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Essays in Public Economics

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Doctor of Philosophy
in
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by

Christopher Malloy

Committee in charge:

Professor Olivier Deschênes, Chair
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June 2023

The Dissertation of Christopher Malloy is approved.

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May 2023

Essays in Public Economics

To my parents and grandparents.

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Publications

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Working Papers

The Precautionary Consequences of Wildfire Liability: Evidence from Power Shutoffs in California

Causal Effects of Renewable Portfolio Standards on Renewable Investments and Generation: The Role of Heterogeneity and Dynamics, with Olivier Deschenes and Gavin McDonald (*Under Review*)

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Abstract

Essays in Public Economics

by

Christopher Malloy

This dissertation consists of three works which use econometric techniques to estimate how shifting liability regimes and green energy transitions impact firm behavior, making contributions to public economics.

In the first chapter, I develop a novel empirical strategy that causally estimates the relationship between firm precautions and the level of liability each firm faces. Across all sectors of the U.S. economy, regulators use liability regulations to encourage firms to take actions that reduce the costs associated with low probability, high severity events such as powerline-ignited wildfires and production defects. Despite the widespread use of these regulations, there is limited evidence of their effectiveness across many sectors of the economy. This study identifies a new channel through which liability regulation influences firm behavior and provides causal evidence of firm responses to the entire distribution of potential liability by studying a regulation in California's electric utility sector. Using exogenous variation in the replacement cost of structures that lie downwind of powerlines, I find that firms increase their precaution by 130% in response to a $680 million increase in liability. In the short run, the estimates from this study imply that the implemented liability regulation had welfare costs between $17 million and $7 billion.

The second chapter, joint with Olivier Deschênes, uses recently developed econometric techniques to estimate how Renewable Portfolio Standards incentivize investments in solar and wind generation across the U.S.. Despite a 30-year long history, Renewable Portfolio Standards (RPS) remain controversial and debates continue to surround their

efficacy in leading the low-carbon transition in the electricity sector. Contributing to the ongoing debates is the lack of definitive causal evidence on their impact on investments in renewable capacity and generation. This paper provides the most detailed analysis to date of the impact of RPSs on renewable electricity capacity investments and on generation. We use state-level data from 1990-2019 and recent econometric methods designed to address dynamic and heterogeneous treatment effects in a staggered adoption panel data design. We find that, on average, RPS policies increase wind generation capacity by 600-700 MW, a 21% increase, but have no significant effect on investments in solar capacity. Additionally, we demonstrate that RPSs have slow dynamic effects: most of the capacity additions occur 5 years after RPS implementation. Estimates for wind and solar electricity generation mimic those for capacity investments. We also find similar results using a modified empirical model that allows states to meet their RPS requirements with pre-existing renewable generation and renewable generation from nearby states.

In the third chapter, also joint with Olivier Deschênes, we quantify how investments in wind generation reshape regional economies across the U.S. and which workers are impacted the most. Most western countries have made commitments or enacted policies aiming to transform their economies to become carbon-neutral by 2050. Many of the leading policies to reduce carbon emissions are also promoted as engines of job creation and local economic development. While low-carbon transition policies continue to be debated and proposed, few have been implemented, and none have operated for a long enough period of time to permit an empirical evaluation of their impact. This paper uses the natural experiment provided by the rapid deployment of wind electricity projects in the United States over the period 2000-2019 to shed light on whether the low-carbon transition can deliver long-lasting and high-quality jobs. We compile detailed data on the location and operation date of 55,000 wind turbines, combined with county-level data on employment, earnings, GDP, and per capita income to estimate the impact of wind

projects on regional economies. Our research design uses two-way fixed effects regression and empirical strategies robust to concerns about heterogeneous treatment effects. The empirical analysis points to a small, but durable positive effect of wind electricity investments on regional economies. Overall, the results suggest that the projected additional 150 GW of wind electricity production capacity from the Inflation Reduction Act will create close to 164,000 jobs.

Contents

0.1 Permissions and Attributions

1. The content of chapter 2 and appendix B is the result of a collaboration with Olivier Deschênes and Gavin McDonald.

2. The content of chapter 3 and appendix C is the result of a collaboration with Olivier Deschênes.

Chapter 1

The Precautionary Consequences of Wildfire Liability: Evidence from Power Shutoffs in California

1.1 Introduction

Low probability, high severity events such as oil spills or product defects characterize many sectors of the U.S. economy. A popular approach to mitigate the frequency of such events is to make firms liable for potential damages in part to incentivize precaution. To understand the effectiveness of liability regulation we need to know how firms' precautions respond to: (1) the application of liability and (2) changes in the amount of damages they are liable for.

In settings where a firm faces large potential liabilities from an accident, its liability cannot exceed its asset value because it may use bankruptcy to avoid further damages. This discrete drop in firms' incentives for precautions at their asset value is commonly termed the judgment-proof problem (Shavell (1986)). One common solution used to solve the judgment proof problem is to cap firms' level of potential liability. However, determining the liability cap level is a difficult task for a regulator: higher caps induce firms to undertake greater precautions as they bear a larger share of liability costs, but setting too high a cap may cause the firm to declare bankruptcy, shifting liability costs onto the public. This creates ambiguity about a fundamental question in public economics: What are the efficiency tradeoffs associated with capping liability?

Motivated by this gap in the literature on liability regulation, this paper provides the first causal evidence of how firms' precautions responds to the imposition of a negligence standard in California's electric utility sector. Between 1999 and 2017, firms faced with covering liabilities due to power line fires were allowed to recoup these costs through increases in retail electricity prices. However, since November 2017, utilities have borne liability costs whenever the regulator found that their imprudence led to an ignition. Using this setting, I estimate an empirical model that shows how firms' use of one type of precaution, called a Public Safety Power Shutoff event (PSPS), changed following the policy shift. Furthermore, I develop an empirical model which uses daily variation in the replacement cost of structures that are downwind of power lines to estimate how firms' use of power shutoffs respond to the entire distribution of potential liability. Since firms in this setting are responsible for the replacement cost of structures damaged by power line-ignited fires, variation in downwind regions across days creates exogenous changes in potential liability.

