are destructive and are expected to become more costly and affect more people in the future. This dissertation investigates how wildfires impact people's migration decisions, the financial consequences of these disasters, and the impact of government aid in three chapters. The first chapter evaluates adaptive migration – the movement from risky to safe areas – as a potential adaptation strategy. Granular, address-level credit and migration data are combined with wildfire and risk measures to trace the locations of residents over time, before and after the fire. The empirical strategy leverages the quasi-random trajectory of the wildfire. The results indicate that people living in blocks that burned for the first time are 6.5 p.p. (18.5%) more likely to remain away, even after four years. Although migration initially leads to safe tracts after two years, this effect vanishes by the fourth year. When focusing on financially unconstrained subgroups, people are statistically significantly more likely to be in safe areas, suggesting that financial constraints may prevent adaptive migration.

The second chapter assesses how experiencing a megafire – a fire that burns over 100,000 acres – affects an individual's household finances and neighborhood decision. There is evidence of financial distress: Credit scores fall and active debt collections rise. Moreover, the likelihood of having a mortgage drops by 5.3 points (19.7 percent) at two years and

remains 3.0 points (10.6 percent) lower at four years. Examining the characteristics of their destinations, the impacted individuals settle in areas with higher amenities, although they are also more expensive.

The third chapter evaluates the Federal Emergency Management Agency (FEMA)'s Individuals and Households Program (IHP). Using political competitiveness as an instrument for aid receipt, I find that IHP increases migration by 33.0 p.p. yet does not improve migration to safe areas, unsurprising given its non-targeted design. Aid recipients experience substantially less financial distress and greater access to credit, underscoring IHP's role in alleviating immediate hardship even though it falls short of promoting adaptive migration. Together, these findings highlight financial barriers to climate-driven adaptation and suggest policy changes to align disaster relief with long-term resilience.

The dissertation of Victoria Lea Wang is approved.

Martha Jane Bailey

Daniel Haanwinckel Junqueira

Gregor Schubert

Adriana Lleras-Muney, Committee Chair

University of California, Los Angeles

2025

Contents

1	Migration as Climate Adaptation: Evidence from California Wildfires	6
1	Introduction	6
2	Background	12
2.1	California Wildfires	12
2.2	Wildfires and Migration Conceptual Framework: Preferences vs. Constraints	14
3	Data	16
3.1	Fire	16
3.2	Wildfire Risk	17
3.3	Migration	17
4	Empirical Strategy	19
4.1	Stacked Difference-in-Differences Event Study	19
4.2	Definitions of Terms	22
4.3	Identification Assumptions	23
4.4	Robustness: Alternative Controls and Empirical Specifications	23
5	Migration Results	25

5.1	Preliminary Results	26
5.2	Event Study Results for Migration	28
5.3	Event Study Results for Adaptive Migration	30
5.4	Heterogeneity Analyses	32
6	Discussion	40
7	Figures	43
8	Tables	48
2	Sustained Burn: Analyzing Post-Wildfire Financial and Location Outcomes	50
1	Introduction	50
2	Data	53
2.1	Fire	53
2.2	Migration and Financial Characteristics	54
2.3	Location Data	54
3	Empirical Strategy	56
3.1	Stacked Difference-in-Differences Event Study	56
3.2	Identification Assumptions	58
4	Financial Outcomes	59
5	Location Results	62
5.1	Amenities for Children	62
5.2	Labor Market	63
5.3	Pollution	64
5.4	Housing	65

6	Conclusion	67
7	Figures	70
7.1	Financial Outcomes	70
7.2	Location Results	75
3	Soothing the Burn: The Impact of Government Aid on Weathering Wild-	
	fires	83
1	Introduction	83
2	Background	87
2.1	Government Aid	87
2.2	Wildfires and Migration Model: How Government Aid Can Impact Individuals	90
3	Data	96
3.1	Fire	96
3.2	Wildfire Risk	96
3.3	Migration	97
3.4	Government Aid	98
4	Empirical Strategy	98
4.1	Preliminary Analysis: Stacked Difference-in-Differences Event Study .	99
4.2	Definitions of Terms	100
4.3	Causal Impact of Aid	101
5	Government Aid Results	107
5.1	Preliminary Analysis Results: Government Aid vs. No Government Aid	107

5.2	Causal Impact of Aid	108
6	Conclusion	110
7	Figures	114
8	Tables	116
4	Appendix and Supplementary Material	122
1	Appendix to “Migration as Climate Adaptation: Evidence from California Wildfires”	124
1.1	Additional Figures	124
1.2	Additional Tables	149
1.3	California Background	154
2	Appendix to “Sustained Burn: Analyzing Post-Wildfire Financial and Location Outcomes”	157
2.1	Financial Outcomes	157
2.2	Amenities for Children	174
2.3	Labor Market	178
2.4	Pollution	186
2.5	Housing	193
3	Appendix to “Soothing the Burn: The Impact of Government Aid on Weath- ering Wildfires”	202
3.1	Additional Figures	202
3.2	Additional Tables	210

List of Figures

1.1	Number of California Fires and Megafires over Time	43
1.2	California Wildfires Burn Areas	44
1.3	Migration Averages for First vs. Never Burned	45
1.4	Moved Away from Tract after First Fire	46
1.5	Wildfire Risk after First Fire	47
2.1	Credit Scores after Megafire	70
2.2	Number of Open Loans after Megafire	71
2.3	Number of Open Mortgage Loans after Megafire	72
2.4	Percent Utilization of Credit Cards after Megafire	73
2.5	Number of Active Collections after Megafire	74
2.6	Upward Mobility after Megafire	75
2.7	Average Test Scores after Megafire	76
2.8	Moved Away from Commuting Zone after Megafire	77
2.9	Median Household Income after Megafire	78
2.10	Pollution Burden after Megafire	79
2.11	Median Housing Value after Megafire	80

2.12	Mortgage after Megafire	81
2.13	Zillow Observed Rent Index after Megafire	82
3.1	Moved Away from Tract after First Fire with Aid	114
3.2	Wildfire Risk after First Fire with Aid	115
4.1	FEMA Wildfire Score Distribution	124
4.2	Map Example of Treatment vs. Control Groups	125
4.3	Average Wildfire Risk for First vs. Never Burned	126
4.4	Moved Away from Tract across Control Groups	127
4.5	Moved Away from Tract across Empirical Specifications	128
4.6	Wildfire Risk across Control Groups	129
4.7	Wildfire Risk across Empirical Specifications	130
4.8	Average Annual Migration Rate for First vs. Never Burned	131
4.9	Annual Migration Rate after First Fire	132
4.10	Migration to Different Wildfire Risk Scores	133
4.11	Counterfactual Moves	134
4.12	UCCCP and Infutor Migration Rates after First Fire	135
4.13	UCCCP and Infutor Wildfire Risk after First Fire	136
4.14	Migration of Damaged vs. Not Damaged Addresses after First Fire	137
4.15	Wildfire Risk of Damaged vs. Not Damaged Addresses after First Fire	138
4.16	Migration of Damaged vs. Not Damaged Addresses after First Fire across Not Damaged Definitions	139

4.17 Wildfire Risk of Damaged vs. Not Damaged Addresses after First Fire across Not Damaged Definitions	140
4.18 Moved Away from Tract after First Fire by Credit Score	141
4.19 Wildfire Risk after First Fire by Credit Score	142
4.20 Moved Away from Tract after First Fire by Education	143
4.21 Wildfire Risk after First Fire by Education	144
4.22 Moved Away from Tract after First Fire by Age	145
4.23 Wildfire Risk after First Fire by Age	146
4.24 Moved Away from Tract after First Fire by Homeownership	147
4.25 Wildfire Risk after First Fire by Homeownership	148
4.26 Credit Scores after First Fire	157
4.27 Number of Open Loans after First Fire	158
4.28 Number of Open Mortgage Loans after First Fire	159
4.29 Number of Open Auto Loans after Megafire	160
4.30 Number of Open Auto Loans after First Fire	161
4.31 Number of Open Student Loans after Megafire	162
4.32 Number of Open Student Loans after First Fire	163
4.33 Number of Open Credit Cards after Megafire	164
4.34 Number of Open Credit Cards after First Fire	165
4.35 Percent Utilization of Credit Cards after First Fire	166
4.36 Number of Credit Card Delinquencies after Megafire	167
4.37 Number of Credit Card Delinquencies after First Fire	168

4.38	Total Credit Card Balance after Megafire	169
4.39	Total Credit Card Balance after First Fire	170
4.40	Total Credit Card Limit after Megafire	171
4.41	Total Credit Card Limit after First Fire	172
4.42	Number of Active Collections after First Fire	173
4.43	Upward Mobility after First Fire	174
4.44	Average Test Scores after First Fire	175
4.45	College Educated Share after Megafire	176
4.46	College Educated Share after First Fire	177
4.47	Moved Away from Commuting Zone after First Fire	178
4.48	Moved Within Commuting Zone after Megafire	179
4.49	Moved Within Commuting Zone after First Fire	180
4.50	Median Household Income after First Fire	181
4.51	Number of Jobs after Megafire	182
4.52	Number of Jobs after First Fire	183
4.53	Employment Share after Megafire	184
4.54	Employment Share after First Fire	185
4.55	Pollution Burden after First Fire	186
4.56	Traffic after Megafire	187
4.57	Traffic after First Fire	188
4.58	PM2.5 after Megafire	189
4.59	PM2.5 after First Fire	190

4.60 Asthma after Megafire	191
4.61 Asthma after First Fire	192
4.62 Median Housing Value after First Fire	193
4.63 Zillow Home Value Index after Megafire	194
4.64 Zillow Home Value Index after First Fire	195
4.65 Mortgage after First Fire	196
4.66 Rent after Megafire	197
4.67 Zillow Observed Rent Index after First Fire	198
4.68 Rent after First Fire	199
4.69 Housing Burden after Megafire	200
4.70 Housing Burden after First Fire	201
4.71 FEMA Wildfire Score Distribution	202
4.72 Wildfire Risk after First Fire with Aid vs. Counterfactual Moves	203
4.73 Moved Away from Tract after First Fire with Aid by Credit Score	204
4.74 Wildfire Risk after First Fire with Aid by Credit Score	205
4.75 Moved Away from Tract for High Credit by Aid	206
4.76 Wildfire Risk for High Credit by Aid	207
4.77 Moved Away from Tract for Low Credit by Aid	208
4.78 Wildfire Risk for Low Credit by Aid	209

List of Tables

1.1	First vs. Never Burned Summary Statistics	48
1.2	Percentage of Population That Moves by Fire Type	49
3.1	Politically Competitive vs. Politically Noncompetitive County Summary Statistics: 2000-2021	116
3.2	First Stage: Politically Competitive 2014-2021	117
3.3	Exclusion Restriction: Politically Competitive Counties and Spending	118
3.4	DID and DID-IV Results: Adaptive Migration	119
3.5	DID and DID-IV Results: Financial Distress Index	120
3.6	DID and DID-IV Results: Credit Access Index	121
4.1	Tracts with Higher Wildfire Risk Have Lower House Prices	149
4.2	Movers vs. Non-Movers Summary Statistics	150
4.3	Return Movers vs. Non-Return Movers Summary Statistics	151
4.4	Characteristics Associated with Migration for Individuals in First- and Never- Burned Blocks	152
4.5	Predicted Migration Rates for Individuals in First-Burn vs. Control Blocks	153
4.6	First Stage: Politically Competitiveness (5 – 15%) 2014-2021	210

4.7	Exclusion Restriction: Politically Competitive Counties and Climate Attitudes	211
4.8	DID-IV Results: Political Competitiveness ($< 5\%$) with County Controls . .	212
4.9	DID and DID-IV Results: Borrowing Index	213

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VITA
Victoria Lea Wang

EDUCATION

UNIVERSITY OF CALIFORNIA, LOS ANGELES

M.A. in Economics 2019-2021

UNIVERSITY OF CHICAGO

M.A. in Social Sciences, Concentration: Economics 2016-2017

RICE UNIVERSITY

B.A. in Mathematical Economic Analysis, Mathematics, Asian Studies, and Policy Studies,
Minor in Poverty, Justice, and Human Capabilities *Cum Laude* 2012-2016

RESEARCH IN PROGRESS

“Adaptive Migration in the Face of Wildfires: Financial Constraints and the Role of Government Aid”

“Burning Inequities: Wildfires, Social Safety Net, and Exacerbating Inequality”

“Post-Fire Housing Market: Evidence from Listings Data”

“Climate Refugees and Housing Prices: Migration-Induced Demand Shocks” “The Impact of Parenthood on the Financial Well-Being of Families”

RELEVANT POSITIONS

UNIVERSITY OF CALIFORNIA LOS ANGELES

Student Affiliate, California Center for Population Research (CCPR) 2020-2025

Graduate Student Researcher, California Policy Lab (CPL) 2020-2023

Graduate Student Researcher, WORLD Policy Center 2020

JOHN JAY RESEARCH AND EVALUATION CENTER

Graduate Research Assistant 2020

NERA ECONOMIC CONSULTING

Analyst 2017-2019

FELLOWSHIPS, HONORS, AND AWARDS

John Clapp Best Poster Award, AREUEA. 2025

Honorable Mention for Best Student Paper, Urban Economics Association 2025

Dissertation Research Grant, Russell Sage Foundation. 2024-2025

Graduate Fellowship, California Policy Lab. 2024

Honorable Mention for Best Poster, UC Demography Conference. 2024

Outstanding Teaching Award, UCLA. 2023

Seed Grant Fellowship, California Policy Lab. 2023

AEA Dissertation Fellowship, Philadelphia Federal Reserve Bank. 2023

Graduate Research Mentorship Fellowship, UCLA. 2023

Humane Studies Fellowship, Institute for Humane Studies. 2020-2025

Research Grant, UCLA Ziman Center for Real Estate. 2020-2023

Tuition Scholarship, University of Chicago. 2016-2017

Max Roy Scholarship, Rice University. 2012-2016

Silver Scholar, Los Alamos National Laboratory Foundation. 2012-2016

PRESENTATIONS

USC Lusk Center for Real Estate, Urban Economics Association, Population Association of America, UCI-UCLA-USC Urban Symposium, Econometric Society World Congress 2025

Population Association of America, Urban Economics Association, UC Demography Conference, All-California Labor Economics Conference, Occidental College, UCLA Ziman Center for Real Estate 2024

Introduction

Natural disasters are expected to impact a large and increasing number of people with climate change. These destructive events are expensive and can have lasting impacts beyond immediate physical damages. The effects of natural disasters can be mitigated through adaptation strategies. In *Migration as Climate Adaptation: Evidence from California Wildfires*, I investigate one of such methods: adaptive migration. Specifically, I study after an area experiences a wildfire, the extent to which residents move and stay away, and the wildfire risk of their new location. To accomplish this, I combine geographically detailed and granular credit data and address-level migration data with wildfire records and risk measures.

Migration is theorized to be an important way to reduce the costs associated with climate change due to the spatial nature of disasters. Wildfires spread where there is fuel and the geography of the area can predict risk. Thus, moving people away from risky areas can theoretically be an effective way to minimize costs. In order for people to move to safe places, they need to both want to and be able to move there. Oftentimes, the very amenities that draw people to live more remotely – being close to nature and the view – are correlated with wildfire risk. Moreover, areas that are safer tend to be more expensive. Thus, differentiating between whether preferences or financial constraints attract people to live in risky areas is key for evaluating adaptation strategies.

Measuring the migration responses of a fire requires being able to follow an individual over time and identifying who is impacted by a fire. I use census block-level credit data from

a sample of everyone who has ever lived in California. My empirical strategy leverages the fact that the exact trajectory of a wildfire is essentially random, and I compare people who are in blocks that burned for the first time with those in blocks that never burned, within the same census tract. I find that the fire increases people's out-migration probability: they are 10.6 percentage points (p.p.) more likely to be in a different census tract the year after, a 79.4% increase. They are 6.5 p.p. (18.5%) more likely to stay away, even four years after the fire. This migration is not adaptive in the longer term: although they are more likely to be in low or medium wildfire risk areas after two years, the effect dissipates to be statistically insignificant by the fourth year. Thus, although the wildfire forces people to move, people are not moving to safe areas.

Financial constraints seem to restrict people's ability to move adaptively. To investigate this, I conduct the same migration analysis for individuals who are less likely to be constrained: people who have high credit scores and people who are likely to receive post-fire payment (i.e., insurance payments for a damaged home). For these groups, I find that they are more likely to be in safe areas in the longer term. Assuming that they are financially able to act according to their preferences after the fire, it seems that financial constraints are one barrier that prevents people from moving adaptively.

Wildfires can push people to move and force them to deliberately choose where they want to live. Moreover, the individual may experience a substantial financial loss as she navigates evacuations, stops in employment, and property damages. However, disasters can unexpectedly have positive effects as people are forced to reoptimize their location decision. Because migration costs are high, people may choose to remain in a place unless they are compelled by some sort of external force, such as a disaster. Neighborhoods have important, lasting impacts on the well-being of their residents. Thus, the financial impact and where a person ends up after a wildfire can have long-term consequences.

The second chapter of my dissertation explores how wildfires impact the financial situation

of the household and where they end up. In *Sustained Burn: Analyzing Post-Wildfire Financial and Location Outcomes*, I combine multiple datasets to assess the difference in neighborhood quality and financial outcomes between people who experience a megafire – a fire that burned over 100,000 acres – and those who do not. Exploring various measures in the credit data, I find that people are not increasing their usage of financial instruments – such as credit cards or loans – to survive after the wildfire. However, they do experience increases in distress, as reflected by decreases in their credit scores and increases in the number of active collections. There are also sustained decreases in the likelihood of having a mortgage: they are 5.3 p.p. (19.7%) less likely to have a mortgage after two years, and this remains statistically significant at 3.0 p.p. (10.6%), even four years after the fire. The financial damages after a wildfire can last into the longer term.

Wildfires, and the associated financial damages, could influence a person’s migration decisions. By considering a variety of measures such as the benefits for children, the labor market, the exposure to pollution, and the cost of housing, I am able to characterize the locations to which impacted people move after a wildfire. I find that the wildfire pushed households into areas that are believed to be beneficial for children, with higher upward mobility, shares of college-educated adults, and standardized test scores in the school district. They are also more likely to be in areas with higher median income and higher housing values. Taken together, these results suggest that people are moving to areas with higher amenities, though perhaps at the cost of homeownership.

Given the financial consequences of wildfires and evidence that financial constraints prevent people from moving adaptively, government aid could substantially influence people’s outcomes. In theory, disaster aid could provide resources to prevent individuals from succumbing to the damages associated with natural disasters. It could also help people overcome their financial constraints to move to safe places. However, the government could inadvertently encourage risky behavior, since individuals no longer bear the full consequences

of living in a location with disaster risk. In order to think about optimal policy design, it is imperative to first understand the effect of the current system.

The third chapter of this dissertation, *Soothing the Burn: The Impact of Government Aid on Weathering Wildfires*, analyzes the effect of government aid on the individual's likelihood of moving to safe areas and their financial situation. Specifically, I study the Federal Emergency Management Agency (FEMA)'s Individuals and Households Program (IHP), the largest disaster aid program that provides money directly to individuals to help empower them in their recovery efforts. In order for FEMA to unlock IHP aid, the president must first make a disaster declaration for a county. Individuals then apply for specific types of aid, which range from short-term rent to repair costs. After, FEMA inspectors assess the extent of damage and individuals receive their payment. The average amount of aid disbursed is \$10,000.

Aid is not randomly assigned. Whether someone receives aid or not differs on a variety of factors which will affect her likelihood of moving. On one hand, more destructive fires are more likely to receive aid and to induce people to move. On the other, the application process is arduous so people who want to stay to rebuild their home – the highest amount of aid available – may be more motivated to apply. Thus, just comparing the outcomes of fires that receive aid to fires that do not does not give the causal effect of aid.

To solve this endogeneity concern, I instrument for aid taking advantage of the political incentives of aid. I find that for counties that are politically competitive – where the difference between the top vote shares in the past presidential election was within 5% – are three times more likely to receive IHP aid, conditional on having a fire. Without this instrument, I see that aid is associated with a 17.0 p.p. increase in migration, and people are 5.5 p.p. more likely to be in safe places. After correcting for the endogeneity of aid, I find that aid is associated with an even larger 33.0 p.p. increase in the likelihood of moving, though they are no longer more likely to be in safe places. This makes sense because IHP does not have location-specific requirements besides the repair aid that actually incentivizes people to return

to the risky area.

Disaster aid does help individuals' financial situations. People are substantially less likely to experience financial distress, which is consistent with the goal of the IHP to support individuals' uninsured and serious needs. There is also an increase in credit access, which may be what people use in order to avoid financial distress.

In conclusion, wildfires have lasting impacts on impacted individuals. People move after experiencing a wildfire, though not necessarily to safe places. However, those who are less likely to be financially constrained are moving to safe places. Besides wildfire risk, people do seem to move to areas with higher amenities, though they are less likely to be homeowners, have lower credit scores, and have a higher number of active collections of their debt. Government aid, in its current design, does not facilitate adaptive migration, though it helps people move away and reduces their likelihood of having financial distress.

Chapter 1

Migration as Climate Adaptation: Evidence from California Wildfires

1 Introduction

Climate change is increasing the number and severity of natural disasters such as floods, hurricanes, and wildfires (Berlemann and Steinhardt 2017; Benevolenza and DeRigne 2019; Yao, Kabundi, and Mlachila 2023). These natural disasters impose an enormous human and economic toll, affecting 93.1 million worldwide and costing \$202.7 billion in 2023 alone (OCHA 2024). The costs of climate change, however, can be mitigated through adaptation. Governments and individuals can take a variety of actions to reduce the harms of these disasters and limit these costs, including promoting innovation (Desmet, Nagy, and Rossi-Hansberg 2018; Cruz and Rossi-Hansberg 2023), modifying trade (Kleinman, Liu, and Redding 2023; Desmet and Rossi-Hansberg 2024), increasing building resiliency (Baylis and Boomhower 2021), and investing in preventative infrastructure (Fried 2022; Hsiao 2023).

Migration is theorized to be another important adaptation mechanism. In order for migration to be climate adaptive, it is not enough for individuals to move away from a

location that is affected by a disaster: They must move to an area that has lower risk. I define migration to a low-risk area as “adaptive migration.” Since climate change will impact areas differently (Flannigan, Stocks, and Wotton 2000; Keeley and Syphard 2016), with some becoming more dangerous and others safer, population movements to safer areas can lower the cost of natural disasters. Indeed, using dynamic spatial equilibrium models, researchers have found that shutting down migration generates the largest welfare losses from climate change, even after accounting for other adaptive measures (Desmet and Rossi-Hansberg 2015; Conte, Desmet, and Rossi-Hansberg 2022; Cruz and Rossi-Hansberg 2023; see Desmet and Rossi-Hansberg 2024 for a review).¹ However, there is limited empirical evidence documenting the extent to which populations actually respond to natural disasters by moving adaptively – that is, to less risky areas. The key difficulty in assessing long-run migration patterns is the absence of individual-level panel data with granular location information.

This paper provides novel empirical results that quantify the extent to which people adaptively migrate after a climate shock and explores who does so by studying migration responses to California wildfires. California is the ideal setting for my study of these questions for two reasons. First, half of the most destructive fires California has experienced since it began keeping records in the early 1900s have occurred in the past two decades (WFCA 2024). An increasing number of people are expected to be impacted by this climate shock in California and the world, highlighting a need to understand how individuals and governments can best respond to these events. Second, wildfires often force people to evacuate, at least temporarily, and occur in high-risk areas, making the climate risk of the area particularly salient to residents (McCaffrey, Wilson, and Konar 2018; McCoy and Walsh 2018; Anderson, Plantinga, and Wibbenmeyer 2023). If adaptive migration does happen, this should be a

¹Bilal and Rossi-Hansberg (2023) find that shutting down migration in their model has a small effect on the average welfare loss of individuals. This is they model individuals as considering economic opportunities in their migration decisions. For the US context, they find that the correlation between areas that are desirable in climate terms and those desirable in economic development terms is close to zero, and so the benefits of moving, considering these dimensions within welfare, are minimal. They acknowledge that, in other contexts, there are large welfare losses from shutting down migration. My paper provides further evidence that adaptive migration is empirically minimal.

setting in which to find it. The fact that I find minimal evidence of adaptive migration suggests that it is unlikely to occur in other contexts.

Using detailed location data from the University of California Consumer Credit Panel (UCCCP) and Infutor Data Solutions, I can follow individuals' locations over time to distinguish between temporary displacement (i.e. moves within the first two years after the wildfire) and adaptive migration, which is characterized by long-term moves away from high-risk areas, overcoming one of the main challenges in studying adaptive migration. I can also investigate the role of financial institutions and government aid in influencing people's migration decisions and assess the extent to which these factors help facilitate adaptive migration.

To quantify the extent of migration, I conduct a stacked difference-in-differences (DID) event study to incorporate information from multiple wildfires as natural experiments. I compare the migration decisions before and after a fire of individuals living in census blocks burned by a fire for the first time, with those of individuals living in census blocks never burned within the same census tract and year.² By comparing individuals in census blocks burned for the first time and those never burned, I can cleanly identify the impact of the fire on migration responses and avoid the "forbidden comparisons" flagged in the recent DID literature (Callaway and Sant'Anna 2021; Baker, Larcker, and Wang 2022; de Chaisemartin and D'Haultfoeuille 2022; Roth et al. 2023).³ I also consider the impact of megafires, which burn over 100,000 acres and the impact of subsequent fires (i.e. second vs. first, third vs. second fire). Finally, I investigate how receiving government aid affects these migration

²Census blocks are the smallest geography defined by the US Census Bureau, which creates census geographies to create statistical areas bounded by visible features (Rossiter 2011). A census block falls within a census tract, which is determined to have an ideal population of 4,000 people (Bureau 2022). Half of the census blocks within the United States are smaller than a tenth of a square mile (Lynch 2020).

³Forbidden comparisons are an issue with staggered treatment and heterogeneous treatment effects, when the early-treated are used as a control group. This can lead to the estimator not only being biased but also having the opposite sign of the individual treatment effects because of negative weights. This negative weighting can arise because the early-treated control group has been treated and the treatment effect for the second period is differenced out by the DID estimator (de Chaisemartin and D'Haultfoeuille 2022).

decisions. I focus on two main outcomes: whether individuals move away and whether they move to a low- or medium- risk location, which I also call safe areas.⁴

Overall, there is minimal evidence of adaptive migration. People living in census blocks hit by a fire for the first time are 10.6 percentage points (p.p.) more likely to be in a different census tract the year after the fire, which corresponds to a 79.4% increase in the probability of moving. The results are not solely due to forced migration: Though individuals whose houses are damaged experience a 123.3% increase in their likelihood of being in a different tract the year after the fire, those whose houses are not damaged also experience a 22.8% increase. In the longer run, after four years, there remains a 6.5 p.p. (18.5%) increase in the likelihood that an individual from a first-burned census block lives in a different census tract. However, though people living in census blocks hit by a wildfire are more likely to move to safe areas compared to those who are never burned, they remain less likely than if they were to move like the average mover in California, where 66% of all moves are to low- or medium-wildfire-risk census tracts. This suggests that people living in areas that are burned may prefer characteristics of the location that correlate with wildfire risk and select into these areas. Moreover, the adaptive migration effect dissipates to become statistically indistinguishable from zero in the longer term, four years after the fire.

To understand potential barriers to moving adaptively, I investigate who is more likely to move and why. In particular, I consider the role of financial constraints, which may prevent people from making their desired migration choices. Financial institutions may influence migration decisions by allowing individuals to borrow through loans or credit cards to make their preferred decisions. Individuals with higher credit scores generally have easier access to borrowing, which may exacerbate existing inequalities. Indeed, I find suggestive evidence that financial constraints impact adaptive migration: Although all individuals, regardless of credit score, have a similarly increased likelihood of moving after experiencing a wildfire,

⁴I define tracts that are in the bottom 50% of the distribution of wildfire risk as low or medium wildfire risk, as scored by the Federal Emergency Management Agency (FEMA) in 2021.

only those with high credit scores move to low- or medium-risk areas in the longer term. Moreover, those whose homes are damaged – and who thus are likely to receive an insurance payout shortly after the wildfire – are also more likely to move to safe areas.

This paper adds to the literature on natural disasters. Previous work has found that, after the most destructive wildfires, there is an increase in out-migration probability in the short term, within the first two years after the event (McConnell et al. 2021; Winkler and Rouleau 2021; An, Gabriel, and Tzur-Ilan 2023; Hennighausen and James 2023).⁵ There have been mixed evidence for other natural disasters: Some studies find hurricanes have little to no impact on county out-migration (Henkel, Kwon, and Magontier 2022; Behrer and Bolotnyy 2023), while others find an increase in the propensity to leave the county (Sheldon and Zhan 2022). Tracts with high risk of coastal flooding have significantly less population growth compared to other tracts within the county (Indaco and Ortega 2023). My unique data allow me to follow individuals over time, so I can identify where they move at a sub-county level and determine whether the migration responses are adaptive and observe the effects over a longer period (i.e. four years later).⁶ Knowing where people move is key because if individuals leave for other risky locations, the migration will not moderate the impact of climate change. My paper is also the first to investigate how migration changes depending on whether the individual’s house was burned, their credit scores, and how often the area has burned, which has important implications since climate change is expected to make wildfires more common.

Moreover, this paper offers insight into the dynamic spatial equilibrium model literature by shedding light on who adaptively migrates, and it proposes new dimensions to be incorporated. In particular, I highlight the importance of financial constraints in influencing how people move because I find that those least likely to be constrained (i.e. those with high credit

⁵My results confirm the increase in out-migration that McConnell et al. (2021) find for the first two years after a destructive wildfire occurs.

⁶The sub-county level is important because there is substantial variation of risk within the county.

scores) are more likely to adaptively migrate. Existing models typically model climate change as a shock to capital depreciation, amenities, and productivity (Desmet and Rossi-Hansberg 2015; Desmet et al. 2021; Bilal and Rossi-Hansberg 2023; Cruz and Rossi-Hansberg 2023; Desmet and Rossi-Hansberg 2024) and need to make simplifying assumptions for tractability. My results suggest that incorporating financial constraints is important because the policy implications differ depending on how preferences and constraints impact migration choices.

Finally, this paper is related to the literature on climate adaptation. There is a broad literature that studies the impact of long-run changes implemented in response to trends in climate or the environment (Dell, Jones, and Olken 2014; Annan and Schlenker 2015; Hsiao 2023; Bento et al. 2023a; Bento et al. 2023b). Although the riskiness of an area can be known, most areas – even those with the highest wildfire risk – will not burn in a given year, and thus experiencing a fire could be viewed as an unexpected event, especially in my empirical setting in which I compare individuals who are in blocks that burned for the first time. People may have limited ability to adapt to unexpected shocks, which is consistent with my finding that financial constraints prevent people from moving. However, people have more time to adjust to long-run climate changes. Nevertheless, my paper is relevant to this literature because migration is a rare outcome and even in this situation where people are forced to move due to a shock that brings climate change to mind, they are not moving to safe areas. Thus, it is unlikely to expect adaptive migration to substantially minimize the costs of natural disasters.

The rest of the paper is organized as follows. Section 2 describes the background, including the California wildfire landscape and how government aid is disbursed, and outlines factors that impact people’s migration decisions. Section 3 describes the data, and Section 4 the empirical strategy. Section 5 presents the main migration results. Section 6 concludes, offering an overview of adaptive migration and offering potential explanations for why it is not occurring, as well as directions for future research.

2 Background

2.1 California Wildfires

There has been an increase in wildfires in California over the past three decades, as can be seen in [Figure 1.1](#). The past decade saw a dramatic increase in the most severe fires, megafires, which burn over 100,000 acres. Since 1990, on average, there have been almost 20 additional fires every year.

Wildfires in California are concentrated in certain geographic areas. Some locations are at higher risk of wildfires because of the environmental landscape, vegetation, slope, and wind direction (Collins et al. 2007; Collins et al. 2009; Thompson and Spies 2009; Estes et al. 2017; Rao et al. 2022). These factors can be used to create fire hazard ratings for a given area, such as the fire hazard severity zone classification published by the California Department of Forestry and Fire Protection (CAL FIRE) and FEMA’s wildfire risk scores. Although wildfire incidence within a given location is cyclical in the short term because the fire destroys vegetation and brush, which limits fuel for a wildfire in the immediate future (Farkhondehmaal and Ghaffarzadegan 2022), many factors that contribute to wildfire risk are immutable. Thus, areas that have previously burned may burn again: the map in [Figure 1.2\(a\)](#) highlights where California wildfires burned in the 1990s, 2000s, 2010s, and 2020s.

Even as certain areas burn again over time, new areas also burn for the first time. From 1950-2021, almost 60% of all of the census blocks that burned did so just once, with approximately 20% of the burned census blocks burning more than three times during this period, as can be seen in [Figure 1.2\(b\)](#). Climate change is changing the landscape such that certain areas will have an increased risk of burning for the first time, and the likelihood of severe burns will also rise (Goss et al. 2020).

Once an area experiences a fire, its likelihood of having another decreases temporarily,

though the fact that it burned reveals the potential for a later fire. This is because the type of vegetation that grows in the area generally remains the same throughout time.⁷ Directly after a fire, when flammable vegetation has been burned, there is no fuel in the short run, with the exact horizon depending on how quickly the local vegetation regrows (Coop et al. 2016; Buma et al. 2020; Parker et al. 2022; Tortorelli, Latimer, and Young 2024). The fact that the area had a fire, however, reveals its potential to burn again: Indeed, multiple environmental studies have shown that areas severely burned in the past are likely to reburn at high severity later (Thompson, Spies, and Ganio 2007; Holden, Morgan, and Hudak 2010; Tortorelli, Latimer, and Young 2024).

Although California is particularly prone to fires, its burning trends are similar to those in other parts of the United States and the world. It is well documented that there is increased and more severe wildfire activity across the western United States (Westerling et al. 2006; Dennison et al. 2014; Mueller et al. 2020; Donovan et al. 2023). In addition, the eastern United States (Donovan et al. 2023) has experienced substantial building into undeveloped wildland in recent decades, so there is a growing population that will be potentially impacted by wildfire risk (Radeloff et al. 2018). Globally, approximately 30% of the world surface is prone to frequent wildfires, and the number and the severity of these events are increasing over time (see Goncalves and Vieira (2015) for a review). Thus, the scope of the question of how people respond to growing wildfire risk extends beyond California and has global relevance.

⁷It has been documented that after a fire in a sufficiently diverse ecosystem, some types of vegetation may grow faster than others, altering the biomass in the area and reducing future fire severity (Coop et al. 2016).

2.2 Wildfires and Migration Conceptual Framework: Preferences vs. Constraints

Individuals consider a multitude of factors in deciding where to live. Three factors influence whether adaptive migration occurs: moving costs, preferences, and constraints. First, individuals need to overcome the initial moving costs. Next, for individuals to choose to move adaptively, they must both want to (i.e., have a preference for it) and be able to (i.e., be able to afford to live in safe areas). Under the standard Rosen-Roback location choice model, individuals choose to live in areas that maximize their utility given local amenities, cost of living, wages, and idiosyncratic location preferences (Rosen 1979; Roback 1982).⁸ Wildfires can impact location attractiveness in several ways, such as by decreasing the amenity value (e.g., ruining the view), increasing the cost of living by raising insurance premiums, and impacting the local economy through business shutdowns. Individuals may also update their beliefs about the riskiness of the area, which would decrease the expected utility of staying as they consider the possibility of future damage. Thus, after a wildfire, people may reconsider their location decision because of these changes.

Moreover, moving costs play a role in whether individuals choose to migrate in the first place. The dynamic spatial equilibrium literature has estimated high moving costs, on the order of tens or hundreds of thousands of dollars (Kennan and Walker 2011; Diamond, McQuade, and Qian 2019).⁹ These moving costs can capture both monetary and psychosocial costs of moving, such as leaving behind a community. As climate change increases the likelihood of natural disasters, even people who become aware of an area's increased risk may not change locations if moving costs are sufficiently high.

Wildfires can force individuals to assume the fixed costs of moving and reoptimize their location choices. As they are forcibly displaced in the short term through mandatory

⁸The local amenities are valued by everyone equally, whereas the idiosyncratic location preferences are other aspects of personal preferences for that location.

⁹The high moving costs are calibrated to rationalize the low migration rates in the United States.

evacuations or their dwellings burning, people may make their location choice as if they did not face moving costs. In addition, individuals may prefer to live in areas with features that are positively correlated with wildfire risk, such as proximity to nature or pleasant views (Hammer, Stewart, and Radeloff 2009; Winkler and Rouleau 2021). Individuals with such preferences may choose to return to the burned area or move somewhere else with high wildfire risk – that is migration, even when it occurs, may not be adaptive.

Financial constraints may also prevent individuals from moving adaptively. Financial constraints can be conceptualized as liquidity and solvency constraints. Liquidity constraints are when people are presently unable to pay an amount, though they could eventually over their lifetime using their expected earnings. If people want to move but they cannot pay the initial moving cost, this could be due to a liquidity constraint. Solvency constraints, on the other hand, are when people are unable to afford a payment because it is larger than their lifetime expected earnings. Areas that are low wildfire risk are also more expensive.¹⁰ Individuals may not be able to afford the housing costs to live in safe areas, which is a solvency constraint. There are large financial costs that are incurred because of a disaster: People could lose their jobs or see their working ability restricted, especially if they were evacuated. Individuals could initially choose to move to a safe area but, as they deplete their savings, choose to move to cheaper areas, which happen to also be at higher wildfire risk. Both types of financial constraints could prevent people from moving adaptively even if they want to.

¹⁰Using housing price index data from the Federal Housing Finance Agency and wildfire risk data from FEMA, I find that there is a negative, statistically significant correlation between the two. The relationship holds for the United States overall and for California specifically.

3 Data

To explore how individuals use migration, financial institutions, and government aid after experiencing a fire, I use detailed location and credit data to assess the extent of adaptive migration, that is, moves from risky to safe areas.

3.1 Fire

I use data from the California Department of Forestry and Fire Protection (CAL FIRE) to define the census blocks that are burned. Responsible for California’s fire suppression efforts, CAL FIRE keeps careful and complete records of wildfires within the state. I merge shapefiles of perimeters of wildfires from 1950 to 2021 with maps to identify which census blocks were affected by wildfires. I define the fires at the census block-level because this is the finest geographic unit for which credit data are available. A census block is smaller than a census tract, which is in turn smaller than a county; at the time of the 2010 census, there were 710,145 census blocks, 8,057 census tracts, and 58 counties in California (Bureau 2021).¹¹ I can also calculate the percentage of the block that is burned as a measure of the intensity of fire damage at block level.

I can also identify which houses within a census block were damaged. I obtain address-level data of damaged properties from CAL FIRE that span 2013-2021, and I separately identify the migration effects based on the extent of wildfire damage.¹² CAL FIRE damage inspection specialists (DINS) complete assessments on damaged properties, in which a team of three or four people perform a walk-around of the property to identify any damage and complete

¹¹I list the categories for the 2010 census because the credit data defines locations using this census. The US Census Bureau defines these census geographies in order to create statistical areas bounded by “visible features such as roads, streams, and railroad tracks, and by nonvisible boundaries such as property lines, city, township, school district, county limits and short line-of-sight extensions of roads” (Rossiter 2011). Census tracts have an ideal population of 4,000 people (Bureau 2022). Half of the census blocks within the United States are smaller than a tenth of a square mile (Lynch 2020).

¹²The dataset has recently been updated to include damaged properties up to 2023. It has been used in other papers, such as Baylis and Boomhower (2021) and Biswas, Hossain, and Zink (2023).

a damage assessment report if any is found (NIST 2019). The majority of the damaged buildings are characterized as “destroyed ($> 50\%$)” burned, though they can also experience “major ($25 - 50\%$)”, “minor ($10 - 25\%$)”, and “affected ($< 10\%$)” damage. The data also include addresses without damage, “no damage.”¹³

3.2 Wildfire Risk

To determine whether individuals move from risky to safe areas, I use FEMA’s wildfire risk index to measure the fire danger of the census tract in which the person lives after the fire. FEMA provides a national risk index of natural disasters for each state at the census tract level for a variety of climate shocks, including wildfires.¹⁴ The data are from 2021 and are applied retrospectively, representing an *ex post* wildfire risk score. The wildfire risk score ranges from 0 to 100, and I classify a tract as low risk if it’s between 0-25 and low or medium risk if it’s between 25-50. I define a move as adaptive if the person appears in the data in a low- or medium-risk tract. The wildfire risk score for the United States approximately follows a uniform distribution (Figure 4.1(a)), while that for California has a large percentage (over 60%) of tracts that are low risk and a slight overrepresentation at the highest part of the risk distribution (Figure 4.1(b)).¹⁵

3.3 Migration

To measure migration, I use two complementary datasets, which yield similar migration rates. Each offers specific advantages and disadvantages, and so when taken together, they paint nuanced picture of how people move after they experience a wildfire.

¹³The “no damage” is recorded beginning in 2018 (CalFire 2024).

¹⁴Other measures include the expected annual loss (EAL) to individual natural disasters and the overall risk, which combines the EAL, social vulnerability, and community resilience measures.

¹⁵There is a large mass of census tracts that are bunched at 0 risk. For the overall US distribution, approximately 25% of all tracts have a risk score below 25. For the California distribution, approximately 60% of all tracts have a risk score of 0, so many locations within the state have low risk. However, there is also a large mass at the highest risk level and underrepresentation at the middle risk levels.

The University of California Consumer Credit Panel (UCCCP) used in this analysis includes credit and demographic data for a longitudinal panel of everyone who has ever lived in California and has a credit record. The data have consistent census block information from 2014-2021.¹⁶ There are approximately 40 million consumers. The data include household members and cosigners as well as their location at the zip code and census block level. They also include demographic information about the consumers (including race, gender, age, education, and occupation), credit scores, and raw tradeline-level information about each loan or collections item (including payment history, credit limits and balances, mortgage delinquency, and foreclosure). This dataset also allows me to measure mobility since I can construct a move based on whether the individual is in a different block from the previous quarter.

Because the dataset covers people with a credit history, the population may be older and wealthier than the general population. I conduct validation exercises and find that the migration rates and population coverage are similar to those of the census and the American Community Survey (ACS).

Infutor is an address-level, commercial dataset of adults that spans 1990-2022. It tracks an individual's ten most recent United States residences and, importantly, includes verification of the residence, ensuring that at least two pieces of documentation confirm an individual's being at the location before it is added to the dataset. A subset of the Infutor data includes the individual's birthdate, which can be used to calculate her age, and the duration of time the individual is seen at an address, both important correlates of mobility. The data also sometimes include the individual's gender and social security number (SSN).

¹⁶The data span 2004-2023, but the census block designation changes from the 2000 census definition of blocks for 2004-2014 to the 2010 census definition for 2014-2021 and to the 2020 census definition for 2021-2023. Even when using crosswalks between the different definitions to standardize, I see a spike in migration in years where the census designation changes. This is because there is not a clean mapping across the censuses' characterization of areas: Some census blocks split into smaller entities, while others are combined. There are also higher rates of missingness for census blocks before 2010. Thus, for ease of analysis, I restrict my sample period to 2014-2021, which also coincides the occurrence of many large fires.

My use of both the UCCCP and Infutor data provides a richer understanding of migration. The UCCCP data provide detailed demographic information that makes it possible to conduct heterogeneity analyses about which groups migrate. In particular, they offer insight on the extent of differential migration responses based on access to financial institutions (proxied by credit score). The main disadvantage of the UCCCP data is that I cannot directly observe whether an individual’s home is damaged by the fire as I do not have address-level data. The time coverage (2014-2021) also restricts the horizon of analysis for long-term migration results; to have adequate power and assess pretrends, I present outcomes up to four years after the disaster using the UCCCP data.

The Infutor data supplement the UCCCP analysis because they include the exact address and follow individuals over a longer period (1990-2021), specifically addressing UCCCP’s limitations. However, Infutor has much less detailed demographic information, and its linkage algorithm is opaque. Nevertheless, the estimates of the increase in migration probabilities after a wildfire calculated across both datasets are similar.

4 Empirical Strategy

To quantify whether people move as a result of a fire, I conduct a stacked difference-in-differences (DID) event study, which relies on the identifying assumption that the trajectories of those in treated and control blocks would be parallel, but-for the fire.

4.1 Stacked Difference-in-Differences Event Study

To assess the impact of a wildfire on migration, I conduct a stacked difference-in-differences event study, which compares the outcomes of people living in census blocks that are burned for the first time with those living in never-burned blocks within the same tract before and after the fire. This is a stacked regression because I take multiple fire experiments and

standardize the time variable to be relative to when the fire occurred, which I define as time 0 ($r = 0$). By using this design, I can take advantage of multiple fire events and obtain greater power for my estimates.¹⁷

I estimate the following regression equation:

$$y_{i,f,t} = \mu_i + \sum_{r=-3, \neq -1}^{N=4} \lambda_{r(f,t)} + \sum_{r=-3, \neq -1}^{N=4} \beta_r \lambda_{r(f,t)} \times fire_{b(i,f)} + \gamma_t + X'_{i,t} \Gamma + \epsilon_{i,f,t} \quad (1.1)$$

The outcome variable ($y_{(i,f,t)}$) is defined for individual i at calendar time t and for fire f .¹⁸ I include fixed effects for the individual (μ_i) to remove time-invariant individual-level factors.¹⁹ I have relative time $\sum \lambda_r(f, t) = \sum \mathbb{I}[t - E_f = r]$ fixed effects, spanning from three years prior to four years after the fire and omitting the year before the fire as the reference year. Each term ($\lambda_r(f, t) = \mathbb{I}[t - E_f = r]$) is equal to one when it is r years from the year when the individual i experienced fire f , which is denoted as E_f . The fire variable ($fire_{b(i,f)}$) is an indicator variable equal to one if the individual i lives in a census block ($b(i, f)$) that is burned for the first time by fire f and equal to zero if she lives in a never-burned census block within the same census tract.²⁰ The coefficients of interest are β_r , which capture how much the experience of the fire impacts the individual r years relative to the time of the fire. I normalize the difference between the treated and the control to be the year before the fire (event time -1). Because the fire is defined at the census block level ($b(i, f)$), I cluster

¹⁷If there are multiple blocks that burn for the first time at different years within a tract, I duplicate the control blocks for each of those fire events so that the burned blocks each get a set of controls and relative years defined by the year of the fire.

¹⁸In my main specification, each individual who lives in a census block burned for the first time in my sample is represented one time. However, there are cases where an individual could live in a never burned block that serves as the control group for multiple fire events. This would happen for tracts that have different blocks within it that burn for the first time across different years. In those cases, following the stacked difference-in-differences literature, I would duplicate the observation, and so the same calendar year t and individual i data would serve as a control across two fires, f_1 and f_2 (Wing, Freedman, and Hollingsworth 2024). Thus, the data is unique at the individual, fire, and calendar year level.

¹⁹The individual fixed effects absorb the treatment fixed effects that are typical in this literature because the individual-level is more granular than the block-level, which is the level that the fire treatment is defined.

²⁰This fire treatment is more granular than the treatments considered in other wildfire papers, which define the wildfire at the census tract level (An, Gabriel, and Tzur-Ilan 2023; McConnell et al. 2021).

standard errors at the block level (Abadie et al. 2023).

For robustness, I include calendar year fixed effects (λ_t) to control for general migration trends over time. I include potentially time-varying controls ($X_{i,t}$) for the age category, marital status, whether the individual completed college, and the duration of time that individual i lived in her census block ($b(i, f)$) at the time of the fire because these variables are found to be associated with migration.

To address the “forbidden comparison” concerns raised in the recent DID literature – namely, that bias may arise from including units that are eventually treated within the untreated group – (Callaway and Sant’Anna 2021; Baker, Larcker, and Wang 2022; de Chaisemartin and D’Haultfoeuille 2022; Roth et al. 2023), my main specification investigates the responses of individuals in places burned for the first time and compares them with those of people living in never-burned blocks. Forbidden comparisons are an issue in the context of staggered treatment and heterogeneous treatment effects, when the early-treated are used as a control group, which can yield estimates that are not only biased but even have the opposite sign of the individual treatment effects. This is because negative weights can arise if the early-treated control group has been treated and the treatment effect for the later period is differenced out by the DID estimator (de Chaisemartin and D’Haultfoeuille 2022). My results are robust to the use of other control groups, such as those living in blocks within a one-mile radius of the fire and those living in blocks one to five miles away from the fire.

I also estimate the DID version of [Equation 1.1](#):

$$y_{i,f,t} = \mu_i + \nu post_{f,t} + \beta post_{f,t} \times fire_{b(i,f)} + \gamma_t + X'_{i,t} \Gamma + \epsilon_{i,f,t} \quad (1.2)$$

In [Equation 1.2](#), I compress the relative year fixed effects terms, $\sum \lambda_r(f, t) = \sum \mathbb{I}[t - E_f = r]$, into an indicator $post_{f,t}$ which equals one if year t is after the year when fire E_f occurred: $t - E_f > 0$. The coefficient of interest β captures the average of the impact of the fire at any

point after the treatment.

4.2 Definitions of Terms

I define the fire treatment in two ways: 1) by whether a block is burned for the first time; and 2) by whether the block experiences a megafire (one that burns over 100,000 acres). With global warming, areas not previously burned are expected to experience a wildfire, and wildfires are expected to become more severe. I use these definitions of fire to address these different consequences of climate change. In my main specification, I define the control group as people living in blocks that never burned but within the same census tract as the fire, as shown in [Figure 4.2\(a\)](#).²¹

I define migration over time to differentiate short-term from longer-term moves and the extent of adaptation – that is, the wildfire risk of the location. For migration to be climate adaptive, the individual must both move away and to a safe location. The first component of adaptive migration is captured in whether the individual is observed in the data in a tract other than the one she lived in the year before the fire (“Moved Away from Tract”). With this definition of migration, I can capture whether people leave the burned area and the extent to which people who are displaced in the short run later return since my credit panel data allows me to track individuals over time for up to four years after the fire. I capture the second component of adaptive migration by whether the individual is observed in a low- or medium-wildfire-risk census tract after the fire (“Low or Medium Wildfire Score”). Because this measure is at the census tract level and the treatment and control groups in my main specification are census blocks within the same tract, they have the same initial wildfire risk scores.

²¹For robustness, I also consider those who live in the one-mile radius ([Figure 4.2\(b\)](#)) and those who live in the one- to five-mile radius surrounding the fire ([Figure 4.2\(c\)](#)).

4.3 Identification Assumptions

The DID event study relies on the assumption that the outcome trends of the treated and control groups would have been parallel in the absence of the fire. Since the exact trajectory of a wildfire is quasi-random because of exogenous factors such as wind direction, this assumption seems reasonable (Beverly and Forbes 2023). I can evaluate its plausibility by checking whether the burned and never-burned areas were on similar trajectories before the fire and thus whether the pre-fire coefficients are statistically significantly different from zero (Roth 2022).

Recent research suggests that since humans can cause wildfires, there is a relationship between building density and wildfire risk (Ang 2024; Kestelman 2024). This could pose a threat to identification if the outcome pretrends for areas with higher density are not parallel with those of lower-density areas. This is not a concern in my setting for two reasons. Although 90% of wildfires are caused by human activity (Radeloff et al. 2018), the specific path of the fire within a census tract can be considered largely caused by external, exogenous factors such as vegetation, slope, and wind direction, particularly at a spatially granular level such as the census block (Collins et al. 2007; Collins et al. 2009; Thompson and Spies 2009; Estes et al. 2017; Rao et al. 2022). Thus, the exact path of the fire is random, even if there is concern about whether its starting point is random.

4.4 Robustness: Alternative Controls and Empirical Specifications

To assess the robustness of the results, I consider alternative control groups and empirical specifications. I use the never-burned blocks as the control group in my main specification to implement the cleanest control group and to address the concerns from the recent DID literature about negative weighting that may arise when the eventually treated are used as the control group (Callaway and Sant’Anna 2021; Baker, Larcker, and Wang 2022; Roth

et al. 2023). Here, I consider other control groups to assess the robustness of my results: people living in blocks within a one-mile radius of the fire and people living in blocks one to five miles away from the fire, an approach that follows the “donut” control design in the urban economics literature, which imposes some geographic distance between the treated and control units to allow for potential spatial spillovers to nearby areas (e.g., An, Gabriel, and Tzur-Ilan (2023) among many others).

I consider other empirical specifications in addition to my main specification, [Equation 1.1](#), which I label “Baseline” or “Relative Year and Calendar Year FE.” For the alternative specifications, I vary how I allow the relative trends to differ, such that they depend on the wildfire risk of the burned area or the specific fire. In all of my empirical specifications, I include individual fixed effects (μ_i), although I do not include this in the labeling of the specifications.

To allow for differential trends based on wildfire risk, I estimate [Equation 1.3](#). I refer to this specification as “Interacted Wildfire Risk and Relative Year FE.”

$$y_{i,f,t} = \mu_i + \sum_{r=-3, \neq -1}^{N=4} \omega_r \lambda_{r(f,t)} \times w_{z(i,f)} + \sum_{r=-3, \neq -1}^{N=4} \beta_r \lambda_{r(f,t)} \times fire_{b(i,f)} + \gamma_t + X'_{i,t} \Gamma + \epsilon_{i,f,t} \quad (1.3)$$

In [Equation 1.3](#), the $\sum_r \omega_r \lambda_{r(f,t)} \times w_{z(i,f)}$ replaces the relative year fixed effects in [Equation 1.1](#) (represented by $\sum_r \lambda_{r(i,f,t)}$). The wildfire risk score index ($w_{z(i,f)}$) is defined at the level of the census tract ($z(i, f)$) in which the individual i resides when fire f occurs.²² This index ranges from 1 to 4, taking a value of 1 if the census tract’s wildfire score is below 25, 2 if between 25 and 50, 3 if between 50 and 75, and 4 if above 75. It is interacted with the relative year indicators ($\lambda_{r(i,f,t)} = \mathbb{I}[t - E_{i,f} = r]$) to allow for differential trends depending on the wildfire risk of the burned area. The ω_r coefficients capture how much the experience of the fire

²²Recall that this variable is defined at the census tract level because this is the level at which the FEMA wildfire risk is defined.

impacts the individual r years relative to the time of the fire, based on the wildfire risk score of the census tract where the fire took place.

To allow for differential trends based on the specific fire, I estimate [Equation 1.4](#). I refer to this specification as “Interacted Fire and Relative Year FE.”

$$y_{i,f,t} = \mu_i + \sum_f \sum_{r=-3, \neq -1}^{N=4} \phi_f \lambda_{r(f,t)} + \sum_{r=-3, \neq -1}^{N=4} \beta_r \lambda_{r(f,t)} \times fire_{b(i,f)} + \gamma_t + X'_{i,t} \Gamma + \epsilon_{i,f,t} \quad (1.4)$$

In [Equation 1.4](#), the $\sum_f \sum_r \phi_f \lambda_{r(f,t)}$ replaces the relative year fixed effects in [Equation 1.1](#). This allows each fire f to have a different time trend. This empirical specification is conservative because it assumes that the fire did not cause any general changes in trends for the burned areas, and it nets out this trend by allowing the local trends to differ by fire. Presumably, there could be a change in the migration trend overall in the burned areas (which is seen in [Figure 1.3](#)), and so not using this variation would lead to a muted estimate of the impact of the fire.

