DISSERTATION

ESSAYS ON ELECTRIC VEHICLES, WILDFIRES, AND REGIONAL ECONOMIC IMPACTS

Submitted by

Thomas C. Gifford

Department of Economics

In partial fulfillment of the requirements

For the Degree of Doctor of Philosophy

Colorado State University

Fort Collins, Colorado

Spring 2026

Doctoral Committee:

Advisor: Edward Barbier

Anders Fremstad
Anita Pena
Jude Bayham

ABSTRACT

ESSAYS ON ELECTRIC VEHICLES, WILDFIRES, AND REGIONAL ECONOMIC IMPACTS

This dissertation presents three essays in applied microeconomics that analyze how local communities respond to regional environmental and technological change. The first essay studies the role of public charging infrastructure in promoting electric vehicle (EV) adoption. Using county-level panel data from Washington State between 2019 and 2023, I estimate the marginal effects of new charging stations on new EV registrations and test for heterogeneity across income levels and charger types. The results show that charging infrastructure increases battery electric vehicle adoption, but the effects are concentrated in higher-income counties. Furthermore, fast chargers are shown to increase EV adoption across a wider income range than slower chargers. These results indicate that infrastructure expansion alone will not produce equitable EV adoption outcomes.

The second essay analyzes the long-run economic impacts of large wildfires on regional tourism spending in Colorado. Using county-level tourism spending data from 1996 to 2019 and a staggered difference-in-differences framework, I find that the largest wildfires generate persistent declines in tourism expenditures and associated state and county tax revenues. These losses are primarily driven by reductions in commercial lodging activity. Results show that extreme wildfire events can produce sustained disruptions to local tourism-dependent economies and pose a growing threat to regional public services.

The third essay examines the relationship between wildfire exposure and drinking water quality in Colorado between 2000 and 2023. Using watershed-level panel data, the analysis shows that wildfire exposure is associated with statistically significant increases in Safe Drinking Water Act violations, particularly turbidity violations. Smaller and moderately sized fires generate significant increases in violations, suggesting that even relatively modest wildfire events can disrupt drinking

water systems. Back-of-the-envelope cost estimates indicate that these violations may impose substantial operational costs on water utilities.

These essays highlight how infrastructure, wildfire disturbances, and environmental systems interact with economic activity and public services, with implications for energy transition policy, wildfire management, and drinking water system resilience.

ACKNOWLEDGEMENTS

This dissertation does not exist without the guidance, support, and patience of a lot of people. It takes a village to support an idealistic graduate student through their Ph.D. journey, and I'm certainly no exception.

I am eternally grateful for the decades of support from my family: my parents Ray and Lisa, my brothers William and Michael, my grandparents, aunts, uncles, cousins, and a host of extended family members who have helped shape me into the person I am today. My parents especially have been nothing but patient and supportive through every twist and turn in my life, and were always there to pick me up and encourage me when I was struggling. I would not be here today without their unwavering love and support. To William and Michael— you are the best brothers a guy could ask for, and I'm incredibly lucky to have you both in my life.

I would not have completed this project without the guidance of my advisor, Dr. Edward Barbier, who has been instrumental in shaping me into the researcher I am today. This project has changed considerably in both scope and focus since he first asked me what I wanted to get out of my dissertation, and it would not be up to the standard it is today without his mentorship. I am thankful to my committee members, Dr. Anders Fremstad, Dr. Anita Pena, and Dr. Jude Bayham for taking the time and energy to provide feedback on the various iterations of this research, as well as providing pro-bono therapy sessions throughout the process. Beyond my committee members, several faculty members at Colorado State University have been generous with their time and ideas throughout my entire Ph.D. journey, including Dr. Ray Miller, Dr. Alex Bernasek, and Dr. Tim Komarek, just to name a few. I also especially want to thank Dr. Stephan Weiler, who provided me with invaluable encouragement at several difficult points throughout my time at CSU and whose relationship I am ineffably grateful for.

The community at CSU defined my time as a Ram, and is something that I'll cherish for the rest of my life. Thank you to my cohort-mates: Greg Miller, Paula Leone-Samuda, Milena Dehn, Matt Harman, Nara Chung, Sean Callahan, and Luke Maddock for providing the friendship and support that is so essential to succeeding in an economics Ph.D. program. I've also been lucky to make friendships with several people within and outside of the economics department who made my experience unforgettable: Tais Menezes, Vinicius Cicero, Brendan Brundage, Kendall Stephenson, Debora Nunes, Sal McCollum, Austin Landini, Brooke Fitzgerald, Madison Morse, Jackie Young, Brendan Davidson, Sarai Benitez, Ben Jones, MJ Sorapuru, and several other people during my five years at CSU.

I would also like to acknowledge the Center for the New Energy Economy and specifically my boss, Dr. Katherine Hoffer, who not only gave me work and supported me throughout the Ph.D., but was invariably patient, flexible, empathetic, and kind to me, even as my life became chaotic for stretches of time. I'm grateful to have made great relationships with several people at CNEE over the years, including Dr. Suzanne Tegen, Alexandra Evans, Susa Delja, Carlyn Petrella, Ellen Eisenbeis, Evelyn Peterson, Jack Portman, Jeff Ackerman, and a host of other fantastic people I've met through my work at the Center.

Lastly, and most importantly, words cannot express how grateful I am for the unflappable love and support of my incredible wife, Bobbi. To say I would not have made it through this journey without you is the understatement of the millennia. In times of struggle and doubt, Bobbi was there to give me love, support, and hope. She gave me the strength to keep going on countless occasions, and I would not have had the courage to even attempt this crazy journey without her by my side. Bobbi— I love you more than you'll ever know and you are the best life partner anyone could ever hope for. I can only hope to give you a fraction of the support you've given me over the last five years. You are the light of my life, the wind at my back, and you motivate me to be a better person every single day. I cannot wait to see what lies ahead for us.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

Introduction

This dissertation presents three essays in applied microeconomics that analyze how local communities respond to regional environmental and technological change. The topics differ across chapters, covering electric vehicle infrastructure and the economic consequences of wildfires. However, all three of these studies help inform how policymakers can design more targeted and effective policies to support electric vehicle adoption and address the community-level impacts of wildfires, two issues of growing importance across the United States and around the world.

Chapter 1, "The Role of Charging Infrastructure and Income on Electric Vehicle Adoption," examines how the expansion of public charging stations influences electric vehicle (EV) adoption in Washington State from 2019 to 2023, with a particular focus on how income shapes adoption responses. Using a fixed-effects panel framework, the analysis shows that while new chargers are generally associated with increased EV adoption, the effect is concentrated in high-income counties. Results also show that fast chargers generate adoption gains across a wider range of income levels than slower alternatives, though income continues to condition adoption responses across charging technology types. These findings underscore the importance of income as a key driver in the transition to electric vehicles and suggest that infrastructure investments alone may be insufficient to ensure equitable access to EVs.

Chapter 2, "Long-Term Effects of Large Wildfires on Tourism Spending in Colorado," examines how large wildfire events affect regional tourism spending and associated tax receipts across Colorado. Using county-level tourism spending data from 1996 to 2019 and a staggered difference-in-differences framework, I estimate how wildfire exposure of varying sizes influences tourism-related economic activity. The results show that extreme events—fires exceeding 30,000 acres in size—generate significant and persistent declines in regional tourism expenditures and related state and county tax revenues. These reductions are driven primarily by declines in commercial lodging spending. The findings highlight the substantial economic disruptions caused by the largest wildfire events and underscore the importance of wildfire mitigation and long-term economic recovery strategies for affected regions.

Chapter 3, "Wildfires and Drinking Water Quality: Evidence from Colorado Watersheds," examines the relationship between wildfire exposure and Safe Drinking Water Act violations across Colorado from 2000 to 2023. Using watershed-level panel data, I find that wildfire exposure is associated with significant increases in reported drinking water violations. The largest increases are associated with relatively small and moderately sized fires—those affecting only a few hundred to a few thousand acres—indicating that even modest wildfire events can disrupt drinking water systems. Turbidity violations in particular increase significantly following wildfire exposure. Preliminary cost estimates indicate that these disruptions may impose substantial operational costs on drinking water utilities, particularly for large systems treating high daily volumes. Overall, the results suggest that small and moderately sized wildfires represent an underappreciated risk to drinking water quality and may impose significant costs on water utilities.

While these three chapters cover a range of topics, including transportation policy, the impact of natural disasters, and the value of environmental quality, they are united by a common focus on the regional impacts of issues that are increasingly important across the western United States. The goal of this research is to provide evidence that can help policymakers design more effective policies related to EV infrastructure deployment, wildfire mitigation, and post-fire recovery.

Several western states, including California, Oregon, Washington, New Mexico, and Colorado, have enacted policies aimed at accelerating a transition away from gasoline-powered vehicles toward alternative technologies such as electric vehicles. Many of these policies also explicitly emphasize environmental justice and an equitable energy transition. The first chapter of this dissertation contributes to this discussion by examining how income conditions the effectiveness of charging infrastructure investments, highlighting the importance of considering economic barriers to EV adoption.

Wildfires, meanwhile, are an increasingly important topic in policy discussions across the western United States. As Chapters 2 and 3 document, the frequency and severity of wildfires in Colorado and other western states have increased, along with the damages associated with these events. Recent years have seen an increase in historically unprecedented fires that burn tens of thousands of acres in mere days, disrupting ecosystems and contributing to long-term environmental degradation.

The increasing size and severity of wildfires carry consequences for local communities across multiple dimensions, including tourism activity, public health, environmental quality, and the long-term economic viability of affected regions. Chapters 2 and 3 examine two of these consequences: tourism spending and drinking water quality. While not a comprehensive accounting of the economic impacts of large wildfires, these chapters provide insight into the magnitude and duration of the disruptions that affected communities face. Long after the flames are extinguished, the effects of wildfire exposure may continue to shape local economic and environmental conditions.

The remainder of the dissertation is structured as follows. Chapter 1 examines the role of income and charging infrastructure in electric vehicle adoption in Washington State. Chapter 2 evaluates the long-term impacts of large wildfires on tourism spending in Colorado. Chapter 3 explores how wildfire exposure influences drinking water quality violations across the state.

Chapter 1

The Role of Charging Infrastructure and Income on Electric Vehicle Adoption

This chapter examines how the expansion of public charging infrastructure influences electric vehicle (EV) adoption across income levels. Using county-level panel data from Washington State (2019–2023), I estimate the marginal effect of new chargers on EV uptake and test for heterogeneity by income and charger type. Results show that charger deployment increases EV adoption, but the effect is concentrated in high-income counties. Fast chargers generate larger and more widely distributed adoption gains than slower charging options, and income consistently conditions these effects. These patterns appear only in the battery electric vehicle (BEV) market; comparable models for plug-in hybrid vehicles (PHEVs) show no significant relationships. Robustness checks also indicate that charger deployment drives BEV growth rather than responding to it. These results highlight both the central role of infrastructure in enabling decarbonized transport and the risk that exclusive reliance on chargers may widen inequities, underscoring the need for complementary policies that expand access to zero-emission mobility. Results indicate that charging investments alone may not yield equitable adoption outcomes and underscore the importance of complementary policies that address barriers to entry beyond charging availability.

1.1 Introduction

Once a novelty, electric vehicles (EVs) have become more mainstream across the United States and play an invaluable role in reaching a carbon neutral economy. Transportation emissions account for 29% of total US carbon emissions, over half of which originate from light-duty, mass-market vehicles, meaning a pivot away from cars that use internal combustion engines is necessary to achieve carbon neutrality. Several states, including Washington, have adopted specific electric vehicle (EV) deployment goals with emphasis on equity and environmental justice, yet several market dynamics in the EV space remain murky to policymakers.

There are two designations of EV: Battery Electric Vehicles (BEVs), which run entirely on electricity, and Plug-in Hybrid Electric Vehicles (PHEVs), which typically have a short battery range (20-40 miles) and have the option of using a gas powered engine for longer trips. Both the BEV and PHEV markets are analyzed in this paper, with each market showing unique characteristics and responses to increased public charging capacity.

Public charging infrastructure is widely recognized as an important determinant of electric vehicle (EV) adoption. Several mechanisms may explain this relationship. Consumers often cite concerns about running out of charge before reaching a destination (often called range anxiety) but other factors also play important roles, including limited access to home charging (particularly in multifamily housing) and general unfamiliarity with EV technology and charging routines (Sheldon, 2022; Rainieri et al., 2023). Together, these concerns shape how prospective buyers perceive the convenience and reliability of EV ownership. This paper estimates the marginal effect of new public chargers on county-level EV adoption and examines how these effects vary across income levels and charging technologies.

Using detailed EV registration data from Washington State between 2019-2023, this analysis shows that the marginal effect of new public chargers on EV adoption is highly dependent on local income levels. Counties with higher average incomes exhibit a stronger positive response to charger expansion than lower-income counties. I also differentiate by charging technology and find that the marginal effect of a Direct Current Fast Charging (DCFC) point is greater than that of a Level 1 or Level 2 charger, with high-income areas showing stronger responses across all charger types.

This paper contributes to the literature in four ways. First, whereas most existing studies rely on stated-preference surveys (Franke et al., 2012; Tal and Nicholas, 2016; Xu et al., 2017; Varghese et al., 2024; Lee and Nilsson, 2025; Zatsarnaja et al., 2025), this analysis uses revealed-preference market data to estimate how charging infrastructure affects EV adoption. Second, I leverage high-resolution geographic data on EV registrations and charger locations, allowing us to measure local infrastructure effects more precisely than the cross-country or cross-state comparisons common in prior work (Sierzchula et al., 2014; Falchetta and Noussan, 2021; Yi et al., 2020; Münzel et al., 2019; Narassimhan and Johnson, 2018; Sheldon, 2022). Third, I provide novel evidence that the effect of new public charging infrastructure on BEV adoption is conditioned by local mean income. Although some research compares the effects of different charger technologies (Qian et al., 2019; Springel, 2021; Haidar and Rojas, 2022; Burra et al., 2024), none examine how these effects vary by income. Finally, this study offers the first revealed-preference estimates of the marginal effects of charger type while explicitly incorporating income-conditioned heterogeneity.

These findings have important policy implications and suggest that infrastructure expansion by itself will not yield equitable adoption outcomes. Although earlier research has estimated average effects of charging infrastructure, few studies distinguish impacts by charger type, particularly DC fast chargers, which only became widely deployable in the 2020s (Kim et al., 2022).

These findings highlight inequities in EV adoption, which are important to consider in the design of policies to encourage greater adoption. The heterogeneous response to charging infrastructure expansion suggests that simply increasing charging availability is not sufficient to drive widespread EV adoption as suggested by previous studies (Sierzchula et al., 2014; Narassimhan and Johnson, 2018; Egnér and Trosvik, 2018; Springel, 2021; Haidar and Rojas, 2022; Schulz and Rode, 2022; Varghese et al., 2024). Instead, policymakers should adopt a multifaceted strategy to maximize EV adoption. Expanding charging infrastructure in high-income areas, where it has the strongest impact, should be paired with direct financial incentives or targeted policy interventions in lower-income areas to encourage broader adoption.

These results show that the effect of new public chargers varies significantly across income levels and charger technologies, indicating that blanket infrastructure expansion will not yield equitable adoption outcomes alone. The heterogeneous response adds nuance to earlier findings that report positive relationships between charging availability and EV adoption (Sierzchula et al., 2014; Narassimhan and Johnson, 2018; Egnér and Trosvik, 2018; Springel, 2021; Haidar and Rojas, 2022; Schulz and Rode, 2022; Varghese et al., 2024).

Overall, these results underscore the need for a multifaceted policy approach: investments in charging infrastructure should be complemented by targeted financial incentives to encourage EV adoption and equity-focused interventions in lower-income communities where the marginal impact of new chargers on adoption is weaker.

1.2 Background

A growing body of research shows that public charging infrastructure plays an important role in electric vehicle (EV) adoption. Consumers consistently report concerns about the convenience and availability of charging, and several studies document a positive association between charger access and EV uptake across a range of regions and data sources. Multiple mechanisms may underlie this relationship, including range anxiety, limited access to home charging, and general unfamiliarity with EV technology and charging routines. Even though most BEV models now provide 100 to 300 miles of range (U.S. Department of Energy, 2023), far exceeding the average U.S. round-trip commute of 26 miles (Federal Highway Administration, 2023), these concerns remain widespread (Sheldon and DeShazo, 2017; Tal and Nicholas, 2016; van Dijk et al., 2022; Li et al., 2017; Pew Research Center, 2024). This paper examines the continuing importance of public charging infrastructure in shaping EV adoption and how its effects vary across income levels and charger technologies.

1.2.1 Factors in EV Adoption in the US

Numerous studies indicate that public charging availability, financial incentives, and vehicle characteristics are important correlates of EV uptake, although the magnitude of these relationships varies across studies. Early cross-country work finds that higher densities of public charging stations are associated with increased EV market shares (Sierzchula et al., 2014). Similar patterns are present in studies from Norway, China, and Europe, where access to public charging, workplace charging, and faster charging speeds are linked to stronger purchase intentions or higher adoption rates (Qian et al., 2019; Springel, 2021; Falchetta and Noussan, 2021; Schulz and Rode, 2022; Zatsarnaja et al., 2025). Multiple studies in the United States also show that increases in public chargers per capita are associated with higher BEV and PHEV registrations, with stronger effects for vehicles that rely exclusively on battery power (Narassimhan and Johnson, 2018; Burra et al., 2024).

At the same time, several reviews point to mixed evidence on the relationship between charging infrastructure and EV adoption. There is substantial variation across empirical studies, with differences in measurement, data sources, and market characteristics. Some survey-based work finds that cost, unfamiliarity, and risk aversion outweigh the influence of charging availability, while other studies identify charging access as a significant determinant of purchase intentions or stated adoption. Other factors influencing the relationship between charging infrastructure and EV demand include regional attributes, market maturity, the availability of home charging, consumer familiarity with EV technology, and political affiliation (Muratori et al., 2020; Miele et al., 2020; Ghasri et al., 2019; Davis et al., 2025).

A considerable literature also documents substantial spatial variation in public charging accessibility. Studies across the United States, Europe, and China show that stations often cluster in commercial districts, along major corridors, or in higher-income neighborhoods, leading to uneven effective access across communities (Falchetta and Noussan, 2021; Peng et al., 2024b,a; Esmaili et al., 2024). Because accessibility depends on siting, reliability, and charging speed—not just the number of chargers—nominal station counts may overstate the practical usability of charging networks.

Behavioral research provides another rationale for why public charging availability can stimulate EV adoption. Although BEVs can accommodate typical daily travel, many potential buyers remain concerned about depleting the battery before completing a trip or reaching a charger, indicating that range-related concerns reflect perceived rather than actual constraints (Franke et al., 2012; Melliger et al., 2018; van Dijk et al., 2022). This ”range anxiety” increases sharply when a vehicle's state of charge falls below a personal comfort threshold and may be heightened among drivers with longer or more variable commutes (Franke et al., 2012; Xu et al., 2017, 2020; Gifford and Barbier, 2025). Because public charging reduces uncertainty about access and shortens perceived or actual refueling time, greater charger density is expected to mitigate these concerns and increase EV demand (Qian et al., 2019; Wang et al., 2023; Rainieri et al., 2023).

Several studies highlight that the characteristics and placement of charging infrastructure are central to its effectiveness. Public EV charging infrastructure varies in speed and convenience, ranging from slow Level 1 chargers that use standard 120V outlets, to Level 2 chargers that offer moderate charging rates, to DC Fast Chargers that can recharge most BEVs from empty to full in under half an hour. Evidence from Norway and France shows that fast-charging availability generates larger adoption increases than slower chargers, underscoring the importance of charging speed (Springel, 2021; Haidar and Rojas, 2022). Discrete choice and synthetic-population models have emphasized the role of workplace charging and proximity to commuting routes (Burra et al., 2024). Utilization studies in the United States find that charging demand is highly uneven across locations. Some sites suffer from congestion while others are consistently underused, which reinforces the importance of siting and spatial convenience (Smart and Salisbury, 2015). Public charging can also substitute for home charging in areas with limited off-street parking, a mechanism particularly relevant in denser or lower-income neighborhoods (Zou et al., 2020).

Infrastructure investments are often compared to financial incentives as policy tools for accelerating EV adoption. Ledna et al. (2022) show that both strategies increase adoption, though the relative effectiveness depends on underlying technology assumptions. Consistent with this, Connolly et al. (2025) find that incentives can increase adoption—particularly in disadvantaged communities—but also emphasize that charging infrastructure remains an important complementary driver of registration growth. Theoretical and empirical studies likewise show that public charging can generate strong indirect network effects and, in some contexts, may be more cost-effective than vehicle subsidies (Li et al., 2017).

1.2.2 Income, Equity, and Distributional Considerations

Income is one of the strongest and most persistent predictors of EV adoption, with higher-income and highly educated households making up a disproportionate share of early adopters (Sheldon and Dua, 2019; DeShazo et al., 2017; Sheldon, 2022). These patterns reflect both financial constraints and differences in baseline familiarity and attitudes toward EVs across income levels.

Evidence from incentive programs illustrates how income conditions behavioral responses to EV related policies. Several studies show that a large share of incentive recipients would have purchased an EV regardless of program participation, limiting policy efficacy and increasing the cost per additional vehicle (Tal and Nicholas, 2016; DeShazo et al., 2017; Jenn et al., 2018; Sheldon, 2022). Programs targeted toward low-income households tend to generate more adoption per dollar, suggesting that responsiveness to incentives varies systematically across income groups (DeShazo et al., 2017). These findings imply that income influences not only the ability to purchase an EV, but also the extent to which households respond to financial and non-financial adoption incentives.

Charging infrastructure displays similar distributional patterns. Low-income communities generally have fewer public charging stations per capita, face higher rates of charger reliability issues, and receive a smaller share of clean-vehicle incentive funds (Connolly et al., 2025; Yu et al., 2025). Spatial analyses show that charging infrastructure often clusters in commercial corridors, along major roadways, or in higher-income neighborhoods, producing uneven effective access even where total charger counts appear sufficient (Esmaili et al., 2024; Falchetta and Noussan, 2021; Peng et al., 2024b,a).

However, national surveys suggest that physical proximity to public charging may not differ sharply across income groups, even though adoption outcomes do. This may indicate that access alone does not guarantee equitable uptake, and further shows that households living within one mile of a public charger are more likely to own or consider owning an EV, even after accounting for demographics and political affiliation. Because public charging can partially substitute for home charging, especially in dense or lower-income areas with limited off-street parking, unequal deployment may reinforce existing socioeconomic adoption gaps (Pew Research Center, 2024; Zou et al., 2020).

Overall, the literature shows inequities in both EV adoption and charging access, shaped by income, housing, political preferences, and infrastructure reliability. Yet despite strong evidence that income matters for adoption, far less is known about how the effect of charging infrastructure on EV adoption is influenced by differences in income across communities. Higher-income communities may be more able to take advantage of new chargers, while lower-income communities, despite relying more heavily on public charging, may remain constrained by cost, reliability, and informational barriers. This gap motivates the empirical analysis of how the marginal effect of new public charging stations varies across mean county-income levels.

1.2.3 Methodological Approaches in EV Adoption Research

Research on EV adoption and the role of public charging infrastructure employs a wide range of methods. Much of the existing literature relies on stated-preference surveys, which provide insight into consumer attitudes, range-related concerns, and perceived charging barriers. These include behavioral surveys (Franke et al., 2012; Rauh et al., 2020; Zatsarnaja et al., 2025), studies of charger access and reliability (Falchetta and Noussan, 2021; Peng et al., 2024b; Esmaili et al., 2024; Yu et al., 2025), and questionnaire-based analyses using structural equation models (Huang and Ge, 2019; Asadi et al., 2021). Numerous national or global consumer-intention surveys also contribute to this body of evidence (Miele et al., 2020; Ghasri et al., 2019; Singer, 2020; Powell and Johnson, 2024). While these studies offer important insight into perceptions and behavioral mechanisms, stated intentions may not generalize to real-world purchasing behavior, and survey instruments cannot fully address endogeneity in infrastructure placement.

A growing set of studies use revealed-preference data to estimate the relationship between charging availability and EV adoption. These studies typically employ cross-sectional or panel-data regressions (Sierzchula et al., 2014; Narassimhan and Johnson, 2018; Egnér and Trosvik, 2018; Connolly et al., 2025; Davis et al., 2025). Although more generalizable than stated-preference approaches, causal inference remains difficult because charging stations are often built in areas with high expected demand creating a "chicken vs. egg" problem. Instrumental-variable strategies, such as those using supermarket counts, parking-space availability, or density of local rivers to instrument for charger deployment (Li et al., 2017; van Dijk et al., 2022; He et al., 2025), are intended to isolate exogenous variation, but these instruments tend to be context dependent. In this study's data, the supermarket count IV was insignificant in the first-stage and is therefore not a valid instrument in this context.

Simulation and systems-based modeling approaches, including scenario analyses comparing infrastructure investment to vehicle subsidies, system-dynamics models, and multi-agent simulations, are employed to study long-run policy effects and technology transitions (Ledna et al., 2022; Huang et al., 2021; Zheng et al., 2025). These approaches, however, rely heavily on calibration to specific market conditions and on parameters frequently drawn from survey responses and behavioral assumptions (Huang et al., 2021). As a result, their conclusions are sensitive to modeling assumptions and may not generalize across regions or policy environments. Discrete choice and probabilistic models are also used to estimate how charging availability influences individual adoption decisions; for example, Burra et al. (2024) combine a discrete choice model with a Bayesian synthetic population to assess EV ownership at the census-tract level. Spatial models, including gravity-based accessibility measures, provide additional tools for assessing heterogeneous access to charging infrastructure across neighborhoods (Esmaili et al., 2024). Finally, machine learning methods, optimization algorithms, and game-theoretic models are increasingly applied to predict charging demand, evaluate siting strategies, or simulate operator behavior (Zheng et al., 2025).

While these studies offer insight into consumer attitudes, their external validity is limited, and robust causal estimates remain rare. Existing instrumental-variable strategies also face endogeneity challenges and lack generalizable and empirically strong instruments for charging deployment. These limitations motivate the empirical strategy in this paper, which employs a two-way fixed effects (TWFE) framework using detailed EV registration data and observed charger deployments to estimate how the impact of new public charging infrastructure is conditioned by county-level differences in income. Likewise, most studies treat charging infrastructure as homogeneous and provide little evidence on how different charger types differentially influence adoption. This paper addresses these gaps by using charger deployment records from Washington State to estimate the marginal effect of new public chargers on EV adoption vary across both income levels and differences in charger technologies.

1.3 EV Adoption in Washington State

Washington State provides an ideal setting to examine the relationship between charging infrastructure and EV adoption for several reasons.

First, Washington's EV market has matured without state-level purchase incentives during the study period (2019–2023), offering a unique opportunity to isolate the effects of charging infrastructure on adoption. Many prior studies focus on regions where EV subsidies played a significant role, making it difficult to disentangle the effects of policy incentives from infrastructure expansion. By contrast, Washington's policy environment allows for a cleaner identification of how charging station availability influences adoption decisions without direct financial incentives distorting consumer behavior.

Second, location-specific data are available for Washington that enable a more precise estimation of local infrastructure effects than cross-state or cross-country studies. Due to limited data availability, most previous research has relied on broad geographic comparisons, which introduce confounding factors such as differences in state-level EV policies, electricity prices, and regional consumer preferences. These variations make it challenging to fully isolate the marginal effect of new charging stations on EV adoption. In contrast, Washington's detailed registration and infrastructure data allow for a county-level analysis, providing a clearer picture of how charging station expansion influences local adoption patterns across different settings and income levels.

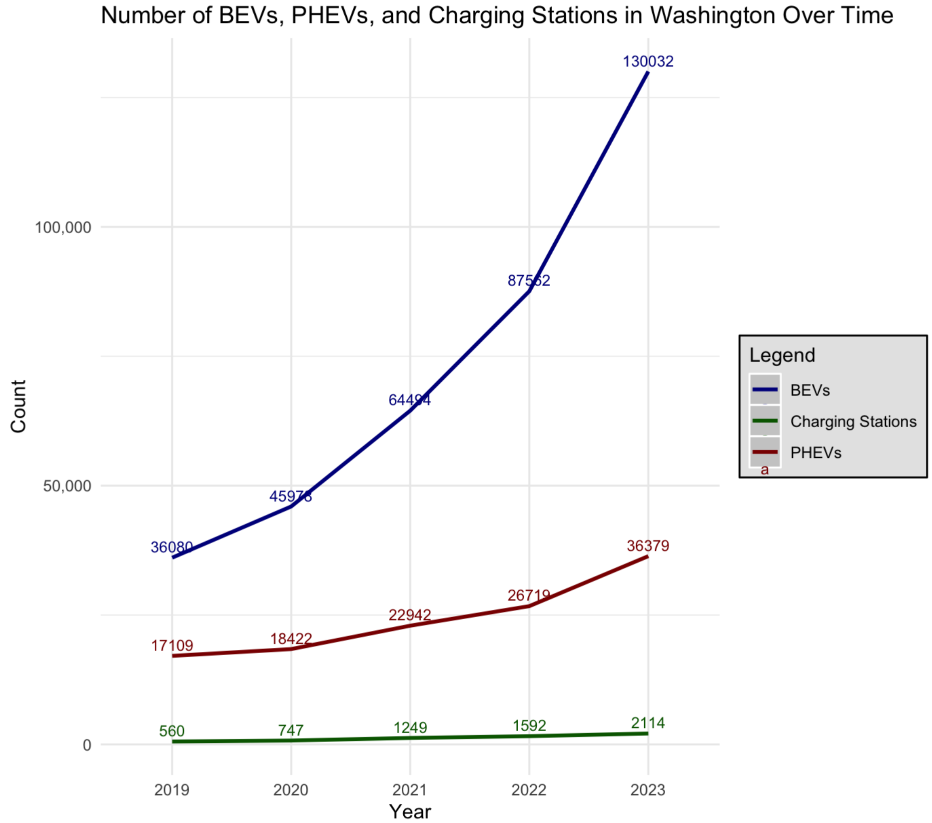
In 2022, Washington enacted Senate Bill 5974, which mandates that all new light-duty vehicles sold, purchased, or registered in the state be electric by the 2030 model year. A central aim of the legislation is equity. The transition is intended to be both rapid and just, with benefits distributed across communities and not only concentrated in higher-income areas. This paper directly informs that policy priority. While infrastructure expansion is often cited as a universally effective tool for accelerating EV uptake, it is not yet clear whether new public chargers generate equal adoption gains across income groups. By identifying how the marginal effect of charger deployment varies with local income and charger technology, the analysis provides evidence relevant for designing equitable, cost-effective infrastructure policies. Washington's new low-income EV rebate program, implemented in 2024, reflects growing attention to these distributional concerns, but it lies outside the study period and therefore does not affect the estimates presented here.

Finally, the study period (2019–2023) captures a critical phase in the maturation of the EV market, as charging technology improved, EV ranges increased, and infrastructure expanded. Previous studies have largely focused on nascent stages of the EV transition (Li et al., 2017; DeShazo et al., 2017; Sierzchula et al., 2014; van Dijk et al., 2022), when infrastructure availability was a more limiting factor. Analyzing a more recent period allows for a better understanding of contemporary adoption patterns and provides more relevant insights for policymakers planning future infrastructure investments.

Washington's EV market experienced substantial growth between 2019 and 2023, outpacing national trends in both vehicle adoption and charging infrastructure expansion. By 2023, EVs made up 2.2% of registered vehicles in Washington, nearly double the national average of 1.2%. This growth was driven primarily by the BEV market, which saw total registrations surge from 36,080 in 2019 to 130,032 in 2023, reflecting an average annual growth rate of 65.1%. In contrast, PHEV adoption grew at a slower pace, with total registrations rising from 17,109 to 36,379 over the same period, an annual growth rate of 25.1%. Growth trends in statewide EV adoption can be found in Figure 1.1.

Charging infrastructure expanded just as rapidly. In 2019, Washington had 560 public charging stations, a number that nearly quadrupled to 2,114 by 2023, averaging 69.3% in annual growth. This rapid increase in charging availability, alongside rising BEV adoption, makes Washington

Figure 1.1: EV and Charging Station Counts in WA

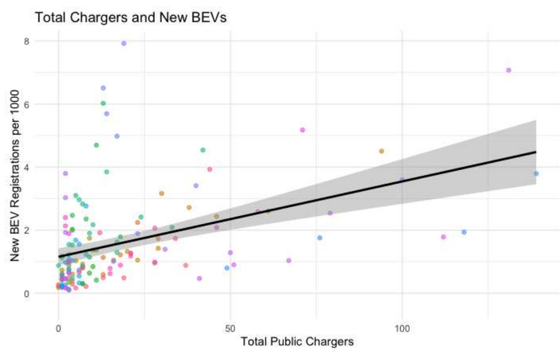


an ideal case study for examining how infrastructure expansion influences EV uptake over a time period when EVs are becoming more mainstream. To normalize the count of EVs and charging stations across counties, I create measures called EV Density and Charger Density, which are measured as the number of EVs or Chargers per 1000 residents in a given area.

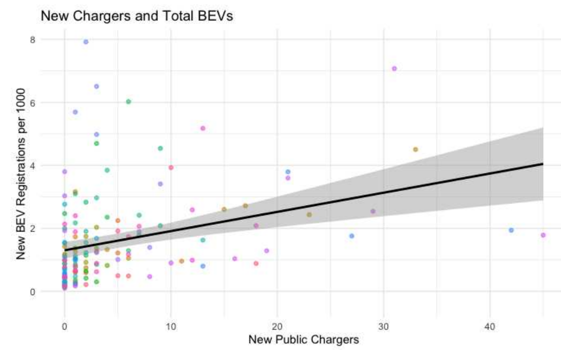
Figure 1.2 displays scatter plots with fitted linear trend lines illustrating the association between public charging infrastructure and BEV adoption. In both panels, the y-axis measures new BEV registrations per 1,000 residents. The left panel plots this outcome against the total stock of public chargers in each county-year, while the right panel plots it against the number of chargers added that year. Each point represents a county-year observation, and both panels show a positive relationship between charging infrastructure and new BEV registrations.

However, it's noteworthy that there are several counties that exhibit high EV adoption with relatively few chargers and vice versa. This variation suggests that other factors, such as income, education, familiarity with EV technology, or other factors may condition how communities respond to infrastructure investments.

Figure 1.2: Charging Infrastructure and BEV Adoption (Excluding King County)



(a) New BEV Registrations and Total Public Chargers



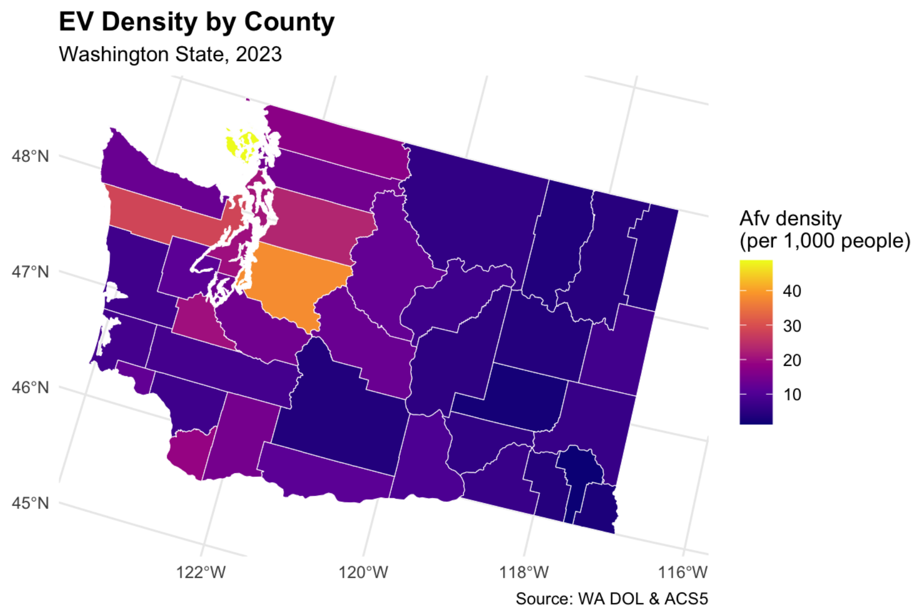
(b) New BEV Registrations and New Public Chargers

EV adoption varies widely across Washington State, as shown in Figure 1.3, which maps 2023 EV density. Generally, EV density is more concentrated on the western seaboard of the state, while the east of the state generally exhibits lower EV adoption on a per capita basis. San Juan County, a relatively small, wealthy group of islands off of the northwest coast of the state, exhibits the highest levels of EV registrations per capita with approximately 49 registered EVs per 1000 residents. King County, home of Seattle and many of its suburbs, also exhibits high levels of EV adoption, with approximately 40 registered EVs per 1000 residents.

On the other hand, smaller rural counties located in the eastern part of the state see relatively low levels of adoption. Garfield, Asotin, and Ferry, rural counties with low population density and few metropolitan areas, all see between 1-3 EV registrations per 1000 residents.

Similarly, EV charging stations are concentrated in certain areas of the state. The spatial distribution of public charging infrastructure can be found in Figures 1.4, with lighter shaded areas signifying a higher concentration of charging stations per capita. Similar to EV adoption, the group of islands that makes up San Juan County has over 1 charger per 1000 residents. Despite its high population relative to other counties, King County also contained a relatively high number of

Figure 1.3: EV Density Map of Washington



chargers per capita. Interestingly, the east-west divide in EV adoption does not exist to the same extent in terms of charging environment. Columbia County, located in the southeastern corner of the state has the second highest number of charging stations per capita, yet has one of the lowest EV registration rates in the state.

It's apparent that EV adoption varies significantly across counties, with the highest adopting counties exhibiting 15-30x more EVs per capita than counties with the lowest adoptions, but I also know that income plays a significant role in one's decision to buy an EV. Two patterns underscore the importance of income in shaping EV adoption.

Figure 1.5a shows the average number of new BEV registrations per year broken down by income quartile. BEV registrations are highly concentrated counties that are in the top quartile of average income. In fact, there are more average new BEV registrations in top quartile counties than counties in the bottom three quartiles combined. Second, Figure 1.5b shows a positive correlation between BEV registrations and log mean household income with each point representing a county-year. While these relationships are only correlations, they suggest that income remains a significant factor in adoption.

Figure 1.4: Charger Density Map of Washington

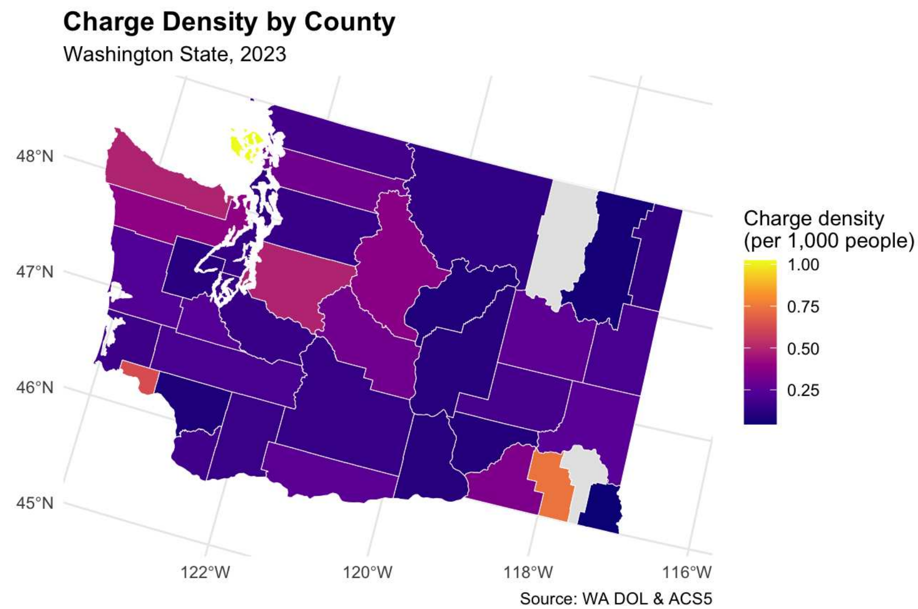
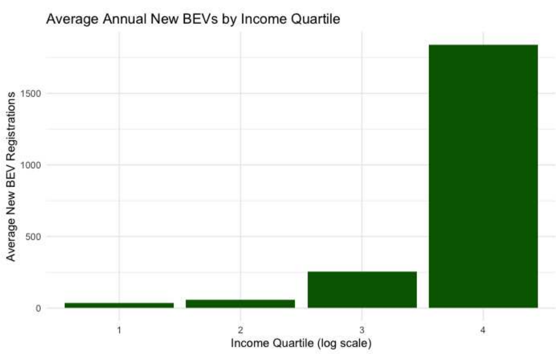
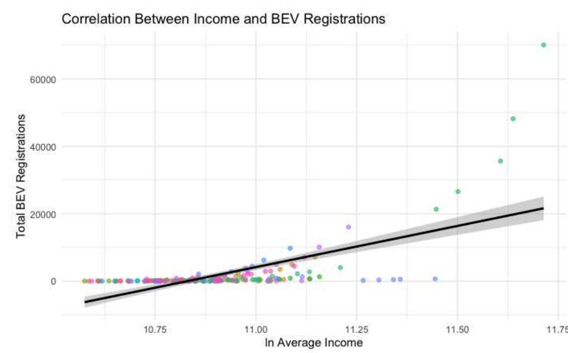


Figure 1.5: Income and EV Adoption Trends



(a) New BEV Registrations by Income Quartile



(b) Plot of Income and BEV Adoption

1.4 Methods

This analysis of the factors influencing EV adoption in Washington state examines two hypotheses:

- H1: The marginal effect of new public chargers on county-level EV adoption varies across income levels.
- H2: Fast chargers (DC Fast Chargers) have a greater effect on county-level EV adoption than slower charging stations (Level 1 or Level 2).