Firms use Public Safety Power Shutoff events to prevent fire ignitions along their power lines. During a power shutoff, utilities turn the power off on sections of their energy infrastructure when forecasted climate conditions suggest an ignition is likely to occur. Because electricity must be running through a power line for an ignition to happen, power shutoffs significantly reduce the likelihood of fire and potential liabilities that a firm faces. In contrast, other types of precaution available to firms such as clearing vegetation away from power lines do not provide the same assurance because an ignition could still occur.

This is an important setting to study liability regulation. Climate change is increasing the severity of power line-ignited fires in the western U.S., making it important to understand how to incentivize firms to prevent ignitions in this setting (Syphard and Keeley (2015)). Furthermore, power line-ignited fires are more damaging than fires from other ignition sources because they typically occur during high wind speed events when the wind carries vegetation into the line. Since fires are also more likely to spread rapidly and grow out of control during windy conditions, power line-ignited fires tend to cause more damage than fires from other sources (Keeley et al. (2018)). For example, one privately owned utility, Pacific Gas and Electric, faced over $30 billion dollars in liability from several fires ignited in 2017 and 2018.[1] Figure A.1 plots total damages in billions of 2021 dollars by source of fire ignition and shows that, although power line-ignited fires make up less than one percent of ignitions historically, they account for most of the damage from fires in California between 2008 and 2019.

My setting also has a key advantage: it allows me to causally estimate the relationship between the level of liability a firm faces and its precaution using exogenous changes in the direction that the wind is blowing across days.[2] Prior work has typically relied on regulatory changes that cap the level of liability a firm faces to study this relationship, but in this setting I am able to measure firms' responses across the full distribution of potential liabilities that they face.

Using administrative data on precautionary measures taken by the three largest privately owned utilities in California, I find three results. First, I show that firms dra-

[1]Los Angleles Times "Pacific Gas and Electric to file for bankruptcy as wildfire costs hit $30 billion. Its stock plunges 52%", January 14, 2019.

[2]The privately owned utilities in California's electric utility industry that I study are representative of most electric utilities in the United States. In fact, in 2017 privately owned utilities supplied 72% of electricity customers in the United States (EIA Annual Electric Power Industry Report).

matically increase their use of power shutoffs following the 2017 policy change. Prior to the reform, power shutoffs occurred on 0.1 percent of days when the ignition risk was elevated and, on average, created 2 lost customer hours of power. After the reform, power shutoffs happened on 4 percent of days with heightened ignition risk and the number of customer hours without power increased by 734 customer hours, on average. Using lower and upper bounds on consumers' value of electricity use from the literature, I find that this increase in power shutoffs translates to between \$150 and \$51,000 of lost consumer surplus at the average distribution circuit.[3] I also show that although firms increase shutoffs most in the regions of greatest *ex ante* ignition risk, these are more likely to be areas with high shares of customers that rely on electricity for their medical needs, making the shutoffs particularly costly.[4]

Second, I show that firms' precaution is positively related to the level of liability that they face. Since utilities are liable for the cost of replacing structures damaged by fires that their power lines ignite, I measure liability using this value. In most settings, causal estimation of the relationship between the level of liability that firms face and precaution is difficult because liability is likely to be endogenous. My setting allows me to remove this endogeneity by using daily variation in the replacement cost of structures that lie downwind of each firm's power lines between 2018 and 2020 to generate daily variation in each utility's potential liability. I estimate that power shutoffs increase by 130 percent relative to the average likelihood of a shutoff when the total replacement cost of structures in downwind areas increases by 10 percent (\$680 million).

[3] The lower bound of consumers' value of electricity is the average retail price of electricity in California as of August 2022 (\$0.22 per kWh) and the upper bound is \$76.11, the largest residential value of electricity use from Collins et al. (2019). The upper bound of consumers' electricity use may be much higher however, because the shutoffs left commercial and industrial consumers, who have higher use values of electricity, without power as well.

[4] Customers relying on electricity for their medical needs may require reliable energy to power respirators, electric wheelchairs, and other devices. Because these customers have an above average use value of electricity, this result implies that using an average value of consumers' value of lost load would systematically underestimate the welfare consequences of power shutoffs.

Third, I estimate that the short-run welfare impact of the 2017 liability rule change is negative and large. Depending on the chosen estimate of consumer's value of electricity from the literature, the policy resulted in a welfare loss of between \$7 billion (\$76.11 per kilowatt hour valuation of electricity use) and \$17 million (\$0.22 per kilowatt hour). From the social planner's perspective, this suggests that utilities have overused shutoffs as a precautionary measure in the short term. I also develop a conceptual framework that suggests this increase in shutoffs reduced utilities' use of other types of precautionary measures such as vegetation management or infrastructure upgrading.

These results have several policy relevant implications. I provide an empirical framework to estimate how firms' precautionary behaviors change across the distribution of potential liabilities, a key parameter for determining the liability cap level. Current and past policy proposals have included limits on the amount of damages homeowners can recover from electric utilities.[5] However, such policy proposals note that it is unclear what level liability should be limited at and how such limits would distribute costs between homeowners, electricity consumers, and utility shareholders.

Furthermore, I estimate how economic incentives influence the reliability of electricity supply using a novel dataset of distribution power lines. This is relevant for regulators across the U.S. who want to incentivize utilities to make investments that improve the reliability of electricity supply and upgrade aging infrastructure. Because of the projected growth of renewable energy generation in the United States, the federal government has made upgrades of energy infrastructure a cornerstone of its energy platform.[6] My work in this paper underscores that having detailed administrative data on distribution networks across the U.S. will be important for effectively upgrading energy infrastructure.

This paper makes three contributions to the literature in public economics. First,

[5] "Allocating Utility Wildfire Costs: Options and Issues for Consideration", California Legislative Analysts Office, 2019.