5 Migration Results

In this section, I show that people move away after their block experiences a wildfire, and though they initially move to areas with low or medium wildfire risk, they are not more likely to stay in safe areas in the longer term. This could be because of liquidity constraints: Those with high credit scores and who are more likely to receive payouts post-fire (i.e., whose homes are damaged) are also more likely to be observed in low- or medium-wildfire-risk areas in the longer run.

5.1 Preliminary Results

I compare blocks that are burned with those in the same tract that are never burned. [Table 1.2\(a\)](#) shows, both overall and separately for fires that do and do not receive aid, the percentage who migrate among those living in blocks that experience the first burn. Those living in blocks burned for the first time are more likely to be in a different tract in both the year after and four years after the fire, than are residents in nearby blocks in the tract that are never burned. Individuals in burned blocks are more likely to have returned by four years later, and for those living in first-burned blocks that receive aid, 21.62% of those who moved in the year of the fire return. Those who move in the year of the fire and whose location does not receive aid are not more likely to return than the individuals in the control group. [Table 1.2\(b\)](#) shows a similar percentage of the population moving the year after megafires and remaining away four years later for places both receiving and not receiving aid. Interestingly, for those places with megafires but without aid, the percentage of people who moved the year of the fire but come back is higher.

To understand the characteristics of the movers, I compare the summary statistics of individuals who moved to a different tract the year of the fire with individuals who did not for the population of people who lived in a census block that burned for the first time ([Table 4.2](#)). All characteristics are measured the year before the fire occurred. The movers and non-movers have similar average age, gender composition, likelihood of being married, and education levels. Movers are less likely to have mortgage loans. People who move also have lower credit scores, a higher number of open credit cards and open loans, and a lower credit limit and payment. These individuals would likely have more difficulty borrowing after the fire as their lower credit scores and existing engagement with financial institutions have put them in a disadvantageous position for subsequent interactions.²³

²³This is consistent with Bilal and Rossi-Hansberg (2021), who argue that those who are financially constrained may use their “location asset” and move to reduce the effect of this constraint.

I also investigate the characteristics of the individuals who returned for people who initially moved in a block that burned for the first time and moved away the year of the fire (Table 4.3). Among the population of people who lived in a census block that burned for the first time and who moved away that year, people who returned within four years are less likely to be bankrupt and to have current delinquencies. There are no statistically significant differences in other characteristics.

The presence of differences between the groups does not necessarily invalidate the identification strategy since the DID event study requires only that trends would have been parallel, not the same level, between the groups but for the fire. Nevertheless, the characteristics of the burned blocks are similar in levels to those of the nearby control blocks before the fire. Table 1.1 reports the averages of multiple demographic, migration, tract-level, and financial characteristics for the treated and control groups, the difference between the averages, and whether they are statistically significantly different.²⁴

The average migration rates in the burned and never-burned areas follow parallel trends. As seen in Figure 1.3, before the fire, the migration rates between the two are nearly identical and thus parallel, and then those in the burned area experience a sharp increase after the fire (time 0), lending credibility to this empirical strategy. The average shares of people moving to low- or medium-wildfire-risk areas in the burned and never-burned blocks also follow parallel trends (Figure 4.3).²⁵

²⁴Statistically significant differences that are small in magnitude may not have a large impact on migration. For example, how much would an extra 0.23 credit cards change migration decisions? I run a regression of the likelihood of a move within the year based on the available covariates and predict the expected migration probabilities separately for the treatment and control groups. I find that the individuals living in areas that are burned are predicted to have lower migration probabilities than those in the control group (Tables 4.4 and 4.5).

²⁵Since the FEMA wildfire risk score utilized in the main specification is defined at the census tract level and the treated and control groups are within the same same tract, they should theoretically have the same risk. However, because of the stacked nature of the regression, there are duplicate entries for the control group, so that is why the estimates diverge. Both the treatment and control groups experience a decrease and then increase around the time of the fire. This could be evidence of regression to the mean, as areas that are burned are of higher wildfire risk, so moving away from these areas will be more likely to be to a low- or medium-wildfire-risk area.

5.2 Event Study Results for Migration

Figure 1.4 shows the results from estimating Equation 1.1, plotting the β_r coefficients. The coefficients are not statistically significant for years before the fire and then become positive and significant for years beginning with the one after the fire. Thus, the fire causes people to move away and stay away. The coefficients imply that, two years after the fire, individuals living in areas burned for the first time are 10.7 p.p. more likely to have moved away from the tract they lived in the year before the fire, a 47.8% increase in the likelihood of moving.²⁶ By the fourth year, this number remains elevated, though it is slightly smaller at 7.4 p.p. (21.0%). If we look at Figure 1.3, the treated group has a dip by year four in the likelihood of moving, suggesting that a group of people in the burned blocks move away and then return, consistent with Table 1.2. Thus, the fire causes an increased likelihood of out-migration that persists over the longer term (i.e., at the four-year horizon), even if a portion of the population returns.

This result is robust to my using a variety of different control groups and empirical specifications. Figure 4.4 plots the migration results for the areas first burned across the various control groups: the never burned (“Never Burned”), those within one mile of the perimeter of the fire (“One Mile Away”), and those in a one- to five-mile radius from the fire (“One to Five Miles Away”). The control group in the main specification is every census block that is never burned that is within the same census tract (“Never”). I use this as my main specification because wildfire risk is defined at the census tract level, which ensures that my treatment and control groups have the same risk. To assuage concerns that the never-burned areas are somehow different for some unmeasured reason (e.g., a deep canyon that acts as a fire break), I also consider as a control the census blocks within one mile of the fire perimeter (“One Mile Away”). Other papers have used a “donut” control group to allow

²⁶This percentage is calculated with the percentage of the control group who are in a different tract two years before the year of the fire as the denominator. All percentage changes are calculated using the migration rates of those in the control group the associated number of years before the fire.

for spillover effects to the immediate area (i.e. An, Gabriel, and Tzur-Ilan 2023), an approach that I employ as well. I include within the control group those living in census blocks one to five miles from the fire perimeter (“One to Five Miles Away”). I find similar migration results across the specifications with these control groups.

Figure 4.5 shows the results of the event study for the first burn across different empirical specifications. The baseline specification (“Relative Year and Calendar Year FE”) controls for general year trends with the calendar year fixed effects and assumes that the treatment effects across the years are similar by having a single set of relative year fixed effects. I also consider another specification that allows for differential trends in wildfire risk by interacting each wildfire risk category with the relative year (“Interacted Wildfire Risk and Relative Year FE”).²⁷ This is to allow for the possibility that areas where the wildfire is more surprising (i.e., those with low wildfire risk) may exhibit different trends from higher-risk areas since there could be selection in the types of people who choose to live in these different areas. The final empirical specification I consider allows each fire to have a specific trend (“Interacted Fire and Relative Year FE”). This is the most conservative specification because it takes out any change in the migration trend driven by the experience of a wildfire in the burned areas, which is arguably part of the relevant treatment effect. Across all three empirical specifications, I find that my coefficients are similar in magnitude and follow similar trajectories (i.e., an increase in the likelihood of moving away and then a dip four years after the fire), though the most conservative estimator yields smaller coefficients than the others. However, for the estimates four years after the fire, all three are of similar magnitude and show a statistically significant increase in the likelihood of being seen in a different tract. Thus, the results, especially the longer-term migration results, are similar across the various empirical specifications.

I also consider another definition of migration: whether the individual is observed in at least two different tracts within the focal year (“Moved Tract this Year”). In Figure 4.9, I find

²⁷I define wildfire risk in four categories: “Low,” “Medium,” “High,” and “Very High.”

that wildfires cause people to be more likely to move in a given year, even four years after the fire.²⁸ This could be because individuals move away after the fire and then move again, which explains why they are in a safe tract after two years but not after four years. It could also capture housing instability as individuals reel from the instability of being displaced. The year of the fire, those in the burned block are 16.0 p.p. more likely to move, a 122.8% increase in the probability of moving. Four years after, individuals still have an elevated likelihood of moving (13.1 p.p. or 87.8%), suggesting the experience of a fire has longer-term migration consequences.

5.3 Event Study Results for Adaptive Migration

The results so far show that people are moving, but the risk level of the destinations need to be considered. Next, I run the same stacked DID event study regression from [Equation 1.1](#) with the outcome being an indicator for whether the individual lives in a census tract with low or medium wildfire risk. Because the wildfire risk score is defined at the census tract level, the treated and control units have the same score because they are within the same tract. This outcome does not condition on migration.

[Figure 1.5](#) shows the results for adaptive migration. After two years, those who are in a block burned for the first time are 5.5 p.p. more likely to be in a low or medium wildfire risk area than those in the never-burned blocks, a 23.9% increase. Four years later, this falls to 1.7 p.p. (6.2%) more likely and is no longer statistically significant.

For robustness, I again consider a variety of different empirical specifications. The same pattern of an initial increase and then decrease by year four in the likelihood of being in a low- or medium-wildfire-risk area appears across the different empirical specifications, as seen in [Figure 4.7](#). None of the coefficients are statistically significant when I control for fire-specific trends in the “Interacted Fire and Relative Year FE” specification with the never

²⁸The pre-fire likelihood of moving is similar ([Figure 4.9](#)).

burned as the control group, and for the fourth year after the fire, the coefficients are all statistically insignificant across the specifications.²⁹

Counterfactuals

Since burning of a block induces people to move, those in burned blocks could be mechanically more likely to be observed in low- or medium-wildfire-risk areas than those in never-burned blocks. This is because the treatment and control blocks have the same risk score as it is defined at the census tract level and tracts that burn tend to have higher wildfire risk. Thus, since those in census blocks that burned have higher migration rates, they could be more likely to appear in low- or medium-wildfire-risk areas just because they are more likely to move. One way to test this possibility is to analyze the risk of the destination conditional on moving.

Taking the population of movers across California overall, movers within areas burned for the first time before the fire, and movers in first-burn blocks after the fire, [Figure 4.10](#) shows that though the likelihood of an individual's being seen after the fire in a tract with very high wildfire risk tract (i.e., FEMA wildfire risk score above 75) is lower than that of being seen in such tracts before the fire for movers in blocks that burned for the first time, it is still over three times as high as the likelihood of the average California mover. To formally compare the movers from the first-burned areas with the average Californian, I take everyone who was living in a census block that is burned for the first time and moves the year of the fire, and I

²⁹It is difficult to interpret the adaptive migration results when I use various control groups because the wildfire risk is defined at the census tract level. In my main specification, because I compare census blocks that are first burned with those never burned within the same tract, they have the same starting wildfire risk score. The control groups that are defined as individuals within one mile and those between one and five miles away from the perimeter of the first fire have pre-fire coefficients that are statistically significantly different from zero and that decrease the year of the fire. This could reflect regression to the mean in that the areas that are burned have higher wildfire risk compared to the control groups in those cases, so those that move to the burned area would be moving from a lower-wildfire-risk area. Nevertheless, the general trends of the coefficients are qualitatively similar across the control groups, in that there is an increase and then a decrease in the likelihood of being in a low- or medium-wildfire-risk tract after the fire, as can be seen in [Figure 4.6](#).

randomly assign them the location of a mover in California. I run the same stacked DID event study regression from [Equation 1.1](#) against the never-burned control group using the randomly assigned moves.³⁰

The results ([Figure 4.11\(a\)](#)) show that the migration rates to low- or medium-wildfire-risk areas is lower than the rate for a random California mover for the two years after the fire and are similar in magnitude for the fourth year after it. Thus, individuals who experienced a wildfire for the first time are not more likely to move to a low- or medium-wildfire-risk area than is the average Californian, suggesting that there could be some selection of individuals who choose to move into these risky areas in the first place.

The population who chooses to move into these risky areas might be selected because of their preferences since they remain three times as likely to move to an area with very high wildfire risk – even after experiencing a fire ([Figure 4.10](#)). A more relevant counterfactual group could be the movers from the control group, who had also initially chosen to live in those areas. To assess this, I estimate [Equation 1.1](#) comparing those who moved the year of the fire in the blocks that burned for the first time with movers in the never-burned blocks within the same census tract. [Figure 4.11\(b\)](#) shows that those who moved from the first-burn blocks are more likely to move to a low- or medium-wildfire-risk than movers from the nearby never-burned blocks. Of those who chose to live in these areas and presumably have similar preferences but one experiences a burn and the other does not, the burned movers are more likely to move to a safe area in the short run, though this effect also becomes statistically insignificant by the fourth year.

5.4 Heterogeneity Analyses

To understand the underlying mechanisms that may influence an individual’s decision to adaptively migrate, I consider situations where there could be evidence of constrained vs.

³⁰I randomly assign locations 100 times to obtain the standard errors for the coefficients.

unconstrained migration. I analyze whether adaptive migration choices differ by whether the individual likely received a payout post-fire and whether the individual had a high credit score. These descriptive analyses suggest that liquidity constraints prevent people from moving adaptively. I also investigate whether migration choices differ over time and as a place burns repeatedly.

Budget Constraints and Constrained Migration

Certain factors could keep people from moving adaptively. There are two requirements for people to choose to adaptively migrate: They must want to move to safe areas, and they must be able to do so.³¹ It is difficult to disentangle the desire from the ability to move when we look solely at the outcome of where people choose to move, but the evidence below suggests that liquidity constraints prevent some people from moving adaptively.

Damaged vs. Undamaged Homes

I compare the adaptive migration responses for those in damaged and undamaged homes to investigate the role of preferences and financial constraints. It is important to separately consider those with and without property damage as their financial decisions differ after the fire (Biswas, Hossain, and Zink 2023).³² One hypothesis is that an individuals' location choice solely reflects their preferences. Migration increases mechanically for those whose homes are damaged because their homes are destroyed. Those whose homes are not damaged could be interpreted as moving according to their preferences because they did not need but rather chose to move. Thus, if moving to a low- or medium-wildfire-risk area is viewed as optimal

³¹Refer to [subsection 2.2](#) for a more detailed discussion.

³²Biswas, Hossain, and Zink (2023) find that 90-day delinquencies were 4 p.p. higher for properties damaged by wildfires and prepayments were 16 p.p. higher for properties that were damaged by wildfires, with no significant changes for undamaged properties inside the burned area. Because there are not increased sales or refinances, they hypothesize that the increased prepayments are due to insurance claims. This suggests that many affected households do not use their insurance settlements to rebuild the damaged homes.

for these individuals, I would expect that people whose homes escape unscathed and move do so.

Another hypothesis takes into consideration the resources provided for those whose homes are damaged vs. those whose homes are not. The former could have increased migration and be able to move adaptively because the damage to their homes unlocked financial resources – for example, an insurance payout.³³ Those with undamaged homes may also move because they are unable to afford to remain, they do not want to stay in the risky area, or infrastructure damage renders the location unlivable. Even for those who desire to move to a safe place, the next location choice could be driven by cost considerations, such as the affordability of the area. Therefore, it could be possible that both people whose homes are damaged and not damaged are more likely to move away, though only the former are more likely to move to safe areas because they received the financial resources to do so.

To test these hypotheses, I estimate [Equation 1.1](#) separately for those whose homes are damaged and those whose homes are not. Although in [Figure 1.4](#) I find that individuals living in census blocks that are burned are more likely to move away, these moves could be mechanically attributable to the fact that people are forcibly displaced because their homes are burned. To investigate this, I combine the CAL FIRE DINS address-level damage data with the Infutor data to separately evaluate the migration responses of people whose homes were found to have damage and those of people whose homes remained unscathed.³⁴ I flag individuals whose address is listed to have damage in the DINS data as “Damaged,” and

³³Other potential factors could lead to those with damaged property receiving more resources. In California, Proposition 19 allows those whose homes are damaged to transfer their tax burden to a new home within the state. Additionally, after a fire, the insurance premiums in the affected area will increase as insurers update their fire risk models based on historical fire incidence. (For details about California’s institutional background, see [subsection 1.3](#).) Higher premiums could in turn price former residents out of the area. There are also specific government grants and loans available to individuals whose homes are damaged, though resources are also available for people whose home escaped damage.

³⁴Because of data limitations and power concerns, for the damage analysis, I define the fire term to be for any fire, instead of restricting it to the first fire. I also validate that the adaptive migration results are similar across the two datasets, using the same specification and the same time period (i.e., people experiencing first burns between 2014 and 2021), and find similar results ([Figure 4.12](#); [Figure 4.13](#)).

I define the addresses within a burned block but not flagged as “Not Damaged.” I find a 10.2 p.p. increase in the likelihood of moving for those whose homes have damage, which is 84.0% higher than that for the control group (Figure 4.14).³⁵ However, I also find a 1.9 p.p. (15.6%) increase in the likelihood of moving for those whose homes are not found to have damage, suggesting that the migration results are not driven solely by those who need to move because their homes are unlivable.³⁶ For the fourth year after the fire, these results grow to 16.7 p.p. (75.6%) for those with damage and 4.1 p.p. (18.6%) for those without.

I find that people whose homes are damaged are 6.1 p.p. (49.6%) more likely to go to a low- or medium-wildfire-risk score tract after two years and are 11.9 p.p. (64.8%) more likely to do so after four years. However, those whose homes are not damaged are not more likely to be seen in a safe area, anytime after the fire (Figure 4.15). This is consistent with the second hypothesis about financial resources helping individuals move to safe areas.

For robustness, I consider other definitions of people whose homes are not damaged and find similar results. My main definition for a home that is not damaged are one that is in a burned block but does not belong to the group of homes found to be damaged (“Within Burned Block, Not Confirmed Damage”). Next, I consider the homes evaluated by CAL FIRE and definitively marked as having no damage (“Confirmed Not Damaged”). I also define an indicator for homes not confirmed to have damage (“Not Confirmed Damage”). The results in Figure 4.17 show an increase in the likelihood of moving away across all of these definitions of undamaged homes, though the statistical significance of the “Confirmed

³⁵The migration rates in the Infutor dataset are lower than in the UCCCP data (Figure 4.12). This could be because of issues with linkage over individuals across time since Infutor, whose product is generally used for advertising, has an incentive to have the correct address for a given individual at a moment in time but not necessarily to connect the same individual over time. The “Damaged” indicator includes all levels of damage, ranging from minimal to destroyed, which could also explain why not all individuals are seen to move if their home is damaged.

³⁶It could be that people whose homes are not directly damaged are unable to live in the area because of infrastructure damage. Nevertheless, this would not change but rather strengthen my interpretation of the importance of financial constraints. Though people with undamaged homes must pay the migration cost by being forcibly displaced, they do not receive the additional resources associated with having damaged property. This suggests that the difference in post-fire resources, which loosens liquidity constraints, can explain the lack of adaptive migration for the group with no damage.

Not Damaged” estimate differs because of power issues. Similarly, in [Figure 4.17](#), there is no statistically significant effect on the likelihood of moving away across all of these definitions of undamaged homes.³⁷

High vs. Low Credit Score

Individuals might turn to financial institutions to help weather the impact of the fire. Having access to loans and credit cards can help people who experienced a fire borrow to have higher liquidity in the present to make optimal decisions. Those with higher credit scores can engage with financial institutions on more advantageous terms. Thus, I view the credit score as a proxy of an individual’s ability to access financial resources needed after a large shock, such as a fire.³⁸

I interpret people with higher credit scores as being less financially constrained and their migration choices to thus better reflect their preferences. Differences in migration decisions between groups with high and low credit scores could be evidence that the latter are constrained, assuming that the two groups have similar preferences.³⁹

I estimate [Equation 1.1](#) separately by credit score. I define people with high credit scores as those with scores of 700 or higher and those with low credit scores as having scores lower than 700.⁴⁰ I compare individuals in the same credit score category and in blocks burned

³⁷There is a larger increase in the magnitude of the coefficients on “Confirmed Not Damaged” for the migration and adaptation results, but this could be because homeowners who requested the damage inspection could have done so for insurance purposes and already wanted to move. Moreover, the results are not statistically significant and have large confidence intervals because there are few observations of inspected properties confirmed to not have any damage.

³⁸Credit scores may also be a proxy for individuals’ income, although the literature is divided on this question. For instance, Albanesi, De Giorgi, and Nosal (2017) find that credit score and income are strongly positively correlated, whereas Beer, Ionescu, and Li (2018) find only a moderate correlation. For the purposes of my interpretation, this relationship is not key as I am interested in the extent to which people move and whether access to financial institutions alters migration choices.

³⁹It could also be that those with higher credit scores are more firmly tied to a place given their likely greater home equity. A fire either could free them from their current location, such that they could be more likely to move, or could make them reluctant to relocate. The results from Biswas, Hossain, and Zink (2023) suggest that individuals are less likely to rebuild their homes, suggesting that they are more likely to move away. I also find similar results when I run the analysis by homeownership status.

⁴⁰This is similar to the average credit score in my treated group (701) and the average credit score in

for the first time with those in never-burned blocks within the same census tract. As seen in [Figure 4.18](#), I find that individuals across the credit distribution are more likely to move, though those with high credit are statistically significantly more likely to remain away after four years. Similarly in [Figure 4.19](#), the high credit scores are statistically significantly more likely to move to a low- or medium-wildfire-risk area after four years. These results suggest that people with high credit are more likely to move away and to safe areas than people with low scores, which could be interpreted as the latter being constrained in their migration choices.

Investigating Other Mechanisms

The previous results suggest that financial constraints could hinder adaptive migration. This is because people whose homes are damaged and people with high credit scores are less likely to be liquidity constrained and are more likely to move to safe areas. To consider additional mechanisms and potential explanations, I estimate [Equation 1.1](#) across other dimensions of heterogeneity, and the results reinforce the importance of liquidity constraints in preventing adaptive migration.

One potential explanation is that more educated people are more likely to move adaptively because they can better update or incorporate additional information into their migration decision. More educated people are also more likely to have high credit scores (LendEDU 2017). To test this channel, I estimate [Equation 1.1](#) separately for the group with a college degree and that without using the UCCCP data, which include education status.⁴¹ I find that both education groups in the first-burned blocks are more likely to move away than their counterparts in never-burned blocks ([Figure 4.20](#)). However, people without a college degree are more likely to move adaptively, which runs counter to the hypothesis that education is

California, which was 712 as of March 2024 (Equifax 2024).

⁴¹In my main specifications, I include whether an individual has a college degree as a control.

driving the adaptive migration effect (Figure 4.21).⁴²

Another potential correlate of credit scores is age. People of working age, which I define as younger than 55, could be more likely to move for reasons beyond wildfire risk, such as work or school continuity for their children. I choose 55 as the age cut-off because Proposition 19 allows those who are above 55 and those whose homes are destroyed by a wildfire to transfer their tax burden to another property in California (BOE 2021). Older people (i.e., over 55) may also be retired and could have chosen to live in the risky places to begin with because of their preferences, and they could be more likely to move to a location with similar environmental amenities. I find that both groups are more likely to move away from the burned area (Figure 4.22), though neither is more likely to be in a safe area in the longer term than their counterparts in never-burned areas (Figure 4.23). This suggests that the adaptive migration results are not driven by age.

I also consider heterogeneity by homeownership status. Typically, homeowners purchase their homes with a mortgage, which would require some sort of wildfire insurance. Those who are owners, and thus more likely to receive a payout after the wildfire, are more likely to move to safe areas in the longer term, providing further evidence that financial constraints are an important barrier to adaptive migration (Figure 4.24; Figure 4.25).

Taken together, these heterogeneity analyses are consistent with financial constraints being a barrier to adaptive migration. These results show that those who are less financially constrained – either by having easier access to financial institutions or who are more likely to receive a payment after the wildfire – are more likely to move to safe areas.

Effects Across Time Periods and Subsequent Fires

Given the increase in wildfires in California and climate change discussions over the past decade, there could have been a change in people’s beliefs about the likelihood of future fires,

⁴²This information channel would offer a less satisfactory account of the results of the damage analysis.

subsequently impacting their migration decision. More recent fires could be expected to be associated with more migration as people become more aware of the increased likelihood of subsequent fires. It could also be that with increased awareness about climate risk, people who choose to live in areas with higher wildfire risk have a strong preference for the associated environmental amenities and thus are less likely to move.

To test this conjecture, I use the Infutor data, which cover a longer period of data than the UCCCP data, and I employ the same empirical strategy across different sample time periods.⁴³ In particular, I look at the migration rates and wildfire risk score for 2000-2019 and then separately for 2000-2010 and 2010-2019 and plot them on the same graph.⁴⁴ I find similar migration rates across periods, consistent with the finding in the political science literature that Americans' attitudes towards climate change have not substantially changed over the past two decades (Egan and Mullin 2017).⁴⁵

I also assess whether there are different migration choices among people in an area that burns repeatedly. To do so, I estimate [Equation 1.1](#) but, instead of comparing people in blocks that burn for the first time with those in blocks never burned, I compare people in blocks that burn for the second time with people in the never-burned blocks, people in twice-burned blocks with people in first-burned blocks, and people in thrice-burned blocks with people in first-burned blocks, where the control is within the same tract.⁴⁶ I find no difference in migration among people in the twice-burned and never-burned blocks. Indeed, I see a statistically significant negative effect on migration among people in a twice-burned

⁴³As an additional validation exercise between the two datasets, I estimate [Equation 1.1](#) using the Infutor and UCCCP datasets across the same time period: 2014-2021. Although the migration rate from the Infutor data is lower than that from UCCCP, they follow qualitatively similar patterns. The migration results can be seen in [Figure 4.12](#) and the adaptation results in [Figure 4.13](#).

⁴⁴I omit 2020 to abstract from the COVID-19-related migration changes.

⁴⁵The finding that people who experienced a wildfire between 2000 and 2010 are statistically significantly more likely to migrate also serves as evidence of the role of liquidity constraints since, before the 2008 financial crisis, all individuals were able to easily obtain a loan, even those with low credit scores. Thus, it could have been that they could more easily finance living in a safe area.

⁴⁶I count the number of fires starting from 1950, the year in the CAL FIRE data where there begins to have more populated entries.

block in comparison to people in a first-burned block, meaning that areas that have previously burned see less out-migration. There is also a statistically insignificant negative migration effect for people in blocks that experience the third vs. second fire. This could reflect that people respond differently to repeated burns. It could also mean that people living in areas that have previously burned are selected in the sense that they may be more tolerant of fires (Zabek 2024).

6 Discussion

This project provides novel empirical estimates of the extent of adaptive migration – the movement from risky to safe areas – and documents that it is minimal in the longer term. Using geographically granular and detailed individual-level data and a stacked DID event study empirical strategy, I can precisely identify who is impacted by a wildfire and categorize their location decision to causally identify the impact of a fire on adaptive migration, which has previously not been estimated in the literature. Overall, although people are more likely to move after a wildfire, they are not more likely to move to a safe area in the longer term.

Although my estimates for adaptive migration in the longer term are low, understanding why this is the case has important welfare implications. For adaptive migration to occur, people need to be able to assume the moving costs and to both want to and be able to migrate to safe areas. Identifying the factors which prevent adaptive migration has important consequences for policy design. I provide suggestive evidence that financial constraints may be a factor that restricts adaptive migration. People with high credit scores and people who are more likely to receive payouts immediately after the fire (i.e., those whose homes are damaged) are statistically significantly more likely to have moved to low- or medium-wildfire-risk areas after four years. These financial constraints can be further distinguished into liquidity constraints – which could prevent people from moving because they cannot

afford the initial moving costs – and solvency constraints – which could prevent people from moving because they cannot afford to live in the safe areas. Those with high credit scores could be less likely to be bound by the solvency constraint so they can live in safer areas in the longer term. Those who received the insurance payments are less likely to be liquidity constrained, and they could also be less likely to be solvency constrained depending on the payment amount and how they use it (i.e., investing it to increase savings vs. spending it immediately as consumption).

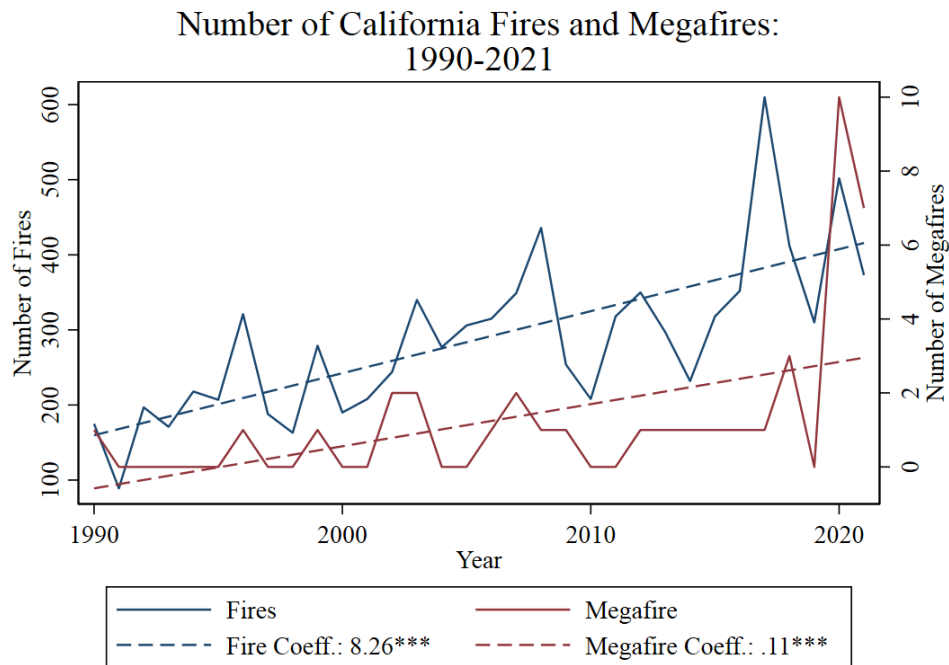
There could be other explanations for why there is too little adaptive migration. People could lack information about the risks of wildfires or about safe areas, which could prevent them from moving adaptively. Although I see some suggestive evidence that people do have some sense of the wildfire safety of an area because I find an increase in the short-term likelihood of moving to safe areas, more detailed survey data about people’s beliefs would be helpful for understanding what information they have about wildfire risk. Another explanation could be that the wildfire does not necessarily impact the individual’s labor market. Goncalves and Vieira (2015), Yagan (2019), among others, find a limited migration response to a labor market shock. In the case of a wildfire, one might expect even lower migration responses since the impacted individuals are still tied to their jobs in the area. I define migration at a much finer level than the commuting zone, so my results can be consistent with their findings: People may move but not move far because they retain their old jobs. Further work on the impact of wildfires on local employment and whether there are differential migration responses based on this dimension would be illuminating. This paper explores financial constraints as one potential mechanism, and assessing the others is key for future research.

In conclusion, although there is evidence that individuals who experience a fire are more likely to move away, they are not more likely to move to a safe place. Financial constraints seem to be one reason why people do not move adaptively. Understanding the other factors

preventing people from moving to safe areas is crucial for designing effective policies.

7 Figures

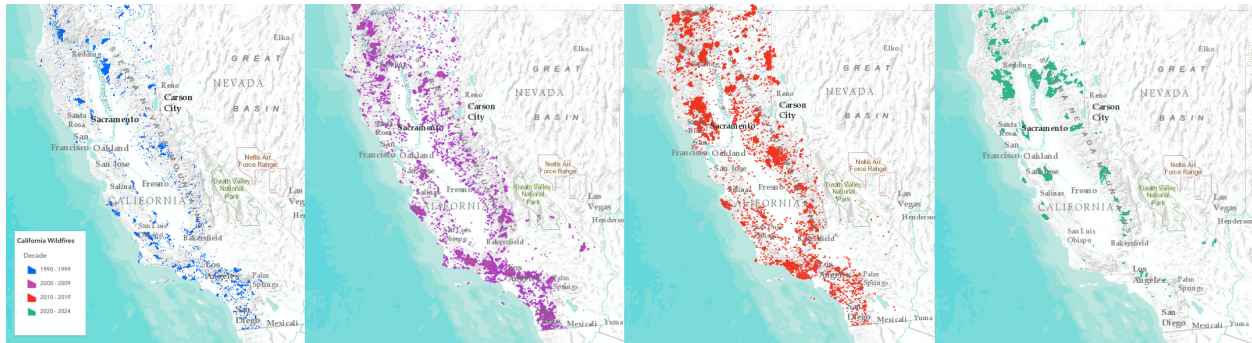
Figure 1.1: Number of California Fires and Megafires over Time



Notes: The California fires data come from CAL FIRE. The original data span from 1900-2021, though the data quality increases after the 1950s. From 1990 to 2021, each year is associated with 8.26 additional fires. There has also been an increase in megafires, which are fires that burned over 100,000 acres. Though the increase in their number is small, the destruction from each megafire is substantial. There was an extraordinary number of megafires in 2020, though even before then, there was a slight increase in the number of expected megafires.

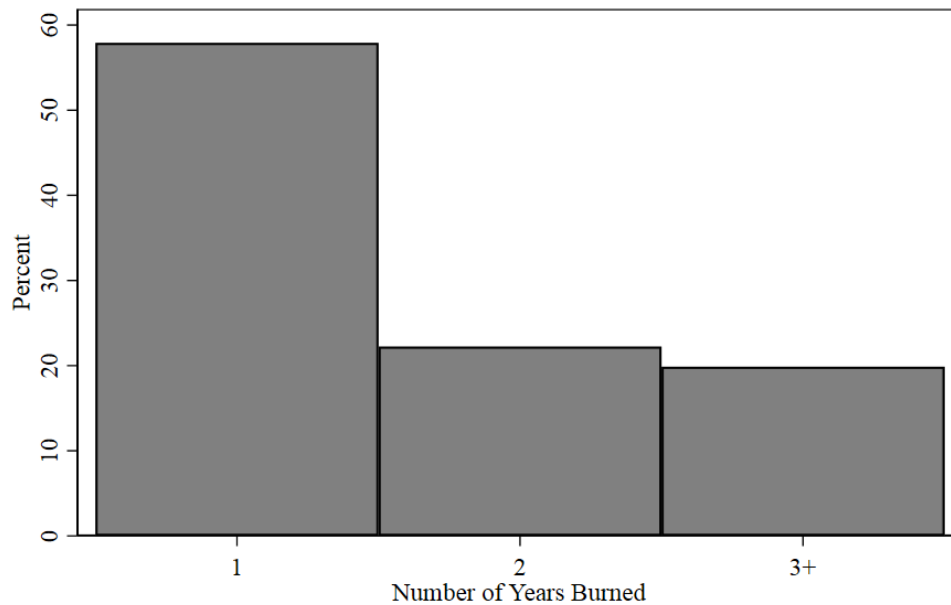
Figure 1.2: California Wildfires Burn Areas

(a) Map of Fires in California (2000-2021)



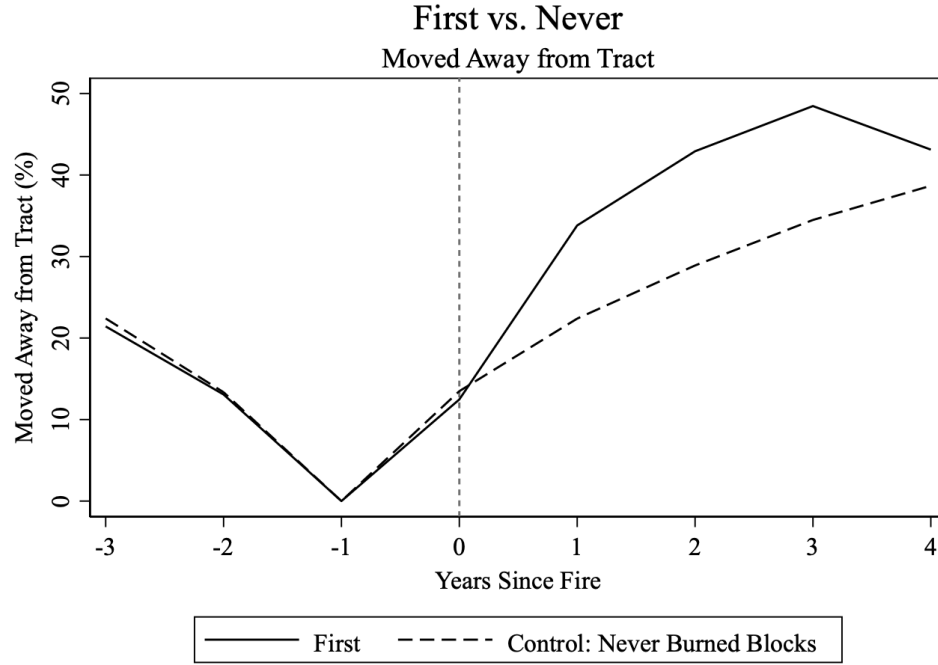
Notes: This map shows the areas that are burned in California throughout the decades, beginning in the 1990s (blue), 2000s (purple), 2010s (red), and 2020-2024 (green). Source: CAL FIRE Historical Wildfires Data and created in ArcGIS.

(b) Number of Years a Census Block Burned (1950-2021)



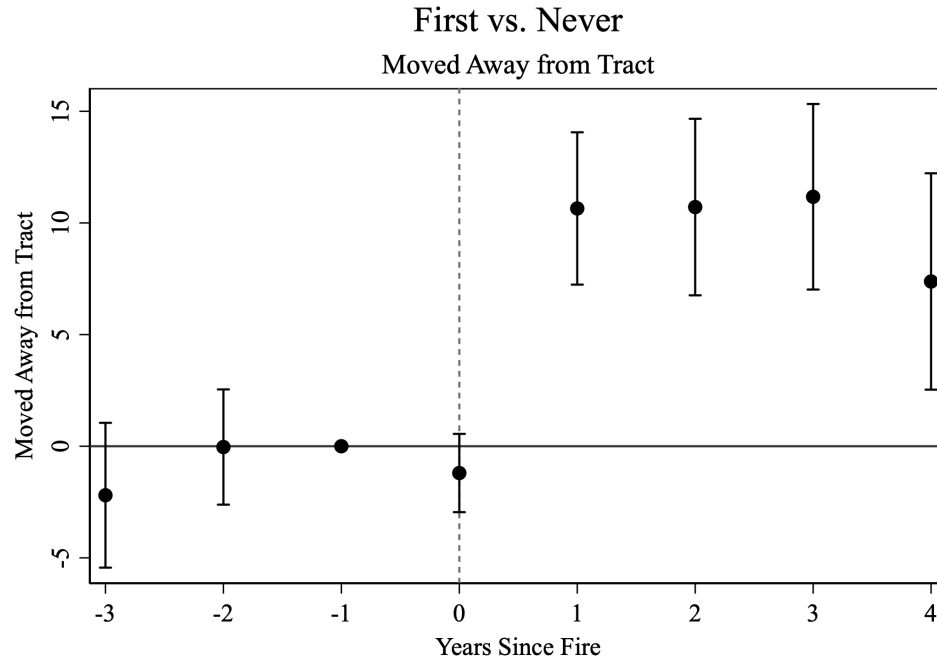
Notes: This graph shows the percentage of census blocks burned by a fire in one, two, or three or more years. Of the sample of census blocks that ever burned from 1950 to 2021, the majority (almost 60%) burned only once. Approximately 20% burned twice. The remaining 20%, which burned more than three times, show that certain blocks face repeated wildfire risk. The data are from CAL FIRE.

Figure 1.3: Migration Averages for First vs. Never Burned



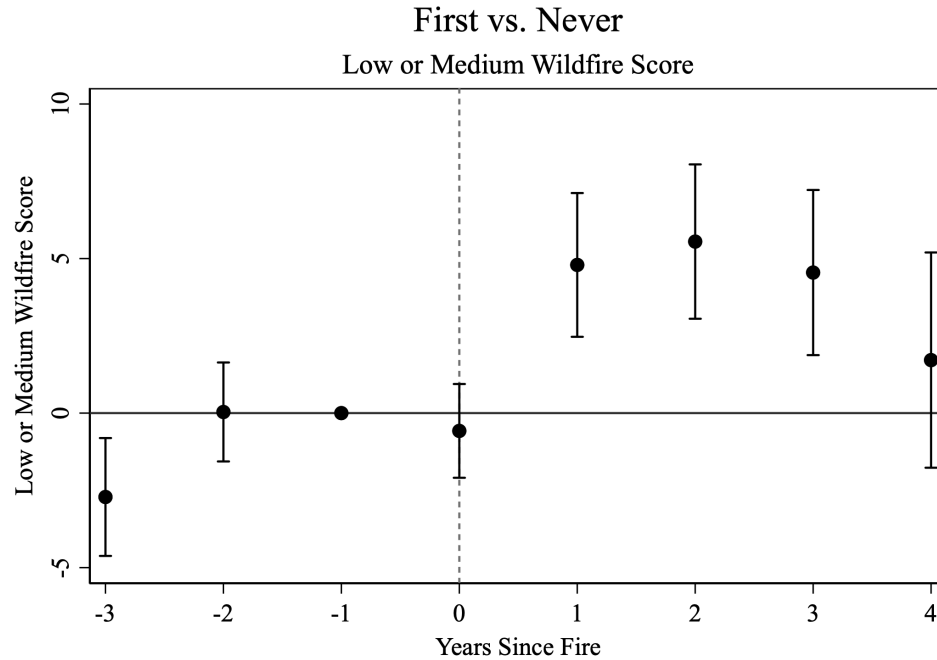
Notes: This graph shows the percentage of individuals whom I observe in a different census tract in the UCCCP data than where they were the year before the first fire (“Moved Away from Tract”) for the treated and the control groups using the UCCCP data. The treated group consists of individuals living in blocks that are burned for the first time. The control group consists of those living within the same census tract as the first burned blocks, but they are living in blocks that are never burned. Time is standardized to be relative to the year of the first fire that impacted the block. Before the fire (i.e., time 0), the migration rates are the same and parallel, lending credibility to this empirical strategy. The decrease in year four for the treated group is suggestive evidence that individuals return to the tract after moving away.

Figure 1.4: Moved Away from Tract after First Fire



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 1.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy. There is a sharp increase in the likelihood of moving away starting the year after the fire, and this effect persists for four years after the fire. [Figure 1.3](#) shows suggestive evidence that the dip in the fourth year is from people who left the burned blocks moving back.

Figure 1.5: Wildfire Risk after First Fire



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 1.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. There are minimal differential likelihoods of an individual being in a safe tract before the fire, lending credibility to this empirical strategy. There is an increase in an individual’s likelihood of being in a safe tract after the fire, though the effect dissipates by four years after the fire.

8 Tables

Table 1.1: First vs. Never Burned Summary Statistics

	Treated		Control		Difference	p-value
	Mean	SD	Mean	SD		
Demographic Characteristics						
Age	52.81	(18.26)	50.94	(18.08)	1.86	0.00
Male	0.50	(0.50)	0.50	(0.50)	-0.00	0.82
Married	0.63	(0.48)	0.63	(0.48)	-0.01	0.59
College	0.68	(0.47)	0.64	(0.48)	0.04	0.01
Duration	1.82	(1.55)	1.70	(1.78)	0.12	0.02
Moved within Year						
Tract	12.33	(32.89)	13.04	(33.68)	-0.71	0.48
County	5.34	(22.48)	5.57	(22.94)	-0.24	0.73
State	1.84	(13.45)	1.61	(12.57)	0.23	0.57
Tract Characteristics						
Wildfire Risk Score	81.86	(30.56)	76.35	(32.24)	5.51	0.00
Overall Risk Score	79.32	(15.81)	79.51	(19.44)	-0.19	0.69
Expected Annual Loss Score	80.13	(16.42)	80.25	(18.88)	-0.12	0.81
Number of Insurance Providers	59.95	(9.20)	60.23	(11.13)	-0.29	0.40
Percent FAIR Policies	0.74	(1.21)	0.52	(1.10)	0.23	0.00
Financial Characteristics						
Credit Score	701.30	(115.15)	694.48	(122.24)	6.81	0.05
Number of Open Credit Cards	2.44	(2.88)	2.54	(3.02)	-0.10	0.25
Total Credit Limit	21,145.96	(23,184.96)	20,486.56	(22,966.83)	659.41	0.44
Total Payment	1,314.02	(2,243.73)	1,278.34	(5,915.75)	35.68	0.67
Number of Open Loans	4.50	(4.43)	4.55	(4.51)	-0.05	0.74
Number of Open Auto Loans	0.32	(0.60)	0.33	(0.60)	-0.01	0.57
Has Mortgage Loans	0.29	(0.46)	0.28	(0.45)	0.01	0.47
Bankruptcy	4.69	(21.16)	4.67	(21.09)	0.03	0.97
Number of Current Delinquencies	0.07	(0.36)	0.07	(0.42)	0.01	0.50

Notes: This table reports the average and standard deviation for a variety of characteristics separately for the treated and control groups. The treated group consists of individuals living in census blocks burned for the first time. The control group consists of people living within the same census tract as the first-burned blocks but in census blocks that are never burned. Demographic characteristics, migration outcomes, and financial characteristics come from the UCCCP data. The tract characteristics come from the FEMA wildfire risk index and California Department of Insurance (CDI).

Table 1.2: Percentage of Population That Moves by Fire Type

((a)) First vs. Never						
	First vs. Never		First with Aid vs. Never		First without Aid vs. Never	
	Treatment	Control	Treatment	Control	Treatment	Control
Different Tract						
Year After Fire	23.10	13.74	28.99	13.29	14.73	14.07
Four Years After Fire	44.21	37.07	45.56	36.65	41.88	37.08
Returned to Tract of Fire						
Four Years Later	4.51	1.63	5.93	1.44	2.09	1.73
Moved Year of Fire, Returned Four Years Later	19.63	12.32	21.62	11.31	12.50	12.68

((b)) Megafire vs. Never						
	Megafire vs. Never		Megafire with Aid vs. Never		Megafire without Aid vs. Never	
	Treatment	Control	Treatment	Control	Treatment	Control
Different Tract						
Year After Fire	26.52	11.59	26.35	11.91	27.78	11.14
Four Years After Fire	39.59	31.01	39.86	30.15	36.84	33.33
Returned to Tract of Fire						
Four Years Later	3.05	1.42	1.45	1.25	7.02	1.16
Moved Year of Fire, Returned Four Years Later	12.77	14.29	6.67	12.77	25.00	11.54

Notes: This table reports the percentage of the population that I observe in the UCCCP data in a different census tract or returned to the census tract of the fire at different points in time after the fire. Panel (a) refers to people living in census blocks burned for the first time and people living within the same census tract as the first-burned blocks but in blocks that are never burned. Panel (b) refers to people living in census blocks burned by a megafire and people living within the same census tract as the megafire-burned blocks but in blocks that are never burned. The “Different Tract” category reflects the percentage of people whom I observe in the data in a different tract from where they were the year before the fire at two points in time: one year after the fire and four years after the fire. The “Returned to Tract” category reflects the percentage of people whom I observe in the data who moved away (i.e., are observed in a different tract) but then came back to the tract they were in the year before the fire. The “Four Years Later” variable includes the percentage of the total population who moved away and then returned. The “Moved Year of Fire, Returned Four Years Later” variable includes the percentage of people who moved away the year of the fire but had returned by four years after the fire.

Chapter 2

Sustained Burn: Analyzing Post-Wildfire Financial and Location Outcomes

1 Introduction

Unexpected disruptions, such as natural disasters, can dramatically alter the trajectory of people's lives. Individuals can try to protect against such uncertainty in order to minimize changes in consumption by purchasing insurance, putting aside savings, and using financial institutions, such as credit cards and loans. People may also respond to an unexpected shock by moving to a different neighborhood (Bilal and Rossi-Hansberg 2021). This change in location may have an impact that extends beyond the initial event since people do not move often. As natural disasters become more common and severe with climate change (Berlemann and Steinhardt 2017; Benevolenza and DeRigne 2019; Yao, Kabundi, and Mlachila 2023), the number of people that are expected to be impacted will also grow.

Residential neighborhoods play a crucial role in shaping people's lives. The characteristics

of neighborhoods vary greatly across the United States, with some areas having high levels of poverty, crime, and pollution, while others offer better schools, lower crime rates, and more job opportunities. The neighborhoods in which people live can have a significant impact on their well-being and life outcomes (Chetty, Hendren, and Katz 2016; Chyn and Katz 2021). Moving to a different neighborhood can be a way to improve one’s living conditions and access to resources. However, the decision to move is often influenced by various factors, including financial constraints, social networks, and personal preferences.

This paper investigates the financial impacts and characterizes the location decisions for people that are affected by a wildfire. Wildfires can upend lives by destroying homes, disrupting labor markets, and fracturing social ties. In the aftermath, people’s living conditions and access to essential services often change dramatically. Whether to move depends on factors such as the fire’s intensity, the scope of government assistance, and each individual’s financial ability to relocate. They may need to rely on financial instruments, such as credit cards or loans, to survive. Understanding all of the ways that people respond to these shocks is necessary to fully understand the costs associated with these natural disasters.

Natural disasters could, in theory, push people to move to “better” neighborhoods and have positive impacts. After Hurricane Katrina, people experienced longevity gains from locating to areas with higher life expectancies (Deryugina and Molitor 2021). Nakamura, Sigurdsson, and Steinsson 2022 finds that the children of families whose homes were destroyed by a volcanic explosion on an Icelandic fishing island had higher income from moving to an area with more diverse occupations and choosing a well-paying career. The Moving to Opportunity (MTO) experiments in which individuals were randomly assigned to receive housing vouchers to move to lower-poverty neighborhoods found that children who moved had better educational outcomes and were less likely to be incarcerated (Chetty, Hendren, and Katz 2016; Chyn and Katz 2021). However, the MTO experiment found that adults who moved did not have higher income or employment, though their mental and physical health

improved (Chetty, Hendren, and Katz 2016; see Chyn and Katz 2021 for an overview).

The existing literature investigating the impact of natural disasters on household finance has found mixed results. Prior studies typically focus on hurricanes and document financial setbacks (Gallagher and Hartley 2017; Deryugina, Kawano, and Levitt 2018; Bleemer and van der Klaauw 2019). Some studies actually find some evidence for financial improvement in the household and of the county overall, likely because of the role of government aid (Bleemer and van der Klaauw 2019; Boustan et al. 2020; Henkel, Kwon, and Magontier 2022). The paper closest to this one is McConnell et al. (2021), which investigates how wildfires affect migration in and out of burned areas and household finances up to two years after the event. My paper builds on this by extending the time period to four years after the fire and including a variety of characteristics to understand where people are moving. My results complement their research in that I also find large and sustained decreases in homeownership rates, even four years after the wildfire. However, I see increases in credit distress, such as falls in credit scores and increases in the number of active collections, which contrast with their findings of improvements, though the changes are not statistically significant. My results suggest that there are long-lasting, negative impacts of wildfires on household finance.

This also contributes to the literature on how people choose where to relocate after a natural disaster. People can have many considerations on how they decide where to live, such as the location's labor market opportunities, cost of living, and school quality. Winkler and Rouleau (2021) shows that after extreme wildfires, people tend to move to other areas with high environmental amenities. Wang (2025) finds that after a wildfire, people are moving away from the burned area, but they are not moving to lower-risk areas on average, though those who are least likely to be financially constrained do. I find that after a wildfire, people tend to move to places that are more expensive with higher housing values. The places they move to also have amenities that particularly benefit children: they have higher upward mobility, higher education quality, higher shares of college educated people, and

higher median income. They also have higher pollution levels, a disamenity, though there are not higher incidences of asthma. The combination of the financial outcomes and the location choices suggests that people are moving to areas with better amenities, though it comes at the cost of homeownership.

The rest of the paper is as follows. Section 2 details the data used in this analysis, and Section 3 explains the empirical strategy. Section 4 explores the financial consequences of wildfires. Section 5 presents the migration results and analyzes the characteristics of their location. Section 6 concludes and includes extensions for future research.

2 Data

To explore the quality of the locations where people move after experiencing a fire, I use detailed location data to characterize the quality of the destinations.¹

2.1 Fire

I use data from the California Department of Forestry and Fire Protection (CAL FIRE) to define the census blocks that are burned. Responsible for California’s fire suppression efforts, CAL FIRE keeps careful and complete records of wildfires within the state. I merge shapefiles of perimeters of wildfires from 1950 to 2021 with maps to identify which census blocks were affected by wildfires. I define the fires at the census block-level because this is the finest geographic unit for which credit data are available. A census block is smaller than a census tract, which is in turn smaller than a county; at the time of the 2010 census, there were 710,145 census blocks, 8,057 census tracts, and 58 counties in California (Bureau 2021).²

¹Some of these data sources are used in previous chapters of my dissertation (i.e. Section 3) from Chapter 1 so I repeat those sections here.

²I list the categories for the 2010 census because the credit data defines locations using this census. The US Census Bureau defines these census geographies in order to create statistical areas bounded by “visible features such as roads, streams, and railroad tracks, and by nonvisible boundaries such as property lines, city,

2.2 Migration and Financial Characteristics

To track where people are moving, I use the University of California Consumer Credit Panel (UCCCP). The UCCCP used in this analysis includes credit and demographic data for a longitudinal panel of everyone who has ever lived in California and has a credit record. The data have consistent census block information from 2014-2021.³ There are approximately 40 million consumers, though I currently use a 1% random sample of the dataset. The data include household members and cosigners as well as their location at the zip code and census block level. They also include demographic information about the consumers (including race, gender, age, education, and occupation), credit scores, and raw tradeline-level information about each loan or collections item (including payment history, credit limits and balances, mortgage delinquency, and foreclosure). This dataset allows me to measure mobility since I can construct a move based on whether the individual is in a different block from the previous quarter. The tradeline data allow me to study the financial impact of the experience of a fire and to consider heterogeneity based on financial situation.

2.3 Location Data

In order to characterize the quality of the destination, I utilize a variety of datasets that are intended to measure various amenity aspects of the location: (1) The Opportunity Atlas, (2) The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviron), and (3) Zillow. I also have measures from the 2010 Census to complement these datasets.

township, school district, county limits and short line-of-sight extensions of roads” (Rossiter 2011). Census tracts have an ideal population of 4,000 people (Bureau 2022). Half of the census blocks within the United States are smaller than a tenth of a square mile (Lynch 2020).

³The data span 2004-2023, but the census block designation changes from the 2000 census definition of blocks for 2004-2014 to the 2010 census definition for 2014-2021 and to the 2020 census definition for 2021-2023. Even when using crosswalks between the different definitions to standardize, I see a spike in migration in years where the census designation changes. This is because there is not a clean mapping across the censuses’ characterization of areas: Some census blocks split into smaller entities, while others are combined. There are also higher rates of missingness for census blocks before 2010. Thus, for ease of analysis, I restrict my sample period to 2014-2021, which also coincides the occurrence of many large fires.

The Opportunity Atlas provides census tract-level data on a variety of measures intended to capture upward mobility. Combining anonymized longitudinal census data, the Opportunity Atlas team constructs metrics that are expected to impact children’s chances to rise out of poverty, including household income, cost of living, and quality of the school system. They calculate the likelihood that a child growing up in that tract will reach the upper quintile of income in adulthood. There are also specific measures of social capital, such as the percentage of single-parent households, the percentage of adults with a college degree, and the percentage of individuals who are foreign-born. They also have labor market indicators including the number of jobs, the job density, and the likelihood of being employed. These measures are calculated for a given moment in time so they do not take into consideration how these measures change over time.