To test these hypotheses, I implement a two-way fixed effects panel framework using county-level data from Washington State between 2019 and 2023. This approach allows for the isolation of the effect of new charger installations on local BEV adoption, while accounting for unobserved heterogeneity across counties and time. I interact charger deployment with income to estimate differential effects across the income distribution and separately estimate marginal effects by charger type. This empirical strategy provides direct evidence on how infrastructure expansion and socioeconomic conditions jointly shape EV adoption.

A key contribution of this study is the use of contemporary panel data to isolate the role that charging infrastructure plays in EV adoption. Much of the prior literature relies on stated-preference surveys, experimental driving studies, or cross-sectional comparisons (Miele et al., 2020; Ghasri et al., 2019; Rainieri et al., 2023; Münzel et al., 2019; Lee and Nilsson, 2025; Singh et al., 2020), which provide insights into attitudes and perceived barriers to EV adoption, but offer limited external validity and cannot directly measure how adoption responds to infrastructure expansion over time. Several revealed-preference studies address endogeneity with IV strategies based on supermarket availability or parking supply, offering valuable insights into the causal relationship between charging and adoption. However, these instruments are highly sensitive to local conditions, and in this panel, they exhibit insignificant first-stage relationships, making them inappropriate to use in this setting (Li et al., 2017; van Dijk et al., 2022).

By leveraging a county-year panel with two-way fixed effects, this study estimates the marginal effect of new charger deployments while controlling for time-invariant local characteristics and common temporal shocks. This approach provides strong evidence on the causal role of public charging infrastructure and allows me to test for heterogeneity across income levels and charger types, offering policy-relevant insights into how infrastructure investments influence adoption dynamics across a variety of localities. While I do not claim to have solved the “chicken-vs-egg” problem that plagues so much EV research, robustness checks suggest that charging stations drive EV adoption, and not vice versa.

1.4.1 Empirical Analysis

To evaluate how the expansion of public charging infrastructure influences EV adoption over time, and how this effects varies across income levels and charger types, I estimate a panel fixed effects model using county-level registration data from 2019-2023. This approach is used to test Hypotheses 1 and 2: that the effect of new chargers varies across income levels, and that faster chargers have a greater impact on adoption than slower ones.

The dependent variable in the analysis is the number of new EV registrations per 1,000 residents in county i in year t, capturing the annual per capita rate of EV adoption. The key explanatory variables include the number of new public charging stations installed in county i in the previous year ($t-1$), the log of mean household income in county i, and an interaction term between the two variables. Control variables include the existing stock of public chargers (charger density), the average EV purchase price (measured by MSRP), and population density of county i. The model also incorporates county and year fixed effects.

The model specification can be found in Equation 1.1:

$$\begin{aligned}\text{NewEV}_{it} = \beta_0 + \beta_1 \log(\text{Income}_{it}) + \beta_2 \text{NewChargers}_{i,t-1} \\ + \beta_3 \left(\text{NewChargers}_{i,t-1} \times \log(\text{Income}_{it})\right) + \beta_4 \text{ChargerDensity}_{it} \\ + \beta_5 \text{PopDensity}_{it} + \beta_6 \text{AvgMSRP}_{it} + \mu_i + \sigma_t + \epsilon_{it}\end{aligned} \tag{1.1}$$

In this specification, NewEV_{it} denotes the number of new EV registrations per 1,000 residents in county i and year t. The term $\log(\text{Income}_{it})$ represents the natural logarithm of mean household income, and $\text{NewChargers}_{i,t-1}$ is the number of new public charging stations installed in the previous year. The interaction term $(\text{Charger} \times \log(\text{Income}_{it}))$ captures how the effect of new charger installations varies with local income levels. $\text{ChargerDensity}_{it}$ measures the stock of existing public charging stations per 1,000 residents, while PopDensity_{it} accounts for the number of residents per square kilometer in a county. Avg MSRP_{it} reflects the average manufacturer suggested retail price of EVs registered a given county, scaled in thousands of dollars. The model includes county fixed effects μ_i and year fixed effects σ_t to control for unobserved heterogeneity and common shocks across counties and years, respectively. The error term ϵ_{it} captures all other unobserved variation.

The interaction term between logged income and lagged new chargers captures how the effect of infrastructure varies by local income. A positive coefficient suggests that higher-income counties are more responsive to charger expansion.

To test Hypothesis 2, I estimate additional specifications that dis-aggregate charger types into Level 1, Level 2, and DC Fast Chargers. This allows for a direct comparison of how the effectiveness of charger investments varies by technology and demographic context.

To test Hypothesis 1, the parameters of interest are β_2 and β_3, the coefficient on the interaction between logged income and charger deployment. The null hypothesis is $H_0 : \beta_2 = \beta_3 = 0$. If I reject the null, this supports H1 by indicating that the local average income is a significant factor in the efficacy of new infrastructure on driving EV adoption. Hypothesis 2 is evaluated by comparing the estimated coefficients on charger deployment by type, specifically the logged income interactions for DC Fast Chargers versus Level 2 chargers. If the estimated logged income threshold (Y^*) at which charger effects become positive is lower for DCFC chargers than for Level 2 chargers, this provides supporting evidence for H2, suggesting that fast chargers are more effective at promoting adoption across a broader portion of the income distribution.

1.5 Data

This analysis employs data on electric vehicle registration records, charging infrastructure, and demographic characteristics for Washington State from 2019-2023. Washington provides an ideal setting for this research for two reasons. First, the Washington State Department of Licensing publishes detailed public registration data for both battery electric vehicles and plug-in hybrid electric vehicles, including geographic identifiers at the county-level for each year of the panel. Second, unlike many states, Washington did not offer any state or local EV rebates during the study period (2019–2023), allowing for cleaner identification of relationships between EV adoption, infrastructure, and local characteristics without policy-induced confounding factors.

The EV registration data used in this analysis are available via the Washington State Electric Vehicle Population data portal from the Washington State Department of Licensing. For the panel analysis, I use annual snapshots from 2019 to 2023, which report vehicle counts at the county-level as of December 31st each year.

Charging infrastructure data are sourced from the U.S. Department of Energy's Alternative Fuels Data Center. These data include the opening dates, geographic coordinates, number of charge points, and charger technology type of all public stations in Washington. Charging stations vary by technology: Level 1 chargers use standard 120-volt outlets and require 40 or more hours to deliver an 80% charge; Level 2 chargers can do so in 4–8 hours and are the most common form of public and workplace charging; Level 3 Direct Current Fast Chargers (DCFCs) are the fastest and most expensive, providing an 80% charge in under 30 minutes. More technical details on charging types can be found at the U.S. Department of Transportation's EV Toolkit.

County-level income data and annual population estimates are provided by the Washington State Office of Financial Management (OFM), the state demographer.

Summary statistics for the county-level panel dataset used in the analysis are presented in Table 1.1.

Table 1.1: Summary Statistics for Panel Data

Variable	Mean	Median	SD	Min	Max
New BEV density	1.705	1.159	1.636	0.097	9.622
New PHEV density	0.460	0.357	0.459	-0.249	2.099
log mean income	10.909	10.887	0.192	10.575	11.714
Charger density	0.163	0.121	0.170	0.000	1.023
Pop Density (n/km)	61.163	15.602	97.589	1.749	415.035
New BEVs	634.810	89.50	2295.384	2.0	21855.0
New Chargers	9.281	1.000	34.666	0.000	345.0

1.6 Results

The estimates from the fixed effect models can be found in Table 1.2. Columns (1) and (2) use BEV density (BEV registrations per 1000 residents) as the dependent variable, while columns (3) and (4) use PHEV density as the dependent variable. For BEV adoption the null, $H_0 : \beta_2 = \beta_3 = 0$ is rejected and thus H1 is affirmed. The marginal effect of new chargers on county-level BEV adoption varies with average income levels. Notably, the PHEV market does not show the same responses to expanded charging infrastructure as the BEV market does.

The results listed in columns (1) and (2) in Table 1.2 show that income is significant in BEV adoption but not PHEV adoption, with a 10% increase in income associated with a 1.06 - 1.20 increase in BEV density. This finding is consistent with previous studies that find BEVs are primarily purchased by high income households (Hardman et al., 2018). Population density does exhibit a positive relationship with BEV adoption, suggesting that rural areas are less likely to have high BEV adoption than urban areas.

The same effects are not observed in the PHEV market. Furthermore, the existing stock of charging stations ($ChargerDensity$) is not significant in PHEV adoption or BEV adoption in any specification of the model. Explanations as to why this may be the case are discussed in the next section of the paper.

Using the coefficients estimated in the model, the critical income level (Y^*) at which the expected marginal effect of new charging stations on BEV density becomes positive can be calculated.

Table 1.2: Regression Results for New BEVs and PHEVs per 1000 People

Variable	**BEVs**	**BEVs**	**PHEVs**	**PHEVs**
log Mean Income	12.001***	10.629***	2.086.	1.775
	(2.625)	(2.531)	(1.178)	(1.183)
$\text{Chargers}_{i,t-1}$	0.006*	-0.988***	0.001	-0.225
	(0.003)	(0.291)	(0.001)	(0.136)
$(\text{Chargers}_{i,t-1} \times Y_{it})$		0.085***		0.019
		(0.025)		(0.012)
Charger Density	1.550	1.020	0.601	0.481
	(1.078)	(1.038)	(0.484)	(0.485)
Population Density	147.482.	183.918*	43.136	51.407
	(74.610)	(71.822)	(33.486)	(33.577)
Avg MSRP ($1000s)	-0.142*	-0.154*	-0.014	-0.016
	(0.069)	(0.065)	(0.031)	(0.031)
County FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
R-Squared	0.292	0.365	0.072	0.097
F-statistic	8.499***	9.759***	1.608	1.821
*Y**	N/A	11.579	N/A	N/A

Significance codes: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, . $p < 0.1$

1.6.1 Marginal Effect and Critical Income Threshold (Y*)

The marginal effect of new chargers on BEV adoption depends on income, given by

$$\frac{\partial \text{BEV Adoption}}{\partial \text{Chargers}} = \beta_2 + \beta_3 \cdot \text{Income}. \tag{1.2}$$

Setting Equation 1.2 equal to zero yields the critical log-income threshold Y^* at which the marginal effect of a new charging station becomes positive:

$$Y^* = -\frac{\beta_2}{\beta_3}. \tag{1.3}$$

Using the coefficient estimates in Equation 1.3 ($\beta_2 = -0.9875$, $\beta_3 = 0.0853$), the implied log-income threshold is 11.579, equivalent to approximately $106,830. Below this threshold, expanding public charging infrastructure is not associated with an increase in local BEV adoption.

1.6.2 Effects of Different Charging Technologies

Using charging stations as a proxy for charging availability is useful and has been done by several previous studies (Li et al., 2017; Münzel et al., 2019; Lee and Nilsson, 2025). However, not all charging stations are powered by the same technology.

Charging stations vary both in the technology they use (Level 1, Level 2, or Level 3 DCFC), and the number of available charging points in a given location. For instance, one charging station may have 5 Level 2 charging points while a station across the street might have 10 DCFC charging points. The distribution of charging technologies and charging points are found in Table 1.3.

Table 1.3: Charging Station Summary by Charger Level

Technology	# of Stations	Avg. Points	Min	Max
Level 1	7	3.00	1	10
Level 2	2,152	2.32	1	68
DCFC	365	3.68	1	20

There is considerable variation between technologies and by the number of available charging points. Level 2 charging stations are by far the most common type of technology, with over 2,000 stations across the state. Each one of those stations has between 1 and 68 charging points, with the average station containing 2.3 charging points. DCFC charging stations are less common with only 365 stations in the state, but with a higher average number of charging points per station than Level 2 charging stations. Public Level 1 charging stations are uncommon, with only 7 stations in the state. This is not surprising, as the charging rates of Level 1 stations are too low to provide much utility to someone reliant on public charging stations to fuel their car.

To account for differences in technology and number of charging points, I run an identical analysis differentiating by charging technology and by counting the total number of charging points, not simply the total number of charging stations. Using the same lagged variable and interaction variable approach used in the previous specifications, I differentiate the marginal effect of installing an additional Level 1, Level 2, or DCFC charging point. Results are listed in Table 1.4

Table 1.4: TWFE Results for New BEV Density by Charging Type

Variables	**Level 1**	**Level 2**	**DC Fast**
Lagged L1	6.598 (17.445)		
Income × L1	-0.574 (1.544)		
Lagged L2		-0.450*** (0.116)	
Income × L2		0.039*** (0.010)	
Lagged DC			-0.999** (0.304)
Income × DC			0.088** (0.027)
Log Mean Income	12.759*** (2.693)	10.754*** (2.502)	10.599*** (2.617)
Charger Density	1.843 (1.114)	0.942 (1.031)	1.422 (1.052)
Average MSRP	-0.150* (0.071)	-0.152* (0.065)	-0.157* (0.067)
Population Density	149.063. (76.509)	159.363* (70.509)	114.702 (73.670)
County FE	✓	✓	✓
Year FE	✓	✓	✓
R-Squared	0.264	0.375	0.334
F-statistic	6.112***	10.198***	8.507***
Observations (N)	148	148	148
Y*	N/A	11.538	11.338

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, . $p < 0.1$

The results presented in 1.4 confirm H2. For county-level BEV adoption, DC Fast Charging stations have a greater effect on adoption than Level 1 or Level 2 chargers. I find meaningful differences across charger technologies in terms of the income threshold at which charger deployment begins to have a positive effect on EV adoption. The estimated Y^* is lower for Direct Current Fast Charging charging points (11.338) than for Level 2 chargers (11.538). These values equate to approximately $84,000 and $104,600 respectively. The difference in value suggests that fast chargers are effective at promoting adoption at lower income levels compared to slower alternatives. However, the overall responsiveness to infrastructure remains greater in higher-income counties. The positive coefficient on the interaction term between log-income and charger deployment across all charger types indicates that infrastructure expansion consistently delivers larger adoption gains in wealthier areas.

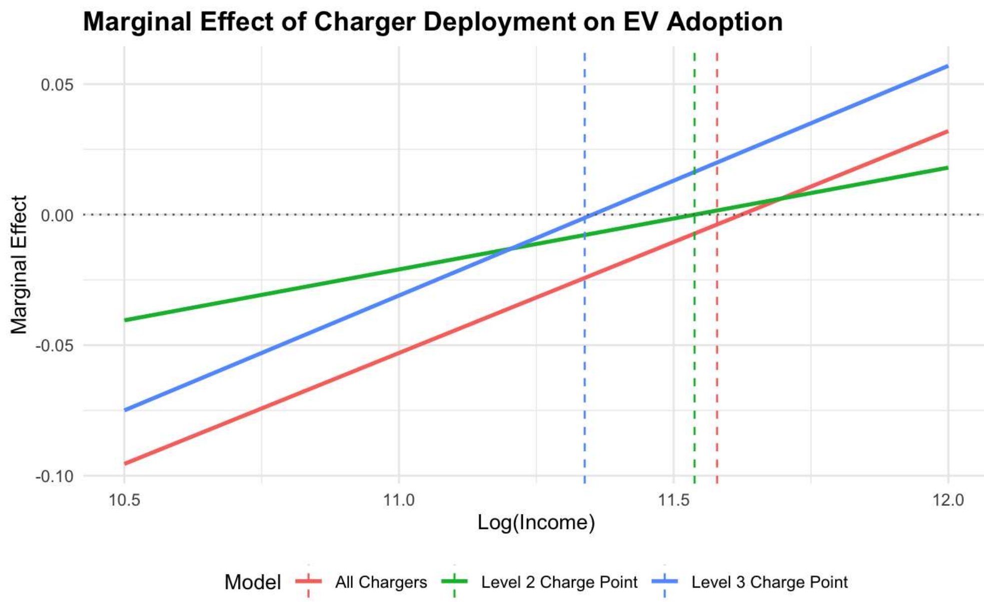
Similar to the baseline and truncated versions of the model, new charging stations have a heterogeneous effect across income levels. In column (1), Level 1 charging points are shown to have zero significant relationship with EV adoption. This is to be expected as there are few Level 1 public chargers in the sample, charging a BEV to full charge using a Level 1 charger can take well over a day, and is not a practical way to stay fully charged, especially on long drives.

In columns (2) and (3), the lagged charging point variables for both Level 2 and DCFC charging points show the same pattern across income levels as the baseline model. The lagged charging point variables are both significantly negative, while the interaction variables with log-income are significantly positive. These coefficients are then used to calculate the Y^* values listed on the bottom row of Table 1.4. The Y^* value for a Level 2 charging point is about 11.538 or $\approx$ \$102,540, while the Y^* value for DCFC charging points is about 11.338 or $\approx$ \$83,950.

Figure 1.6 depicts how the marginal effects of new charging stations vary with income levels. The graph reveals the threshold income levels Y^* values for each specification, with Level 3 DCFC charging points associated with positive marginal effects at a lower income threshold than either Level 2 charging points or all charging stations combined. Additionally, the steeper slope of the Level 3 model indicates that fast charging infrastructure is more strongly associated with increased EV adoption across the income distribution than slower alternatives.

The lower Y^* value for DCFC charging points indicates that these chargers are associated with positive adoption effects at lower average income levels compared to Level 2 chargers. This suggests that faster charging technology is more effective at encouraging EV adoption across a broader portion of the population.

Figure 1.6: Marginal Effects Across Specifications



1.6.3 Dropping King County

The baseline results show that wealthy areas are most responsive to new charging stations, so I perform the analysis again excluding the largest and wealthiest county in Washington: King County. Home county for the city of Seattle and many of its suburbs, King County is an outlier in terms of BEV registrations, number of charging stations, income, and population density. By excluding it from the analysis, I can more confidently identify broader dynamics in BEV adoption across the state to test the robustness of these adoption patterns.

The results of the analysis excluding King County are found in Table 1.5. The same pattern is found across the baseline analysis and the analysis omitting King County. Again, income is significantly positive in BEV adoption, however the magnitude of the effect is significantly smaller in the King-less sample. Again, the existing stock of charging infrastructure is insignificant in new BEV registrations.

Unlike the baseline model, the analysis without King county did not find a significant relationship between lagged charging stations and new BEV density in the model specification without the interaction variable. However, in the specification with the interaction variable, I find the same heterogeneous response across income levels, with a Y^* estimate of 11.05, or approximately \$63,810. This threshold is significantly lower than that of the baseline specification, therefore, while the inclusion of King County may have inflated the initial Y^* estimate, the heterogeneous response to expanded charging infrastructure persists in rural counties.

The marginal effects of specifications with and without King County are shown in Figure 1.7.

Figure 1.7: Marginal Effects with and without King County

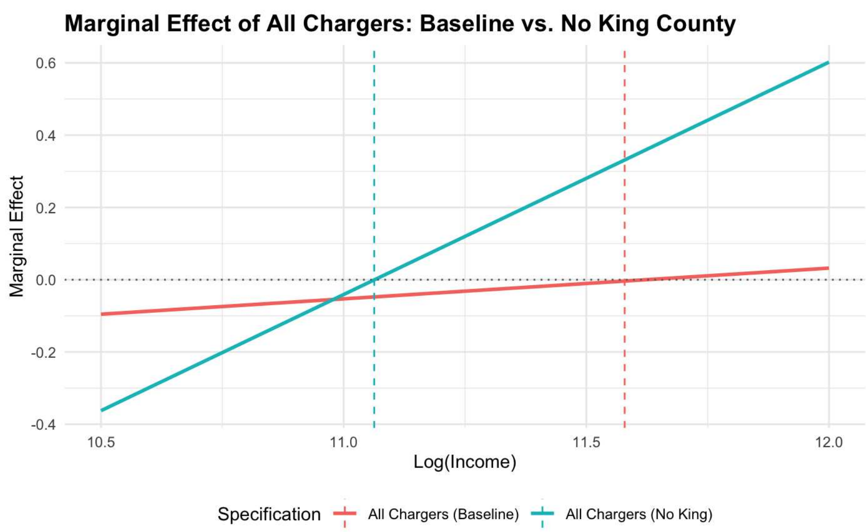


Table 1.5: Regression Results for BEVs per 1000 People Excluding King County

Variable	**New BEV Density**	**New BEV Density**
log Mean Income	9.572***	5.968*
	(2.379)	(2.274)
Chargers_{t-1}	0.023	-7.114***
	(0.018)	(1.472)
(Chargers_{t-1} x Income)		0.643***
		(0.133)
Charger Density	0.482	0.438
	(0.980)	(0.886)
Population Density	138.494.	-9.463
	(73.924)	(73.437)
Avg MSRP ($1000s)	-0.126*	-0.158**
	(0.061)	(0.056)
County FE	✓	✓
Year FE	✓	✓
R-Squared	0.238	0.384
F-statistic	6.233***	10.279***
Y*	N/A	11.063

Significance codes: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, . $p < 0.1$

1.7 Robustness Checks

Three robustness checks were performed to test the validity of the estimates. Across all sets of robustness checks—reverse causality, placebo outcomes, and lagged specifications—the results consistently support the results of the baseline model. The models testing for reverse-causality show that lagged BEV registrations do not predict subsequent charger deployment. The placebo tests using PHEV registrations as the outcome likewise yield null results, reinforcing that the BEV effects are not artifacts of broader EV market dynamics or unobserved demand shocks. Finally, the lagged models indicate that the effect of charging infrastructure on BEV adoption strengthens with time: a three-year lag produces significantly positive coefficients, while the income interaction remains robust, with a lower Y^* threshold than in shorter lags. These checks provide evidence that the relationship between charging infrastructure, income, and BEV adoption is robust.

1.7.1 Reverse Causality

To assess whether infrastructure follows adoption (rather than the other way around), I estimate a reversed specification in which the dependent variable is the number of new public charging points and the key regressor is lagged BEV registrations per 1,000.

Table 1.6 reports the baseline model and the version excluding King County. Across both, the coefficient on lagged BEV adoption is small and statistically insignificant, and overall model fit is low. Excluding King County does not change this conclusion: lagged BEV adoption does not predict subsequent charger deployment. The only notable associations are with siting—charger stock density and population density are positively related to new chargers when King County is excluded—consistent with infrastructure being placed where networks already exist and where population density is highest.

Table 1.6: Reverse Causality: New Chargers as Dependent Variable (TWFE)

	(1) Baseline	(2) Excl. King County
log Mean Income	29.124	21.227
	(83.426)	(17.461)
Lagged New BEV Density	-1.662	-0.224
	(1.729)	(0.364)
Charger Density	36.081	15.613*
	(36.299)	(7.616)
Population Density	789.989	1581.778***
	(1619.896)	(339.073)
BEV Density	0.984	-0.345
	(1.053)	(0.234)
County FE	✓	✓
Year FE	✓	✓
Observations	147	143
R-squared	0.055	0.208
Adj. R-squared	-0.353	-0.136
F-statistic	1.190	5.191***

Two-way fixed effects (county & year); within estimator. Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I also estimate the reversed model by charger type (Level 1, Level 2, DCFC), the results can be found in Table 1.7. Again, lagged BEV adoption does not predict new points of any type.

Table 1.7: Reverse Causality by Charger Type: New Charging Points as Dependent Variable (TWFE, Baseline)

	(1) Level 1 Points	(2) Level 2 Points	(3) DC Fast Points
log Mean Income	-0.962	28.308	40.924
	(0.731)	(135.250)	(30.495)
Lagged New BEV Density	0.008	-3.038	0.167
	(0.015)	(2.803)	(0.632)
Charger Density	0.296	45.645	0.701
	(0.318)	(58.848)	(13.269)
Population Density	-1.017	604.122	228.160
	(14.194)	(2626.172)	(592.123)
BEV Density	0.009	2.460	-0.227
	(0.009)	(1.707)	(0.385)
County FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	147	147	147
R-squared	0.051	0.066	0.020
Adj. R-squared	-0.359	-0.337	-0.403
F-statistic	1.092	1.432	0.418

Two-way fixed effects (county & year); within estimator. Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

These results reduce concern that the main BEV results are driven by adoption driving charger expansion. Instead, they support the interpretation that the primary direction runs from charger availability to higher BEV adoption, not the reverse, albeit primarily in wealthy areas.

1.7.2 Placebo Tests: PHEV Market

As an additional robustness exercise, I conduct a placebo test by re-estimating the adoption models with PHEV registrations per 1,000 residents as the dependent variable. Because PHEVs can rely on gasoline and are far less dependent on public charging infrastructure, the expectation is that charger deployment should have little or no effect on PHEV uptake. Evidence of strong charger–PHEV relationships would call into question whether the BEV results simply reflect general EV market growth rather than a causal link between public charging and BEV adoption.

Table 1.8 presents results for the baseline and for the sample excluding King County. Coefficients on lagged chargers and the charger–income interaction are small, unstable in sign, and statistically insignificant.

Table 1.9 breaks the placebo test down by charger type. Across Level 1, Level 2, and DC fast chargers, the estimated effects of lagged charger deployment insignificant, with no systematic pattern emerging. The placebo results confirm that the main findings for BEVs are not simply an artifact of unobserved demand shocks or general pro-EV trends that would also boost PHEVs. Instead, they underscore the unique dependence of BEV adoption on the availability of public charging infrastructure.

Table 1.8: Placebo Outcome Test: PHEV Adoption and Public Charging (TWFE)

	(1) Baseline	(2) Excl. King County
log Mean Income	1.775	1.001
	(1.183)	(1.238)
Lagged New Chargers	-0.225	-0.799
	(0.136)	(0.801)
Charger × Income	0.019	0.073
	(0.012)	(0.072)
Charger Density	0.481	0.301
	(0.485)	(0.482)
Population Density	51.407	30.981
	(33.577)	(39.961)
Average MSRP	-0.016	-0.012
	(0.031)	(0.030)
County FE	✓	✓
Year FE	✓	✓
Observations	148	144
R-squared	0.097	0.059
Adj. R-squared	-0.302	-0.359
F-statistic	1.821	1.042

Two-way FE (county & year); within estimator. Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.9: Placebo Outcome Test by Charger Type: PHEV Adoption (TWFE, Baseline)

	(1) Level 1 Points	(2) Level 2 Points	(3) DC Fast Points
log Mean Income	2.324*	1.851	1.850
	(1.162)	(1.183)	(1.205)
Lagged Chargers	6.950	-0.081	-0.151
	(7.525)	(0.055)	(0.140)
Charger × Income	-0.638	0.007	0.013
	(0.666)	(0.005)	(0.012)
Charger Density	0.835	0.486	0.581
	(0.481)	(0.487)	(0.484)
Population Density	44.042	45.234	37.603
	(33.002)	(33.338)	(33.926)
Average MSRP	-0.014	-0.015	-0.016
	(0.030)	(0.031)	(0.031)
County FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	148	148	148
R-squared	0.110	0.091	0.081
Adj. R-squared	-0.283	-0.310	-0.325
F-statistic	2.097	1.702	1.490

Two-way FE (county & year); within estimator. Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

1.7.3 Lagged Models

To more fully understand the role of timing in charging infrastructure and EV adoption, I include multiple lagged variables and their interaction in Table 1.10. Four specifications assess robustness of baseline results and regional heterogeneity. In columns (1) and (2), I run the analysis on the full sample with and without the interaction variables between lagged new chargers and income. In columns (3) and (4), I run an identical analysis excluding King County to capture the dynamics of the EV market in smaller rural counties.

Table 1.10: Regression Results for BEV Adoption with Charger Lags and Income Interaction

	Full Sample		**Excluding King County**	
	(1) Base	(2) Interaction	(3) Base	(4) Interaction
log(mean income)	7.290**	6.698**	6.387**	5.212*
	(2.169)	(2.115)	(2.066)	(2.047)
Lag Chargers t-1	-0.005	0.023	-0.007	-0.532
	(0.003)	(0.344)	(0.016)	(1.918)
Income × Chargers t-1		-0.002		0.047
		(0.030)		(0.173)
Lag Chargers t-2	0.007**	1.104**	0.072***	0.305
	(0.003)	(0.364)	(0.019)	(1.721)
Income × Chargers t-2		-0.093**		-0.023
		(0.031)		(0.155)
Lag Chargers t-3	0.096***	-0.021	0.106***	-5.061**
	(0.021)	(0.771)	(0.024)	(1.647)
Income × Chargers t-3		0.010		0.466**
		(0.069)		(0.149)
Charge Density	0.535	0.491	0.689	0.759
	(0.869)	(0.848)	(0.830)	(0.795)
Average MSRP	-0.124*	-0.143**	-0.145**	-0.152**
	(0.054)	(0.053)	(0.052)	(0.050)
Population Density	102.490.	24.251	-44.910	-28.932
	(59.510)	(69.353)	(69.627)	(70.310)
County FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	148	148	144	144
R-squared	0.564	0.603	0.468	0.528
Adj. R-squared	0.366	0.404	0.224	0.290
Y^*	N/A	11.871†	N/A	10.861

Standard errors in parentheses.

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, . $p < 0.1$

† This value indicates the income at which marginal chargers stop having a positive impact on BEV adoption. The implied income level ($143,052) exceeds that of every county in the sample, implying positive marginal effects in all counties.

The inclusion of additional lagged charger variables reveals further complexity in regional responses to expanding charging infrastructure. Chargers installed one year prior (lag 1) show insignificant effects across all models. However, chargers installed two and three years prior (lags 2 and 3) yield significant results. Columns (1) and (2) show positive, significant impacts from chargers installed two and three years prior, indicating a meaningful delayed effect from charging infrastructure on EV adoption. This finding aligns with evidence that charging infrastructure has stronger and growing effects in areas with limited baseline access (Springel, 2021; Haidar and Rojas, 2022; Narassimhan and Johnson, 2018; Burra et al., 2024).

Introducing interaction terms between lagged charger installations and income adds further complexity. The two-year lagged variable demonstrates a strong positive effect in column (1), but a significantly negative interaction term appears in column (2), opposite to the income effect observed in the baseline model. While the sign of the interaction term reverses, the coefficient estimates can still be used to calculate Y^*, though its interpretation is also reversed. In practice, the interpretation is not meaningful because $Y^* = 11.71$, which corresponds to an average income of more than $140,000, and no counties in the sample exhibit income levels that high. As a result, the estimated marginal effect of two-year lagged charging station installations is positive in every county in the sample.

This anomalous pattern disappears in columns (3) and (4) in the models excluding King County. In column (3), both the two- and three-year lags are significantly positive for BEV adoption. However, in column (4), only the three-year lag and the interaction term are statistically significant. These estimates allow for the calculation of a Y^* threshold income level at which the marginal effect of a new charging station becomes positive. Using the coefficient estimates from column (4), the estimated Y^* is approximately 10.861, corresponding to $52,078.89. Combined with the insignificance of the one- and two-year lags, this pattern suggests that the largest benefits of expanding charging infrastructure may not be realized until several years after deployment.

Excluding King County significantly alters the results of the models, highlighting the disproportionate influence of this large metropolitan area on estimates. The change in estimates may reflect that, outside highly urbanized the wealthy metropolitan areas, charger installations have more substantial long-term impacts, particularly in moderately affluent counties where public charging infrastructure is less saturated and there are likely fewer private charging options. The inclusion of King County appears to mask these regional variations by potentially introducing saturation effects due it its comparatively well-developed charger network. While the unexpected negative interactions observed in columns (1) and (2) may initially appear counterintuitive, plausible explanations such as market saturation in wealthier areas, alternative private charging options, or statistical noise due to sample size may explain these anomalies. Importantly, these wrinkles do not undermine the core takeaway that charging infrastructure significantly impacts EV adoption, although the impact varies meaningfully by county characteristics.

1.8 Discussion & Policy Implications

These results show that new public chargers are associated with increased BEV adoption, but these gains are concentrated in higher-income counties. The interaction terms indicate that responsiveness to new chargers rises with local income, and the disaggregated estimates show that DC fast chargers generate positive effects at lower income thresholds than Level 1 or Level 2 chargers.

These patterns suggest that infrastructure expansion alone is unlikely to produce widespread adoption in lower-income areas, where financial constraints and limited access to home charging may outweigh concerns over charging availability. For states that have made equity and environmental justice central to transportation decarbonization, such as Washington, these findings show that charger deployment is not a uniformly effective policy lever. Infrastructure investments will lead to disproportionately high adoption in higher-income areas unless paired with policies that directly address affordability and access barriers. This conclusion is supported by findings from the incentive literature, where income-blind rebate programs have been shown to deliver most benefits to higher-income households, even when the policy goal is broader equity (Tal and Nicholas, 2016; DeShazo et al., 2017; Sheldon and Dua, 2019; Jenn et al., 2018; Clinton and Steinberg, 2019; Sheldon, 2022; Connolly et al., 2025).

The results of this study also partially support Li et al. (2017), who conclude that public charging investments are more cost-effective than direct purchase incentives at driving EV adoption.

However, results of this study also show that charging infrastructure alone is insufficient to increase EV adoption in lower-income counties. To be more effective in encouraging EV adoption in these counties, infrastructure expansion should be complemented by income-targeted rebates, financing support, or programs that improve access to workplace or multifamily charging.

Finally, the result that DC fast chargers generate adoption gains at lower income levels than slower chargers indicates that the type of charger deployed matters for EV adoption in low-income counties. Prioritizing fast-charging stations, particularly in lower-income areas, places with limited off-street parking, or long commuter flows, may yield more equitable adoption outcomes than uniform deployment strategies that prioritize slower, cheaper chargers.

The finding that the critical income threshold (Y^*) decreases once King County is excluded from the sample indicates that public charging infrastructure is largely effective across a broader income range outside the state's most urbanized and charging infrastructure-dense county. In other words, the muted effects in King County appear to reflect local conditions, such as congestion, reliability issues, or charger saturation, rather than a statewide pattern. This result suggests that a single statewide deployment strategy is less effective in encouraging EV adoption, and that policymakers should consider region-specific approaches that account for differences in income, population density, commuting patterns, housing stock, and baseline EV adoption.

The reverse-causality tests further reinforce the plausibility of a directional effect running from chargers to BEV adoption. Across specifications, new BEV registrations are not significant in subsequent charger deployment, while the main models consistently show that charger expansion leads to higher BEV uptake. The placebo tests using PHEVs provide additional credibility: because PHEVs are not dependent on public charging, the absence of statistically significant effects is expected. That the PHEV models yield no meaningful relationship suggests that the BEV results are not simply artifacts of broader EV market growth or county-level trends but instead reflect the specific importance of public charging availability for fully electric vehicles.

The lagged-effects models show that the strongest adoption responses occur two to three years after charger installation, with minimal immediate effects. This pattern indicates that EV adoption adjusts gradually to infrastructure improvements, likely as information diffuses, households update expectations about convenience and reliability, and vehicle turnover occurs. For policymakers, these dynamics underscore the importance of a long-run perspective: infrastructure programs may take several years to generate observable effects, and short-term evaluations risk understating their true impact. Importantly, the conditioning effect of income is present even in three years after installation.

In the specification excluding King County and incorporating multiple lags, only the three-year lagged charger variable remains significantly associated with increased BEV adoption. The interaction between income and this three-year lag is also positive and significant, mirroring the heterogeneous income patterns seen in the baseline results. Importantly, the estimated income threshold ($Y^* = 10.86$) is lower than in other specifications, suggesting that over a longer time horizon, a wider range of income levels becomes responsive to new charging infrastructure.

These results are broadly consistent with prior evidence showing that public charging infrastructure supports higher BEV adoption (Sierzchula et al., 2014; Narassimhan and Johnson, 2018; Egnér and Trosvik, 2018; Qian et al., 2019; Ghasri et al., 2019; Springel, 2021; Haidar and Rojas, 2022; Burra et al., 2024). The observed lagged effects, with adoption responding two to three years after installation, also align with work emphasizing that infrastructure effects materialize gradually as information spreads and households adjust expectations (Springel, 2021; Haidar and Rojas, 2022; Schulz and Rode, 2022).

In addition, these findings help explain why previous studies report mixed evidence on the role of charging infrastructure (Muratori et al., 2020; Miele et al., 2020). Results show that the effectiveness of new chargers depends on local income, with high-income counties responding more to deployment than lower-income ones. This heterogeneity adds nuance to studies such as Li et al. (2017), which argues that reallocating subsidy funds toward charging infrastructure would be more cost-effective than direct purchase incentives. Results of this study show that this conclusion does not hold uniformly: infrastructure expansion generates sizeable adoption gains in high-income areas but has limited to no impact in low-income counties. The findings also extend charger-type comparisons by showing that DC fast chargers lower the income threshold at which infrastructure becomes effective and align with emerging evidence that reliability issues in dense, urban counties can dampen adoption responses (Yu et al., 2025).

1.9 Limitations

Several limitations of this study should be acknowledged. First, the analysis relies on the U.S. Department of Energy's Alternative Fuels Data Center database, which reports the location and type of publicly accessible chargers but does not systematically track charger functionality or utilization. This omission is consequential because recent evidence shows that the reliability and accessibility of public charging infrastructure vary across locations. Yu et al. (2025) document, using detailed operational data from California charging networks, that chargers in dense urban areas exhibit higher failure rates, lower accessibility, and lower operational reliability than chargers in suburban and rural communities. If a similar pattern holds in Washington, where King County contains a large share of the state's urban charging infrastructure, then AFDC data may overstate true charging availability in the most urbanized parts of the sample. This provides a plausible explanation for why the marginal effect of new chargers is positive at a lower income threshold once King County is removed from the sample. If chargers in King County are systematically less reliable than those elsewhere, then increases in charging infrastructure might be expected to have a weaker effect on local BEV adoption. Unfortunately, the reliability data required to validate this hypothesis are not publicly available outside of California. Future research should incorporate charger-level reliability and uptime data to determine whether uneven operational performance, particularly in urban areas, contributes to the heterogeneous effects observed here.

Second, although the analysis cannot include every factor that has been linked to EV adoption, the two-way fixed effects framework helps accounts for stable county characteristics such as political affiliation, renter share, and racial demographics. Given the short 2019–2023 study period, these variables do not meaningfully vary year-to-year at the county level, and annual data are not available; most would need to be drawn from the ACS 5-year estimates, which provide only a single observation for this timeframe. As a result, incorporating them directly into a fixed effect specification is not feasible. However, their largely time-invariant nature means they are mostly captured by the fixed effects.

Third, while the panel structure improves inference relative to cross-sectional designs, the estimates should not be interpreted as fully causal. The instrumental-variable strategies used in prior work (Li et al., 2017; van Dijk et al., 2022) were found to be not valid in this setting. Recently, He et al. (2025) found urban density interacted with local river density to be a valid instrument in the Chinese EV market, however this was not tested in the Washington context. Given the absence of a valid instrument, it is not possible to isolate truly exogenous changes in charging infrastructure, and the results should therefore be interpreted as conditional correlations rather than causal effects. However, the fact that the results are significant only in one direction (charging infrastructure is associated with BEV adoption, but not vice versa) provides evidence of a directional relationship consistent with a causal mechanism. Future research that incorporates stronger exogenous sources of variation would enable more causal identification of infrastructure effects.

Finally, the analysis is conducted at the county level, which aggregates away local heterogeneity in housing stock, commuting patterns, charger accessibility, and neighborhood socioeconomic composition. Although county fixed effects capture time-invariant differences across places, within-county variation remains unobserved. Prior research shows that EV adoption and charger utilization are sensitive to small changes in spatial patterns, which are not fully captured in this study. Future work that leverages more granular registration data, charger-level uptime records, or operator-reported utilization rates would allow for a more precise assessment of how public charging infrastructure interacts with local characteristics and built-environment constraints.

1.10 Conclusion

The results of this study show that the marginal effect of new public chargers on county-level EV adoption varies across income levels, and that fast chargers have a greater effect on county-level EV adoption than slower charging stations. Increasing the number of public charging stations in a given county is effective at increasing BEV adoption in high income counties and ineffective in low-income counties. These results are consistent in estimations that both include and exclude King County.

Together, these results suggest that infrastructure expansion alone is not sufficient for increasing EV adoption. While public chargers play a crucial role in mitigating concerns about range anxiety, policymakers must recognize that their effectiveness varies across income groups and charger technologies. By refining charging strategies and pairing infrastructure expansion with complementary policies, governments can design more effective interventions to accelerate EV adoption in a just and equitable manner.

This paper highlights the heterogeneous effects of charging infrastructure on BEV adoption across income groups. However, further research is needed to more precisely identify what is driving these disparities and how they can be addressed. Future research should investigate how financial barriers, behavioral biases, and differences in home charging access factor into the differences across average income levels. Similarly, examining how vehicle financing programs, used EV markets, or workplace charging availability interact with public charging infrastructure access would provide a better understanding of the constraints on EV adoption in low-income areas. Finally, long-term studies of changes in consumers' behavior and the EV market are necessary to ensure that EV policies are both effective and equitable.