[6] See here.

it poses a channel, expected damages, through which liability regulations impact firms' decisions and quantifies how the burden of precautionary costs is distributed between firms and electricity consumers. I show that, when firms bear liability costs, they direct more precautionary effort to areas with high levels of expected liability. Since power shutoffs are socially costly, utilities' increased reliance on shutoffs to prevent ignitions causes electricity consumers to bear a greater share of costs associated with ignition prevention. This adds to previous work documenting other determinants of firms' choice of precaution such as bankruptcy (Shavell (1986)), subjective firm beliefs (Currie and MacLeod (2013)), risk aversion (Shavell (1982)), and market structure (Chen and Hua (2017)). Furthermore, this result contributes to a growing literature that examines the determinants of wildfire suppression (Plantinga, Walsh and Wibbenmeyer (2022), Baylis and Boomhower (2022)).

Second, I show how precaution varies across the distribution of potential liabilities that firms face. Previous research has estimated how capping medical liability impacts doctors' prescribing behavior (Helland et al. (2021)), medical outcomes (Danzon (1985), Kessler and McClellan (1996), Currie and MacLeod (2008), Frakes (2013)), and the labor supply of doctors (Malani and Reif (2015), Kessler, Sage and Becker (2005), Klick and Stratmann (2007), Matsa (2007)). Another related literature examines how changes in liability impact toxic waste discharges and abatement technology adoption (Akey and Appel (2021), Alberini and Austin (2002), Stafford (2002)). Many of these studies estimate how precaution responds to the level of liability a firm faces at one point in the liability distribution because their variation comes from caps on liability at a particular value. The empirical strategy in this paper allows me to estimate how precaution changes across the entire distribution of liability that firms face in practice.

Previous work on the judgment-proof problem by Boomhower (2019) shows that requiring firms to purchase insurance which covers damages beyond their own assets encouraged greater production by larger firms with better environmental outcomes in Texas' oil and gas sector. This paper complements Boomhower (2019) by directly estimating how firms' precautions change across the distribution of potential liability they face. Since requirements to cover damages beyond firm assets may not be feasible in settings with concentrated market power, such as the electric utility sector, the estimates in this paper provide relevant information that can be used to implement other solutions to the judgment-proof problem such as capping liability.

This paper also makes important contributions to a recent literature in environmental economics and engineering. I show that liability considerations drive firms' decision to declare power shutoffs. Previous work by Abatzoglou et al. (2020) applied one utility's publicly stated climate thresholds for declaring power shutoffs to observed weather data during 2019, finding that the utility used shutoffs more than would be predicted by its own decision rules. I provide an economic explanation for this overuse of power shutoffs by documenting the role of liability in determining firms' precaution.

I also provide evidence that utilities' use of power shutoffs are costly. This adds to a recent literature that estimates the costs and benefits of public safety power shutoffs in California (Sotolongo, Bolon and Baker (2020), Wong-Parodi (2020), Zanocco et al. (2021), Mildenberger et al. (2022)). I provide the first causal estimates of power shutoffs' impact on customers that rely on electricity for their medical needs, finding results consistent with the descriptive analysis performed by Sotolongo, Bolon and Baker (2020).

Finally, I provide the first evidence of fire liability's impact on firms in the electric utility industry. Yoder (2008) shows that the number of fires escaping from private landowners' property during a prescribed burn declines following the implementation of strict liability regulations. I add to this evidence by causally showing that electric utilities increase precautionary actions to prevent fire ignitions along their power lines in response to greater liability for fire damages.

The rest of this paper proceeds as follows: section 2.2 provides background on liability regulation for power line-ignited fires in California and utilities' ignition prevention decision environment. Section 1.3 presents a simple theoretical model with testable predictions of liability regulation's effect on utility's precautionary effort. In section 1.4, I develop an empirical framework to study how the application of liability impacts precaution, describe the data used in this analysis, and present results. Section 1.5 develops an empirical strategy to causally estimate the relationship between liability and shutoffs, describes the data sources used in this analysis, and presents results. Section 2.6 discusses the results and outlines opportunities for future research.

1.2 Background

1.2.1 Institutional Background

This paper focuses on electricity distribution to residential and commercial consumers, the final link in the U.S. electricity supply chain which consists of generation, transmission, and distribution. Electric distribution utilities are generally considered natural monopolies and most are regulated by Public Utility Commissions (PUCs). The California Public Utility Commission (CPUC) mandate states that its goal is to provide "...access to safe, clean, and affordable utility services and infrastructure."

PUCs' primary regulatory tool to influence utilities' actions is called a rate case. CPUC defines rate cases as quasi-judicial "proceedings used to address the costs of operating and maintaining the utility system and the allocation of those costs among customer classes." At each rate case proceeding, the PUC determines the fixed electricity price which a utility can charge customers until its next rate case proceeding. The three largest Investor Owned Utilities (IOUs) in California each have their own separate rate cases every three years. In this way, utilities in the U.S. face price cap regulation with periodic adjustment of the cap. The PUC adjusts the price cap so that each utility earns a fair rate of return on its capital and recovers its operating expenses. However, the PUC may disallow a capital investment if it does not meet a standard of being "used and useful."

Importantly, in California utilities could request to recover uninsured costs associated with fires ignited by their distribution infrastructure during rate cases between 1999 and 2017. Thus, while utilities paid for residential damages and suppression costs associated with fires ignited by their equipment, the expectation was that these costs could be recovered through an increase in the electricity price cap. After a 2017 ruling in a rate case proceeding that rejected San Diego Gas and Electric's application to recover fire-related costs through electricity rates, utilities faced a greater likelihood that they would be financially accountable for such costs, increasing their liability. The next section discusses the history of fire liability for utilities in California.