The Office of Environmental Health Hazard Assessment ranks California census tracts on the environmental quality in the CalEnviron data, including data about pollutants, socioeconomic factors, and health conditions. It combines multiple data sources to calculate a score about the exposure to a variety of census tract characteristics and then translates this to a percentile so each census tract can be compared with others in California. The pollution data includes information about traffic and PM2.5 – the particulate matter which has been identified to pose significant health risk because its small size can cause respiratory issues. The pollution burden measure incorporates ozone, PM2.5, diesel, drinking water, lead, pesticides, toxic releases, traffic, cleanup sites, groundwater threats, hazardous waste, impaired water bodies, and solid waste. The socioeconomic data includes measures of unemployment, poverty, and housing burden, which captures the households that are both low-income and highly burdened by housing costs. To standardize the measures, the percentile version of each variable is used as the outcome, which transforms the scores that is calculated from combining multiple data sources into a distribution for all California census tracts. The data for the CalEnviron model come from national and state sources. Since this dataset was released in

2021, this is an *ex post* measure of environmental conditions. Although this is only available for California, the majority of migration after a wildfire happens within the state.

Zillow provides housing indices to capture how home values and rents change over time. The Zillow Home Value Index (ZHVI) reflects the typical value for homes in a zipcode (measured between the 35th to 65th percentile range) from January 2000 - March 2025. The Zillow Observed Rent Index (ZORI) is intended to capture the typical observed market rate rent (a repeat-rent index of the mean of listed rents that fall in the 35th to 65th percentile range for the rental stock of the area). These data capture the costs associated with housing, which comprises a substantial proportion of living costs.

3 Empirical Strategy

To quantify the causal effect of the fire, I conduct a stacked difference-in-differences (DID) event study, which relies on the identifying assumption that the trajectories of those in treated and control blocks would be parallel, but-for the fire.⁴

3.1 Stacked Difference-in-Differences Event Study

To quantify the causal effect of the fire, I conduct a stacked difference-in-differences event study, which compares the outcomes of people living in census blocks that are burned by a fire with those living in never-burned blocks within the same tract before and after the fire. This is a stacked regression because I take multiple fire experiments and standardize the time variable to be relative to when the fire occurred, which I define as time 0 ($r = 0$). By using this design, I can take advantage of multiple fire events and obtain greater power for

⁴This empirical strategy is used in other parts of this dissertation so I am repeating those sections here (i.e. Section 4 from Chapter 1), though my main definition of the fire treatment differs from the census block being burned for the first time to the census block being burned by a megafire.

my estimates.⁵

I estimate the following regression equation:

$$y_{i,f,t} = \mu_i + \sum_{r=-3, \neq -1}^{N=4} \lambda_{r(f,t)} + \sum_{r=-3, \neq -1}^{N=4} \beta_r \lambda_{r(f,t)} \times fire_{b(i,f)} + \gamma_t + X'_{i,t} \Gamma + \epsilon_{i,f,t} \quad (2.1)$$

The outcome variable ($y_{(i,f,t)}$) is defined for individual i at calendar time t and for fire f .⁶ I include fixed effects for the individual (μ_i) to remove time-invariant individual-level factors.⁷ I have relative time $\sum \lambda_r(f,t) = \sum \mathbb{I}[t - E_f = r]$ fixed effects, spanning from three years prior to four years after the fire and omitting the year before the fire as the reference year. Each term ($\lambda_r(f,t) = \mathbb{I}[t - E_f = r]$) is equal to one when it is r years from the year when the individual i experienced fire f , which is denoted as E_f . The fire variable ($fire_{b(i,f)}$) is an indicator variable equal to one if the individual i lives in a census block ($b(i,f)$) that is burned for the first time by fire f and equal to zero if she lives in a never-burned census block within the same census tract.⁸ The coefficients of interest are β_r , which capture how much the experience of the fire impacts the individual r years relative to the time of the fire. I normalize the difference between the treated and the control to be the year before the fire (event time -1). Because the fire is defined at the census block level ($b(i,f)$), I cluster standard errors at the block level (Abadie et al. 2023).

For robustness, I include calendar year fixed effects (λ_t) to control for general migration

⁵If there are multiple blocks that burn for the first time at different years within a tract, I duplicate the control blocks for each of those fire events so that the burned blocks each get a set of controls and relative years defined by the year of the fire.

⁶In my main specification, each individual who lives in a census block burned in my sample is represented one time. However, there are cases where an individual could live in a never burned block that serves as the control group for multiple fire events. This would happen for tracts that have different blocks within it that burn across different years. In those cases, following the stacked difference-in-differences literature, I would duplicate the observation, and so the same calendar year t and individual i data would serve as a control across two fires, f_1 and f_2 (Wing, Freedman, and Hollingsworth 2024). Thus, the data is unique at the individual, fire, and calendar year level.

⁷The individual fixed effects absorb the treatment fixed effects that are typical in this literature because the individual-level is more granular than the block-level, which is the level that the fire treatment is defined.

⁸This fire treatment is more granular than the treatments considered in other wildfire papers, which define the wildfire at the census tract level (An, Gabriel, and Tzur-Ilan 2023; McConnell et al. 2021).

trends over time. I include potentially time-varying controls ($X_{i,t}$) for the age category, marital status, whether the individual completed college, and the duration of time that individual i lived in her census block ($b(i, f)$) at the time of the fire because these variables are found to be associated with migration.

I define the fire treatment as in two ways: 1) by whether the block experiences a megafire (one that burns over 100,000 acres); and 2) by whether a block is burned for the first time. With global warming, wildfires are expected to become more severe, and areas not previously burned are expected to experience a wildfire. I use these definitions of fire to address these different consequences of climate change. The main fire treatment I consider is a megafire because these are the larger wildfires that are more likely to cause destruction. In my main specification, I define the control group as people living in blocks that never burned but within the same census tract as the fire.

3.2 Identification Assumptions

The DID event study relies on the assumption that the outcome trends of the treated and control groups would have been parallel in the absence of the fire. Since the exact trajectory of a wildfire is quasi-random because of exogenous factors such as wind direction, this assumption seems reasonable (Beverly and Forbes 2023). I can evaluate its plausibility by checking whether the burned and never-burned areas were on similar trajectories before the fire and thus whether the pre-fire coefficients are statistically significantly different from zero (Roth 2022).

Recent research suggests that since humans can cause wildfires, there is a relationship between building density and wildfire risk (Ang 2024; Kestelman 2024). This could pose a threat to identification if the outcome pretrends for areas with higher density are not parallel with those of lower-density areas. This is not a concern in my setting for two reasons. Although 90% of wildfires are caused by human activity (Radeloff et al. 2018), the specific

path of the fire within a census tract can be considered largely caused by external, exogenous factors such as vegetation, slope, and wind direction, particularly at a spatially granular level such as the census block (Collins et al. 2007; Collins et al. 2009; Thompson and Spies 2009; Estes et al. 2017; Rao et al. 2022). Thus, the exact path of the fire is random, even if there is concern about whether its starting point is random.

4 Financial Outcomes

Experiencing a wildfire can impact an individual’s financial situation. Beyond the destruction of material possessions which constitute a large portion of their personal wealth, people may rely on financial tools, such as credit cards and loans, in order to finance being temporarily or permanently evacuated and weather against disruptions to their employment. However, there is no evidence that people are opening new credit cards or loans to smooth their consumption after the wildfire. Instead, there is a decrease in the number of open loans after the wildfire, largely driven by fewer mortgages as individuals are less likely to be homeowners after the fire. There are harsher negative financial consequences for megafires, with sustained decreases in credit score and an increase in the number of active collections for debt.

The financial consequences differ depending on the type of fire, with lasting, negative effects seen even four years after an individual is affected by a megafire. One might expect that the more severe fires may negatively hit the individual’s financial situation and have higher migration rates. As seen in [section 5](#), the location destination characteristics are similar for individuals who are in a block that burns for the first time or in a block that is burned by a megafire, which is more severe, and the short-term migration magnitudes are higher for megafires. The financial distress is seen for megafires, suggesting that individuals are more financially impacted by these more destructive fires.

There is a large, sustained decrease in the person’s credit score after a megafire. Credit scores are important as they are used to determine the individual’s creditworthiness, which impacts her ability to access loans, credit cards, and other financial products as well as other services, such as housing and job opportunities. After a megafire, there is a 15-point decrease in credit score that can be seen even in year four (Figure 2.1). A drop is also seen after the first fire, though not as large and not consistently statistically significant (Figure 4.26). To put this credit score change in context, a removal of a bankruptcy flag – one of the most important indicators of potential risk for lenders – is associated with a five-point increase (Howard, Liebersohn, and Rodrigues 2025). The decrease after a megafire is three times as large and can be thought of as a sizable increase in the person’s financial risk to lenders.

Indeed, the number of loans open to the individual decreases after a fire for both first and megafires. This decrease of around 0.4 loans constitutes an approximately 10% decrease in the number of loans, since the average individual has around four open loans (Figure 2.2; Figure 4.27). This is statistically significant for three years, but becomes statistically insignificant by year four. There is a decrease in the number of mortgage loans (Figure 2.3; Figure 4.28), which is consistent with Biswas, Hossain, and Zink (2023), which finds that people are both paying off their mortgages and defaulting after a fire. I also see a decrease in the number of auto loans (Figure 4.29; Figure 4.30), suggesting that people may be delaying other purchases after a fire. There is also a decrease in the number of open student loans for megafires (Figure 4.31) that is not seen for first fires (Figure 4.32), which could be related to student loan forbearance programs that are available after a disaster (Aid 2025).

People do not seem to be using credit as a way to increase their liquidity after a fire. There is not an increase in the number of credit cards for either type of fire (Figure 4.33; Figure 4.34) nor is there an increase in the percent of credit utilized (Figure 2.4; Figure 4.35) or the number of credit card delinquencies (Figure 4.36; Figure 4.37). Indeed, there is actually a statistically significant decrease in the total credit balances three years after the

fire (Figure 4.38; Figure 4.39) without a statistically significant change in the credit limit (Figure 4.40; Figure 4.41). This suggests that people are not using credit cards to finance their consumption after a fire, though this may hide the increases that may happen immediately after the fire since this is aggregated to the annual level.

There is an increase in the number of active collections, signifying that debt is being collected. There is a sustained, statistically significant increase after a megafire (Figure 2.5). The increase is around 33% increase in the number of active collections, since this is a rare event. This is also a statistically insignificant increase after the first fire (Figure 4.42). These results suggest that the fire makes it more difficult for an individual to pay off existing debts or loans, leading to delinquency that triggers the involvement of active collection agencies. This can have serious financial consequences, such as further negative impacts on credit scores, the accumulation of late fees, and even potential legal action.

Financial distress is not just a short-term effect but can persist for years after the event. These results complement the findings in McConnell et al. (2021), which evaluates wildfires that cause widespread destruction for the United States up to two years after the fire. They also find significant drops in homeownership, though their effects are for major fires and concentrated in people over 60. Similarly, I find significant decreases in having a mortgage that last four years after the wildfire, an effect seen not just for people who lived in census blocks that burned by a megafire but also for those in blocks that burned for the first time. Moreover, they did not find decreases across various measures of credit distress, and even some improvements, though they are not statistically significant. In contrast, I find sustained, statistically significant decreases in credit score and increases in debt collection after a megafire, highlighting the long-term, negative financial consequences that destructive wildfires can have on affected individuals. This is particularly concerning for individuals who may already be vulnerable due to pre-existing financial conditions or those who are unable to recover quickly from the economic shock of a wildfire. The sustained decrease in credit

scores and the increase in debt collection activities indicate that the financial repercussions of wildfires can have lasting effects on individuals' creditworthiness and overall financial health.

5 Location Results

In this section, I characterize the locations of where people move to after experiencing a wildfire. People are more likely to move to areas with amenities that are considered to be better for children: they are more likely to be in census blocks with higher upward mobility, higher schooling quality, and higher shares of the population with at least a college education. The areas also have higher median income, higher median housing values, and higher foreign shares. The destination locations have some disamenities, most notably an increase in pollution.

5.1 Amenities for Children

The choice of neighborhood can have an especially long-lasting impact on children. [Figure 2.6](#) shows the results from estimating [Equation 2.1](#) for blocks that were burned by a megafire and [Figure 4.43](#) shows for blocks that were burned for the first time, plotting the β_r coefficients. The measure of upward mobility comes from the Opportunity Atlas and is measured as the probability that a child in that census tract will reach the top 20% of the national income distribution in adulthood. For those impacted by a megafire, there is a large increase in upward mobility that continues even four years after the fire. Individuals in blocks that burn for the first time are also more likely to be in higher upward mobility areas, though this effect dissipates by the fourth year.

People living in census blocks burned by wildfires are also more likely to be in areas with higher schooling quality, measured in the Opportunity Atlas as the average school district level standardized test scores in 3rd grade. [Figure 2.7](#) and [Figure 4.44](#) show that individuals

in blocks that are burned by a megafire and for the first time, respectively, are more likely to be in areas with higher schooling quality, even four years after the fire.

After a wildfire, people in blocks that burned are more likely to be in areas that are more educated, defined as the share of residents with at least a college degree from the Opportunity Atlas. [Figure 4.45](#) and [Figure 4.46](#) show that individuals in blocks that burned are more likely to be in areas that are more highly educated, though this increase is statistically insignificant by the fourth year for both definitions of wildfires. Though the wildfire pushed people to move away from their chosen homes, there could be potential benefits to their children in terms of the quality of their destination neighborhoods.

5.2 Labor Market

Individuals' location choices may also be impacted by their labor market. The wildfire may have impacted the labor market opportunities of the area, especially if the main industry is related to the physical environment, such as agriculture or tourism. Moreover, people may choose to locate nearby in order to keep their jobs. To investigate this, I run the same stacked difference-in-differences event study regression from [Equation 2.1](#) with the outcome being an indicator for whether the individual lives in a census tract within the same commuting zone (CZ) as the burned location. [Figure 2.8](#) shows an increase in likelihood of being in a different CZ for individuals in blocks that are burned by a megafire, which remains statistically significant in the fourth year. [Figure 4.47](#) shows the same initial increase for individuals in blocks that burned for the first time, though this is no longer statistically significant by the fourth year after the fire. This is not solely driven by migration since the same effects are not seen when assessing whether individuals moved within a CZ compared to their never-burned peers ([Figure 4.48](#); [Figure 4.49](#)).

These results suggest that people are moving to new labor markets. To assess the quality of these labor markets, I consider aspects of the destination location using data from the

Opportunity Atlas, such as the median household income, the number of jobs, and the share of residents that are employed. They are in areas with higher median income, at least initially: [Figure 2.9](#) shows that people in blocks burned by a megafire are in areas that have higher household median income three years after the fire, which decreases and is statistically insignificant by the fourth year, and [Figure 4.50](#) shows a similar pattern for people in blocks burned by a fire for the first time. [Figure 4.51](#) and [Figure 4.52](#) show that individuals in blocks that burned are in destinations with a similar number of jobs as their never-burned counterparts. People in blocks burned by a megafire are statistically significantly more likely to be in areas with higher employment shares, though the increase is no longer significant in the fourth year ([Figure 4.53](#)). A similar pattern is seen for people in blocks that burned for the first time, though it is not statistically significant ([Figure 4.54](#)). These results suggest that experiencing a wildfire could push people into more advantageous labor markets, at least temporarily.

5.3 Pollution

Areas that have high environmental amenities, such as being close to wildlife and having cleaner air, are also more likely to experience wildfires. Indeed, the average pollution exposure in census tracts that experience a wildfire are in the bottom half of the pollution distribution in California, as measured by the CalEnviron data. After the wildfire, individuals in blocks burned by a megafire are more likely to be in a census tract with higher pollution the first three years after the fire that becomes statistically insignificant at year four, as seen in [Figure 2.10](#). There is a similar pattern for people in blocks burned for the first time in [Figure 4.55](#). This increase in pollution is partially driven by increases in traffic, which follows a similar pattern of statistically significant increases before becoming statistically insignificant in the fourth year for people living in blocks burned by a megafire and a first fire ([Figure 4.56](#); [Figure 4.57](#)). People’s exposure to PM 2.5, a measure of fine particulate matter in the air and

can pose significant health risks because they can be inhaled into the lungs, also increases. For individuals living in blocks that are burned by a megafire, the increase in exposure remains statistically significant even in the fourth year (Figure 4.58). There is a similar rise and fall in exposure for people living in blocks burned for the first time, though it is no longer statistically significant by the fourth year (Figure 4.59). Interestingly, when considering asthma, there is not an associated increase but rather a statistically significant decrease in the likelihood of the asthma percentile of the census tracts after the wildfire (Figure 4.60; Figure 4.61). However, it should be noted that these measures do not capture the variability and increase in pollution exposure that happens after a wildfire, but rather the measurement was taken in 2021 and applied retroactively. This could mean that though people living in blocks that were burned by a wildfire are more likely to be in areas with higher pollution, this could be an underestimate of their personal pollution exposure.

5.4 Housing

Housing is a large component of individuals' costs, and housing affordability can be an important driver of location choices. This can be especially true if the wildfire has damaged the person's home or made the area unlivable. Individuals living in blocks that burned for the first time and by a megafire are more likely to be in areas with higher housing value in the three years after the fire, as measured by the median housing value in the Census (Figure 2.11; Figure 4.62) and the Zillow Home Value Index (ZHVI) (Figure 4.63; Figure 4.64).⁹ After a megafire, there is a decrease in the likelihood that people in the burned areas have a mortgage, though the effect is statistically insignificant by the fourth year (Figure 2.12). This is consistent with McConnell et al. 2021 in that they also find a large decrease in mortgages two years after destructive wildfires. The decrease in likelihood of having a mortgage is sustained and statistically significant for individuals in blocks that burned for the first time

⁹However, the pre-trends of the ZHVI for the megafire suggest that adjusting for that difference may lead to the post-fire coefficients to be no longer statistically significantly different from zero.

(Figure 4.65). These results suggest that people are moving to areas with higher housing values, but are less likely to be homeowners.

Given that the impacted individuals are likely to be renters, I assess the rental prices of the areas, though there is not a clear story. I use two rent measures: the rent prices for two bedroom homes as measured by the Opportunity Atlas and the Zillow Observed Rent Index (ZORI). Because the ZORI measure changes over time whereas the Opportunity Atlas measures are static, I use this as my preferred rental measure since rents can change dramatically after disasters (Coalition 2024). There are no statistically significant changes in rent for people in blocks burned by a megafire in either rent measure (Figure 2.13; Figure 4.66).¹⁰ Although the rent prices from the Opportunity Atlas increases for three years for people in blocks that burn for the first time (Figure 4.68), there is no change in the ZORI (Figure 4.67). The CalEnviron data has a metric for housing burden, which is intended to capture the extent to which lower-income individuals in the census tract are able to afford housing. The housing burden does not increase after the wildfire for individuals in blocks that burned by a megafire (Figure 4.69) or in blocks burned for the first time (Figure 4.70). This could be because the increase in housing values is matched with an increase in income that is also seen in those areas (Figure 2.9; Figure 4.50).

Since individuals are less likely to have mortgages, although the housing values of the destination locations are higher, rental prices – which may be more relevant – did not meaningfully differ. However, it is difficult to know the changes in housing prices that the affected individuals face using these measures since there can be dramatic jumps in rental prices directly after the disaster. Individuals may double up with friends or family or stay in hotels. Thus, without directly observing where people go in the immediate aftermath of the fire, it is difficult to make claims about how much their housing costs are changing.

¹⁰However, the pre-trends of the Opportunity Atlas measure suggests that adjusting for that difference may lead to the post-fire coefficients to be positive and statistically significantly different from zero.

6 Conclusion

This paper investigates the financial and location consequences of wildfires. It seems that people living in areas that burned have long-term financial setbacks, though they may be pushed to neighborhoods with more opportunities that can be realized in the future. I find that people experience long-term financial setbacks, such as lower credit scores and higher rates of collections, even four years after the fire. I also see that people are more likely to move after a wildfire to areas with higher housing prices and better amenities particularly for children, such as areas with higher education quality and upward mobility. However, they are less likely to own their homes, suggesting that they may be trading off homeownership for these higher amenity neighborhoods. These results highlight that the effects of wildfires propagate far beyond the initial damages and can last years after the event, far beyond the coverage of most government aid programs.

Understanding the effects on people directly affected by the wildfires is the first part of unpacking the distributional consequences of wildfires. There is concern about the gentrification effects of disasters; after hurricanes, the incoming homeowners into affected areas are higher income than their predecessors (Graff Zivin, Liao, and Panassié 2023). There is an increase in home prices and square footage in areas impacted by wildfires in California five years post-fire as the homes are rebuilt (Issler et al. 2024), so a higher income is needed to purchase. On the other hand, areas with higher risk tend to have lower housing prices, suggesting that the people who are moving to these areas are not necessarily wealthier. I will conduct more analyses to better understand the recovery process. Currently, I run the difference-in-differences event-study regression following individuals over time with the housing prices as the outcome, using the Zillow Home Value Index (ZHVI) and Zillow Observed Rent Index (ZORI) (Figure 4.64; Figure 4.67). This regression also captures that individuals are moving to differently priced areas as well as the changes in the prices of the origin location. I can also run a regression on the location level to see how the housing prices

change over time. I would compare the ZHVI and ZORI of zipcodes that burned for the first time with those that never burned nearby, before and after the wildfire. This would allow me to see how the housing prices differentially evolved over time because of the wildfire. This is important because housing costs remain the largest expense for most families, and the way that the neighborhood changes can have a large impact on both the current residents and the types of people that will move in.

Disentangling the reasons why people are moving to high-risk areas is important for designing policy as these disasters become more common. If people are moving to these areas despite knowing the risk and they have the means to protect themselves (i.e. have high enough incomes to save), then there may be large moral hazard concerns for substantial government-provided aid. On the other hand, if people are moving to risky areas because they cannot afford to live elsewhere, then there are large equity concerns, especially since I do see evidence of long-term financial harm. In order to further investigate this, I will look at the characteristics of the people who are moving to these areas, after the wildfire occurs. I will look at where people are moving from: if the in-migrant composition changes substantially after the fire, then it suggests that the people who are moving to these areas are not the same as those who were there before. I will look at the cross-section of people who move into the burned area every year, before and after the fire happened. Some of the characteristics I will include are whether they are from out of state, whether they are renters or homeowners, and their credit scores. If there is a large increase in those who are moving from afar, this could be suggestive evidence that people who are not as familiar with the area are moving in, and that there may be a lack of information in their migration process. The change in renters or homeowners may reflect a change in the housing landscape. The credit scores show how financially constrained the newcomers may be, which is connected to how gentrified the area is becoming.

Assessing how the effects of the wildfire differ based on the the underlying characteristics

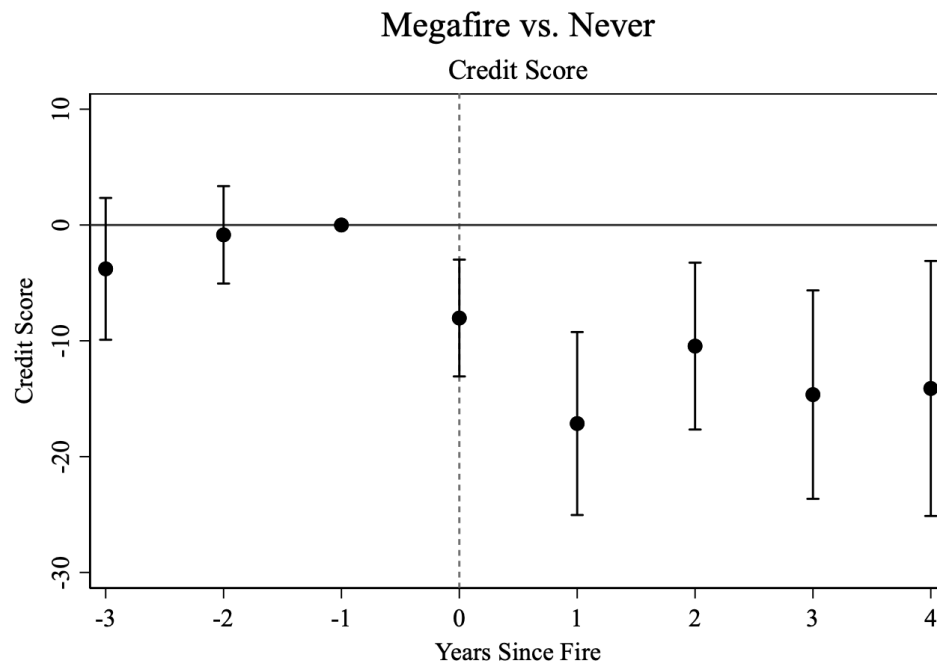
of the burned area will also provide important insights into equity concerns. In Los Angeles in January 2025, the consequences of the Pacific and the Eaton wildfires differ because of the underlying characteristics of the neighborhoods and the people who live there. Given that the residents chose to live there, the types of areas that residents of each neighborhood will want to live in after the wildfire will likely also differ. Indeed, a New York Times investigation using postal change of addresses data finds that people tend to relocate nearby: in the Palisades fire were mostly moving to Westside and Central Los Angeles, whereas those displaced by the Eaton fire moved to Pasadena. The difference in the average living costs between the areas will likely have an impact on the financial situation of the family, which will affect long-term outcomes. To more formally test this, I can look at the housing costs of the surrounding neighborhoods to the burned area to get a sense of the expected change in costs for the individual. I can then use this metric as the expected cost shock to the individual and use that as the treatment on their financial outcomes in the same difference-in-differences event-study framework. This will highlight how location can be used as an asset with traditional financial ones (Bilal and Rossi-Hansberg 2021). To see if there's a trade-off between staying close and financial considerations, I will measure the distance between the burned area and the individual's new location and the financial consequence, measured by changes in credit scores, the number of loans, credit card utilization, and the number of active collections.

In conclusion, the consequences of these natural disasters far extend beyond the wildfire destruction and can persist even beyond when the physical capital is repaired. People are relocating to areas with better amenities on some dimensions, though their financial situation is worse and they are less likely to be homeowners. These results help in understanding the distributional consequences of wildfires and how they can be mitigated.

7 Figures

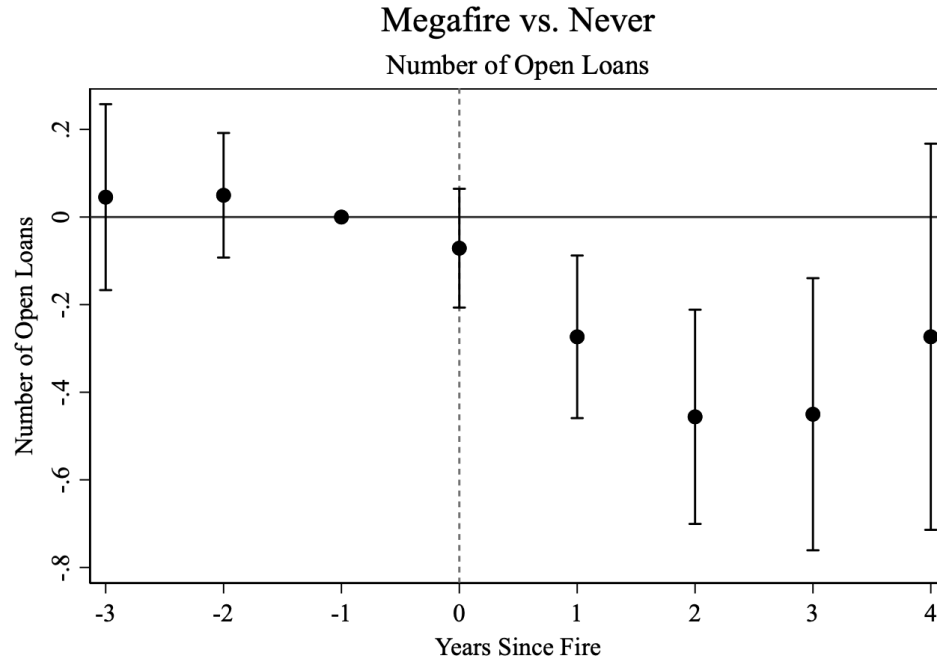
7.1 Financial Outcomes

Figure 2.1: Credit Scores after Megafire



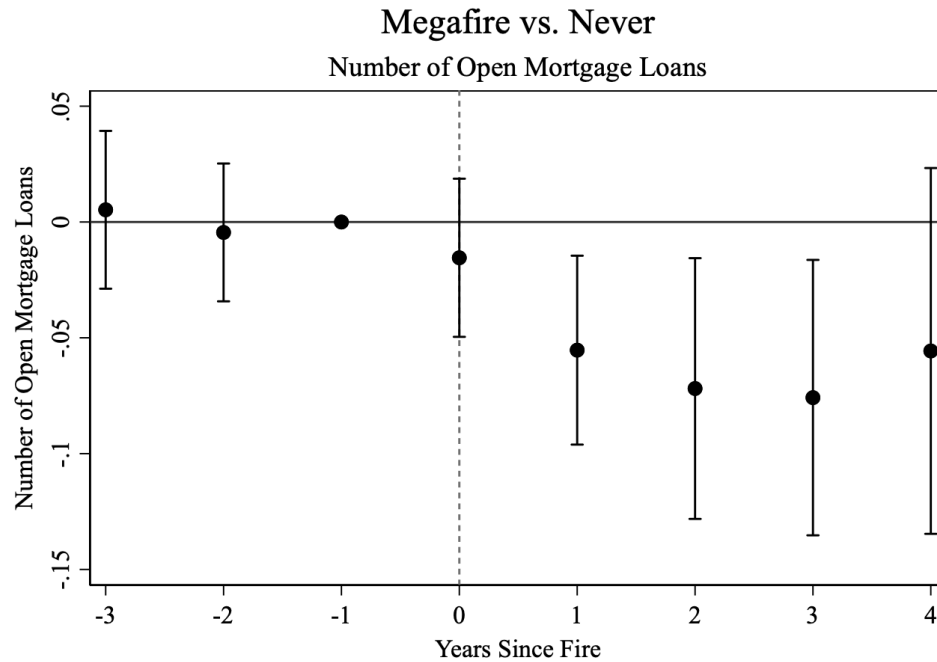
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the average credit score of an individual in a year. There are no statistically significant differences in credit score before the fire, lending credibility to this empirical strategy. There is a decrease in credit score after the fire, and the effect persists and is statistically significant even four years after.

Figure 2.2: Number of Open Loans after Megafire



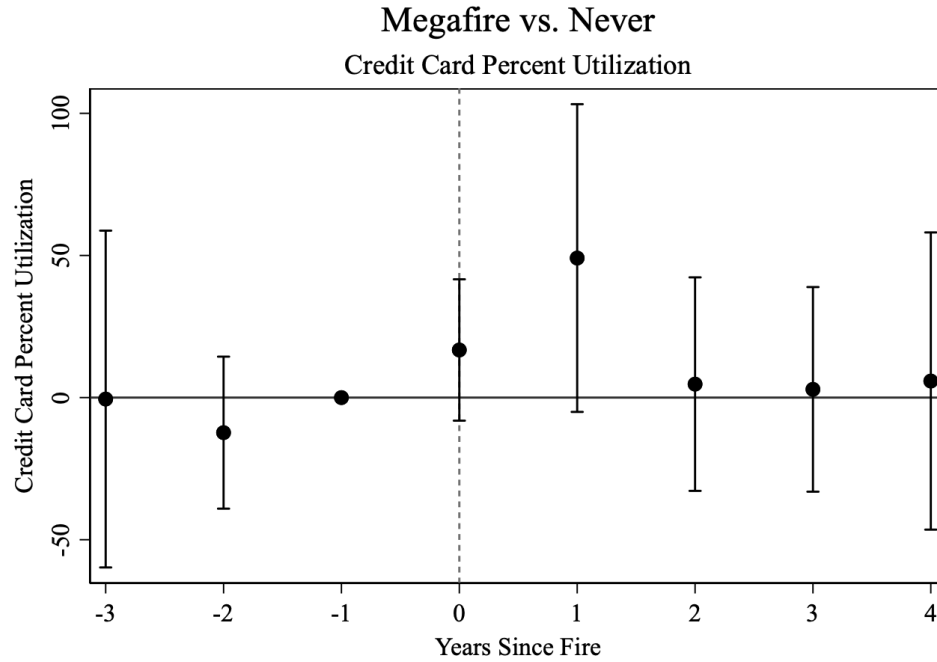
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of open loans that an individual holds. There are no statistically significant differences in the number of open loans before the fire, lending credibility to this empirical strategy. There is a decrease in the number of open loans after the fire, though the effect dissipates by the fourth year after the fire.

Figure 2.3: Number of Open Mortgage Loans after Megafire



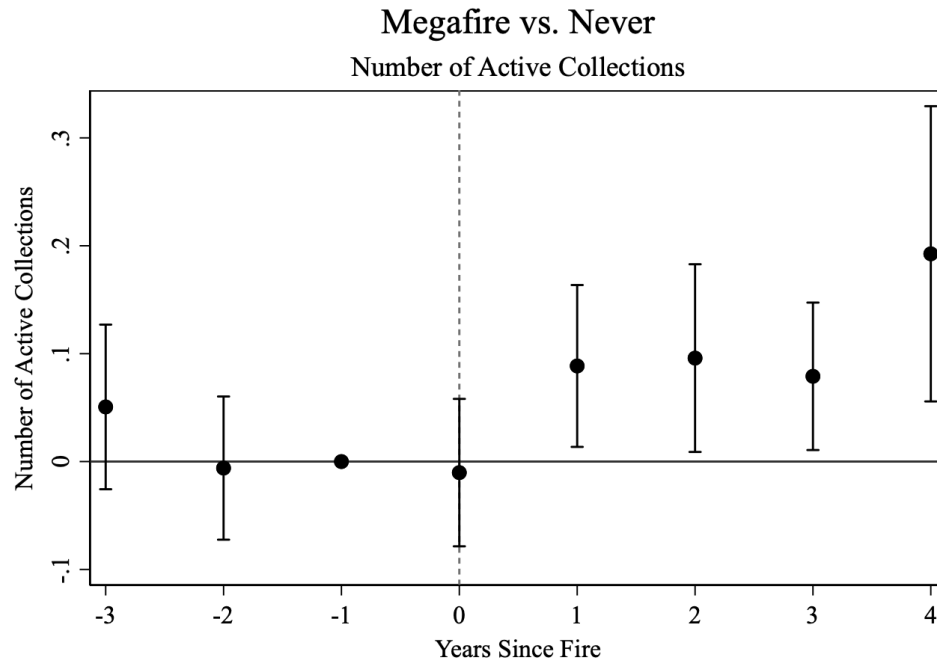
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of open mortgage loans that an individual holds. There are no statistically significant differences in the number of open mortgage loans before the fire, lending credibility to this empirical strategy. There is a decrease in the number of open mortgage loans after the fire, though the effect becomes statistically insignificant by four years after the fire.

Figure 2.4: Percent Utilization of Credit Cards after Megafire



Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the percent utilization of credit cards that an individual holds. There are no statistically significant differences in the percent utilization of credit cards before the fire, lending credibility to this empirical strategy. There is not a statistically significant change in the percent utilization of credit cards after the wildfire; though there is a large spike the year after the wildfire, it is not statistically significant.

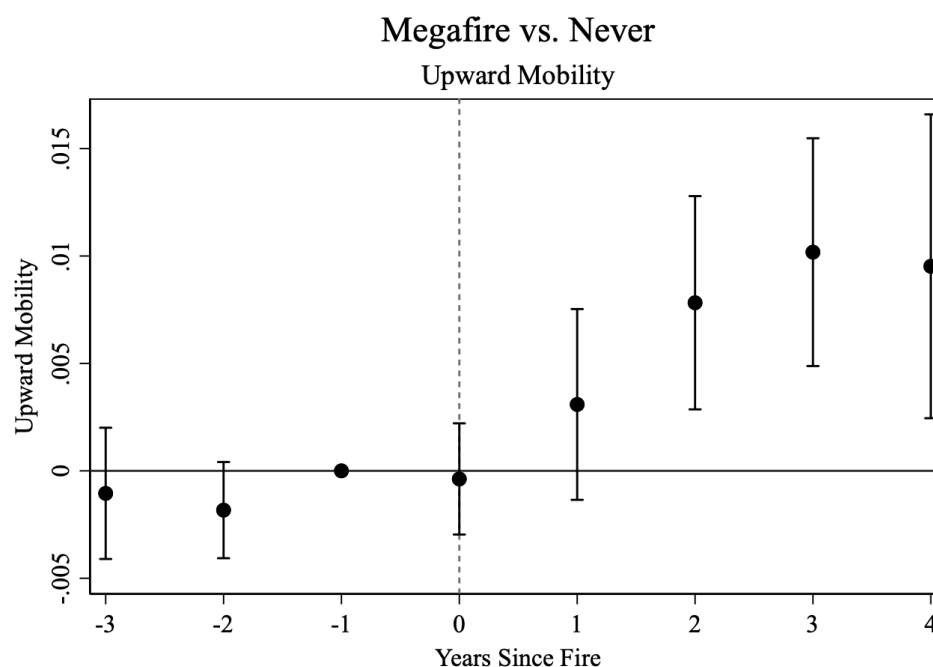
Figure 2.5: Number of Active Collections after Megafire



Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of active collections that an individual experiences. There are no statistically significant differences in the number of active collections before the fire, lending credibility to this empirical strategy. There is an increase in the number of active collections after the fire, and the effect persists and is statistically significant even after four years.

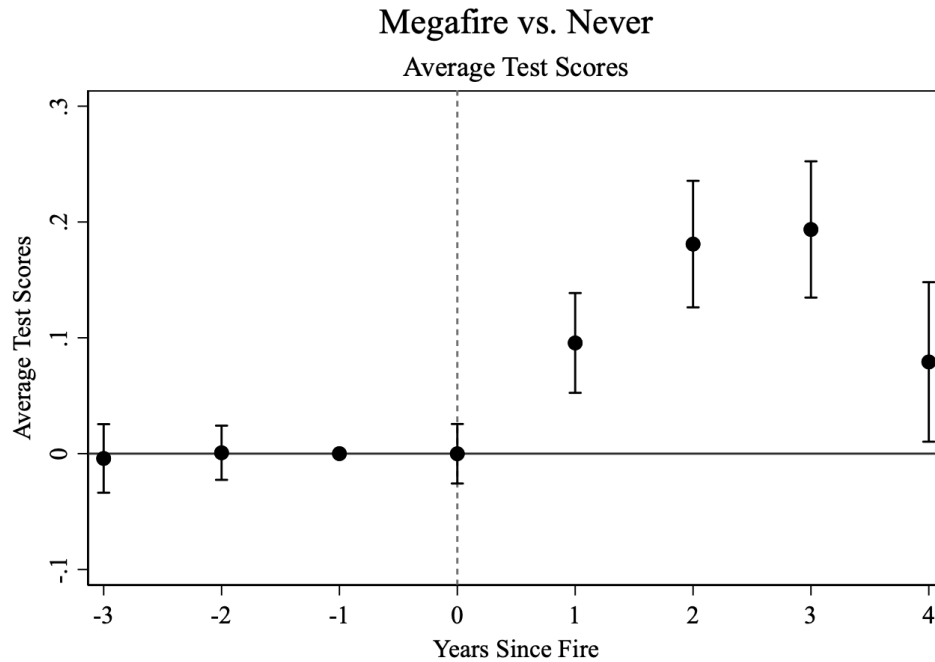
7.2 Location Results

Figure 2.6: Upward Mobility after Megafire



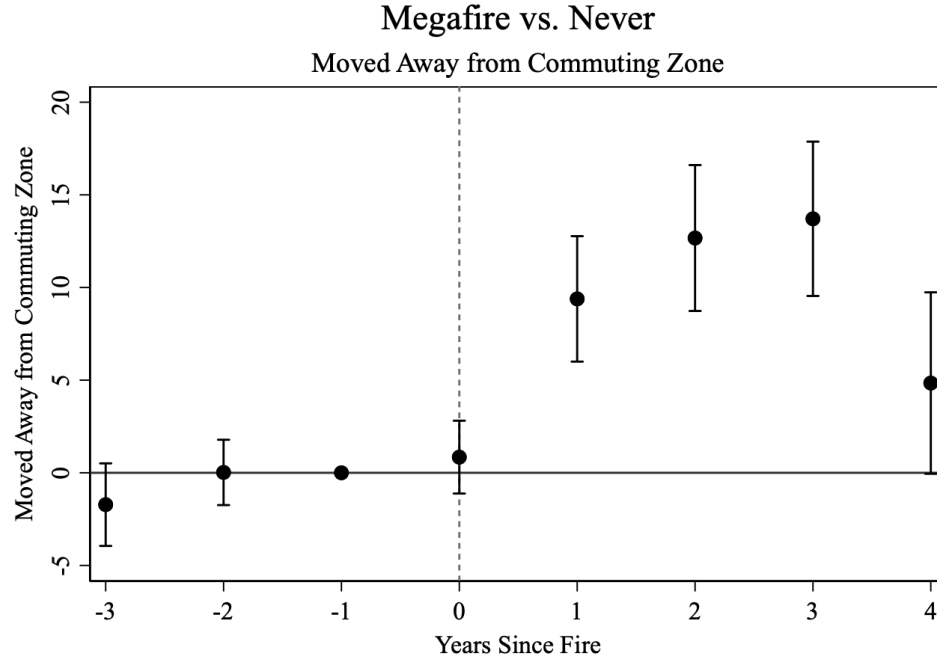
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is upward mobility and comes from the Opportunity Atlas, measured as the probability that a child in that census tract will reach the top 20% of the national income distribution in 2014-2015. There are no statistically significant differences in upward mobility before the fire, lending credibility to this empirical strategy. There is an increase in upward mobility after the fire, and the effect persists and is statistically significant even after four years.

Figure 2.7: Average Test Scores after Megafire



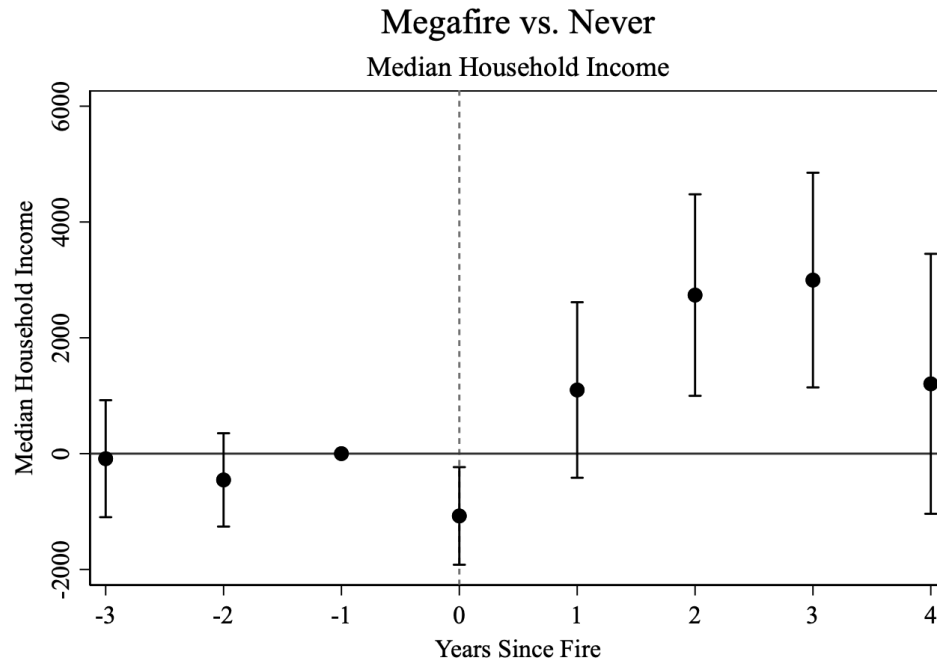
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is average test scores and comes from the Opportunity Atlas, measured as the average school district level standardized test scores in the third grade in 2013. There are no statistically significant differences in upward mobility before the fire, lending credibility to this empirical strategy. There is an increase in upward mobility after the fire, and the effect persists and is statistically significant even after four years.

Figure 2.8: Moved Away from Commuting Zone after Megafire



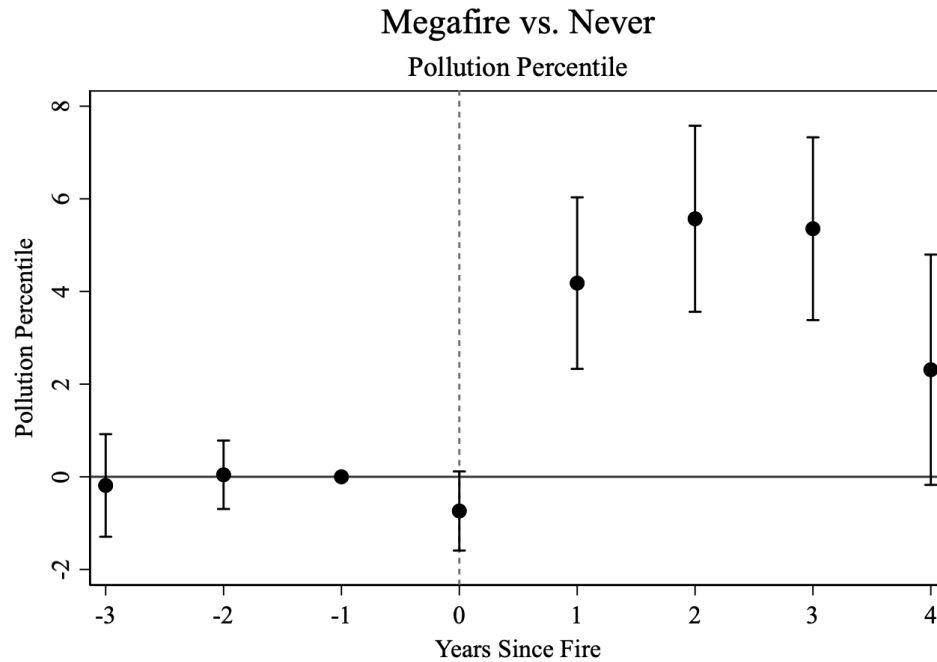
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different commuting zone from where she was at the year before the first fire (“Moved Away from Commuting Zone”). There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy. There is a sharp increase in the likelihood of moving away starting the year after the fire, though the effect dissipates by the fourth year after the fire.

Figure 2.9: Median Household Income after Megafire



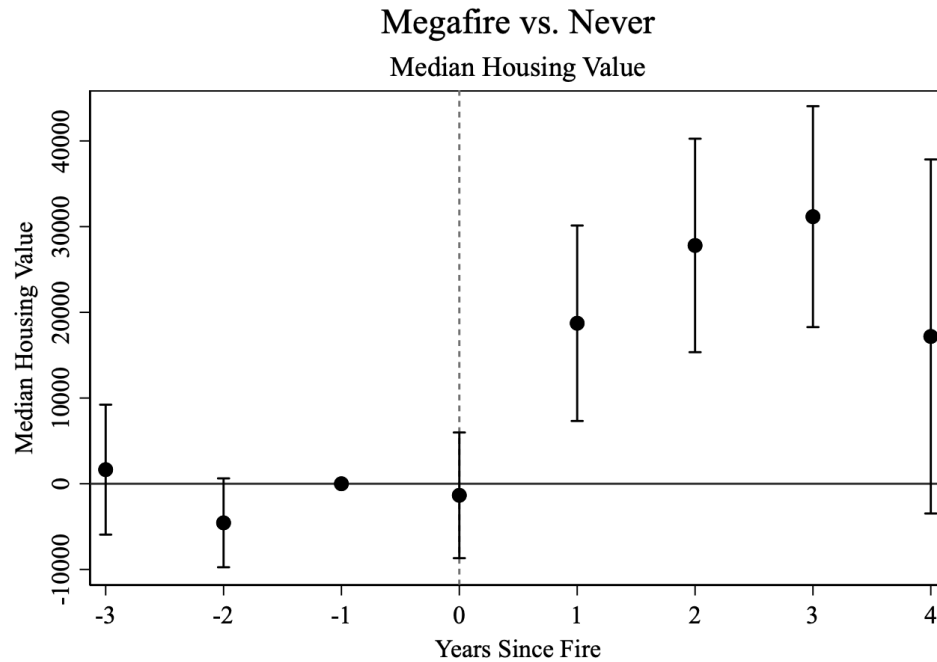
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the median household income and comes from the Opportunity Atlas, measured as the median household income in 2016. There are no statistically significant differences in the median household income before the fire, lending credibility to this empirical strategy. There is an increase in the median household income, though the effect dissipates by the fourth year after the fire.

Figure 2.10: Pollution Burden after Megafire



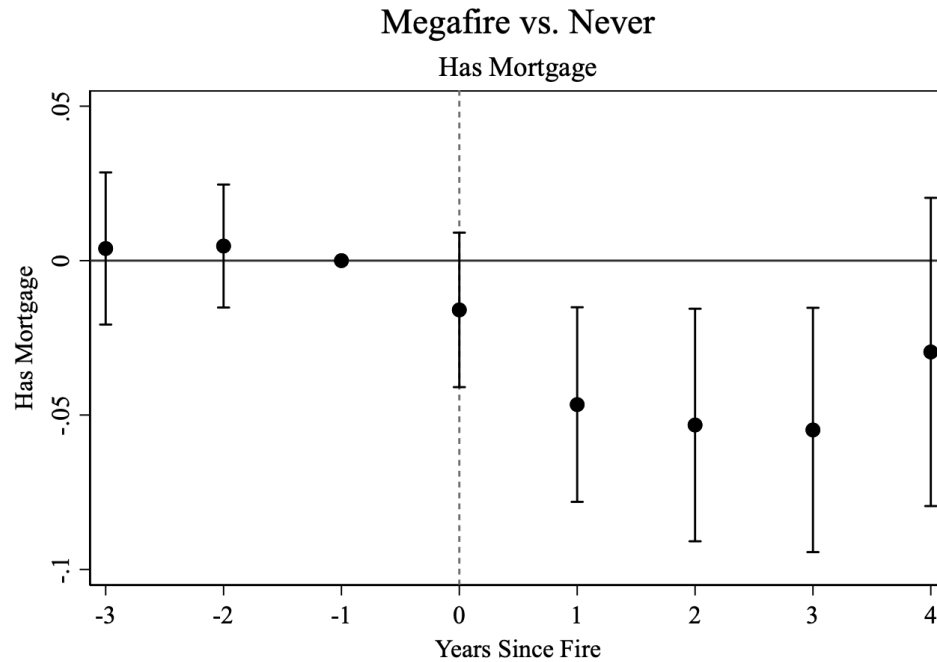
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the pollution burden percentile and comes from The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviron), measured as the percentile of pollution burden (which incorporates ozone, PM 2.5, diesel, drinking water, lead, pesticides, toxic releases, traffic, cleanup sites, groundwater threats, hazardous waste, impaired water bodies, and solid waste) for census tracts in California and released in 2021. There are no statistically significant differences in the pollution burden before the fire, lending credibility to this empirical strategy. There is an increase in the pollution burden, though the effect dissipates by the fourth year after the fire.

Figure 2.11: Median Housing Value after Megafire



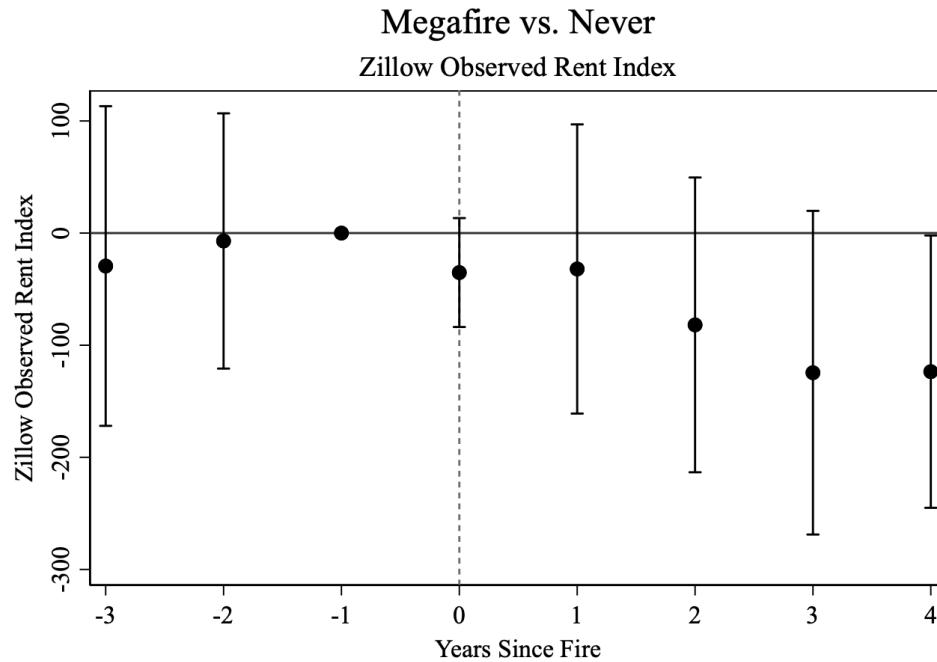
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the median housing value of the census tract and comes from 2010 Census. There are no statistically significant differences in the median housing value before the fire, lending credibility to this empirical strategy. There is an increase in the median housing value, though the effect dissipates by the fourth year after the fire.

Figure 2.12: Mortgage after Megafire



Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is whether the individual has a mortgage. There are no statistically significant differences in having a mortgage before the fire, lending credibility to this empirical strategy. There is a decrease in the likelihood of having a mortgage, though this effect dissipates by the fourth year after the fire.

Figure 2.13: Zillow Observed Rent Index after Megafire



Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the Zillow Observed Rent Index (ZORI), which is the typical observed market rate rent for the zipcode measured as a weighted average of the rental values of the middle third of apartments and homes that is then averaged over a year. There are no statistically significant differences in the ZORI before the fire, lending credibility to this empirical strategy. There is a decrease in the ZORI that people living in blocks that burned by a megafire face after the fire, though it is not statistically significant.

Chapter 3

Soothing the Burn: The Impact of Government Aid on Weathering Wildfires

1 Introduction

Natural disasters are costly and devastating events. From 1980-2024, the United States alone had over 400 disasters where the damages exceeded \$1 billion for a total cost of over \$2.9 trillion (NCEI 2025). The U.S. government responded to these disasters by providing financial assistance through the Disaster Relief Fund (DRF), which is administered by the Federal Emergency Management Agency (FEMA) to provide immediate disaster recovery, cover the needs of affected households, and mitigate the effects of future disasters. FEMA has spent \$347 billion from 1992-2021, for an average of \$12 billion spent per year (CBO Tue, 11/29/2022 - 12:00). These disasters have become more likely in recent years, and they are forecast to be even more common and severe with climate change, which will increase the costs associated with them (Berlemann and Steinhardt 2017; Benevolenza and DeRigne 2019;

Yao, Kabundi, and Mlachila 2023).

The increasing number of people expected to be impacted by natural disasters, coupled with the substantial financial resources dedicated to disaster relief, highlight the need to carefully consider the optimal response and the role of the government to promote that. In order to do so, one must first understand how current policies are operating. One key adaptation strategy available for individuals is adaptive migration: the movement from high- to low-risk areas (Wang 2025). Since natural disasters tend to hit certain types of areas ((Flannigan, Stocks, and Wotton 2000; Keeley and Syphard 2016)), moving people away from those areas is theorized to be an important way to minimize the costs of climate change (Desmet and Rossi-Hansberg 2015; Conte, Desmet, and Rossi-Hansberg 2022; Cruz and Rossi-Hansberg 2023; see Desmet and Rossi-Hansberg 2024 for a review). However, the damages associated with a natural disaster can be so large that they can overwhelm the financial resources of the affected individuals, which can prevent adaptive migration (Wang 2025).