Chapter 2

Long Term Effects of Large Wildfires on Tourism Spending in Colorado

Reflecting trends across the western United states and globally, wildfires in Colorado have become bigger, more frequent and highly destructive. Yet, to date, there have been few studies of the long-term impacts of these mega-fires on local economies dependent on forest-related tourism. This paper examines the economic consequences of these mega-fires for tourism-dependent communities in Colorado using county-level tourism spending data from 1996 to 2019. A staggered difference-in-differences framework is employed to estimate the effects of wildfire exposure across counties experiencing fires of varying sizes. The results indicate that the most extreme events, fires exceeding 30,000 acres, generate statistically significant and persistent declines in tourism expenditures and related state and county tax receipts. These reductions are primarily driven by reductions in spending on commercial lodging. The largest fires cause the most substantial economic disruptions, underscoring the need for policymakers to invest in wildfire mitigation strategies and long-term economic support for affected communities.

2.1 Introduction

Since the 1990s, as a result of climate change and extreme drought, wildfires in the western United States have become more frequent and more severe (Bayham et al., 2022). The 2018 Camp Fire in Northern California resulted in the destruction of over 18,000 structures, burned over 150,000 acres of land, caused over $15 billion in damages, displaced over 50,000 people, and resulted in 85 fatalities (California Air Resources Board, 2020). The 2023 Maui wildfires were similarly destructive, destroying over 2000 structures, killing over 100 people, and causing $5.5 billion dollars in damages (U.S. Fire Administration, 2024). In 2025, a series of wildfires across

Southern California displaced over 200,000 people destroyed over 18,000 structures, and resulted in the deaths of 29 people (Los Angeles County Economic Development Corporation, 2025).

While the economic and social consequences of wildfires are wide ranging, they are uniquely problematic for local communities reliant on tourism and outdoor recreation. Wildfires typically occur on public land, and "wildfire season" coincides with peak outdoor recreation season. The conflict between tourism and wildfire is acutely problematic for Western states like California, Colorado, Utah, Oregon, Idaho, and Washington as the forest amenities that attract tourists to the region (e.g. scenic forest trails, remote cabins, campgrounds) are increasingly vulnerable to wildfires. The goal of this paper is to examine the effects of large wildfires on local tourism spending. Specifically, this paper addresses the research question: how do wildfires affect regional tourism-related economic activity, and what are the long-term economic consequences for local communities?

Colorado provides a representative western setting for this analysis, as large wildfires have become more frequent while tourism spending, totaling tens of billions of dollars annually, remains central to many local economies. Wildfire trends in Colorado mirror broader patterns across the western United States, where the frequency and area burned by large fires have increased significantly since the 1980s (Westerling, 2016; Dennison et al., 2014). Across western forests, fire seasons have lengthened substantially and annual burned area has risen markedly in recent decades (Wasserman, 2020; Wasserman and Mueller, 2023). Colorado has experienced similar trends, including a pronounced increase in exceptionally large fires. At the same time, tourism and outdoor recreation constitute major economic drivers for many Colorado counties, particularly in forested and mountainous regions where wildfire risk is high.

These trends extend beyond the western United States. Across much of North America, wildfire activity has intensified in recent decades. In Canada, four of the most severe wildfire seasons of the past century have occurred within the last seven years (Parisien et al., 2023), and the 2023 season was unprecedented in total area burned, exceeding previous records by nearly two million hectares (Pelletier et al., 2024). Similar increases in large wildfire occurrence and total area burned have also been documented in the eastern United States, where a majority of assessed ecoregions exhibit upward trends in large fire activity (Donovan et al., 2023). Together, this evidence indicates that the escalation of large wildfire events observed in Colorado reflects a broader continental shift in fire regimes rather than an isolated state-level phenomenon.

This research contributes to a growing literature on the economic impacts of wildfires by addressing two notable gaps. Botzen et al. (2019) identify failure to account for regional economic impacts as a major shortcoming of most previous studies on natural hazards. Furthermore, Bayham et al. (2022) notes that there is sparse research on the impact of wildfire on recreational activities, and that most existing studies rely on survey data in relatively small selected locations. This research addresses those gaps in two key ways. First, it estimates the average regional effects of large wildfires on tourism spending across Colorado from 1996 to 2019, rather than focusing on the impacts of individual fires in a specific region or location. Second, by using data from the Colorado Office of Tourism on county-level tourism expenditure, this paper provides a more robust analysis than studies that rely solely on survey data from a single location impacted by a wildfire.

This research is pertinent to rural communities in Colorado that rely on tourism spending to maintain a healthy economy, as peak wildfire season often coincides with peak tourism season in the summer months. Counties affected by large wildfires are expected to experience declines in tourism spending and related tax revenues due to the degradation of forest ecosystem services and recreational opportunities. A staggered difference-in-differences approach is used to estimate the average treatment effects of counties affected by wildfires of varying sizes. The findings indicate that the occurrence of a "mega-fire" of at least 30,000 acres has significant and persistent negative effects on tourism-generated tax revenue and overall visitor spending.

These results have implications beyond Colorado. Many western U.S. states, as well as parts of Canada and other fire-prone regions, share similar combinations of expanding wildfire risk and tourism-dependent local economies. As large wildfires become more frequent and severe across western North America, the economic disruptions identified in Colorado may represent a broader pattern affecting rural communities that depend on recreation and seasonal visitation to sustain their local economy. The negative impacts are most pronounced in counties affected by the largest fires, underscoring the relevance of these findings for other regions experiencing increasingly extreme wildfire events.

2.2 Literature Review

A large literature examines the long-run economic effects of natural disasters. Studies of federally declared disasters in the United States find persistent increases in out-migration and declines in housing values following severe events (Felbermayr and Gröschl, 2014; Boustan et al., 2020). Research on earthquakes shows that these shocks can have lasting impacts on economic growth (Lackner, 2018), while studies of hurricanes document reductions in local economic growth following storm exposure (Strobl, 2011). Natural disasters are associated with short-run output declines that can persist over time, particularly in smaller or developing economies (Noy, 2009).

Despite this growing body of work, relatively few studies focus specifically on the long-term economic impacts of wildfires. As noted by Coulombe and Rao (2025), much of the existing wildfire literature emphasizes short-run effects, leaving longer-run dynamics understudied.

Wildfires have long played a critical role in shaping ecosystems, with some tree and plant species even evolving to depend on fire for regeneration and reproduction (Pausas and Keeley, 2019). Unlike many natural disasters, not all wildfires are harmful; low-intensity fires can support ecological health and biodiversity. However, the growing scale and frequency of wildfires across the western U.S. have made their economic and social costs increasingly severe (Bayham et al., 2022).

These costs include direct damage to property, declines in nearby home values (Mueller et al., 2009), and significant health impacts linked to smoke exposure (Chen et al., 2024; Du et al., 2024). Suppression costs alone exceed $3.5 billion annually (U.S. Congress Joint Economic Committee Democrats, 2023), with subsidies and insurance market inefficiencies often masking the full economic burden in high-risk areas (Baylis and Boomhower, 2019). Additionally, wildfires can destabilize ecosystems, increasing risks of erosion, flooding, and long-term degradation of ecosystem services (Pereira et al., 2021; Bayham et al., 2022).

A key feature of wildfire dynamics is that a small number of extreme events account for a disproportionate share of total damages. Strauss et al. (1989) find that 1–2% of fires are responsible for 90–99% of the total area burned in the western United States, and that suppression efforts are largely ineffective under these extreme conditions. Large wildfires can also severely hinder an ecosystem's ability to recover, delaying or even preventing the return of natural amenities that attract tourism (Crowley et al., 2023).

Recent work further emphasizes the importance of fire characteristics in shaping economic impacts. Tavor (2024) find that wildfire size and intensity play a crucial role in determining the magnitude of economic effects and that these impacts vary across sectors. Similarly, Wang and Lewis (2024) show that large fires ($\geq$ 5000 hectares) near timberland have a more pronounced negative impact on timberland values than smaller fires.

A growing literature examines how wildfires affect tourism activity. Gellman et al. (2022) find that wildfires occurring within 20 kilometers of a campground reduce occupancy rates by 6.4% and more than double cancellation rates. Lee et al. (2023) document persistent declines in campground visits lasting up to six years after the initial burn. These findings suggest that wildfire impacts on tourism are not limited to the immediate aftermath of a fire.

Several mechanisms may contribute to these declines. Wildfires can degrade environmental amenities that are central to recreation demand. For example, wildfire-induced changes in water quality can reduce the attractiveness of recreation sites, affecting tourism-related economic activity (Wibbenmeyer et al., 2023; Holmes, 1988; Olmstead, 2010). Large fires also increase the risk of erosion, landslides, and flash flooding, further degrading local ecosystems (Bayham et al., 2022). Studies have found that large wildfires have substantial negative impacts on ecosystem services in both the short and long run, including impacts on fisheries and water systems that support tourism (Pereira et al., 2021; Wibbenmeyer et al., 2023). These effects are partly driven by the increased intensity of more frequent large fires, which can limit ecosystem recovery relative to smaller or prescribed burns.

Behavioral responses and risk perceptions also play an important role. Thapa et al. (2013) show that tourists respond heterogeneously to wildfire risk, with some significantly altering their travel behavior while others remain relatively insensitive to exposure.

A related literature examines broader local economic impacts of wildfire. Walls and Wibbenmeyer (2023) find that employment effects vary by spatial scale, with short-run gains at the county level but declines in areas closer to fire perimeters. Jones and McDermott (2021) show that megafires reduce per capita wage earnings for up to two years following the event, while Coulombe and Rao (2025) find that employment impacts can be both delayed and persistent, with negative effects emerging two years after the initial fire. These findings show that wildfire impacts often unfold over multiple years rather than occurring immediately or attenuating over time.

Evidence from Europe shows similar patterns. Otrachshenko and Nunes (2022) find persistent declines in tourist arrivals in Portugal following wildfire activity, while Meier et al. (2023) document reductions in GDP growth and tourism-related employment in southern Spain. However, traditional cost-based approaches may understate the full economic impact of wildfire on tourism by excluding complementary expenditures such as lodging, food, and transportation (Molina et al., 2019).

While the academic literature on wildfire and tourism is still emerging, a large gray literature documents similar patterns. Case studies from Canada and the western United States report sharp declines in visitation and revenue following wildfire events due to evacuation orders, smoke exposure, and perceived risk (Cariboo Chilcotin Coast Tourism Association, 2017; Sumic et al., 2013; Sierra Business Council, 2020). Broader analyses also highlight persistent losses in tourism revenue and business activity following major fires (Headwaters Economics, 2018). Despite this, comprehensive studies of long-term tourism impacts remain limited (Western Forestry Leadership Coalition, 2022).

Overall, the existing literature suggests that wildfires pose a significant and persistent threat to tourism activity, particularly in regions that depend heavily on natural amenities and outdoor recreation.

2.3 Background

In Colorado, the threat of wildfires on tourism is especially acute for three reasons. First, wildfires across the state have become increasingly frequent, destructive, and expansive over the past three decades, driven in part by prolonged drought, higher temperatures, and increased development in wildland-urban interface areas. Second, Colorado's tourism sector has experienced substantial growth and now accounts for a significant share of both state and local tax revenues. County governments, in particular, rely heavily on tourism-generated tax receipts to fund essential public services. Third, the vulnerability of local communities dependent on tourism to wildfires is amplified by the geographic overlap between fire-prone landscapes and tourism-intensive regions. Many of the state's most tourism-dependent counties are located in heavily forested areas of the Rocky Mountains, where the risk of large wildfires is especially high.

2.3.1 Wildfire Trends in Colorado

Trends in Colorado wildfires from 1996-2019 are consistent with previous research indicating that fires are increasing in frequency and size (see Section 2.2). Table 2.1 presents wildfire statistics from 1996 to 2019 aggregated across all decades, while Table 2.2 shows a decade-by-decade breakdown of summary statistics. Figure 2.1 shows cumulative area burned with linear trend lines for each decade, showing an increase in burned rate in each successive decade.

In Colorado, the number of fires per year has increased dramatically, rising from approximately 23 fires per year in the 1990s to over 60 fires per year in the 2010s, and 67 fires per year in the 2010s. The annual mean of total burned area has also grown over time, albeit more modestly, increasing from approximately 1,075 acres per fire in the 1990s to over 1500 acres per fire in the 2010s. Additionally, the variance in wildfire size has grown considerably in recent decades, with the standard deviation of burned area rising from approximately 2,030 acres in the 1990s to approximately 3,900 acres in the 2000s, and reaching nearly 5,400 acres in the 2010s. These trends show that wildfires in Colorado have grown in frequency, size, and unpredictability. This trend is further illustrated by the cumulative burned acreage across decades, shown in Figure 2.1. The linear trend line for each successive decade increases in slope, showing an acceleration in the cumulative burned acreage in each decade. The growing scale of the problem is also shown in Figure 2.2, which maps the boundaries of each wildfire, broken down by decade.

Table 2.1: Summary Statistics for Wildfire Sizes (All Fires 1996-2019)

Statistic (Acres)	Count	Mean	Median	SD	Min	Max
Wildfire Size	1,375	1,587.34	79.54	7,354.21	0.07	138,395.8

Table 2.2: Summary Statistics of Wildfires by Decade (includes fires between 1996-2019)

Decade	Annual Fires	Mean Burned Acreage	SD	Max	Min
1990s	23.5	1075.24	2030.60	16,572.87	0.17
2000s	60.2	1168.06	3889.26	138,395.79	0.08
2010s	67.9	1556.03	5394.86	108,131.28	0.07

Figure 2.1: Cumulative acres burned in Colorado over time, with decade-specific linear trend lines (1996-2019)

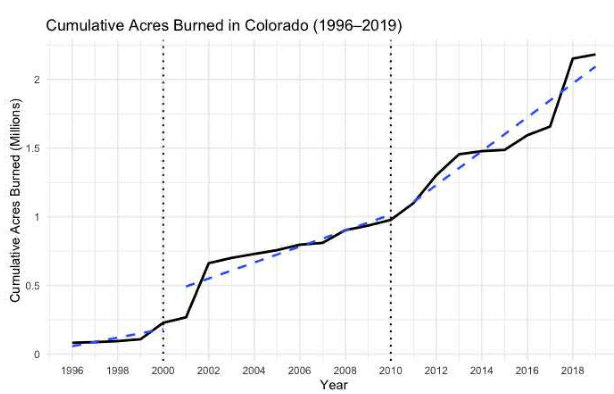
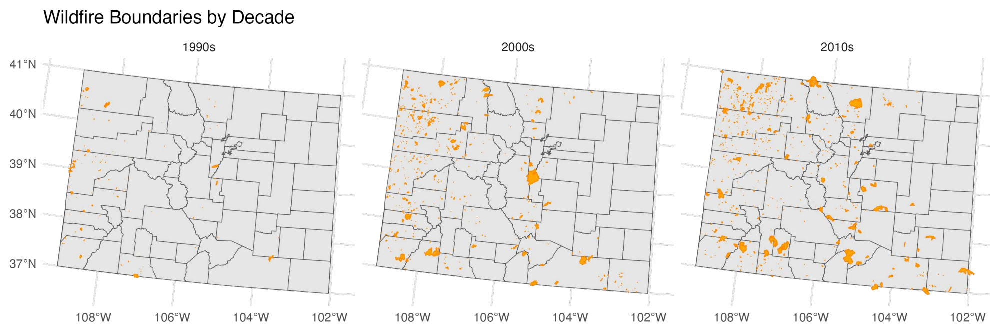


Figure 2.2: Wildfire Boundaries by Decade (all fires between 1996-2019)

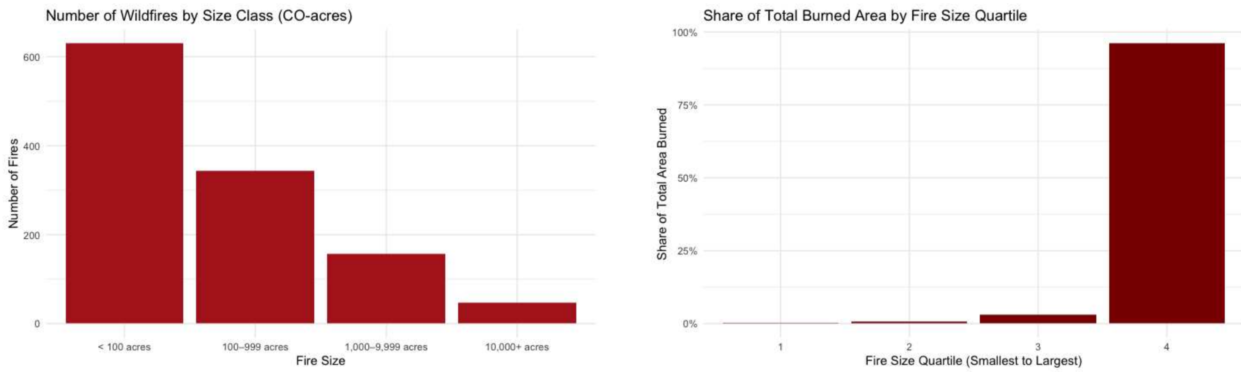


In this sample, the top 10% of wildfires by area are responsible for 86.29% of total burned area, the top 5% were responsible for over 73.28%, and the top 1% of wildfires were responsible for 38.7% of total burned area. In other words, the vast majority of total burned area was the result of a relatively small number of fires.

The distribution of wildfire sizes and their associated burned area is further illustrated in Figure 2.3. As shown in Figure 2.3a, the vast majority of wildfires burn fewer than 1,000 acres. However, a small number of exceptionally large fires account for nearly all of the total area burned (Figure 2.3b). Fires in the top quartile by size, those exceeding 475 acres, are responsible for 96.3% of the cumulative burned area. It is the impact of these infrequent but exceptionally large and destructive fires that is the focus of this study.

Figures 2.4 show the total number of wildfires and total acreage burned over time. Both measures of wildfire frequency and impact show positive trends across this time period, again under-

Figure 2.3: Distribution of Wildfire Burn Patterns

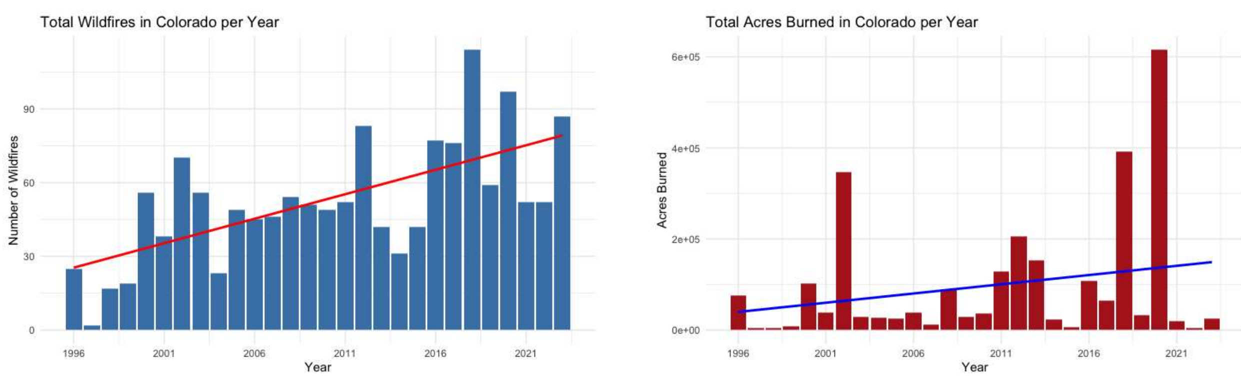


(a) Wildfire Count by Burn Area

(b) Burn Share by Size Quartile

pinning the importance of understanding the long-term impacts of these increasingly frequent and destructive wildfires.

Figure 2.4: Trends in Colorado Wildfires



(a) Wildfires Per Year

(b) Acres Burned Per Year

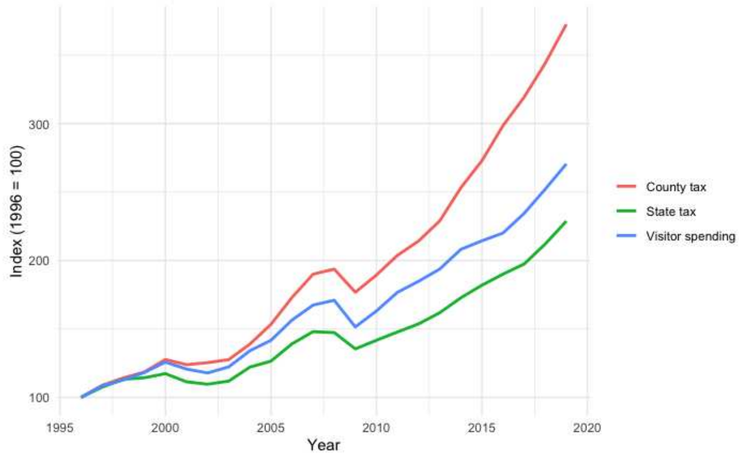
2.3.2 Economic Impact of Tourism in Colorado

Tourism plays a critical role in shaping Colorado's economy, contributing to job growth at a faster pace than any other sector (Colorado Tourism Office, 2024). In 2023, tourism spending accounted for more than 4.5% of state GDP. It generates substantial business income and tax revenues that help fund public services (including education, infrastructure, and local government operations) through tourism-supported programs and initiatives. Understanding the long-term impacts of large wildfires on regional tourism spending is especially pertinent to residents and policymakers in Colorado as tourism plays a critical role in mountain communities who are acutely at risk of experiencing a large wildfire.

Colorado provides a useful setting for this analysis because its economic structure mirrors that of many tourism-dependent regions across the western United States and beyond. Recreation and tourism are important engines of rural economic development, contributing to employment growth, higher income levels, and reductions in poverty (Reeder and Brown, 2005; Liu et al., 2023). At the national level, outdoor recreation contributes approximately \$640 billion annually to the U.S. economy, with several western states, including Montana, Alaska, Wyoming, Utah, Nevada, and Idaho, deriving an even greater share of state GDP from outdoor recreation than Colorado (Lawson and Story, 2024). States such as Washington, Arizona, and Oregon also rank above the median in the US in outdoor recreation economic activity, underscoring that Colorado's tourism-reliant economic structure is broadly representative of the American West. More generally, tourism accounts for roughly 8–10 percent of global GDP and is widely recognized as a central mechanism for local economic development in rural regions (Meyer and Meyer, 2015). As a result, the economic disruptions documented in Colorado are likely informative for other regions where tourism functions as a primary driver of regional development.

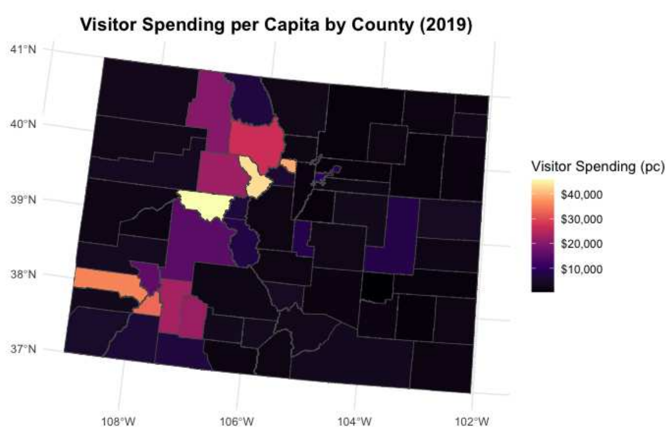
Colorado's tourism sector expanded substantially between 1996 and 2019. Over this period, nominal visitor spending rose from approximately \$9 billion to nearly \$25 billion, an increase of roughly 170%. Revenues from tourism-related taxes grew even more rapidly. County-level tax receipts increased from \$243 million in 1996 to more than \$900 million by 2019, representing a 272% increase, while state-level tourism tax revenues rose from \$258 million to \$591 million, an increase of 128% over the same period. The faster growth of county tourism tax revenues relative to total visitor spending suggests that county governments have become increasingly reliant on tourism activity to finance local public services, suggesting a potential heightened fiscal vulnerability to negative shocks from environmental disasters such as large wildfires (Figure 2.5).

Figure 2.5: Indexed trends in total visitor spending, county tourism tax receipts, and state tourism tax receipts in Colorado, 1996–2019 (1996 = 100).



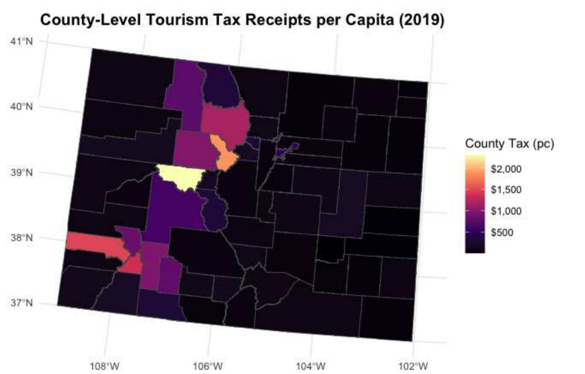
The physical impacts of wildfire are concentrated to certain regions; so too are the economic activities most vulnerable to these disruptions. In Colorado, tourism is not only a major economic driver but also largely localized in high fire risk, forested counties. Understanding the geography of tourism provides critical context for interpreting the economic consequences of mega-fires.

Figure 2.6: Total Tourism Spending per Capita

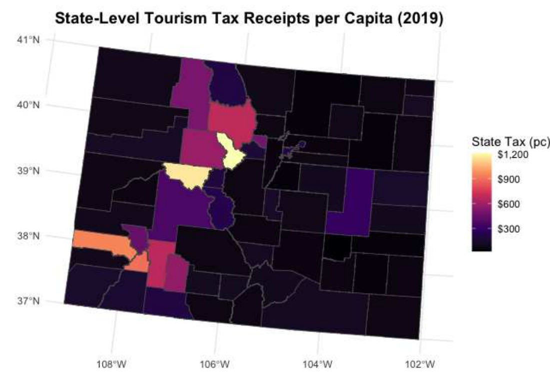


The economic reliance on tourism varies substantially across Colorado counties, as shown in Figure 2.6, which maps tourism spending per capita in 2019. Counties such as Pitkin and Summit, home to major resort destinations like Aspen and Breckenridge, exhibit exceptionally high levels of tourism expenditures relative to their resident populations, with per capita spending in the tens of thousands of dollars. In contrast, more populous counties along the Front Range and less tourism-intensive areas on the Western Slope display significantly lower per capita tourism revenues. It is readily apparent that most counties that have relatively high levels of tourism spending per capita are located in mountainous, forested areas, more likely to experience a large wildfire in a given year. Similar trends can be seen in total county and state tax dollars generated from tourism spending, seen in Figure 2.7.

Figure 2.7: State and County Tax Receipts Generated by Tourism Spending



(a) County Tax Receipts Generated from Tourism Spending



(b) State Tax Receipts Generated by Tourism Spending

Between 1996 and 2019, Colorado saw substantial growth in both wildfire activity and tourism-related spending. Many of the counties most dependent on tourism revenues to sustain their local economies and fund public services are located in areas of relatively high wildfire risk. As wildfire risk continues to escalate, it is increasingly likely that fires will have larger and more persistent effects on tourism. Understanding the long-term impacts of wildfire on tourism spending is therefore particularly important for policymakers and land managers in Colorado, as well as in other heavily forested states across the American West.

2.4 Methods

One key feature of wildfires in Colorado is the heterogeneous timing of mega-fires across counties. Between 1996 and 2019, different counties experienced a local mega-fire in various years. Because tourism spending is observed annually over that time span, the effects of the fire are staggered, and some counties are treated earlier than others, while several counties are never treated.

Consequently, analyzing the impact of mega-fires on local tourism requires considering these singular large-scale events as staggered treatments that impact county-level tourism expenditures. The first mega-fire event qualifying as a treatment occurred in 2002, allowing for a substantial pre-treatment period for all counties. To estimate the impact of this and subsequent tourism spending involves employing the Callaway and Sant'Anna (2021) staggered difference-in-differences (staggered DiD) estimation strategy, in which treated counties are compared against a group of never-treated counties that provide a control group for the analysis. The time frame of the study is restricted to pre-2020, as the COVID-19 pandemic had substantial impacts on tourism spending that were not directly related to the occurrences of large wildfires.

The staggered DiD approach is well-suited for this context due to the quasi-random occurrence of historically unprecedented mega-fires in specific counties and years. It allows for the estimation of group-time average treatment effects, $ATT(g,t)$, defined as the average treatment effect in period t for the group of counties first treated in period g. This method is preferred over the traditional two-way fixed effects model because it allows for the estimation of effects based on the length of exposure to the treatment, and avoids biases that arise when treatments are staggered across time periods. The estimator accommodates variation in treatment timing and yields group-specific effects ATT(g,t) that are subsequently aggregated into overall average treatment effects across groups and time.

Following the framework of Callaway and Sant'Anna (2021), the target parameter is the group–time average treatment effect (ATT) shown in Equation 2.1:

$$ATT(g,t) = \mathbb{E}[\,Y_{it}(1) - Y_{it}(0)\,|\,G_i = g,\ t\,], \tag{2.1}$$

where $ATT(g,t)$ represents the average treatment effect at time t for the group of counties first treated in period g. The estimator compares each treated cohort to never-treated counties that serve as a control group using a doubly robust estimation technique.[1]

The outcome variable Y_{it} denotes a tourism-related measure for county i in year t, normalized to per capita values. Outcomes include total visitor spending, county and state tax revenues from tourism, and sector-specific expenditures such as hotel/motel and campground spending.

The staggered DiD approach accounts for variation in treatment timing and avoids potential biases that arise from negative weighting in a traditional two-way fixed effects (TWFE) model (Roth et al., 2023). In a TWFE specification, counties treated earlier in the sample can be inappropriately used as controls for counties treated later, even in years when the earlier-treated counties are already treated. For example, a county that experienced a wildfire in 2002 may be used as a control for a county first affected in 2012 during the years 2003 to 2011, despite already being treated throughout that period. As shown by Sun and Abraham (2021), such "forbidden comparisons" are more likely to occur for early-treated units, which can lead to biased estimates and, in some cases, negative weights in the aggregation of treatment effects. The estimator proposed by Callaway and Sant'Anna (2021) avoids these issues by comparing each treated group only to never-treated units and by aggregating treatment effects using non-negative, policy-relevant weights.

2.4.1 Data

The data used in this analysis come from a variety of government agencies. Full details on these data sources can be found in Appendix A.1. Wildfire data was sourced from the National Interagency Fire Center, which is the primary coordinating body for wildland firefighting in the U.S. The Center is composed of representatives from several key agencies including the Bureau of Land Management, U.S. Forest Service, National Park Service, Bureau of Indian Affairs, the National Weather Service, and other agencies. More information can be found on their website.

[1] Estimation is implemented using the `att_gt` function from Callaway and Sant'Anna (2021) in the `did` package in R, employing the doubly robust specification that combines outcome regression and inverse probability weighting.

They provide comprehensive data on wildfire occurrences across North America, including the year, size, and shapefile of every recorded fire dating back to the early 20th century.

Data on tourism is sourced from the Colorado Tourism Office in partnership with Dean Runyan Associates (see Appendix A.1). Using a combination of public and private data, they provide annual county-level travel impact reports that detail tourism spending by accommodation type (e.g., hotels, camping, second homes) and commodity category (e.g., lodging, transportation, food). These reports document tax receipts generated from tourism spending, along with industry earnings and employment supported by tourism activity. Annual data covering the period 1996–2019 are drawn from these reports. Annual county-level population data are obtained from the Colorado State Demography Office.

2.4.2 Defining the Treatment Group

Defining the treatment group is critical to estimating equation 2.1, as the vast majority of the total area burned is attributable to a small number of extremely large fires. As discussed in Section 2.3.1, in Colorado between 1996-2019, the distribution of wildfire sizes is highly skewed. the top 10% of wildfires by area are responsible for 86.29% of total burned area during this period, the top 5% were responsible for over 73.2%, and the top 1% of wildfires were responsible for 38.7% of total burned area. This concentration of burned area accounted for by a small number of large fires suggests that most wildfires in the dataset are not substantial enough to be considered a meaningful treatment.

To address this, three percentile-based thresholds are established to determine which wildfires qualify as treatments for a given county in a given year. Wildfires are classified as treatments if they fall within the top 10% ($\geq 2,555$ acres), top 5% ($\geq 7,100$ acres), top 1% ($\geq 30,000$ acres), or top 0.5% ($\geq 50,000$ acres) of wildfires by area. To classify whether a county was affected by a wildfire in a given year, wildfire boundary shapefiles were used to identify overlaps with county boundaries. A county is considered treated if a wildfire occurs entirely within its borders or intersects them, with all counties sharing any portion of a wildfire's area classified as affected.

This tiered approach enables an examination of whether the size of a wildfire influences its impact on tourism spending, providing a more nuanced understanding of wildfire effects across different levels of burn severity. While the ATT estimates for all treatment cutoffs were negative, they were only significant at the 1% and 0.5% cutoff levels. ATT estimates for the 5% and 10% treatment cutoffs are found in Table 2.7 in Appendix A.2.

Table 2.3 presents a balance table comparing per capita spending in treated and untreated counties at the 1% treatment cutoff (affected by fires greater than 30k acres in size). At this cutoff, there are 18 treated counties and 46 counties in the control group. This results in a total of 259 treated county-years, and 1274 untreated county-years. Counties in both the control and treatment group show similar levels of taxes generated from tourism spending. Counties in the control group average slightly higher levels of total tourism spending and hotel/motel spending than treated counties, but treated counties average significantly more campground spending per capita than those in the control group. Balance tables across all four wildfire size thresholds are found in Table A.1 in Appendix A.1.

One of the outcomes examined in this analysis is state tax revenue generated from tourism activity within each county. Although these revenues are collected at the state level, they are attributed to the county where the economic activity occurred. As a result, the estimates should be interpreted as the effect of wildfire on tax revenue originating in the affected county, not on statewide tax collections.

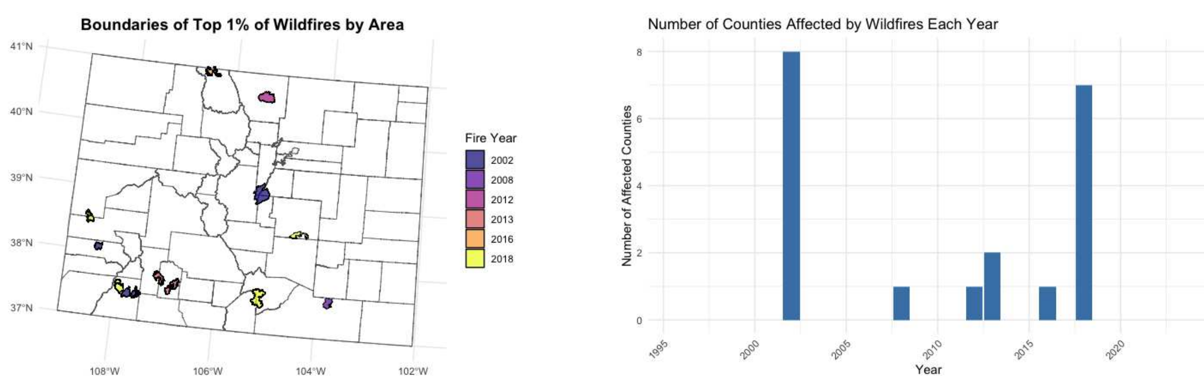
Table 2.3: Balance Table for Treatment Groups

Variable	**Never-Treated**				**Treated**			
	Mean	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**
State Tax (pc)	159.71	211.49	0.00	1386.32	151.86	196.68	19.99	1399.61
County Tax (pc)	191.50	357.25	0.00	3092.45	189.63	314.86	5.12	2593.59
Visitor Spending (pc)	5980.43	9223.48	0.00	51163.41	5583.98	7520.93	407.14	51138.91
Campground Spending (pc)	374.87	762.45	0.00	6024.55	708.10	1396.13	4.66	5713.49
Hotel/Motel Spending (pc)	3643.44	6496.04	0.00	38194.30	3005.52	4405.57	0.00	32524.55

Unlike traditional two-way fixed effects (TWFE) models, which can produce biased estimates when treatment effects vary over time or across groups, a staggered DiD method estimates group-time average treatment effects, $ATT(g,t)$. This allows for heterogeneous treatment effects by explicitly accounting for the timing of treatment and enabling comparisons between treated and untreated groups at each period. Additionally, the approach allows for event-study analyses, allowing for an examination of dynamic treatment effects over time. It also provides robust inference through doubly robust estimation techniques, ensuring greater reliability of the estimated treatment effects even in the presence of potential model misspecification.

The staggered timing of wildfire treatments across Colorado is illustrated in Figure 2.8. Figure 2.8a displays the location and shape of each wildfire included in the sample, with darker shades indicating fires that occurred earlier in the study period. Figure 2.8b highlights that no counties experienced the largest mega-fires until 2002, when eight counties were affected. Between 1996 and 2019, mega-fires occurred in six different years, each impacting between 1-8 counties.

Figure 2.8: Occurrence of mega-fires Across Time



(a) Top 1% of Wildfires Boundaries

(b) Affected Counties Per Year

2.5 Results

The empirical analysis examines three primary county-level tourism outcomes: per capita county tax revenue from tourism, per capita state tax revenue from tourism, and per capita total visitor spending. In addition, ATT estimates for campground and hotel/motel spending are reported. The results highlight consistent negative impacts following large wildfires across all outcome variables. Significant drops in tourism spending and associated tax receipts are observed at the 1% and 0.5% cutoffs and are the focus of the following two sections. Table 2.4 lists the average treatment effects of a wildfire on tourism spending per capita.

Table 2.4: Average Treatment Effects on Tourism-Related Outcomes for full sample. All estimates reported in per capita terms. All specifications include fixed effects for county and year. Asterisks (*) indicate statistical significance at the 5% level.

Outcome	**ATT**	**Std. Error**	**95% CI**	**Sig**
Total Spending	-776.93	335.71	[-1434.90, -118.95]	*
County Tax	-49.25	20.47	[-89.37, -9.13]	*
State Tax	-19.65	9.49	[-38.25, -1.04]	*
Campground Spending	-32.33	26.15	[-83.58, 18.92]	
Hotel/Motel Spending	-647.78	324.59	[-1283.96, -11.60]	*

The analysis reveals that the largest wildfires have significant and long-lasting negative effects on tourism spending in affected counties. Specifically, the occurrence of a mega-fire leads to substantial declines in overall visitor spending, as well as reductions in county and state tax revenues generated from tourism. These negative impacts are statistically significant a decade after the initial fire. The decreases in visitor spending are primarily driven by reductions in hotel/motel spending. Importantly, these effects emerge only when applying the most stringent treatment definitions, where only the top 1% and top 0.5% of wildfires by area are classified as treatments. Negative spending effects persist when the analysis is restricted to rural counties, which are more prone to wildfire exposure and are generally more economically dependent on tourism.

Seen in Table 2.4, the estimated average treatment effects (ATT) reveal significant and economically meaningful impacts of large wildfires on tourism-related economic outcomes. For county tax revenue, the occurrence of a mega-fire reduces per capita county tax receipts by $49.25, with a 95% confidence interval ranging from -$9.13 to -$89.87. State tax revenue shows a similar pattern, declining by -$19.65 per capita, also statistically significant, with a confidence interval between -

\$1.04 and -\$38.25. Total visitor spending per capita shows a significant decline, falling by roughly -\$777 following a mega-fire, albeit with a wide 95% confidence interval of -\$118.95 to -\$1,434.90.

Although the ATT estimate for per capita campground spending is -\$32.33 following a major wildfire, this estimate is not statistically significant. In contrast, hotel and motel spending exhibits a statistically significant average treatment effect of -\$647.78 per capita. The magnitude and significance of the latter suggest that the decrease in tourism spending after wildfires is more pronounced among visitors who rely on commercial lodging than those who choose to camp. This discrepancy implies that wildfire events may deter higher-spending, hotel-dependent tourists more than those engaging in lower-cost recreational activities such as camping, although more study is needed on this topic.

Figure 2.9 reports group-time estimates for total visitor spending across exposure periods, and indicates the impact of large wildfires on total visitor spending intensifies over time. During the first three years following a wildfire, declines in spending are modest and statistically insignificant. By year four, the estimated reduction reaches \$507.73 per capita and increases to \$879.48 in year five, though these effects remain statistically insignificant. Beginning in year six, declines in visitor spending become larger and statistically significant. Spending decreases by \$1,354.15 per capita in year six, deepens to \$1,623.12 in year seven, and remains substantially below pre-fire levels thereafter. The largest estimated reduction occurs ten years after the wildfire, with spending lower by \$1,498.55 per capita (95% confidence interval: \$280.41 to \$2,716.70). Complete group-time estimates are reported in Appendix A.2 in Table A.2.

Figure 2.10 shows the time dynamics that the length of exposure to treatment has on state and county tax receipts generated from tourism spending. Figures 2.10a and 2.10b present event study plots illustrating pre- and post-treatment trends for state and county tax receipts respectively. These graphs display the estimated coefficients for each period relative to the wildfire event, along with 95% confidence intervals. The full tables listing the ATT estimates, standard errors, and the confidence intervals are found in Appendix A.2.