1.2.2 Liability Regulation in California

Liability regulations impact the incentives for individuals and firms to take risk and exert precaution. In the case of fire ignited by utility-operated infrastructure, utilities may adjust their level of precaution according to the proportion of fire-related damages they would be held accountable for if an ignition occurs. Similarly, individual homeowners may increase effort to reduce the probability of wildfire-related damage to their property when a firms' share of liability from a power line ignited wildfire is low. Regulators choose the degree of liability that a firm faces by choosing from two types of regulations: strict liability and a negligence rule. Under strict liability, the firm is fully liable for the resulting damages of a fire ignited by their equipment. In contrast, the negligence rule sets a minimum threshold of precaution that firms must meet in order to avoid financial responsibility for damages. In the simplest model, the firm will take the highest level of precaution under strict liability and reduce its level of ignition prevention to just meet the threshold when subject to the negligence rule (Kaplow and Shavell (1999)).

Since the California Supreme Court held Southern California Edison liable for damages resulting from a fire ignited by its equipment in the case *Barham v. Southern California Edison Company (1999)*, IOUs have been held to a strict liability standard for fire damages. A key factor in the Court's decision was the fact that, just as a government can raise revenue through taxes, IOUs can raise revenue through retail electricity rates in California.[7] The Court reasoned that since the state government is strictly liable for damages it causes under the Takings clause of the California constitution, IOUs could be held strictly liable for damages related to power line-ignited fires. As a result, IOUs faced strict liability for fire damages in excess of their insurance coverage, but could recover these costs through increases in the retail price of electricity. IOUs continued to challenge the Court's ruling in *Barham* as recently as 2012, arguing that they could not have the same liability status as a government because their ability to raise rates is subject to the approval of the CPUC.[8] The Court continued to maintain, however, that because there was no evidence CPUC would not allow IOUs to recover costs through electricity rate increases, strict liability would continue to apply.

Although IOUs faced strict liability, the precedent established by *Barham* ensured that their liability net of revenue increases from raised electricity rates would be low. The precedent that IOUs could recover liability costs through increased electricity rates was not tested until several damaging fires ignited by power lines operated by San Diego

[7] The Court's decision argues that IOUs' ability to raise electricity rates is akin to a government's ability to levy taxes. IOUs are currently challenging this logic in court by pointing out that their ability to raise electricity rates is subject to approval by the CPUC.

[8] Pacific Bell Telephone Co. v. Southern California Edison Co., 208 Cal. App. 4th 1400, 1403 (2012).

Gas and Electric in 2007. The 2007 fires were the first time since the *Barham* decision that the liability costs associated with power line-ignited fires exceeded an IOU's liability insurance coverage (Hafez (2020)). As a result, San Diego Gas and Electric's application to recover uninsured liability costs through electricity rate increases was a novel test of the strict liability standard. Ultimately, CPUC rejected San Diego Gas and Electric's application to recover liability costs through electricity rates in December 2017, citing San Diego Gas and Electric's lack of precaution in preventing the 2007 fires as the deciding factor.[9] Because IOUs could no longer expect to automatically recover costs through electricity rate increases following the 2017 CPUC decision, their liability for fire damages increased dramatically. CPUC's decision states that "If the preponderance of the evidence shows that the utility acted prudently, the Commission will allow the utility to recover costs from the ratepayers." While CPUC declined to define a precise negligence threshold in its decision, the decision dramatically increased the share of liability that each IOU is responsible for.

The "prudent manager" standard remained in effect until SB 901 added section 451.1 to the Public Utilities Commission Code which took effect for all fires ignited after January 1, 2019. Section 451.1 replaced the "prudent manager" standard with twelve non-exclusive criteria that CPUC uses to determine whether an IOU can recover costs associated with fire liabilities through electricity rates. The criteria take into account the IOU's design, maintenance, and operation of assets in addition to the severity and unpredictability of the weather event which caused the ignition. While, section 451.1 clarified the standard used to judge each utility's negligence it still significantly increased the share of costs associated with fire damages utilities expected to bear relative to the pre-2017 regulatory environment. If the reader is interested in learning more about the

[9] Application of San Diego Gas and Electric Company (U 902 E) for Authorization to Recover Costs Related to the 2007 Southern California Wildfires Recorded in the wildfire Expense Memorandum Account, filed Sept. 25, 2015. Decided Dec. 26, 2017.

history of liability law and IOUs in California, Hafez (2020) provides a complete and succinct description. The next section describes utilities' allocation of ignition prevention effort and demonstrates how increasing the share of damages born by IOUs changes this allocation.

1.2.3 The ignition prevention decision environment

This section draws largely from Wildfire Mitigation Plans submitted by Southern California Edison, Pacific Gas and Electric, and San Diego Gas and Electric to CPUC in 2019, 2020, and 2021. IOUs face a complex decision making environment as they determine how and where to invest in strategies that lower the risk of fire ignited by their electrical infrastructure. Despite accounting for 1-5% of total fire ignitions in Southern California, utility-operated equipment accounts for 20-30% of total area burned by wildfires (Syphard and Keeley (2015)). Ignitions by power lines typically occur between July and December and their two leading causes are wind-blown vegetation and equipment failure. Much of the transmission and distribution infrastructure operated by IOUs in California is quite old (in 2017 Pacific Gas and Electric estimated that the average age of its transmission towers was 68 years old). As climate change has increased vegetative aridity and the severity of weather events in IOU service territories, the risk of fire ignition has also risen. In determining which areas to prioritize for ignition mitigation activities, utilities weigh the benefits of providing electricity to their residential, industrial, and commercial customers with the cost of each activity and the ignition risk associated with each section of their distribution and transmission infrastructure.

To determine the ignition risk of a section of power line, utilities consider historical and forecasted weather conditions, infrastructure age, vegetative growth, presence of outdated equipment with known ignition risk, and the value of electricity demanded by customers on that section. After determining the baseline risk of a power line segment, a team at each utility then chooses an ignition mitigation activity which reduces the risk at least cost. Utilities perform a range of ignition prevention activities including vegetation management, installation of weather stations along power lines, burying power lines underground, upgrading equipment, inspecting power lines, and turning off the power to targeted sections of the grid when weather conditions elevate the probability of ignition. Use of ignition prevention activities differs across utilities and over time as conditions change and utilities learn more about the effectiveness of each action. For example, Pacific Gas and Electric primarily deployed shutoff events and infrastructure upgrades in 2019 to reduce the probability of ignition, while Southern California Edison focused on installing covered conductors that reduce the probability of ignition on high risk assets. Recently, each IOU has increased efforts to bury sections of high-risk assets underground.