The government could, in theory, increase adaptive migration by providing aid to lessen liquidity concerns. If people are unable to move optimally because of credit constraints and aid increases liquidity (Wang 2025), this can promote adaptive migration – as long as individuals want to move to lower-risk areas. On the other hand, the tying of aid to incentives to return to the damaged homes can hinder adaptive migration: Areas that burn are generally high-risk areas, and so remaining in the same location is not adaptive.

Estimating the impact of government aid is difficult because there are factors that simultaneously affect both aid receipt and migration. A simple comparison of the migration responses between individuals who experienced fires that receive government aid with those in fires that do cannot identify the causal impact of the assistance because areas that experience more severe fires – and thus larger displacement effects – are more likely to receive aid (GAO 2020). Moreover, within an area that receives aid, not all individuals receive payment: The

aid application process is onerous, opaque, and confusing (GAO 2020). Applicants must provide extensive documentation before receiving aid, which has been criticized as a reason why some applicants who qualify for aid, particularly those from disadvantaged groups, are denied it (GAO 2020; Billings, Gallagher, and Ricketts 2022).

To address this endogeneity concern, I leverage the fact that there are political incentives to provide assistance as an instrument to identify the causal impact of government aid. To unlock federal disaster assistance, which is disseminated through the Federal Emergency Management Agency (FEMA), the president must make a disaster declaration for the impacted county (FEMA 2003). I discover that counties that are politically competitive – which I proxy by their having a margin of less than 5% between the top two vote shares in the most recent presidential election – are more likely to receive FEMA aid. This is consistent with the political science and economic history literature highlighting that politically competitive areas receive more government assistance (Wright 1974; Fleck 2001; Fishback, Haines, and Kantor 2007; Reeves 2011; Husted and Nickerson 2014; Boustan et al. 2017; Jou and Morgan 2024). Based on this insight, I implement this measure of political competitiveness as a novel instrument for aid receipt, which provides strong first-stage results in the instrumental variables (IV) strategy I employ to identify the causal impact of aid on adaptive migration. I call this specification the difference-in-differences instrumental variables (DID-IV) regression.

In both the difference-in-differences (DID) regression and the DID-IV regression, which corrects for aid endogeneity, I find that individuals living in areas that receive aid after a fire are more likely to migrate than those who do not. The OLS results indicate that there is on average an 17.0 p.p. increase in the likelihood of an individual in an area that receives aid of moving to a different census tract after a first fire, with no migration effect for the fires for which aid is not distributed. Moreover, individuals in areas with first fires that receive aid are 5.5 p.p. more likely to move to a low- or medium-wildfire-risk census tract. The DID-IV regression yields an even larger average estimate of the effect of aid on moving: a

33.0 p.p. increase. The difference between the DID-IV and DID estimates suggests that the latter suffers from downward bias, consistent with the conjecture that people who are more likely to migrate are the least likely to receive aid. The DID-IV results indicate no effect of aid on moving to low- or medium-wildfire-risk areas, which could be because there are no adaptive location requirements for aid receipt. However, the DID-IV results show that aid decreases the financial distress for the individual and increases their credit access. This suggests that although aid helps people move, the current implementation of aid does not promote adaptive migration. Nevertheless, aid does assist individuals financially, helping them minimize the monetary damages of the fire.

The key contribution of this study is the finding that government aid has a null effect on adaptive migration. The migration literature has flagged the need to consider the way government policies influence location decisions in the United States (Jia et al. 2023). Studies have speculated about the importance of government involvement rationalizing the different migration responses to historical and current disasters (e.g. between Boustan, Kahn, and Rhode 2012 and Deryugina 2017), by exploring changes in migration after the creation of FEMA (Boustan et al. 2017), and by documenting an increase in government investment and higher population growth for election-year hurricanes (Henkel, Kwon, and Magontier 2022). These papers do not explicitly link government aid and migration but rather document a positive correlation. A handful of papers study the direct impact on migration, though they focus only on specialized, small-scale programs (Gregory 2017; Elliott and Wang 2023). All the studies focus on hurricanes and study only migration, not whether individuals move adaptively. I contribute to this literature by providing causal estimates of the impact a large-scale government aid program has on adaptive migration in the context of wildfires, using a novel instrument.

This paper also highlights the importance of government aid and how it can be incorporated into the dynamic spatial equilibrium model literature. Because financial constraints have been

found to be important in considering who can migrate adaptively (Wang 2025), including government intervention into these models would be fruitful because aid can be a means of addressing liquidity constraints and repairing existing amenities or capital (as modeled in Henkel, Kwon, and Magontier (2022), though they abstract from capital destruction). Including different forms of government aid into these models would shed light onto which policies can be most effective for addressing climate change.

The rest of the paper is organized as follows. Section 2 describes the background on how government aid is disbursed and outlines factors that impact people’s migration decisions. Section 3 describes the data, and Section 4 outlines the empirical strategy, including the endogeneity concerns of aid and proposes a solution. Section 5 explores the role of aid and presents a causal analysis of federal FEMA aid receipt. Section 6 concludes and includes suggestions about more effective government strategies.

2 Background

2.1 Government Aid

After a natural disaster, government aid – ranging from short-term housing assistance to long-term reconstruction loans – is mobilized to help the impacted individuals. FEMA is the national provider of aid and administers all federal-level assistance (FEMA 2023). To activate FEMA, the US president must issue a disaster declaration, a power exclusive to the president and not requiring the approval of Congress. Typically, albeit not necessarily, the process of disaster declaration issuance begins with a request from the state governor, which includes documentation that the state’s resources are insufficient to ameliorate the situation. The state must provide an initial assessment of the extent of damage, which FEMA-appointed inspectors will confirm, and the FEMA regional administrator will present a recommended plan of action to the president. Once the president declares a disaster for a county, FEMA

is approved to mobilize workers and start the application process whereby individuals can receive grants (FEMA 2003).

Because this paper focuses on individual adaptive behavior, I examine FEMA’s Individuals and Households Program (IHP), the most prevalent form of assistance, whereby financial assistance is provided directly to individuals to address disaster-induced needs.¹ The IHP provides grants for individuals to assist with immediate needs, such as rent, living expenses, and medical costs, as well as grants to rebuild or repair damaged property. I calculate that, across 2014-2024, FEMA spent \$13.9 billion in IHP aid, with \$319 million for California and \$169 million specifically for California wildfires.²

In order to receive FEMA assistance, individuals must first choose to apply for specific types of aid, which reflects their preferences, and be approved for that aid, which depends on the verification process. Applications for FEMA assistance require a series of background checks and submission of approved documentation. Some of the eligibility checks include citizenship status, identity confirmation through a valid Social Security number (SSN), ownership or occupancy verification, and insurance status (FEMA 2023). This information is often validated automatically through public records databases.³ Applicants can appeal FEMA’s

¹Another individual-level government program is the Small Business Administration (SBA) loans program, which extends loans of up to \$500,000 on highly desirable terms for individuals to rebuild their homes. I have not yet incorporated this type of assistance into my analysis. However, application for any type of federal disaster assistance requires an initial FEMA application (FEMA 2023). FEMA has several other programs to assist local governments. It utilizes three forms of disaster aid: individual assistance (IA, which subsumes the IHP program studied in this paper), public assistance (PA), and hazard mitigation assistance (HMA) (CRS 2024). I abstract from non-individual assistance, though higher-level aid could impact the desirability of a location as individuals make their migration decisions. Indeed, Henkel, Kwon, and Magontier (2022) argue that post-disaster government reconstruction improves the area’s amenities, boosting the county-level population after a hurricane.

²This calculated amount is similar to the amount that the GAO calculated for the FEMA budget between fiscal years 2004 and 2013: \$95 billion total, with \$25.9 billion for individual assistance, \$12.7 billion for overhead, \$6.1 billion for mission assignment, and \$5.2 billion for hazard mitigation (Lunney 2014). Individual assistance accounts for 27% of the overall FEMA disaster relief fund.

³The IHP grant application process has been criticized as inaccessible, especially for disadvantaged individuals. The US Government Accountability Office (GAO) has found that FEMA has often denied IHP claims for inadequate documentation, such as proof of residency, extent of damages, and insurance status. In the face of concerns over fraudulent claims, FEMA’s approval rates have fallen drastically over time. Though applicants can contest denials, the appeals process is convoluted, often incomplete, and difficult to understand, “requiring a reading level of a high school senior” (GAO 2020), which could disproportionately exclude

decision or the amount of assistance received within 60 days of decision notification.(FEMA 2023).

The two largest components of the IHP are rental aid and home repair aid. People who want to move away can apply for rental aid, which could help ease their transition to a new living place. There is also aid specifically for moving expenses.⁴ Rental assistance is available for people unable to live in their homes because of direct housing damage or surrounding damage to infrastructure (FEMA 2024a). It is intended to cover temporary housing for one to two months, and extensions up to 18 months can be requested. This funding can be used to cover rent in any location, though the amount is based on the US Department of Housing and Urban Development’s calculation of the Fair Market Rent of the burned area.⁵ A disaster declaration that unlocks federal aid could help people who want to move to migrate, leading to higher migration out of the burned areas. Conversely aid to repair damaged property could help people who want to stay in the area rebuild their homes, decreasing out-migration. The maximum amount of financial assistance for home repairs is adjusted annually, and in 2024, it was \$42,500, though this does not include rental costs (FEMA 2024b). This could help people who want to stay in the area to rebuild their homes, decreasing migration from the area. From 2002 to 2024, the average amount of aid received in a county for a given wildfire disaster for homeowners was \$61,440, with \$37,859 dedicated to repair and replacement aid, \$7,661 to short-term rental assistance, and \$13,875 to other needs. Renters received \$39,094 on average in total, with \$6,109 for rental assistance and \$31,784 for other needs.⁶ Thus, receiving a disaster declaration and unlocking federal aid could theoretically facilitate or hinder migration.

those with lower education and income. FEMA automatically scans public records databases and rejects applications in which the applicant is not found to own their home. Low-income and minority households are more likely to rely on more informal inheritance norms so that the heir was bequest the home but does not have the required paperwork necessary to prove ownership. In 2024, in response to the GAO report and other criticisms, FEMA updated its grant application process (FEMA 2024b).

⁴Moving expenses are counted under the broad “other needs assistance” category of aid (FEMA 2024a).

⁵From conversations with FEMA officers, the intent of the rental aid is to help individuals remain nearby to minimize the impact on their employment and oversee the rebuilding process.

⁶These numbers are winsorized at 1% at each tail by disaster and beneficiary type (homeowner vs. renter).

2.2 Wildfires and Migration Model: How Government Aid Can Impact Individuals

If governments seriously consider adaptive migration a promising strategy for minimizing the impact of natural disasters, understanding how individuals make migration decisions is key for designing policies that promote it. Individuals consider three factors which influence whether they adaptively migrate: moving costs, preferences, and constraints. Individuals need to move, which involves overcoming the moving costs. For them to move to safe areas, individuals must both have the preference for it and the means to achieve it. If individuals want to move to safe areas but cannot afford to (i.e., they are financially constrained), monetary assistance through disaster aid could increase adaptive migration. However, if they want to stay in the area, then aid may not lead to adaptive migration. Additionally, requirements attached to the aid can incentivize certain behaviors. Thus, differentiating between preferences and constraints is crucial for designing effective policies.

I construct a simple location choice model to guide the discussion about how wildfires can interact with individual preferences and financial constraints to influence migration decisions.⁷ Assume each location l has exogenous amenities (A_l), rents (R_l), and wages (w_l). Let the indirect utility of location l at time t be determined by the location characteristics: $U_{l,t} = w_{l,t} + A_{l,t} - R_{l,t}$. To allow heterogeneity among individuals in their valuation of a location, let individual i draw an idiosyncratic location preference shock each period t across all locations, and the shock for location l is denoted as $\epsilon_{l,t}^i$. I also include the *ex-ante* expected loss each period t that comes from a natural disaster ($Pr^i(f_l)(\mathbb{E}[q_l|f_l = 1] + Pr^i(f_h^i|f_l = 1)\mathbb{E}[q_h^i|f_h^i = 1])$), which incorporates individual i 's *ex-ante* belief about the probability of disaster f occurring in location l ($Pr^i(f_l)$), the damage to the location when location l is impacted by disaster f ($\mathbb{E}[q_l|f_l = 1]$), individual i 's *ex-ante* belief about the probability of individual i 's specific home

⁷This is similar to and builds upon the model in Section 2 in Chapter 1, though that was a simplified version that solely uses words and does not include government aid.

being damaged if disaster f impacts location l ($Pr^i(f_h^i|f_l = 1)$),⁸ and her *ex-ante* expected damage q if her specific home is damaged ($\mathbb{E}[q_h^i|f_h^i = 1]$).⁹

I allow individuals to anticipate their ability to make migration decisions in future periods and the future utility obtained from different locations so they make forward-looking migration decisions, which include a moving cost ($m^{l,d}$) defined for those who start in location l as 0 if she stays in the same location ($l = d$) and some positive $c^{l,d}$ if she moves ($l \neq d$). Individuals are subject to a budget constraint, which includes the individual's savings (S^i). I assume that each individual i 's initial level of savings S_0^i is exogenously drawn, and they make their first location choice given that amount. Each period, the savings follow a deterministic function based on the characteristics of the location and whether the individual chooses to move from l to d : $S_{t+1}^i = S_t^i + w_{l,t} - R_{l,t} - m^{l,d}$. Let $\beta\mathbb{E}[V_{t+1}^l]$ represent the option value of choosing l , which includes the *ex-ante* expected value from choosing location l ($\mathbb{E}[V_{t+1}^l]$) and the discount rate (β).¹⁰ The timing of the model is as follows: In each period t , an individual i in location l receives the idiosyncratic location shock draws and chooses a destination d – which may be the same as her starting location l – and executes the move and pays the moving cost ($m^{l,d}$). Next, the destination d will draw f_d and f_h^i , which determines whether a disaster happens in location d and whether individual i 's specific home is damaged, respectively. Once the disaster occurs, the individual realizes the preference shock ($\epsilon_{d,t}^i$), and then works and consumes in the chosen location d until the end of the period.

⁸If an individual takes on many home-specific mitigation efforts, this can impact her belief about the probability of her home being damaged.

⁹The expected damage to the location l (q_l) encapsulates the general loss to the amenities, rents, or wages of the location that can occur after the disaster.

¹⁰Consider the *ex-ante* expected value term for location l . The *ex-ante* expected value term is defined in every period t and includes the expected utility from the future characteristics of the location, the moving cost, and the expected realization from the idiosyncratic location shock: $V_t^l = \mathbb{E}_\epsilon[\max_k\{w_{k,t} + A_{k,t} - R_{k,t} - m^{l,k} + \epsilon_{k,t}^i - Pr^i(f_k)(\mathbb{E}[q_k|f_k = 1] + Pr^i(f_h^i|f_k = 1)\mathbb{E}[q_h^i|f_h^i = 1]) + \beta\mathbb{E}[V_{t+1}^k]\}]$

$$\begin{aligned}
& \max_d \left\{ w_{d,t} + A_{d,t} - R_{d,t} + \beta \mathbb{E}[V_{t+1}^d] - m^{l,d} + \epsilon_{d,t}^i \right. \\
& \quad \left. - \underbrace{Pr^i(f_{l,t})}_{\text{Prob of disaster in location}} \left(\underbrace{\mathbb{E}[q_{l,t}|f_{l,t}=1]}_{\text{Location damage}} + \underbrace{Pr^i(f_{h,t}^i|f_{l,t}=1)}_{\text{Prob of home damage}} \underbrace{\mathbb{E}[q_{h,t}^i|f_{h,t}^i=1]}_{\text{Home damage}} \right) \right\} \\
& \text{subject to } S_t^i + w_{d,t} \geq R_{d,t} + m^{l,d}
\end{aligned}$$

Wildfires can impact the location in several ways. If a disaster occurred in time t , this impacts the amenities, rents, and wages for the burned location in the next period ($t+1$), which is known when the individual makes her next location decision and is part of the expected utility in the next period (V_{t+1}). Suppose location l experiences a wildfire at time t so that the subscript pair l, t represents location l at time t and $l, t+1$ is location l the period after the wildfire occurred. The disaster can decrease the desirability of the location, such as by decreasing the amenity value by marring the view ($A_{l,t+1} < A_{l,t}$), increasing the housing cost by raising insurance premiums ($R_{l,t+1} > R_{l,t}$),¹¹ and impacting the local economy through business shutdowns ($w_{l,t+1} < w_{l,t}$). Even if their homes are not damaged, individuals may update their beliefs about the riskiness of the area ($Pr^i(f_{l,t+1}) > Pr^i(f_{l,t})$).¹² All of these factors combine to decrease the indirect utility of remaining after the fire ($w_{l,t+1} + A_{l,t+1} - R_{l,t+1} < w_{l,t} + A_{l,t} - R_{l,t}$) and the *ex-ante* expected value function of l ($\mathbb{E}[V_{t+1}^l] < \mathbb{E}[V_t^l]$). Thus, after a wildfire, people may reconsider their location decision.

Moreover, moving costs ($m^{l,d}$) play a role in whether individuals choose to migrate in the first place. The dynamic spatial equilibrium literature has estimated high moving costs, on

¹¹This change could be negative if people try to offload their homes and demand for housing does not change. However, in the short term, there could be a steep decrease in local housing supply from the fire destruction, and if people are trying to stay nearby to oversee the damage assessment process, rental costs can be expected to be higher. This conjecture is corroborated by anecdotal evidence (Bittle 2023).

¹²There could also be changes in the actual wildfire risk of the area: $Pr(f_{l,t})$. Immediately after a wildfire, the risk decreases as the flammable vegetation is already burned. However, the fact that this area previously burned shows its potential to burn again.

the order of tens or hundreds of thousands of dollars (Kennan and Walker 2011; Diamond, McQuade, and Qian 2019).¹³ These moving costs can capture both monetary and psychosocial costs of moving, such as leaving behind a community.¹⁴ As climate change increases the likelihood of natural disasters, even people who become aware of an area's increased risk may not move to location d if moving costs are sufficiently high. After the disaster occurred ($t + 1$), the condition for individual i to move from l to d in this model can be written as:

$$\begin{aligned}
m^{l,d} \leq & (w_{d,t+1} - w_{l,t+1}) - (R_{d,t+1} - R_{l,t+1}) + (A_{d,t+1} - A_{l,t+1}) \\
& + \beta(\mathbb{E}[V_{t+1}^d] - \mathbb{E}[V_{t+1}^l]) + (\epsilon_{d,t+1}^i - \epsilon_{l,t+1}^i) \\
& - \{Pr^i(f_{d,t+1})(\mathbb{E}[q_{d,t+1}|f_{d,t+1} = 1] + Pr^i(f_{h,t+1}^i|f_{d,t+1} = 1)\mathbb{E}[q_{h,t+1}^i|f_{h,t+1}^i = 1]) \\
& - Pr^i(f_{l,t+1})(\mathbb{E}[q_{l,t+1}|f_{l,t+1} = 1] + Pr^i(f_{h,t+1}^i|f_{l,t+1} = 1)\mathbb{E}[q_{h,t+1}^i|f_{h,t+1}^i = 1])\}
\end{aligned}$$

This shows that the migration decision jointly depends on the difference between the initial location l and the destination location d 's wages (w), rents (R), amenities (A), expected losses ($Pr^i(f)(\mathbb{E}[q|f = 1] + Pr^i(f_h^i|f = 1)\mathbb{E}[q_h^i|f_h^i = 1])$), expected values ($\mathbb{E}[V_{t+1}]$), and idiosyncratic location preference shocks (ϵ^i). A wildfire hitting the initial location l can dramatically change the location values: For instance, the fire could decrease $A_{l,t+1}$, which would make the difference between the destination and initial locations larger than what it was before the fire ($A_{d,t+1} - A_{l,t+1} > A_{d,t} - A_{l,t}$), making it more likely that the moving cost $m^{l,d}$ inequality holds and the individual chooses to move. This equation formalizes why individuals may choose to move after a disaster occurs. It also shows how if the moving cost is sufficiently large, people will not choose to move, even if the location characteristics have changed.

Wildfires can force individuals to assume the fixed costs of moving and reoptimize their

¹³The high moving costs are calibrated to rationalize the low migration rates in the United States.

¹⁴For ease of interpretation, I interpret them in terms of monetary costs to include them in the budget constraint.

location choices. As they are forcibly displaced in the short term through mandatory evacuations or their dwellings burning, people may make their location choice as if they did not face moving costs. In addition, individuals may prefer to live in areas with features that are positively correlated with wildfire risk – that is $Corr(A_l, p_l) > 0$ – such as proximity to nature or pleasant views (Hammer, Stewart, and Radeloff 2009; Winkler and Rouleau 2021). Individuals with such preferences may choose to return to the burned area or move somewhere else with high wildfire risk – that is migration, even when it occurs, may not be adaptive.

Financial constraints may also prevent individuals from moving adaptively. Financial constraints can be conceptualized as liquidity and solvency constraints. Liquidity constraints are when people are presently unable to pay an amount, though they could eventually over their lifetime using their expected earnings. If people want to move but they cannot pay the initial moving cost, this could be due to a liquidity constraint. Solvency constraints, on the other hand, are when people are unable to afford a payment because it is larger than their lifetime expected earnings. Areas that are low wildfire risk are also more expensive (i.e., $Pr(f_l)$ is negatively correlated with R_l).¹⁵ Individuals may not be able to afford the housing costs to live in safe areas, which is a solvency constraint. There are large financial costs that are incurred because of a disaster: People could close their jobs or see their working ability restricted, especially if they were evacuated. Individuals could initially choose to move to a safe area but, as they deplete their savings, choose to move to cheaper areas, which happen to also be at higher wildfire risk. Both types of financial constraints could prevent people from moving adaptively even if they want to.

This conceptual framework shows how, in theory, government interventions could facilitate or hinder adaptive migration, depending on how aid is disbursed. First, the government could

¹⁵Using house price index data from the Federal Housing Finance Agency (FHFA) and wildfire risk data from FEMA, I find that there is a negative, statistically significant correlation between the two. The relationship holds for the United States overall and for California specifically (Table 4.1).

improve the amenities of the location by restoring infrastructure ($A_{l,t+1} \geq A_{l,t}$) or increase wages by revitalizing the local economy ($w_{l,t+1} \geq w_{l,t}$). This is unlikely to encourage adaptive migration because it increases the value of the burned location compared to alternative destinations.¹⁶ Second, the government could give a one-time transfer (G) to individuals impacted by the disaster. This should facilitate adaptive migration if the fixed moving cost is the main barrier to migration because this transfer could help people overcome their liquidity constraints: $G + S^i + w_l \geq R_l + m_l > S^i + w_l$. However, this one-time transfer would not lead to adaptive migration if people prefer high risk areas for the amenities or idiosyncratic location preferences. Even if the one-time transfer helped individuals move to safe places initially, this type of aid would not facilitate long-term adaptive migration if people are solvency constrained and cannot afford the increased cost of living in the safe locations: that is, $G + S_t^i + w_{l,t} \geq R_{l,t} + m_{l,t}$ but $S_{t+n}^i + w_{l,t+n} < R_{l,t+n}$ after n time periods. Finally, the government could give location-specific aid. One example of such aid could be providing job search assistance or longer-term transfers to low-risk areas. This should lead to increased adaptive migration if the barrier is primarily a solvency issue, but again might not if the migration decision is driven solely by preferences.¹⁷ Another example of location-specific aid could be home repair aid, which should not facilitate adaptive migration as people are incentivized to return to the burned areas, which are typically high-risk locations. The characteristics of California insurance coverage landscape also interact with the amount of aid individuals receive, further complicating the migration decision.¹⁸ For instance, people whose homes are damaged are likely to receive insurance payouts, I , which would also loosen the budget constraint: $I + S^i + w_l > S^i + w_l \geq R_l + m_l$. Thus, government aid can make adaptive migration more or less attractive depending on the way it is implemented.

¹⁶There is suggestive evidence that this phenomenon occurs after hurricanes (Deryugina, Kawano, and Levitt 2018; Henkel, Kwon, and Magontier 2022).

¹⁷The amount given could sway individuals who are indifferent between the safe and risky areas. Given that the amenity differences between these types of locations are stark (i.e. different environmental landscapes), depending on the monetary amount, there may not be many people switching to safe areas.

¹⁸FEMA explicitly states that it is not a substitute for insurance. It can cover costs that insurance does not, though it requires a copy of insurance settlement about the damage (FEMA 2023).

3 Data

To explore the impact of the government on affected individuals, I use detailed location data to characterize the quality of the destinations.¹⁹

3.1 Fire

I use data from the California Department of Forestry and Fire Protection (CAL FIRE) to define the census blocks that are burned. Responsible for California’s fire suppression efforts, CAL FIRE keeps careful and complete records of wildfires within the state. I merge shapefiles of perimeters of wildfires from 1950 to 2021 with maps to identify which census blocks were affected by wildfires. I define the fires at the census block-level because this is the finest geographic unit for which credit data are available. A census block is smaller than a census tract, which is in turn smaller than a county; at the time of the 2010 census, there were 710,145 census blocks, 8,057 census tracts, and 58 counties in California (Bureau 2021).²⁰ I can also calculate the percentage of the block that is burned as a measure of the intensity of fire damage at block level.

3.2 Wildfire Risk

To determine whether individuals move from risky to safe areas, I use FEMA’s wildfire risk index to measure the fire danger of the census tract in which the person lives after the fire. FEMA provides a national risk index of natural disasters for each state at the census

¹⁹Some of these data sources are used in previous chapters of my dissertation (i.e. Section 3 from Chapter 1 and Section 2 from Chapter 2) so I repeat those sections here.

²⁰I list the categories for the 2010 census because the credit data defines locations using this census. The US Census Bureau defines these census geographies in order to create statistical areas bounded by “visible features such as roads, streams, and railroad tracks, and by nonvisible boundaries such as property lines, city, township, school district, county limits and short line-of-sight extensions of roads” (Rossiter 2011). Census tracts have an ideal population of 4,000 people (Bureau 2022). Half of the census blocks within the United States are smaller than a tenth of a square mile (Lynch 2020).

tract level for a variety of climate shocks, including wildfires.²¹ The data are from 2021 and are applied retrospectively, representing an *ex post* wildfire risk score. The wildfire risk score ranges from 0 to 100, and I classify a tract as low risk if it's between 0-25 and low or medium risk if it's between 0-50. I define a move as adaptive if the person appears in the data in a low- or medium-risk tract. The wildfire risk score for the United States approximately follows a uniform distribution (Figure 4.71(a)), while that for California has a large percentage (over 60%) of tracts that are low risk and a slight overrepresentation at the highest part of the risk distribution (Figure 4.71(b)).²²

3.3 Migration

To track where people are moving, I use the University of California Consumer Credit Panel (UCCCP). The UCCCP used in this analysis includes credit and demographic data for a longitudinal panel of everyone who has ever lived in California and has a credit record. The data have consistent census block information from 2014-2021.²³ There are approximately 40 million consumers, though I currently use a 1% random sample of the dataset. The data include household members and cosigners as well as their location at the zip code and census block level. They also include demographic information about the consumers (including race, gender, age, education, and occupation), credit scores, and raw tradeline-level information about each loan or collections item (including payment history, credit limits and balances,

²¹Other measures include the expected annual loss (EAL) to individual natural disasters and the overall risk, which combines the EAL, social vulnerability, and community resilience measures.

²²There is a large mass of census tracts that are bunched at 0 risk. For the overall US distribution, approximately 25% of all tracts have a risk score below 25. For the California distribution, approximately 60% of all tracts have a risk score of 0, so many locations within the state have low risk. However, there is also a large mass at the highest risk level and underrepresentation at the middle risk levels.

²³The data span 2004-2023, but the census block designation changes from the 2000 census definition of blocks for 2004-2014 to the 2010 census definition for 2014-2021 and to the 2020 census definition for 2021-2023. Even when using crosswalks between the different definitions to standardize, I see a spike in migration in years where the census designation changes. This is because there is not a clean mapping across the censuses' characterization of areas: Some census blocks split into smaller entities, while others are combined. There are also higher rates of missingness for census blocks before 2010. Thus, for ease of analysis, I restrict my sample period to 2014-2021, which also coincides the occurrence of many large fires.

mortgage delinquency, and foreclosure). This dataset allows me to measure mobility since I can construct a move based on whether the individual is in a different block from the previous quarter. The tradeline data allow me to study the financial impact of the experience of a fire and to consider heterogeneity based on financial situation.

3.4 Government Aid

FEMA offers aid for individuals and households impacted by presidentially declared natural disasters through the Individual and Households Program (IHP). This program provides grants for various purposes, including short-term rental assistance – which can be used in any location – and rebuilding grant – which is used to repair damaged homes. These data are publicly available at the geographic level, though the finest geographic unit is the zipcode, which is substantially larger than a census block, the level at which I define the fire treatment.²⁴ I define aid as an indicator of whether a zip code received any IHP aid and categorize fires on the basis of which ones received any aid and which did not.

4 Empirical Strategy

To quantify the impact government aid has on how people move as a result of a fire, I conduct an instrumental variables difference-in-differences (IV-DID).

²⁴Census tracts, which are larger than census blocks, are typically around 4,000 individuals, whereas zipcodes are constructed by the U.S. Postal Service to coordinate mail delivery and have on average 10,000 individuals (Bureau 2022; Bureau 2023).

4.1 Preliminary Analysis: Stacked Difference-in-Differences Event Study

To assess the impact of receipt of government aid on migration, I estimate the following equation:

$$\begin{aligned}
 y_{i,f,t} = & \mu_i + \sum_{r=-3, \neq -1}^{N=4} \lambda_r(f,t) + \sum_{r=-3, \neq -1}^{N=4} \alpha_r \lambda_r(f,t) \times fire_{b(i,f)} \times aid_{c(i,t)} \\
 & + \sum_{r=-3, \neq -1}^{N=4} \beta_r \lambda_r(f,t) \times fire_{b(i,f)} + \gamma_t + X'_{i,t} \Gamma + \epsilon_{i,f,t}
 \end{aligned} \tag{3.1}$$

The outcome variable ($y_{i,f,t}$) is defined for individual i at calendar time t and for fire f .²⁵ The term that captures the effect of aid receipt is $\sum \alpha_r \lambda_r(f,t) \times fire_{b(i,f)} \times aid_{c(i,t)}$ and is equal to one if the individual i lives in a county c authorized to receive aid in year t . I include fixed effects for the individual (μ_i) to remove time-invariant individual-level factors.²⁶ I have relative time $\sum \lambda_r(f,t) = \sum \mathbb{I}[t - E_f = r]$ fixed effects, spanning from three years prior to four years after the fire and omitting the year before the fire as the reference year. Each term ($\lambda_r(f,t) = \mathbb{I}[t - E_f = r]$) is equal to one when it is r years from the year when the individual i experienced fire f , which is denoted as E_f . The fire variable ($fire_{b(i,f)}$) is an indicator variable equal to one if the individual i lives in a census block ($b(i,f)$) that is burned for the first time by fire f and equal to zero if she lives in a never-burned census block within the same census tract.²⁷ The coefficients of interest are α_r , which capture how

²⁵In my main specification, each individual who lives in a census block burned in my sample is represented one time. However, there are cases where an individual could live in a never burned block that serves as the control group for multiple fire events. This would happen for tracts that have different blocks within it that burn across different years. In those cases, following the stacked difference-in-differences literature, I would duplicate the observation, and so the same calendar year t and individual i data would serve as a control across two fires, f_1 and f_2 (Wing, Freedman, and Hollingsworth 2024). Thus, the data is unique at the individual, fire, and calendar year level.

²⁶The individual fixed effects absorb the treatment fixed effects that are typical in this literature because the individual-level is more granular than the block-level, which is the level that the fire treatment is defined.

²⁷This fire treatment is more granular than the treatments considered in other wildfire papers, which

much the experience of receiving aid impacts the individual r years relative to the time of the fire. I normalize the difference between the treated and the control to be the year before the fire (event time -1). Because the fire is defined at the census block level ($b(i, f)$), I cluster standard errors at the block level (Abadie et al. 2023).

For robustness, I include calendar year fixed effects (λ_t) to control for general migration trends over time. I include potentially time-varying controls ($X_{i,t}$) for the age category, marital status, whether the individual completed college, and the duration of time that individual i lived in her census block ($b(i, f)$) at the time of the fire because these variables are found to be associated with migration.

4.2 Definitions of Terms

I define the fire treatment by whether a block is burned for the first time.²⁸ I define the control group as people living in blocks that never burned but within the same census tract as the fire. I define the receipt of aid to be whether the individual lives in a county that received Individual and Households Program (IHP) aid.

I define migration over time to differentiate short-term from longer-term moves and the extent of adaptation – that is, the wildfire risk of the location. For migration to be climate adaptive, the individual must both move away and to a safe location. The first component of adaptive migration is captured in whether the individual is observed in the data in a tract other than the one she lived in the year before the fire (“Moved Away from Tract”). With this definition of migration, I can capture whether people leave the burned area and the extent to which people who are displaced in the short run later return since my credit panel data allows me to track individuals over time for up to four years after the fire. I capture the second component of adaptive migration by whether the individual is observed in a low- or

define the wildfire at the census tract level (An, Gabriel, and Tzur-Ilan 2023; McConnell et al. 2021).

²⁸I focus on blocks that burned by the first time only because there is not much variation in terms of aid receipt for megafires since larger fires are much more likely to receive aid.

medium-wildfire-risk census tract after the fire (“Low or Medium Wildfire Score”). Because this measure is at the census tract level and the treatment and control groups in my main specification are census blocks within the same tract, they have the same initial wildfire risk scores.

In order to investigate the financial outcomes, I construct various indices by combining standardized versions of various variables from the UCCCP data, following Miller, Wherry, and Foster 2020. I standardize the variables by taking each individual’s outcome and subtracting the mean and dividing it by the standard deviation of that variable the year before the fire of the control group (i.e. those in blocks that never burned). Next, I average the standardized values of all of the variables to get a singular number to represent the financial outcome. First, I construct an index of financial distress, combining the number of active collections, the total collection amounts, the number of current delinquencies, the total number of public records, the total amount past due, an indicator for whether the individual had a bankruptcy, and an indicator for whether the participant has a credit score below 600 (“low credit”). Next, I construct an index on credit access, which includes the total amount of credit available on credit cards, an indicator for whether the participant has a credit score above 660 (“prime credit”), the credit score, and the number of open credit cards. Finally, I construct an index for borrowing, that considers whether the individual has a mortgage, the number of open loans, and the total amounts across all of their credit cards. These indices allow me to combine various outcomes to have greater power to investigate how government aid may influence various aspects of the individual’s financial situation after a fire.

4.3 Causal Impact of Aid

The DID event study provides correlations between aid and the outcomes. In order for the DID event study to reveal the causal effect of aid on migration, I need to assume that the treatment and control groups would have had parallel trends in the absence of the

aid. Factors correlated with aid receipt could also impact migration, which would bias my estimates such that the coefficients from [Equation 3.1](#) fail to reflect the causal impact of aid. For example, fires for which aid is extended might be more destructive, which would also cause more people to move. This would lead to overestimation of the impact of the aid on migration. Alternatively, areas that receive aid may have disproportionate shares of groups who have lower migration rates, such as homeowners. This would lead to an underestimation of the impact. Thus, this empirical strategy does not yield the causal effect of aid.

Instrument: Political Competitiveness

To assess the causal effect of aid, I utilize a novel instrument that takes advantage of the fact that electoral considerations affect aid provision. I find that wildfires in counties that are politically competitive are more likely to receive FEMA IHP aid.²⁹ I define political competitiveness as the difference in the voter share between the highest and second highest candidates in the most recent presidential election, which I obtain from the MIT Election Labs.³⁰ I find that counties that are within 5% are statistically significantly much more likely to receive aid, including when conditioning on experiencing a fire or a megafire ([Table 3.1](#)). Among counties that experience a fire, the politically competitive ones are almost three times more likely to receive aid (0.14 vs. 0.05). Counties that experience a megafire are four times more likely to draw aid (0.28 vs. 0.07).

Politically competitive and noncompetitive counties are similar on a variety of characteristics, giving credibility to the idea that electoral incentives are what lead to greater aid.

²⁹This result is consistent with previous papers that connect post-disaster policies with political motives. For instance, Henkel, Kwon, and Magontier (2022) find that hurricanes in an election year (i.e., an even-numbered year) are more likely to trigger larger amounts of government transfers and that the affected areas experience larger population growth as a result. Although I try their instrument, I do not find evidence that temporal proximity to an election year predicts receipt of government aid for the California wildfires.

³⁰For years with presidential elections, political competitiveness is defined by vote shares in the election that year. For years without presidential elections, political competitiveness is defined by the vote shares in the most recent presidential election. My specification is robust to my using the previous years' presidential election results.

Although politically competitive counties are more likely to experience any fire, conditional on counties' experiencing a fire, they still are three times more likely to receive aid. Competitive and noncompetitive counties are indistinguishable in the size of the fires, number of fires and megafires, and likelihood of experiencing a megafire (Table 3.1). Conditional on the counties' receiving aid, there is no statistically significant difference in the amount of aid received. This makes sense, as the scope for the president's discretion is limited to whether the disaster is declared, not the specific amounts given out, which are determined following FEMA protocol and are less manipulable.

Since California has voted reliably Democrat in every election since 2000, the start of the considered period, it may seem surprising that the president would have electoral incentives to extend aid to politically close counties. However, the political science and economic history literatures have highlighted the political benefits from aid and politicians taking advantage of this phenomenon in swing areas (Wright 1974; Fleck 2001; Fishback, Haines, and Kantor 2007; Reeves 2011; Husted and Nickerson 2014; Boustan et al. 2017; Jou and Morgan 2024). Congressional districts within California are sometimes competitive, and boosts to representation in the House of Representatives results in additional political power. Vote shares in presidential elections may proxy the political closeness of a county overall or the congressional districts within it, which is relevant for local elections. Because the benefits of aid receipt for incumbent politicians are well documented (Reeves 2011; Clemens, Payson, and Veuger 2024; among others), goodwill may transfer to the party in congressional elections. This seems plausible because straight-ticket voting, the practice of voting for down-ballot candidates of the same political party as the president, is common in the United States, with the Pew Research Center finding that in the 2020 election, only 4% of registered voters said that they planned to vote for Biden and the Republican candidate for House in their district or Donald Trump and the Democratic House candidate (Atske 2020).³¹ Thus, though

³¹Some 78% of voters said that they would vote for either Biden and the Democratic House candidate or Trump and the Republican candidate in their congressional district. The remaining 16% were split in a

California overall may not be politically competitive, the president may still have an incentive to provide aid to help the other members of his or her party to increase overall political representation.

Empirical Strategy: DID and DID-IV

For ease of interpretation for the instrumental variable (IV) analysis, I use a difference-in-differences (DID) specification to evaluate the impact of receipt of any FEMA IHP aid by condensing the main event study specification:

$$\begin{aligned} y_{i,f,t} = & \mu_i + post_{f,t} + \alpha post_{f,t} \times fire_{b(i,f)} \times aid_{c(i,t)} \\ & + \beta post_{f,t} \times fire_{b(i,f)} + \gamma_t + X'_{i,t} \Gamma + \epsilon_{i,f,t} \end{aligned} \quad (3.2)$$

In Equation 3.2, I add an interaction term of an indicator for county receipt of aid at time t , $post_{f,t} \times fire_{b(i,f)} \times aid_{c(i,t)}$. I compress the relative year fixed effects terms, $\sum \lambda_r(f, t) = \sum \mathbb{I}[t - E_f = r]$, into an indicator $post_{f,t}$ which equals one if year t is after the year when fire E_f occurred: $t - E_f > 0$. The coefficient of interest α captures the average impact of wildfire aid receipt at any point after the treatment. I call this specification the “DID regression.”

To obtain the causal effect of IHP aid, I instrument for the aid indicator by whether the individual is living in a politically competitive county at the time of the fire. The first stage of the specification is:

$$\begin{aligned} post_{f,t} \times fire_{b(i,f)} \times aid_{c(i,t)} = & \mu_i + post_{f,t} + \alpha post_{f,t} \times fire_{b(i,f)} \times flip_{c(i,t)} \\ & + \beta post_{f,t} \times fire_{b(i,f)} + \gamma_t + X'_{i,t} \Gamma + \epsilon_{i,f,t} \end{aligned} \quad (3.3)$$

The instrument, $post_{f,t} \times fire_{b(i,f)} \times flip_{c(i,t)}$, is defined at the county level and is equal

different way or not sure/refused to answer. This voting breakdown is similar to that for the 2016 election (Atske 2020).

to one if the year is after the year of the fire, the individual was in a block burned by the fire, and the difference in the top two vote shares in the last presidential election was within 5%. I then use the estimated $post_{f,t} \times fire_{b(i,f)} \times flip_{c(i,t)}$ term in Equation 3.2 to obtain the causal impact of aid on adaptive migration.

For political competitiveness to be a valid instrument for government aid, it must satisfy the relevance condition and the exclusion restriction. The relevance condition requires that a county’s political competitiveness be predictive of its likelihood of receiving aid. The first stage of my two-stage least squares regression (Table 3.2) shows that a politically competitive county is more likely to receive FEMA IHP aid, and the F-statistic is substantially greater than 10. This result is robust to using different measures of political competitiveness, where the likelihood of aid receipt monotonically decreases and loses statistical significance as political competitiveness decreases, lending additional credibility about the instrument’s relevance (Table 4.6).³²

The exclusion restriction requires that a county’s political competitiveness impact migration outcomes through only aid receipt, nothing else. Although this condition is not directly testable, I evaluate the likelihood of receipt of other forms of government aid not related to wildfires by political competitiveness.³³ To do so, I run the following regression specification:

$$y_{c,t} = \alpha + \beta fire_{c,t} + \gamma comp_{c,t} + \zeta fire_{c,t} \times comp_{c,t} + \epsilon_{c,t} \quad (3.4)$$

The outcome variable ($y_{c,t}$) is defined for county c at calendar year t . The fire variable ($fire_{c,t}$) is an indicator variable equal to one if county c experienced a fire in year t . The political competitiveness variable ($comp_{c,t}$) is equal to one if the difference in the top two vote shares in the last presidential election was within 5% for county c . The interaction between

³²In cases where the gap in political competitiveness is 10% or 15%, the margin might seem too large to be considered a close election, so the instrument no longer statistically significantly predicts aid receipt.

³³This is a negative control test in the nomenclature of Danieli et al. (2023).

the fire and political competitiveness terms ($fire_{c,t} \times comp_{c,t}$) captures whether there are differences in the outcome for politically competitive counties that experience a fire. I cluster standard errors at the year level since the budget is determined annually.

Gathering a variety of county-level data on local expenditures, non-FEMA government aid, and tax amounts, I find that though there is often increased spending after a fire, the spending increase for politically competitive counties is minimal (Table 3.3).³⁴ For politically competitive counties that experience a fire, I do find increased fire-related spending, consistent with increased aid receipt after the fire and exactly what my instrument is intended to capture.

There could be another concern that individuals in politically competitive counties may be different in ways that impact their likelihood of moving adaptively. One such difference could be their climate attitudes as those who do not believe in climate change may be not want be inclined to move in an adaptive fashion. In order to test this, I estimate the equation Equation 3.4 with outcomes from a climate survey as the outcome. The survey data comes from the Yale Program on Climate Change Communication, which conducts a national survey about beliefs, perceptions, and preferences regarding climate change and global warming. The regression coefficients on the interacted Fire $\times$ 5% term shows that individuals in politically competitive counties that burn are more likely to believe that global warming is happening and to support requiring utility companies to utilize renewable sources (Table 4.7).

³⁴Even if the amount of government aid does differ by political competitiveness, the instrument would still be relevant for government aid, defined more broadly than the FEMA program. It would be an instrument for the bundle of provided aid. However, I do not find evidence that a politically competitive county that experiences a fire receives increased aid of other types. The public assistance expenditure and the reinstated Medi-Cal are both close to statistically significance at the 10% level for the interacted Fire $\times$ 5% term. However, they are both negative, meaning that politically competitive areas that experience a fire receive less of these programs, in contrast to what would be predicted for this instrument.

5 Government Aid Results

5.1 Preliminary Analysis Results: Government Aid vs. No Government Aid

Since receipt of government aid is not as-good-as-random and can be related to factors that also impact migration, this specification does not yield a causal estimate of the effect of aid. Nonetheless, the correlations shed light on the relationship between aid and individual outcomes. In [Figure 3.1](#), I find that individuals in blocks that are burned for the first time and that receive aid are 16.3 p.p. (72.8%) more likely to have moved away two years after the fire whereas those in blocks that are burned but do not receive aid are 2.2 p.p. (9.8%) more likely to have moved. After four years, those in burned blocks that receive aid are 9.5 p.p. (27.0%) more likely to remain away, and those in blocks that did not receive aid are 3.9 p.p. (11.0%) more likely. The coefficients for the fires with aid and those without aid are statistically significantly different from each other for the four post-fire years at the 5% level.

Government aid is also associated with more adaptive migration. [Figure 3.2](#) shows that individuals in blocks that are burned for the first time that receive aid are 6.8 p.p. (27.3%) more likely to move to a low- or medium-wildfire-risk tract after two years, and those in burned blocks that did not receive aid are 3.4 p.p. (13.5%) more likely, though the coefficients are not statistically significantly different at the 5% level. After four years, people in burned blocks that receive aid are 5.3 p.p. (18.3%) more likely to be in safe areas, and those in burned blocks that do not receive aid are 2.2 p.p. (7.5%) less likely; these coefficients are statistically significantly different at the 5% level. However, conducting a placebo counterfactual analysis in which I replace the migration location for those living in blocks that were burned by a fire, I find that people are less likely to move to low- or medium-wildfire-risk areas after two years than if they were a random mover within the sample, though they are more likely after four years ([Figure 4.74](#)).

Receipt of aid is hypothesized to help those with low credit scores. I find that individuals across the credit groups who are in a block hit by a first fire that receives aid are more likely to move away (Figure 4.73), though even with aid, only the individuals with high credit scores are statistically significantly more likely to be in a low- or medium-wildfire-risk area (Figure 4.74). People with high credit scores are more likely to move (Figure 4.75) and to do so to a safe place (Figure 4.76), even without aid, though the coefficients are not statistically significant for the fourth year for people in areas with fires but not aid. For people with low credit scores, receiving aid is associated with an increased likelihood of moving (Figure 4.77) and of doing so to a safe place (Figure 4.78), though the coefficients are not statistically significant for the fourth year. However, in the case of fires without aid, people with low credit scores are less likely to be in a safe place after four years, though the coefficient is not statistically significant. Since the short-term rental aid can be provided for up to 18 months (FEMA 2024b), the dissipation of the effects after two years could reflect the impact of no longer receiving aid.

In conclusion, government aid is associated with increased migration and migration to safe areas. People with low credit scores do not move to safe areas, even with aid. These results could be interpreted as more severely affected areas having larger migration responses and being more likely to receive aid or as aid helping individuals move away and move to safer areas.

5.2 Causal Impact of Aid

DID-IV Results

When estimating the DID regression (Equation 3.2), I find FEMA IHP aid receipt leads to a 17.0 p.p. increase (statistically significant at 1%) in the likelihood of moving away from the tract and a 5.5 p.p. increase in the likelihood of an individual's being in a low- or

medium-wildfire-risk area (statistically significant at 5%, [Table 3.4](#)). However, when I use the instrument, I find that this measure of aid is associated with a 33.0 p.p. increase in the probability of moving away (statistically significant at 1%), suggesting that aid receipt helps individuals relocate. This estimate is almost twice the size of the DID result. Moreover, this measure of aid is associated with a statistically insignificant -2.6 change in the likelihood of an individual’s being in a low- or medium-wildfire-risk-area. This result is robust to including the county-level data on local expenditures, non-FEMA government aid, and tax amounts that were used to evaluate the exclusion restriction into the regression ([Table 4.8](#)).³⁵ Although government aid does help individuals move, it does not lead them to migrate to safe areas.

These results are consistent with the fact that there are no restrictions on where the individual can move to receive aid ([Table 3.4](#)). This result could also be related to the results in [section 5.4](#), in that the aid helps those whose homes are not damaged move away – thus leading to larger migration rates – but these groups are not more likely to move to safe areas.

Although government aid did not facilitate adaptive migration, it did help individual’s financial situations. The receipt of IHP aid leads to a 0.008 decrease in the financial distress index ([Table 3.5](#)), which is similar in magnitude to the impact of having a denied abortion (Miller, Wherry, and Foster 2020). It also leads to a 0.006 increase in the credit access index ([Table 3.6](#)), which could be the mechanism through which individuals access in order to weather the financial hit from the fire. There is no impact of aid on the borrowing index ([Table 4.9](#)).

The financial results further highlight the importance of correctly estimating the impact of aid since the DID and the DID-IV results differ. The DID results suggest that aid is not associated with any meaningful change in the financial distress index, the credit access index, nor the borrowing index ([Table 3.5](#); [Table 3.6](#); [Table 4.9](#)). However, once the endogeneity of aid is corrected using the political competitiveness instrument, I find that aid

³⁵I exclude the Medi-Cal data as a county-level control in this regression because there is only one year overlap in time with the other datasets.

does help individuals' financial situations by decreasing their likelihood for financial distress and increasing their credit access. This showcases an important benefit of aid in helping individuals recover from wildfires, even if it does not lead to adaptive migration.

6 Conclusion

This project provides novel empirical estimates of the impact of government disaster aid on the extent of adaptive migration – the movement from risky to safe areas – and peoples' financial outcomes after natural disasters. Although government aid helps people move after a wildfire, it does not induce them to move to safe areas in the longer term. However, aid does help people from falling into financial distress and increases their access to credit.

To assess whether existing government aid can address the financial constraints that may restrict people after a wildfire, I conduct a novel causal analysis of government aid, using the fact that politically competitive counties are more likely to receive any FEMA Individual and Households Program (IHP) funding as an instrument for aid. I interpret the results from Section 5 through the lens of my conceptual framework from Section 2. If the barrier to adaptive migration is only the initial migration cost – a liquidity constraint – I would expect that people who receive this short-term disaster aid would have higher migration rates out of the burned area and to safe areas. If the issue is instead an inability to pay long-term rental costs – a solvency constraint – I would expect that the government aid would assist people in moving adaptively, for as long as the disaster aid allows them to afford to (i.e., until the government aid runs out). My instrumental variables analysis finds that aid leads to increased migration, though not to safe places. This suggests that the government aid helps some individuals assume the initial migration cost and that a liquidity constraint prevents some people from moving away. Since people initially move to safe areas but do not stay in the longer term, this suggests that there could be a solvency constraint that

the government is not adequately addressing. These results suggest that the current aid system does not provide enough financial resources to promote long-term adaptive migration. Indeed, that FEMA’s IHP aid program provides short-term rental assistance for up to 18 months after the disaster could explain why there is an initial increase in adaptive migration that dissipates after two years, as people become unable to afford to live in the safe areas.³⁶ Although the structure of our current aid system succeeds in helping some undertake the initial migration cost, it is not conducive for long-term adaptive migration. If the government wants to promote this adaptation strategy, it needs to change the way that aid is distributed.

Nevertheless, the government aid does reduce the financial distress that people experience after a wildfire. There is evidence that it does so by providing people access to credit, which could help tide them over as they navigate the monetary damages associated with experiencing a wildfire. These results show that aid is helping people weather the negative shock of a wildfire, which is an important benefit of the program. Government aid is beneficial for the financial well-being of individuals, even if it does not lead to adaptive migration.

One way for the government to facilitate adaptive migration is to incorporate location-specific conditions to aid. Instead of providing aid to rebuild homes damaged by natural disasters, the government could instead restrict reconstruction in risky areas, which is what Canada has done in very high flood hazard zones (Flavelle 2019). This would not only discourage the initial residents from returning, but it would also restrict the number of people moving in. Managed retreat is another potential adaptive migration policy that moves entire communities away from areas vulnerable to climate change, such as the relocation of Isle de Jean Charles away from coastal Louisiana (Louisiana Office of Community Development 2021). This policy is intended to maintain the community – one of the amenities of the neighborhood – and to create a planned community in a safe area for the residents. However,

³⁶An alternative interpretation could be that individuals “forget” about the wildfire risk and change their beliefs about wildfire risk over time. This seems highly unlikely given the destructive and traumatic nature of surviving a wildfire.

managed retreat is expensive and requires significant coordination: In 2016, Louisiana was awarded \$48 million from the US Department of Housing and Urban Development to relocate the residents of Isle de Jean Charles, though the plan took years to implement and frustrated the local community (Wendland 2019). However, it would guarantee that the residents move to a safe area. The government could also provide aid conditional on the safety of the location. I find that individuals are already moving adaptively in the short term, suggesting that individuals have a preference for safe areas. However, people seem to return to riskier areas in the longer term, suggesting they might not be able to afford to stay in the safer locations. Providing job search assistance or longer-term rental assistance could help people stay in these safe areas. Although these policies can seem expensive, they could be cost-effective if they prevent losses from future disasters. Evaluating the cost-effectiveness of these different types of government programs would be helpful to determine the most efficient way to promote adaptive migration.

There could be other explanations for why there is too little adaptive migration. People could lack information about the risks of wildfires or about safe areas, which could prevent them from moving adaptively. The government could intervene by requiring more information about the riskiness of an area to be disclosed to potential buyers. Although I see some suggestive evidence that people do have some sense of the wildfire safety of an area because I find an increase in the short-term likelihood of moving to safe areas, more detailed survey data about people's beliefs would be helpful for understanding what information they have about wildfire risk. Another explanation could be that the wildfire does not necessarily impact the individual's labor market. Autor et al. (2014), Yagan (2019), among others, find a limited migration response to a labor market shock. In the case of a wildfire, one might expect even lower migration responses since the impacted individuals are still tied to their jobs in the area. I define migration at a much finer level than the commuting zone, so my results can be consistent with their findings: People may move but not move far because they retain their

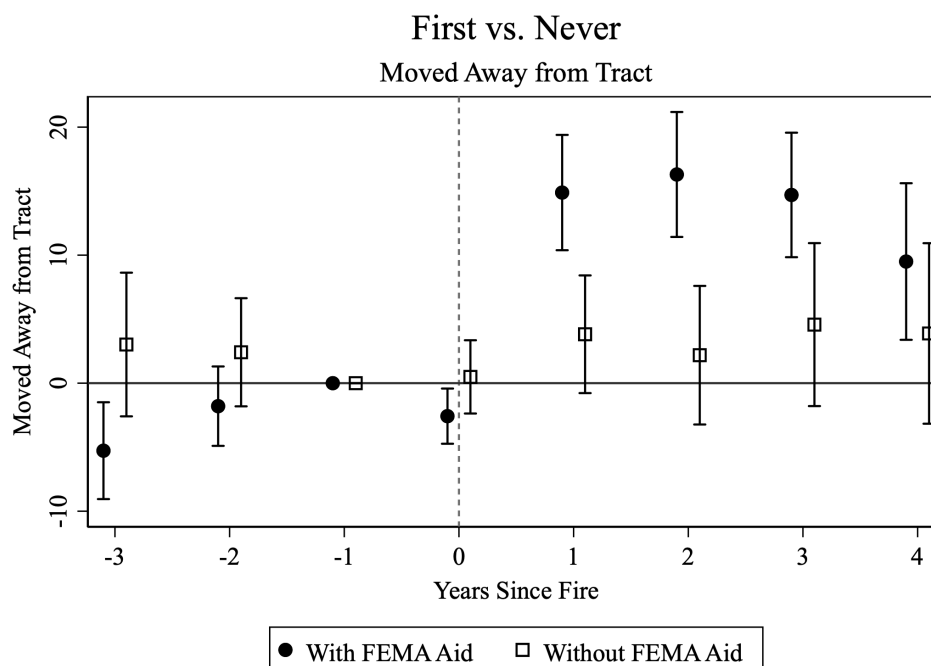
old jobs. Further work on the impact of wildfires on local employment and whether there are differential migration responses based on this dimension would be illuminating. This paper explores financial constraints as one potential mechanism, and assessing the others is key for future research.