Figure 2.9: Event Study: Total Visitor Spending (1% cutoff)

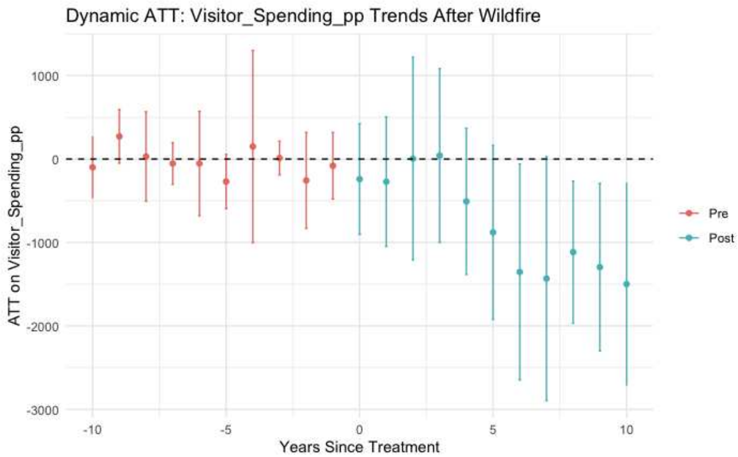
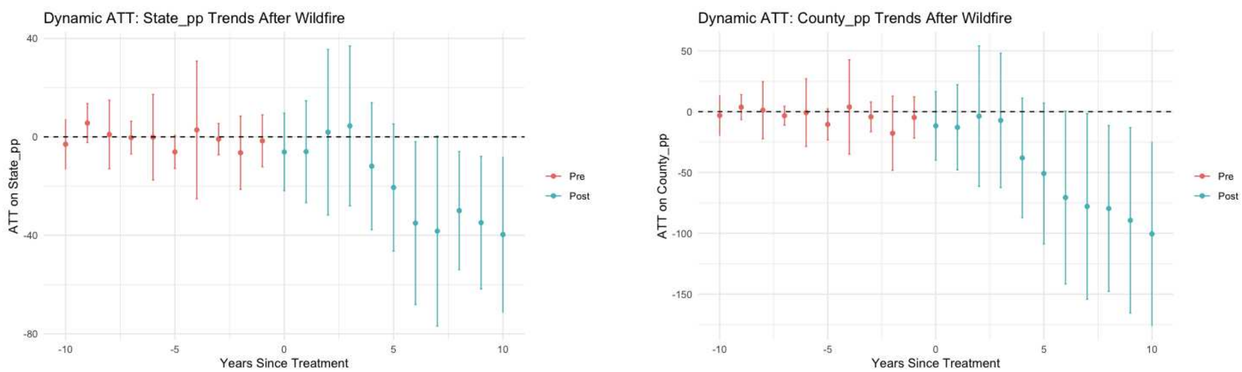


Figure 2.10: Event Study: Tax Receipts Generated from Tourism Spending (1% cutoff)



(a) State Tax Receipts **(b)** County Tax Receipts

The event-study graphs in Figures 2.9 and 2.10 provide evidence consistent with the parallel trends assumption, as all pre-treatment coefficients are statistically insignificant. The figures also illustrate that the negative effects of wildfire exposure grow with time since treatment, indicating persistent and worsening impacts on tourism-related tax receipts over the following decade. Complete event-study estimates, including coefficients and corresponding confidence intervals used to construct these figures, are reported in Appendix A.2, Tables A.2, A.3, and A.4.

The long-term effects on county tax receipts intensify in the years following a mega-fire. Five years after the initial event, county tax revenue per capita declines by an average of \$50.80. The magnitude of this reduction increases to \$70.55 in year six and \$77.87 in year seven. A decade after treatment, affected counties experience an average decline of more than \$100 per capita in tourism-generated county tax revenue. Full group-time estimates are reported in Appendix A.2, Table A.3.

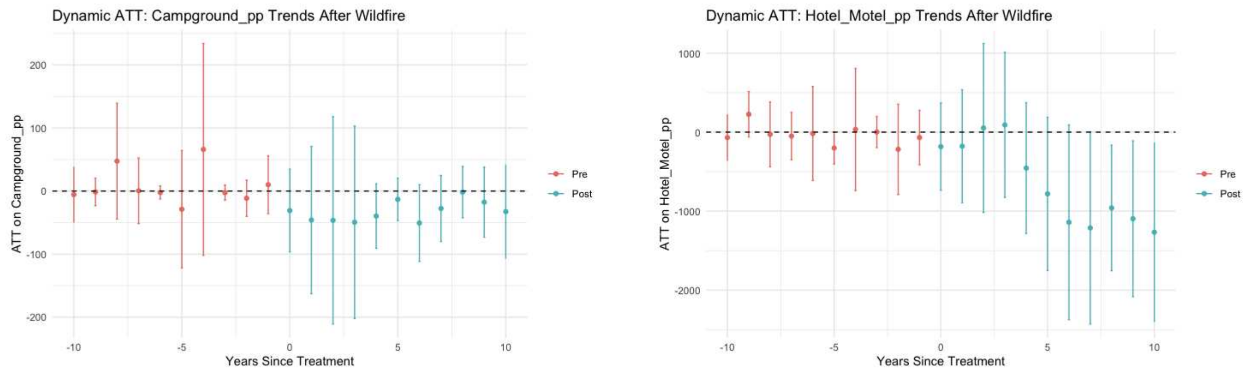
State tax receipts generated from tourism spending follow a similar trajectory. Statistically significant declines first emerge six years after the initial burn, with an average reduction of \$35.03 per capita, deepening to \$39.67 per capita ten years after the mega-fire. Corresponding group-time estimates are presented in Appendix A.2, Table A.4.

These results suggest that the negative economic consequences of large wildfires not only persist but intensify over time. The most pronounced reductions occur 4-5 years after the initial event, potentially reflecting prolonged disruptions in tourism infrastructure, permanent damages inflicted on the local ecosystem, changes in visitor perceptions of wildfire-affected areas, or broader regional economic adjustments. This pattern may also reflect the fact that ecosystems struggle to recover after mega-fires due to their above-average temperatures and the vastness of the burn scar Pausas and Keeley (2019). The delayed nadir in spending reductions highlights the importance of considering long-term recovery strategies when assessing the economic consequences of large-scale wildfires. The wide confidence intervals on these estimates also underscore the importance of further research with more granular data to obtain more precise estimates on these long-term economic impacts.

Finally, Figure 2.11 presents the dynamic effects on campground and hotel/motel spending. While hotel and motel expenditures exhibit negative and statistically significant declines following a mega-fire, no statistically significant effect is observed for campground spending. These results suggest that the economic response to wildfire events varies by accommodation type, with commercial lodging more adversely affected than lower-cost options such as camping. Complete group-time estimates are reported in Tables A.5 and A.6 in Appendix A.2.

The negative impacts are only significant when the treatment definition is restricted to fires larger than 30,000 acres, corresponding to the 13 largest fires (top 1% by area burned) in the sample. While the top 5% cutoff shows similar trends to the 1% threshold, the estimates are not statistically significant. In contrast, the top 10% cutoff yields null results across all outcomes,

Figure 2.11: Event Study: Campground and Hotel/Motel Spending (1% cutoff)



(a) Campground Spending per Capita **(b)** Hotel/Motel Spending per Capita

suggesting that many fires within this broader category are too small to have lasting economic effects on tourism spending. The event graphs of the analysis at the 5% and 10% thresholds can be found in Appendix A.3.

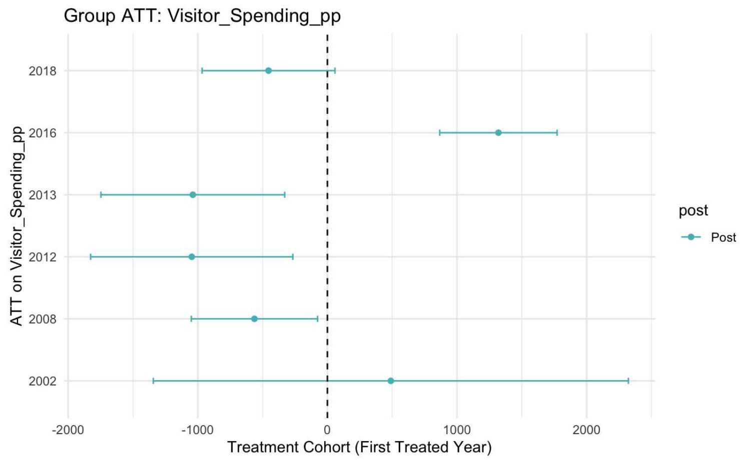
2.5.1 Group ATT Estimates at the 1% Cutoff

To further investigate the dynamics underlying the group-time estimates, I examine the treatment effects separately for each cohort. This approach allows us to identify whether the aggregate results are being driven by specific wildfire events or are consistent across treatment groups. Results of the group effects are seen in Table 2.5 and Figures 2.12.

Table 2.5: Group ATT Estimates (1% Cutoff)

Group	Estimate	Std. Error	95% Conf. Interval
2002	489.39	841.44	[-1343.22, 2322.01]
2008	-562.83*	223.59	[-1049.79, -75.87]
2012	-1046.62*	358.11	[-1826.58, -266.66]
2013	-1038.35*	325.52	[-1747.31, -329.39]
2016	1319.41*	207.81	[866.80, 1772.02]
2018	-454.20	235.31	[-966.69, 58.29]

Figure 2.12: Cohort-Specific Event Study Estimates

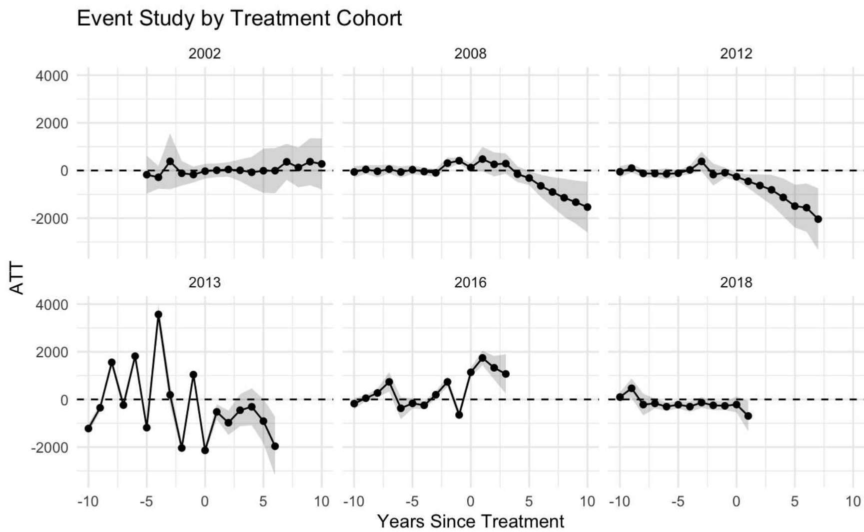


The results reveal a relatively consistent pattern across treatment cohorts. With the exception of the 2016 treatment cohort, all estimated effects are either negative or statistically insignificant. The 2008, 2012, 2013 cohorts exhibit statistically significant declines in tourism spending following mega-fire exposure, while the 2018 cohort shows a negative but insignificant impact.

The notable exception is the 2016 treatment cohort, which exhibits a large, significant positive effect post-treatment. This cohort appears to drive the delayed effect observed in the baseline specification, offsetting the otherwise negative impacts of wildfire exposure on tourism spending observed across other treatment cohorts.

This pattern is further supported by the cohort-specific event study results shown in Figure 2.13. Though the estimates become noisier when disaggregated, the 2008, 2012, 2013, and 2018 cohorts display immediate or near-immediate declines in tourism spending following treatment. In contrast, the 2016 cohort exhibits a pronounced positive spike in tourism spending the first three years after the fire. This divergence highlights heterogeneity in post-fire tourism responses across wildfire events and helps explain why significant effects do not appear in the baseline specification until after year 5.

Figure 2.13: Cohort-specific event study estimates



The 2016 treatment cohort consists of a single county, Jackson County, and is treated by the Beaver Creek Fire, which burned approximately 38,000 acres near the Colorado–Wyoming border. One possible explanation for the positive effect observed in this cohort is unusually strong ski conditions in nearby Steamboat Springs. Snowpack levels were well above average in 2016, 2017, and 2019, with particularly strong conditions in 2017 (USDA Natural Resources Conservation Service, 2024). The pull of favorable snowfall may have offset, or even temporarily dominated, the negative effects of wildfire exposure, leading to increased tourism activity in the region. While this explanation is speculative, it suggests that the interaction between wildfire impacts and snowfall-driven tourism demand are important and should be explored more thoroughly in future studies.

In the more restrictive 0.5% cutoff specification, the Beaver Creek Fire no longer qualifies as a treatment, as it falls below the 50,000-acre threshold. When restricting attention to these larger fires, the estimated effects on tourism spending appear immediately following treatment. This pattern provides further evidence that the delayed decline observed in the baseline specification is driven, at least in part, by the inclusion of Jackson County following the 2016 Beaver Creek fire.

2.5.2 0.5% Mega-Fire Cutoff Results

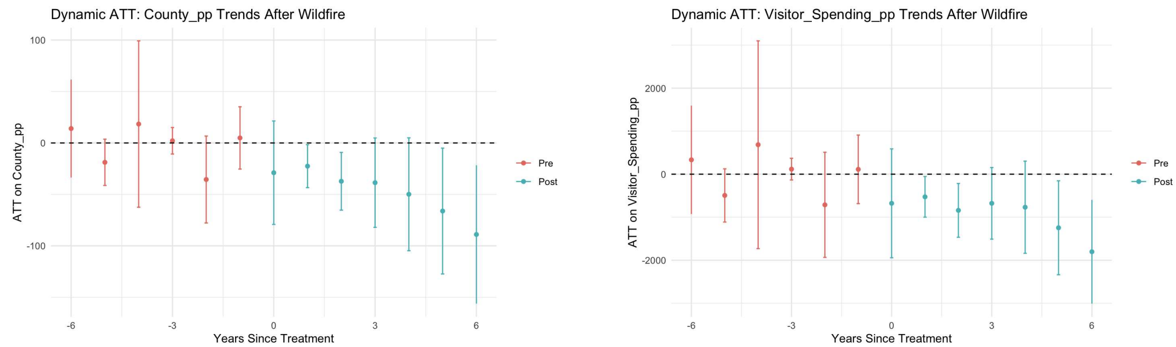
To ensure that the analysis isolates the economic consequences of only the most extreme wildfire events, the staggered DiD models are re-estimated using a more restrictive treatment threshold. This threshold corresponds to the top 0.5% of wildfires by burned area in Colorado. Under this definition, only the seven largest fires in the sample are classified as treatments, only including fires exceeding 50,000 acres in size. This stricter treatment cutoff captures exceptionally large, high-intensity events that are most likely to disrupt regional tourism infrastructure, degrade air quality, and alter visitor behavior. The 0.5% cutoff reduces the number of treated counties but provides a clearer distinction between typical wildfire activity and truly catastrophic events.

Table 2.6 reports the estimated average treatment effects (ATT) on per-capita tourism outcomes under this specification. The results indicate statistically significant declines in total visitor spending, county lodging tax revenues, state tourism tax revenues, and hotel/motel spending in the years following a mega-fire. Although campground spending also decreases on average, this effect is not statistically significant. The magnitudes imply that a single mega-fire event is associated with reductions of nearly \$934 in total visitor spending per resident, \$47 in county tax revenue per capita, and \$22 in state tax revenue per capita, relative to never-treated counties. Event-study estimates (Figure 2.14) show that these effects persist for several years after the fire, with no evidence of a return to baseline spending levels.

Table 2.6: Average Treatment Effects of Mega-Fires (Top 0.5% by Burned Area). All estimates reported in per-capita terms. Asterisks (*) indicate statistical significance at the 5% level.

Outcome	**ATT**	**Std. Error**	**95% CI**	**Sig.**
Total Visitor Spending	-933.70	286.22	[-1494.68, -372.73]	*
County Tax	-47.46	16.65	[-80.10, -14.83]	*
State Tax	-22.13	7.88	[-37.58, -6.69]	*
Campground Spending	-90.23	55.59	[-199.18, 18.73]	
Hotel/Motel Spending	-768.06	267.34	[-1292.03, -244.09]	*

Figure 2.14: Event-study estimates of the dynamic effects of mega-fires (top 0.5% cutoff) on county tax receipts and total visitor spending.



The dynamic event-study estimates in Figure 2.14 reveal that the negative effects of mega-fires on tourism spending emerge immediately following the year of the mega-fire. The first post-treatment period shows a significant decline in visitor spending and tax revenues, suggesting that the very largest wildfires have the most immediate and severe impacts on local tourism activity. Similar to baseline results, there are no signs of recovery and that reductions in visitor spending remain significantly reduced six years after the initial burn.

2.5.3 Alternative Treatment Thresholds

To assess whether the estimated economic impacts depend on wildfire severity, ATT estimates are compared across alternative treatment thresholds corresponding to the top 10%, 5%, 1%, and 0.5% of fires by burned area. As shown in Table 2.7, the magnitude and statistical significance of the estimated effects increase as the cutoff becomes more restrictive. When all large fires (top 10%) are included, reductions in tourism spending and tax receipts are small and statistically insignificant. In contrast, as the definition narrows to the top 1% and 0.5% of fires, the estimated declines in visitor spending, lodging tax revenues, and hotel/motel expenditures grow sharply in magnitude and become statistically significant.

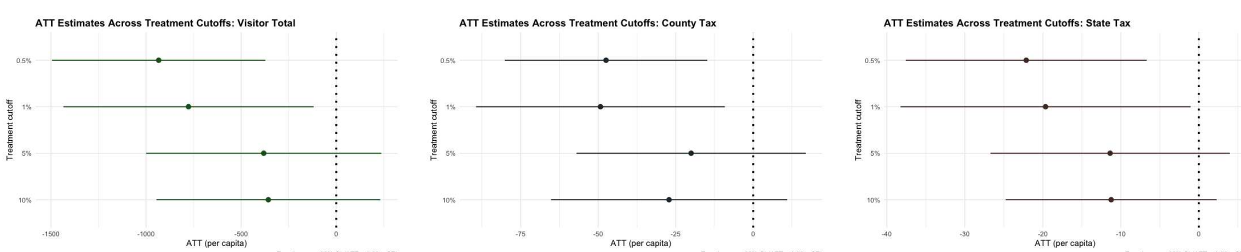
This pattern suggests that the size of the wildfire is a key driver of the observed impacts rather than unobserved confounding factors common to all fire years. The results imply that only the very largest, most destructive wildfires generate persistent and measurable reductions in local tourism activity, and that the observed reductions in visitor spending are driven by the size of the wildfire. ATT estimates across cutoff levels are listed in Table 2.7 and shown graphically in Figures 2.15. Full event-study graphs across all specifications can be found in Appendix A.3.

Table 2.7: Comparison of Average Treatment Effects Across Wildfire Size Cutoffs

	0.5% Cutoff		1% Cutoff		5% Cutoff		10% Cutoff	
Outcome	**ATT**	**SE**	**ATT**	**SE**	**ATT**	**SE**	**ATT**	**SE**
Visitor Total	**-933.70***	(286.22)	**-776.93***	(335.71)	**-381.34**	(315.58)	**-357.18**	(300.13)
County Tax	**-47.46***	(16.65)	**-49.25***	(20.47)	**-20.02**	(18.87)	**-27.14**	(19.43)
State Tax	**-22.13***	(7.88)	**-19.65***	(9.49)	**-11.37**	(7.83)	**-11.24**	(6.90)
Campground	**-90.23**	(55.59)	**-32.33**	(26.15)	**-9.99**	(20.46)	**1.55**	(17.11)
Hotel/Motel	**-768.06***	(267.34)	**-647.78***	(324.59)	**-327.67**	(252.96)	**-327.91**	(239.71)

Notes: ATT estimates from Callaway–Sant'Anna staggered DiD models. All outcomes are expressed in per capita terms. Asterisks (*) indicate statistical significance at the 5% level.

Figure 2.15: ATT estimates across wildfire size cutoffs. Points denote estimated effects; bars indicate 95% confidence intervals.



(a) Total Spending **(b)** County Tax **(c)** State Tax

2.5.4 Spillover Effects

To evaluate whether wildfire impacts extend beyond directly affected counties, potential spatial spillovers are examined by constructing buffers of varying distances around each county and re-estimating the analysis. Buffer zones of 10, 25, and 50 miles are generated around each county boundary. Counties whose buffers intersect any wildfire perimeter are reclassified as treated under the corresponding buffer specification. For example, a county whose boundary lies within 10 miles of a wildfire is treated as affected under the 10-mile cutoff, while counties beyond 10 miles remain untreated. This approach is implemented at the 1% treatment cutoff as a robustness check for spillover effects. The model is then re-estimated using the same staggered difference-in-differences framework and intersection-based treatment assignment.

Table 2.8 reports the average treatment effects across buffer distances. The magnitude and statistical significance of the estimated impacts decline as the buffer distance increases, indicating that proximity plays a key role in determining the severity of economic disruption. At the 10-mile cutoff, significant reductions in county tax revenues are observed, along with negative but imprecisely estimated effects for total visitor spending and state tax receipts. At 25 and 50 miles, the estimates become smaller in magnitude and lose statistical significance, suggesting that the adverse economic effects of wildfires are geographically concentrated and dissipate with distance from the burn perimeter.

Table 2.8: Spillover Robustness: Buffer Zones Around Wildfire Boundaries

Buffer Distance	**Total Spending**	**County Tax**	**State Tax**
10 miles	-634.7 (383.1)	-45.1* (21.6)	-16.6 (9.1)
25 miles	-429.3 (647.9)	-29.2 (37.3)	-13.2 (16.4)
50 miles	-269.8 (675.4)	-29.4 (55.0)	-5.3 (17.3)

Notes: Coefficients reported with robust standard errors in parentheses. $^{*}p < 0.05$. All results reported in per capita terms.

These results provide evidence of localized spillover effects: economic disruptions to tourism are strongest within roughly 10 miles of the burn perimeter but fade quickly with distance. This pattern is consistent with short-range channels such as road closures, smoke intrusion, burn scar salience, and perceived fire risk that directly affect neighboring communities, while more distant areas remain largely unaffected. These results provide evidence that the negative effects of mega-fires on tourism spending are largely localized to areas closest to the burn scar, with some evidence of spillovers to the closest counties. Event study graphs at different buffer thresholds are found in Appendix A.4.

2.5.5 Alternative Specifications

To assess the robustness of the main findings, two alternative model specifications are estimated. First, the sample is restricted to smaller, rural counties by excluding the more densely populated Front Range, isolating effects in less urbanized regions where tourism activity is more directly linked to natural amenities. Second, La Plata County, which experienced two mega-fires during the sample period, is excluded to ensure that repeated exposure does not drive the results.

Across these specifications, the estimated average treatment effects remain negative and broadly similar in magnitude to the baseline results, though standard errors increase and estimates become less precise. In the model excluding Front Range counties, the ATT estimates lose statistical significance, suggesting that declines in tourism spending may be more pronounced for counties along the Front Range that experience a mega-fire. Even so, several group–year estimates remain significantly negative across both specifications, indicating that large wildfires continue to depress visitor spending and tax receipts across alternative samples. Full ATT estimates are presented in Table 2.9, and corresponding event-study plots are provided in Appendix A.5.

The persistence of negative effects across specifications supports the conclusion that observed declines in tourism-related spending are causally attributable to the occurrence of mega-fires rather than to sample composition or outlier events.

Table 2.9: Average Treatment Effects on tourism-related outcomes across model specifications. All ATT estimates are measured in per capita terms for county i. The Front Range specification excludes counties along the urban corridor (El Paso, Denver, Arapahoe, Jefferson, Adams, Douglas, Larimer, Boulder, and Broomfield) while the twice-affected specification excludes La Plata County. Asterisks (*) indicate statistical significance at the 5% level.

Outcome	ATT	Std. Error	95% CI	Sig.
Main Specification				
Total Visitor Spending	-776.93	335.71	[-1434.90, -118.95]	*
County Tax	-49.25	20.47	[-89.37, -9.13]	*
State Tax	-19.65	9.49	[-38.25, -1.04]	*
Campground Spending	-32.33	26.15	[-83.58, 18.92]	
Hotel/Motel Spending	-647.78	324.59	[-1283.96, -11.60]	*
Excluding Front Range Counties				
Total Visitor Spending	-670.40	371.28	[-1398.11, 57.29]	
County Tax	-45.22	20.60	[-85.60, -4.85]	*
State Tax	-16.65	9.26	[-34.79, 1.49]	
Campground Spending	-34.08	37.88	[-108.45, 40.17]	
Hotel/Motel Spending	-554.15	303.85	[-1149.07, 41.39]	
Excluding Twice-Treated Counties				
Total Visitor Spending	-776.93	370.80	[-1503.68, -50.17]	*
County Tax	-49.25	21.15	[-90.72, -7.78]	*
State Tax	-19.65	9.63	[-38.52, -0.77]	*
Campground Spending	-35.14	39.70	[-87.79, 23.14]	
Hotel/Motel Spending	-565.34	331.45	[-1214.96, 84.29]	

2.6 Discussion & Policy Implications

The findings of this study indicate that large wildfires have significant and persistent negative effects on tourism spending, with the most severe impacts occurring several years after a fire. These results suggest that the economic consequences of wildfires extend well beyond the immediate aftermath, underscoring the need for long-term recovery strategies and proactive policy interventions. Wildfires not only pose an immediate threat to local ecosystems and public health, but also threaten long-term tourism spending and the public goods and services that rely on revenues generated by tourist-driven economic activity.

These findings are relevant beyond Colorado. A substantial body of evidence documents increasing wildfire frequency, size, and area burned across the western United States over recent decades, as longer fire seasons, changes in precipitation patterns, and higher fuel loads have contributed to rising wildfire risk. (Westerling, 2016; Dennison et al., 2014; Wasserman, 2020; Bayham et al., 2022; Wasserman and Mueller, 2023). Similar dynamics have been observed in Canada, where four of the most severe wildfire seasons of the past century have occurred within the last seven years (Parisien et al., 2023), and the 2023 wildfire season set a new record for total area burned, exceeding previous records by nearly two million hectares (Pelletier et al., 2024).

Another important finding of this study is that the economic impact of wildfires, including mega-fires, differs significantly with tourism activity. The ATT estimates show that wildfires significantly reduce hotel and motel spending, but not campground spending, suggesting that different types of tourists may respond differently to wildfire events. Visitors who rely on commercial lodging may be more sensitive to wildfire-related disruptions, either because they tend to book further in advance, incur higher costs, or have lower tolerance for risk and uncertainty. In contrast, campers may be more flexible in their travel plans or less deterred by the presence of recent fire activity. Future research should investigate this pattern more closely and why different types of tourists respond differently to mega-fires. Such distributional dynamics may be particularly relevant for resort counties and gateway communities throughout the Sierra Nevada, Cascade Range, Northern Rockies, and southwestern United States.

One key policy implication of these findings is the importance of sustained support for tourism-dependent communities. Given that tourism spending continues to decline for up to a decade following a mega-fire, local and state governments may consider developing long-term recovery programs for affected communities. These could include marketing campaigns aimed at restoring visitor confidence, investments in rebuilding trails and tourist destinations, and targeted policies to support local businesses most affected by declines in tourism activity. In states such as California, Oregon, Washington, and Idaho, where rural and mountainous counties depend more heavily on forest-based recreation and seasonal visitation, similar long-term fiscal pressures may arise following extreme wildfire events.

2.6.1 Mitigating Large Wildfires

The economic losses documented in this study underscore the importance of wildfire prevention and mitigation strategies aimed at reducing the likelihood and severity of extreme events. A growing body of research highlights the role of proactive fuel management, prescribed burning, and improved forest management in lowering wildfire risk (Bayham et al., 2022; Michetti and Pinar, 2019; Pereira et al., 2021). Historically, western forests experienced frequent, low-severity fires that naturally reduced surface and ladder fuels, thereby limiting the risk of high-intensity crown fires (Shafran, 2008). Prescribed burning can replicate these natural processes, reducing fuel accumulation and the probability of severe wildfires (Kaval et al., 2007; Pereira et al., 2021). Mechanical fuel treatments, such as thinning or mastication, similarly interrupt fuel continuity and reduce stand density, though they are often more costly and labor-intensive than prescribed burns (Amacher et al., 2005; Halbritter et al., 2020). Enhanced forest monitoring, early detection systems, and emerging machine learning techniques can further shift wildfire management from reactive suppression toward mega-fire prevention and sustainable management (Michetti and Pinar, 2019). Finally, fire-conscious silvicultural practices, including the cultivation of less flammable tree species, may also reduce future fire intensity (Michetti and Pinar, 2019).

Mitigation strategies also extend beyond landscape-level fuel management to property-level risk reduction. Individual property owners and community organizers can create and maintain defensible space around high-value structures by clearing nearby vegetation, thereby reducing the likelihood that fires spread to buildings (Gritzo, 2024; Meldrum et al., 2014; Shafran, 2008). Research consistently identifies defensible space as a primary, cost-effective tool for wildfire risk reduction on private property (Meldrum et al., 2014, 2018). Similarly, building codes that mandate fire-resistant materials in new construction can reduce structural vulnerability, although such policies operate over a longer time horizon.

Despite evidence supporting prevention-oriented strategies, funding allocations are often misaligned with their relative effectiveness. Dale and Barrett (2023) find that home hardening and mandatory building codes are among the most effective tools for reducing community wildfire risk, yet they remain underfunded. In contrast, wildfire suppression efforts receive substantial public resources despite being less effective at reducing long-term community risk. This imbalance suggests that policymakers may need to reevaluate funding priorities to better align expenditures with strategies that yield the greatest reductions in long-run wildfire vulnerability.

These considerations are particularly salient in the context of climate change. Rising temperatures and altered precipitation patterns create conditions more conducive to ignition and rapid fire spread (Abatzoglou and Williams, 2016; McGinnis et al., 2023). Climate projections indicate longer and more intense fire seasons and increasing simultaneity of large wildfire events across regions (Wasserman, 2020; McGinnis et al., 2023). As wildfire frequency and severity increase across the western United States and Canada (Dennison et al., 2014; Westerling, 2016; Donovan et al., 2023; Parisien et al., 2023), the economic risks faced by tourism-dependent regions are likely to intensify. Integrating wildfire mitigation into broader climate adaptation strategies—including economic diversification, year-round tourism development, and improved risk communication—may help reduce long-term vulnerability.

Overall, the evidence suggests that comprehensive wildfire management policies should combine prevention, targeted mitigation, and long-term adaptation planning. Given documented increases in large-fire activity across western North America, the persistent tourism and fiscal impacts identified in Colorado may characterize a broader set of wildfire-prone, tourism-dependent communities. Aligning funding with the most effective risk-reduction strategies may help limit the persistent economic disruptions associated with mega-fires in these regions.

2.6.2 Fiscal Implications

The results of this study can be employed to estimate the fiscal implications of large wildfires for local economies. Because the estimated average treatment effects (ATT) are expressed in per capita terms, the aggregate economic consequences of a large wildfire depend directly on county population size. Converting these per capita effects into dollar magnitudes provides clearer intuition about their fiscal relevance and highlights how wildfire impacts differ across counties with varying population bases and budgetary capacity.

To illustrate, consider Summit County, a popular county for tourism with a population of about 31,000 residents. Applying the estimated ATT for total visitor spending implies an average annual reduction of approximately $24 million in total tourism spending following a mega-fire. The corresponding decline in county-level tourism tax revenue is roughly $1.5 million per year, or about 3.6% of Summit County's 2024 general fund. While substantial in absolute terms, this loss is spread across a relatively large tax base and budget, providing greater fiscal flexibility to absorb wildfire-related shocks.

In contrast, the same per capita effects imply different fiscal consequences for a remote, rural county such as Mineral County, which has a population of just 794 residents. For Mineral County, the estimated ATT corresponds to an annual reduction of approximately $616,100 in total visitor spending and about $39,000 in county tourism tax revenue. Although modest in absolute terms, this tax loss represents nearly 10% of the county's 2024 general fund, which was approximately $400,000 at the beginning of 2024 according to publicly available budget records.

This comparison underscores an important policy implication. Smaller counties may experience lower aggregate revenue losses, yet the relative fiscal burden can be considerably larger due to limited budgetary capacity and reliance on tourism-generated revenues. Even moderate declines in tourism activity can therefore impose meaningful constraints on public service provision and long-term recovery in highly tourism-dependent rural communities.

2.6.3 Limitations

This study has several important limitations, which should be addressed by future research analyzing the effects of wildfire on tourism.

First, wildfire exposure is defined at the county level, with a county classified as treated if a wildfire intersects any portion of its boundary in a given year, regardless of the size of the overlap or the share of county land area affected. This binary treatment definition does not distinguish between counties that experience extensive burn coverage and those affected only marginally at their periphery. As a result, treatment intensity is measured imperfectly, which may attenuate estimated effects toward zero and mask heterogeneity in impacts across counties with differing degrees of exposure. The spillover robustness check (Section 2.5.4) partially addresses these concerns, but does not completely solve this limitation.

Secondly, the analysis relies on annual, county-level data, which necessarily aggregates over spatial and temporal variation. Within-county differences in wildfire exposure, tourism activity, and economic reliance on recreation are not observed, nor is the timing of fires relative to peak tourism seasons. This aggregation may obscure short-run disruptions or localized effects and limits the ability to identify how seasonality in tourism spending shapes wildfire responses.

Finally, while the empirical design controls for time-invariant county characteristics and common shocks, it does not account for potentially important spatial and contextual factors that may influence tourism responses to wildfire. These include proximity to major metropolitan areas, seasonal composition of tourism demand, accessibility and visibility of burn scars, and variation in transportation infrastructure or alternative recreational amenities. If these factors are correlated with wildfire exposure and tourism outcomes, they may contribute to residual unobserved heterogeneity in the estimated effects.

2.7 Conclusion

This paper examines the long-run effects of large wildfires on county-level tourism activity and tourism-generated tax revenues in Colorado. The analysis finds that large wildfires have a statistically and economically significant negative impact on total visitor spending, as well as on county- and state-level tax receipts generated from tourism. These effects are persistent, with estimated reductions lasting for up to a decade following a wildfire and no evidence of a subsequent rebound in spending. The duration and persistence of these impacts suggest that large wildfires impose long-term economic costs on affected communities well beyond the year of a large fire. As large-fire frequency continues to increase across western states, these findings have clear relevance for tourism-dependent regions throughout the region and even globally.

The decline in tourism spending following large wildfires is not uniform across different tourism accommodations. Declines in spending are driven primarily by decreases in hotel and motel expenditures, while campground spending does not exhibit a statistically significant reduction. This pattern suggests that wildfire events may disproportionately deter higher-spending visitors who rely on commercial lodging, while lower-cost, more flexible forms of recreation such as camping are less affected. Further research should explore whether similar patterns of wildfire's impacts on tourism are observed in other western states and Canada, or other countries and regions where forest-based tourism is prevalent.

Wildfire size plays a critical role in determining the magnitude of estimated economic effects. As the treatment definition becomes more restrictive, classifying only larger and more extreme fires as treatments, the estimated average treatment effects grow in both magnitude and statistical significance. The largest wildfires are associated with more immediate and more severe declines in tourism spending, consistent with the idea that high-intensity, large-scale fires cause lasting damage to forest amenities and local infrastructure. These findings are robust across multiple specifications, including models that exclude Front Range counties and those that drop counties exposed to multiple mega-fires, reinforcing confidence in the central results. Given documented increases in large-fire occurrence across much of North America, the concentration of economic losses among the largest fires has important implications for other wildfire-prone states and provinces experiencing similar shifts in fire regimes.

The analysis also shows limited evidence of spillover effects beyond directly affected counties. While some significant effects are observed when treatment is defined using broader buffer zones around wildfire boundaries, both the magnitude and statistical significance of the estimated economic disruptions to tourism are strongest within roughly 10 miles of the burn perimeter, but fade quickly with distance. This pattern suggests that the results primarily capture localized economic effects rather than broad regional shocks, a finding that may inform regional planning efforts in western states and other countries or regions confronting similar wildfire exposure.

An important avenue for future research is to examine how seasonality in tourism spending shapes the economic impacts of wildfires, particularly through the interaction between wildfire activity and snowpack conditions. The analysis in this study relies on annual data, which limits the ability to distinguish between short-run disruptions and seasonal shifts in tourism activity. This is relevant in light of anomalies observed in the 2016 treatment group, where strong snowpack conditions may have played a role in offsetting wildfire-related declines in tourism.

Future work should investigate whether favorable snow conditions in ski-dependent regions attenuate the negative effects of wildfire exposure, or more broadly, whether areas with a higher reliance on winter tourism are more resilient to wildfire impacts than those dependent on summer recreation. Access to higher-frequency data (e.g., monthly or seasonal tourism measures) would allow for a more precise evaluation of these dynamics and help disentangle the relative contributions of wildfire activity and snow conditions to local economic outcomes.

Future research should also examine how proximity to population centers and the salience of burn scars influence tourism responses. Wildfires that occur closer to major urban areas or in highly trafficked recreational regions may have larger and more immediate impacts due to increased visibility and media coverage, while more remote fires may generate weaker or more localized effects. Incorporating measures of accessibility and visibility would help refine our understanding of how wildfire exposure translates into changes in tourism demand and identify fires that pose the highest risk to local economies.

Overall, these results highlight the vulnerability of rural, mountain counties that rely heavily on tourism spending to support local economic activity and fund public services when exposed to large wildfire events. Large wildfires not only disrupt recreation and visitation in the short run but also generate persistent fiscal consequences for local governments with limited revenue bases. As wildfire risk escalates across the western United States, Canada, and other forested regions of the world, the economic vulnerabilities identified in Colorado are likely to characterize many other communities. The findings underscore the importance of investments in forest management strategies that reduce the risk of mega-fires, as well as the need for sustained post-fire recovery efforts aimed at restoring tourism demand and stabilizing local public finances in wildfire-prone regions.

Chapter 3

Wildfires and Drinking Water Quality: Evidence from Colorado Watersheds

This paper examines the relationship between wildfire exposure and Safe Drinking Water Act violations in Colorado between 2000-2023. Using watershed-level panel data, the analysis shows that wildfire exposure is associated with statistically significant increases in reported drinking water violations. Relatively small and moderately sized fires, those affecting only a few hundred to a few thousand acres, are associated with the most significant increases in violations. Turbidity violations in particular increase significantly following wildfire exposure. Back-of-the-envelope cost estimates suggest that these disruptions can impose substantial operational costs on drinking water utilities, particularly for large systems treating high daily volumes. Overall, the results highlight that even relatively small fires can have significant effects on drinking water systems and utility operations that persist beyond the year of a wildfire.

3.1 Introduction

As wildfires grow in size and frequency across the western United States, there is increasing concern about their cascading impacts on critical infrastructure, including drinking water systems (Dennison et al., 2014; Westerling, 2016; Wasserman, 2020; Weber and Yadav, 2020; Parisien et al., 2023; Pelletier et al., 2024). Wildfires can degrade watersheds through vegetation loss, erosion, and post-fire runoff, which may compromise downstream source water quality (Smith et al., 2011; Abraham et al., 2017; Brucker et al., 2025). These changes can increase the likelihood that public water systems experience Safe Drinking Water Act regulatory violations, presenting an important channel through which wildfires increase treatment costs and affect public health beyond the well-documented impacts of wildfire smoke (Allaire et al., 2018; Bayham et al., 2022; Alzahrani and Alhussain, 2025).

This paper investigates the relationship between wildfire exposure and the incidence of Safe Drinking Water Act violations in public water systems across Colorado. This study assembles a panel dataset covering HUC8 watersheds in Colorado from 2000 to 2023. Wildfire data come from the National Interagency Fire Center and include detailed spatial boundaries and fire years for all recorded wildfires over the study period within the state. Records of drinking water quality violations are sourced from the Colorado Department of Public Health and Environment, which tracks violations of drinking water standards under the Safe Drinking Water Act for public water systems across a range of chemical, metallic, and microbial contaminants.

The analysis examines how wildfire exposure affects the incidence of drinking water quality violations and whether increases in violations persist beyond the year of a wildfire. Fixed effect negative binomial count models are estimated using a watershed–year panel of Colorado data, where the outcome of interest is the number of reported Safe Drinking Water Act violations in each watershed–year. Wildfire exposure is measured using various bins of burned area and burn share. All specifications include watershed and year fixed effects to control for time-invariant differences across watersheds and common temporal shocks. Poisson models are estimated as a robustness check.

The results indicate that wildfire exposure is associated with statistically significant increases in reported drinking water quality violations. Notably, relatively small fires are associated with measurable increases in violations, suggesting that post-fire watershed disturbances can affect downstream water systems even when burned areas are limited. In contrast, larger wildfire bins do not consistently produce larger or more precisely estimated effects, highlighting the complexity of the relationship between fire size and downstream water quality outcomes. Turbidity violations, in particular, increase significantly in the year of a wildfire, consistent with post-fire erosion and runoff that elevate sediment loads in downstream water supplies (Hohner et al., 2019; Smith et al., 2011; Paul et al., 2022). Overall, the findings suggest that wildfire exposure can increase drinking water quality risks and treatment challenges, even when fires are relatively small.