Historically, utilities in California have not relied on power shutoffs to reduce the likelihood of ignition because they disrupt the service of electricity to customers, proving costly. The California Public Utilities Commission (CPUC) defines Public Safety Power Shutoff events as actions taken by utilities to temporarily turn off power to specific areas in order to reduce the risk of fires caused by electric infrastructure. Of the three largest IOUs in California, only San Diego Gas and Electric utilized shutoffs to prevent ignitions prior to 2017.[10] Because shutoff events require the utility to interrupt service to customers it is seen as a measure of last resort to mitigate fire ignitions. As a result, each IOU has invested in devices which further segment high-risk areas of their transmission and distribution networks, allowing more targeted blackouts that affect fewer customers.

CPUC approves the use of power shutoff events by IOUs, first granting approval to San Diego Gas and Electric in 2012, Pacific Gas and Electric in 2018, and Southern

[10]San Diego Gas and Electric sought and received approval from CPUC to initiate power shutoffs in its service territory starting in 2013.

California Edison in 2018. Figure A.2 plots the total number of customer hours impacted by shutoff events over time separately for each of the 3 largest California IOUs. The most affected customer hours occurred during 2019 in Pacific Gas and Electric's service territory. A similar pattern exists for the number of commercial customer hours and medically vulnerable customer hours affected by power shutoffs.

IOUs consider climatic conditions, the condition of electrical infrastructure, and the value of lost electricity load in potentially impacted areas to determine when and where to declare power shutoffs. Pacific Gas and Electric reports the criteria it uses to declare shutoff events on page 982 of their 2021 Wildfire Mitigation Plan. The minimum criteria for deciding a shutoff in a high fire threat area are sustained wind speeds greater than 20 MPH, dead fuel moisture below 9%, relative humidity below 30%, and a fire potential index (FPI) greater than 0.2.[11] Despite these criteria, utilities have discretion in declaring power shutoffs–Abatzoglou et al. (2020) provide evidence that shutoff events are used more frequently by Pacific Gas and Electric than would be implied by their minimum climate criteria.[12]

According to the standard economic model of liability regulation, the increase in the share of liability born by IOUs following CPUC's 2017 decision should increase the level of ignition prevention effort (Kaplow and Shavell (1999)). Furthermore, increasing the liability born by IOUs should also increase their use of more costly prevention activities such as shutoff events. Finally, the increase in IOU liability should cause IOUs to direct ignition prevention efforts to regions of their service area with a high property values. Since destroyed property values make up a significant portion of liability damages born by IOUs when their equipment ignites a fire, IOUs have an incentive to direct ignition

[11]The FPI measures the likelihood of an ignition causing a catastrophic wildfire using wind speeds, temperature, humidity, dead and live fuel moisture, and vegetative cover types.

[12]Abatzoglou et al. (2020) note that this could be due to differences in climate modelling between their study and Pacific Gas and Electric's internal methods.

prevention activities to these regions.

The next section develops a simple model of liability in the context of the electric utility industry, presents several testable hypotheses, and derives a sufficient condition for estimating the welfare change from an increase in the share of liability born by firms.

1.3 Conceptual Framework

The conceptual framework demonstrates three points: (1) Decreasing the return on defensive capital investment increases utilities' use of shutoffs, leading to less defensive investment that mitigates ignitions along power lines. (2) Increasing the level of potential liabilities leads firms to use more power shutoffs. (3) Utilities use more shutoffs when ignitions are likely.

The model in this paper is adapted from Lim and Yurukoglu (2018) who show that a regulator's inability to commit to a predictable path of capital returns leads utilities to systematically underinvest in capital. Here, I consider a simplified version of the model with no strategic interaction between the regulator and the utility. In this model, the utility takes the regulator's choice of capital return as given rather than as an output from a negotiation process.

For simplicity, I model a single utility's decision to make defensive capital investments and supply electricity to one distribution circuit. If the utility supplies electricity, it receives future net revenue and faces expected liability damages from a potential ignition along its power lines. However, if the utility declares a power shutoff it receives no revenue and faces no expected damages. The utility self protects against expected damages by making defensive capital investments that reduce the probability of ignition.[13] In making its decisions, the firm compares the marginal reduction in damages from self protection

[13] I define self protection in the same way as Ehrlich and Becker (1972), where defensive investments reduce the probability of ignition rather than total damages.

to total expected damages. Whenever expected damages exceed the marginal benefit of self protection, the firm shuts off the power.

I make several important assumptions in this model. First, because the model only considers one distribution circuit, the firm will never make additional defensive capital investments if it shuts off the power. In practice, defensive investments may complement power shutoffs because utilities could self protect against damages on days when the ignition risk is low. Second, in a departure from reality, I do not allow for strategic interaction between the firm and the regulator. The results from Lim and Yurukoglu (2018) suggest that allowing for such interaction would cause firms to increase shutoff use more and invest in defensive capital less. Third, I do not model the firm's non-defensive capital investment decisions. Finally, the model assumes that consumers value their homes at the structure replacement cost. This simplifying assumption does not affect the framework's predictions, but it would increase the benefit of shutoffs for households in the welfare effect of the liability regulation.