When evaluating overall welfare and the impacts of government aid, it is also important to consider the implications for residents of the safe areas into which people move. People in the receiving areas will experience higher congestion, increased housing demand, and increased labor supply, which may translate to increased housing prices (Schubert 2024) and lower wages if housing supply and labor demand do not change. The government should consider changes within the receiving areas as part of policies that promote adaptive migration to minimize the negative welfare impacts on the safe areas.

Although government aid increases migration, in its current structure, it does not increase migration to safe places. Aid does decrease financial distress, though, so it is effective in helping people survive the damages associated with a wildfire. However, as natural disasters become more common and the expected costs increase, it is important to consider which adaptation strategies are most effective and the role of the government to promote them. The government has the duty to not only protect its constituents after a disaster but also to use resources in a fiscally responsible manner. Finding the balance between these two goals is key for the future of designing disaster policies.

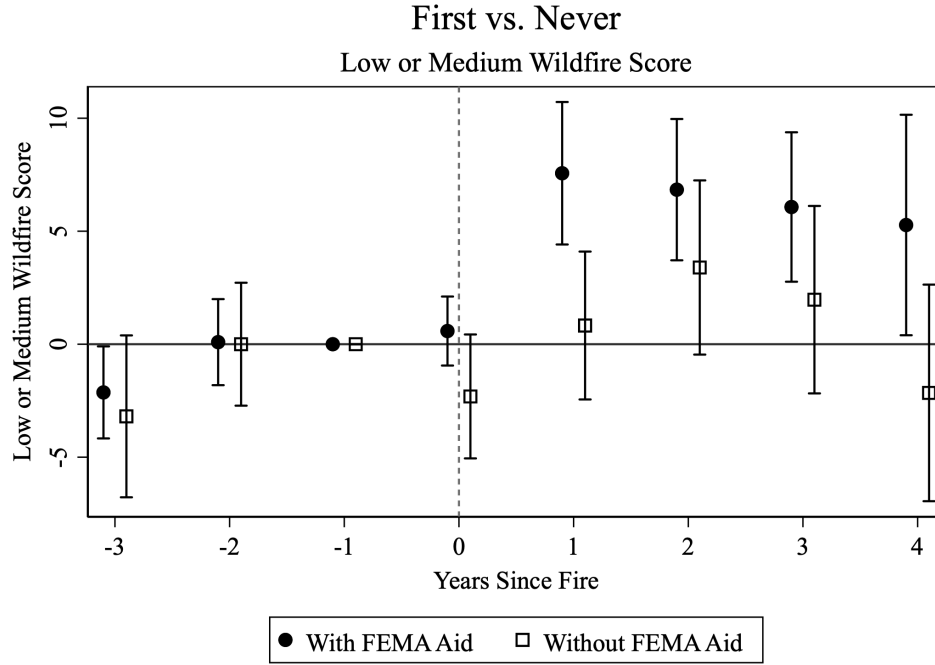
7 Figures

Figure 3.1: Moved Away from Tract after First Fire with Aid



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 1.1](#) in the UCCCP data from 2014 to 2021, separately for first fires for which FEMA Individuals and Households Program aid is disbursed and first fires for which it is not. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the fire (“Moved Away from Tract”). There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy. Individuals who experience first fires for which aid is disbursed are more likely to move away, whereas those who experience first fires for which aid is not disbursed show no migration effects. This is correlational evidence of the effect of aid on migration.

Figure 3.2: Wildfire Risk after First Fire with Aid



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating Equation 1.1 in the UCCCP data from 2014 to 2021, separately for first fires for which FEMA Individuals and Households Program aid is disbursed and first fires for which it is not. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. There are minimal differential likelihoods of an individual being in a safe tract before the fire, lending credibility to this empirical strategy. Individuals who experience first fires for which aid is disbursed are more likely to be in safe areas, even four years after the fire, whereas those who experience first fires for which aid is not extended are not. This is correlational evidence of the effect of aid on migration.

8 Tables

Table 3.1: Politically Competitive vs. Politically Noncompetitive County Summary Statistics: 2000-2021

	<5%		>5%			
	Mean	SD	Mean	SD	Difference	p-value
Area Characteristics						
Total Voters	252,300	(282,937)	223,150	(486,939)	29,150	0.00
Experienced Fire	0.84	(0.37)	0.75	(0.43)	0.09	0.01
Experienced Megafire	0.24	(0.43)	0.30	(0.46)	-0.06	0.17
Number of Fires	7.21	(8.62)	7.55	(10.51)	-0.34	0.71
Number of Megafires	0.11	(0.35)	0.07	(0.31)	0.04	0.28
Average Fire	8.07	(37.92)	5.55	(26.66)	2.52	0.48
Burned Total	45.15	(148.87)	29.10	(122.48)	16.05	0.26
Largest Fire	33.74	(116.71)	22.25	(91.46)	11.50	0.30
Aid Characteristics						
IHP Aid	0.12	(0.32)	0.04	(0.20)	0.08	0.01
IHP Aid per Capita	9.78	(81.92)	1.40	(16.69)	8.38	0.27
Total Approved IHP Aid	1,069,911	(8,012,799)	81,232	(715,451)	988,679	0.00
Total	119		1,157			
IHP Aid if Fire	0.14	(0.35)	0.05	(0.23)	0.09	0.02
IHP Aid per Capita if Fire	11.64	(89.32)	1.87	(19.26)	9.77	0.28
Total Burned by Fire	100		867			
IHP Aid if Megafire	0.28	(0.45)	0.07	(0.25)	0.21	0.02
IHP Aid per Capita if Megafire	38.01	(164.75)	2.17	(20.51)	35.84	0.25
Total Burned by Megafire	29		349			
Log IHP Aid per Capita	1.83	(2.80)	1.36	(2.40)	0.48	0.58
Percent Approved for FEMA Aid	16.11	(10.17)	13.99	(12.22)	2.12	0.52
Total Received IHP	14		47			

Notes: This table reports summary statistics for counties that are politically competitive vs. those that are not. I define a politically competitive county as one in which the difference in the vote share between the top two candidates in the most recent presidential election is less than 5%. I use data from MIT Election Labs to define the vote shares. The area characteristics are calculated from data from CAL FIRE, with the exception of Total Voters, which comes from MIT Election Labs. The aid characteristics are calculated from FEMA's Housing Assistance Program Data, combining the Owners and Renters data. There are 58 counties in California, and there were six presidential elections between 2000 and 2021. For the election years in this time period, there are 32 county-years that are politically competitive, including 18 unique counties and 9 counties that are politically competitive for more than one election.

Table 3.2: First Stage: Politically Competitive 2014-2021

	Post $\times$ Treated $\times$ IHP
Post $\times$ Treated $\times$ Political Competitiveness ($< 5\%$)	0.329*** (0.076)
Post $\times$ Treated	0.610*** (0.068)
R-squared	0.616
N	288,299
Mean	0.228
F-stat	58.02
Controls	Yes
Relative Time FE	Yes
Calendar Year FE	Yes
Individual FE	Yes
Cluster	County

Notes: I instrument receipt of any FEMA Individuals and Households Program (IHP) aid by political competitiveness. I define a politically competitive county as one in which the difference in the vote share is within five percent ($< 5\%$). I use data from MIT Election Labs to define the vote shares. I estimate [Equation 3.3](#) using UCCCP data from 2014 to 2021. I include the average size of the fires within the county (in thousands of acres), the age category of the individual, whether she is married, whether she has a college education, and the duration of time she lives in the census block as controls. I also include relative year, calendar year, and individual fixed effects. I cluster the regression at the county level.

Table 3.3: Exclusion Restriction: Politically Competitive Counties and Spending

	Fire		< 5%		Fire x < 5%		
	Coefficient	p-value	Coefficient	p-value	Coefficient	p-value	Time Frame
Expenditures (\$)							
Total	393,727,240	0.001	174,315,184	0.491	-266,342,283	0.312	2003-2019
General	20,403,124	0.005	3,539,983	0.820	1,543,792	0.933	2003-2019
Health	28,135,304	0.077	82,868,661	0.228	-65,642,667	0.328	2003-2019
Public Assistance	129,160,127	0.000	103,394,595	0.114	-110,227,330	0.112	2003-2019
Fire Spending	7,454,573	0.005	-2,836,463	0.009	14,079,207	0.009	2003-2019
Food Stamps							
Households	114,405	0.000	141,937	0.012	-77,897	0.221	2003-2018
Benefits (\$)	35,133,306	0.000	44,845,163	0.009	-15,849,734	0.434	2003-2018
Value (\$)	35,088,473	0.000	45,048,065	0.009	-15,940,276	0.428	2003-2018
Medi-Cal							
Renewals Due	19,726	0.326	-83,609	0.006	-10,214	0.638	2018-2021
Continued	5,486	0.783	-50,578	0.026	-12,837	0.526	2018-2021
Reinstated	4,340	0.111	-878	0.443	-4,166	0.115	2018-2021
Taxes							
Collected	1,524,605	0.470	-11,285,056	0.010	778,062	0.883	2012-2018

Notes: The exclusion restriction requires that a county's political competitiveness impact migration through only aid receipt, nothing else. Although this condition is not directly testable, I evaluate the likelihood of increased expenditures, non-FEMA government aid, and tax amounts as a way of assessing the validity of the exclusion restriction. I define a politically competitive county as one in which the difference in the vote share between the top two candidates in the most recent presidential election is within 5%. I use data from MIT Election Labs to define vote shares. I estimate Equation 3.4 using a variety of county-level data sources from the California Open Data website. The expenditures data come from California's County Financial Transactions Report on Expenditures. The food stamps data come from CalFresh, California's implementation of the Supplemental Nutrition Assistance Program (SNAP). The data for Medi-Cal (California's version of Medicaid) come from California Health and Human Services. The tax data come from the California Department of Tax and Fee Administration. I find that politically competitive counties that experience a fire only have higher fire-related spending, which is consistent with what my instrument is intended to capture.

Table 3.4: DID and DID-IV Results: Adaptive Migration

	<u>DID</u>		<u>DID-IV</u>	
	Moved Away from Tract	Low or Medium Wildfire Risk Tract	Moved Away from Tract	Low or Medium Wildfire Risk Tract
Post $\times$ Treated $\times$ IHP	18.898*** (2.711)	7.714** (1.893)	35.170*** (8.762)	-0.540 (8.923)
Post $\times$ Treated	-2.651 (2.044)	-0.685 (1.457)	-14.195** (7.928)	4.577 (4.686)
N	250,594	288,299	250,594	288,299
Mean	17.40	22.23	17.40	22.23
First Stage F-stat			17.18	18.92
Controls	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes
Calendar Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Cluster	County	County	County	County

Notes: This table reports the coefficients from the difference-in-differences (DID) specification, which estimates [Equation 3.2](#), and the DID instrumental variables specification (DID-IV), which first estimates [Equation 3.3](#) for FEMA Individuals and Households Program (IHP) aid. In both specifications, I control for the average size of the fires within the county (in thousands of acres), the age category of the individual, whether she is married, whether she has a college education, and the duration of time she lives in the census block. I also include relative year, calendar year, and individual fixed effects. I cluster the regression at the county level.

Table 3.5: DID and DID-IV Results: Financial Distress Index

	DID	DID-IV
Post x Treated x IHP Aid	0.000 (0.000)	-0.008** (0.004)
Post x Treated	-0.026*** (0.002)	-0.020*** (0.003)
Post	0.052*** (0.004)	0.052*** (0.004)
Individual Fixed Effects	Yes	Yes
Controls	Yes	Yes
N	250,594	250,594
First Stage F-stat		73.86

Notes: This table reports the coefficients from the difference-in-differences (DID) specification, which estimates [Equation 3.2](#), and the DID instrumental variables specification (DID-IV), which first estimates [Equation 3.3](#) for FEMA Individuals and Households Program (IHP) aid. In both specifications, I control for the average size of the fires within the county (in thousands of acres), the age category of the individual, whether she is married, whether she has a college education, and the duration of time she lives in the census block. I also include relative year, calendar year, and individual fixed effects. I cluster the regression at the county level.

Table 3.6: DID and DID-IV Results: Credit Access Index

	DID	DID-IV
Post x Treated x IHP Aid	-0.000 (0.000)	0.006** (0.003)
Post x Treated	0.019*** (0.002)	0.015*** (0.003)
Post	-0.039*** (0.005)	-0.039*** (0.005)
Individual Fixed Effects	Yes	Yes
Controls	Yes	Yes
N	250,594	250,594
First Stage F-stat		73.86

Notes: This table reports the coefficients from the difference-in-differences (DID) specification, which estimates [Equation 3.2](#), and the DID instrumental variables specification (DID-IV), which first estimates [Equation 3.3](#) for FEMA Individuals and Households Program (IHP) aid. In both specifications, I control for the average size of the fires within the county (in thousands of acres), the age category of the individual, whether she is married, whether she has a college education, and the duration of time she lives in the census block. I also include relative year, calendar year, and individual fixed effects. I cluster the regression at the county level.

Chapter 4

Appendix and Supplementary Material

1	Appendix to “Migration as Climate Adaptation: Evidence from California Wildfires”	124
1.1	Additional Figures	124
1.2	Additional Tables	149
1.3	California Background	154
2	Appendix to “Sustained Burn: Analyzing Post-Wildfire Financial and Location Outcomes”	157
2.1	Financial Outcomes	157
2.2	Amenities for Children	174
2.3	Labor Market	178
2.4	Pollution	186
2.5	Housing	193
3	Appendix to “Soothing the Burn: The Impact of Government Aid on Weathering Wildfires”	202

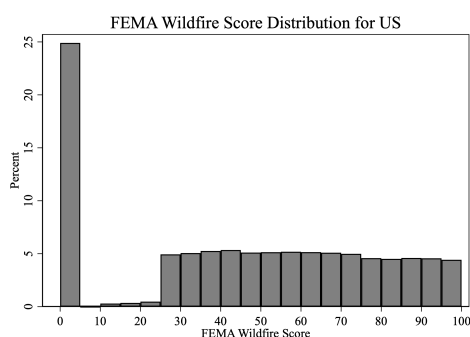
3.1	Additional Figures	202
3.2	Additional Tables	210

1 Appendix to “Migration as Climate Adaptation: Evidence from California Wildfires”

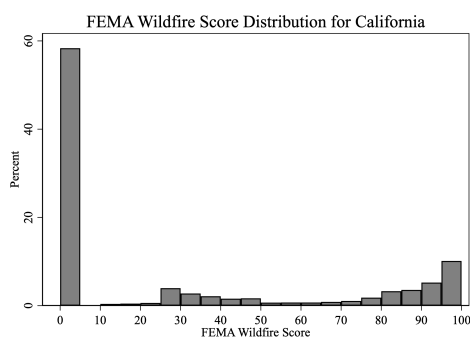
1.1 Additional Figures

Figure 4.1: FEMA Wildfire Score Distribution

(a) FEMA Wildfire Score Distribution for the United States



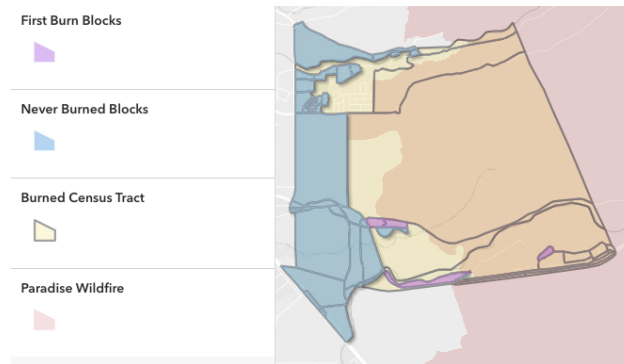
(b) FEMA Wildfire Score Distribution for California



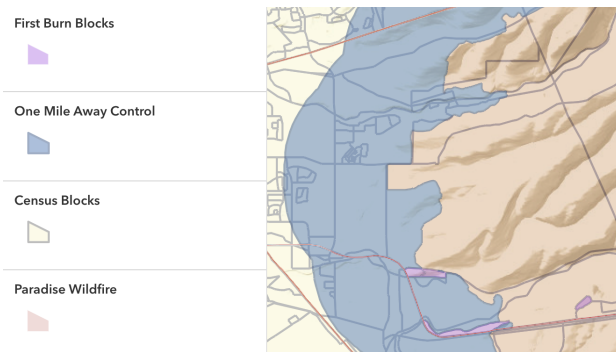
Notes: These graphs show the wildfire score distribution for the United States and California. The data are from FEMA and list the wildfire risk score at the census tract level. This is a one-year measure of wildfire risk, defined at the year 2021. [Figure 4.1](#) suggests that the wildfire risk score in the United States overall is roughly uniform when considering the tracts with a score lower than 25 as one entity (i.e., <25 , or “Low Wildfire Risk”). [Figure 4.1\(b\)](#) suggests that the wildfire risk score in California is not uniform but skewed to the left, with a higher proportion of tracts with minimal wildfire risk. This may seem surprising but is likely due to California’s large surface area and the fact that most of the state is not at the wildland-urban interface. There is also a larger proportion of tracts that are “Very High Wildfire Risk” (i.e., >75) in California than in the United States overall.

Figure 4.2: Map Example of Treatment vs. Control Groups

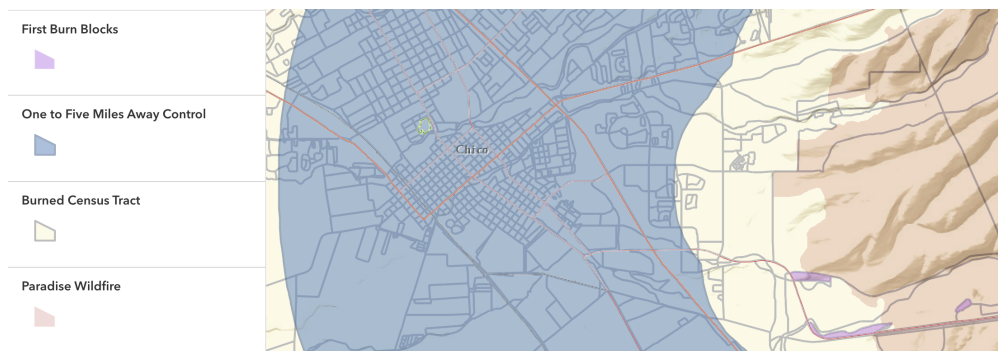
(a) Map Example of First Burn vs. Never Burned



(b) Map Example of First Burn vs. Within One Mile Away

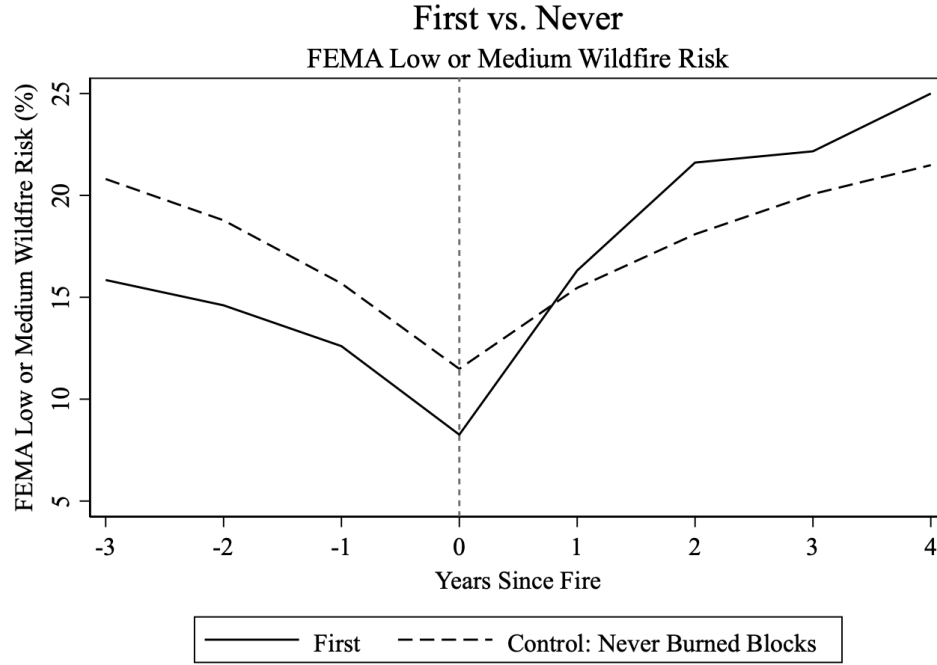


(c) Map Example of First Burn vs. One to Five Miles Away



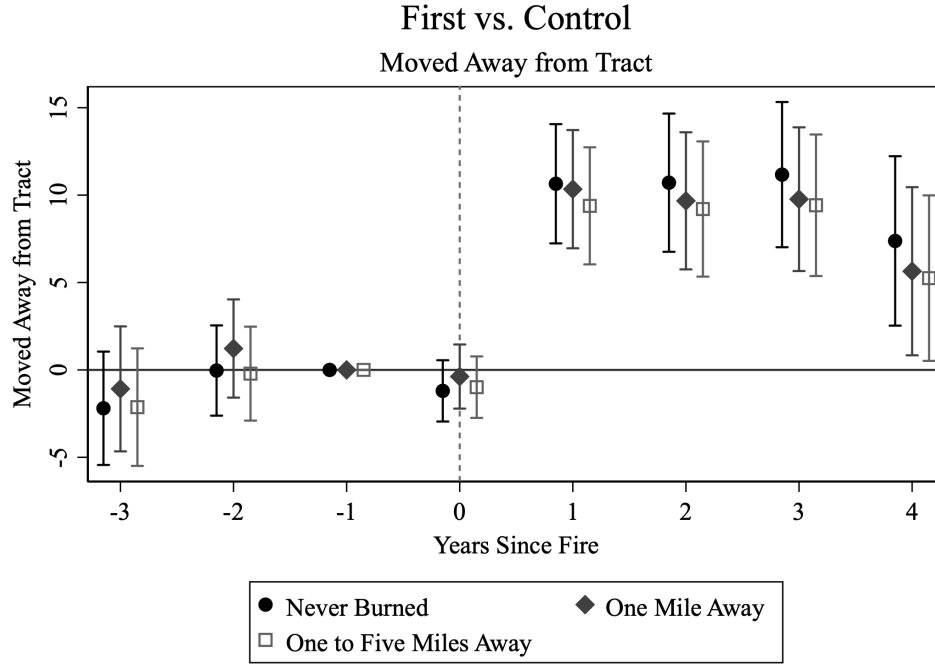
Notes: These maps show an example of the treatment and various control groups used in the analysis. The purple area represents the census blocks that are burned for the first time (in this case, by the Paradise Fire). The blue area denotes the various definitions of the control groups: blocks that are never burned but within a census tract that has a block that is burned for the first time, blocks within the one mile perimeter of the fire that burns a block for the first time, and blocks within a one- to five-mile perimeter of that fire.

Figure 4.3: Average Wildfire Risk for First vs. Never Burned



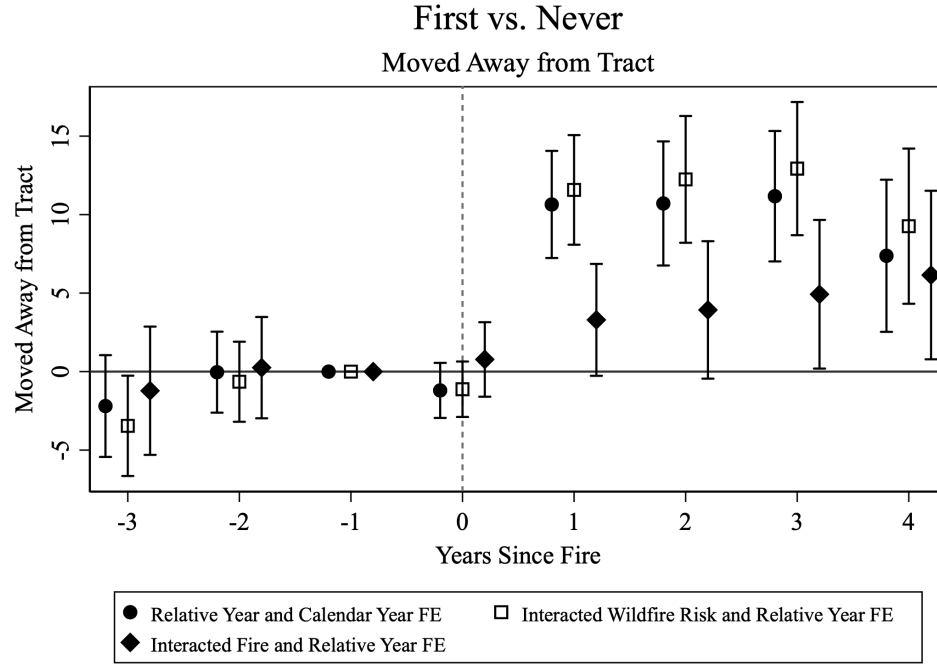
Notes: This graph shows the percentage of individuals in a low- or medium-wildfire-risk census tract for the treated and control groups using the UCCCP data. The wildfire risk data come from FEMA. The treated group consists of individuals living in blocks that are burned for the first time. The control group consists of people living within the same census tract as the first-burned blocks but in blocks that are never burned. The time is standardized to be relative to the year of the first fire that impacted the block. Before the fire (i.e. time 0), the wildfire risks are parallel, giving credibility to this empirical strategy. They are not identical because the control group is defined separately for each fire, so there could be individuals within the control group who are represented multiple times in the dataset. For instance, it could be that a relatively safe census tract has multiple census blocks within it that are burned for the first time, and people in the control group areas with a lower wildfire risk score are counted multiple times, raising the proportion of people in low- or medium-wildfire-risk tracts for the control group.

Figure 4.4: Moved Away from Tract across Control Groups



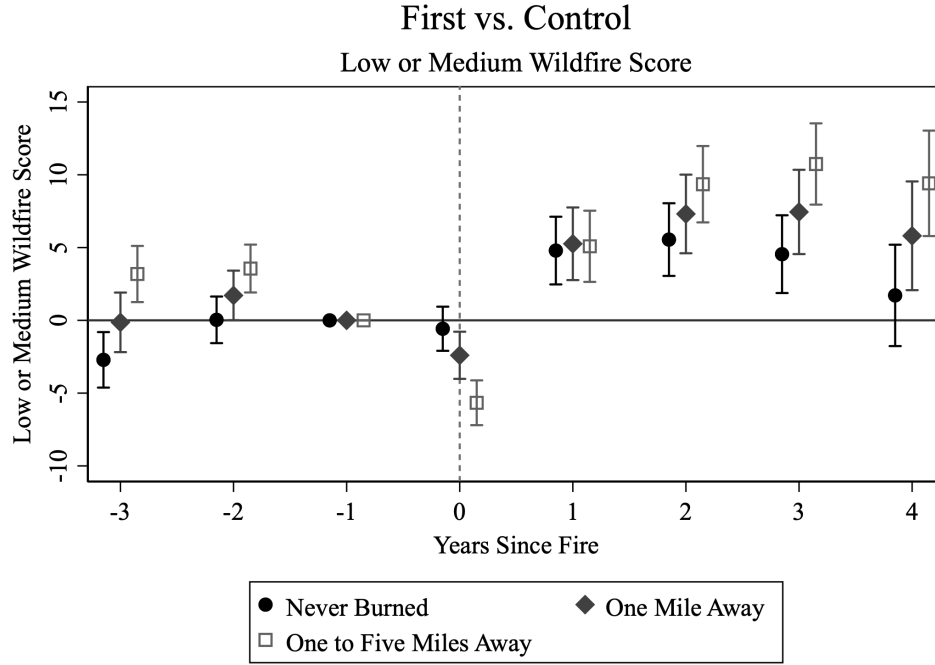
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 1.1](#) in the UCCCP data from 2014 to 2021 and with different control groups. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy. There is a sharp increase in the likelihood of an individual’s likelihood of moving away starting the year after the fire, and this effect persists for four years after the fire. [Figure 1.3](#) shows suggestive evidence that the dip in the fourth year is from people in the burned blocks moving back.

Figure 4.5: Moved Away from Tract across Empirical Specifications



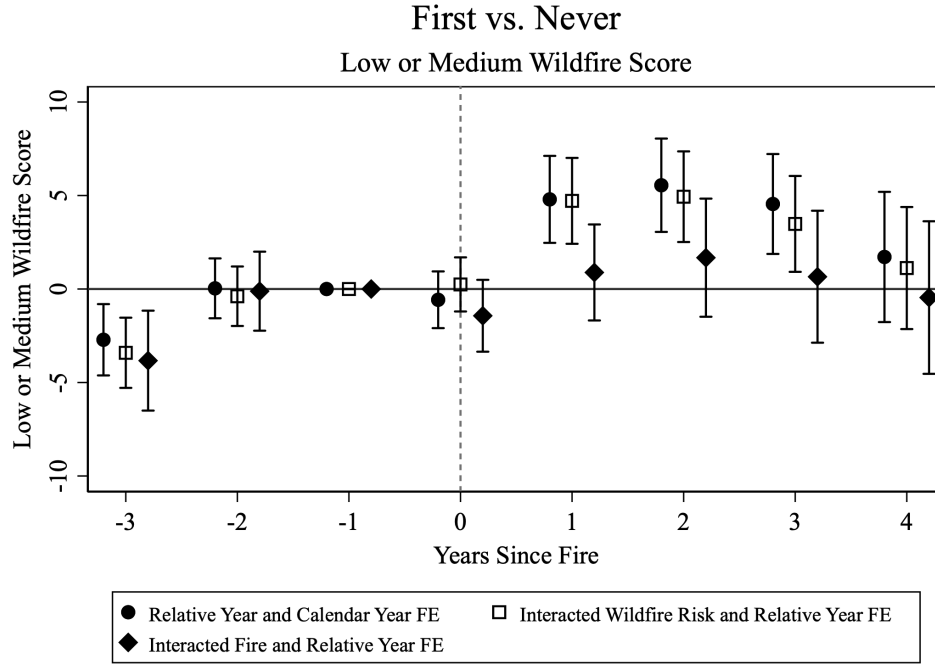
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating different empirical specifications in the UCCCP data from 2014 to 2021. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). There is an increase in the likelihood of moving away starting the year after the fire, and this effect persists for four years after the fire across all specifications. The “Relative Year and Calendar FE” plots the coefficients from estimating [Equation 1.1](#). The “Interacted Wildfire Risk and Relative Year FE” plots the coefficients from estimating [Equation 1.3](#). The “Interacted Fire and Relative Year FE” plots the coefficients from estimating [Equation 1.4](#).

Figure 4.6: Wildfire Risk across Control Groups



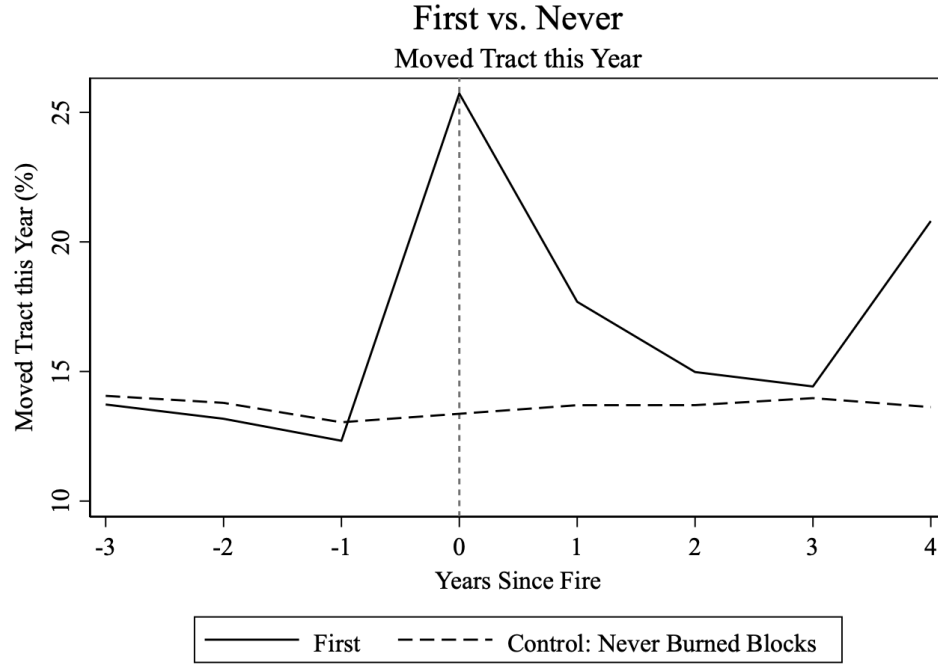
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 1.1](#) in the UCCCP data from 2014 to 2021 and with different control groups. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. My preferred control group is individuals in never-burned blocks, because by definition, the wildfire risk scores are the same for the treated and the control because the wildfire risk is defined at the census tract level. Using the other control groups – people within one mile away and people one to five miles away from the perimeter of the fire – may introduce bias because the wildfire risk scores for the control groups are different from the treated groups: At time 0, the year of the fire that I use to define the treated and the control groups, people in blocks burned for the first time are less likely to be in a safe area. For ease of interpretation, I prefer the never-burned block as the control group, but I present the results for the other control groups for transparency and completeness.

Figure 4.7: Wildfire Risk across Empirical Specifications



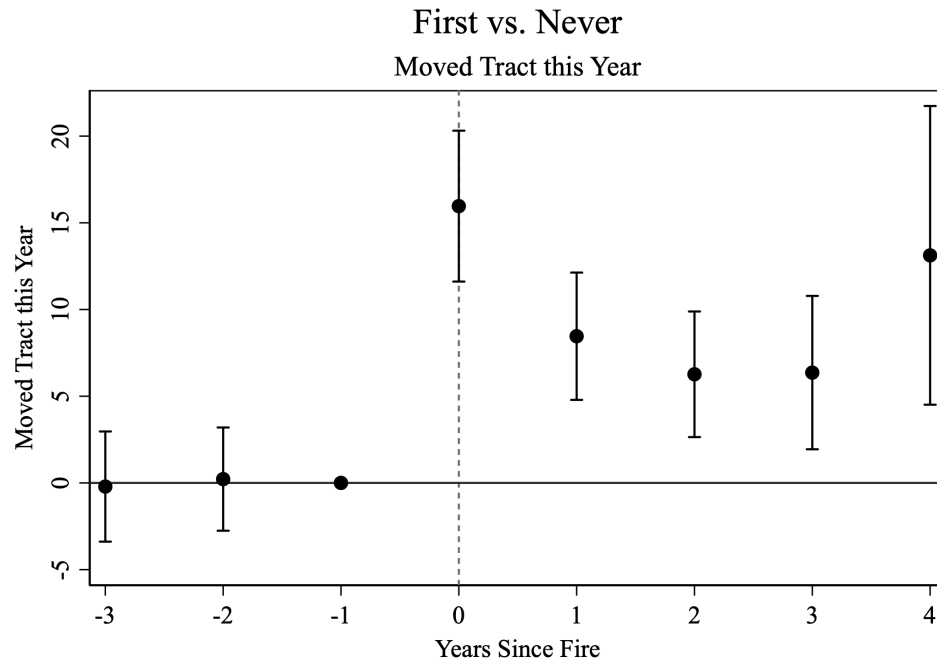
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating different empirical specifications in the UCCCP data from 2014 to 2021. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. There is an initial increase in the likelihood of moving to a safe area, though this falls over time. The “Relative Year and Calendar FE” plots the coefficients from estimating [Equation 1.1](#). The “Interacted Wildfire Risk and Relative Year FE” plots the coefficients from estimating [Equation 1.3](#). The “Interacted Fire and Relative Year FE” plots the coefficients from estimating [Equation 1.4](#).

Figure 4.8: Average Annual Migration Rate for First vs. Never Burned



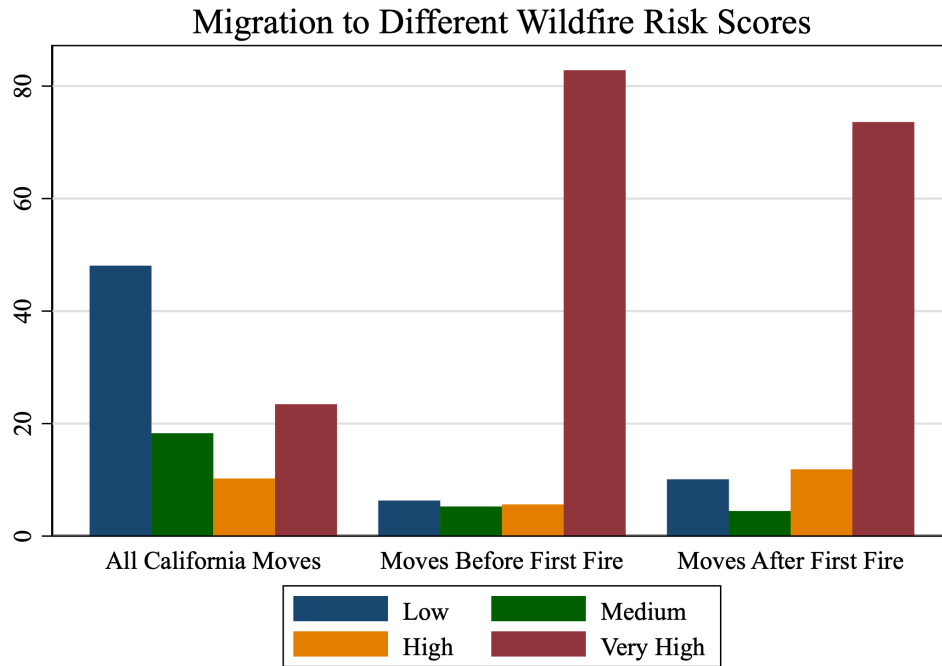
Notes: This graph shows the percentage of individuals who moved tracts within a given relative year from the fire (“Moved Tract This Year”) for the treated and control groups in the UCCCP data. The treated group consists of individuals living in census blocks that are burned for the first time. The control group consists of people living within the same census tract as the first-burned blocks but in blocks that are never burned. The time is standardized to be relative to the year of the first fire that impacted the block. Before the fire (i.e., time 0), the migration rates are the same and parallel, giving credibility to this empirical strategy. The increase in the likelihood of moving within a given year persists even years after the wildfire, suggests that individuals experience housing instability after the event.

Figure 4.9: Annual Migration Rate after First Fire



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 1.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data moved tracts within a given year (“Moved Tract this Year”). There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy. There is a sharp increase in the likelihood of moving within a given year starting the year of the fire, and this effect persists for four years after the fire.

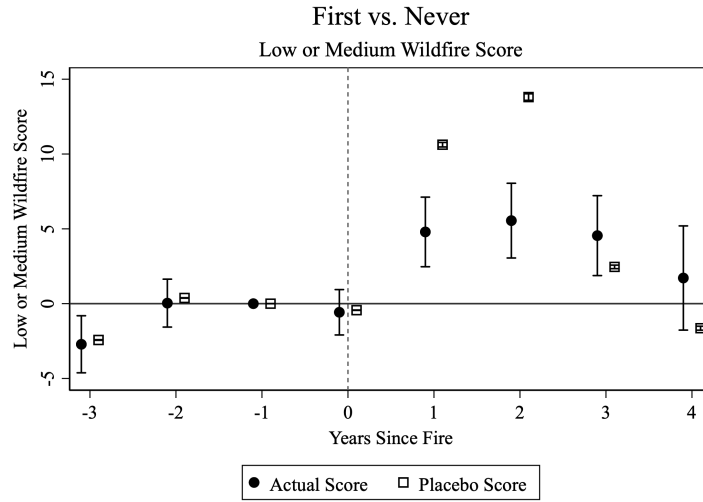
Figure 4.10: Migration to Different Wildfire Risk Scores



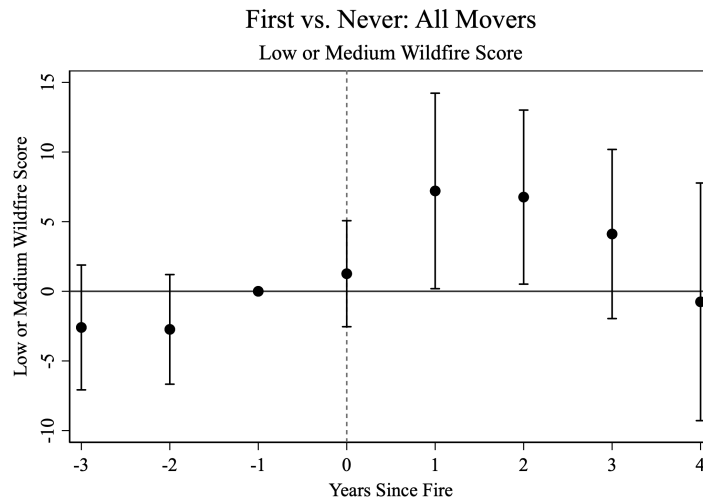
Notes: This graph shows the wildfire risk score distribution for people who moved in three different populations. “All California Moves” is the population of all movers within California from 2014 to 2021 in the UCCCP data. “Movers Before First Fire” is the population of individuals who lived in a block that is burned for the first time but who moved before the fire. “Movers After First Fire” is the population of individuals who lived in a block that is burned for the first time but who moved after the fire. Although there is an increase in the percentage of people who go to low- or medium-wildfire-risk areas, it is a far smaller proportion than would be expected from the overall California mover population. This suggests that people who live in high-risk areas may be selected in some way since their choice of wildfire risk is not similar to that of California movers overall.

Figure 4.11: Counterfactual Moves

(a) Random Moves by California Movers

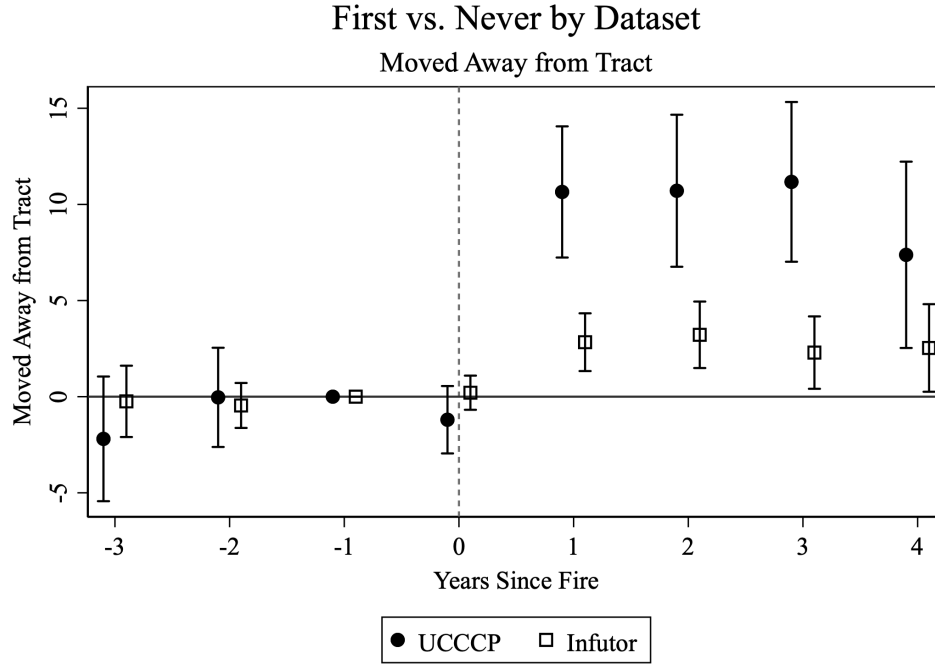


(b) Movers from Never-Burned Blocks



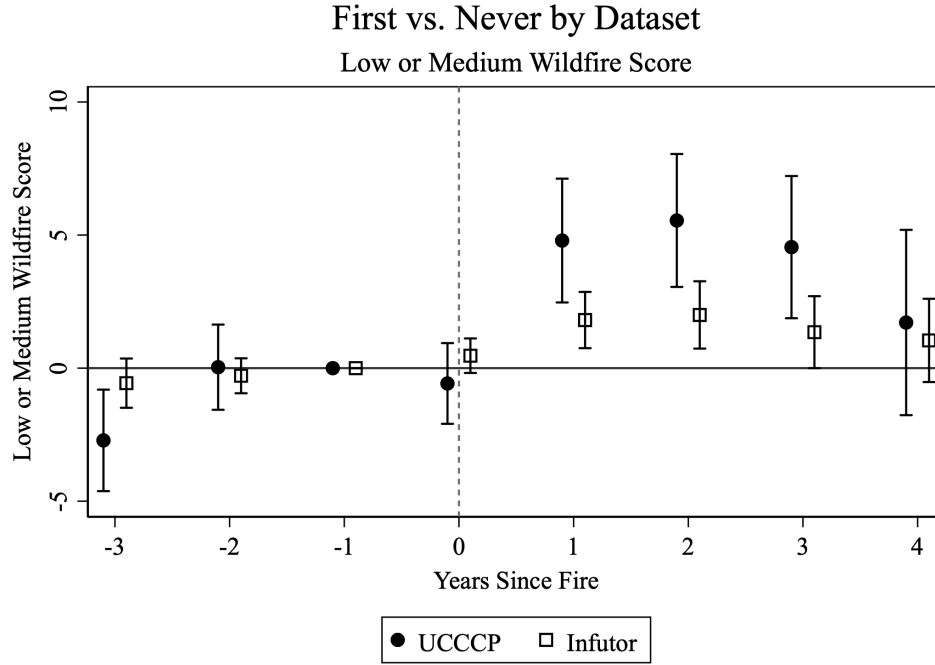
Notes: These graphs show the coefficients for the relative time indicator variables and the first fire interaction terms from estimating Equation 1.1 in the UCCCP data from 2014 to 2021 across two alternative control groups. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. There are minimal differential likelihoods of an individual being in a safe tract before the fire, lending credibility to this empirical strategy. In Figure 4.11(a), “Actual Score” shows the regression coefficients from taking the actual location decisions seen in the data, whereas “Placebo Score” shows the regression coefficients from replacing the location decisions of the treated after the wildfire with those of a random mover in California. These results show that though there is some evidence of adaptive migration in the first two years, it is lower than would be expected for the average Californian mover. The location choices are similar between people from burned blocks and the average mover after four years. Figure 4.11(b) plots the regression coefficients for the population of individuals who moved the year of the fire. Those in the area burned for the first time who moved are initially more likely to move to safe areas than people who moved from the never-burned blocks, though there are no differences in the longer term.

Figure 4.12: UCCCP and Infutor Migration Rates after First Fire



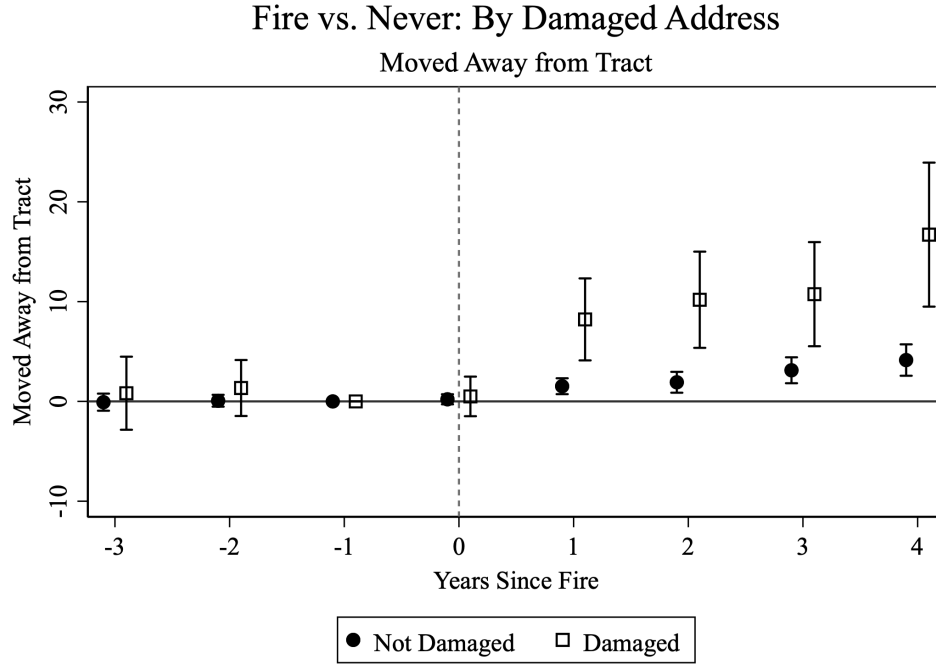
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 1.1](#) from 2014 to 2021 in two datasets. The first dataset is the University of California Consumer Credit Panel (UCCCP), and the second dataset is Infutor. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy across these datasets. There is a sharp increase in the likelihood of moving away starting the year after the fire, and this effect persists for four years after the fire for both datasets.

Figure 4.13: UCCCP and Infutor Wildfire Risk after First Fire



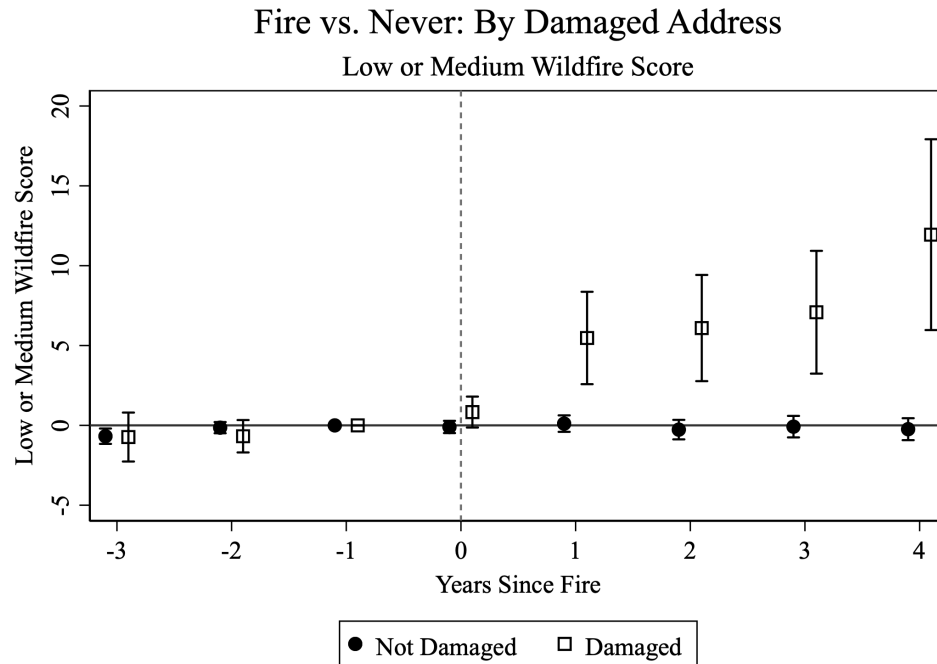
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating Equation 1.1 from 2014 to 2021 in two datasets. The first dataset is the University of California Consumer Credit Panel (UCCCP), and the second dataset is Infutor. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. There are minimal differential likelihoods of being in a safe tract before the fire, lending credibility to this empirical strategy. There is an increase in the likelihood of being in a safe tract after the fire, though the effect dissipates four years after the fire for both datasets.

Figure 4.14: Migration of Damaged vs. Not Damaged Addresses after First Fire



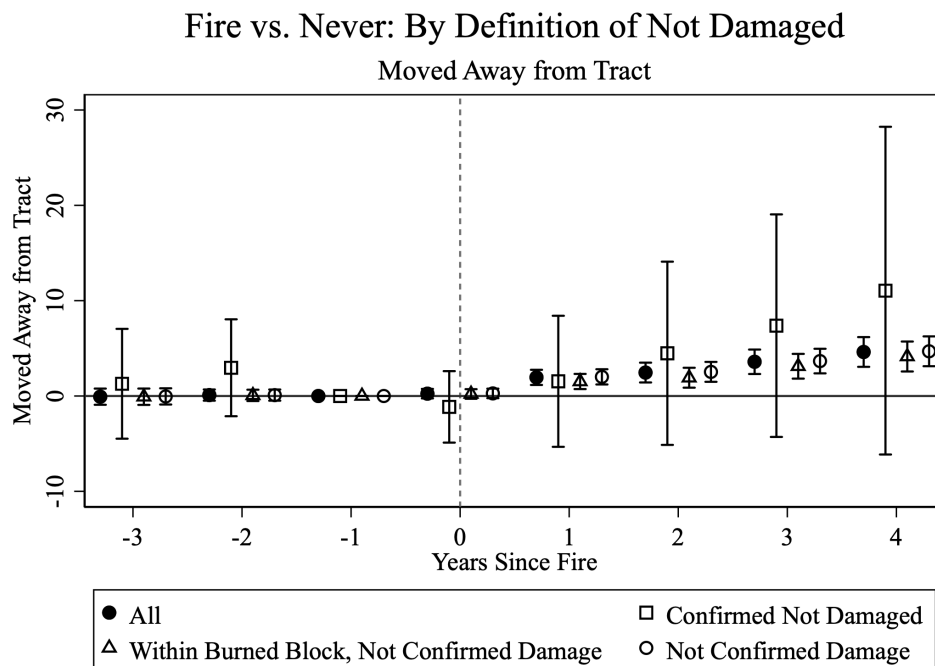
Notes: This graph shows the coefficients for the relative time indicator variables and the fire interaction terms from estimating [Equation 1.1](#) in the Infutor and the Cal Fire Damage Inspection (DINS) data from 2013 to 2021 across two different treatment groups. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). The “Not Damaged” coefficients are for people who live in a census block that is burned but whose homes are not identified as damaged from the DINS data. The “Damaged” coefficients are for people whose homes are identified as damaged from the DINS data. There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy. There is an increase in the likelihood of moving away starting the year after the fire across both groups, though understandably the coefficients are higher for those whose homes are damaged.