3.2 Background

Forested watersheds play a critical role in supplying drinking water to communities across North America. Many public water systems rely on forested headwaters to provide high-quality source water because intact vegetation and soils regulate runoff, stabilize sediments, and limit the transport of contaminants into downstream water bodies (Emelko et al., 2011; Abraham et al., 2017). As a result, disturbances to forest ecosystems can have important implications for the reliability and safety of drinking water supplies (Smith et al., 2011). Maintaining the health of forested watersheds is therefore widely recognized as an important component of drinking water protection and water resource management (Emelko et al., 2011; Paul et al., 2022; Brucker et al., 2025).

At the same time, wildfire activity has increased substantially across the western United States since the late twentieth century due to a combination of historical fire suppression, increasing drought severity, and climate change (Abatzoglou and Williams, 2016; Bayham et al., 2022; Wasserman and Mueller, 2023). As wildfires grow larger and more frequent, they pose an increasing threat to forested headwaters that supply drinking water to downstream communities. Wildfires can dramatically alter watershed conditions by removing vegetation, destabilizing soils, and increasing erosion and runoff, ultimately affecting both the quantity and quality of downstream water resources (DeBano, 2000; Neary et al., 2005; Rhoades et al., 2019; Wibbenmeyer et al., 2023; Brucker et al., 2025). Understanding the relationship between wildfire activity and drinking water systems is therefore increasingly important for effective wildfire management and water resource planning.

From a policy perspective, these findings highlight the importance of wildfire mitigation and post-fire watershed management for drinking water system resilience. Wildfire exposure increases water quality violations and can impose substantial treatment and operational costs on utilities, affecting both large systems and smaller, resource-constrained providers. Investments that reduce burn severity, limit erosion, or accelerate watershed recovery may therefore yield benefits for both ecosystem health and public water systems (Emelko et al., 2011; Hohner et al., 2016; Warziniack et al., 2017). As wildfire risk continues to rise due to climate change (Abatzoglou and Williams, 2016; McGinnis et al., 2023; Wasserman and Mueller, 2023), incorporating water quality impacts into wildfire policy and infrastructure planning will be increasingly important in accounting for the economic impacts of wildfires.

3.2.1 Wildfire and Water Quality

Wildfires trigger a cascade of physical and chemical processes that can degrade downstream source water quality. The combustion of vegetation and organic soil layers removes protective ground cover and exposes soils to erosion, increasing the mobilization of sediment, nutrients, metals, and organic matter into nearby streams and reservoirs (Rhoades et al., 2011; Writer et al., 2014; Hohner et al., 2019; Smith et al., 2011; Abraham et al., 2017). In burned watersheds, concentrations of these constituents can increase by orders of magnitude relative to pre-fire conditions, and in some cases may exceed aquatic ecosystem thresholds or drinking water standards (Paul et al., 2022). Fire also alters soil structure and hydrologic processes by creating water-repellent soil layers, which increase surface runoff and accelerate erosion following precipitation events (DeBano, 2000; Neary et al., 2005).

These physical changes interact with post-fire precipitation to generate episodic pulses of sediment, nutrients, and dissolved organic carbon (DOC), particularly during high-flow events. Such events can sharply increase turbidity and organic loading in surface waters, especially when they immediately follow wildfire activity (Hohner et al., 2016, 2019; Paul et al., 2022; Brucker et al., 2025). While many of these effects diminish over time, elevated concentrations of sediment, nutrients, and organic matter frequently persist for multiple years, with some studies documenting impacts lasting up to a decade or more (Paul et al., 2022; Brucker et al., 2025). In addition, wildfire-induced changes to soil chemistry—including increased aromaticity of soil organic matter—can persist long after the initial burn, further influencing the composition and treatability of downstream water supplies (Chen et al., 2020).

A growing body of research documents these impacts across forested watersheds in the western United States. Field studies consistently find large post-fire increases in sediment, nutrient, and trace element fluxes in streams draining burned catchments (Smith et al., 2011; Paul et al., 2022; Zhang et al., 2025). In some cases, concentrations of metals, nutrients, and organic compounds increase by several orders of magnitude relative to pre-fire conditions and may exceed aquatic ecosystem thresholds or drinking water standards (Paul et al., 2022; Brucker et al., 2025). These changes are often most pronounced during post-fire precipitation events, when runoff mobilizes ash, sediment, and organic material into surface waters, generating short-run spikes in turbidity and contaminant concentrations.

There is evidence that shows wildfire-induced water quality degradation persists for multiple years. Elevated levels of organic carbon and phosphorus have been observed for one to five years following fire, while increases in sediment and nitrogen can persist for up to eight years in affected watersheds (Brucker et al., 2025). One study found that most water quality impacts last less than five years on average, but can extend for a decade or more in severely burned systems (Paul et al., 2022). Importantly, these effects are not limited to the largest wildfire events. Even relatively small fires and prescribed burns have been linked to measurable declines in water quality through increased runoff, erosion, and changes in soil and nutrient dynamics (Brucker et al., 2025; Zhang et al., 2025).

Case studies from Colorado illustrate these dynamics. The Hayman Fire (2002) and the High Park Fire (2012) both produced substantial increases in sediment loads, nutrient concentrations, and dissolved organic carbon in downstream water supplies, highlighting the vulnerability of mountain headwater systems to wildfire disturbances (Rhoades et al., 2011; Writer et al., 2014).

These changes in source water quality can create significant operational challenges for drinking water treatment facilities. Elevated turbidity and dissolved organic carbon (DOC) reduce treatment efficiency and increase the amount of chemical treatment required to produce safe drinking water (Emelko et al., 2011; Hohner et al., 2016). Evidence from post-fire watersheds shows substantial increases in turbidity and DOC relative to unburned conditions. For example, Emelko et al. (2011)

find that turbidity levels in burned basins are approximately three times higher than in unburned reference watersheds during high-flow periods, with corresponding increases in organic carbon concentrations.

Wildfire-affected source water can also alter the composition of dissolved organic matter, increasing the formation potential of disinfection byproducts (DBPs) such as trihalomethanes and haloacetonitriles during treatment processes (Hohner et al., 2016; Jones et al., 2022). Following wildfire events, treatment facilities have been shown to require higher chemical inputs to remove DOC, while also experiencing elevated DBP formation, particularly during post-storm runoff events (Hohner et al., 2016). These dynamics increase the likelihood that utilities will face compliance challenges with turbidity regulatory standards, particularly in the months immediately following wildfire exposure.

Changes in source water quality translate directly into higher operating costs for drinking water systems. Evidence shows that increases in turbidity and organic carbon are associated with higher treatment expenditures, with a 1% increase in turbidity raising treatment costs by approximately 0.19% and a 1% increase in organic carbon increasing costs by roughly 0.46% (Warziniack et al., 2017). As a result, utilities drawing from wildfire-affected watersheds often face both short-run operational disruptions and sustained increases in treatment costs, and the need to modify treatment processes following wildfire events (Emelko et al., 2011; Warziniack et al., 2017; Zhang et al., 2025).

Wildfires can also damage drinking water infrastructure and contaminate distribution systems, particularly in wildland–urban interface areas. During the Camp Fire in California, system depressurization allowed smoke and pyrolyzed materials to enter service lines and distribution pipes, introducing volatile organic compounds into drinking water supplies (Solomon et al., 2021). Post-fire investigations have documented widespread contamination in affected systems, including the presence of benzene and other hazardous compounds, in some cases necessitating “Do Not Use” advisories and extensive system flushing or replacement (Proctor et al., 2020). These impacts represent an additional pathway through which wildfires can disrupt drinking water systems, particularly in communities directly exposed to fire damage.

This literature demonstrates that wildfires can degrade water quality through multiple pathways, creating both environmental and infrastructure risks for drinking water systems. As wildfire activity increases across the western United States, understanding the implications of these disturbances for water supply reliability and water treatment systems has become an increasingly important policy concern (Wibbenmeyer et al., 2023).

3.2.2 Economic and Social Impacts of Water Quality

There are several studies on the economic value of water quality. Cutler and Miller (2005) finds that improvements in urban water systems in the early twentieth century resulted in large reductions in infant and child mortality in US cities, highlighting the health benefits associated with access to clean drinking water. Similar evidence from developing countries suggests that improvements in water and sanitation infrastructure can generate large welfare gains and increases in life expectancy (Olmstead, 2010). Survey studies show that individuals are willing to pay significant amounts for improvements in tap water quality and reliability, particularly when concerns about health risks or unpleasant taste and odor are salient (Carson and Mitchell, 1993; Chatterjee et al., 2017).

Economic impacts can also be measured through the operational costs faced by water utilities. Degraded source water quality increases treatment requirements and raises operating costs for drinking water systems. For example, increases in sediment loads from upstream land disturbances have been shown to increase treatment costs for downstream water utilities (Holmes, 1988). Other studies have found that increases in turbidity and organic carbon in source waters are associated with measurable increases in costs at treatment facilities (Heberling et al., 2015; Warziniack et al., 2017). Other studies estimate the value of water quality improvements through avoidance behavior, finding that households who perceive tap water to be unsafe or unpleasant often substitute toward alternatives such as bottled water, revealing the implicit costs they are willing to incur to avoid poor water quality (Zivin et al., 2011; Chatterjee et al., 2017).

Despite a rich literature documenting how wildfires degrade watershed conditions and water quality, relatively little economic research has examined the downstream impacts on drinking water systems (Jones et al., 2017; Wibbenmeyer et al., 2023). This gap persists even though wildfire-related watershed damage can impose meaningful costs on water utilities and communities (Bayham et al., 2022; Warziniack and Thompson, 2013). This study contributes to that emerging literature by examining whether wildfire exposure is associated with increases in Safe Drinking Water Act violations across Colorado watersheds from 2000 to 2023, providing new evidence on a potentially overlooked economic consequence of increasing wildfire activity.

3.2.3 Wildfire and Water in Colorado

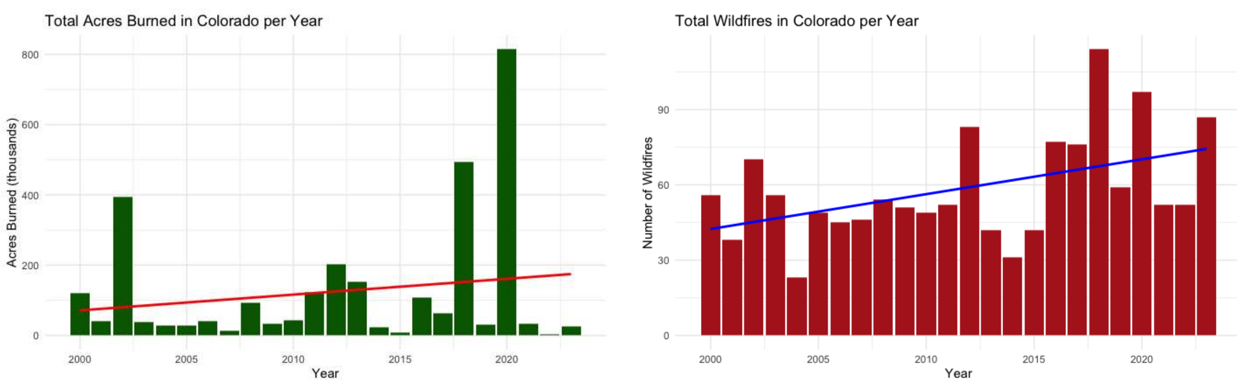
Colorado provides a useful setting for examining the relationship between wildfire activity and drinking water systems. Wildfire trends in Colorado mirror broader patterns observed across the western United States, where both the frequency and area burned by large fires have increased significantly since the late twentieth century (Westerling, 2016; Dennison et al., 2014; Bayham et al., 2022). Across western forests, fire seasons have lengthened and annual burned area has risen in recent decades, driven in part by rising temperatures, earlier snowmelt, and increasing drought severity (Wasserman, 2020; Wasserman and Mueller, 2023). Similar trends have been observed in Colorado, where wildfire activity has intensified and exceptionally large fires have become more common in the twenty-first century.

Several recent fires illustrate the scale of this change. Notable examples include the Hayman Fire (2002), the High Park Fire (2012), and the Spring Creek Fire (2018), as well as the historic 2020 fire season, which produced the three largest wildfires in Colorado's recorded history: the Cameron Peak, East Troublesome, and Pine Gulch fires. These fires have affected large forested landscapes and, in several cases, burned within watersheds that supply drinking water to downstream communities.

Wildfire risks to water systems are particularly important in Colorado because many public water systems rely on mountain headwaters for their source water. Severe burns can destabilize soils, remove vegetation, and increase erosion, leading to elevated sediment loads and chemical contamination in downstream waterways. Following the Hayman Fire, for example, Denver Water invested millions of dollars in watershed restoration to prevent sediment from clogging reservoirs and treatment infrastructure. Similar concerns emerged after the High Park and Cameron Peak fires, when water providers in Fort Collins, Colorado implemented emergency measures to protect intake systems from elevated turbidity, nutrient loading, and heavy metal contamination (Rhoades et al., 2011; Writer et al., 2014).

Evidence of increasing wildfire activity in Colorado can be seen in Figure 3.1, which show trends in total acres burned and the number of wildfires per year since 2000. Both the frequency of wildfire events and the total area burned have increased over this period, reflecting broader regional trends in wildfire activity.

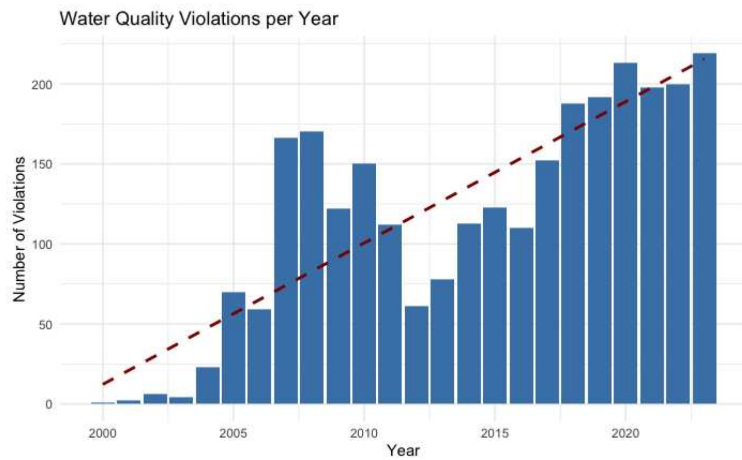
Figure 3.1: Trends in Colorado wildfires, 2000–2023. Total acreage burned per year is shown on the left, and the number of fires per year is shown on the right.



At the same time, reported drinking water quality violations have also increased across the state. Figure 3.2 illustrates the upward trend in annual Safe Drinking Water Act violations reported by public water systems in Colorado. While a variety of factors may contribute to this pattern, the increasing overlap between wildfire activity and drinking water watersheds raises important questions about whether wildfire exposure contributes to these violations.

Trends in Colorado are representative of broader changes in fire activity across North America. In Canada, four of the most severe wildfire seasons of the past century have occurred within the

Figure 3.2: Statewide Water Quality Violations by Year



last decade, including the unprecedented 2023 season that set new records for total area burned (Parisien et al., 2023; Pelletier et al., 2024). Similar increases in large wildfire occurrence have also been documented in parts of the eastern United States (Donovan et al., 2023).

These trends highlight the importance of understanding how wildfire exposure affects drinking water systems. While previous studies have documented the short-run impacts of individual fires on watershed conditions, relatively little research has examined whether wildfire activity contributes to broader patterns in regulatory drinking water violations. The analysis in this study addresses this gap by examining how wildfire exposure across Colorado watersheds is associated with changes in Safe Drinking Water Act violations from 2000 to 2023.

3.3 Data

3.3.1 Wildfire Data

Wildfire exposure is measured using wildfire perimeter shapefiles from the National Interagency Fire Center (NIFC). These shapefiles provide spatial information on the location, extent, and year of all reported wildfires in Colorado over the study period. Fire perimeters are used to calculate total burned area within each watershed in each year, allowing wildfire exposure to be measured both in absolute terms (acres burned) and relative to watershed size (burn share).

To construct watershed-level wildfire exposure measures, wildfire perimeters are spatially intersected with HUC8 hydrologic unit boundaries sourced from the United States Geological Survey. For each watershed–year observation, total acreage burned is calculated by all fires overlapping the watershed in that year. In cases where multiple fires occur within a watershed in a single year, burned area is aggregated across events. Burn share is computed as the ratio of total burned acreage to total watershed area.

3.3.2 Watershed Boundaries

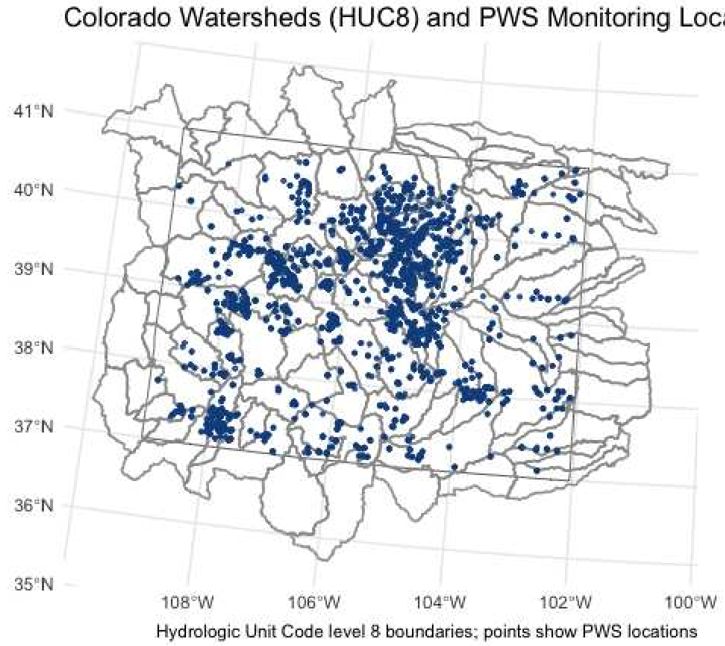
Watershed boundaries are defined using Hydrologic Unit Code (HUC) shapefiles obtained from the U.S. Geological Survey (USGS). The analysis is conducted at the HUC8 (eight-digit hydrologic unit) level. Each HUC8 watershed defines a drainage basin in which precipitation falling within the watershed boundary drains to a common outlet, such as a river confluence or reservoir. These units provide a natural geographic boundary for studying how landscape disturbances affect downstream water systems.

The HUC8 level provides a balance between spatial resolution and sufficient variation in wildfire exposure and water quality outcomes. Smaller watershed units may capture finer hydrologic variation but would result in sparse wildfire exposure for many observations, while larger watershed units may obscure localized wildfire effects. HUC8 watersheds therefore provide a practical intermediate scale for linking wildfire activity to downstream water quality outcomes.

These watershed boundaries are fixed over time and serve as the spatial units for linking wildfire exposure to drinking water quality violations.

All wildfire exposure measures are assigned to watersheds based on spatial overlap between fire perimeters and watershed polygons, without conditioning on the specific location of fires within a watershed or upstream–downstream flow paths. Watershed boundaries and the locations of drinking water monitoring stations are shown in Figure 3.3.

Figure 3.3: Colorado Watersheds and Drinking Water Monitoring Stations



Notes: The map displays watershed boundaries used in the analysis along with the locations of drinking water monitoring stations. Each station corresponds to a public water system monitoring location used to record drinking water quality violations.

3.3.3 Water Quality Violations

Public Water Systems (PWSs) in the United States are regulated under the Safe Drinking Water Act and are required to monitor drinking water quality for a range of contaminants using rule-specific sampling protocols. Monitoring frequency varies by contaminant: some measures, such as turbidity levels for surface water systems, are measured nearly continuously or at high frequency, while others, including some chemical contaminants and disinfection byproducts, are sampled on a more periodic daily or weekly basis. Violations occur when measured contaminant levels exceed federal regulatory thresholds. (U.S. Environmental Protection Agency, 2023a,b).

Drinking water quality outcomes are measured using violation data reported by public water systems to the Colorado Department of Public Health and Environment (CDPHE). The dataset includes all reported violations of federal drinking water standards under the Safe Drinking Water Act from 2000 to 2023, along with information on violation type and reporting year. Violations are aggregated to the watershed–year level based on the geographic location of the reporting public water system.

The primary outcome variable is the total number of reported water quality violations in each watershed–year. In addition, violation counts are analyzed for turbidity, which is the most frequently reported violation category and is commonly linked to wildfire-related sediment and runoff impacts in previous studies (Emelko et al., 2011; Hohner et al., 2016; Warziniack et al., 2017).

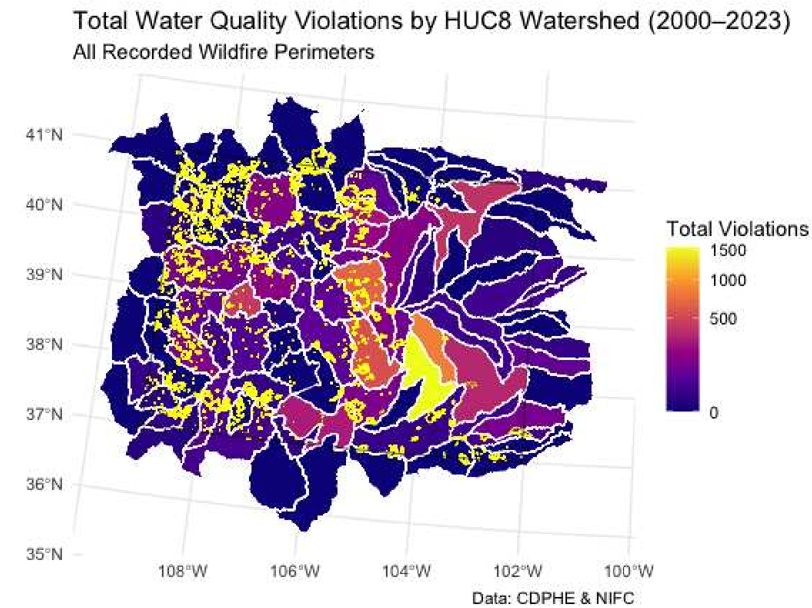
3.3.4 Panel Construction

The final dataset is organized as an watershed–year panel covering Colorado watersheds from 2000 to 2023. Each observation corresponds to a single watershed–year and includes measures of wildfire exposure, Safe Drinking Water Act violations, and watershed characteristics. Watersheds with no wildfire exposure or no reported violations in a given year make up a large share of the final panel.

All variables are measured at the annual level. Wildfire exposure variables capture fires occurring within the calendar year, while violation counts reflect reported violations during the same year. Lagged wildfire exposure measures are constructed to assess whether wildfire impacts persist beyond the year of the burn.

Figure 3.4 illustrates the spatial distribution of drinking water quality violations across Colorado watersheds alongside the location of wildfire perimeters during the sample period. Watersheds are shaded according to the total number of reported violations, while wildfire boundaries are overlaid to provide visual context for where wildfire exposure occurs.

Figure 3.4: Drinking Water Violations and Wildfire Perimeters in Colorado Watersheds



Notes: Watersheds are shaded according to the total number of reported drinking water violations during the sample period. Wildfire perimeters are overlaid to illustrate the spatial distribution of wildfire exposure across watersheds.

3.3.5 Summary Statistics

Table 3.1 reports summary statistics for the watershed–year panel. The final sample includes annual observations for 92 HUC8 watersheds across Colorado between 2000–2023, for a total of 2,208 watershed–year observations. HUC8 watersheds have a mean area of just over one million acres. Wildfire exposure is skewed: while the mean burned area is 1,336 acres per watershed–year, there is a large variance in burned acreage of over 9,000 acres, with many observations showing no wildfire activity. Drinking water quality violations are relatively infrequent on average but highly dispersed, with a mean of 3.34 total violations per watershed–year. Turbidity violations occur less frequently but also exhibit substantial variability across watersheds and over time.

Watersheds are grouped by both burned area and burn share into bins based on the empirical distribution of wildfire exposure across watershed–year observations. Bin thresholds are chosen to reflect meaningful differences in wildfire severity while ensuring an adequate number of observations in each category for separate estimation. This approach allows the analysis to capture

Table 3.1: Summary Statistics: HUC8 Watersheds

	Count	Mean	SD	Min	Max
Watershed Characteristics					
HUC8 area (acres)		1,064,735	531,830	365,027	2,468,598
Burned area (acres)		1,336	9,214	0	235,994
Burn share (%)		0.118	0.737	0	13.17
Water Quality Outcomes					
Total violations	2732	3.34	11.75	0	158
Turbidity violations	427	0.45	2.78	0	44
Wildfire Statistics					
Fire size (acres)	1472	2,007.40	11,822.73	0.03	208,913

Notes: Watershed variables are measured at the watershed–year level. Wildfire statistics summarize individual wildfire events between 2000 and 2023. Burn share is reported as a percentage.

nonlinear responses to wildfire exposure without relying on extreme values or sparsely populated ranges of the data.

Table 3.2: Distribution of Wildfire Exposure Across Watershed–Years

Burned Area (acres)

Bin	Obs.	%
0	1,521	68.9
<300	330	14.76
300–2k	193	8.7
2k–7.5k	93	4.2
7.5k–25k	41	1.9
25k+	30	1.4

Burn Share of Watershed Area

Bin	Obs.	%
0	1,521	67.9
<0.01%	226	10.2
0.01–0.1%	237	10.7
0.1–0.25%	80	3.6
0.25–0.5%	45	2.0
0.5–0.75%	26	1.2
0.75–1%	17	0.8
1%+	57	2.6

Notes: Counts reflect HUC8 watershed–year observations from 2000–2023. Percentages denote the share of the full panel.

Table 3.2 summarizes the distribution of wildfire exposure across watershed–year observations. The majority of watershed–years experience no wildfire activity, while positive exposure is relatively rare and highly skewed. Most nonzero fire events involve small burned areas or very low burn shares, though a small number of watershed–years experience large fires or substantial portions of watershed area burned. This skewed distribution motivates the use of binned exposure measures in the empirical analysis. Section 3.5.3 addresses the skewness of the data and replicates the analysis using bins with equal numbers of observations in each bin.

3.4 Empirical Approach

The outcome of interest in this paper is the number of reported drinking water quality violations in a watershed in a given year. Because violations are non-negative count outcomes and exhibit overdispersion, the analysis employs a negative binomial specification. Coefficients are reported as incidence rate ratios (IRRs), which represent proportional changes in expected violation counts associated with wildfire exposure.

Wildfire exposure varies substantially across watersheds and over time and is highly skewed, with most watershed–year observations experiencing no fire activity and a small number affected by larger events. To capture potential nonlinear responses, wildfire exposure is grouped into bins based on the distribution of exposure across watershed–years. Two measures of exposure are used: total burned area within a watershed (in acres) and the share of total watershed area burned in a given year (percentage of area burned). Bin thresholds are selected to reflect meaningful differences in fire magnitude while maintaining sufficient observations within each category, and alternative bin definitions are employed to assess the robustness of the results.

The baseline specification is shown in Equation 3.1.

$$\mathbb{E}[Y_{wt} \mid X_{wt}] = \exp\left(\beta_1 \text{Fire}_{wt} + \beta_2 \text{Fire}_{w,t-1} + \alpha_w + \gamma_t\right). \tag{3.1}$$

where Y_{wt} denotes the number of drinking water quality violations observed in watershed w during year t. The variables Fire_{wt} and $\text{Fire}_{w,t-1}$ are indicators for wildfire exposure in the current and previous year, respectively, defined using either burned area bins or burn share bins. Watershed fixed effects (α_w) control for time-invariant differences across watersheds, and year fixed effects (γ_t) control for common temporal shocks.

The model is estimated using a negative binomial specification to account for overdispersion in violation counts. In the data, the mean number of violations per weatershed-year is 3.34, while the standard deviation is 11.75, indicating substantial dispersion relative to the mean. This violates the equidispersion assumption of the Poisson model. The negative binomial model relaxes this restriction by allowing the variance to exceed the mean, making it more appropriate for modeling highly skewed count data such as water quality violations.

In the negative binomial specification, coefficients are estimated in log terms and are therefore not directly interpretable in levels. To make estimates interpretable, all estimates are reported as incidence rate ratios (IRRs), obtained by exponentiating the estimated coefficients. An IRR greater than one indicates that wildfire exposure is associated with a proportional increase in the expected number of drinking water quality violations relative to the reference category, while an IRR less than one indicates a proportional decrease. Standard errors are clustered at the watershed level to account for within-watershed correlation over time.

Poisson models are estimated as a robustness check; these results are discussed in depth in Appendix B.1. Although statistical significance is generally weaker in the Poisson specifications, the estimated effects are similar in magnitude and direction to the baseline negative binomial models, suggesting the main results are not driven by the choice of count model.

3.5 Results

3.5.1 Wildfire Burn Size and Drinking Water Violations

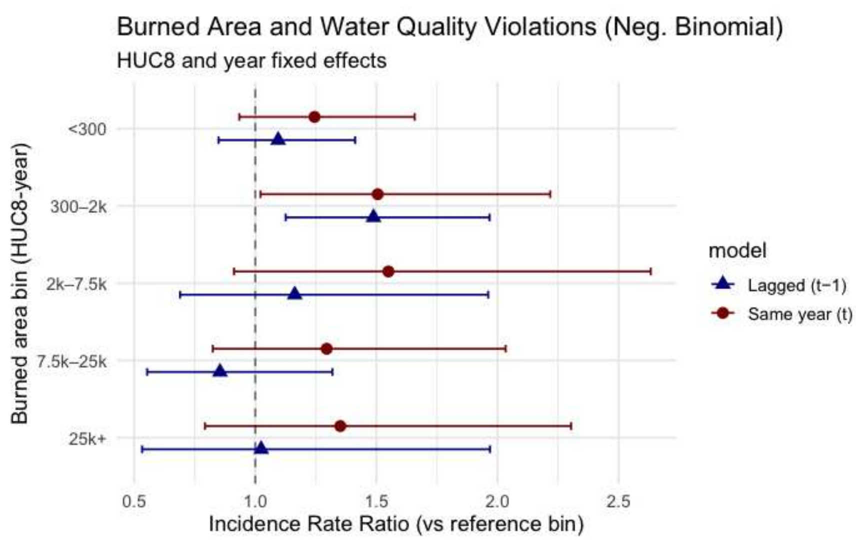
The results from estimating Equation 3.1 using burned area as the measure of wildfire exposure are reported in Table 3.3. The estimated incidence rate ratios are positive across nearly all wildfire size categories. However, statistically significant effects are only seen in the 300–2,000 acre bin. Larger wildfire bins also exhibit positive point estimates, but these effects are insignificant. Column (1) reports estimates from a specification including only contemporaneous wildfire exposure, while column (2) includes both current-year and one-year-lagged wildfire measures. Graphical representations of the estimated coefficients are shown in Figure 3.5.

Table 3.3: Wildfire Burned Area and Drinking Water Quality Violations (Negative Binomial)

	(1)	(2)	
Burned Area Bin	**t**	**t**	**t−1**
< 300	**1.18**	**1.20**	**1.09**
	[0.88, 1.58]	[0.91, 1.60]	[0.85, 1.41]
300–2k	**1.50***	**1.48***	**1.49****
	[1.02, 2.19]	[1.01, 2.18]	[1.13, 1.96]
2k–7.5k	**1.60**	**1.53**	**1.16**
	[0.90, 2.85]	[0.90, 2.60]	[0.69, 1.97]
7.5k–25k	**1.20**	**1.28**	**0.86**
	[0.77, 1.85]	[0.82, 2.00]	[0.56, 1.33]
25k+	**1.38**	**1.33**	**1.02**
	[0.78, 2.44]	[0.78, 2.28]	[0.53, 1.97]
Watershed FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: IRRs shown with 95% confidence intervals in brackets. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Reference category: 0 burned acres.

Figure 3.5: Baseline Negative Binomial Estimates: Wildfire Burned Area (Total Violations)



Notes: Points show incidence rate ratios (IRRs) from negative binomial models including burned area in years t and $t-1$. Horizontal bars denote 95% confidence intervals. The vertical line at IRR$= 1$ indicates no effect. The reference category is zero burned acres.

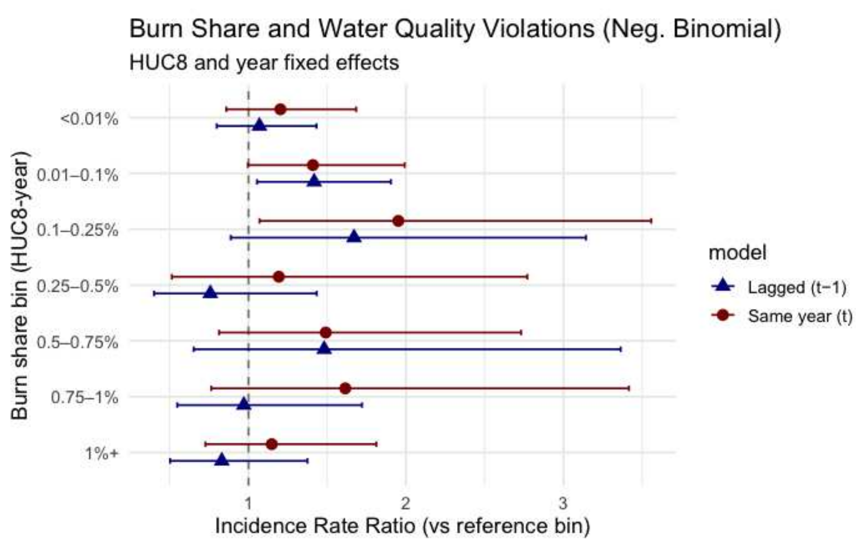
The estimated effects vary across wildfire size categories. The smallest fire bin (<300 acres) is not associated with statistically significant changes in drinking water violations in either the contemporaneous or lagged specifications. In contrast, fires in the 300–2k acre range are associated with statistically significant increases in violations both in the year of the burn and in the subsequent year. The estimated increase is approximately 48% contemporaneously (95% CI: 1–118%) and 49% in the following year (95% CI: 13–96%).

The results for the bins corresponding to larger fires yield positive IRR estimates; however, these effects are not statistically significant across the three largest burn categories in either the contemporaneous or lagged specifications.

3.5.2 Wildfire Burn Share and Drinking Water Violations

The results from estimating Equation 3.1 using burn share as the measure of wildfire exposure are reported in Table 3.4. Burn share captures wildfire intensity relative to watershed size and provides a complementary perspective to total burned area. As with the burned area analysis, the estimated effects vary across exposure groups, with only smaller share bins showing significant increases, indicating a nonlinear relationship between wildfire severity and drinking water quality violations. Graphical representations of the estimated coefficients and confidence intervals can be found in Figure 3.6.

Figure 3.6: Baseline Negative Binomial Estimates: Wildfire Burn Share (Total Violations)



Notes: Points show incidence rate ratios (IRRs) from negative binomial models including burn share in years t and $t-1$. Horizontal bars denote 95% confidence intervals. The vertical line at IRR$=1$ indicates no effect. The reference category is zero burn share.

Table 3.4: Wildfire Burn Share and Drinking Water Quality Violations (Negative Binomial)

	(1)	(2)	
Burn Share Bin	**t**	**t**	**t−1**
<0.01%	**1.24**	**1.20**	**1.07**
	[0.88, 1.74]	[0.86, 1.68]	[0.80, 1.43]
0.01–0.1%	**1.38**	**1.41**	**1.42***
	[0.99, 1.94]	[1.00, 1.99]	[1.05, 1.90]
0.1–0.25%	**1.91***	**1.95***	**1.67**
	[1.07, 3.42]	[1.07, 3.56]	[0.89, 3.14]
0.25–0.5%	**1.16**	**1.19**	**0.76**
	[0.47, 2.84]	[0.51, 2.77]	[0.40, 1.43]
0.5–0.75%	**1.34**	**1.49**	**1.48**
	[0.74, 2.41]	[0.81, 2.73]	[0.65, 3.36]
0.75–1%	**1.60**	**1.62**	**0.97**
	[0.86, 2.97]	[0.76, 3.42]	[0.55, 1.72]
1%+	**1.09**	**1.15**	**0.83**
	[0.69, 1.71]	[0.73, 1.81]	[0.50, 1.37]
Watershed FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: IRRs shown with 95% confidence intervals in brackets. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Reference category: 0 burned share.

Very small burn shares (below 0.01% of watershed area) are not associated with statistically significant changes in drinking water violations in either the contemporaneous or lagged specifications. The next category, burn shares between 0.01–0.1%, shows positive effects in both periods, with a statistically significant increase of roughly 42% in violation rates appearing in the year following the fire, showing that even relatively small disturbances to a watershed can produce measurable downstream impacts on drinking water systems.

Burn shares between 0.1–0.25% are associated with the largest contemporaneous effects in the table. In this range, wildfire exposure increases violation rates by roughly 90–95% in the year of the fire, although the lagged estimate is positive but not significant. For larger burn share categories, the estimated effects remain positive in several cases but are insignificant across the four largest bins. Burn shares above 0.25% do not show statistically significant impacts in either the contemporaneous or lagged specifications. As in the burned area analysis, these results indicate that significant increases in drinking water violations are concentrated in relatively small wildfire exposure bins rather than the largest fire categories.

3.5.3 Quintile Bin Analysis

To address the skewed distribution of wildfire exposure, the analysis also employs five, equal-sized bins constructed from the distribution of watershed–year observations. Unlike the threshold-based bins used previously, this approach partitions the sample into groups containing nearly equal numbers of observations, reducing the possibility that estimated effects are driven by sparsely populated exposure categories. Alternative specifications using three and seven equal bins are reported in Appendix B.2. These results confirm that the central findings are not sensitive to the choice of bin count. The acreage cutoff thresholds are located in Table 3.5.

Table 3.5 reports the burn acreage cutoffs used to construct the five quintile burned area bins. Each bin contains approximately the same number of watershed–year observations with positive wildfire exposure. The lower and upper bounds define the range of total acres burned within a watershed for each quintile of the distribution. As shown, the distribution is highly skewed: the

Table 3.5: Equal-Bin Burned Area Summary

Bin	Low (Acres)	High (Acres)	Obs.	Mean Burned Acreage	Mean Violations
0	0	0	1521	0.00	1.06
Q1	0.03	34.51	138	14.23	1.57
Q2	34.51	169.52	137	84.99	1.22
Q3	169.52	714.96	137	367.07	1.07
Q4	714.96	2,983.71	137	1,464.97	0.91
Q5	2,983.71	235,993.76	138	19,494.85	1.34

Notes: Burned area bins are based on quintiles of the empirical distribution of positive watershed–year burned area observations. N denotes the number of watershed–year observations in each bin.

lowest quintile includes very small burn totals (up to roughly 35 acres), while the upper quintile spans a wide range of larger events (3,000-236,000 acres), reflecting the relatively small number of watershed–years affected by very large fires.

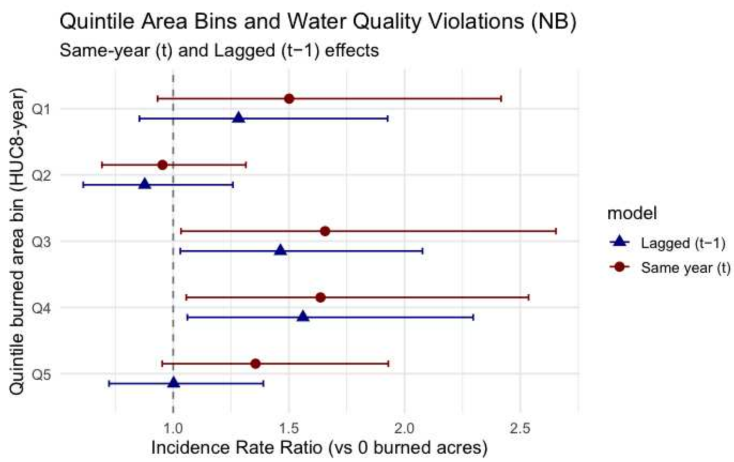
Table 3.6: Wildfire Burned Area (Equal Five-Bin Specification) and Drinking Water Quality Violations (Negative Binomial)

	(1)	(2)	
Burn Quintile	**t**	**t**	**t−1**
Q1	**1.48**	**1.50**	**1.28**
	[0.94, 2.33]	[0.93, 2.42]	[0.85, 1.93]
Q2	**0.91**	**0.95**	**0.88**
	[0.65, 1.28]	[0.69, 1.31]	[0.61, 1.26]
Q3	**1.57**	**1.66***	**1.46***
	[1.00, 2.45]	[1.03, 2.65]	[1.03, 2.08]
Q4	**1.59***	**1.64***	**1.56***
	[1.04, 2.42]	[1.06, 2.54]	[1.06, 2.30]
Q5	**1.36**	**1.36**	**1.00**
	[0.94, 1.95]	[0.95, 1.93]	[0.72, 1.39]
Watershed FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: IRRs shown with 95% confidence intervals in brackets. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Reference category: no wildfire exposure.

Using the quintile cutoffs reported in Table 3.5, the five-bin specification provides additional evidence that relatively small to moderately sized fires (170–3000 acres in size) lead to significant increases in water quality violations. In the specification including only contemporaneous exposure, most quintiles are not statistically significant, with the exception of Q4, which corresponds to watershed–years experiencing between approximately 715 and 2,984 burned acres. For this group, violations increase by roughly 59% in the year of the fire (IRR = 1.59; 95% CI: 1.04–2.42).