1.3.1 Firm's Problem

The regulator sets a per unit output price p that allows the utility to recoup a reasonable return on defensive capital (γk) and per-unit liability costs (ν).

$$p = \frac{\gamma k + \nu}{Q} \tag{1.1}$$

Where k represents the stock of firm defensive investment which it uses to self insure against damages from a potential fire ignition and ν is the exogenous rate of return on defensive investment that is set by the regulator. The firm inelastically supplies Q units of electricity to consumers who purchase a quantity Q of electricity up to a "choke" price ($\bar{p}$) above which they are no longer willing to pay.

$$D(p) = Q\text{if } p \leq \bar{p}0\text{otherwise} \tag{1.2}$$

The utility earns revenue by supplying electricity to retail consumers and reduces expected liability costs by renting defensive capital that reduces damages from a potential ignition from households at the prevailing interest rate (r). The utility can also prevent ignitions by supplying no electricity to consumers.

$$\max\{\pi_1, \pi_0\}$$

Where

$$\pi_1(k) = \max_{k'}\{-r(k' - (1-\delta)k) + \beta(\phi p Q)\}$$

$$\pi_0(k) = \max_{k'}\{-r(k' - (1-\delta)k) + \beta(pQ - \theta(k')\bar{d})\}$$

Where the utility earns π_1 in profits if it shuts off the power and π_0 in profits if it supplies power, δ is the capital depreciation rate, and $\bar{d}$ is the dollar amount of expected liability damages if an ignition occurs. In the empirical analysis later in this paper, $\bar{d}$ is the total replacement cost of structures threatened by a power line ignition. The utility can self protect against liability costs by investing in defensive capital (k') which reduces the probability of ignition ($\theta(k')$). The utility chooses whether to declare a blackout and investment in capital subject to an uncertain probability of ignition ($\theta(k')$). When the utility shuts off the power it recoups a fraction $\phi \in (0, 1)$ of its revenues by exerting market power in wholesale electricity markets. β is the per-period discount factor. Substituting the price of electricity from equation 1.1, allows us to rewrite the utility's profit functions.

$$\pi_1 = \max_{k'}\{-r(k' - (1-\delta)k) + \phi\beta(\gamma k + \nu)\}$$
$$\pi_0 = \max_{k'}\{-r(k' - (1-\delta)k) + \beta(\gamma k + \nu - \theta(k')\bar{d})\}$$

Intuitively, when the firm supplies electricity (π_0) it pays defensive capital rental costs today and receives future net revenues (pQ) while facing expected liability costs from a potential ignition ($\theta(k')\bar{d}$). When the firm chooses to shutoff the power (π_1), it pays capital rental costs today and receives only a fraction of its revenue in the future, but since an ignition cannot occur it also faces no expected damages.

Figure A.3 presents a simplified version of the firm's shutdown decision to demonstrate its incentive to use a power shutoff. Both graphs show example demand (red) and supply (blue) curves for electricity when the utility supplies electricity (left) and shuts off the power (right). For simplicity, I am showing the case where the utility does not recoup any revenue when it declares a blackout ($\phi = 0$). When the utility shuts off the power, the supply curve shifts all the way to the left, creating lost producer and consumer surplus. Intuitively, the utility incurs a private cost from shutoffs through lost producer surplus and benefits from shutoffs because it faces no expected liability cost. So the utility's privately optimal choice of shutoffs depends on the relative magnitude of producer surplus and expected liability costs. Importantly, the utility does not internalize the loss in consumer surplus when it turns off the power, causing the utility's privately optimal choice of shutoffs to exceed the socially optimal level.

Assuming without loss of generality that the utility starts with no defensive capital ($k = 0$), solving the firm's problem when it does not declare a blackout is trivial. When the firm shuts off the power, its profit is constant regardless of defensive capital investment made by the firm.

$$\pi_1^* = \beta\phi\nu \quad \forall \quad k_1^{'*}$$

In the state of the world where it does not declare a blackout (π_0) the firm invests in defensive capital such that the marginal benefit of investment (reduction in expected damages and increased revenue) equals the marginal cost of investment (the rental rate paid to households).

$$-\beta\theta'(k')\bar{d} + \beta\gamma = r \tag{1.3}$$

Where $-\beta\theta'(k')\bar{d}$ is the reduction in expected liability costs from increasing defensive investment, $\beta\gamma$ is the increase in revenue the firm receives by increasing its defensive capital stock, and r is the rental rate of capital. The utility then chooses whether or not to declare a shutoff by comparing its optimized profit when it declares a shutoff (π_1^*) to when it supplies electricity (π_0^*). Figure A.4 presents the utility's decision rule for declaring a shutoff. Whenever the firm can earn greater expected profits by supplying electricity, it does not shut off the power.

This paper empirically studies how two changes impact utilities' use of power shutoffs. First, I use a difference in differences research design to study how a policy which effectively reduced the rate at which utilities pass liability costs on to consumers. In the model, the policy is akin to reducing the rate of pass through (ν). As a result of the policy, we expect the firms' profit function when it supplies electricity (π_0) to decrease by more than its shutoff profit function (π_1) decreases. As a result, if the firm supplies electricity prior to the policy, it is unclear whether it will increase or decrease blackouts following the change. I test this ambiguous prediction and show that utilities increase their use of shutoffs following the policy change.

Second, I use exogenous variation in wind direction and speed across days to estimate how utilities' use of shutoffs changes when they face higher total expected liability costs. In the model, firms face higher liability costs when the total replacement cost of structures threatened by a potential ignition ($\bar{d}$) is large. Increasing $\bar{d}$ in the model shifts π_0 down, but leaves π_1 unchanged. Depending on how large the drop in π_0 is, the firm may use more shutoffs or keep supplying electricity. I show that firms increase their use of shutoffs when areas with higher total structure replacement cost are threatened by a potential ignition. Finally, utilities should utilize blackouts more when they face high realizations of the probability of ignition ($\theta(k')$). As a result, we expect there to be more blackouts on days when the weather is conducive to fire ignitions along power lines (prediction (3)).