Figure 4.15: Wildfire Risk of Damaged vs. Not Damaged Addresses after First Fire



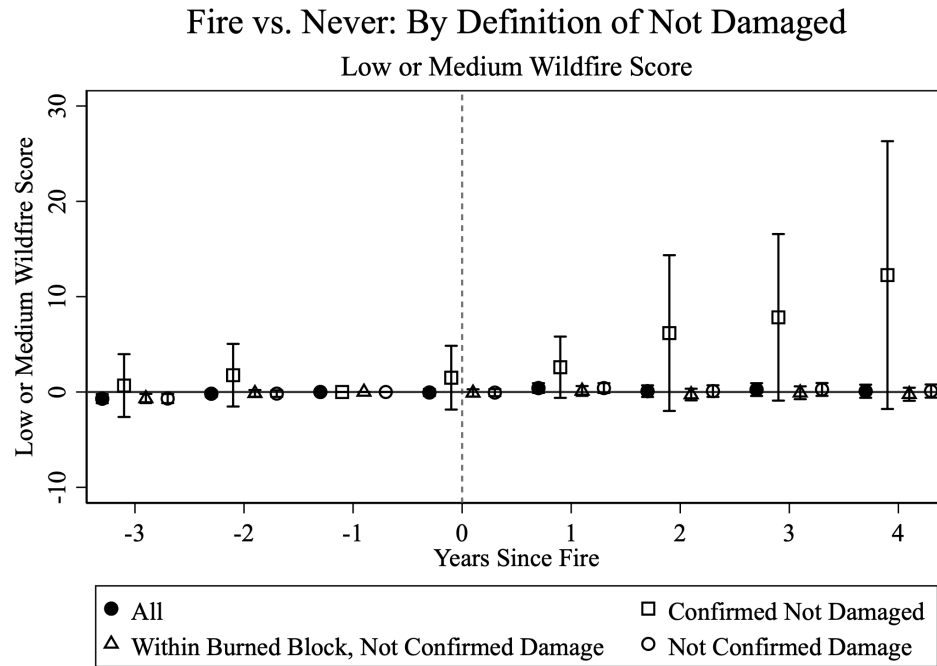
Notes: This graph shows the coefficients for the relative time indicator variables and the fire interaction terms from estimating Equation 1.1 in the Infutor and the Cal Fire Damage Inspection (DINS) data from 2013 to 2021 across two different treatment groups. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. The “Not Damaged” coefficients are for people who live in a census block that is burned but whose homes are not identified as damaged from the DINS data. The “Damaged” coefficients are for people whose homes are identified as damaged from the DINS data. There are no differential likelihoods of being in a safe tract before the fire, lending credibility to this empirical strategy. There is an increase in the likelihood of moving to a safe area starting the year after the fire only for those whose homes are damaged, which could be because only those whose homes are damaged receive substantial compensation after the fire.

Figure 4.16: Migration of Damaged vs. Not Damaged Addresses after First Fire across Not Damaged Definitions



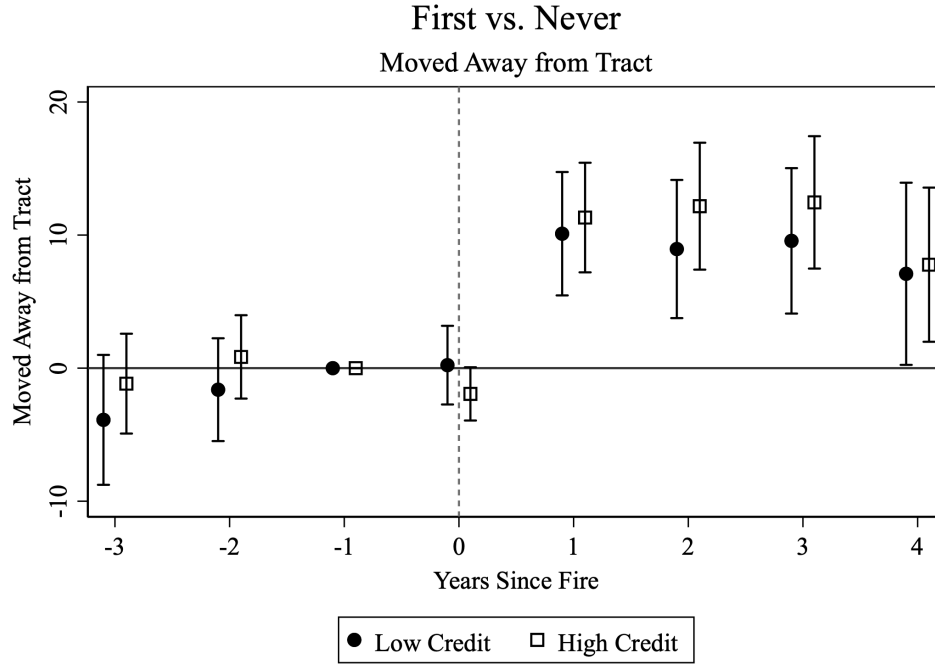
Notes: This graph shows the coefficients for the relative time indicator variables and the fire interaction terms from estimating [Equation 1.1](#) in the Infutor and the Cal Fire Damage Inspection (DINS) data from 2013 to 2021 across three different definitions of “Not Damaged.” The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). The “All” coefficients are for everyone who lived in a census block that is burned. The “Confirmed Not Damaged” coefficients are for people who live in a home that is confirmed to have no damage from the DINS data. Because this is a rare classification that they began to incorporate after 2018, the standard errors for this are likely large because of low power. The “Within Burned Block, Not Confirmed Damage” – the main definition of “Not Damaged” used – is for people who live in a census block that is burned for the first time but whose homes are not identified as damaged from the DINS data. The “Not Confirmed Damage” coefficients are for people whose homes are not in the DINS data. There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy. The trends are qualitatively similar across these different definitions of “Not Confirmed.”

Figure 4.17: Wildfire Risk of Damaged vs. Not Damaged Addresses after First Fire across Not Damaged Definitions



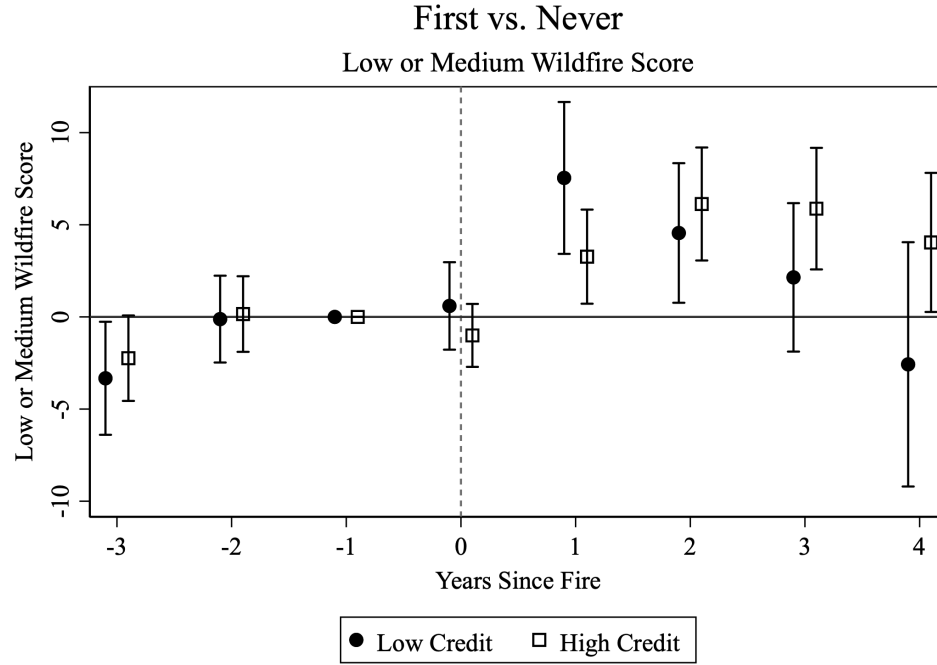
Notes: This graph shows the coefficients for the relative time indicator variables and the fire interaction terms from estimating Equation 1.1 in the Infutor and the Cal Fire Damage Inspection (DINS) data from 2013 to 2021 across three different definitions of “Not Damaged.” The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. The “All” coefficients are for everyone who lived in a census block that is burned. The “Confirmed Not Damaged” coefficients are for people who live in a home that is confirmed to have no damage from the DINS data. Because this is a rare classification that they began to incorporate after 2018, the standard errors for this are likely large because of low power. The “Within Burned Block, Not Confirmed Damage” – the main definition of “Not Damaged” used – is for people who live in a census block that is burned for the first time but whose homes are not identified as damaged from the DINS data. The “Not Confirmed Damage” coefficients are for people whose homes are not in the DINS data. There are no differential likelihoods of being in a safe tract before the fire, lending credibility to this empirical strategy. The trends are qualitatively similar across these different definitions of “Not Confirmed.” The increase in adaptive migration for those that are confirmed to not have damage is not statistically significant, though it could reflect that those who were motivated enough to request a inspection – even though they did not have any damage – could be more likely to want to move away from wildfire risk.

Figure 4.18: Moved Away from Tract after First Fire by Credit Score



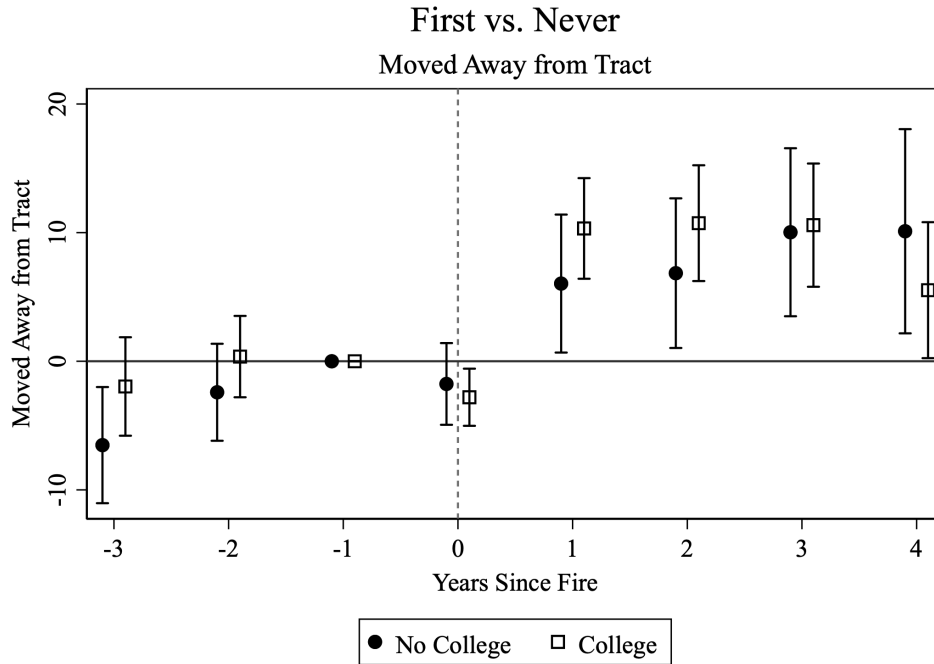
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 1.1](#) in the UCCCP data from 2014 to 2021 across two populations. The outcome variable is an indicator variable that equals one if the individual is at a different tract than where they were the year before the first fire (“Moved Away from Tract”). The populations are defined as people who have high credit scores the year before the fire (≥ 700) and people who have low credit scores (< 700). There are no statistically significant differences in migration rates before the fire for these groups, lending credibility to this empirical strategy. There is a sharp increase in the likelihood of moving away starting the year after the fire, and this effect persists four years after the fire for both credit groups.

Figure 4.19: Wildfire Risk after First Fire by Credit Score



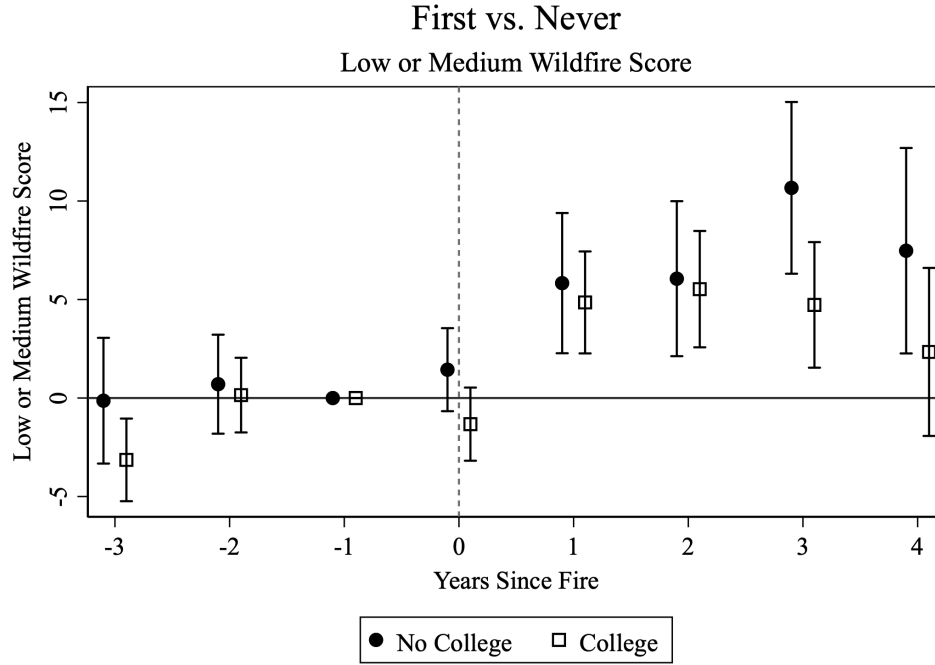
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating Equation 1.1 in the UCCCP data from 2014 to 2021 across two populations. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. The populations are defined as people who have high credit scores the year before the fire (≥ 700) and people who have low credit scores (< 700). There are no differential likelihoods of being in a safe tract before the fire, lending credibility to this empirical strategy. There is an increase in the likelihood of moving to a safe place starting the year after the fire. This increase persists only for people with high credit scores, who are statistically significantly more likely to be in a safe area four years after the fire. The coefficient for people with high credit scores is also statistically significantly different from the coefficient for people with low credit scores.

Figure 4.20: Moved Away from Tract after First Fire by Education



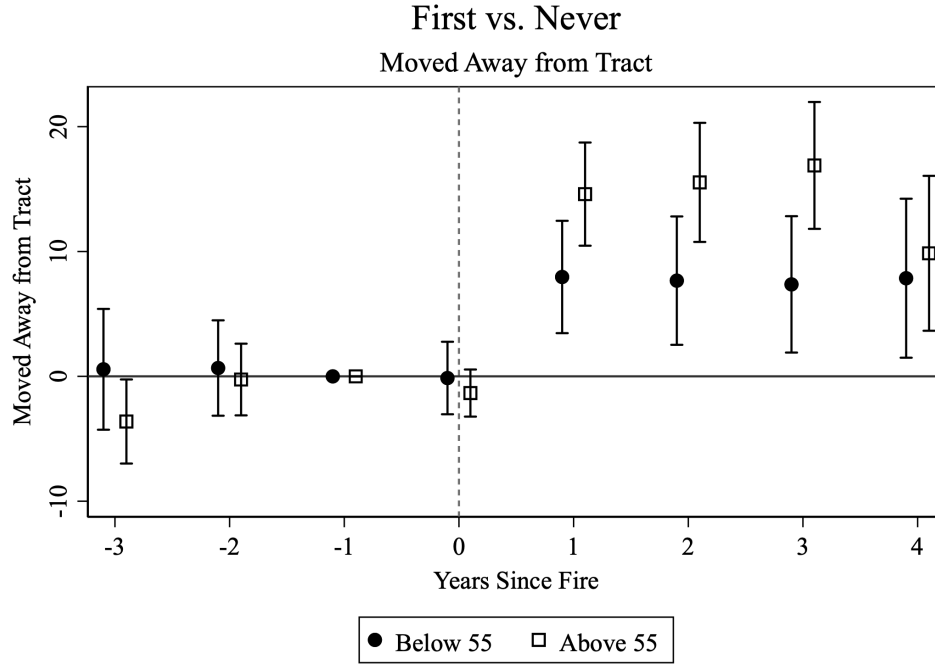
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 1.1](#) in the UCCCP data from 2014 to 2021 across two populations. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). The populations are defined as people who have a college degree and people without a college degree. There are minimal statistically significant differences in migration rates before the fire for these groups, lending credibility to this empirical strategy. There is a sharp increase in the likelihood of moving away starting the year after the fire, and this effect persists for four years after the fire for both education groups.

Figure 4.21: Wildfire Risk after First Fire by Education



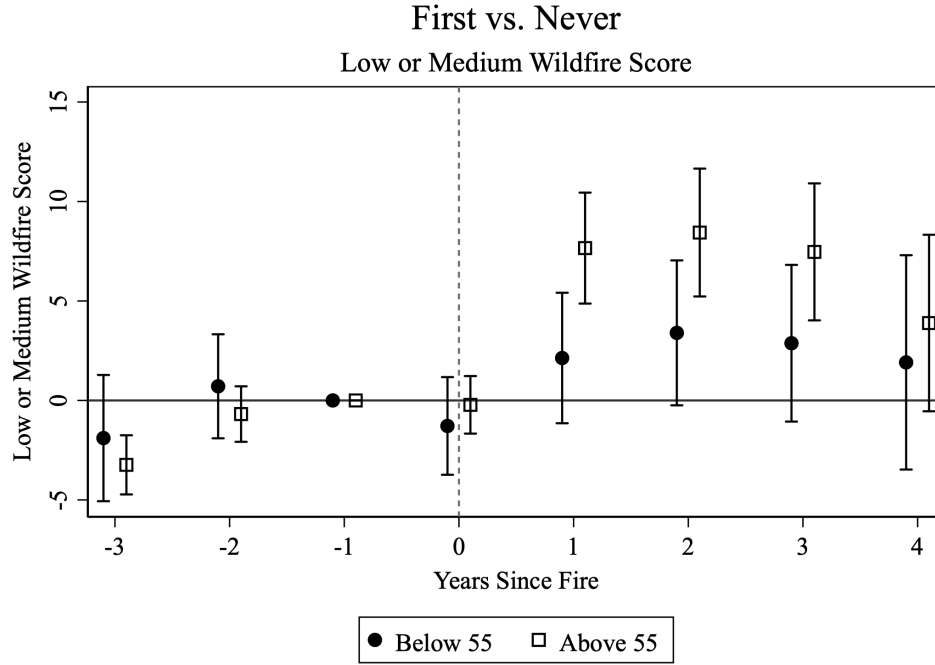
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating Equation 1.1 in the UCCCP data from 2014 to 2021 across two populations. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. The populations are defined as people who have a college degree and people without a college degree. There are minimal statistically significant differences in wildfire risk before the fire for these groups, lending credibility to this empirical strategy. There is an increase in the likelihood of moving to a safe place starting the year after the fire. This increase persists only for people without college, who are statistically significantly more likely to be in a safe area. However, the increase for those without college is not statistically significantly different from those with college.

Figure 4.22: Moved Away from Tract after First Fire by Age



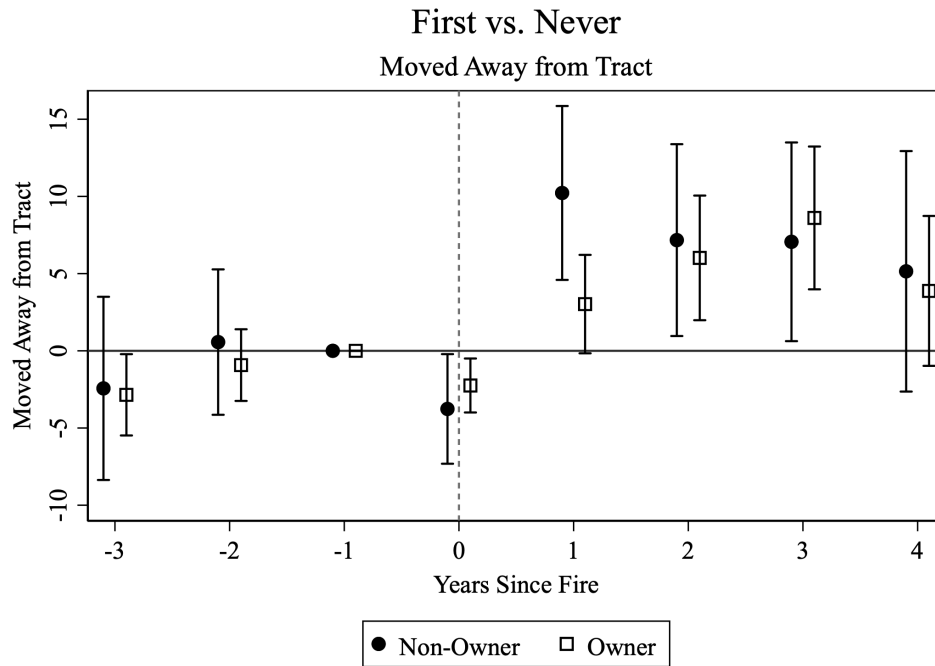
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating Equation 1.1 in the UCCCP data from 2014 to 2021 across two populations. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). The populations are defined as people who are below 55 at the time of the fire and people above 55. I choose 55 as the age cut-off because Proposition 19 allows people who are above 55 as well as people whose homes are destroyed by a wildfire to transfer their tax burden to another property in California. There are minimal statistically significant differences in migration rates before the fire for these groups, lending credibility to this empirical strategy. There is a sharp increase in the likelihood of moving away starting the year after the fire, and this effect persists four years after the fire for both age groups.

Figure 4.23: Wildfire Risk after First Fire by Age



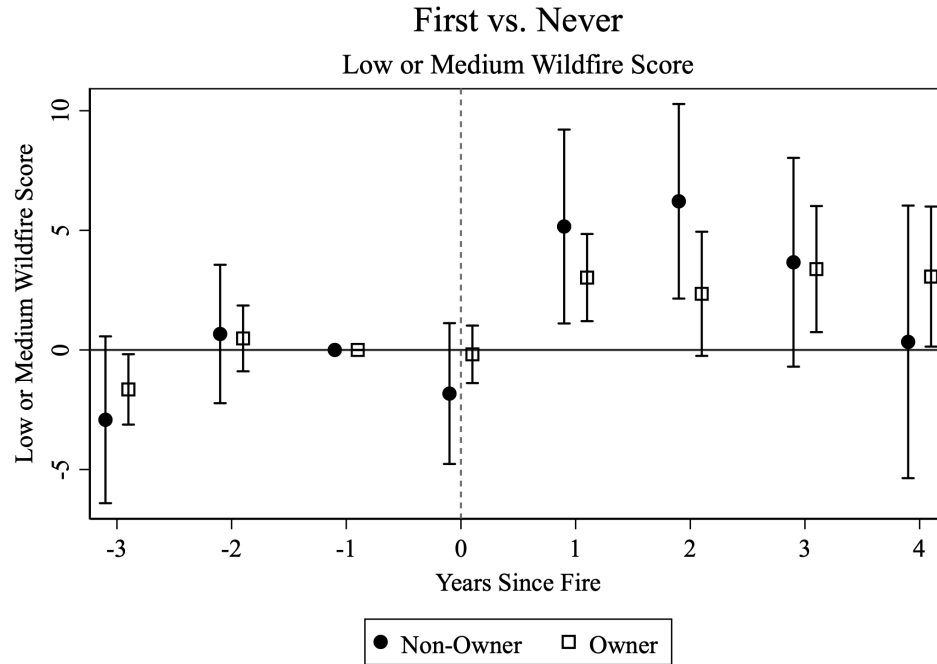
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating Equation 1.1 in the UCCCP data from 2014 to 2021 across two populations. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. The populations are defined as people who are below 55 at the time of the fire and people above 55. I choose 55 as the age cut-off because Proposition 19 allows people who are above 55 as well as people whose homes are destroyed by a wildfire to transfer their tax burden to another property in California. There are minimal differential likelihoods of being in a safe tract before the fire, lending credibility to this empirical strategy. There is an increase in the likelihood of moving to a safe place starting the year after the fire for people above 55. Neither group is more likely to be in a safe area compared to their control groups four years after the fire.

Figure 4.24: Moved Away from Tract after First Fire by Homeownership



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 1.1](#) in the UCCCP data from 2014 to 2021 across two populations. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). The populations are defined as people who are identified as a homeowner and people who are not. There are minimal statistically significant differences in migration rates before the fire for these groups, lending credibility to this empirical strategy. There is an increase in the likelihood of moving away starting the year after the fire. Neither group is more likely to move away compared to their control groups four years after the fire.

Figure 4.25: Wildfire Risk after First Fire by Homeownership



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating Equation 1.1 in the UCCCP data from 2014 to 2021 across two populations. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. The populations are defined as people who are identified as a homeowner and people who are not. There are minimal differential likelihoods of being in a safe tract before the fire, lending credibility to this empirical strategy. There is an increase in the likelihood of moving to a safe place starting the year after the fire. This increase persists only for owners, who are statistically significantly more likely to be in a safe area. However, the increase for owners is not statistically significantly different from people who are not.

1.2 Additional Tables

Table 4.1: Tracts with Higher Wildfire Risk Have Lower House Prices

	United States		California	
FEMA Wildfire Score	-1.550*** (0.028)		-1.088*** (0.086)	
FEMA Wildfire Risk Category		-43.171*** (0.799)		-32.219*** (2.732)
Constant	401.895*** (1.782)	436.583*** (2.407)	599.317*** (4.825)	629.791*** (6.862)
N	62,468	62,468	7,635	7,635

Notes: This table reports the coefficients from regressing wildfire risk on house prices. The wildfire risk data are from FEMA and list the wildfire risk score at the census tract level. “FEMA Wildfire Score” ranges from 0 to 100. “FEMA Wildfire Risk Category” is 1 if the tract is low wildfire risk (i.e., risk score < 25), 2 if medium wildfire risk (i.e., 25 to 50), 3 if high wildfire risk (i.e., 50 to 75), and 4 if very high wildfire risk (i.e., > 75). The house price data are from the Federal Housing Finance Agency (FHFA) and at the census tract level. The house price index (HPI) is calculated by measuring average price changes in repeat sales on the same set of properties. A higher HPI corresponds to more expensive properties. The data used in this regression are for the year 2021. The coefficients reflect that areas with higher wildfire risk have lower house prices, for both the United States overall and California alone.

Table 4.2: Movers vs. Non-Movers Summary Statistics

	Moved		Did Not Move		Difference	p-value
	Mean	SD	Mean	SD		
Demographic Characteristics						
Age	51.39	(18.67)	52.83	(18.02)	-1.44	0.29
Male	0.53	(0.50)	0.49	(0.50)	0.03	0.37
Married	0.59	(0.49)	0.63	(0.48)	-0.05	0.19
College	0.65	(0.48)	0.70	(0.46)	-0.05	0.14
Financial Characteristics						
Credit Score	687.72	(131.97)	708.42	(111.10)	-20.70	0.02
Number of Open Credit Cards	2.81	(3.13)	2.35	(2.79)	0.46	0.04
Total Credit Limit	19,014.30	(20,603.89)	22,233.68	(24,915.54)	-3,219.37	0.09
Total Payment	919.52	(1,177.67)	1,492.68	(2,615.52)	-573.16	0.00
Number of Open Loans	5.12	(4.71)	4.39	(4.32)	0.73	0.03
Number of Open Auto Loans	0.38	(0.66)	0.30	(0.57)	0.08	0.10
Has Mortgage Loans	0.26	(0.44)	0.32	(0.47)	-0.06	0.05
Bankruptcy	5.34	(22.53)	4.37	(20.45)	0.98	0.54
Number of Current Delinquencies	0.10	(0.37)	0.07	(0.38)	0.03	0.31

Notes: This table reports summary statistics for individuals that I observe in the UCCCP data who moved to a different tract the year of the fire with individuals who did not for the population of people who lived in a census block that burned for the first time. All characteristics are measured the year before the fire occurred.

Table 4.3: Return Movers vs. Non-Return Movers Summary Statistics

	Returned		Did Not Return		Difference	p-value
	Mean	SD	Mean	SD		
Demographic Characteristics						
Age	51.44	(15.95)	50.88	(17.86)	0.57	0.90
Male	0.63	(0.50)	0.61	(0.49)	0.02	0.89
Married	0.72	(0.46)	0.64	(0.48)	0.09	0.49
College	0.56	(0.51)	0.65	(0.48)	-0.10	0.48
Financial Characteristics						
Credit Score	712.33	(102.38)	733.41	(72.38)	-21.08	0.42
Number of Open Credit Cards	3.78	(4.92)	2.98	(3.12)	0.79	0.52
Total Credit Limit	30,945.83	(38,793.25)	23,198.04	(26,819.52)	7,747.80	0.52
Total Payment	1,763.71	(1,695.61)	1,320.39	(1,800.64)	443.33	0.39
Number of Open Loans	6.22	(6.09)	5.42	(4.98)	0.80	0.61
Number of Open Auto Loans	0.28	(0.46)	0.27	(0.54)	0.01	0.97
Has Mortgage Loans	0.39	(0.50)	0.27	(0.45)	0.12	0.38
Bankruptcy	0.00	(0.00)	6.06	(24.04)	-6.06	0.04
Number of Current Delinquencies	0.00	(0.00)	0.05	(0.21)	-0.05	0.08

Notes: This table reports summary statistics for individuals that I observe in the UCCCP data who returned within four years after the fire with individuals who did not return for the population of people who lived in a census block that burned for the first time and who moved the year of the fire. All characteristics are measured four years after the fire occurred.

Table 4.4: Characteristics Associated with Migration for Individuals in First- and Never-Burned Blocks

Variable	Coefficient	Standard Error
Age	-0.161***	(0.018)
College	0.410	(0.556)
Married	0.025	(0.508)
Male	-0.055	(0.403)
Duration	-1.497***	(0.180)
Wildfire Risk Score	-0.199***	(0.030)
Overall Risk Score	0.416**	(0.192)
Social Vulnerability Score	-0.067	(0.044)
Expected Annual Loss Score	-0.462**	(0.195)
Number of Insurance Providers	-0.121***	(0.038)
Percent FAIR Policies	0.808**	(0.336)
Credit Score	-0.025***	(0.004)
Number of Open Credit Cards	-0.893***	(0.186)
Total Credit Limit	-0.000	(0.000)
Total Payment	-0.000	(0.000)
Number of Open Loans	0.702***	(0.138)
Number of Open Auto Loans	0.443	(0.401)
Has Mortgage Loans	-5.486***	(0.571)
Bankruptcy	-0.005	(0.010)
Number of Current Delinquencies	-1.430**	(0.651)
Constant	70.956***	(5.525)
R^2	0.089	
N	83048	
Mean	14.05	

Notes: This table reports the coefficients from regressing the listed characteristics on whether the individual moved. The population included in the regression are people living in census blocks that burned for the first time or people living within the same census tract as the first-burned blocks but in blocks that are never burned. These individual-level characteristics are taken from the year before the wildfire burned. The coefficients reflect whether the characteristic is positively or negatively associated with the likelihood of moving. These regression coefficients are used to predict the expected migration rates in [Table 4.5](#). The data used in estimating this is from the University of California Consumer Credi Panel (UCCCP).

Table 4.5: Predicted Migration Rates for Individuals in First-Burn vs. Control Blocks

	Treated		Control		Difference	p-value
	Mean	SD	Mean	SD		
Control Groups						
Never	11.817	(9.649)	13.169	(10.072)	-1.352	0.000
One Mile Away	11.388	(7.401)	12.978	(7.883)	-1.590	0.000
One to Five Miles Away	12.667	(6.735)	13.550	(6.942)	-0.882	0.000

Notes: This table reports the predicted migration rates from estimating regressions of characteristics on whether the individual moved, as seen in [Table 4.4](#). This regression and prediction exercise is done for three different control groups: blocks that are never burned but within a census tract that has a block that is burned for the first time, blocks within the one mile perimeter of the fire that burns a block for the first time, and blocks within a one- to five-mile perimeter of that fire. The data used in estimating this is from the University of California Consumer Credi Panel (UCCCP). The predicted migration rates show that the burned areas have lower predicted migration rates compared to the various control groups.

1.3 California Background

Insurance

The insurance landscape in California also may cause distortions in people’s location decisions. For many years, Californian insurance companies were not able to incorporate forecasting models into their prices due to their lack of transparency so that they could only rely on historical fires (Frazier 2021). As a result, right after a fire occurs, the premiums of the area will increase, though the fire risk decreases immediately after a fire. Insurance companies also calculate damages in a way that disincentivizes migration. In 2021, California insurance companies were required to include the value of the land of destroyed property in their payouts if the person chose to relocate (CDI 2021). Before this, there would be large differences in the value of the land for those who chose to move away vs. rebuild, and this 2021 amendment was specifically put in place to address this discrepancy. Additionally, California state insurance regulators do not allow for the costs of reinsurance risk, which are secondary insurance contracts with global insurers to hedge against correlated or severe risks such as wildfires, to be included in insurer rate requests. Keys and Mulder 2024 finds that reinsurance costs have doubled from 2018-2023. These factors compound to dissuade private insurance carriers to stay within the California insurance market. As of June 2024, State Farm and All State have noted that they will not write new homeowner policies in the state Jacobson 2024. Since State Farm uses the most granular wildfire risk algorithms and has the largest market presence in high-risk areas (Boomhower et al. 2024), their withdrawal from the insurance market will have a large impact on homeowners.

Insurance companies in California respond to wildfires by increasing their prices or cancelling policies, which theoretically will make it more expensive to stay in these areas. Incumbent residents may be more likely to move out, and newcomers may find it more difficult to obtain a mortgage to move in. For those who stay but do not have insurance (i.e. if they

already paid off their homes so do not need a homeowner insurance policy, which is required for mortgages), experiencing another wildfire without the protection of insurance may have an even larger negative impact on the individual. Although California issued a mandatory one year moratorium on non-renewals on insurance companies after a wildfire beginning in 2019, this protects certain zip codes and requires a Governor emergency declaration for a nearby fire (CDI 2021).

California's insurance landscape incentivizes rebuilding the home. Many home insurance payouts are also contingent on rebuilding the home, drawing people back to the burned area. Until 2021, Californian insurance companies would not count the value of the land in their payout if the owner were to move away (Issler et al. 2024). Because the individual could gain hundreds of thousands of dollars more if they decided to stay, this would discourage migration. The California Insurance Commissioner, Ricardo Lara, issued a memo that provides an example of how some policyholders are penalized for choosing to exercise their right to purchase a home at a new location rather than rebuild. "If the insured's home was fully destroyed and it is determined that the cost to rebuild at its original location (up to policy limits) is \$400,000, and the cost to purchase the new property (including the home and the land) at a new location is at least \$400,000, the insured would receive \$400,000 to purchase an existing home at the new location. However, if the cost to purchase the new property is \$400,000 (including the home and the land and assuming the land value is \$200,000), some insurers would only pay \$200,000" (Lara 2019). However, since insurance companies are not allowed to use predictive models to price their products, they rely on historic wildfire incidents to justify higher prices. That means that although immediately after a wildfire occurs, the risk of another one is lower because the fuel in the area has been burned, these areas will experience an increase in their insurance prices. This increase could price out individuals who either decide or can no longer afford to live in that area.

Tax Benefits

Another California-specific institutional detail is Proposition 19, which includes an allowance for homeowners whose home is destroyed by a wildfire to take their tax burden if they were to purchase a new home in California. This follows a series of similar legislations over the years, which allowed this tax burden reallocation within the burned county as well as to specific counties throughout California (CBPC 2020; Dillon 2020; BOE 2021).

The most recent iteration of this legislation, Proposition 19, passed on November 3, 2020 and became effective on December 16, 2020. It allows homeowners who are over 55, severely disabled, or whose homes are destroyed by natural disasters to transfer their property tax bill from one home to another of any value anywhere in the state. Individuals who are in these categories and occupies their home as a principal residence and purchase a replacement home within two years qualify for this tax benefit (BOE 2021). This builds upon Proposition 13, which limits the property tax for a home every year to 1% of the property's value on the date of purchase. This prohibits the property from being reassessed for a new base year value unless the property is sold or construction is done to the property. These factors keep the property tax of the house far lower than the market value (CBPC 2020).

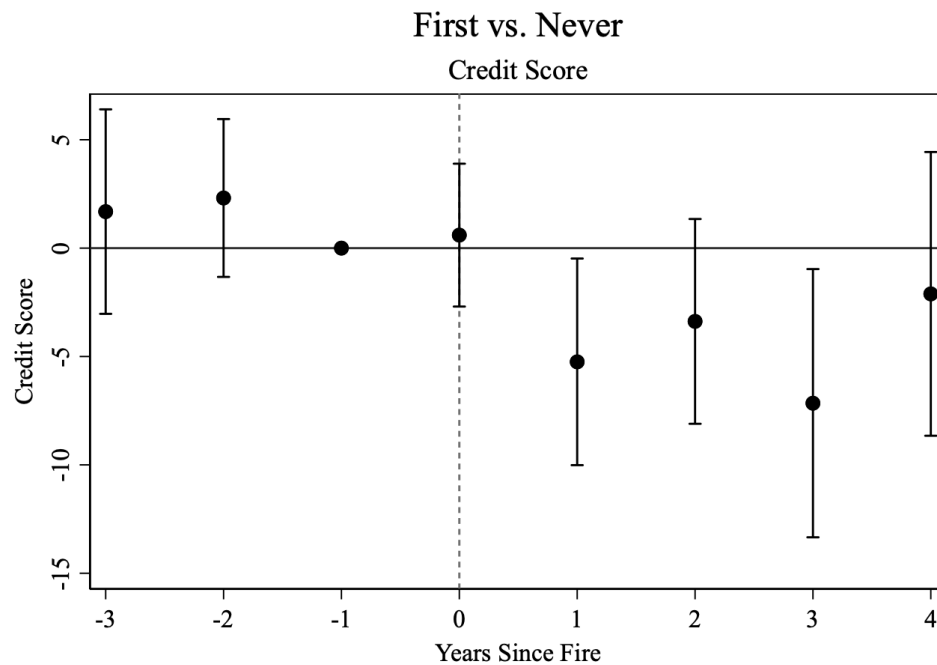
Proposition 19 has been criticized for disproportionately benefitting older, white, and wealthy California homeowners, with disaster-affected homeowners comprising under 1% of those eligible for tax relief (Dillon 2020; Kimberlin and Kitson 2020). However, the legislation requires the state to put the increased tax revenues that may be generated into wildfire response (Kimberlin and Kitson 2020). This legislation could incentivize people to stay within California to take advantage of keeping their, likely lower, tax burden. It could also help those who are able to afford it to move to more expensive homes – perhaps in safer areas – without incurring a higher property tax burden.

2 Appendix to “Sustained Burn: Analyzing Post-Wildfire Financial and Location Outcomes”

Additional Figures

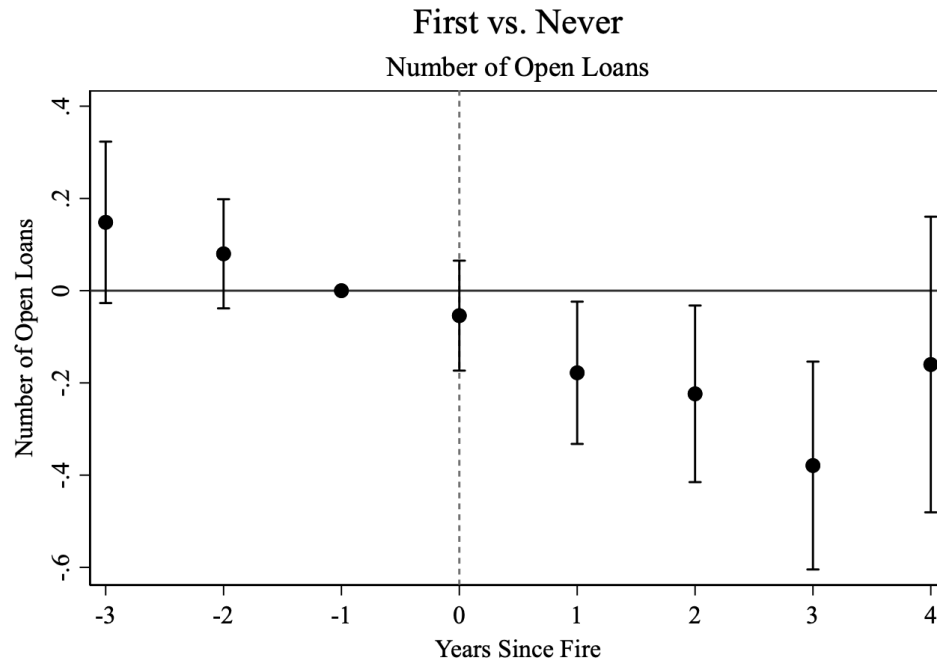
2.1 Financial Outcomes

Figure 4.26: Credit Scores after First Fire



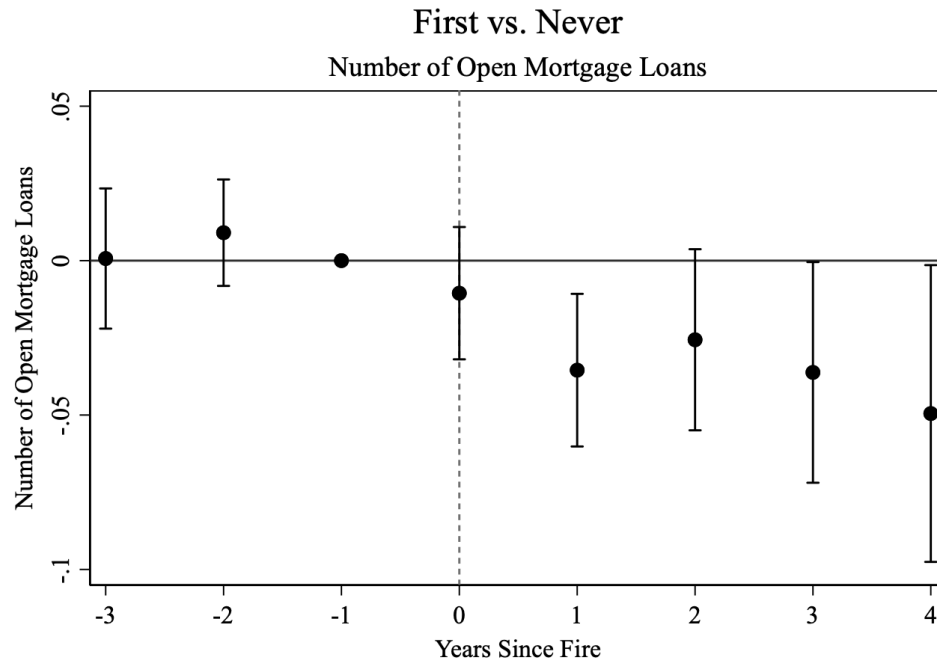
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the average credit score of an individual in a year. There are no statistically significant differences in credit score before the fire, lending credibility to this empirical strategy. There is a decrease in credit score after the fire, though the effect is not consistently statistically significant.

Figure 4.27: Number of Open Loans after First Fire



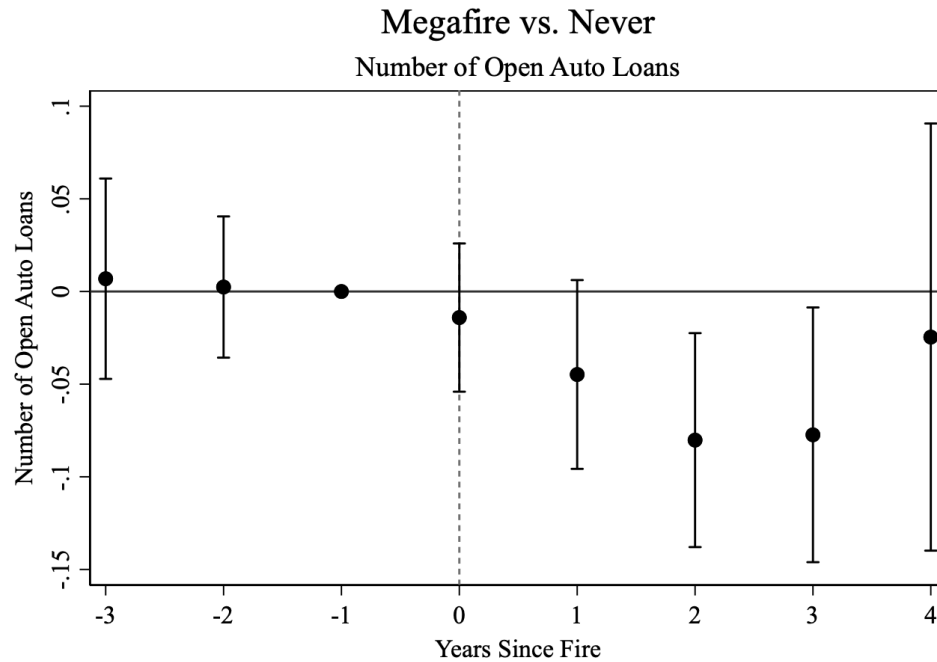
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of open loans that an individual holds. There are no statistically significant differences in the number of open loans before the fire, lending credibility to this empirical strategy. There is a decrease in the number of open loans after the fire, though the effect dissipates by the fourth year after the fire.

Figure 4.28: Number of Open Mortgage Loans after First Fire



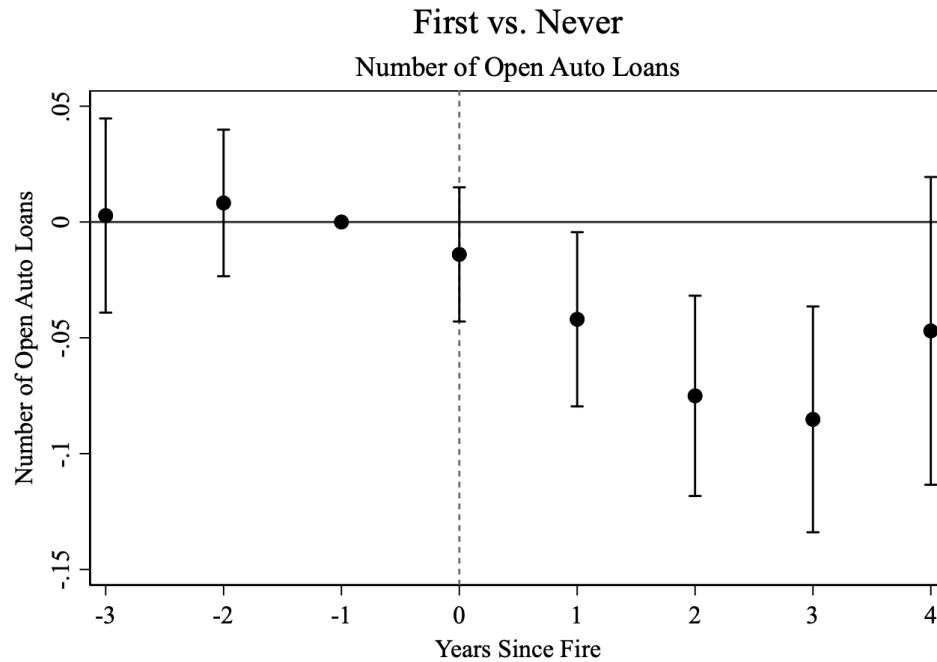
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of open loans that an individual holds. There are no statistically significant differences in the number of open loans before the fire, lending credibility to this empirical strategy. There is a decrease in the number of open loans after the fire, and the effect remains statistically significant four years after the fire.

Figure 4.29: Number of Open Auto Loans after Megafire



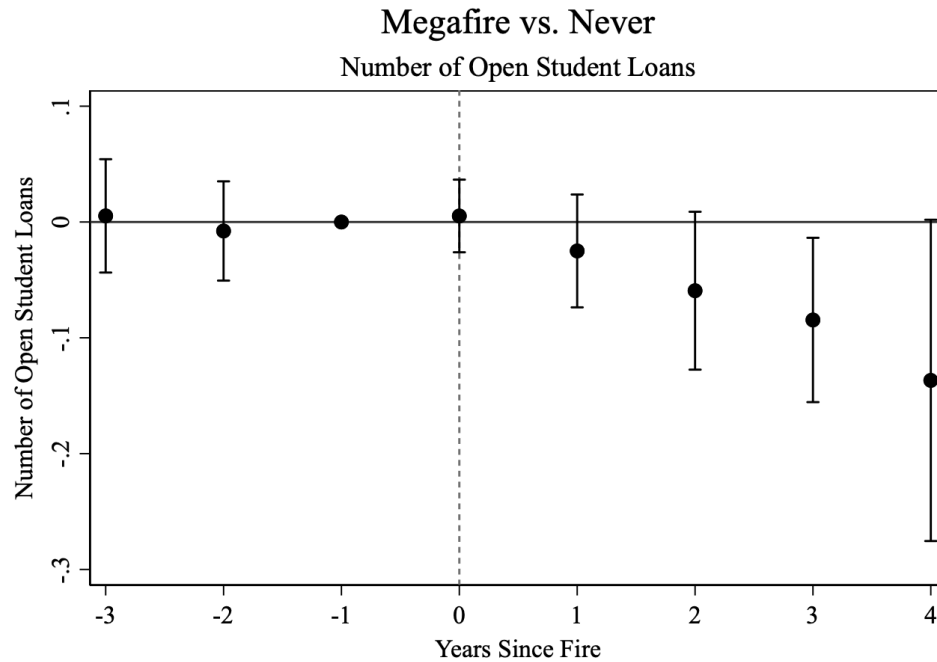
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of open auto loans that an individual holds. There are no statistically significant differences in the number of open auto loans before the fire, lending credibility to this empirical strategy. There is a decrease in the number of open auto loans after the fire, though the effect dissipates by the fourth year after the fire.

Figure 4.30: Number of Open Auto Loans after First Fire



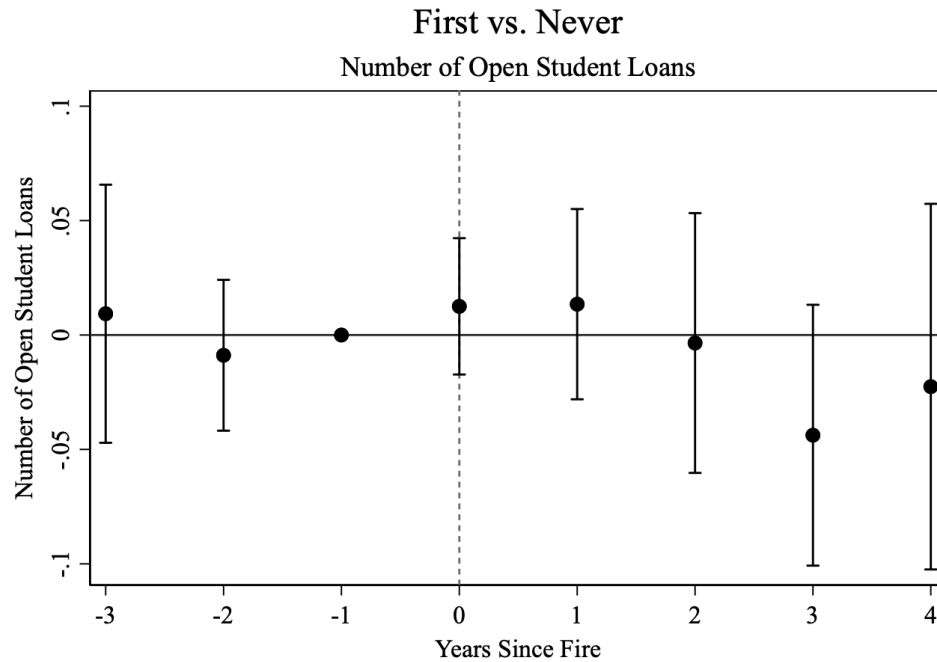
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of open auto loans that an individual holds. There are no statistically significant differences in the number of open auto loans before the fire, lending credibility to this empirical strategy. There is a decrease in the number of open auto loans after the fire, though the effect dissipates by the fourth year after the fire.

Figure 4.31: Number of Open Student Loans after Megafire



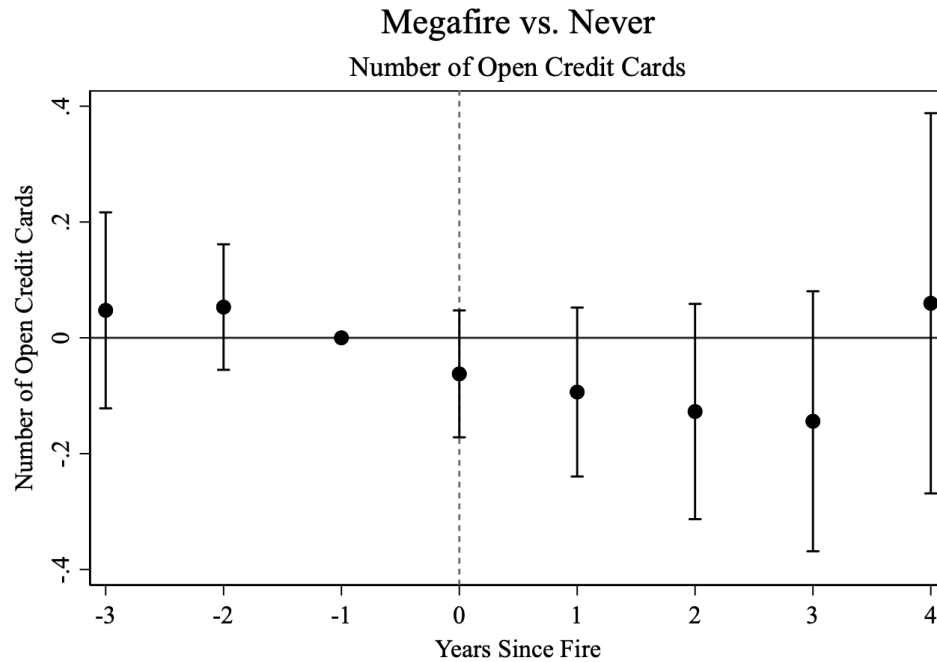
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of open student loans that an individual holds. There are no statistically significant differences in the number of open student loans before the fire, lending credibility to this empirical strategy. There is a steady, though not always statistically significant, decrease in the number of open student loans after the fire.

Figure 4.32: Number of Open Student Loans after First Fire



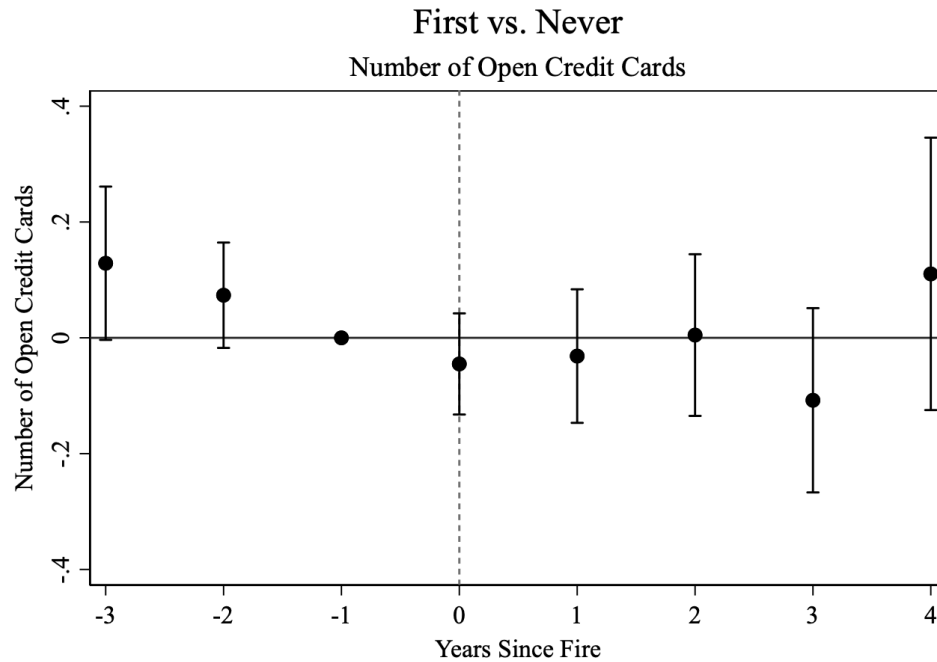
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of open student loans that an individual holds. There are no statistically significant differences in the number of open student loans before the fire, lending credibility to this empirical strategy. There is not a statistically significant change in the number of open student loans after the fire.