Figure 3.7: Negative Binomial Estimates: Wildfire Exposure (Quintile Specification, Total Violations)



Notes: Points show incidence rate ratios (IRRs) from negative binomial models using wildfire exposure quintiles and including exposure in years t and $t-1$. Horizontal bars denote 95% confidence intervals. The vertical line at IRR$=1$ indicates no effect. The reference category is no wildfire exposure.

When including both contemporaneous and lagged exposure, both Q3 (approximately 170–715 acres) and Q4 (715–2,984 acres) exhibit significant increases in water quality violations. The contemporaneous effects are similar in magnitude across these bins, with violations increasing by about 66% (Q3) and 64% (Q4). In the subsequent year, Q3 and Q4 remain statistically significant, with estimated increases of approximately 46% and 56%, respectively. In contrast, the largest quintile (Q5), representing watershed–years affected by fires exceeding roughly 3,000 acres, does not exhibit statistically significant increases in violations in either specification. Graphical representations of these estimates are seen in Figure 3.7

3.5.4 Zero Inflated Negative Binomial Robustness

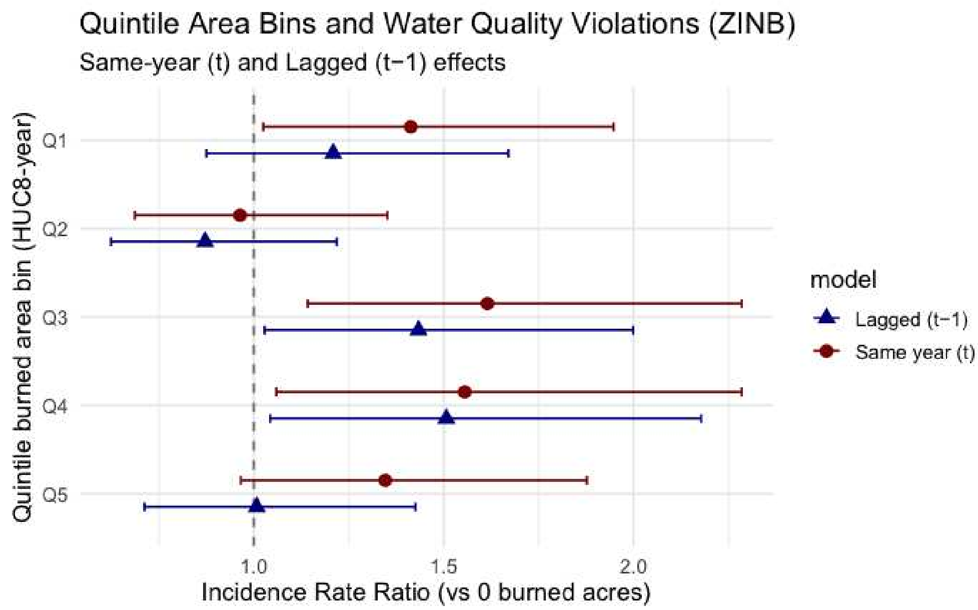
A zero-inflated negative binomial (ZINB) model is used as a robustness check to account for the high prevalence of zero outcomes in the data. Approximately 78% of watershed-year observations report zero drinking water violations, indicating a highly skewed distribution with excess zeros. In this context, some zeros may reflect "structural" conditions (watersheds where violations are unlikely or effectively impossible due to limited monitoring, system characteristics, or consistently high water quality) rather than random variation. The ZINB model addresses this by combining two processes: a binary model that estimates the probability an observation is always zero, and a count model (negative binomial) that estimates violation counts among observations at risk of experiencing violations. This approach effectively excludes observations that are structurally unlikely to exhibit violations, allowing the estimated effects of wildfire exposure to be driven by variation among watersheds where violations are plausibly observed. The results of the quintile analysis using a zero-inflated negative binomial specification are found in Table 3.7 and Figure 3.8.

Table 3.7: Wildfire Exposure (Equal Five-Bin Specification) and Water Quality Violations (ZINB)

	t	**t−1**
Q1 (<35 acres)	**1.41***	**1.21**
	[1.03, 1.95]	[0.88, 1.67]
Q2 (35–170 acres)	**0.96**	**0.87**
	[0.69, 1.35]	[0.62, 1.22]
Q3 (170–715 acres)	**1.61****	**1.43***
	[1.14, 2.28]	[1.03, 2.00]
Q4 (715–2,984 acres)	**1.56***	**1.51***
	[1.06, 2.28]	[1.04, 2.18]
Q5 (2,984–235,994 acres)	**1.35**	**1.01**
	[0.97, 1.88]	[0.71, 1.43]
Watershed FE	Yes	Yes
Year FE	Yes	Yes

Notes: IRRs shown with 95% confidence intervals in brackets. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Reference category: no wildfire exposure.

Figure 3.8: Wildfire Exposure and Water Quality Violations (ZINB)



Notes: Points represent incidence rate ratios (IRRs) from a zero-inflated negative binomial model, with 95% confidence intervals. Results are shown separately for contemporaneous effects (t) and one-year lagged effects ($t-1$). The dashed horizontal line at 1 indicates no effect. Bins correspond to quintiles of burned acreage. Reference category: no wildfire exposure.

The results from the zero-inflated negative binomial model are broadly consistent with those from the baseline negative binomial specification, as seen in Table 3.8. Across both models, statistically significant effects are observed in the middle burn bins (Q3 and Q4), reinforcing the finding that small to moderately sized fires drive meaningful increases in drinking water violations. The magnitude of these effects is slightly attenuated in the ZINB model, with contemporaneous violations increasing by approximately 56–61% compared to 64–66% under the negative binomial specification. Lagged effects for these bins remain positive and statistically significant across both models. The primary difference between the specifications is that the ZINB model detects a significant increase in violations for the smallest burn bin (Q1), whereas the negative binomial model does not, albeit with similar point estimates. Overall, the consistency in both the pattern and magnitude of results suggests that the main findings are not driven by excess zeros in the data.

Table 3.8: Wildfire Exposure and Drinking Water Violations: Negative Binomial vs. ZINB

	Negative Binomial		ZINB	
Burn Quintile	**t**	**t−1**	**t**	**t−1**
Q1	**1.50**	**1.28**	**1.41***	**1.21**
	[0.93, 2.42]	[0.85, 1.93]	[1.03, 1.95]	[0.88, 1.67]
Q2	**0.95**	**0.88**	**0.96**	**0.87**
	[0.69, 1.31]	[0.61, 1.26]	[0.69, 1.35]	[0.62, 1.22]
Q3	**1.66***	**1.46***	**1.61****	**1.43***
	[1.03, 2.65]	[1.03, 2.08]	[1.14, 2.28]	[1.03, 2.00]
Q4	**1.64***	**1.56***	**1.56***	**1.51***
	[1.06, 2.54]	[1.06, 2.30]	[1.06, 2.28]	[1.04, 2.18]
Q5	**1.36**	**1.00**	**1.35**	**1.01**
	[0.95, 1.93]	[0.72, 1.39]	[0.97, 1.88]	[0.71, 1.43]
Watershed FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: IRRs shown with 95% confidence intervals in brackets. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Reference category: no wildfire exposure.

As an additional robustness check, I restrict the sample to watersheds that contain at least one drinking water monitoring station, dropping all watershed-year observations where violations are not observable due to the absence of monitoring infrastructure. This approach serves a similar purpose to the zero-inflated model by addressing the prevalence of structural zeros, but does so through sample restriction rather than model-based adjustment. The results from this specification are reported in Appendix B.3 and are consistent with the main findings.

3.5.5 Wildfire and Turbidity Violations

The results from estimating Equation 3.1 using turbidity violations as the outcome and different measures of wildfire exposure are reported in Table 3.9. Burn share captures wildfire intensity relative to watershed size and allows turbidity responses to be evaluated across proportional exposure groups. As in the total-violations analysis, estimated effects vary across burn share bins.

For the burned area bins, turbidity violations exhibit strong and statistically significant responses even for relatively small fires. Watersheds affected by fires smaller than 300 acres experience a 79% increase in turbidity violations in the year of the fire (IRR = 1.79) and a 61% increase

Table 3.9: Wildfire Exposure and Turbidity Violations (Negative Binomial)

Burned Area Bins (acres)

Bin	t	t−1
<300	**1.79****	**1.61***
	[1.17, 2.75]	[1.03, 2.53]
300–2k	**2.56****	**1.33**
	[1.40, 4.65]	[0.74, 2.38]
2k–7.5k	**1.56**	**0.59**
	[0.85, 2.86]	[0.19, 1.81]
7.5k–25k	**1.17**	**0.69**
	[0.53, 2.59]	[0.23, 2.10]
25k+	**0.74**	**2.94***
	[0.11, 4.99]	[1.03, 8.37]

Burn Share Bins (% of watershed)

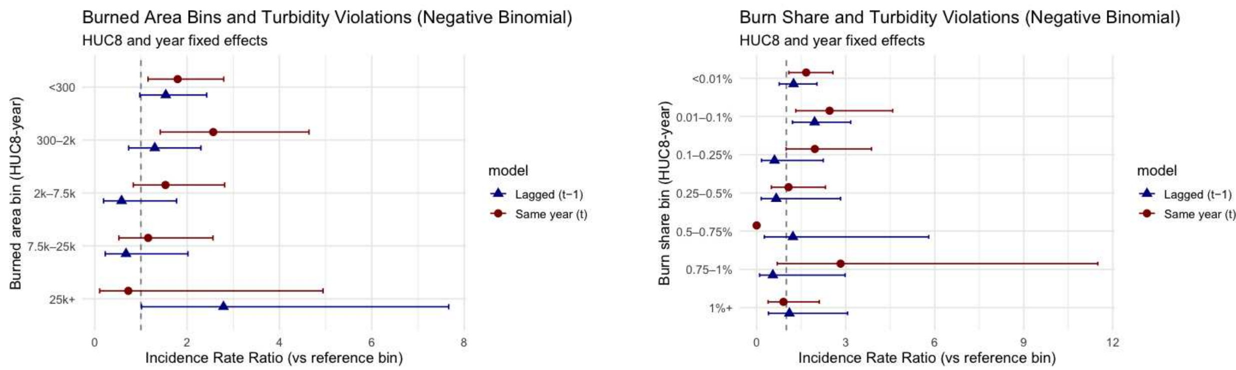
Bin	t	t−1
<0.01%	**1.67***	**1.24**
	[1.08, 2.57]	[0.76, 2.03]
0.01–0.1%	**2.46****	**1.95***
	[1.32, 4.58]	[1.20, 3.17]
0.1–0.25%	**1.96**	**0.60**
	[0.99, 3.87]	[0.16, 2.24]
0.25–0.5%	**1.07**	**0.66**
	[0.50, 2.32]	[0.15, 2.83]
0.5–0.75%	$\approx 0^{***}$	**1.22**
	[≈ 0, ≈ 0]	[0.26, 5.80]
0.75–1%	**2.83**	**0.55**
	[0.70, 11.50]	[0.10, 2.98]
1%+	**0.90**	**1.10**
	[0.39, 2.11]	[0.40, 3.07]

Notes: IRRs shown with 95% confidence intervals in brackets. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Reference category: no wildfire exposure.

in the subsequent year (IRR = 1.61). Fires between 300 and 2,000 acres are associated with a 156% contemporaneous increase in violations (IRR = 2.56), though this effect does not persist into the following year. Most larger burn categories are not statistically significant in the contemporaneous specification; however, the lagged coefficient for the largest bin (25k+ acres) is positive and significant, showing a 194% increase in violations in the year after exposure, however, this estimate is imprecise, with a wide confidence interval (3%-737%).

Results based on burn share bins show a similar pattern. Watersheds with very small burn shares (<0.01%) experience a 67% increase in turbidity violations in the year of the fire (IRR = 1.67). The next category, 0.01–0.1% of watershed area burned, is associated with a 146% contemporaneous increase (IRR = 2.46) and a 95% increase in the subsequent year (IRR = 1.95). Higher burn share categories do not exhibit consistent or statistically significant effects, underscoring a nonlinear relationship between burn severity and water degradation.

Figure 3.9: Negative Binomial Estimates: Wildfire Exposure and Turbidity Violations



(a) Burned Area Bins (b) Burn Share Bins

Notes: Points show incidence rate ratios (IRRs) from negative binomial models including wildfire exposure in years t and $t-1$. Horizontal bars denote 95% confidence intervals. The vertical line at IRR$=1$ indicates no effect. The reference category is zero wildfire exposure.

The equal-bin specification provides a complementary perspective on the turbidity results by reducing the influence of sparsely populated exposure categories. Because wildfire exposure is highly skewed, fixed threshold bins can produce imprecise estimates in categories with relatively few observations. By constructing quintiles that contain approximately equal numbers of watershed–year observations, the equal-bin approach improves balance across exposure groups and allows for a clearer assessment of nonlinear patterns. The persistence of statistically significant effects in the middle quintiles reinforces the main findings while mitigating concerns that the results are driven by small cell sizes or extreme observations.

The quintile specification, shown in Table 3.10 and Figure 3.10, indicates that turbidity violations increase for relatively small to mid-sized fire exposures. In the contemporaneous specification, Q3 (approximately 170–715 burned acres) is associated with a 301% increase in turbidity violations (IRR = 4.01, 95% CI: 1.98–8.13) relative to unburned areas, representing the largest effect across quintiles. No other contemporaneous quintile is statistically significant, though Q1 (0–35 acres) shows a sizable, but insignificant estimate (IRR = 1.98). In the subsequent year, Q3 remains statistically significant, with a 157% increase in violations (IRR = 2.57, 95% CI: 1.40–4.70). The remaining quintiles, including the largest exposure category (Q5), do not exhibit statistically significant lagged effects. Overall, the equal-bin results reinforce a non-linear relationship between

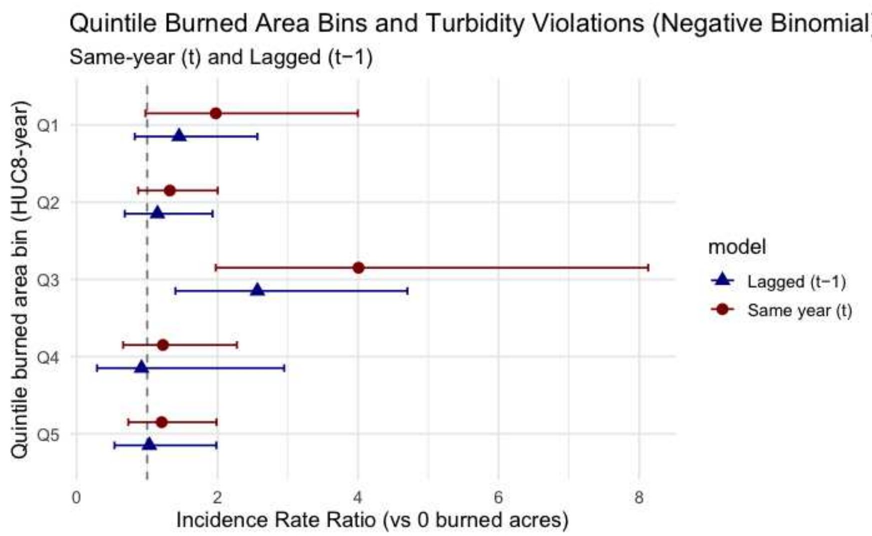
Table 3.10: Wildfire Exposure (Equal Five-Bin Specification) and Turbidity Violations (Negative Binomial)

	t	**t−1**
Q1 (<35 acres)	**1.98**	**1.45**
	[0.98, 4.00]	[0.82, 2.57]
Q2 (35–170 acres)	**1.32**	**1.15**
	[0.87, 2.00]	[0.68, 1.93]
Q3 (170–715 acres)	**4.01*****	**2.57****
	[1.98, 8.13]	[1.40, 4.70]
Q4 (715–2,984 acres)	**1.23**	**0.92**
	[0.66, 2.28]	[0.29, 2.95]
Q5 (2,984–235,994 acres)	**1.21**	**1.03**
	[0.73, 1.99]	[0.54, 1.98]
Watershed FE	Yes	Yes
Year FE	Yes	Yes

Notes: IRRs shown with 95% confidence intervals in brackets. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Reference category: no wildfire exposure.

burned acreage and water degradation, with the strongest and most robust impacts concentrated in the middle of the exposure distribution rather than among the largest fires.

Figure 3.10: Negative Binomial Estimates: Wildfire Exposure (Quintile Specification) and Turbidity Violations



Notes: Points show incidence rate ratios (IRRs) from negative binomial models using wildfire exposure quintiles and including exposure in years t and $t - 1$. Horizontal bars denote 95% confidence intervals. The vertical line at IRR$= 1$ indicates no effect. The reference category is no wildfire exposure.

3.5.6 Estimated Treatment Costs of Wildfire on Water Quality

To provide an order-of-magnitude estimate of the economic implications of wildfire-related drinking water violations, the estimated effects on violation counts are translated into approximate treatment and response costs. This exercise serves as a rough approximation rather than a comprehensive cost accounting and is intended to contextualize the magnitude of the estimated effects.

Let $\bar{V}$ denote the mean number of drinking water violations per watershed–year in the sample. The baseline mean is $\bar{V} = 3.34$. In the main quintile specification, mid-sized wildfire exposure (Q3, approximately 170–715 acres) is associated with an incidence rate ratio (IRR) of 1.66 (95% CI: [1.03, 2.65]). The adjacent quintile (Q4, approximately 715–3,000 acres) yields a similar magnitude and confidence interval. Accordingly, the cost estimates presented in this section should be interpreted as the expected additional costs following wildfire exposure in the range of roughly 170 to 3,000 burned acres. The implied increase in violations per affected watershed–year is given by:

$$\Delta V = \bar{V}\,(IRR - 1), \tag{3.2}$$

which yields

$$\Delta V \in [0.10,\ 5.51], \tag{3.3}$$

with a point estimate of

$$\Delta V = 2.20. \tag{3.4}$$

This implies that wildfire exposure in the range of approximately 170 to 3,000 acres is associated with an average increase of 2.2 additional violations per affected watershed–year, with a 95% confidence interval of 0.1 to 5.51 violations.

To translate changes in violation counts into economic terms, let C denote the average treatment or operational response cost associated with a single violation. The total cost per affected watershed–year is then

$$\text{Cost}_{\text{HUC8}} = \Delta V \times C, \tag{3.5}$$

which implies a range of

$$\text{Cost}_{\text{HUC8}} \in [0.10\,C_L,\ 5.51\,C_H], \tag{3.6}$$

where C_L and C_H denote lower- and upper-bound cost assumptions. Actual costs depend on system size, treatment technology, duration of response, and operational adjustments.

To illustrate how wildfire-related treatment costs scale with system size, Table 3.11 reports illustrative daily cost ranges for selected Colorado drinking water systems under the assumption that a treatment-related violation requires additional expenditures of, conservatively, \$500–\$1,000 per million gallons (MG) treated per day, although costs can easily increase to thousands of dollars per MG (U.S. Environmental Protection Agency, 2024). Cost estimates calculated in this section should be interpreted as conservative ranges of additional costs for water utilities.

For large systems such as Denver Water, which operates treatment plants with capacities ranging from approximately 120 to 280 MG per day (Denver Water, 2026), this implies daily costs ranging from roughly \$6,000 to \$14,000 under the lower-bound estimate, with an average estimate of approximately \$132,000 to \$616,000 per day. Under the upper bound of the confidence interval, daily costs could plausibly reach \$330,000 to more than \$1.5 million.

A medium-sized system such as Fort Collins Utilities, treating roughly 25 MG per day (City of Fort Collins Utilities, 2026), faces estimated daily costs of approximately \$1,250 under the lower-bound estimate, \$27,500 at the point estimate, and up to roughly \$137,750 under the upper-bound scenario.

Smaller systems, like that of the City of Durango in southwest Colorado, with average production of about 4 MG per day (City of Durango, 2017), incur lower but still meaningful costs of roughly \$200 under the lower-bound estimate, \$4,400 at the point estimate, and up to approximately \$22,000 per day under the upper-bound scenario.

These estimates are intentionally conservative and are intended to provide context for the potential operational burden imposed by wildfire-related water quality disruptions across systems of different sizes.

Table 3.11: Estimated Daily Treatment Costs by Colorado Water Treatment Plant Size

System	Avg MG per Day	Low	High
Large: Denver Water	120–280	\$6,000–\$14,000	\$661,000–\$1,543,000
Medium: Fort Collins Utilities	25	\$1,250	\$137,750
Small: City of Durango	4	\$200	\$22,000

Notes: Daily cost estimates are calculated by multiplying average daily treated volume by an assumed marginal treatment and operational response cost of \$500–\$1,000 per million gallons (MG) treated per day. This range is drawn from EPA and AWWA guidance and reflects conservative, lower-bound estimates of additional chemical use, energy, labor, and residuals-handling costs associated with elevated turbidity or treatment intensity. Denver Water operates multiple treatment plants with capacities ranging from approximately 120 to 280 MG per day; reported values reflect this range. Estimates are intended to be illustrative rather than comprehensive..

While the estimates above illustrate daily treatment costs, the total economic burden of a wildfire-related water quality event depends critically on the duration of the required treatment or operational response. From the Negative Binomial estimates, a wildfire between 170–3,000 acres in size will lead to an increase of $\Delta V \in [0.10,\ 5.51]$ additional violations in the year of the fire. Translating these violations into total costs requires an assumption about the number of days, d, over which additional treatment or response is required for each violation.

Letting MGD denote average daily treated volume and assuming marginal treatment costs of \$500–\$1,000 per million gallons treated per day, the implied total cost can be written as

$$\text{Total Cost} = \Delta V \times d \times (\$500\text{–}\$1{,}000) \times \text{MGD}. \tag{3.7}$$

Table 3.12: Wildfire-Related Treatment Costs by Event Duration (Point Estimate)

System	**MGD**	**Total cost** ($d = 1$)	**Total cost** ($d = 7$)
Large: Denver Water (plant range)	120–280	\$132k–\$616k	\$924k–\$4.31M
Medium: Fort Collins Utilities	25	\$27.5k–\$55k	\$192.5k–\$385k
Small: City of Durango	4	\$4.4k–\$8.8k	\$30.8k–\$61.6k

Notes: Total costs assume an estimated increase of 2.2 violations following wildfire exposure and marginal treatment costs of \$500–\$1,000 per MG-day. Duration d denotes the number of days of elevated treatment activity.

Table 3.12 shows that the costs associated with the average increase in water quality violations following wildfire exposure can escalate rapidly when treatment responses persist for multiple days, reaching into the millions of dollars for large systems and tens of thousands for smaller utilities.

3.6 Discussion and Policy Implications

This paper provides evidence that wildfire exposure is associated with meaningful increases in drinking water quality violations across Colorado watersheds. Across multiple specifications, fires well below the largest and most catastrophic events are linked to statistically significant increases in water quality violations. In particular, mid-sized fires—those affecting roughly a few hundred to a few thousand acres—are associated with sizable contemporaneous increases in violations, and in several cases these effects persist into the subsequent year. Interestingly, the largest fires do not generate the largest estimated impacts. Overall, these results highlight the underappreciated risk that small to moderately sized fires pose to drinking water systems, potentially costing utilities millions of dollars in additional treatment costs. This reinforces the findings of previous studies showing that relatively modest wildfire disturbances and prescribed burns can produce measurable, persistent declines in water quality (Brucker et al., 2025; Zhang et al., 2025; Paul et al., 2022; Smith et al., 2011; Abraham et al., 2017).

These findings are robust across alternative exposure measures, binning strategies, and model specifications. Shown in Appendix B.1, estimates vary slightly across negative binomial and Poisson specifications, but the central pattern remains: small to moderately sized wildfire events are associated with measurable increases in drinking water quality violations. The economic and operational risks to water systems are therefore not limited to rare, extreme mega-fires, but also arise from a broader set of wildfire events that occur with much greater frequency.

Results for turbidity violations further reinforce this conclusion. Across burned acreage bins, burn share bins, and burn quintile specifications, small to moderately sized fires are associated with large and statistically significant increases in turbidity violations. Watersheds exposed to fires below the largest fire size categories experience substantial contemporaneous increases in turbidity violations, with evidence that these effects often persist into the following year.

This pattern underscores two important points. First, the relationship between wildfire size and turbidity violations appears nonlinear: the largest fires do not produce the largest estimated effects. Second, meaningful water quality disruptions are not confined to rare, extreme events. Instead, fires affecting only a few hundred to a few thousand acres—or modest shares of watershed area—can generate sharp increases in turbidity-related violations. These findings are consistent with well-documented mechanisms linking wildfire-driven erosion, sediment mobilization, and ash deposition to short-run spikes in source water turbidity (Emelko et al., 2011; Hohner et al., 2016; Warziniack et al., 2017; Paul et al., 2022). In many cases, wildfire-driven changes to watershed chemistry and sediment transport can persist beyond the initial burn period, prolonging treatment challenges for downstream utilities (Paul et al., 2022; Brucker et al., 2025).

From a policy perspective, these findings highlight the importance of wildfire mitigation and post-fire watershed management as critical components of drinking water system resilience. Even short-lived increases in treatment intensity can generate substantial operational costs for water utilities, both for large systems that treat high daily volumes, and small, rural systems that have smaller operating budgets. Wildfire exposure is associated with increases in local water quality violations, and the resulting treatment and operational costs can reach millions of dollars for large water systems such as Denver Water. Investments that reduce burn severity, limit post-fire erosion, or accelerate watershed recovery may therefore yield benefits not only for ecosystem health but also for public water systems and ratepayers (Emelko et al., 2011; Hohner et al., 2016; Warziniack et al., 2017). More broadly, as wildfire risk continues to increase due to climate change (Abatzoglou and Williams, 2016; McGinnis et al., 2023; Wasserman and Mueller, 2023), incorporating downstream water quality impacts into wildfire planning and infrastructure investment decisions may be critical for managing long-run public health and economic risks.

3.7 Limitations

Several limitations are important to consider when interpreting these results. First, observed impacts likely reflect not only changes in underlying water quality but also responses by water utilities. Following major wildfire events in Colorado, affected utilities undertook substantial preventative investments, including expanded treatment capacity, source switching, and enhanced monitoring (Rhoades et al., 2011; Writer et al., 2014). Utilities such as Denver Water and Fort Collins Utilities invested millions of dollars to mitigate post-fire water quality risks. These responses likely reduced the number of observed violations following the largest wildfire events, which may help explain why the largest burn bins across specifications show insignificant increases in water quality violations. In this sense, the results should be interpreted as conservative estimates of wildfire impacts, reflecting outcomes that already incorporate costly utility intervention. The absence of strong effects in the largest burn bins may therefore reflect successful mitigation rather than an absence of risk, and that small to moderately sized fires may pose an underappreciated threat to drinking water systems.

Second, the analysis relies on spatial aggregation at the watershed (HUC8) level, which may obscure important within-watershed heterogeneity. Watersheds are a useful unit for analyzing hydrologic processes, but they do not fully account for the location of individual water intakes or monitoring stations relative to burned areas. In particular, wildfire exposure is not conditioned on whether burns occur upstream of specific drinking water infrastructure. As a result, some fires classified as affecting a watershed may have limited relevance for monitored water systems, while downstream spillovers across watershed boundaries may be missed. More spatially detailed data linking burn locations to upstream source waters would allow for sharper identification of exposure pathways.

Finally, there is imprecision in the timing of measured water quality violations. Wildfire exposure is measured at an annual level, reflecting the year of the fire rather than the exact timing within the year. Lagged specifications partially address this mismatch. Future work using higher-frequency water quality data and more precise fire timing could provide additional insight into the temporal evolution of post-fire water quality impacts.

3.8 Conclusion

This study documents a systematic relationship between wildfire exposure and drinking water quality violations across watersheds in Colorado. Using watershed-level panel data, the analysis shows that wildfire exposure is associated with contemporaneous increases in reported water quality violations across multiple specifications. Importantly, these effects are not limited to the largest fire events. Small to moderately sized fires—those affecting only a few hundred to a few thousand acres or modest shares of watershed area—are consistently associated with meaningful increases in violations across model specifications.

Results are robust across alternative measures of wildfire exposure and modeling approaches, and the effects are particularly pronounced for turbidity violations. Across burned acreage bins, burn share bins, and quintile specifications, turbidity violations increase significantly following wildfire exposure, even for relatively small fires. These findings highlight drinking water quality as an important and underappreciated channel through which wildfires affect communities and impose additional operating costs on water systems.

These results carry direct implications for wildfire management, water infrastructure planning, and climate adaptation policy. The analysis shows that meaningful water quality disruptions are not limited to rare, catastrophic fires. Relatively frequent, small to moderate wildfire events generate substantial operational challenges for drinking water systems. For large utilities treating hundreds of millions of gallons per day, these disruptions can translate into significant treatment costs, while smaller systems may face meaningful financial strain relative to limited budgets and technical capacity.

The weaker effects observed for the largest fires may reflect the presence of costly but effective preventative investments, such as sediment management, upgraded filtration systems, or switching source water. As wildfire risk continues to rise across the western United States, integrating downstream water quality impacts into wildfire mitigation strategies, watershed management, and water infrastructure investment decisions will become increasingly important for protecting public health and managing long-run economic risk.

Bibliography

Abatzoglou, J. T. and Williams, A. P. (2016). Impact of anthropogenic climate change on wildfire across western us forests. *Proceedings of the National Academy of sciences*, 113(42):11770–11775.

Abraham, J., Dowling, K., and Florentine, S. (2017). Risk of post-fire metal mobilization into surface water resources: A review. *Science of the Total Environment*, 599:1740–1755.

Allaire, M., Wu, H., and Lall, U. (2018). National trends in drinking water quality violations. *Proceedings of the National Academy of Sciences*, 115(9):2078–2083.

Alzahrani, F. and Alhussain, K. (2025). Health implications of safe drinking water act violations: a county-level analysis. *Frontiers in Public Health*, 13:1588338.

Amacher, G. S., Malik, A. S., and Haight, R. G. (2005). Not getting burned: the importance of fire prevention in forest management. *Land Economics*, 81(2):284–302.

Asadi, S., Nilashi, M., Samad, S., Abdullah, R., Mahmoud, M., Alkinani, M. H., and Yadegaridehkordi, E. (2021). Factors impacting consumers' intention toward adoption of electric vehicles in malaysia. *Journal of Cleaner Production*, 282:124474.

Bayham, J., Yoder, J. K., Champ, P. A., and Calkin, D. E. (2022). The economics of wildfire in the united states. *Annual Review of Resource Economics*, 14(1):379–401.

Baylis, P. and Boomhower, J. (2019). Moral hazard, wildfires, and the economic incidence of natural disasters. Technical report, National Bureau of Economic Research.

Botzen, W. W., Deschenes, O., and Sanders, M. (2019). The economic impacts of natural disasters: A review of models and empirical studies. *Review of Environmental Economics and Policy*.

Boustan, L. P., Kahn, M. E., Rhode, P. W., and Yanguas, M. L. (2020). The effect of natural disasters on economic activity in us counties: A century of data. *Journal of Urban Economics*, 118:103257.

Brucker, C. P., Livneh, B., Rosario-Ortiz, F. L., Yao, F., Williams, A. P., Becker, W. C., Kampf, S. K., and Rajagopalan, B. (2025). Wildfires drive multi-year water quality degradation over the western united states. *Communications Earth & Environment*, 6(1):489.

Burra, L. T., Al-Khasawneh, M. B., and Cirillo, C. (2024). Impact of charging infrastructure on electric vehicle adoption: A synthetic population approach. *Travel Behaviour and Society*, 37:100834.

California Air Resources Board (2020). Camp fire air quality data analysis. Accessed: 2025-04-24.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of econometrics*, 225(2):200–230.

Cariboo Chilcotin Coast Tourism Association (2017). Wildfire impacts on tourism in the cariboo chilcotin coast region. Accessed April 2025.

Carson, R. T. and Mitchell, R. C. (1993). The value of clean water: the public's willingness to pay for boatable, fishable, and swimmable quality water. *Water resources research*, 29(7):2445–2454.

Chatterjee, C., Triplett, R., Johnson, C. K., and Ahmed, P. (2017). Willingness to pay for safe drinking water: A contingent valuation study in jacksonville, fl. *Journal of environmental management*, 203:413–421.

Chen, G., Guo, Y., Yue, X., Xu, R., Yu, W., Ye, T., Tong, S., Gasparrini, A., Bell, M. L., Armstrong, B., et al. (2024). All-cause, cardiovascular, and respiratory mortality and wildfire-related ozone: a multicountry two-stage time series analysis. *The Lancet Planetary Health*, 8(7):e452–e462.

Chen, H., Rhoades, C. C., and Chow, A. T. (2020). Characteristics of soil organic matter 14 years after a wildfire: A pyrolysis-gas-chromatography mass spectrometry (py-gc-ms) study. *Journal of Analytical and Applied Pyrolysis*, 152:104922.

City of Durango (2017). City of durango comprehensive plan. Accessed: March 31, 2026.

City of Fort Collins Utilities (2026). Water utility services. Accessed: March 31, 2026.

Clinton, B. C. and Steinberg, D. C. (2019). Providing the spark: Impact of financial incentives on battery electric vehicle adoption. *Journal of Environmental Economics and Management*, 98:102255.

Colorado Tourism Office (2024). Colorado tourism office overview. Accessed: 2025-04-21.

Connolly, R., Coffee, D., and Pierce, G. (2025). Do electric vehicle purchase incentives drive equity? the relationship between incentive policies and vehicle registration rates across california. *Transport Policy*.

Coulombe, R. G. and Rao, A. (2025). Fires and local labor markets. *Journal of Environmental Economics and Management*, 130:103109.

Crowley, C., Miller, A., Richardson, R., and Malcom, J. (2023). Increasing damages from wildfires warrant investment in wildland fire management. *US Department of Interior, May*, 25.

Cutler, D. and Miller, G. (2005). The role of public health improvements in health advances: the twentieth-century united states. *Demography*, 42(1):1–22.

Dale, L. A. and Barrett, K. (2023). Missing the mark: Effectiveness and funding in community wildfire risk reduction. Technical report, Headwaters Economics.

Davis, L. W., Li, J., and Springel, K. (2025). Political ideology and us electric vehicle adoption. Technical report, National Bureau of Economic Research.

DeBano, L. F. (2000). The role of fire and soil heating on water repellency in wildland environments: a review. *Journal of hydrology*, 231:195–206.

Dennison, P. E., Brewer, S. C., Arnold, J. D., and Moritz, M. A. (2014). Large wildfire trends in the western united states, 1984–2011. *Geophysical Research Letters*, 41(8):2928–2933.

Denver Water (2026). Key facts: How denver water operates. Accessed: March 31, 2026.

DeShazo, J. R., Sheldon, T. L., and Carson, R. T. (2017). Designing policy incentives for cleaner technologies: Lessons from california's plug-in electric vehicle rebate program. *Journal of environmental economics and management*, 84:18–43.

Donovan, V. M., Crandall, R., Fill, J., and Wonkka, C. L. (2023). Increasing large wildfire in the eastern united states. *Geophysical Research Letters*, 50(24):e2023GL107051.

Du, R., Liu, K., Zhao, D., and Fang, Q. (2024). Climate disaster and cognitive ability: Evidence from wildfire. *International Journal of Public Health*, 69:1607128.

Egnér, F. and Trosvik, L. (2018). Electric vehicle adoption in sweden and the impact of local policy instruments. *Energy policy*, 121:584–596.

Emelko, M. B., Silins, U., Bladon, K. D., and Stone, M. (2011). Implications of land disturbance on drinking water treatability in a changing climate: Demonstrating the need for "source water supply and protection" strategies. *Water research*, 45(2):461–472.

Esmaili, A., Oshanreh, M. M., Naderian, S., MacKenzie, D., and Chen, C. (2024). Assessing the spatial distributions of public electric vehicle charging stations with emphasis on equity considerations in king county, washington. *Sustainable Cities and Society*, 107:105409.

Falchetta, G. and Noussan, M. (2021). Electric vehicle charging network in europe: An accessibility and deployment trends analysis. *Transportation Research Part D: Transport and Environment*, 94:102813.

Federal Highway Administration (2023). Summary of travel trends: 2022 national household travel survey. Technical report, U.S. Department of Transportation. Accessed: 2025-04-28.

Felbermayr, G. and Gröschl, J. (2014). Naturally negative: The growth effects of natural disasters. *Journal of development economics*, 111:92–106.

Franke, T., Neumann, I., Bühler, F., Cocron, P., and Krems, J. F. (2012). Experiencing range in an electric vehicle: Understanding psychological barriers. *Applied Psychology*, 61(3):368–391.

Gellman, J., Walls, M., and Wibbenmeyer, M. (2022). Wildfire, smoke, and outdoor recreation in the western united states. *Forest Policy and Economics*, 134:102619.

Ghasri, M., Ardeshiri, A., and Rashidi, T. (2019). Perceived advantage in perspective application of integrated choice and latent variable model to capture electric vehicles perceived advantage from consumers perspective. *arXiv preprint arXiv:1905.11606*.

Gifford, T. and Barbier, E. B. (2025). Commuting time, charging infrastructure, and electric vehicle adoption for sustainable transportation: A case study of washington state. *Sustainability*, 17(21):9591.

Gritzo, L. A. L. (2024). Mitigating social and economic impact of wildfires. *Applications in Energy and Combustion Science*, 20:100285.

Haidar, B. and Rojas, M. T. A. (2022). The relationship between public charging infrastructure deployment and other socio-economic factors and electric vehicle adoption in france. *Research in Transportation Economics*, 95:101208.

Halbritter, A., Deegen, P., and Susaeta, A. (2020). An economic analysis of thinnings and rotation lengths in the presence of natural risks in even-aged forest stands. *Forest Policy and Economics*, 118:102223.

Hardman, S., Jenn, A., Tal, G., Axsen, J., Beard, G., Daina, N., Figenbaum, E., Jakobsson, N., Jochem, P., Kinnear, N., et al. (2018). A review of consumer preferences of and interactions with electric vehicle charging infrastructure. *Transportation Research Part D: Transport and Environment*, 62:508–523.

He, X., Jiang, S., Yu, Y., and Su, S. (2025). Impact of novel infrastructure investments on productivity: Evidence from public procurement of ev charging facilities in china. *Energy*, page 137699.

Headwaters Economics (2018). The full community costs of wildfire. Technical report, Headwaters Economics. Accessed April 2025.

Heberling, M. T., Nietch, C. T., Thurston, H. W., Elovitz, M., Birkenhauer, K. H., Panguluri, S., Ramakrishnan, B., Heiser, E., and Neyer, T. (2015). Comparing drinking water treatment costs to source water protection costs using time series analysis. *Water Resources Research*, 51(11):8741–8756.

Hohner, A. K., Cawley, K., Oropeza, J., Summers, R. S., and Rosario-Ortiz, F. L. (2016). Drinking water treatment response following a colorado wildfire. *Water Research*, 105:187–198.

Hohner, A. K., Rhoades, C. C., Wilkerson, P., and Rosario-Ortiz, F. L. (2019). Wildfires alter forest watersheds and threaten drinking water quality. *Accounts of chemical research*, 52(5):1234–1244.

Holmes, T. P. (1988). The offsite impact of soil erosion on the water treatment industry. *Land economics*, 64(4):356–366.

Huang, X. and Ge, J. (2019). Electric vehicle development in beijing: An analysis of consumer purchase intention. *Journal of cleaner production*, 216:361–372.

Huang, X., Lin, Y., Zhou, F., Lim, M. K., and Chen, S. (2021). Agent-based modelling for market acceptance of electric vehicles: evidence from china. *Sustainable Production and Consumption*, 28:206–217.

Jenn, A., Springel, K., and Gopal, A. R. (2018). Effectiveness of electric vehicle incentives in the united states. *Energy policy*, 119(C):349–356.

Jones, B. A. and McDermott, S. (2021). The local labor market impacts of us megafires. *Sustainability*, 13(16):9078.

Jones, K. W., Cannon, J. B., Saavedra, F. A., Kampf, S. K., Addington, R. N., Cheng, A. S., MacDonald, L. H., Wilson, C., and Wolk, B. (2017). Return on investment from fuel treatments to reduce severe wildfire and erosion in a watershed investment program in colorado. *Journal of Environmental Management*, 198:66–77.

Jones, K. W., Gannon, B., Timberlake, T., Chamberlain, J. L., and Wolk, B. (2022). Societal benefits from wildfire mitigation activities through payments for watershed services: Insights from colorado. *Forest Policy and Economics*, 135:102661.

Kaval, P., Loomis, J., and Seidl, A. (2007). Willingness-to-pay for prescribed fire in the colorado (usa) wildland urban interface. *Forest Policy and Economics*, 9(8):928–937.

Kim, H., Kim, D.-W., and Kim, M.-K. (2022). Economics of charging infrastructure for electric vehicles in korea. *Energy Policy*, 164:112875.

Lackner, S. (2018). Earthquakes and economic growth. Technical report, FIW Working Paper.

Lawson, M. and Story, S. (2024). Outdoor recreation economy by state. Accessed February 23, 2026.

Ledna, C., Muratori, M., Brooker, A., Wood, E., and Greene, D. (2022). How to support ev adoption: Tradeoffs between charging infrastructure investments and vehicle subsidies in california. *Energy Policy*, 165:112931.

Lee, M. C., Suter, J. F., and Bayham, J. (2023). Reductions in national forest campground reservation demand from wildfire. *Journal of Agricultural and Resource Economics*, 48(3):483–499.