1.4 How does firms' precautions change when they face any liability?

1.4.1 Empirical Framework

I estimate the overall effect of the regulatory change on one utility's power shutoff use in a two way fixed effects empirical strategy. As explained in section 2.2, a 2017 regulatory change made by the California Public Utility Commission shifted the burden for liability costs from consumers of electricity to utilities in California. Several factors make estimation of the causal effect of the regulatory change on utilities' shutoff use difficult. First, due to stronger winds and an ever-drier climate over time, the likelihood of fire ignited by power lines has increased over time. As a result, a pre-post regulatory change comparison of shutoff use may reflect this increasing trend in ignition probability.

Second, the regulatory change affected all utilities at the same time, making it difficult to separate the policy effect from annual changes in firms' investment behavior.

I overcome these difficulties by using a two way fixed effects research design that compares the pre/post regulatory change in shutoff use at circuits with high ex ante ignition risk to their counterparts with low ex ante ignition risk. Low ignition risk circuits are a valid control group because the regulatory change was unlikely to impact firms' behavior in regions with low fire risk. Indeed, in section 1.3 I show that changing the amount of liability born by the firm does not impact its behavior if there is no chance of an ignition occurring at a circuit. Importantly, I control for daily weather conditions at each circuit such as wind speed, humidity, temperature, and relative humidity which are significant determinants of ignition risk.

Equation 1.4 models shutoff use (y_{it}) at each circuit i on day t as a function of daily weather conditions, infrastructure age, ex ante ignition risk, and the regulatory change.

$$y_{it} = \beta_0 + \beta_1 Treated_i \times Post_t + \beta_2 X_{it} + \gamma_i + \delta_t + \varepsilon_{it} \tag{1.4}$$

Where y_{it} is either equal to one when there is an active shutoff at circuit i on day t or the total number of customer hours of lost power at circuit i on day t. $Treated_i$ is one for all circuits with positive ex post ignition risk, and $Post_t$ equals one for all days following the regulatory change in December 2017. I determine ex post ignition risk at each circuit using San Diego Gas and Electric's modeled measure of ignition risk which they included in their 2021 Wildfire Mitigation Plan that was submitted to the California Public Utility Commission.[14] The ignition risk reflects the annual likelihood and consequence of fire risk at each circuit as of 2021.

The vector X_{it} contains daily wind speed, temperature, humidity, and precipitation

[14]See San Diego Gas and Electric's 2021 Wildfire Mitigation Plan. Modeled ignition risk is included in the attachment "2021 WMP CalPA-SDGE-DR1 02-11-2021".

binned into septiles as well as the average age of infrastructure in circuit i. The circuit fixed effects account for characteristics of each circuit, such as the slope of the land, which do not change over time. The calendar day fixed effects control for seasonality in the ignition threat across all circuits. The coefficient of interest ($\beta_1 \times 100$) measures how the likelihood of a shutoff changes, on average, at high risk circuits relative to low risk circuits after the policy shift. I cluster standard errors at the high fire threat district by week level to allow for correlation in shutoff use in areas with similar ignition risk during a calendar week.[15]

The two way fixed effects research design relies on a conditional parallel trends assumption which states that, conditional on the covariates, the trend in shutoff use would have been the same at high and low ignition risk circuits. I provide suggestive evidence of this assumption by estimating the following event-study model.

$$y_{it} = \alpha + \sum_{j=-3}^{2} \nu_j Treated_{i,t-j} + \psi X_{it} + \gamma_i + \delta_t + \varepsilon_{it} \tag{1.5}$$

Where the event time end points are binned at $t = -3, 2$ following Schmidheiny and Siegloch (2020).[16] The variable $Treated_{i,t}$ again takes a value of one if circuit i has a non-zero probability of ignition at post regulatory change calendar day t. Just as in equation 1.4, X_{it} includes nonlinear controls for daily changes in temperature, precipitation, humidity, wind speed, and infrastructure age. I cluster standard errors at the high fire threat district by calendar week level. Figure A.5 shows the event study results.

Each coefficient represents the cumulative annual effect of the 2017 rule change on power shutoff declaration in percentage points for years leading up to and following 2017.

[15] High fire threat districts were determined by the California Public Utility Commission in 2012. They are designed to show the areas which represent elevated risk for power line-ignited wildfires.

[16] Using binned end points in this way assumes that the treatment effect is constant more than 3 periods prior to treatment or 2 periods after treatment.

All coefficients are interpreted as the effect relative to the year prior to the rule change. For example, shutoff events were used around 0.025 percentage points more one year after the rule change than they were used one year prior to the rule change. The pre-treatment coefficients in Figure A.5 demonstrate that there are no anticipatory effects or underlying time trends in shutoff use that drive the estimated effect in Table A.4. All pre-period coefficients are statistically indistinguishable from zero and economically insignificant, providing support for the parallel trends assumption.

1.4.2 Data Used in Extensive Margin Analysis

Power Shutoff Events I obtain the date, duration, location, and number of impacted customers from power shutoff post-event reports for the period 2013-2020 from the California Public Utilities Commission.[17] Since Pacific Gas and Electric, Southern California Edison, and San Diego Gas and Electric serve the majority of electricity consumers in California and account for the largest share of power shutoffs historically, I restrict the sample to events initiated by one of these utilities. Furthermore, I exclude publicly owned utilities from this analysis because they have not been granted the authority to conduct power shutoff events by the regulator. Since San Diego Gas and Electric was the only utility to receive permission to use power shutoff events prior to 2018, I use exclude Pacific Gas and Electric and Southern California Edison when estimating how the 2017 liability rule change influenced utilities' use of shutoffs. The intensive margin analysis of the relationship between replacement costs and power shutoff use between 2018 and 2020 uses data from all three of California's largest private utilities.

Energy Infrastructure Information on the geographic location of distribution and transmission lines operated by Pacific Gas and Electric, Southern California Edison, and

[17] Utilities are required to submit under Ordering Paragraph 1 of California Public Utilities Commission (CPUC) Decision (D.) 19-05-042.