Figure 4.33: Number of Open Credit Cards after Megafire



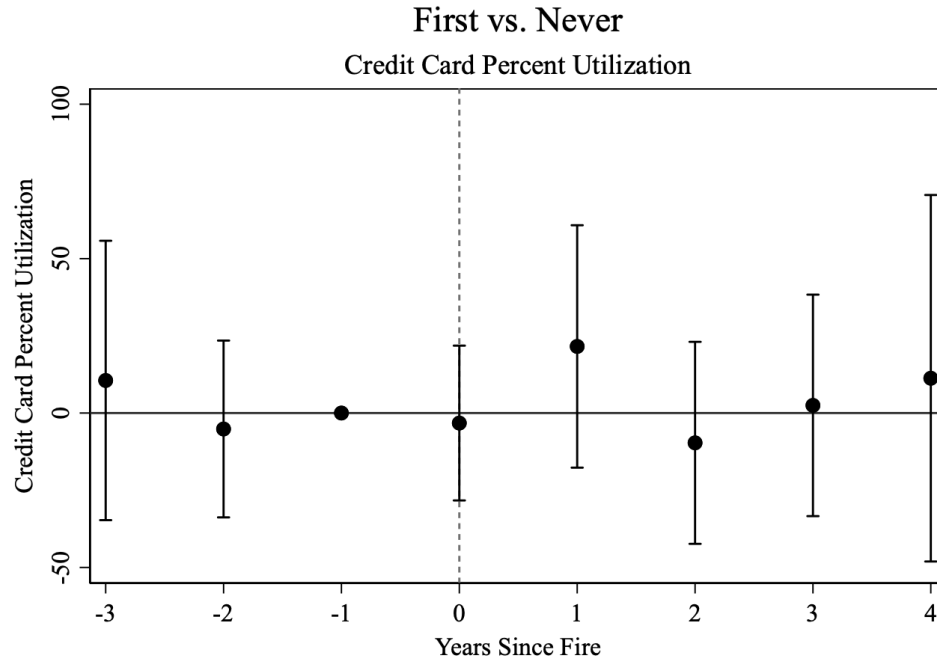
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of open credit cards that an individual holds. There are no statistically significant differences in the number of open credit cards before the fire, lending credibility to this empirical strategy. There is not a statistically significant change in the number of open credit cards after the fire.

Figure 4.34: Number of Open Credit Cards after First Fire



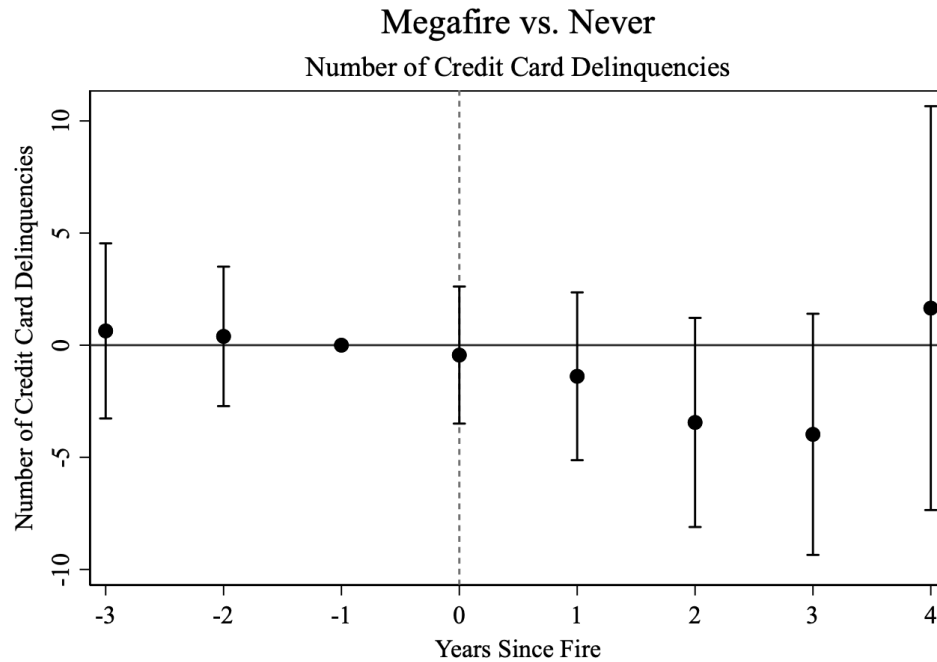
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of open credit cards that an individual holds. There are minimal differences in the number of open credit cards before the fire, lending credibility to this empirical strategy. There is not a statistically significant change in the number of open credit cards after the fire.

Figure 4.35: Percent Utilization of Credit Cards after First Fire



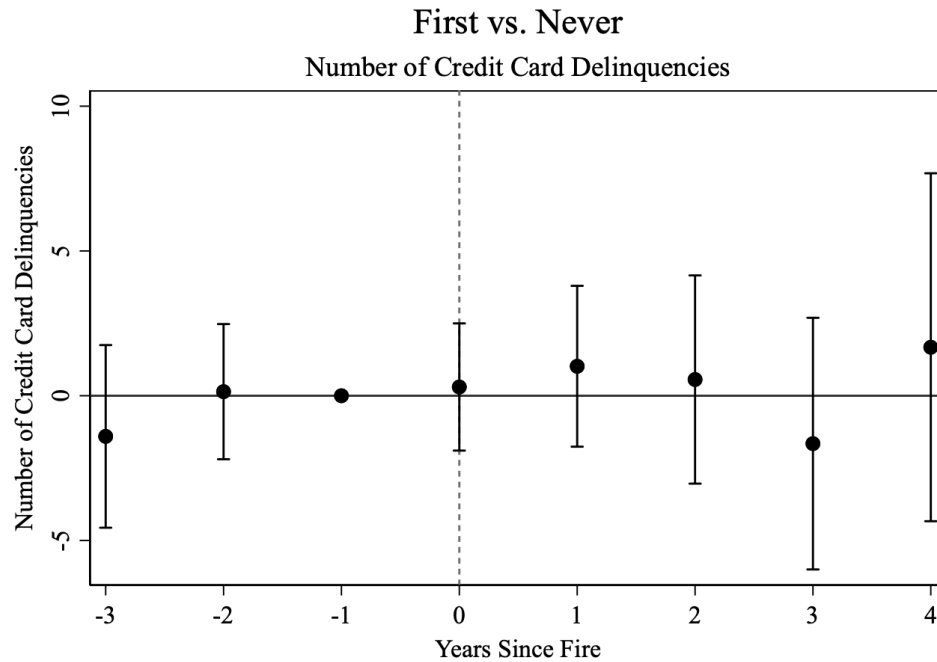
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the percent utilization of credit cards that an individual holds. There are minimal differences in the percent utilization of credit cards before the fire, lending credibility to this empirical strategy. There is not a statistically significant change in the percent utilization of credit cards after the wildfire.

Figure 4.36: Number of Credit Card Delinquencies after Megafire



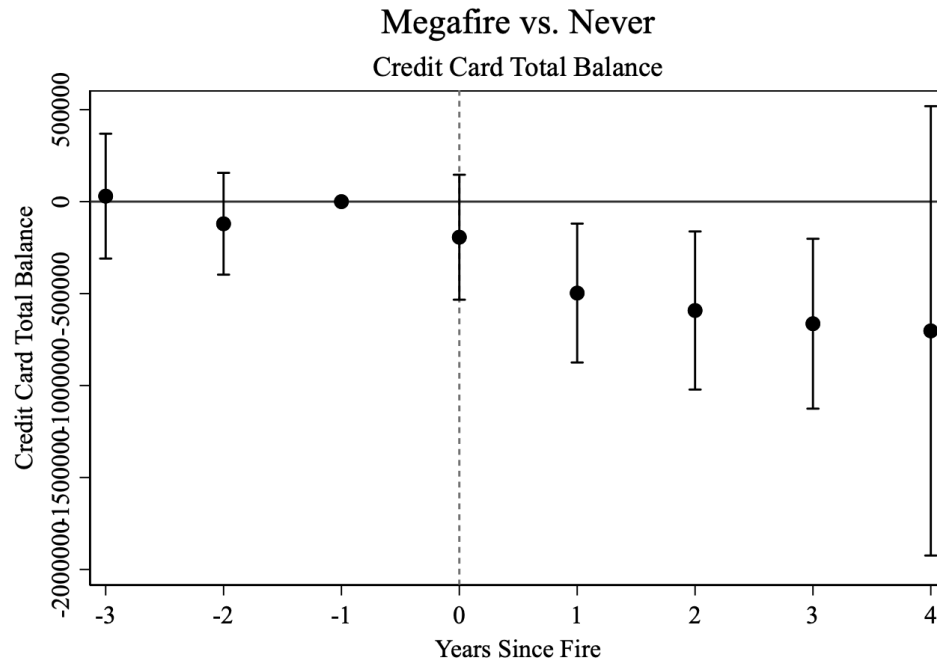
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of credit card delinquencies that an individual experiences. There are minimal differences in the number of credit card delinquencies before the fire, lending credibility to this empirical strategy. There is not a statistically significant change in the number of credit card delinquencies after the wildfire.

Figure 4.37: Number of Credit Card Delinquencies after First Fire



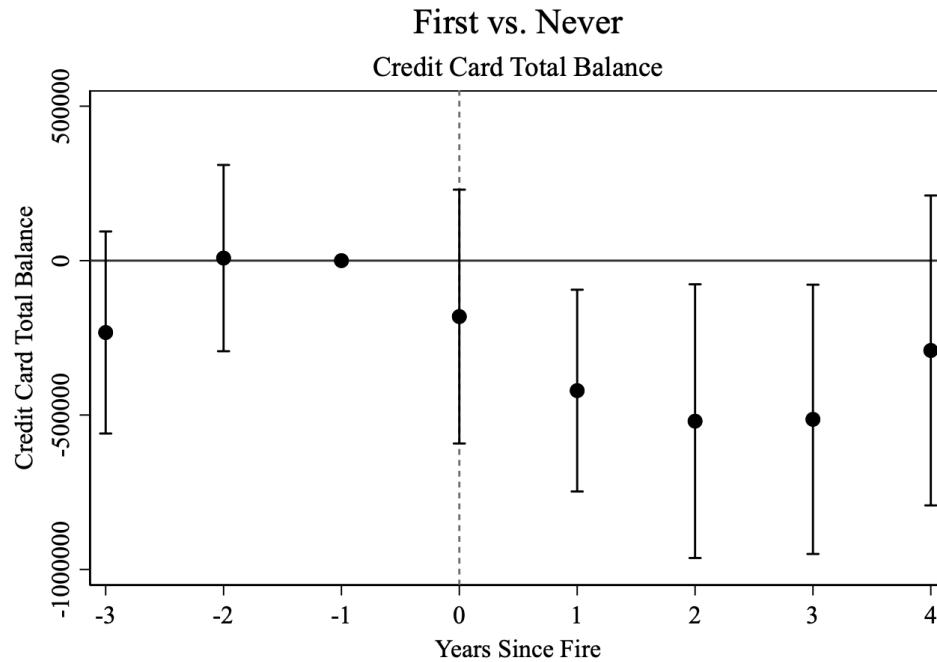
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of credit card delinquencies that an individual experiences. There are minimal differences in the number of credit card delinquencies before the fire, lending credibility to this empirical strategy. There is not a statistically significant change in the number of credit card delinquencies after the wildfire.

Figure 4.38: Total Credit Card Balance after Megafire



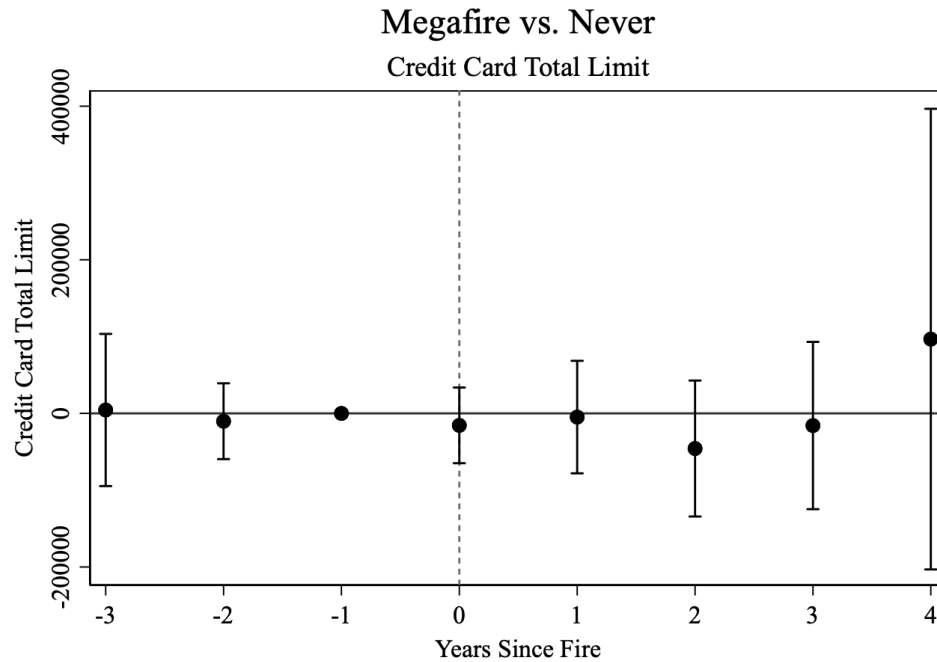
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the total balance of credit cards that an individual holds. There are minimal differences in the total balance of credit cards before the fire, lending credibility to this empirical strategy. There is a decrease in the total balance of credit cards after the fire, though this is no longer statistically significant four years after the fire.

Figure 4.39: Total Credit Card Balance after First Fire



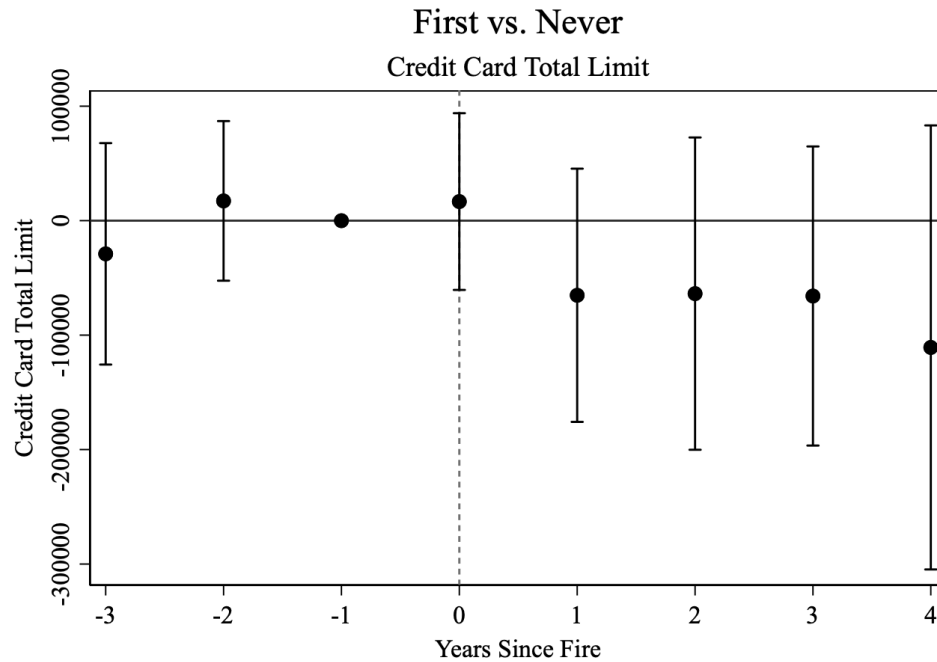
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the total credit card balance that an individual holds. There are minimal differences in the total credit card balance before the fire, lending credibility to this empirical strategy. There is a decrease in the total credit card balance after the fire, though the effect dissipates by the fourth year after the fire.

Figure 4.40: Total Credit Card Limit after Megafire



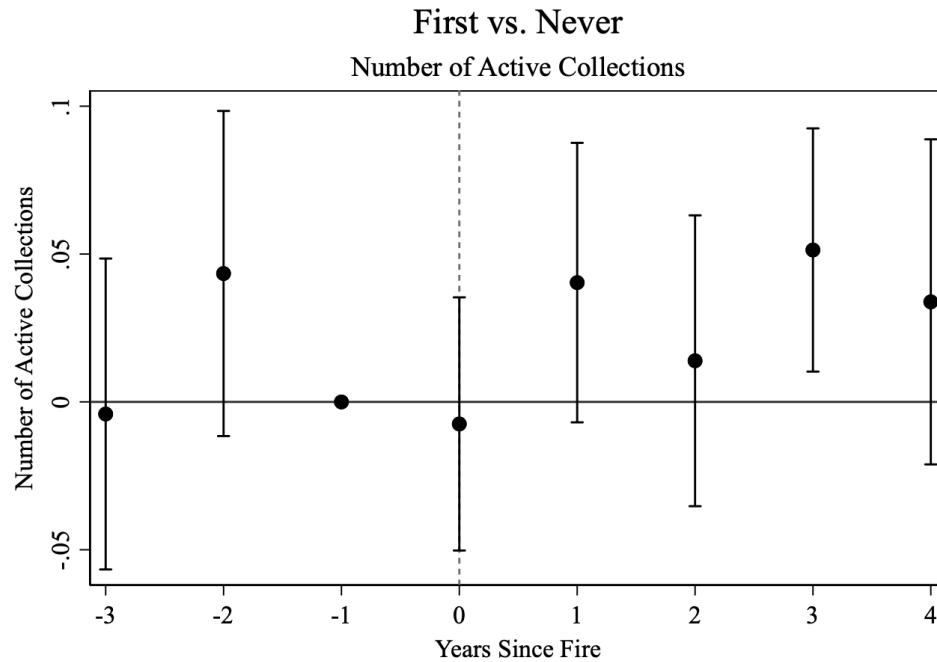
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the total credit card limit that an individual holds. There are minimal differences in the total credit card limit before the fire, lending credibility to this empirical strategy. There is not a statistically significant change in the total credit card limit after the wildfire.

Figure 4.41: Total Credit Card Limit after First Fire



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the total credit card limit that an individual holds. There are minimal differences in the total credit card limit before the fire, lending credibility to this empirical strategy. There is not a statistically significant change in the total credit card limit after the wildfire.

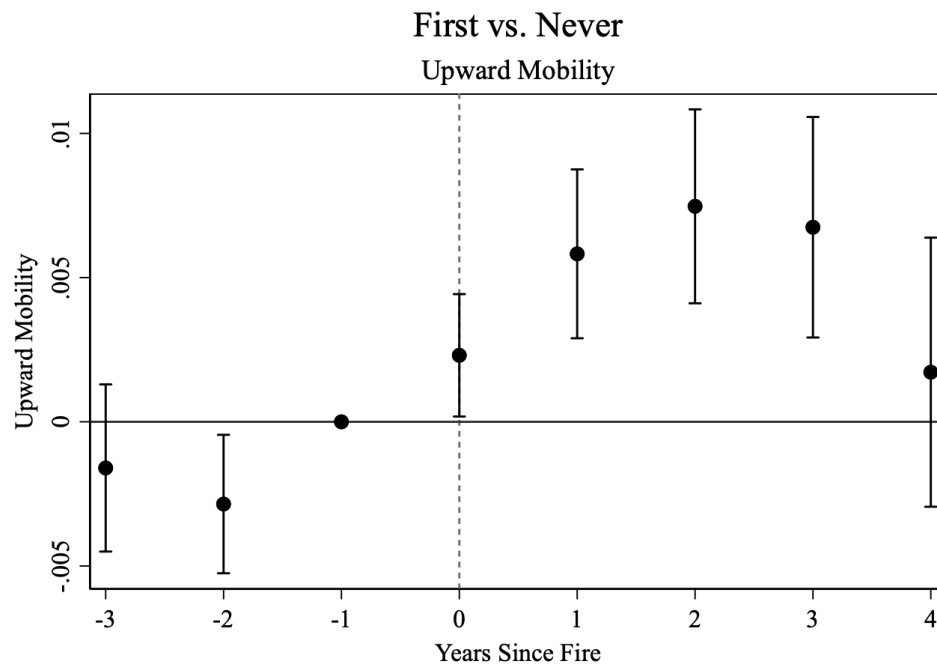
Figure 4.42: Number of Active Collections after First Fire



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the number of active collections that an individual experiences. There are minimal differences in the number of active collections before the fire, lending credibility to this empirical strategy. There is not a consistently statistically significant change in the number of active collections after the wildfire.

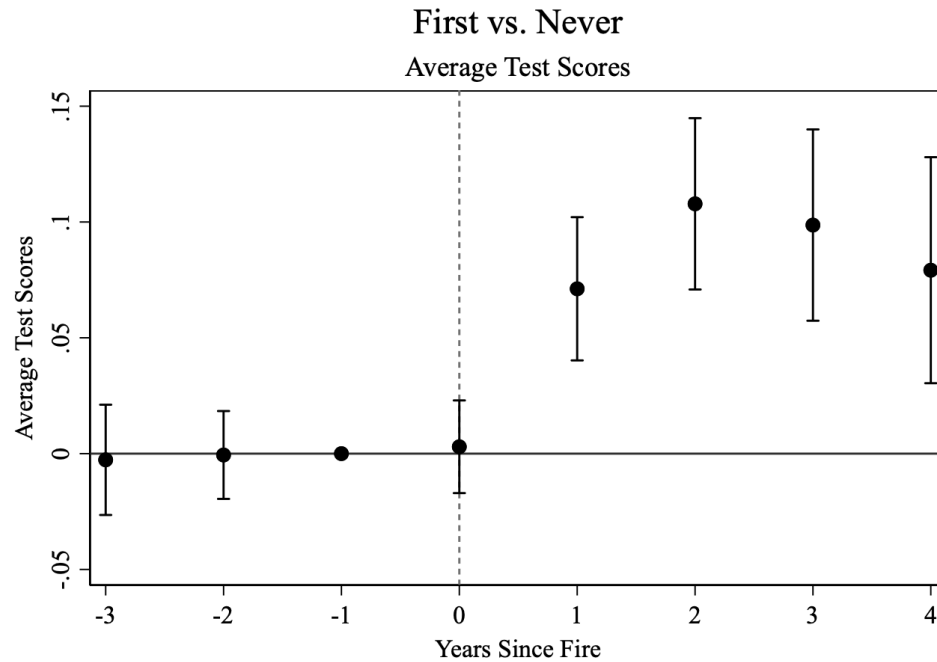
2.2 Amenities for Children

Figure 4.43: Upward Mobility after First Fire



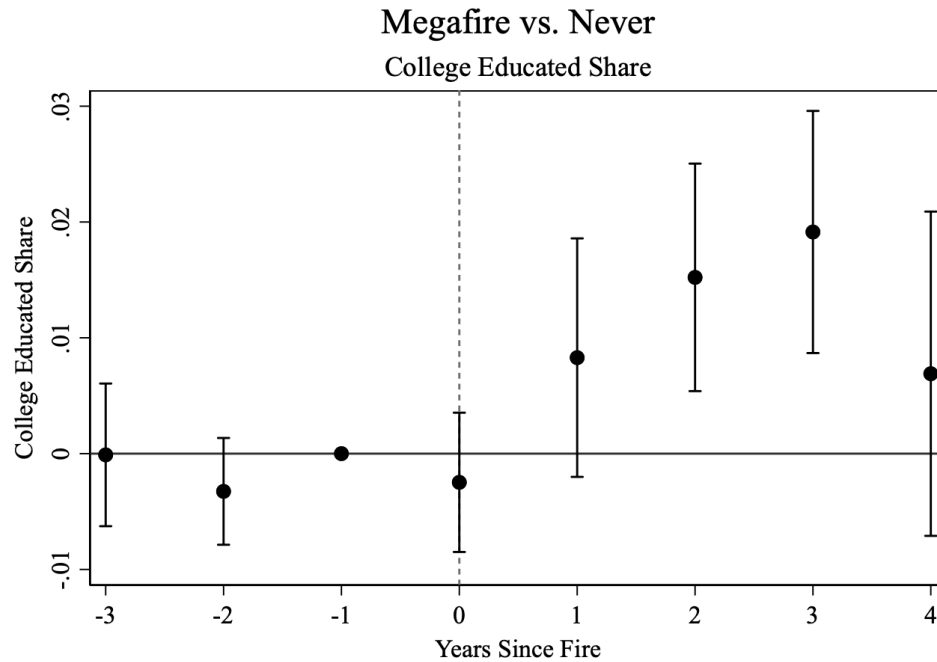
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is upward mobility and comes from the Opportunity Atlas, measured as the probability that a child in that census tract will reach the top 20% of the national income distribution in 2014-2015. There are minimal differences in upward mobility before the fire, lending credibility to this empirical strategy. There is an increase in upward mobility after the fire, though it is no longer statistically significant four years after.

Figure 4.44: Average Test Scores after First Fire



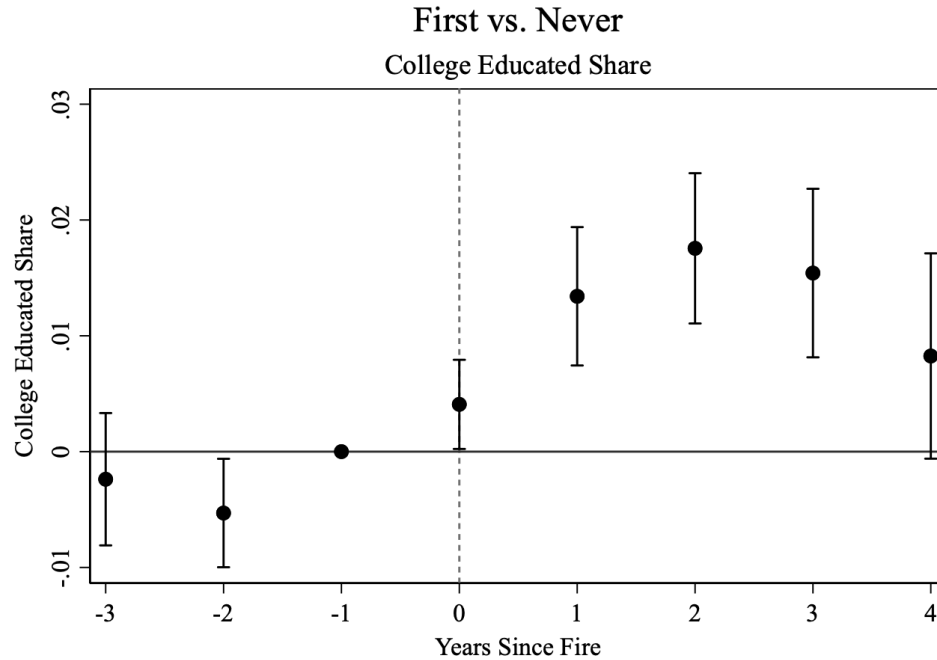
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is average test scores and comes from the Opportunity Atlas, measured as the average school district level standardized test scores in the third grade in 2013. There are no statistically significant differences in average test scores before the fire, lending credibility to this empirical strategy. There is an increase in average test scores after the fire, and the effect persists and is statistically significant even after four years.

Figure 4.45: College Educated Share after Megafire



Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the college educated share and comes from the Opportunity Atlas, measured as the fraction of residents with a college degree or more in 2010. There are minimal differences in the college educated share before the fire, lending credibility to this empirical strategy. There is an increase in the college educated share after the fire, though the effect dissipates by the fourth year after the fire.

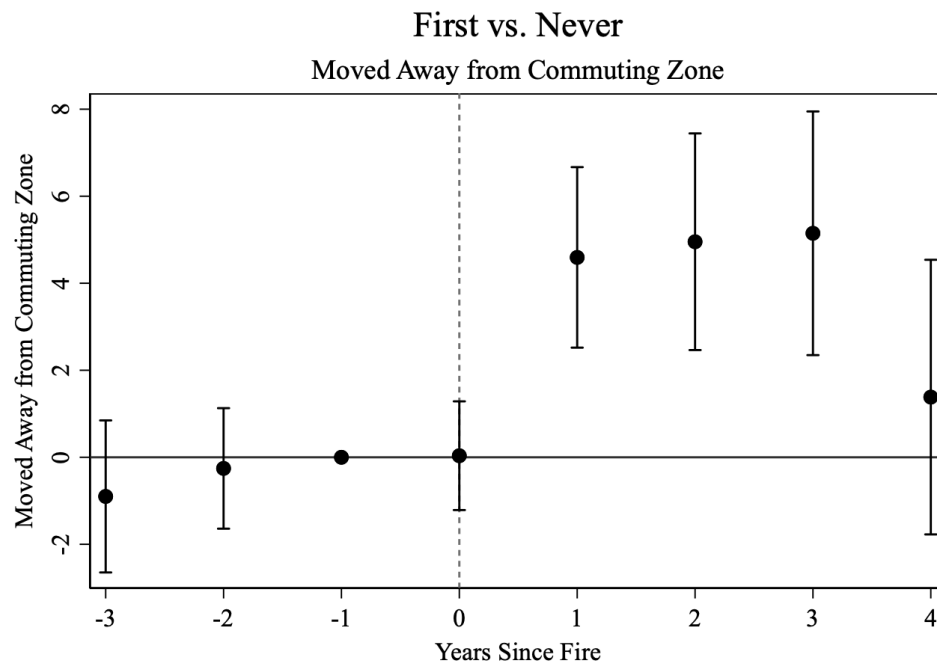
Figure 4.46: College Educated Share after First Fire



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is the college educated share and comes from the Opportunity Atlas, measured as the fraction of residents with a college degree or more in 2010. There are minimal differences in the college educated share before the fire, lending credibility to this empirical strategy. There is an increase in the college educated share after the fire, though the effect dissipates by the fourth year after the fire.

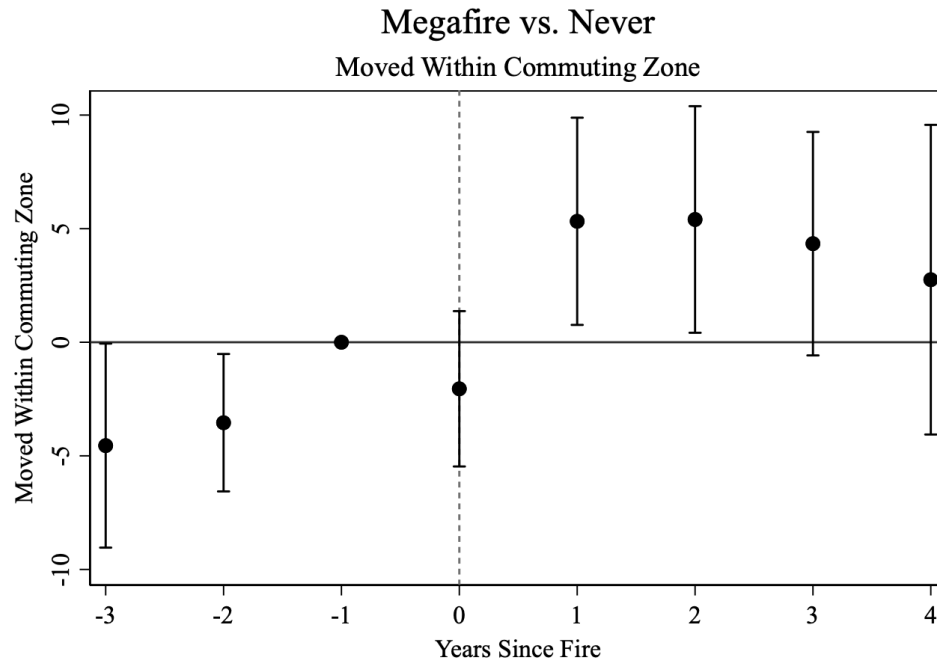
2.3 Labor Market

Figure 4.47: Moved Away from Commuting Zone after First Fire



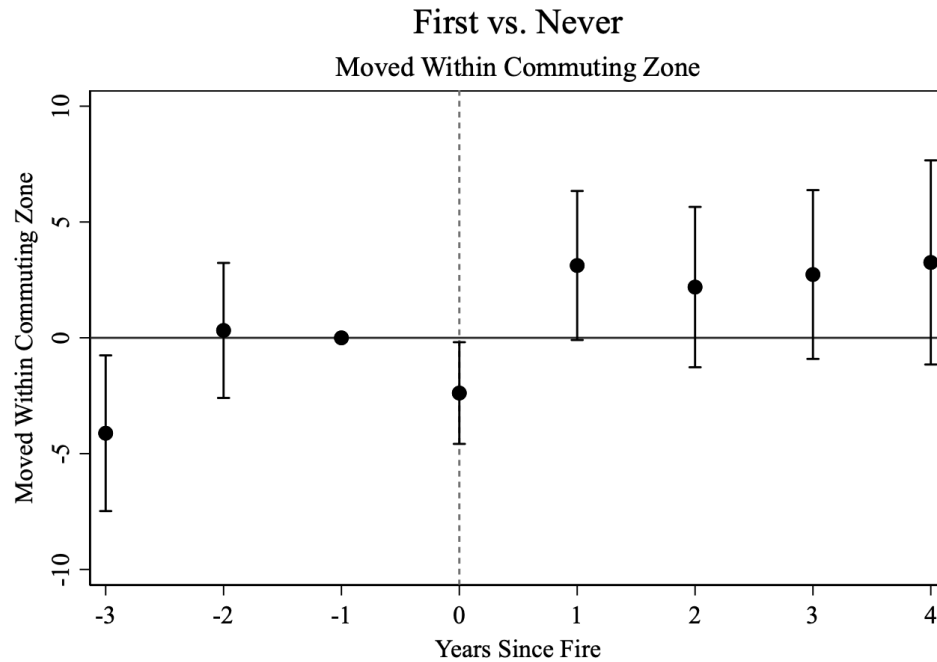
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different commuting zone from where she was at the year before the first fire (“Moved Away from Commuting Zone”). There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy. There is a sharp increase in the likelihood of moving away starting the year after the fire, though this dissipates by the fourth year after the fire.

Figure 4.48: Moved Within Commuting Zone after Megafire



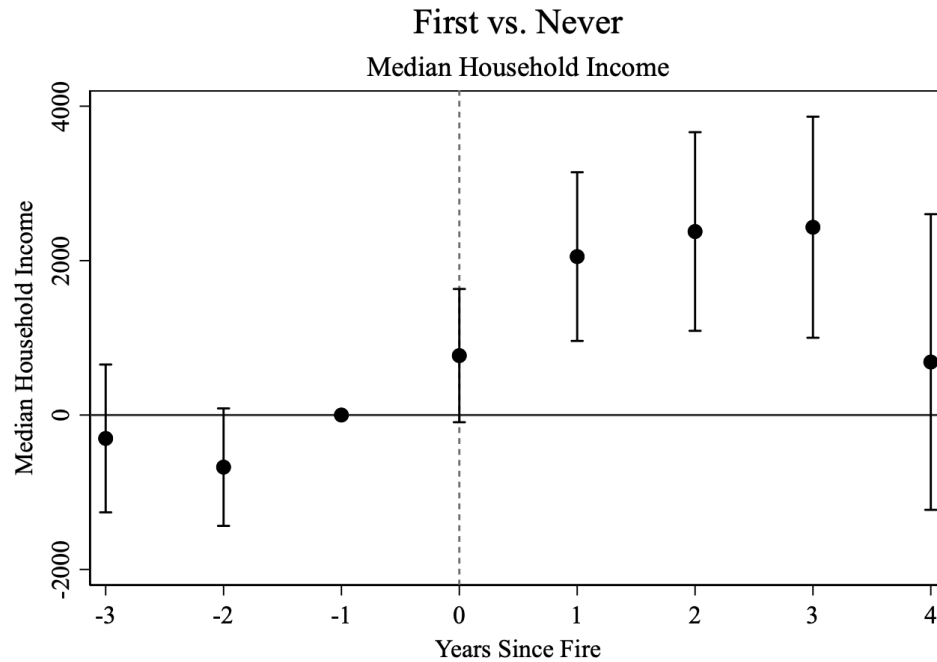
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different location but within the same commuting zone where she was at the year before the first fire (“Moved Within Commuting Zone”). There are minimal differences in migration rates before the fire, lending credibility to this empirical strategy. There is initially a statistically significant increase in the likelihood of moving within the commuting zone, though this effect dissipates by the third and fourth years after the wildfire.

Figure 4.49: Moved Within Commuting Zone after First Fire



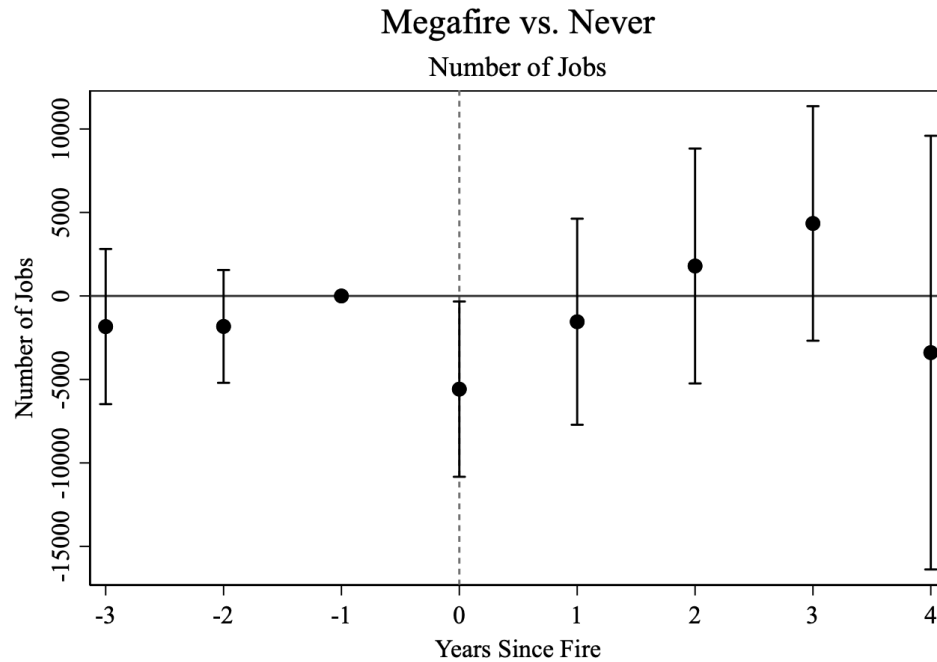
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different location but within the same commuting zone where she was at the year before the first fire (“Moved Within Commuting Zone”). There are minimal differences in migration rates before the fire, lending credibility to this empirical strategy. There is not a consistently statistically significant change in the likelihood of moving within the commuting zone after the wildfire.

Figure 4.50: Median Household Income after First Fire



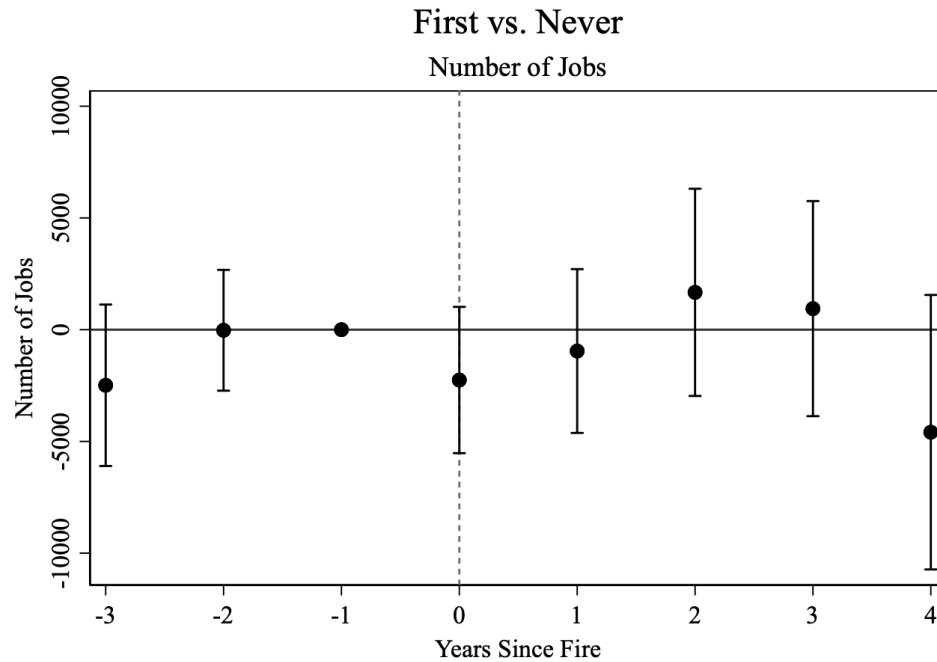
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the median household income and comes from the Opportunity Atlas, measured as the median household income in 2016. There are no statistically significant differences in the median household income before the fire, lending credibility to this empirical strategy. There is an increase in the median household income, though the effect dissipates by the fourth year after the fire.

Figure 4.51: Number of Jobs after Megafire



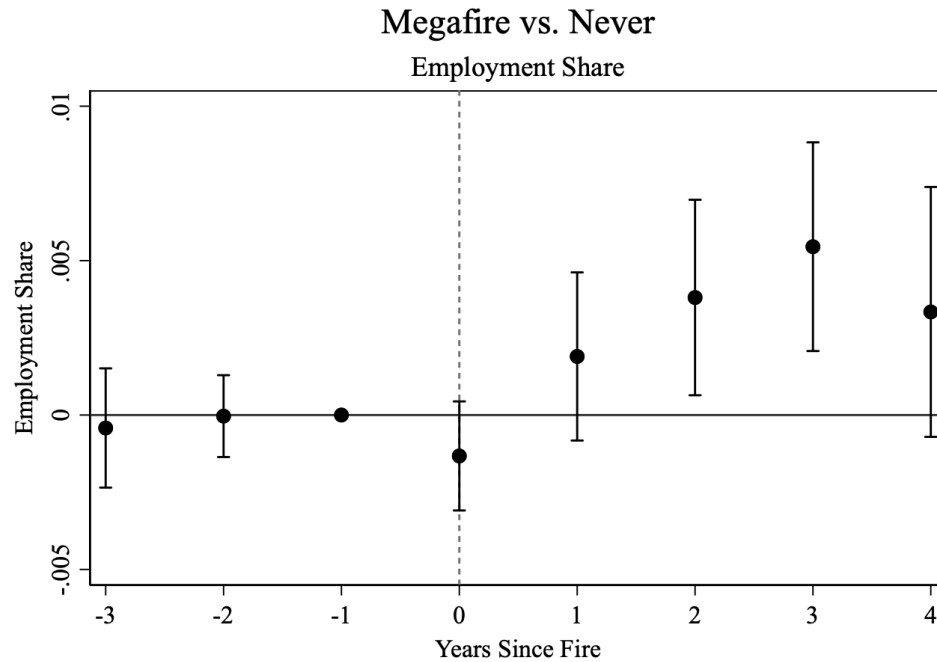
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the number of jobs and comes from the Opportunity Atlas, measured as the total number of jobs within five miles of the census tract in 2015. There are no statistically significant differences in the number of jobs before the fire, lending credibility to this empirical strategy. There are no statistically significant changes in the number of jobs after the fire.

Figure 4.52: Number of Jobs after First Fire



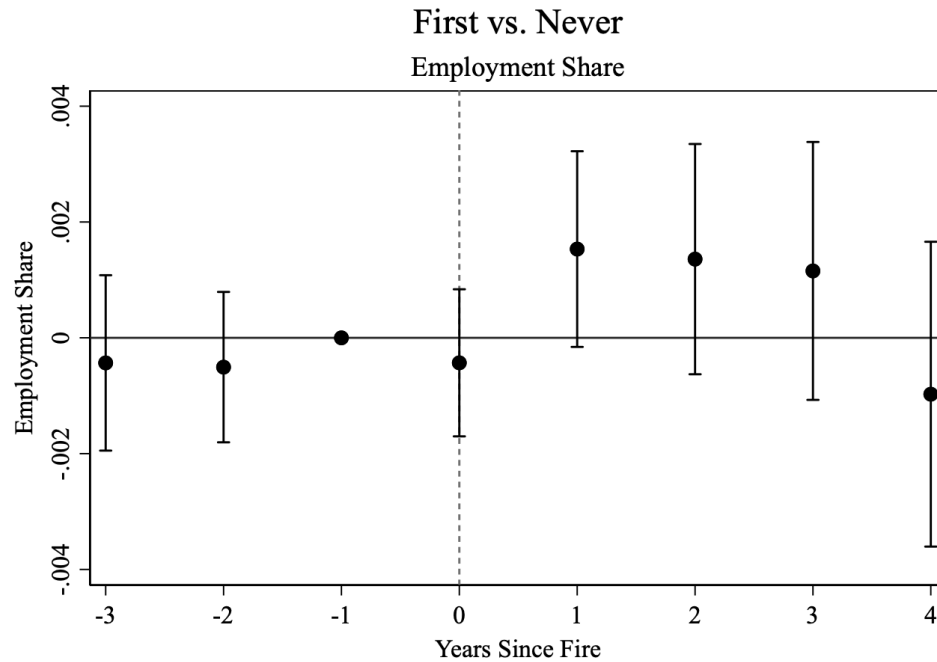
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the number of jobs and comes from the Opportunity Atlas, measured as the total number of jobs within five miles of the census tract in 2015. There are no statistically significant differences in the number of jobs before the fire, lending credibility to this empirical strategy. There are no statistically significant changes in the number of jobs after the fire.

Figure 4.53: Employment Share after Megafire



Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the employment share and comes from the Opportunity Atlas, measured as the fraction of people who have positive W-2 earnings in 2015. There are no statistically significant differences in the employment share before the fire, lending credibility to this empirical strategy. There is an increase in the employment share, though the effect dissipates by the fourth year after the fire.

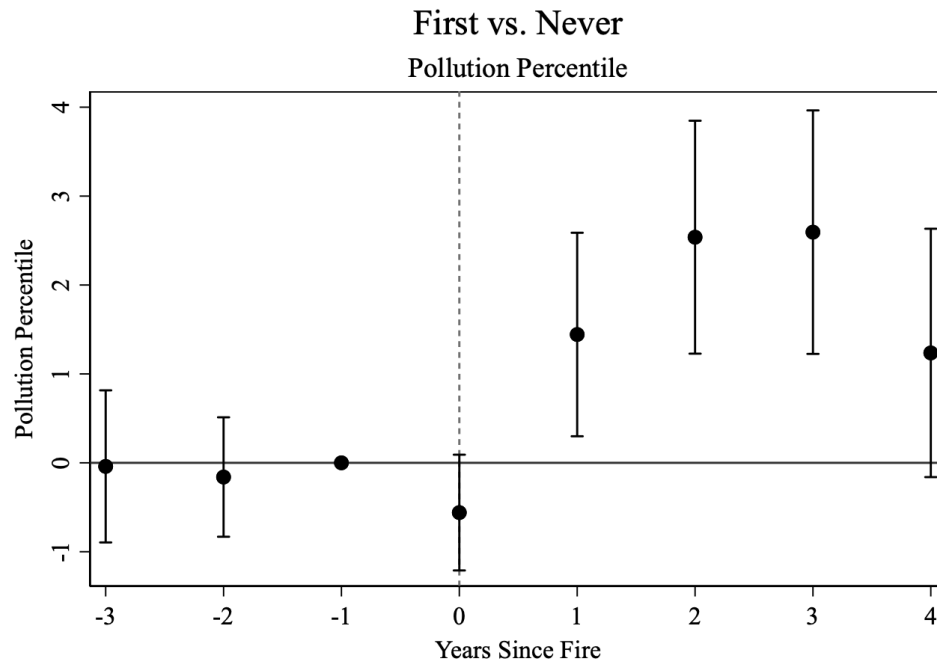
Figure 4.54: Employment Share after First Fire



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the employment share and comes from the Opportunity Atlas, measured as the fraction of people who have positive W-2 earnings in 2015. There are no statistically significant differences in the employment share before the fire, lending credibility to this empirical strategy. There are no statistically significant changes in the employment share after the fire.

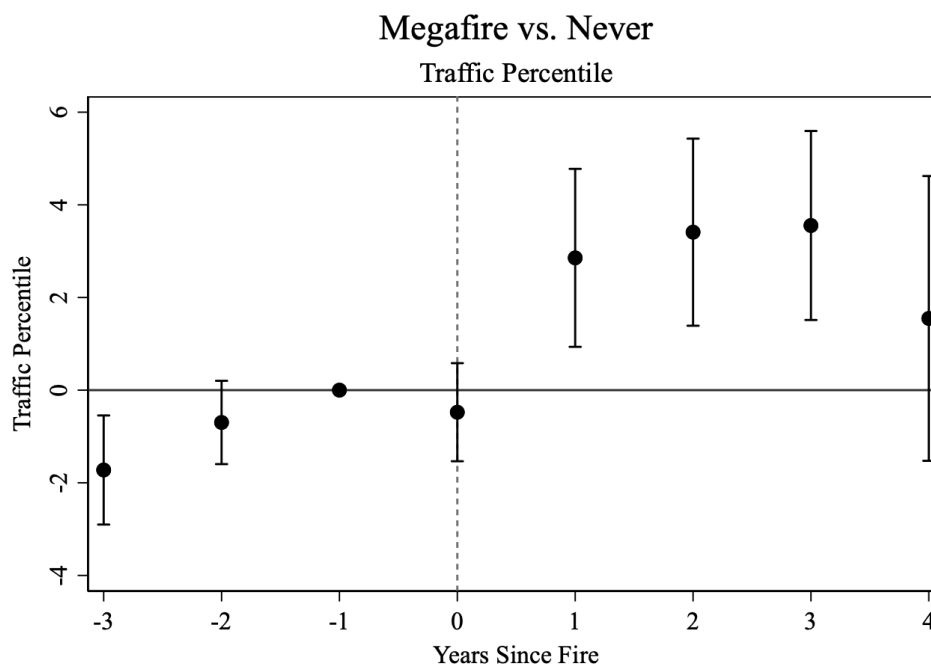
2.4 Pollution

Figure 4.55: Pollution Burden after First Fire



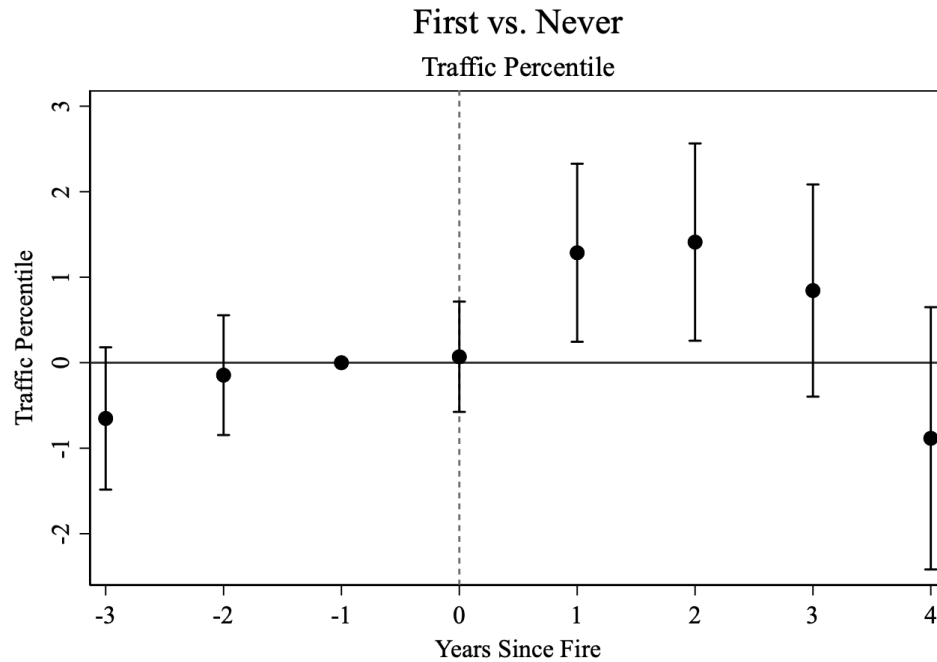
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the pollution burden and comes from The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviron), measured as the percentile of pollution burden (which incorporates ozone, PM2.5, diesel, drinking water, lead, pesticides, toxic releases, traffic, cleanup sites, groundwater threats, hazardous waste, impaired water bodies, and solid waste) for census tracts in California and released in 2021. There are no statistically significant differences in the pollution burden before the fire, lending credibility to this empirical strategy. There is an increase in the pollution burden, though the effect dissipates by the fourth year after the fire.

Figure 4.56: Traffic after Megafire



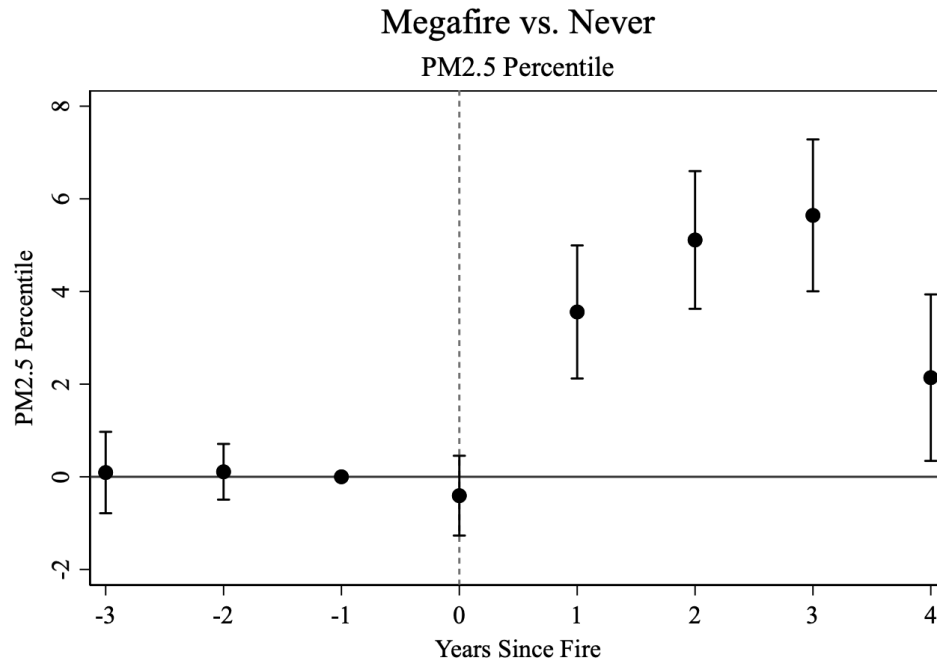
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is traffic and comes from The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviroScreen), measured as the percentile of traffic density in vehicle-kilometers per hour per road length, within 150 meters of the census tract boundary for census tracts in California and released in 2021. There are minimal statistically significant differences in traffic before the fire, lending credibility to this empirical strategy. There is an increase in traffic, though the effect dissipates by the fourth year after the fire.