Lee, Y.-J. A. and Nilsson, I. (2025). An examination of the effect of external factors on zero-emission vehicle adoption in the united states. *Travel Behaviour and Society*, 38:100904.

Li, S., Tong, L., Xing, J., and Zhou, Y. (2017). The market for electric vehicles: Indirect network effects and policy design. *Journal of the Association of Environmental and Resource Economists*, 4(1):89–133.

Liu, Y.-L., Chiang, J.-T., and Ko, P.-F. (2023). The benefits of tourism for rural community development. *Humanities and Social Sciences Communications*, 10(1):137.

Los Angeles County Economic Development Corporation (2025). Impact of 2025 los angeles wildfires and comparative study. Accessed: 2025-04-24.

McGinnis, S., Kessenich, L., Mearns, L., Cullen, A., Podschwit, H., and Bukovsky, M. (2023). Future regional increases in simultaneous large western usa wildfires. *International Journal of Wildland Fire*, 32(9):1304–1314.

Meier, S., Strobl, E., Elliott, R. J., and Kettridge, N. (2023). Cross-country risk quantification of extreme wildfires in mediterranean europe. *Risk analysis*, 43(9):1745–1762.

Meldrum, J. R., Brenkert-Smith, H., Champ, P. A., Falk, L., Wilson, P., and Barth, C. M. (2018). Wildland–urban interface residents' relationships with wildfire: variation within and across communities. *Society & Natural Resources*, 31(10):1132–1148.

Meldrum, J. R., Champ, P. A., Warziniack, T., Brenkert-Smith, H., Barth, C. M., and Falk, L. C. (2014). Cost shared wildfire risk mitigation in log hill mesa, colorado: survey evidence on participation and willingness to pay. *International journal of wildland fire*, 23(4):567–576.

Melliger, M. A., van Vliet, O. P., and Liimatainen, H. (2018). Anxiety vs reality–sufficiency of battery electric vehicle range in switzerland and finland. *Transportation Research Part D: Transport and Environment*, 65:101–115.

Meyer, D. and Meyer, N. (2015). The role and impact of tourism on local economic development: a comparative study and leisure. *African Journal for Physical Health Education, Recreation and Dance*, 21(1.1):197–214.

Michetti, M. and Pinar, M. (2019). Forest fires across italian regions and implications for climate change: a panel data analysis. *Environmental and Resource Economics*, 72:207–246.

Miele, A., Axsen, J., Wolinetz, M., Maine, E., and Long, Z. (2020). The role of charging and refuelling infrastructure in supporting zero-emission vehicle sales. *Transportation Research Part D: Transport and Environment*, 81:102275.

Molina, J. R., González-Cabán, A., and y Silva, F. R. (2019). Wildfires impact on the economic susceptibility of recreation activities: Application in a mediterranean protected area. *Journal of environmental management*, 245:454–463.

Mueller, J., Loomis, J., and González-Cabán, A. (2009). Do repeated wildfires change homebuyers' demand for homes in high-risk areas? a hedonic analysis of the short and long-term effects of repeated wildfires on house prices in southern california. *The Journal of Real Estate Finance and Economics*, 38:155–172.

Münzel, C., Plötz, P., Sprei, F., and Gnann, T. (2019). How large is the effect of financial incentives on electric vehicle sales?–a global review and european analysis. *Energy Economics*, 84:104493.

Muratori, M., Greene, D., Kontou, E., and Dong, J. (2020). The role of infrastructure to enable and support electric drive vehicles: A transportation research part d special issue.

Narassimhan, E. and Johnson, C. (2018). The role of demand-side incentives and charging infrastructure on plug-in electric vehicle adoption: analysis of us states. *Environmental Research Letters*, 13(7):074032.

Neary, D. G., Ryan, K. C., and DeBano, L. F. (2005). Wildland fire in ecosystems: effects of fire on soils and water. *Gen. Tech. Rep. RMRS-GTR-42-vol. 4. Ogden, UT: US Department of Agriculture, Forest Service, Rocky Mountain Research Station. 250 p.*, 42.

Noy, I. (2009). The macroeconomic consequences of disasters. *Journal of Development economics*, 88(2):221–231.

Olmstead, S. M. (2010). The economics of water quality. *Review of Environmental Economics and Policy*.

Otrachshenko, V. and Nunes, L. C. (2022). Fire takes no vacation: Impact of fires on tourism. *Environment and Development Economics*, 27(1):86–101.

Parisien, M.-A., Barber, Q. E., Bourbonnais, M. L., Daniels, L. D., Flannigan, M. D., Gray, R. W., Hoffman, K. M., Jain, P., Stephens, S. L., Taylor, S. W., et al. (2023). Abrupt, climate-induced increase in wildfires in british columbia since the mid-2000s. *Communications Earth & Environment*, 4(1):309.

Paul, M., LeDuc, S., Lassiter, M., Moorhead, L., Noyes, P., and Leibowitz, S. (2022). Wildfire induces changes in receiving waters: A review with considerations for water quality management. *Water Resources Research*, 58(9):e2021WR030699.

Pausas, J. G. and Keeley, J. E. (2019). Wildfires as an ecosystem service. *Frontiers in Ecology and the Environment*, 17(5):289–295.

Pelletier, F., Cardille, J. A., Wulder, M. A., White, J. C., and Hermosilla, T. (2024). Revisiting the 2023 wildfire season in canada. *Science of Remote Sensing*, 10:100145.

Peng, R., Tang, J. H. C. G., Yang, X., Meng, M., Zhang, J., and Zhuge, C. (2024a). Investigating the factors influencing the electric vehicle market share: A comparative study of the european union and united states. *Applied Energy*, 355:122327.

Peng, Z., Wang, M. W. H., Yang, X., Chen, A., and Zhuge, C. (2024b). An analytical framework for assessing equitable access to public electric vehicle chargers. *Transportation Research Part D: Transport and Environment*, 126:103990.

Pereira, P., Bogunovic, I., Zhao, W., and Barcelo, D. (2021). Short-term effect of wildfires and prescribed fires on ecosystem services. *Current Opinion in Environmental Science & Health*, 22:100266.

Pew Research Center (2024). Electric vehicle charging infrastructure in the u.s. Accessed: 2025-03-10.

Powell, B. and Johnson, C. (2024). Impact of electric vehicle charging station reliability, resilience, and location on electric vehicle adoption. Technical report, National Renewable Energy Laboratory (NREL), Golden, CO (United States).

Proctor, C. R., Lee, J., Yu, D., Shah, A. D., and Whelton, A. J. (2020). Wildfire caused widespread drinking water distribution network contamination. *AWWA Water Science*, 2(4):e1183.

Qian, L., Grisolía, J. M., and Soopramanien, D. (2019). The impact of service and government-policy attributes on consumer preferences for electric vehicles in china. *Transportation Research Part A: Policy and Practice*, 122:70–84.

Rainieri, G., Buizza, C., and Ghilardi, A. (2023). The psychological, human factors and socio-technical contribution: A systematic review towards range anxiety of battery electric vehicles' drivers. *Transportation research part F: traffic psychology and behaviour*, 99:52–70.

Rauh, N., Günther, M., and Krems, J. F. (2020). Positive influence of practical electric vehicle driving experience and range related knowledge on drivers' experienced range stress. *Transportation Research Part F: Traffic Psychology and Behaviour*, 71:182–197.

Reeder, R. J. and Brown, D. M. (2005). *Recreation, Tourism, and Rural Well-Being*. Number 7 in Economic Research Report. US Department of Agriculture, Economic Research Service, Washington, DC.

Rhoades, C. C., Entwistle, D., and Butler, D. (2011). The influence of wildfire extent and severity on streamwater chemistry, sediment and temperature following the hayman fire, coloradoa. *International Journal of Wildland Fire*, 20(3):430–442.

Rhoades, C. C., Nunes, J. P., Silins, U., and Doerr, S. H. (2019). The influence of wildfire on water quality and watershed processes: New insights and remaining challenges. *International Journal of Wildland Fire*, 28(10):721–725.

Roth, J., Sant'Anna, P. H., Bilinski, A., and Poe, J. (2023). What's trending in difference-in-differences? a synthesis of the recent econometrics literature. *Journal of Econometrics*, 235(2):2218–2244.

Schulz, F. and Rode, J. (2022). Public charging infrastructure and electric vehicles in norway. *Energy Policy*, 160:112660.

Shafran, A. P. (2008). Risk externalities and the problem of wildfire risk. *Journal of urban economics*, 64(2):488–495.

Sheldon, T. L. (2022). Evaluating electric vehicle policy effectiveness and equity. *Annual Review of Resource Economics*, 14:669–688.

Sheldon, T. L. and DeShazo, J. (2017). How does the presence of hov lanes affect plug-in electric vehicle adoption in california? a generalized propensity score approach. *Journal of environmental economics and management*, 85:146–170.

Sheldon, T. L. and Dua, R. (2019). Assessing the effectiveness of california's "replace your ride". *Energy policy*, 132:318–323.

Sierra Business Council (2020). The impact of wildfires on the tourism economy in the sierra nevada region. Technical report, Sierra Business Council. Accessed April 2025.

Sierzchula, W., Bakker, S., Maat, K., and Van Wee, B. (2014). The influence of financial incentives and other socio-economic factors on electric vehicle adoption. *Energy policy*, 68:183–194.

Singer, M. (2020). Plug-in electric vehicle showcases: Consumer experience and acceptance. Technical report, National Renewable Energy Lab.(NREL), Golden, CO (United States).

Singh, V., Singh, V., and Vaibhav, S. (2020). A review and simple meta-analysis of factors influencing adoption of electric vehicles. *Transportation Research Part D: Transport and Environment*, 86:102436.

Smart, J. G. and Salisbury, S. D. (2015). Plugged in: How americans charge their electric vehicles. Technical report, Idaho National Lab.(INL), Idaho Falls, ID (United States).

Smith, H. G., Sheridan, G. J., Lane, P. N., Nyman, P., and Haydon, S. (2011). Wildfire effects on water quality in forest catchments: A review with implications for water supply. *Journal of Hydrology*, 396(1-2):170–192.

Solomon, G. M., Hurley, S., Carpenter, C., Young, T. M., English, P., and Reynolds, P. (2021). Fire and water: assessing drinking water contamination after a major wildfire. *ACS Es&t Water*, 1(8):1878–1886.

Springel, K. (2021). Network externality and subsidy structure in two-sided markets: Evidence from electric vehicle incentives. *American Economic Journal: Economic Policy*, 13(4):393–432.

Strauss, D., Bednar, L., and Mees, R. (1989). Do one percent of the forest fires cause ninety-nine percent of the damage? *Forest science*, 35(2):319–328.

Strobl, E. (2011). The economic growth impact of hurricanes: Evidence from us coastal counties. *Review of Economics and Statistics*, 93(2):575–589.

Sumic, N., Sirohi, S., and Lipinski, M. (2013). Economic impact of wildfire smoke on the ashland tourism industry. Accessed April 2025.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of econometrics*, 225(2):175–199.

Tal, G. and Nicholas, M. (2016). Exploring the impact of the federal tax credit on the plug-in vehicle market. *Transportation research record*, 2572(1):95–102.

Tavor, T. (2024). Assessing the financial impacts of significant wildfires on us capital markets: sectoral analysis. *Empirical Economics*, pages 1–34.

Thapa, B., Cahyanto, I., Holland, S. M., and Absher, J. D. (2013). Wildfires and tourist behaviors in florida. *Tourism Management*, 36:284–292.

U.S. Congress Joint Economic Committee Democrats (2023). Climate-exacerbated wildfires cost the u.s. between $394 to $893 billion each year in economic costs and damages. Accessed: 2025-02-18.

U.S. Department of Energy (2023). Electric drive vehicles: Current status and trends. Technical report, Office of Energy Efficiency & Renewable Energy, Alternative Fuels Data Center. Accessed: 2025-04-28.

U.S. Environmental Protection Agency (2023a). National primary drinking water regulations. Accessed: 2026-03-27.

U.S. Environmental Protection Agency (2023b). Overview of the safe drinking water act. Accessed: 2026-03-27.

U.S. Environmental Protection Agency (2024). Point-of-use and point-of-entry treatment cost estimating tool. Accessed: March 31, 2026.

U.S. Fire Administration (2024). Preliminary after-action report on the 2023 maui wildfire. Accessed: 2025-04-24.

USDA Natural Resources Conservation Service (2024). Colorado snow survey and water supply products. https://www.wcc.nrcs.usda.gov/ftpref/support/states/CO/products/#state=co&element=wteq. Accessed: 2026-03-30.

van Dijk, J., Delacrétaz, N., and Lanz, B. (2022). Technology adoption and early network infrastructure provision in the market for electric vehicles. *Environmental and Resource Economics*, 83(3):631–679.

Varghese, A. M., Menon, N., and Ermagun, A. (2024). Equitable distribution of electric vehicle charging infrastructure: A systematic review. *Renewable and Sustainable Energy Reviews*, 206:114825.

Walls, M. A. and Wibbenmeyer, M. (2023). How local are the local economic impacts of wildfires. *Resources for the Future: Washington, DC, USA*.

Wang, J., Huang, C., He, D., and Tu, R. (2023). Range anxiety among battery electric vehicle users: Both distance and waiting time matter. In *Human Factors and Ergonomics Society Annual Meeting*, volume 67, pages 1309–1315. SAGE Publications Sage CA: Los Angeles, CA.

Wang, Y. and Lewis, D. J. (2024). Wildfires and climate change have lowered the economic value of western us forests by altering risk expectations. *Journal of Environmental Economics and Management*, 123:102894.

Warziniack, T., Sham, C. H., Morgan, R., and Feferholtz, Y. (2017). Effect of forest cover on water treatment costs. *Water Economics and Policy*, 3(04):1750006.

Warziniack, T. and Thompson, M. (2013). Wildfire risk and optimal investments in watershed protection. In *Western Economics Forum. 12 (2): 19-28.*, pages 19–28.

Wasserman, T. (2020). Wildfire trends across the western us: Forest fires have increased in size, severity, and frequency across western forests. *ERI White Paper—Issues in Forest Restoration Ecological Restoration Institute, Northern Arizona University*, 10.

Wasserman, T. N. and Mueller, S. E. (2023). Climate influences on future fire severity: a synthesis of climate-fire interactions and impacts on fire regimes, high-severity fire, and forests in the western united states. *Fire Ecology*, 19(1):43.

Weber, K. T. and Yadav, R. (2020). Spatiotemporal trends in wildfires across the western united states (1950–2019). *Remote Sensing*, 12(18):2959.

Westerling, A. L. (2016). Increasing western us forest wildfire activity: sensitivity to changes in the timing of spring. *Philosophical Transactions of the Royal Society B: Biological Sciences*, 371(1696).

Western Forestry Leadership Coalition (2022). The true cost of wildfire in the western u.s. Technical report, Western Forestry Leadership Coalition. Accessed April 2025.

Wibbenmeyer, M., Sloggy, M. R., and Sánchez, J. J. (2023). Economic analysis of wildfire impacts to water quality: a review. *Journal of Forestry*, 121(4):374–382.

Writer, J. H., Hohner, A., Oropeza, J., Schmidt, A., Cawley, K. M., and Rosario-Ortiz, F. L. (2014). Water treatment implications after the high park wildfire, colorado. *Journal-American Water Works Association*, 106(4):E189–E199.

Xu, M., Meng, Q., Liu, K., and Yamamoto, T. (2017). Joint charging mode and location choice model for battery electric vehicle users. *Transportation Research Part B: Methodological*, 103:68–86.

Xu, M., Yang, H., and Wang, S. (2020). Mitigate the range anxiety: Siting battery charging stations for electric vehicle drivers. *Transportation Research Part C: Emerging Technologies*, 114:164–188.

Yi, T., Zhang, C., Lin, T., and Liu, J. (2020). Research on the spatial-temporal distribution of electric vehicle charging load demand: A case study in china. *Journal of Cleaner Production*, 242:118457.

Yu, Q., Que, T., Cushing, L. J., Pierce, G., Shen, K., Kejriwal, M., Yao, Y., and Zhu, Y. (2025). Equity and reliability of public electric vehicle charging stations in the united states. *Nature Communications*, 16(1):5291.

Zatsarnaja, J., Reiter, K., Mehdizadeh, M., Nayum, A., and Nordfjaern, T. (2025). Charged up with peace in mind: Unraveling the factors of range anxiety among norwegian electric vehicle drivers. *Transportation Research Part F: Traffic Psychology and Behaviour*, 110:15–28.

Zhang, Y., Li, W., Caldwell, P., Sebestyen, S. D., Tang, C., Toot, R., Mihiar, C., Mondry, Z., Song, Y., Mosher, D., et al. (2025). Contemporary patterns of prescribed fire and its risks and benefits to water quantity and water quality over the conterminous united states. *Environmental Research Letters*, 20(12):123002.

Zheng, Y., Liu, D., An, F., Wang, J., Gao, X., and Jia, N. (2025). Impact of charging infrastructure construction on electric vehicle diffusion based on a multi-agent model. *iScience*, 28(4).

Zivin, J. G., Neidell, M., and Schlenker, W. (2011). Water quality violations and avoidance behavior: Evidence from bottled water consumption. *American Economic Review*, 101(3):448–453.

Zou, T., Khaloei, M., and MacKenzie, D. (2020). Effects of charging infrastructure characteristics on electric vehicle preferences of new and used car buyers in the united states. *Transportation Research Record*, 2674(12):165–175.

Appendix A

Supplementary Material for Chapter 2

A.1 Data Sources

Colorado Tourism Office (Dean Runyan Associates). County-level tourism spending data are obtained from the Colorado Tourism Office, which partners with Dean Runyan Associates (DRA) to produce the annual *Colorado Travel Impacts Report*. DRA uses its Regional Travel Impact Model (RTIM), a "bottom-up" framework developed to estimate the economic impacts of visitor activity at fine geographic scales without relying on annual survey sampling. The model integrates a wide array of private and public data sources to estimate visitor spending, employment, earnings, and tax receipts attributable to tourism in each county. Core private data inputs include lodging metrics from Smith Travel Research (STR) and Key Data for hotels, motels, and short-term vacation rentals, as well as occupancy and pricing information from AllStays and the U.S. Forest Service for public and commercial campgrounds. Public data sources include U.S. Bureau of Labor Statistics employment and wage records, Bureau of Economic Analysis industry earnings, U.S. Census Bureau business patterns, second-home inventories, and state and local government finance data.

Expenditures are modeled separately by lodging category (commercial lodging, camping, second homes, and day travel) each calibrated using lodging tax receipts, inventory data, and visitor expenditure profiles. For commercial accommodations, lodging sales derived from tax receipts are multiplied by expenditure distribution shares to generate estimates of total visitor spending across sectors (lodging, food service, retail, recreation, and transportation). Campground impacts are estimated from site inventories, occupancy data, and average party expenditures, while second-home travel spending is derived from census-based housing utilization and expenditure benchmarks. All spending estimates are reconciled with state-level aggregates and cross-validated against independent datasets (e.g., BLS industry employment, STR sales, and Key Data metrics) to ensure internal consistency. The resulting dataset provides annual county-level estimates of total visitor spending, tourism-related employment, and both county and state tax revenues from 1996–2019.

National Interagency Fire Center (NIFC). Wildfire data are obtained from the National Interagency Fire Center (NIFC) through the Wildland Fire Interagency Geospatial Services (WFIGS) Group, which provides authoritative geospatial data products under the interagency Wildland Fire Data Program. The WFIGS database, hosted within the National Interagency Fire Center ArcGIS Online Organization (the NIFC Org), serves as the official repository for national wildfire perimeter data. It integrates records from the Integrated Reporting of Wildland Fire Information (IRWIN) system and includes all incidents categorized as Wildfire (WF) or Prescribed Fire (RX) that are valid, non-quarantined, and publicly accessible. Each perimeter record must meet interagency publication standards—specifically, a *Feature Access* of *Public*, a *Feature Status* of *Approved*, and an *Is Visible* setting of *Yes*.

Perimeters are derived from multiple authoritative federal sources, with internal rules ensuring that the most current or most reliable polygon is retained when overlaps occur. Data are refreshed continuously, with updates propagated every five minutes, though changes in perimeter sources may take up to fifteen minutes to appear. The database is an ongoing project aimed at developing a comprehensive national history of wildland fire perimeters extending indefinitely into the past. While coverage before 2021 remains incomplete, WFIGS represents the best-available spatial record of wildfire activity across the United States.

For this study, annual wildfire shapefiles were downloaded from WFIGS and used to identify the spatial intersection between burn perimeters and county boundaries in Colorado. A county is classified as treated if any portion of its land area intersects a wildfire perimeter meeting the "mega-fire" threshold ($\geq$ 30,000 acres). This spatial overlay approach ensures that treatment assignments reflect the true geographic extent of wildfire exposure rather than administrative or reporting boundaries.

Colorado State Demography Office. Population data are obtained from the Colorado State Demography Office, which provides annual county-level population estimates. These data are used to normalize all monetary variables to a per capita basis, allowing for consistent comparison of tourism spending and tax revenues across counties and over time.

A.1.1 Variable Construction

This subsection defines the key variables used in the analysis and describes their transformations.

- $VisitorSpending_{it}$: total visitor spending per capita in county i and year t.
- $CountyTax_{it}$ and $StateTax_{it}$: county-level and state-level tourism-generated tax revenues per capita in county i and year t.
- $first\_treated\_year_i$: the first year in which county i was exposed to a mega-fire (burn area $\geq 30{,}000$ acres for the 1% treatment cutoff).
- Treatment status is defined using wildfire size thresholds corresponding to the top 10%, 5%, and 1% of fires by burned area.

A.1.2 Summary Statistics

Balance tables for different model specifications across treatment cutoffs are found in Table A.1

Table A.1: Balance Statistics for Treated and Never-Treated Counties Across Wildfire Size Cutoffs. All estimates are expressed in per capita terms.

Variable	Never-Treated				Treated			
	Mean	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**
Panel A: 0.5% Cutoff								
Visitor Spending	5615.69	6535.23	407.14	27416.38	5921.32	9175.67	0.00	51163.41
County Tax	183.40	256.29	5.12	1168.03	192.55	361.64	0.00	3092.45
State Tax	151.00	172.66	19.99	792.60	158.85	213.95	0.00	1399.61
Campground Spending	1040.63	1693.29	4.66	5713.49	349.77	718.93	0.00	6024.55
Hotel/Motel Spending	2737.97	3293.68	0.00	17308.92	3614.60	6396.93	0.00	38194.30
Panel B: 1% Cutoff								
Visitor Spending	5980.43	9223.48	0.00	51163.41	5583.98	7520.93	407.14	51138.91
State Tax	159.71	211.49	0.00	1386.32	151.86	196.68	19.99	1399.61
County Tax	191.50	357.25	0.00	3092.45	189.63	314.86	5.12	2593.59
Campground Spending	374.87	762.45	0.00	6024.55	708.10	1396.13	4.66	5713.49
Hotel/Motel Spending	3643.44	6496.04	0.00	38194.30	3005.52	4405.57	0.00	32524.55
Panel C: 5% Cutoff								
Visitor Spending	4663.98	6422.02	407.14	51138.91	7078.77	10494.96	0.00	51163.41
County Tax	157.11	270.45	2.97	2593.59	224.99	404.92	0.00	3092.45
State Tax	130.59	165.58	19.99	1399.61	184.52	239.31	0.00	1386.32
Campground Spending	544.30	1079.11	0.00	5713.49	393.00	895.48	0.00	6024.55
Hotel/Motel Spending	2555.36	4026.92	0.00	32524.55	4377.61	7345.73	0.00	38194.30
Panel D: 10% Cutoff								
Visitor Spending	4750.61	6334.67	407.14	51138.91	7408.98	11130.82	0.00	51163.41
County Tax	159.41	264.43	2.97	2593.59	234.46	429.89	0.00	3092.45
State Tax	134.64	163.01	19.99	1399.61	188.98	253.02	0.00	1386.32
Campground Spending	491.45	1012.76	0.00	5713.49	437.77	968.43	0.00	6024.55
Hotel/Motel Spending	2662.99	4050.73	0.00	32524.55	4569.09	7785.76	0.00	38194.30

A.2 Event-Study ATT(g, t) Tables

Each table reports time-specific effects relative to the wildfire year at the 1% wildfire threshold. Full event tables for other wildfire size thresholds are available upon request.

Table A.2: Event Study ATT Estimates for Total Visitor Spending (Per Capita) at 1% cutoff threshold

t	ATT	Std. Error	95% CI Lower	95% CI Upper	Signif.
-10	-98.52	161.10	-467.73	270.69	
-9	271.95	147.35	-65.76	609.65	
-8	31.01	228.12	-491.81	553.84	
-7	-52.87	116.10	-318.95	213.20	
-6	-53.20	282.68	-701.07	594.67	
-5	-269.70	132.19	-572.65	33.25	
-4	150.16	513.32	-1026.29	1326.62	
-3	12.24	91.51	-197.49	221.97	
-2	-256.07	249.87	-828.74	316.60	
-1	-80.26	162.51	-452.71	292.19	
0	-239.57	269.55	-857.34	378.20	
1	-270.94	328.77	-1024.45	482.56	
2	6.37	507.57	-1156.91	1169.64	
3	42.86	438.14	-961.30	1047.02	
4	-507.74	366.07	-1346.71	331.23	
5	-879.49	447.56	-1905.23	146.25	
6	-1354.16	561.48	-2640.99	-67.33	*
7	-1433.23	573.10	-2746.70	-119.76	*
8	-1115.84	397.24	-2026.27	-205.41	*
9	-1295.90	447.64	-2321.84	-269.97	*
10	-1498.56	522.11	-2695.17	-301.95	*

Table A.3: Event Study ATT Estimates for County Tax Receipts (Per Capita) at 1% cutoff threshold

t	ATT	Std. Error	95% CI Lower	95% CI Upper	Signif.
-10	-3.13	6.74	-19.29	13.03	
-9	3.76	3.87	-5.52	13.05	
-8	1.25	8.26	-18.56	21.07	
-7	-3.28	3.19	-10.93	4.38	
-6	-0.79	11.56	-28.53	26.94	
-5	-10.46	4.61	-21.53	0.60	
-4	3.92	15.33	-32.84	40.68	
-3	-4.25	4.97	-16.18	7.68	
-2	-17.71	12.82	-48.46	13.04	
-1	-4.72	6.98	-21.47	12.02	
0	-11.63	10.99	-37.99	14.74	
1	-12.86	14.46	-47.54	21.82	
2	-3.65	21.98	-56.37	49.07	
3	-7.14	23.29	-63.00	48.72	
4	-37.95	22.42	-91.71	15.82	
5	-50.81	24.74	-110.14	8.53	
6	-70.55	29.18	-140.54	-0.56	*
7	-77.87	29.92	-149.63	-6.11	*
8	-79.54	25.58	-140.89	-18.20	*
9	-89.30	28.79	-158.35	-20.25	*
10	-100.44	31.86	-176.85	-24.04	*

Table A.4: Event Study ATT Estimates for State Tax Receipts (Per Capita) at 1% cutoff threshold

t	ATT	Std. Error	95% CI Lower	95% CI Upper	Signif.
-10	-3.00	4.57	-13.65	7.65	
-9	5.59	3.22	-1.92	13.10	
-8	0.96	5.84	-12.65	14.56	
-7	-0.33	2.90	-7.08	6.43	
-6	-0.16	7.42	-17.46	17.14	
-5	-6.13	3.00	-13.13	0.87	
-4	2.81	11.88	-24.88	30.50	
-3	-0.93	2.64	-7.08	5.23	
-2	-6.46	5.69	-19.72	6.81	
-1	-1.62	4.48	-12.07	8.82	
0	-6.15	6.36	-20.97	8.68	
1	-6.02	8.94	-26.85	14.81	
2	1.90	15.28	-33.71	37.50	
3	4.43	11.86	-23.20	32.06	
4	-11.92	10.50	-36.37	12.53	
5	-20.55	12.26	-49.11	8.00	
6	-35.03	14.41	-68.60	-1.47	*
7	-38.28	15.83	-75.17	-1.40	*
8	-29.97	10.12	-53.56	-6.39	*
9	-34.85	11.12	-60.75	-8.95	*
10	-39.67	14.06	-72.44	-6.91	*

Table A.5: Event Study ATT Estimates for Hotel/Motel Spending (Per Capita) at 1% cutoff threshold

t	ATT	Std. Error	95% CI Lower	95% CI Upper	Signif.
-10	-68.61	117.42	-348.44	211.23	
-9	225.71	122.31	-65.77	517.18	
-8	-28.00	147.54	-379.61	323.62	
-7	-50.22	126.28	-351.18	250.73	
-6	-17.23	257.78	-631.56	597.10	
-5	-201.40	82.53	-398.07	-4.73	*
-4	32.97	329.99	-753.47	819.41	
-3	1.70	80.30	-189.68	193.08	
-2	-217.49	219.12	-739.69	304.72	
-1	-68.25	137.23	-395.30	258.81	
0	-183.05	225.50	-720.46	354.36	
1	-178.60	295.28	-882.31	525.10	
2	52.93	414.18	-934.15	1040.02	
3	91.63	366.20	-781.08	964.35	
4	-454.94	323.90	-1226.86	316.98	
5	-780.87	388.93	-1707.77	146.03	
6	-1140.00	443.87	-2197.82	-82.18	*
7	-1211.84	541.98	-2503.47	79.80	
8	-959.21	308.09	-1693.45	-224.96	*
9	-1096.00	364.12	-1963.78	-228.22	*
10	-1265.67	445.07	-2326.37	-204.98	*

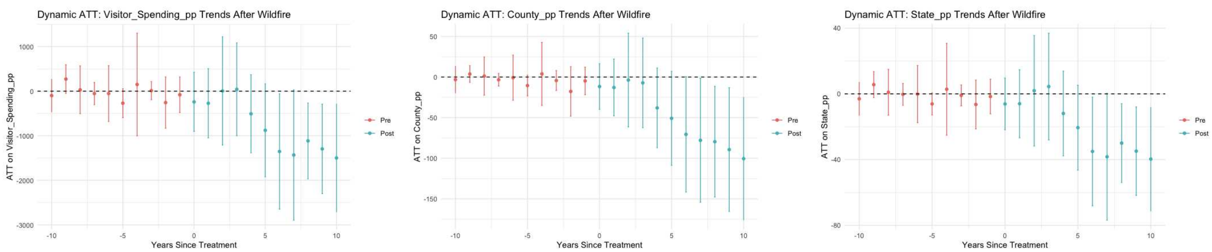
Table A.6: Event Study ATT Estimates for Campground Spending (Per Capita) at 1% cutoff threshold

t	ATT	Std. Error	95% CI Lower	95% CI Upper	Signif.
-10	-5.72	21.71	-124.45	113.01	
-9	-1.43	10.76	-60.27	57.42	
-8	47.47	42.73	-186.27	281.21	
-7	0.43	25.42	-138.59	139.45	
-6	-2.26	5.00	-29.60	25.08	
-5	-28.81	44.32	-271.21	213.59	
-4	66.03	78.37	-362.63	494.70	
-3	-2.47	5.68	-33.53	28.60	
-2	-11.42	13.99	-87.94	65.10	
-1	10.08	22.16	-111.15	131.30	
0	-30.89	31.78	-204.72	142.93	
1	-45.92	58.02	-363.30	271.45	
2	-46.35	80.84	-488.51	395.81	
3	-49.43	91.77	-551.43	452.57	
4	-39.62	26.30	-183.49	104.26	
5	-13.20	15.16	-96.14	69.75	
6	-50.72	31.84	-224.90	123.47	
7	-27.59	25.16	-165.20	110.03	
8	-1.70	20.42	-113.40	110.00	
9	-17.63	26.96	-165.11	129.85	
10	-32.55	36.37	-231.50	166.40	

A.3 Event Study Figures

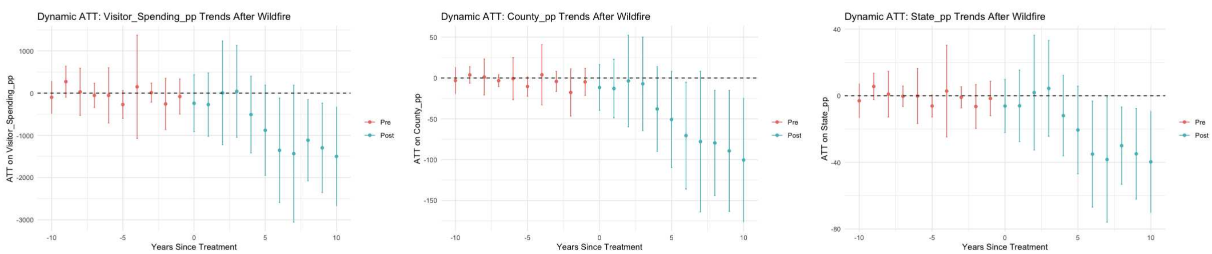
A.3.1 1% Cutoff

Figure A.1: Baseline specification: event-study estimates under the 1% wildfire size cutoff.



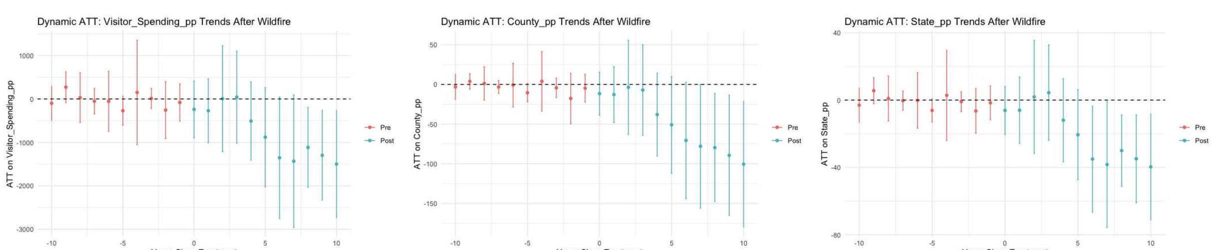
(a) Visitor Spending **(b)** County Tax **(c)** State Tax

Figure A.2: Rural-only specification: event-study estimates under the 1% wildfire size cutoff.



(a) Visitor Spending **(b)** County Tax **(c)** State Tax

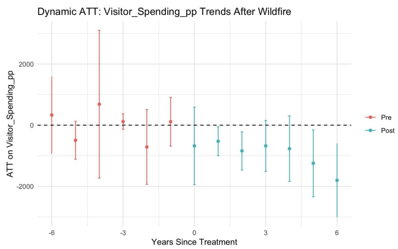
Figure A.3: Excluding La Plata County: event-study estimates under the 1% wildfire size cutoff.



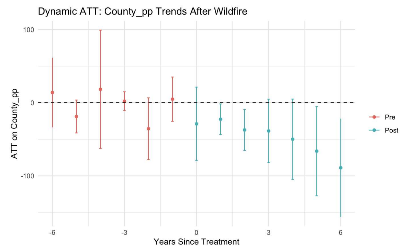
(a) Visitor Spending **(b)** County Tax **(c)** State Tax

A.3.2 0.5% Cutoff

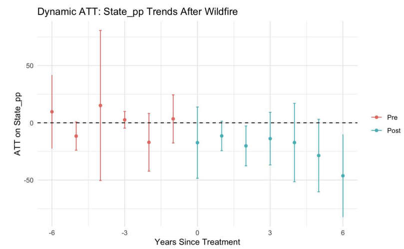
Figure A.4: Baseline specification: event-study estimates under the 0.5% wildfire size cutoff.



(a) Visitor Spending



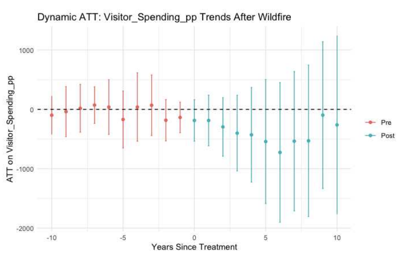
(b) County Tax



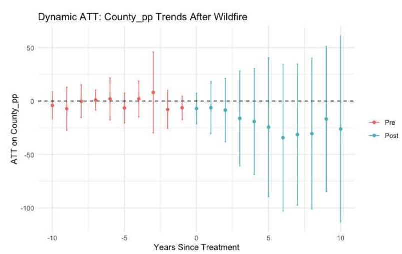
(c) State Tax

A.3.3 5% Cutoff

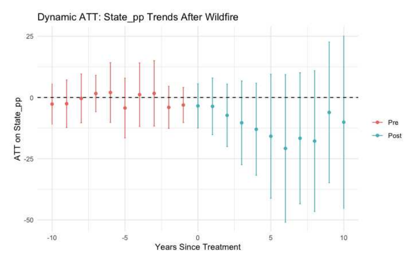
Figure A.5: Baseline specification: event-study estimates under the 5% wildfire size cutoff.



(a) Visitor Spending



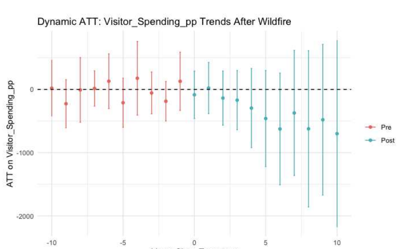
(b) County Tax



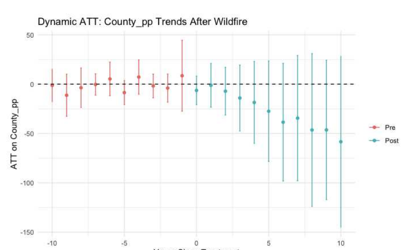
(c) State Tax

A.3.4 10% Cutoff

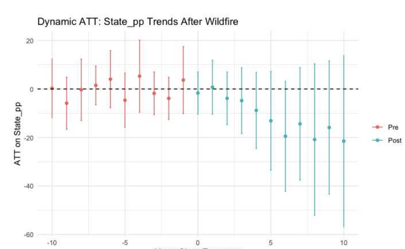
Figure A.6: Baseline specification: event-study estimates under the 10% wildfire size cutoff.



(a) Visitor Spending



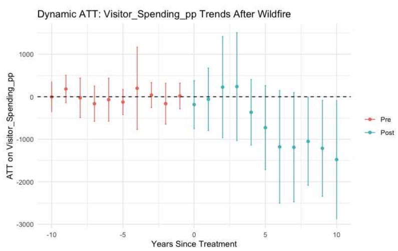
(b) County Tax



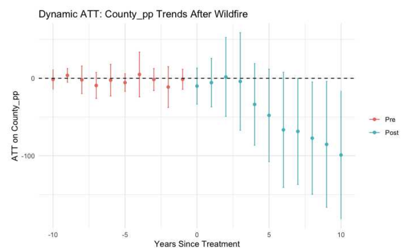
(c) State Tax

A.4 Spillover Event-Study Figures

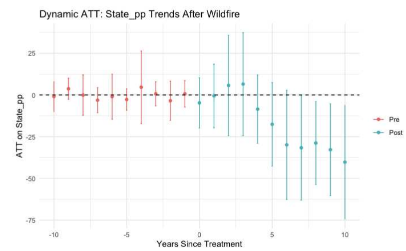
Figure A.7: Event-study estimates for spillover exposure buffers around wildfire perimeters. Rows vary buffer radius (10/25/50 miles); columns vary outcome (Visitor Spending, County Tax, State Tax).



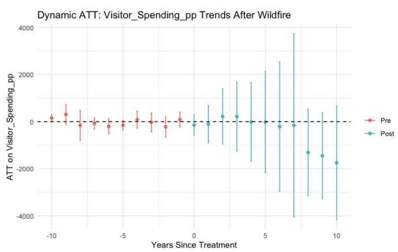
(a) Visitor Spending, 10 miles



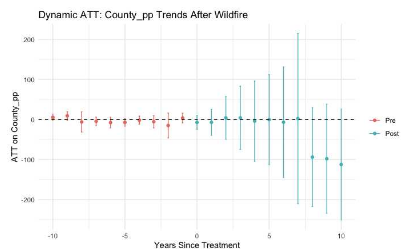
(b) County Tax, 10 miles



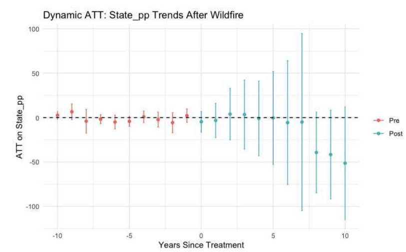
(c) State Tax, 10 miles



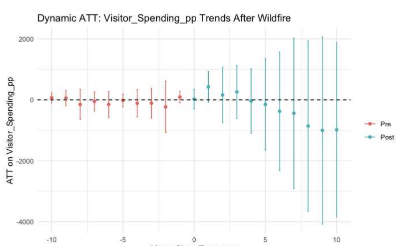
(d) Visitor Spending, 25 miles



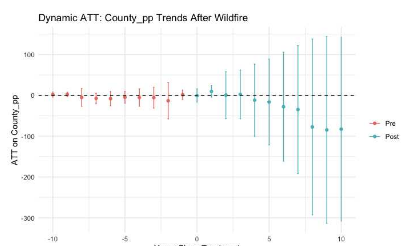
(e) County Tax, 25 miles



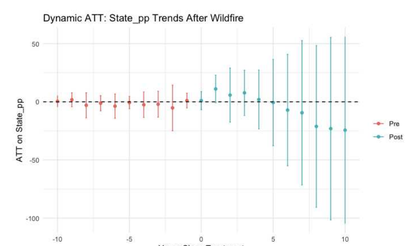
(f) State Tax, 25 miles



(g) Visitor Spending, 50 miles



(h) County Tax, 50 miles



(i) State Tax, 50 miles

A.5 Event-Study Figures for Robustness Checks (1% Cutoff)

A.5.1 Excluding Front Range Counties

Figure A.8: Event-study estimates for total visitor spending (excluding Front Range counties).