Figure 4.57: Traffic after First Fire



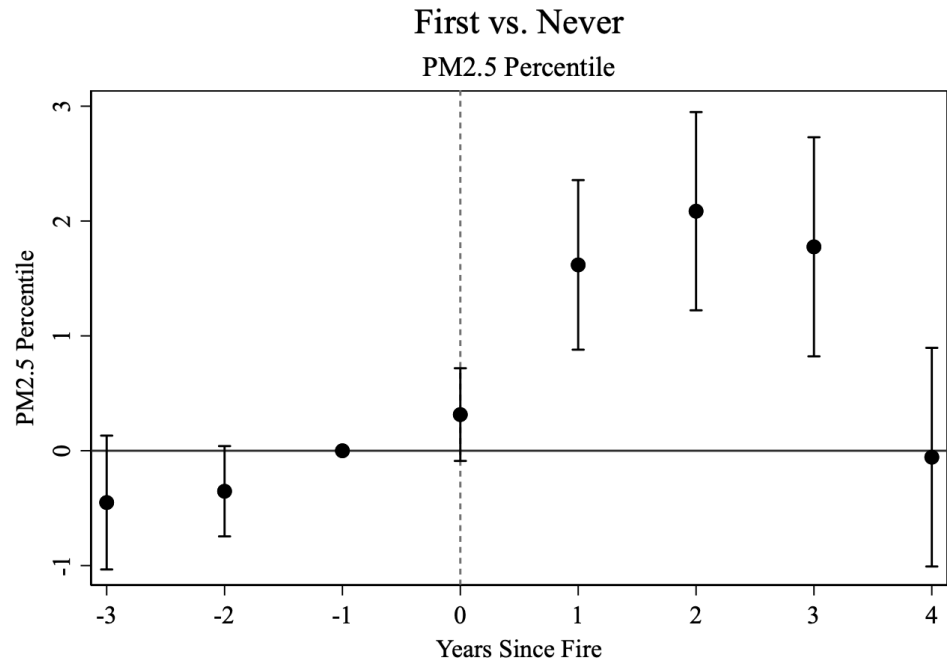
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is traffic and comes from The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviron), measured as the percentile of traffic density in vehicle-kilometers per hour per road length, within 150 meters of the census tract boundary for census tracts in California and released in 2021. There are no statistically significant differences in traffic before the fire, lending credibility to this empirical strategy. There is an increase in traffic, though the effect dissipates by the fourth year after the fire.

Figure 4.58: PM2.5 after Megafire



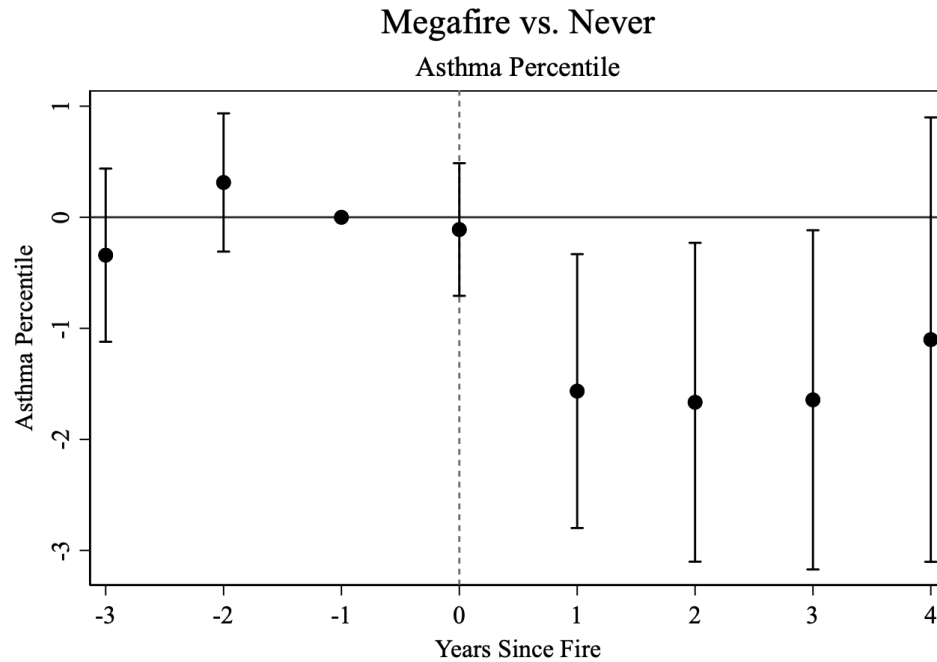
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is PM2.5 and comes from The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviron), measured as the percentile of the annual mean PM2.5 concentrations for census tracts in California and released in 2021. There are no statistically significant differences in PM2.5 before the fire, lending credibility to this empirical strategy. There is an increase in PM2.5, and this effect persists and is statistically significant four years after the fire.

Figure 4.59: PM2.5 after First Fire



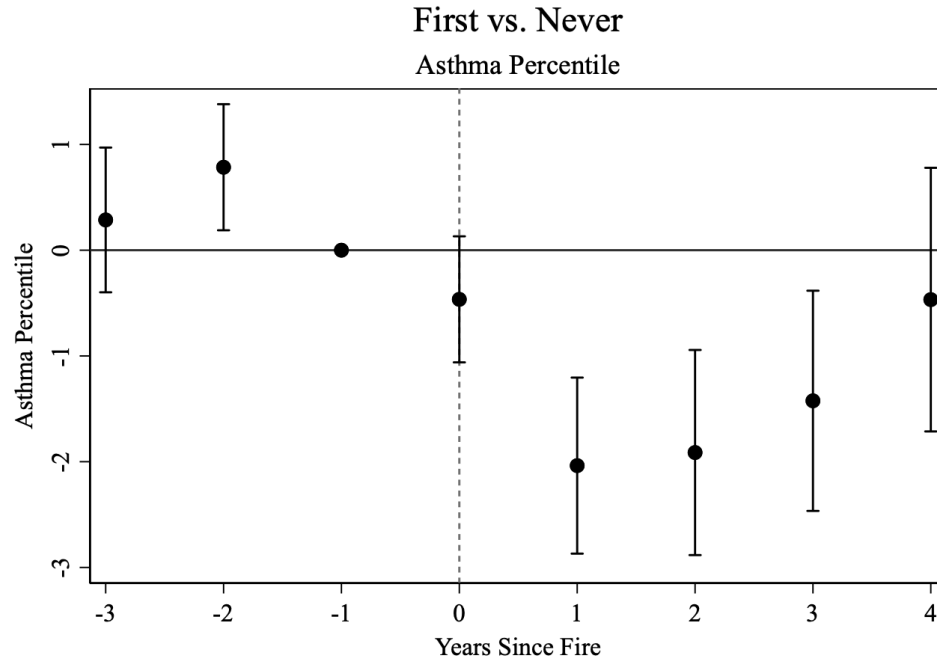
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is PM2.5 and comes from The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviron), measured as the percentile of the annual mean PM2.5 concentrations for census tracts in California and released in 2021. There are no statistically significant differences in PM2.5 before the fire, lending credibility to this empirical strategy. There is an increase in PM2.5, though the effect dissipates by the fourth year after the fire.

Figure 4.60: Asthma after Megafire



Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is asthma and comes from The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviroScreen), measured as the age-adjusted rate of emergency department visits for asthma for census tracts in California and released in 2021. There are no statistically significant differences in asthma before the fire, lending credibility to this empirical strategy. There is a decrease in asthma, though the effect dissipates by the fourth year after the fire.

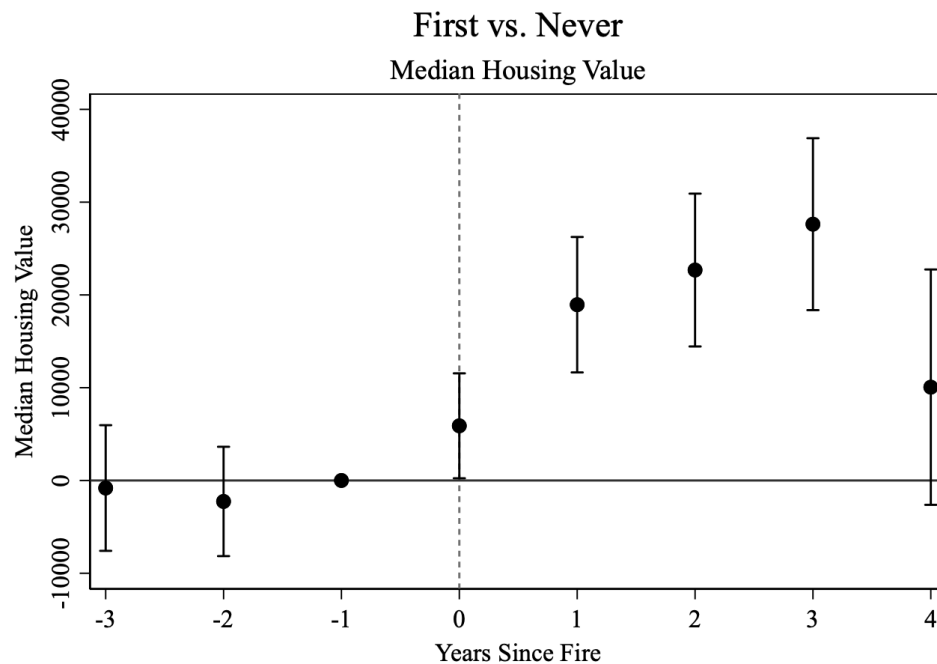
Figure 4.61: Asthma after First Fire



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is asthma and comes from The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviroScreen), measured as the age-adjusted rate of emergency department visits for asthma for census tracts in California and released in 2021. There are minimal statistically significant differences in asthma before the fire, lending credibility to this empirical strategy. There is a decrease in asthma, though the effect dissipates by the fourth year after the fire.

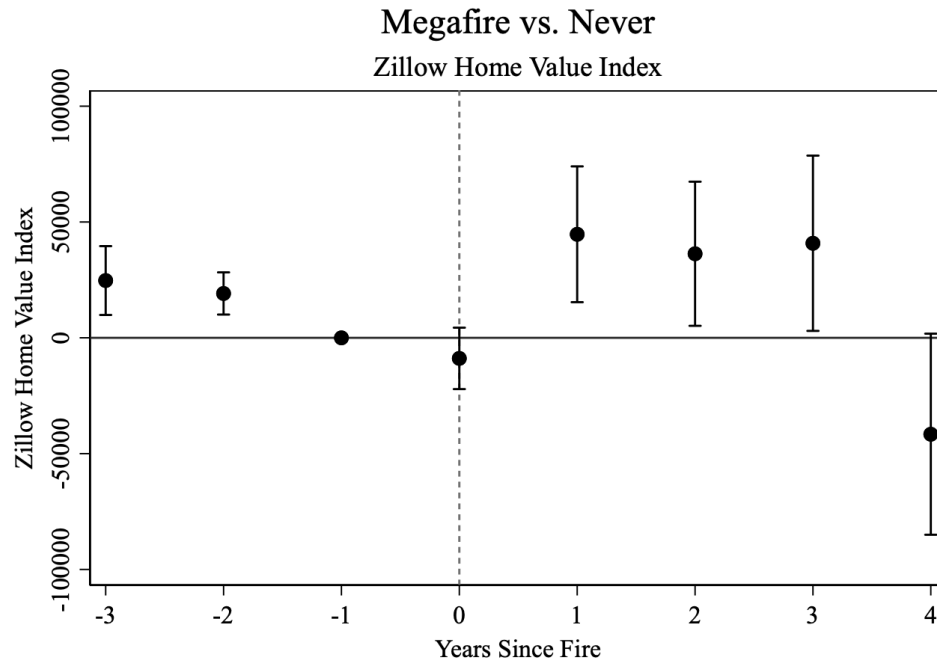
2.5 Housing

Figure 4.62: Median Housing Value after First Fire



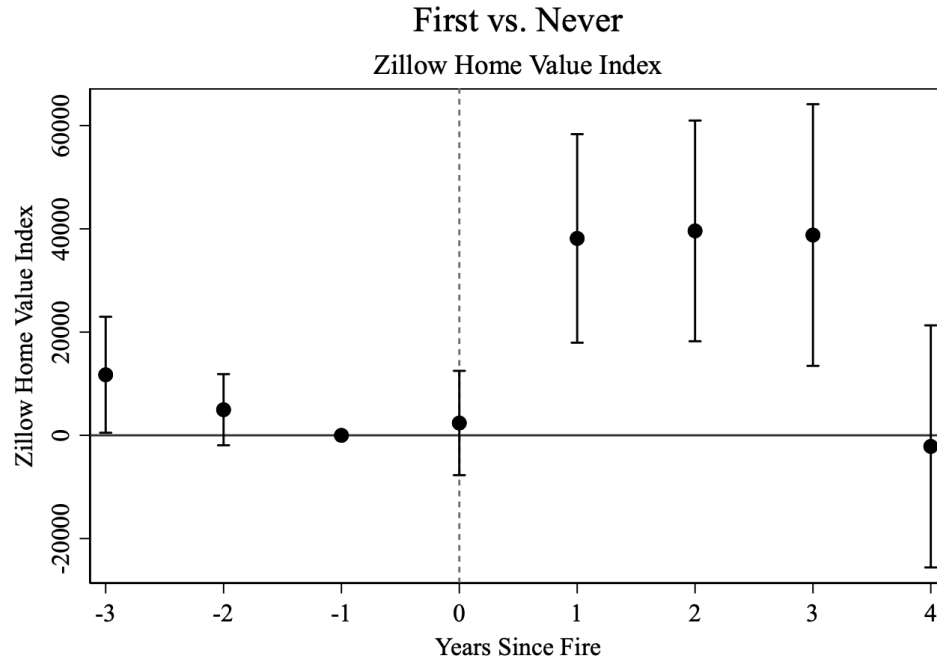
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the median housing value of the census tract and comes from 2010 Census. There are no statistically significant differences in the median housing value before the fire, lending credibility to this empirical strategy. There is an increase in the median housing value, though the effect dissipates by the fourth year after the fire.

Figure 4.63: Zillow Home Value Index after Megafire



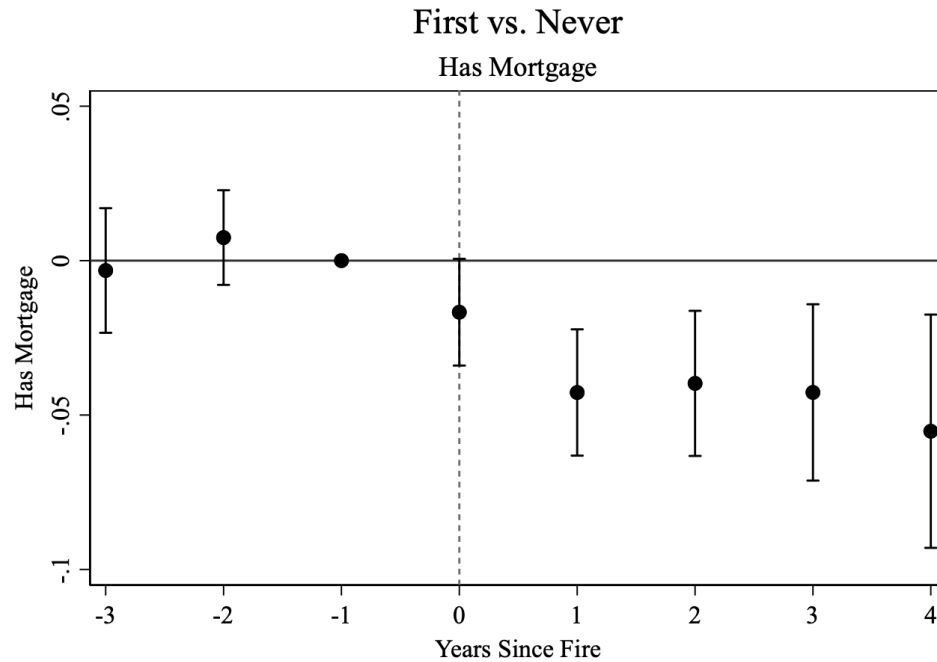
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the Zillow Home Value Index (ZHVI), which is the typical home value for the zipcode measured as a weighted average of the home values of the middle third of homes that is then averaged over a year. There are minimal statistically significant differences in the ZHVI before the fire, lending credibility to this empirical strategy. Adjusting for these may change the statistical significance of the post-fire coefficients. There is an increase in the ZHVI, though the effect drops dramatically and is no longer statistically significant by the fourth year after the fire.

Figure 4.64: Zillow Home Value Index after First Fire



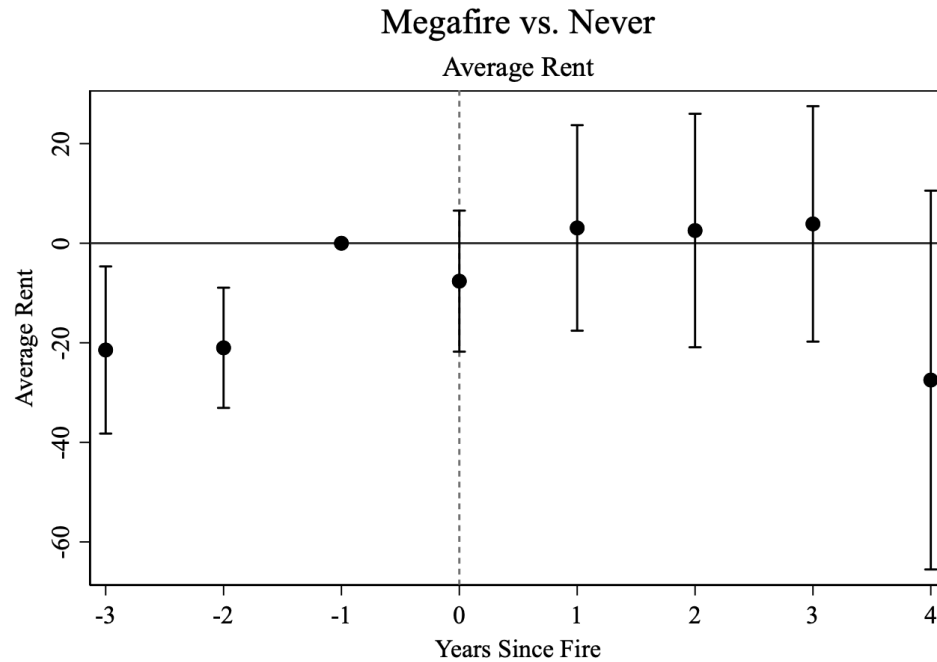
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the Zillow Home Value Index (ZHVI), which is the typical home value for the zipcode measured as a weighted average of the home values of the middle third of homes that is then averaged over a year. There are minimal statistically significant differences in the ZHVI before the fire, lending credibility to this empirical strategy. There is an increase in the ZHVI, though the effect drops dramatically and is no longer statistically significant by the fourth year after the fire

Figure 4.65: Mortgage after First Fire



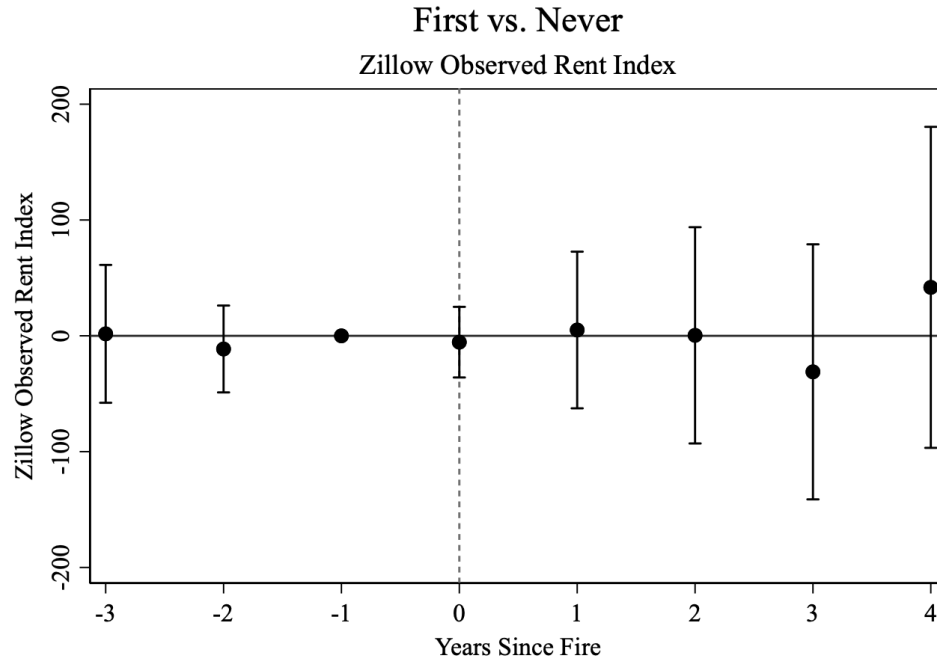
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in the UCCCP data from 2014 to 2021. The outcome variable is whether the individual has a mortgage. There are no statistically significant differences in having a mortgage before the fire, lending credibility to this empirical strategy. There is a decrease in the likelihood of having a mortgage after the fire, and this effect persists and is statistically significant even after four years.

Figure 4.66: Rent after Megafire



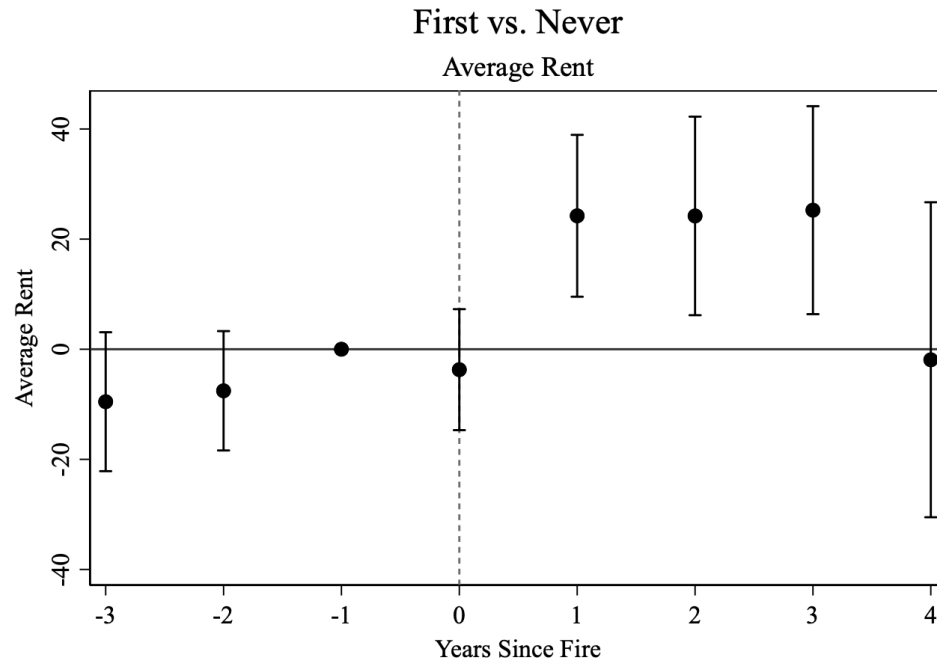
Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the average rent and comes from the Opportunity Atlas, measured as the average rent of a two bedroom home in 2015. There are minimal statistically significant differences in the average rent before the fire, lending credibility to this empirical strategy. Adjusting for these are unlikely to change the statistical significance of the post-fire coefficients. There is no meaningful change in the average rent after the fire.

Figure 4.67: Zillow Observed Rent Index after First Fire



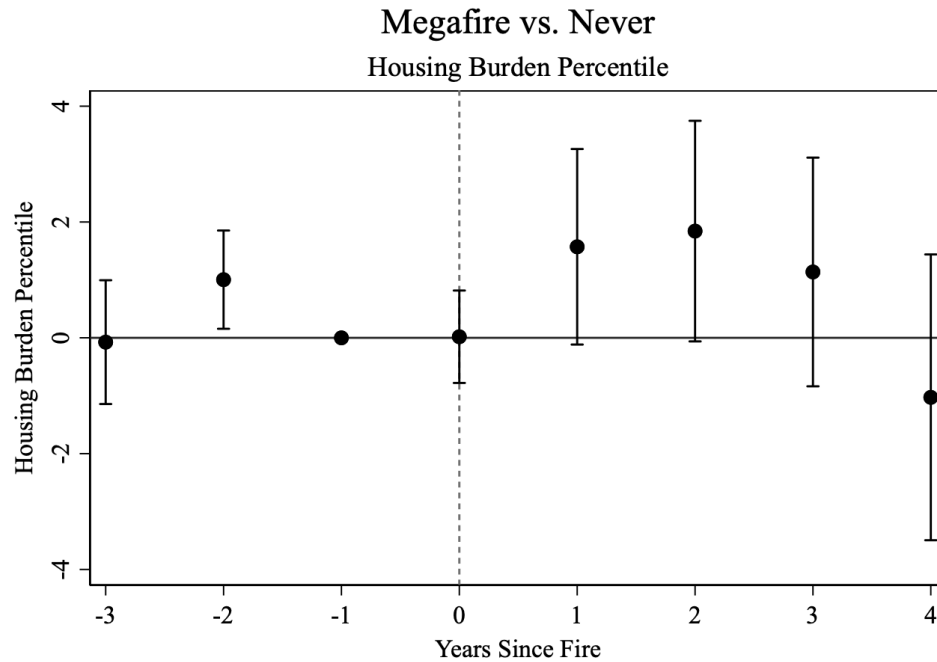
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the Zillow Observed Rent Index (ZORI), which is the typical observed market rate rent for the zipcode measured as a weighted average of the rental values of the middle third of apartments and homes that is then averaged over a year. There are no statistically significant differences in the ZORI before the fire, lending credibility to this empirical strategy. There is no meaningful change in the ZORI that people living in blocks that burned by a first fire face.

Figure 4.68: Rent after First Fire



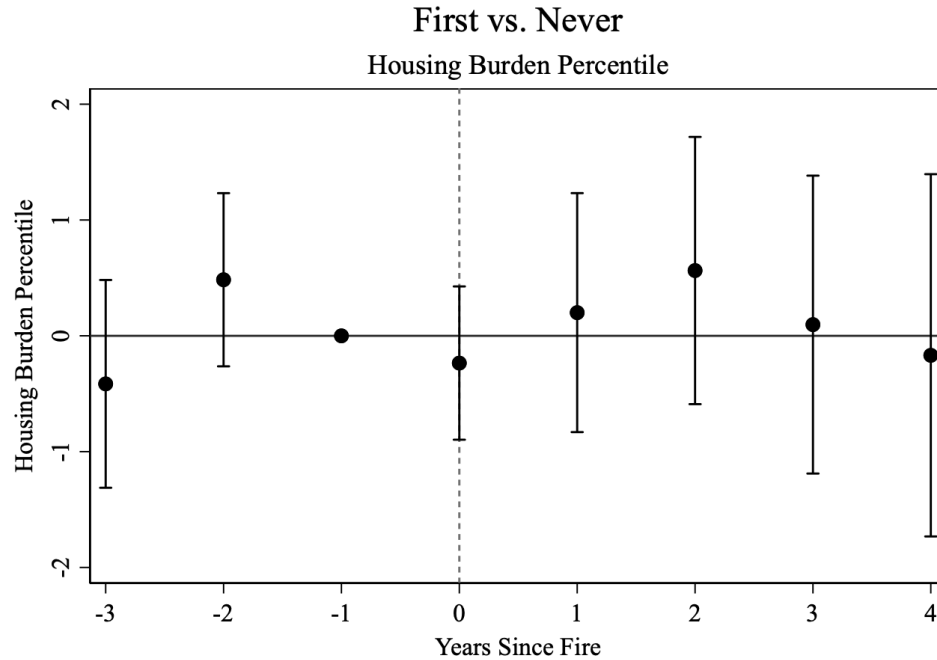
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the average rent and comes from the Opportunity Atlas, measured as the average rent of a two bedroom home in 2015. There are minimal statistically significant differences in the average rent before the fire, lending credibility to this empirical strategy. Adjusting for these are unlikely to change the statistical significance of the post-fire coefficients. There is no meaningful change in the average rent after the fire.

Figure 4.69: Housing Burden after Megafire



Notes: This graph shows the coefficients for the relative time indicator variables and the megafire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the housing burden percentile and comes from The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviron), measured as the percent of low-income households (making less than 80% of their county's median family income) who are severely burdened by housing costs (paying greater than 50% of their income for housing costs) for census tracts in California and released in 2021. There are minimal statistically significant differences in the housing burden before the fire, lending credibility to this empirical strategy. There is no meaningful change in the housing burden after the fire.

Figure 4.70: Housing Burden after First Fire



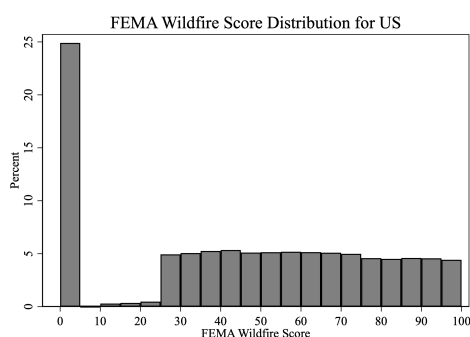
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 2.1](#) in using the UCCCP data from 2014 to 2021. The outcome variable is the housing burden percentile and comes from The Office of Environmental Health Hazard Assessment CalEnviroScreen (CalEnviron), measured as the percent of low-income households (making less than 80% of their county's median family income) who are severely burdened by housing costs (paying greater than 50% of their income for housing costs) for census tracts in California and released in 2021. There are minimal statistically significant differences in the housing burden before the fire, lending credibility to this empirical strategy. There is no meaningful change in the housing burden after the fire.

3 Appendix to “Soothing the Burn: The Impact of Government Aid on Weathering Wildfires”

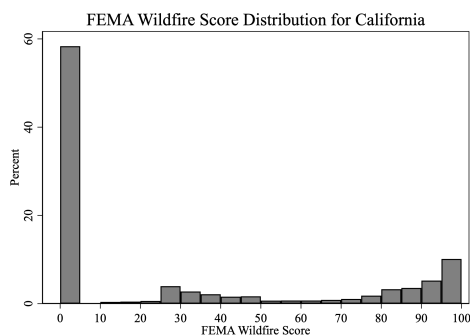
3.1 Additional Figures

Figure 4.71: FEMA Wildfire Score Distribution

(a) FEMA Wildfire Score Distribution for the United States

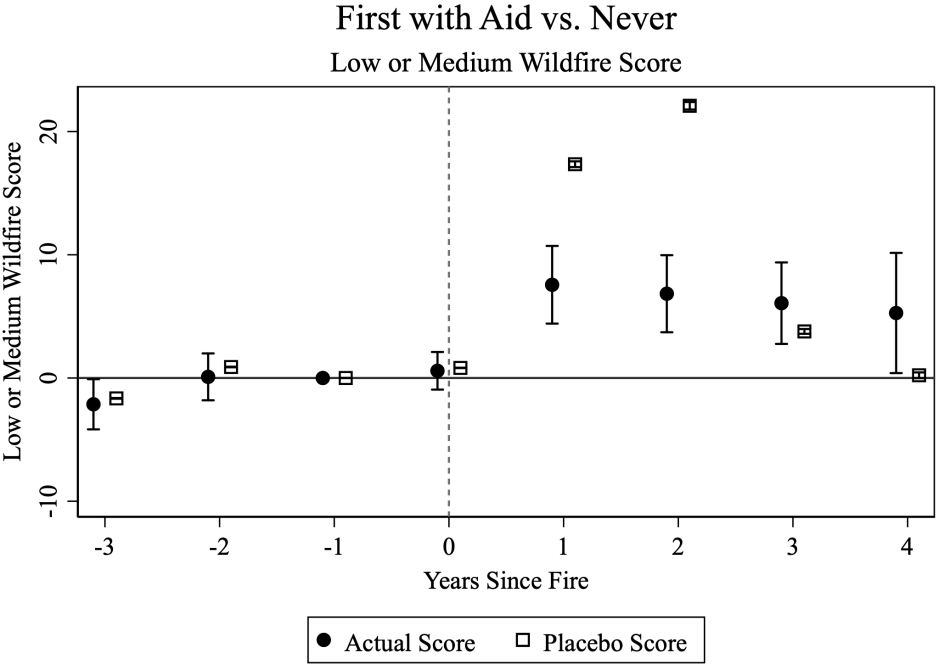


(b) FEMA Wildfire Score Distribution for California



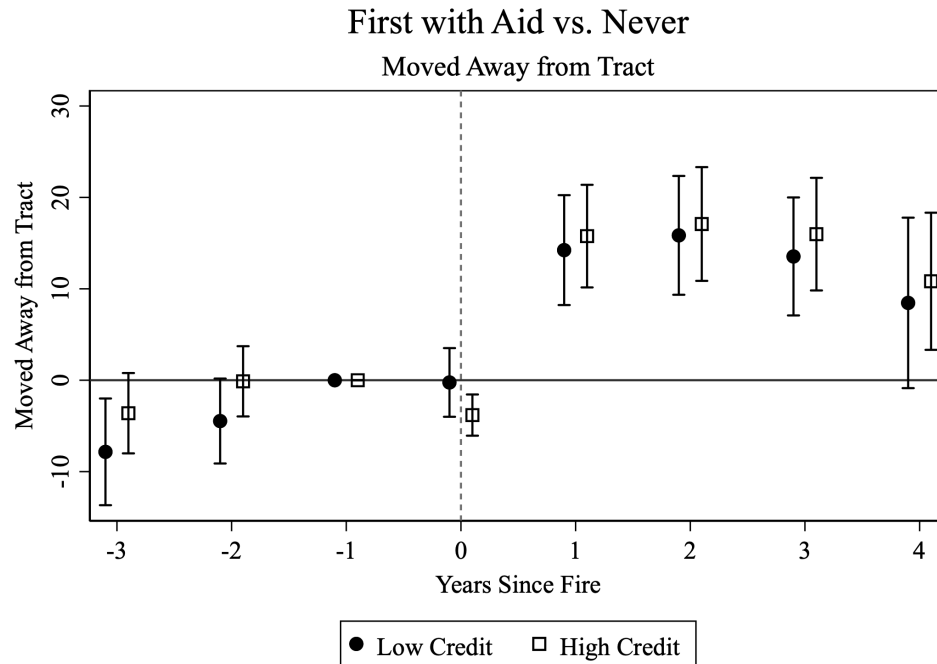
Notes: These graphs show the wildfire score distribution for the United States and California. The data are from FEMA and list the wildfire risk score at the census tract level. This is a one-year measure of wildfire risk, defined at the year 2021. [Figure 4.71](#) suggests that the wildfire risk score in the United States overall is roughly uniform when considering the tracts with a score lower than 25 as one entity (i.e., <25 , or “Low Wildfire Risk”). [Figure 4.71\(b\)](#) suggests that the wildfire risk score in California is not uniform but skewed to the left, with a higher proportion of tracts with minimal wildfire risk. This may seem surprising but is likely due to California’s large surface area and the fact that most of the state is not at the wildland-urban interface. There is also a larger proportion of tracts that are “Very High Wildfire Risk” (i.e., >75) in California than in the United States overall.

Figure 4.72: Wildfire Risk after First Fire with Aid vs. Counterfactual Moves



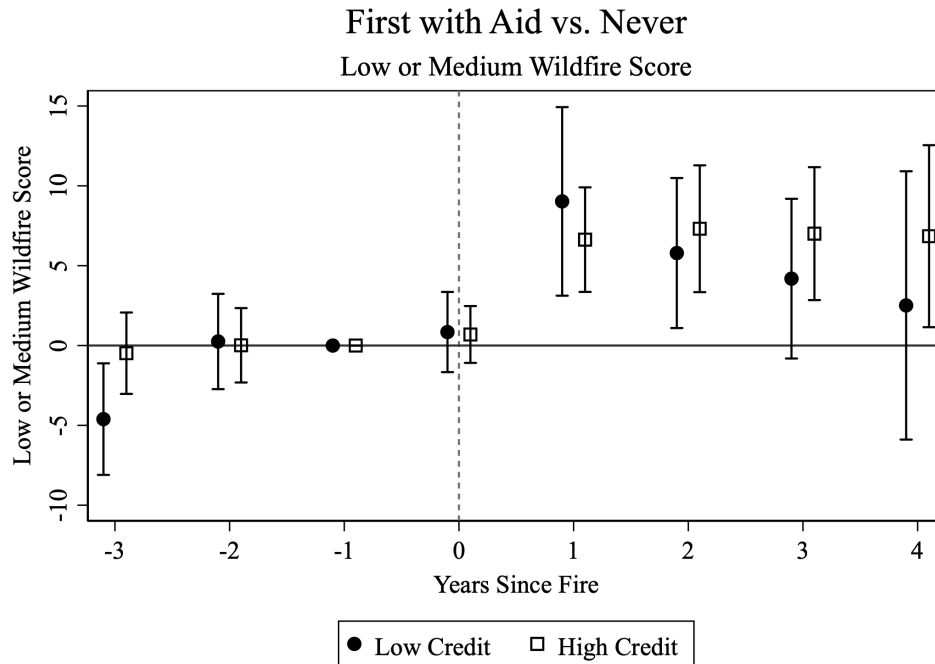
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire with aid interaction terms from estimating Equation 3.1 in the UCCCP data from 2014 to 2021. The aid is defined as whether any FEMA Individual Households Program assistance is disbursed for the fire. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. There are minimal differential likelihoods of being in a safe tract before the fire, lending credibility to this empirical strategy. “Actual Score” shows the regression coefficients from taking the actual location decisions seen in the data, whereas “Placebo Score” shows the regression coefficients from replacing the location decisions of the treated after the wildfire with those of a random mover in California. These results show that though there is some evidence of adaptive migration in the first two years, it is lower than would be expected for the average Californian mover. People in blocks that experience a first fire for which aid is disbursed are more likely to be in safe areas after four years.

Figure 4.73: Moved Away from Tract after First Fire with Aid by Credit Score



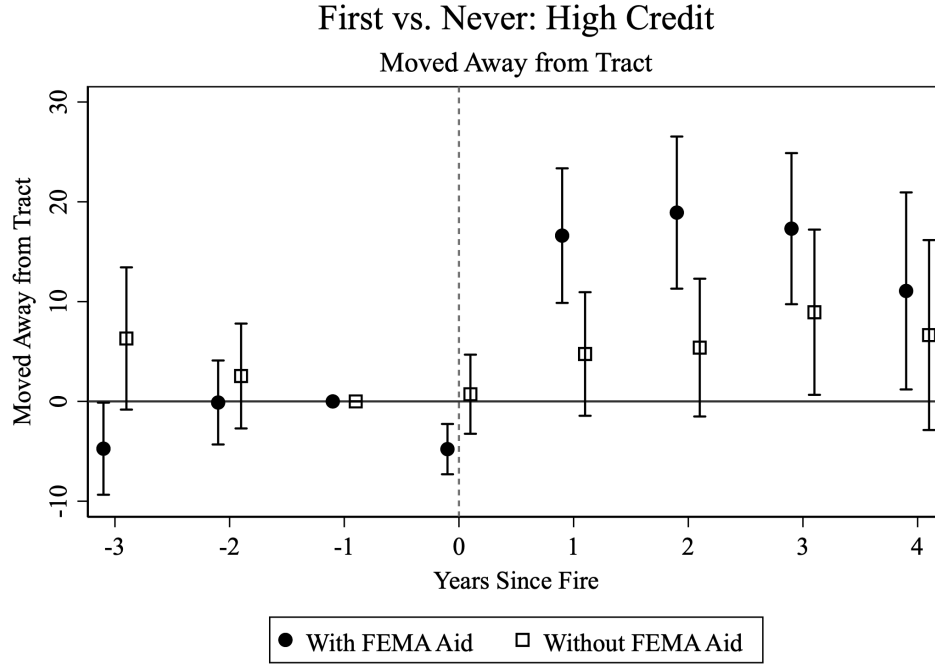
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire with aid interaction terms from estimating [Equation 3.1](#) in the UCCCP data from 2014 to 2021 across two groups. The aid is defined as whether any FEMA Individual Households Program assistance is disbursed for the fire. The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). The populations are defined as people who have high credit scores the year before the fire (≥ 700) and people who have low credit scores (< 700). There are minimal statistically significant differences in migration rates before the fire for these groups, lending credibility to this empirical strategy. There is a sharp increase in the likelihood of moving away starting the year after the fire, and this effect persists four years after the fire for people with high credit scores. However, the year four coefficient for people with high credit scores and aid is not statistically significantly different from the coefficient for people with low credit scores and aid.

Figure 4.74: Wildfire Risk after First Fire with Aid by Credit Score



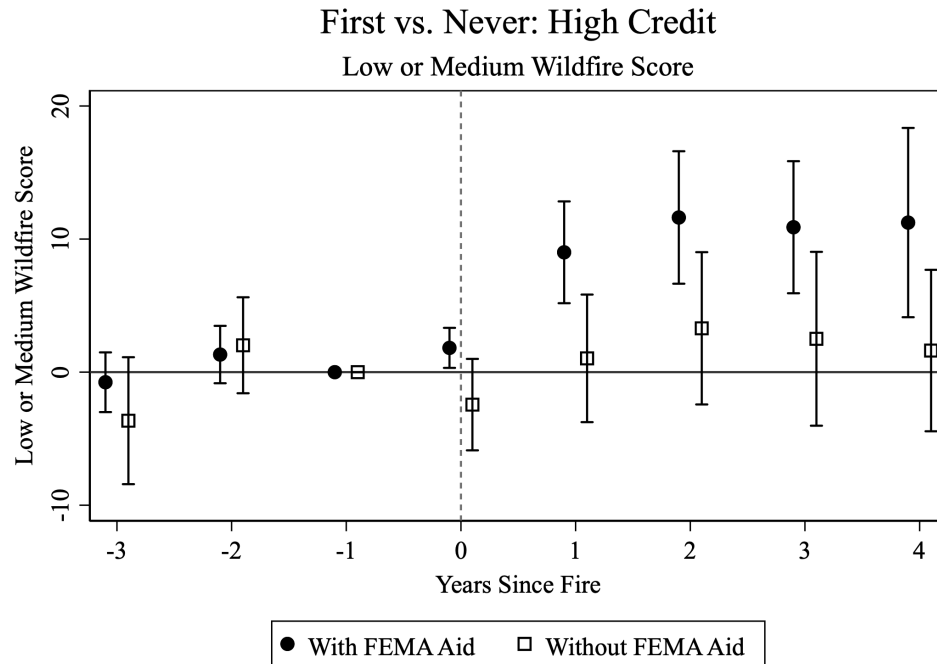
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire with aid interaction terms from estimating [Equation 3.1](#) in the UCCCP data from 2014 to 2021 across two groups. The aid is defined as whether any FEMA Individual Households Program assistance is disbursed for the fire. The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. The populations are defined as people who have high credit scores the year before the fire (≥ 700) and people who have low credit scores (< 700). There are minimal differential likelihoods of being in a safe tract before the fire, lending credibility to this empirical strategy. There is an increase in the likelihood of moving to a safe place starting the year after the fire. This increase persists only for people with high credit scores, who are statistically significantly more likely to be in a safe area four years after the fire. However, the year four coefficient for people with high credit scores and aid is not statistically significantly different from the coefficient for people with low credit scores and aid.

Figure 4.75: Moved Away from Tract for High Credit by Aid



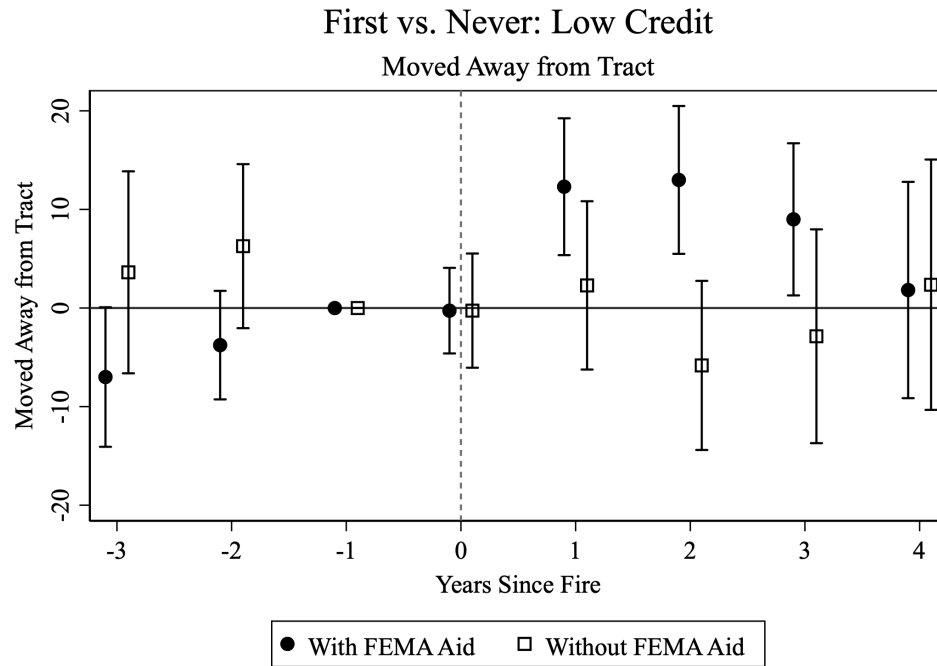
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 3.1](#) in the UCCCP data from 2014 to 2021 separately for first fires for which any FEMA Individuals and Households Program aid is disbursed and first fires for which no aid is disbursed for people with high credit scores the year before the fire (≥ 700). The outcome variable is an indicator variable that equals 100 if I observe the individual in the data in a different tract from where she was at the year before the first fire (“Moved Away from Tract”). There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy. People with high credit scores that experience first fires for which aid is disbursed are more likely to move away, whereas those that experience first fires for which aid is not disbursed have lower migration effects.

Figure 4.76: Wildfire Risk for High Credit by Aid



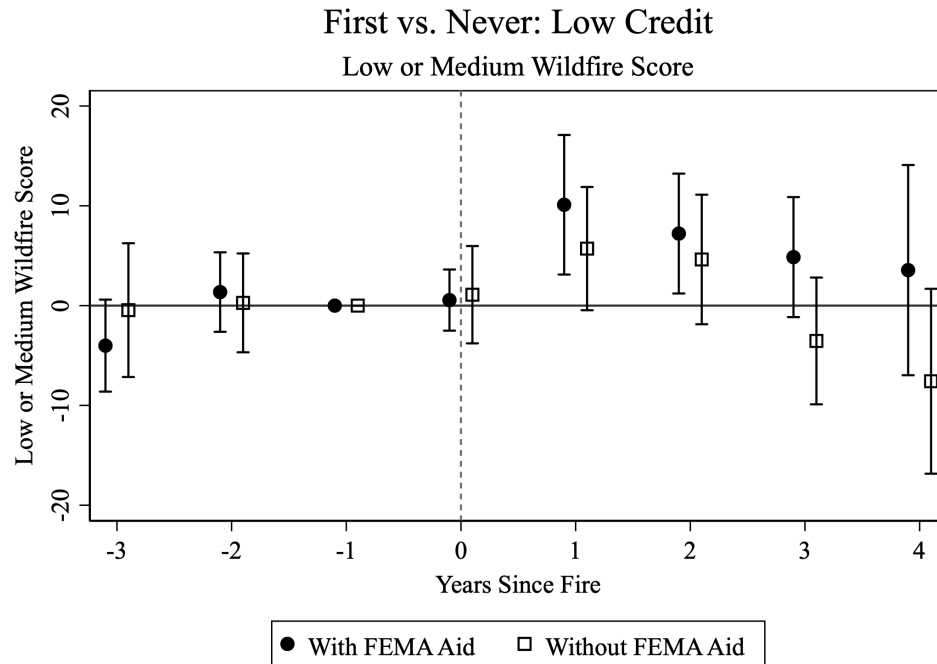
Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating Equation 3.1 in the UCCCP data from 2014 to 2021 separately for first fires for which any FEMA Individuals and Households Program aid is disbursed and first fires for which no aid is disbursed for people with high credit scores the year before the fire (≥ 700). The outcome variable is an indicator variable that equals 100 if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), as defined by FEMA’s wildfire risk index, which ranges from 0 to 100. There are minimal differential likelihoods of being in a safe tract before the fire, lending credibility to this empirical strategy. People with high credit scores that experience first fires for which any aid is disbursed are more likely to be in safe areas, even four years after the fire, whereas those that experience first fires for which no aid is disbursed are not.

Figure 4.77: Moved Away from Tract for Low Credit by Aid



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating [Equation 3.1](#) using the UCCCP data from 2014-2021, separately for first fires that receive FEMA Individuals and Households Program aid and first fires that do not, for those with low credit (those whose credit score at the time of the fire is < 700). The outcome variable is an indicator variable that equals one if the individual is at a different tract than where they were the year before the first fire (“Moved Away from Tract”). There are no statistically significant differences in migration rates before the fire, lending credibility to this empirical strategy. Low-credit individuals – regardless of whether they are in a first fire that receives aid or not – are not more likely to move away in the longer-term.

Figure 4.78: Wildfire Risk for Low Credit by Aid



Notes: This graph shows the coefficients for the relative time indicator variables and the first fire interaction terms from estimating Equation 3.1 using the UCCCP data from 2014-2021, separately for first fires that receive FEMA Individuals and Households Program aid and first fires that do not, for those with low credit (those whose credit score at the time of the fire is < 700). The outcome variable is an indicator variable that equals one if the individual is in a tract that has a wildfire risk score that is below 50 (“Low or Medium Wildfire Score”), which is defined using FEMA’s wildfire risk index that ranges from 0-100. There are minimal differential likelihoods of being in a safe tract before the fire, lending credibility to this empirical strategy. Low-credit individuals – regardless of whether they are in a first fire that receives aid or not – are not more likely to be in a safe area in the longer-term, with those without aid having a decrease in the likelihood.

3.2 Additional Tables

Table 4.6: First Stage: Politically Competitiveness (5 – 15%) 2014-2021

	Post $\times$ Treated $\times$ Political Competitiveness (%)			
	< 5%	< 7%	< 10%	< 15%
Post $\times$ Treated $\times$ IHP	0.329*** (0.076)	0.287*** (0.098)	0.115 (0.123)	0.072 (0.129)
Post $\times$ Treated	0.610*** (0.068)	0.612*** (0.069)	0.637*** (0.075)	0.651*** (0.078)
R-squared	0.616	0.619	0.629	0.633
N	288,299	288,299	288,299	288,299
Mean	0.228	0.228	0.228	0.228
F-stat	58.02	29.65	14.54	14.83
Controls	Yes	Yes	Yes	Yes
Relative Time FE	Yes	Yes	Yes	Yes
Calendar Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Cluster	County	County	County	County

Notes: I instrument receipt of any FEMA Individuals and Households Program (IHP) aid by political competitiveness. I define a politically competitive county as one in which the difference in the vote share between the top two candidates in the most recent presidential election ranges from “< 5%” to “< 15%.” I use shares of “< 5%” for my main definition of political competitiveness. I use data from MIT Election Labs to define the vote shares. I estimate [Equation 3.3](#) using UCCCP data from 2014 to 2021. I include the average size of the fires within the county (in thousands of acres), the age category of the individual, whether she is married, whether she has a college education, and the duration of time she lives in the census block as controls. I also include relative year, calendar year, and individual fixed effects. I cluster the regression at the county level. The instrument is robust to my defining political competitiveness with vote margins at various thresholds, where the likelihood of aid receipt monotonically decreases as political competitiveness decreases, lending additional credibility to the instrument’s relevance.

Table 4.7: Exclusion Restriction: Politically Competitive Counties and Climate Attitudes

	Fire		< 5%		Fire x < 5%	
	Coefficient	p-value	Coefficient	p-value	Coefficient	p-value
Global Warming is Happening	-3.517	0.000	-3.705	0.029	2.548	0.088
Global Warming Will Harm US Within 10 Years	-6.464	0.000	-3.618	0.035	2.097	0.296
Worried About Global Warming	-3.849	0.000	-2.901	0.010	1.883	0.109
Global Warming Will Personally Harm Me	-2.483	0.000	-3.322	0.003	1.095	0.251
Global Warming Will Start to Harm US Citizens	-3.748	0.000	-3.061	0.009	1.376	0.309
Scientists Agree Global Warming is Happening	-5.769	0.000	-4.320	0.051	2.739	0.226
Support Requiring Renewable Sources for Utilities	-1.859	0.001	-1.409	0.074	2.083	0.006
Support Funding Renewable Energy Research	-2.831	0.000	-1.206	0.390	0.997	0.442

Notes: The exclusion restriction requires that a county's political competitiveness impact migration through only aid receipt, nothing else. Although this condition is not directly testable, I evaluate the likelihood of there being different climate attitudes as a way of assessing the validity of the exclusion restriction. I define a politically competitive county as one in which the difference in the vote share between the top two candidates in the most recent presidential election is within 5%. I use data from MIT Election Labs to define vote shares. I estimate [Equation 3.4](#) using a variety of county-level climate attitudes from the Yale Program on Climate Change Communication. The climate attitudes data are the percentage of people within the county that are estimated to believe in the following statements related to climate change and global warming asked between 2014 and 2021. I find that there are no statistically significantly differential climate attitudes for politically competitive counties that experience a fire.

Table 4.8: DID-IV Results: Political Competitiveness ($< 5\%$) with County Controls

	<u>DID-IV</u>		<u>DID-IV with County Controls</u>	
	Moved Away from Tract	Low or Medium Wildfire Risk Tract	Moved Away from Tract	Low or Medium Wildfire Risk Tract
Post $\times$ Treated $\times$ IHP	35.170*** (8.762)	-0.540 (8.923)	45.438*** (11.161)	-3.952 (10.933)
Post $\times$ Treated	-14.195** (7.928)	4.577 (4.686)	-2.408 (10.933)	2.849 (2.509)
N	250,594	288,299	91,656	106,055
Mean	17.40	22.23	17.40	22.23
First Stage F-stat	42.76	301.26	17.18	18.92
Individual Controls	Yes	Yes	Yes	Yes
County Controls	No	No	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes
Calendar Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Cluster height	County	County	County	County

Notes: This table reports the coefficients from the difference-in-differences instrumental variables specification (DID IV), which first estimates Equation 3.3 for FEMA Individuals and Households Program (IHP) aid. In both specifications, I control for the average size of the fires within the county (in thousands of acres), the age category of the individual, whether she is married, whether she has a college education, and the duration of time she lives in the census block. I also include relative year, calendar year, and individual fixed effects. The county controls I include are the ones I use in Table 3.3, in which I estimate Equation 3.4 using a variety of county-level data sources from the California Open Data website. These county-level controls are the expenditures data from California's County Financial Transactions Report on Expenditures, the food stamps data come from CalFresh, and the tax data from the California Department of Tax and Fee Administration. I cluster the regression at the county level.

Table 4.9: DID and DID-IV Results: Borrowing Index

	DID	DID-IV
Post x Treated x IHP Aid	-0.000 (0.000)	0.002 (0.001)
Post x Treated	0.005** (0.002)	0.004** (0.002)
Post	-0.010** (0.004)	-0.010** (0.004)
Individual Fixed Effects	Yes	Yes
Controls	Yes	Yes
N	250,594	250,594
First Stage F-stat		73.86

Notes: This table reports the coefficients from the difference-in-differences (DID) specification, which estimates [Equation 3.2](#), and the DID instrumental variables specification (DID-IV), which first estimates [Equation 3.3](#) for FEMA Individuals and Households Program (IHP) aid. In both specifications, I control for the average size of the fires within the county (in thousands of acres), the age category of the individual, whether she is married, whether she has a college education, and the duration of time she lives in the census block. I also include relative year, calendar year, and individual fixed effects. I cluster the regression at the county level.

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