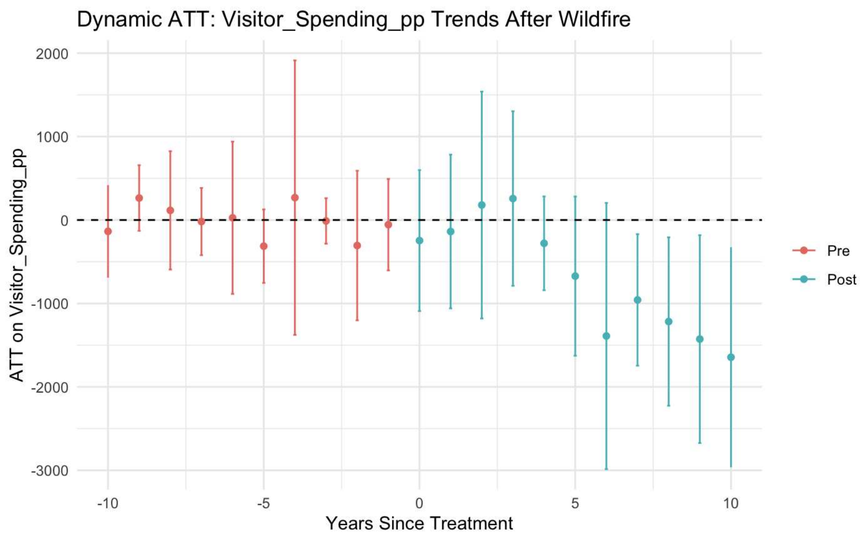


Figure A.9: Event-study estimates for county tax receipts (excluding Front Range counties).

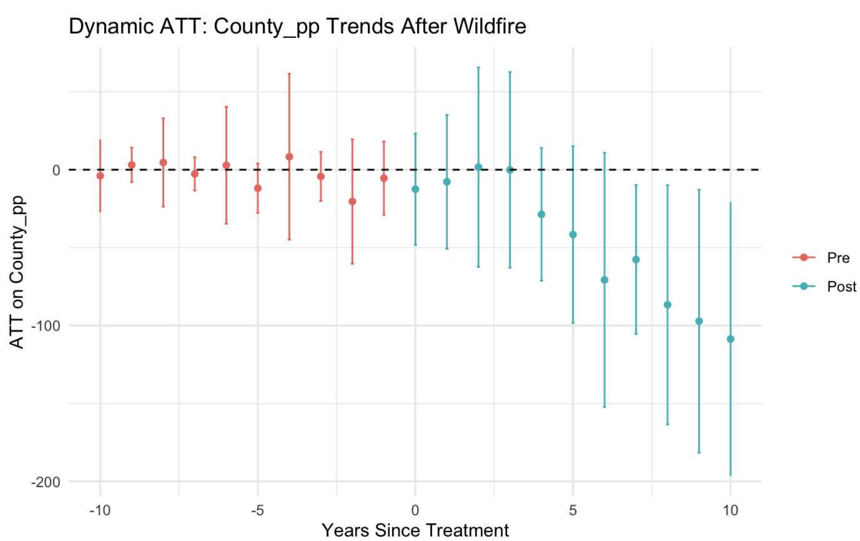


Figure A.10: Event-study estimates for state tax receipts (excluding Front Range counties).

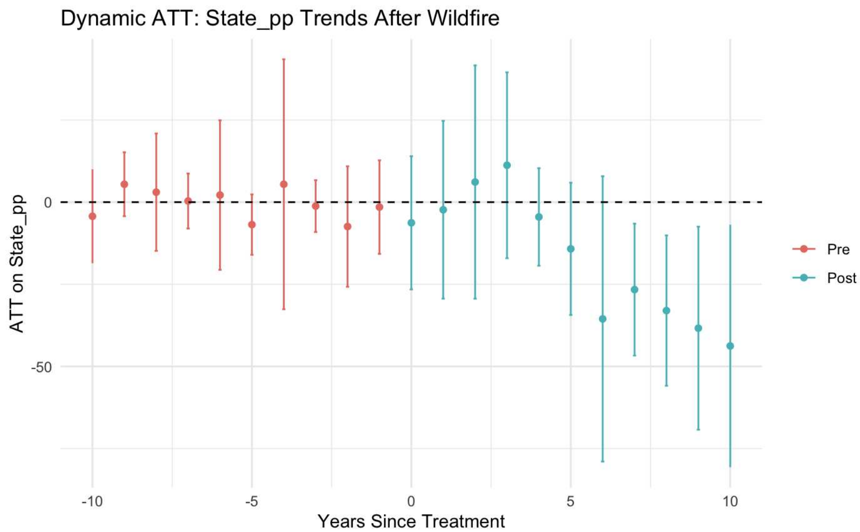


Figure A.11: Event-study estimates for campground spending (excluding Front Range counties).

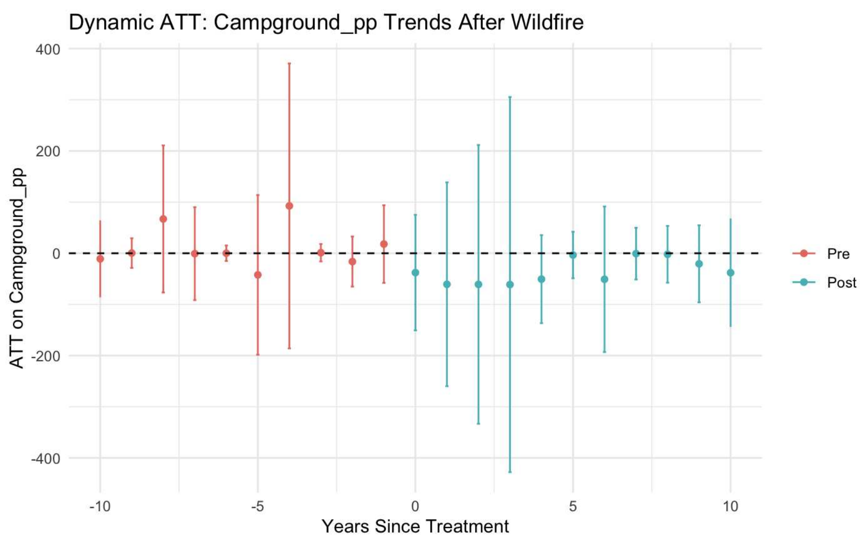
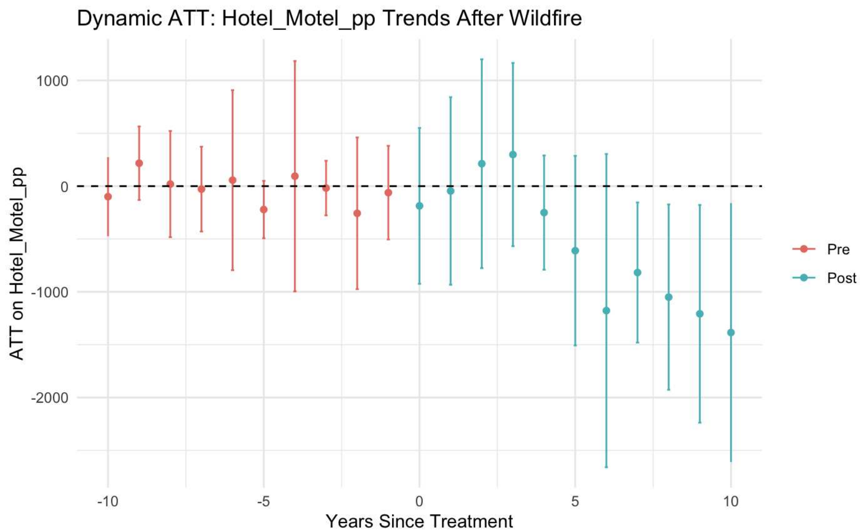


Figure A.12: Event-study estimates for hotel and motel spending (excluding Front Range counties).



A.5.2 Excluding Twice Treated Counties (1% cutoff)

Figure A.13: Event-study estimates for total visitor spending (excluding twice-treated counties).

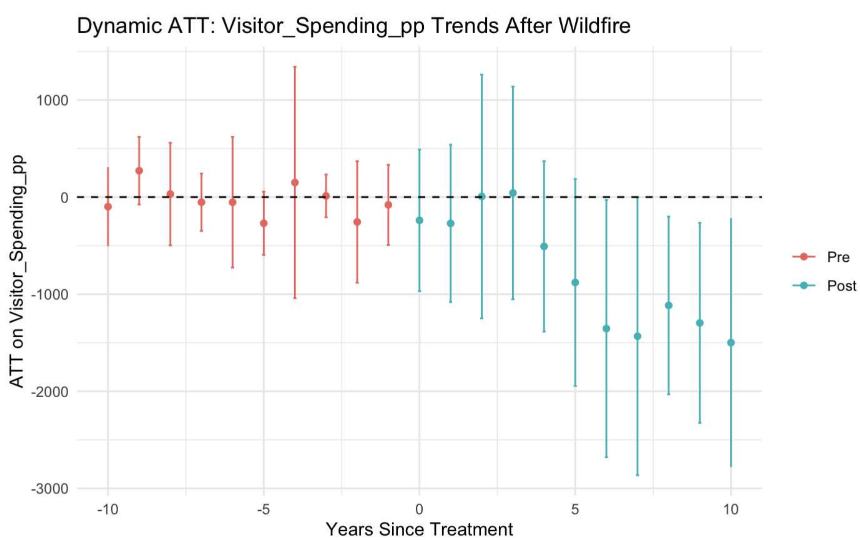


Figure A.14: Event-study estimates for county tax receipts (excluding twice-treated counties).

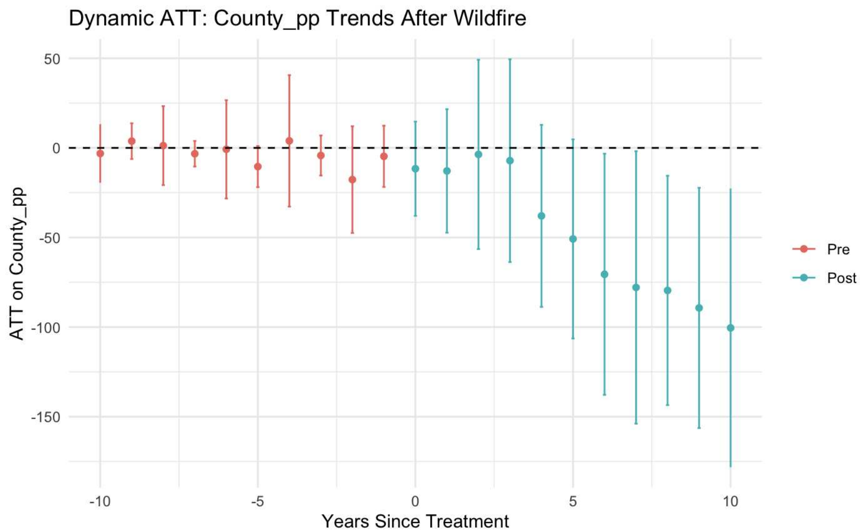


Figure A.15: Event-study estimates for state tax receipts (excluding twice-treated counties).

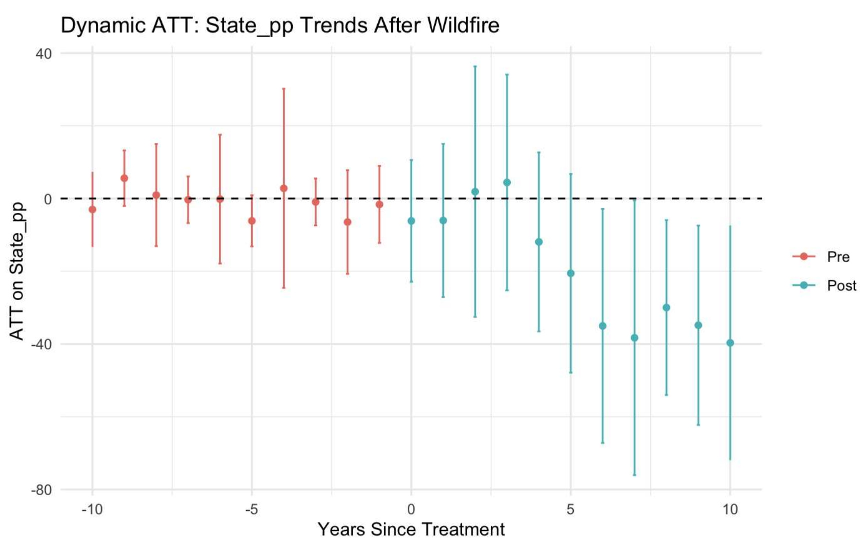


Figure A.16: Event-study estimates for campground spending (excluding twice-treated counties).

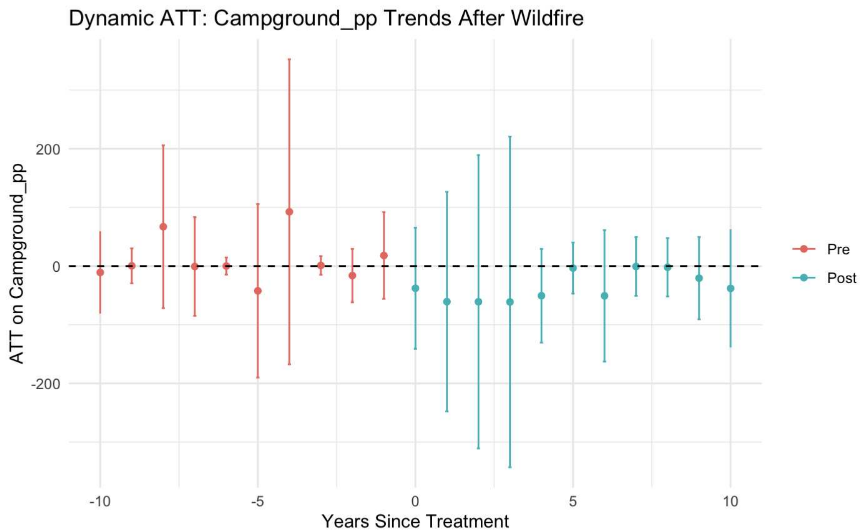
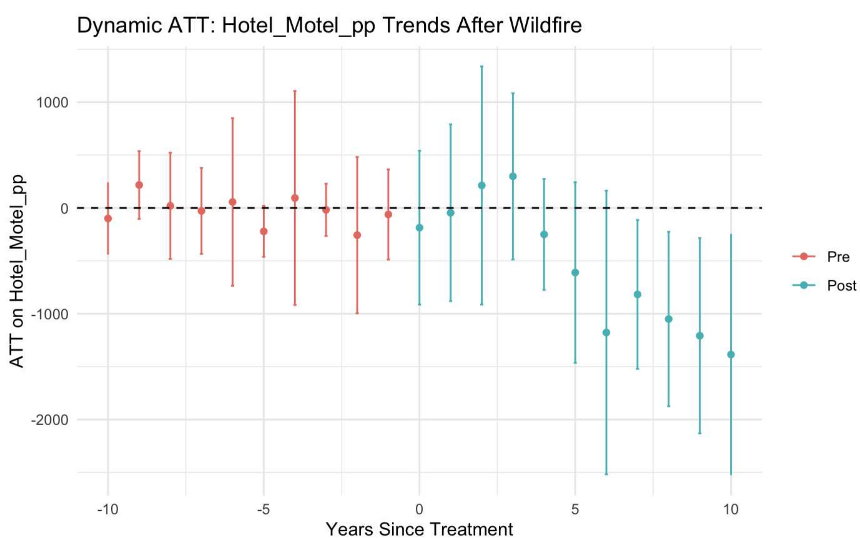


Figure A.17: Event-study estimates for hotel and motel spending (excluding twice-treated counties).



Appendix B

Supplementary Material for Chapter 3

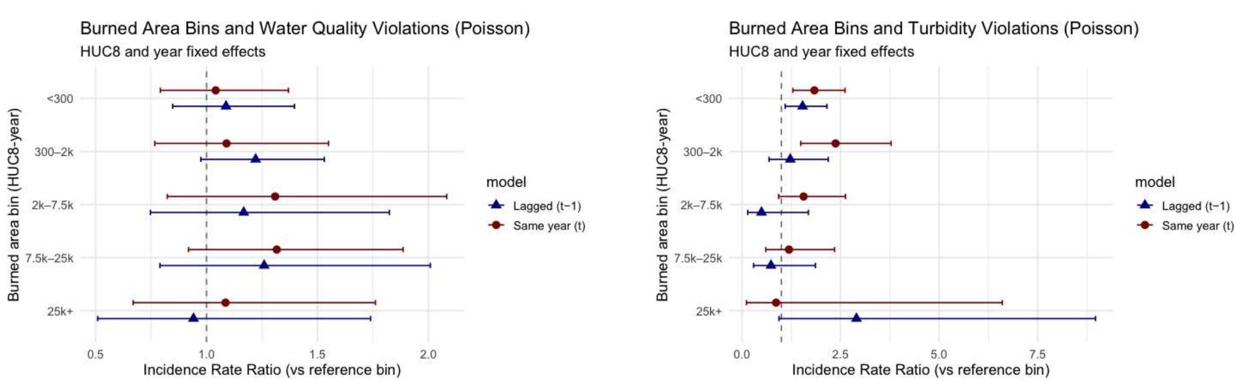
B.1 Poisson Robustness Check

As a robustness check, the baseline specifications are re-estimated using Poisson models, which impose the stronger assumption that the conditional mean and variance of the outcome are equal. In contrast, the negative binomial specification allows for overdispersion in the count outcome. While this assumption is restrictive, the Poisson model provides a useful benchmark under alternative variance assumptions. Overall, the Poisson estimates are qualitatively consistent with the baseline negative binomial results, reinforcing the central findings of the analysis.

B.1.1 Baseline Poisson Estimates

Table B.1 reports Poisson estimates using the same wildfire exposure bins as the baseline specification, while Figure B.1 provides a graphical representation of the estimated incidence rate ratios. These models include wildfire exposure in both the contemporaneous year and the following year, along with watershed and year fixed effects.

Figure B.1: Poisson Estimates: Wildfire Exposure and Water Quality Violations



(a) Total Violations

(b) Turbidity Violations

Notes: Points show incidence rate ratios (IRRs) from Poisson models including wildfire exposure in years t and $t-1$. Horizontal bars denote 95% confidence intervals. The vertical line at IRR$=1$ indicates no effect. The reference category is zero wildfire exposure.

Table B.1: Poisson Estimates: Wildfire Burned Area and Water Quality Violations

	Total Violations		Turbidity Violations	
Burned Area Bin	**t**	**t−1**	**t**	**t−1**
<300	**1.02**	**1.11**	**1.84*****	**1.58****
	[0.77, 1.37]	[0.88, 1.40]	[1.29, 2.63]	[1.12, 2.23]
300–2k	**1.08**	**1.23**	**2.37*****	**1.24**
	[0.76, 1.54]	[0.99, 1.53]	[1.47, 3.80]	[0.69, 2.23]
2k–7.5k	**1.31**	**1.17**	**1.59**	**0.50**
	[0.82, 2.09]	[0.76, 1.83]	[0.95, 2.66]	[0.14, 1.71]
7.5k–25k	**1.31**	**1.28**	**1.20**	**0.73**
	[0.91, 1.89]	[0.80, 2.04]	[0.61, 2.37]	[0.28, 1.90]
25k+	**1.08**	**0.95**	**0.89**	**3.01**
	[0.67, 1.76]	[0.51, 1.75]	[0.12, 6.68]	[0.96, 9.49]
Watershed FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Incidence rate ratios (IRRs) shown with 95% confidence intervals in brackets. Poisson models include burned area in year t and $t-1$. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. The reference category is zero burned acres.

The Poisson estimates generally align with the negative binomial results in terms of direction and magnitude, although statistical significance varies somewhat across bins. For total violations, the Poisson estimates are more imprecise and do not yield statistically significant effects across bins. Nevertheless, the direction and relative magnitude of the coefficients remain broadly comparable to those obtained under the negative binomial specification.

For turbidity violations, the Poisson results are similar to the baseline estimates. In both models, the two bins associated with the smallest burned areas (fires < 2000 acres, or less than 0.1% of watershed area) show significant increases in turbidity violations. The magnitude of the estimates is also similar across specifications: the Poisson model estimates an 84–137% increase in turbidity violations, compared with a 79–156% increase in the negative binomial model.

Table B.2: Comparison of Poisson and Negative Binomial Estimates (Total Violations)

	Poisson		Negative Binomial	
Burned Area Bin	**t**	**t−1**	**t**	**t−1**
<300	1.02	1.11	1.20	1.09
300–2k	1.08	1.23	1.48*	1.49**
2k–7.5k	1.31	1.17	1.53	1.16
7.5k–25k	1.31	1.28	1.28	0.86
25k+	1.08	0.95	1.33	1.02

B.1.2 Quintile Specification

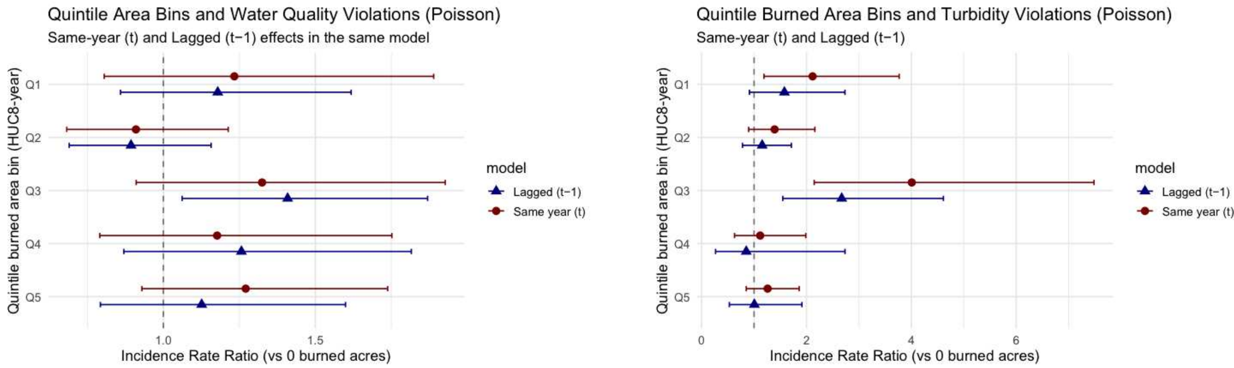
To further examine the robustness of the results, the analysis is repeated using an equal-sized quintile binning of wildfire exposure. Table B.3 reports Poisson estimates under this specification, while Figure B.2 presents the corresponding graphical results.

Table B.3: Poisson Estimates: Wildfire Exposure (Equal Five-Bin Specification) and Water Quality Violations

	Total Violations		Turbidity Violations	
Burn Quintile	**t**	**t−1**	**t**	**t−1**
Q1	**1.23**	**1.18**	**2.12***	**1.58**
	[0.81, 1.89]	[0.86, 1.62]	[1.19, 3.77]	[0.91, 2.73]
Q2	**0.91**	**0.89**	**1.39**	**1.15**
	[0.68, 1.21]	[0.69, 1.16]	[0.90, 2.16]	[0.78, 1.71]
Q3	**1.32**	**1.41***	**4.01*****	**2.67*****
	[0.91, 1.93]	[1.06, 1.87]	[2.15, 7.48]	[1.55, 4.61]
Q4	**1.18**	**1.26**	**1.12**	**0.85**
	[0.79, 1.75]	[0.87, 1.82]	[0.63, 1.99]	[0.26, 2.73]
Q5	**1.27**	**1.13**	**1.26**	**1.00**
	[0.93, 1.74]	[0.79, 1.60]	[0.85, 1.86]	[0.53, 1.91]
Watershed FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Incidence rate ratios (IRRs) shown with 95% confidence intervals in brackets. Poisson models include wildfire exposure in years t and $t-1$. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. The reference category is no wildfire exposure.

Figure B.2: Poisson Estimates: Wildfire Exposure (Quintile Specification)



(a) Total Violations

(b) Turbidity Violations

Notes: Points show incidence rate ratios (IRRs) from Poisson models using wildfire exposure quintiles and including exposure in years t and $t-1$. Horizontal bars denote 95% confidence intervals. The vertical line at IRR$=1$ indicates no effect. The reference category is no wildfire exposure.

Tables B.4 and B.5 compare the quintile results across Poisson and negative binomial specifications.

The quintile results are broadly consistent across the negative binomial and Poisson specifications. For total violations, Q1 and Q2 are statistically insignificant under both models. Q3 is statistically significant in both the contemporaneous and lagged periods under the negative binomial specification, while under the Poisson model significance appears only for the lagged effect. Estimated magnitudes are similar across the two models. Q4 is statistically significant in the negative binomial model but not in the Poisson specification, although the point estimates are comparable.

A similar pattern emerges for turbidity violations. Q3 is statistically significant under both the Poisson and negative binomial models, with nearly identical estimated magnitudes across specifications. The primary difference occurs in Q1, which is statistically significant under Poisson but not under the negative binomial model; however, the estimated effects are similar in magnitude. The remaining quintiles are statistically insignificant under both specifications. Overall, the consistency in magnitude and direction across models suggests that the quintile results are not driven by functional form assumptions.

Table B.4: Quintile Specification: Poisson vs. Negative Binomial (Total Violations)

	Poisson		Negative Binomial	
Burn Quintile	**t**	**t−1**	**t**	**t−1**
Q1	**1.23**	**1.18**	**1.50**	**1.28**
	[0.81, 1.89]	[0.86, 1.62]	[0.93, 2.42]	[0.85, 1.93]
Q2	**0.91**	**0.89**	**0.95**	**0.88**
	[0.68, 1.21]	[0.69, 1.16]	[0.69, 1.31]	[0.61, 1.26]
Q3	**1.32**	**1.41***	**1.66***	**1.46***
	[0.91, 1.93]	[1.06, 1.87]	[1.03, 2.65]	[1.03, 2.08]
Q4	**1.18**	**1.26**	**1.64***	**1.56***
	[0.79, 1.75]	[0.87, 1.82]	[1.06, 2.54]	[1.06, 2.30]
Q5	**1.27**	**1.13**	**1.36**	**1.00**
	[0.93, 1.74]	[0.79, 1.60]	[0.95, 1.93]	[0.72, 1.39]
Watershed FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Incidence rate ratios (IRRs) shown with 95% confidence intervals in brackets. Both specifications include wildfire exposure in years t and $t-1$. $^{*}p < 0.05$.

Table B.5: Quintile Specification: Poisson vs. Negative Binomial (Turbidity Violations)

	Poisson		Negative Binomial	
Burn Quintile	**t**	**t−1**	**t**	**t−1**
Q1	**2.12***	**1.58**	**1.98**	**1.45**
	[1.19, 3.77]	[0.91, 2.73]	[0.98, 4.00]	[0.82, 2.57]
Q2	**1.39**	**1.15**	**1.32**	**1.15**
	[0.90, 2.16]	[0.78, 1.71]	[0.87, 2.00]	[0.68, 1.93]
Q3	**4.01*****	**2.67*****	**4.01*****	**2.57****
	[2.15, 7.48]	[1.55, 4.61]	[1.98, 8.13]	[1.40, 4.70]
Q4	**1.12**	**0.85**	**1.23**	**0.92**
	[0.63, 1.99]	[0.26, 2.73]	[0.66, 2.28]	[0.29, 2.95]
Q5	**1.26**	**1.00**	**1.21**	**1.03**
	[0.85, 1.86]	[0.53, 1.91]	[0.73, 1.99]	[0.54, 1.98]
Watershed FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Incidence rate ratios (IRRs) shown with 95% confidence intervals in brackets. Both specifications include wildfire exposure in years t and $t-1$. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

B.2 Alternative Binning Specifications

B.2.1 Three-Bin Wildfire Exposure Specification

As an additional robustness check, wildfire exposure is grouped into three bins based on the empirical distribution of positive burned area observations. watershed–years with no wildfire exposure remain the omitted reference category. The remaining observations are divided into three equally sized groups representing relatively small, moderate, and large wildfire events.

Using fewer bins aggregates wildfire exposure into broader categories, which can improve statistical power but may obscure finer nonlinear patterns that appear when more bins are used. Comparing results across specifications with different numbers of bins therefore provides a useful robustness check on the relationship between wildfire size and drinking water quality violations.

Table B.6 reports the acreage ranges and number of observations within each bin, while Table B.7 presents the corresponding regression estimates.

Table B.6: Burned Area Cutoffs: Three-Bin Specification

Bin	N	% Sample	Min Acres	Max Acres
0	1521	68.89	0	0
Q1	229	10.37	0.03	98.23
Q2	229	10.37	98.78	1034.66
Q3	229	10.37	1051.68	235,993.76

Notes: Positive wildfire observations are divided into three equal-sized bins based on the empirical distribution of burned acreage.

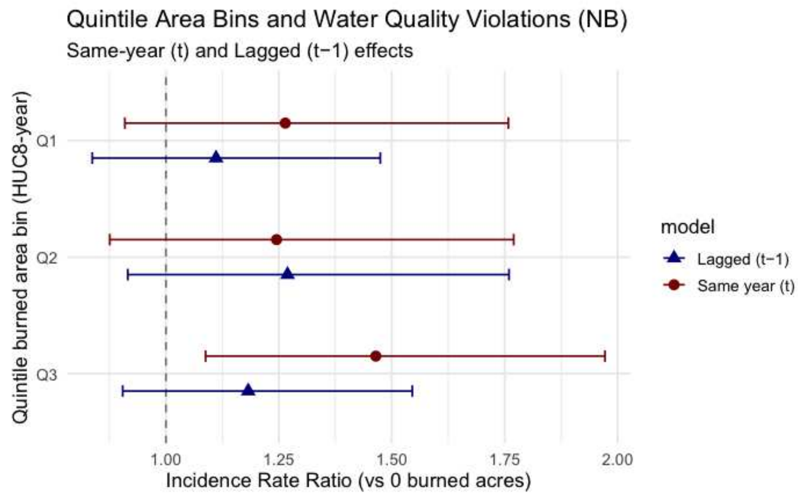
Table B.7 reports the negative binomial estimates using the three-bin wildfire exposure specification. Relative to watershed–years with no wildfire exposure, the largest wildfire bin (Q3) is associated with a statistically significant increase in drinking water quality violations in the year of the fire of about 46%, with an incidence rate ratio of 1.46 (95% CI: 1.09–1.97). The smaller wildfire bins (Q1 and Q2) also produce positive point estimates, though these effects are not statistically significant. Lagged wildfire exposure terms are positive across all bins but are not statistically distinguishable from zero. Overall, the three-bin specification shows that significant increases in water quality violations are associated with fires larger than roughly 1,000 acres, although the coarse binning obscures within-bin variation. While the largest bin is statistically significant in this specification, this result remains consistent with the baseline analysis, which finds elevated violations for fires between roughly 170 and 3,000 acres— a range that encompasses the 1,000+ acre threshold captured in the three-bin specification.

Table B.7: Wildfire Burned Area and Drinking Water Quality Violations (Three-Bin Specification, Negative Binomial)

	t	**t−1**
Q1 (0.03–98 acres)	**1.26**	**1.11**
	[0.91, 1.76]	[0.84, 1.47]
Q2 (98–1,035 acres)	**1.25**	**1.27**
	[0.88, 1.77]	[0.92, 1.76]
Q3 (1,035–235,994 acres)	**1.46***	**1.18**
	[1.09, 1.97]	[0.90, 1.55]
Watershed FE	Yes	Yes
Year FE	Yes	Yes

Notes: IRRs shown with 95% confidence intervals in brackets. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Reference category: no wildfire exposure.

Figure B.3: Negative Binomial Estimates: Three-Bin Wildfire Exposure Specification



Notes: Points show incidence rate ratios (IRRs) from the negative binomial model including wildfire exposure in years t and $t-1$. Horizontal bars denote 95% confidence intervals. The vertical line at IRR$=1$ indicates no effect. The reference category is no wildfire exposure.

B.2.2 Alternative Binning Specifications: Seven-Bin Analysis

As an additional robustness check, wildfire exposure is also grouped into seven bins based on the distribution of positive burned area observations. As in the baseline analysis, watershed–years with no wildfire exposure remain the omitted reference category. Dividing wildfire exposure into a larger number of bins allows for a more flexible characterization of the relationship between wildfire size and drinking water quality violations, particularly if effects vary within relatively narrow ranges of fire size.

Table B.8 reports the acreage ranges and number of observations within each bin, while Table B.9 presents the corresponding regression estimates. Comparing results across the three-, five-, and seven-bin specifications provides a robustness check on whether the estimated relationship between wildfire size and water quality violations depends on the choice of bin structure.

Table B.9 reports the negative binomial estimates using the seven-bin wildfire exposure specification. The results again indicate that increases in drinking water quality violations are concentrated among small to mid-sized wildfire events. In particular, fires between roughly 200 and 4,500 acres (bins Q4–Q6) are associated with statistically significant increases in violations in the

Table B.8: Burned Area Cutoffs: Seven-Bin Specification

Bin	**N**	**% Sample**	**Min Acres**	**Max Acres**
0	1521	68.89	0	0
Q1	99	4.48	0.03	19.04
Q2	98	4.44	19.11	71.87
Q3	98	4.44	72.21	203.11
Q4	98	4.44	209.26	544.85
Q5	97	4.39	545.02	1,390.41
Q6	99	4.48	1,408.41	4,572.25
Q7	98	4.44	4,668.11	235,993.76

Notes: Positive wildfire observations are divided into seven equal-sized bins based on the empirical distribution of burned acreage.

Table B.9: Wildfire Burned Area and Drinking Water Quality Violations (Seven-Bin Specification, Negative Binomial)

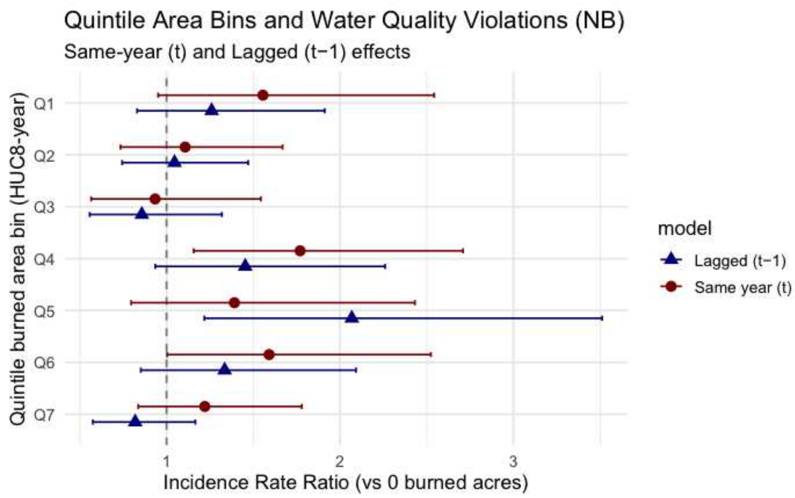
	t	**t−1**
Q1 (0.03–19 acres)	**1.56**	**1.26**
	[0.95, 2.54]	[0.83, 1.91]
Q2 (19–72 acres)	**1.11**	**1.05**
	[0.73, 1.67]	[0.74, 1.47]
Q3 (72–203 acres)	**0.93**	**0.86**
	[0.57, 1.54]	[0.56, 1.32]
Q4 (203–545 acres)	**1.77****	**1.45**
	[1.16, 2.71]	[0.93, 2.26]
Q5 (545–1,390 acres)	**1.39**	**2.07****
	[0.80, 2.43]	[1.22, 3.51]
Q6 (1,390–4,572 acres)	**1.59***	**1.33**
	[1.00, 2.52]	[0.85, 2.09]
Q7 (4,572+ acres)	**1.22**	**0.82**
	[0.84, 1.78]	[0.58, 1.17]
Watershed FE	Yes	Yes
Year FE	Yes	Yes

Notes: IRRs shown with 95% confidence intervals in brackets. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Reference category: no wildfire exposure.

contemporaneous year or the following year. These results closely mirror the baseline findings, which identify elevated violations for fires between approximately 170 and 3,000 acres.

While the finer bin structure introduces additional variation, the overall pattern remains consistent: wildfire exposure in the mid-sized range is associated with the largest and most significant increases in drinking water quality violations, whereas the smallest and largest fire bins do not exhibit consistent effects.

Figure B.4: Negative Binomial Estimates: Seven-Bin Wildfire Exposure Specification



Notes: Points show incidence rate ratios (IRRs) from the negative binomial model including wildfire exposure in years t and $t-1$. Horizontal bars denote 95% confidence intervals. The vertical line at IRR$=1$ indicates no effect. The reference category is no wildfire exposure.

B.3 Dropping Watersheds without Monitors

This appendix presents results from a restricted sample that excludes watersheds without any drinking water monitoring stations. By focusing only on watersheds where violations are observable, this specification provides an additional check on whether the main results are influenced by the presence of structural zeros. The findings, found in Table B.10 and Figure B.5, are consistent with those reported in the baseline specification, seen in in Table B.11.

Table B.10: Wildfire Exposure and Water Quality Violations: Restricted Sample (Monitoring Only)

	t	**t−1**
Q1 (<38 acres)	1.48	1.31
	[0.95, 2.31]	[0.87, 1.97]
Q2 (38–179 acres)	0.95	0.82
	[0.67, 1.36]	[0.59, 1.14]
Q3 (179–723 acres)	1.63*	1.45*
	[1.02, 2.61]	[1.04, 2.03]
Q4 (723–2,805 acres)	1.66*	1.66*
	[1.09, 2.53]	[1.13, 2.43]
Q5 (2,805–235,994 acres)	1.35	0.95
	[0.94, 1.93]	[0.69, 1.31]

Notes: IRRs shown with 95% confidence intervals in brackets. Results are from a negative binomial model estimated on a restricted sample excluding watersheds without monitoring stations. Reference category: no wildfire exposure. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure B.5: Wildfire Exposure and Water Quality Violations: Restricted Sample

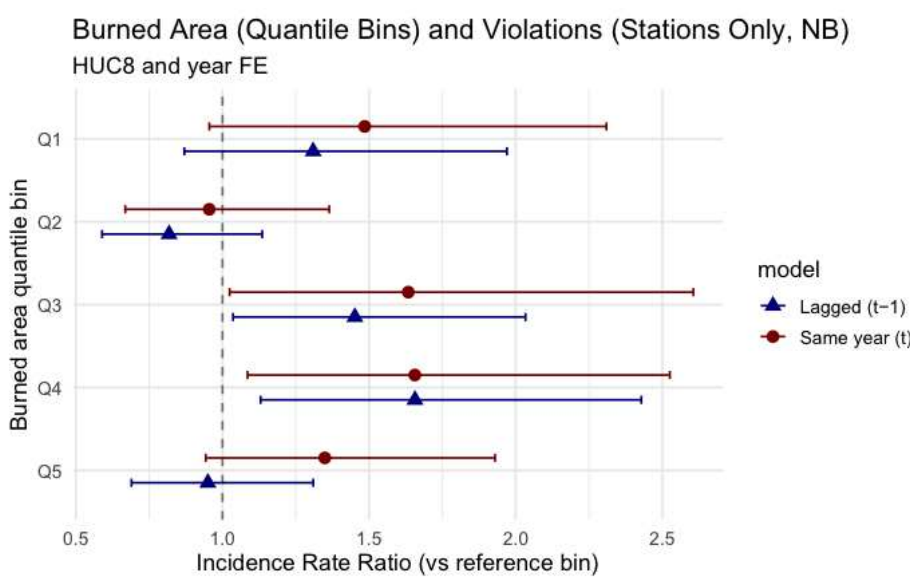


Table B.11: Wildfire Exposure and Water Quality Violations: Baseline vs. Restricted Sample

	Baseline (All Watersheds)		**Restricted (Monitoring Only)**	
Burn Quintile	**t**	**t−1**	**t**	**t−1**
Q1	**1.50**	**1.28**	**1.48**	**1.31**
	[0.93, 2.42]	[0.85, 1.93]	[0.95, 2.31]	[0.87, 1.97]
Q2	**0.95**	**0.88**	**0.95**	**0.82**
	[0.69, 1.31]	[0.61, 1.26]	[0.67, 1.36]	[0.59, 1.14]
Q3	**1.66***	**1.46***	**1.63***	**1.45***
	[1.03, 2.65]	[1.03, 2.08]	[1.02, 2.61]	[1.04, 2.03]
Q4	**1.64***	**1.56***	**1.66***	**1.66***
	[1.06, 2.54]	[1.06, 2.30]	[1.09, 2.53]	[1.13, 2.43]
Q5	**1.36**	**1.00**	**1.35**	**0.95**
	[0.95, 1.93]	[0.72, 1.39]	[0.94, 1.93]	[0.69, 1.31]
Watershed FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: IRRs shown with 95% confidence intervals in brackets. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Reference category: no wildfire exposure.