San Diego Gas and Electric is collected from publicly available Geographic Information System (GIS) files submitted to the California Office of Infrastructure Safety in 2020. The GIS data shows the location of each transmission and distribution line within a circuit and exclude critical energy infrastructure. Since the California Public Utility Commission reports shutoff events at the circuit level, I aggregate the line level data to the circuit level before string matching events to circuits by circuit name. On average across the three utilities, I match 97 percent of events to circuits using string matching.[18]

Climate Data I obtain wind speed and direction at ten minute intervals from the 3,041 weather stations operated by Pacific Gas and Electric, Southern California Edison, and San Diego Gas and Electric along their energy infrastructure and temperature, relative humidity, and precipitation from the 892 weather stations operated by the National Weather Service and Remote Automatic Weather Stations in California.[19] For each station, I compute daily average and maximum temperature, humidity, precipitation, and wind speed. Then, for each circuit I compute the inverse distance weighted average for each climate variable across all stations within 20 kilometers the circuit, generating daily average and maximum temperature, relative humidity, precipitation, and wind speed for each distribution circuit operated by Pacific Gas and Electric, Southern California Edison, and San Diego Gas and Electric in California.

Replacement Cost of Structures Since electric utilities are liable for the cost of replacing structures damaged by power line-ignited fires, I use parcel-level structure replacement costs to measure potential damages rather than the market value of each property. I obtain parcel level replacement costs of each property in California in the year that it is assessed from the Zillow Transaction and Assessment Database (ZTRAX) which contains parcel-level assessed values and transaction information for most counties

[18]For only 2019, events are geocoded at the line level. In the future I will conduct a robustness analysis that verify the property value results using line level data.

[19]I accessed the weather station data through the Mesonet API.

in the U.S. Zillow computes the replacement cost by taking the difference between the market value of the property and the market value of the land in the year of assessment. I adjust replacement costs to 2021 dollars using the consumer price index.[20]

Expected Damages Because the welfare calculation in equation 1.10 requires a measure of expected damages at each distribution circuit ($D(m_i, z_i)$), I obtain parcel level replacement costs and a measure of the fire risk faced by each structure. To compute the expected damages at each circuit if an ignition were to occur, I multiply the replacement cost of each parcel by a measure of fire risk. First, I compute the total replacement cost of structures within 5 kilometers of a circuit using the ZTRAX data described above. Second, I use the Risk to Potential Structures (RPS) index created by Scott et al. (2020) to capture the likelihood that each structure in the ZTRAX database would be damaged by a fire. The RPS index ranges from 0 (no damage) to 12 (fully destroyed) at the 30 meter pixel level and answers the question: "What would be the relative risk to a house if one existed here?" Since the RPS uses data from 2014, it reflects the risk to structures based on 2014 vegetation conditions. I compute expected damages at each circuit by multiplying the property value at each parcel by the inverse of its RPS index and summing to the circuit level.

Likelihood of Ignition at Each Circuit Since the welfare calculation in 1.10 requires knowledge of the ignition probability at each circuit (θ), I collect circuit level wildfire risk scores from publicly available data files submitted by San Diego Gas and Electric as part of its 2021 Wildfire Mitigation Plan.[21] The circuit level ignition probabilities are raw wildfire risk scores from San Diego Gas and Electric's internal fire risk model called the Wildfire Next Generation System. Each risk score represents the probability of ignition adjusted for wind patterns, vegetation, and infrastructure hardening at

[20] https://fred.stlouisfed.org/series/CPALTT01USA659N.
[21] See San Diego Gas and Electric's response to data request CALPA-SDGE-01 questions 4 and 5 here.

a circuit. Since San Diego Gas and Electric only applied the model to its circuits in areas with elevated ignition risk, the probability of ignition is assumed to be zero at circuits that were not modelled.

Energy Usage The final information necessary for the welfare calculation in equation 1.10 is a measure of energy usage at each circuit. For now, I use publicly available energy usage data reported by San Diego Gas and Electric at the zip code level for each quarter of the year. For each zip code, I compute the average energy use between 2013 and 2017. Then, I assign energy use to each circuit in a zip code in proportion to its share of total circuit miles in that zip code. For example, if there are two circuits in a zip code that have the same total length of power lines, then I would assign half of the total zip code energy use to each circuit. In the future, I hope to replace this approximation with the actual reported circuit level energy use.[22]

Summary Statistics Table A.1 reports summary characteristics for the daily panel of distribution circuits operated by San Diego Gas and Electric between 2013 and 2020. At the daily level shutoff events are rare, occurring on 0.002 percent of circuit-days, on average.[23] The average maximum daily wind speed across all circuits between 2013 and 2020 is 7.4 meters per second, but there is substantial variation with some circuits experiencing wind speeds as high as 96 meters per second. Across San Diego Gas and Electric's service territory, 88 distribution circuits ever experience a shutoff event between 2013 and 2020, reflecting San Diego Gas and Electric's efforts to target only the highest risk circuits.

[22] I have a pending application with San Diego Gas and Electric to access this data.

[23] However, shutoff events become much more prevalent conditional on high wind speeds. In particular, conditional on a circuit having wind speeds greater than 9 m/s the average likelihood of a power shutoff event increases to 0.3 percent.

1.4.3 Results

Since prior theoretical work shows that applying liability to firms may be ineffective, I first estimate how San Diego Gas and Electric's use of power shutoffs changes when they fully bear liability costs. Table A.4 presents results from the model presented in equation 1.4. The reported estimates reflect how San Diego Gas and Electric's use of shutoffs changes at *ex ante* high ignition risk circuits relative to low risk circuits following the 2017 policy change which increased their expected liability. Column 1 displays the effect of applying liability on the likelihood of a shutoff and column 2 presents the same effect, but for the number of customer hours without power. Both specifications include weather controls, circuit fixed effects, and calendar fixed effects.

The estimated effect suggests that the rule change led to a 5.6 percentage point increase in power shutoffs. Relative to the average probability of a shutoff prior to the 2017 reform, this amounts to more than an 80-fold increase in shutoff use. The estimate in column 2 implies that, on average, the number of customer hours impacted by shutoff events increased by 923 customer hours following the rule change (a 6-fold increase relative to the pre-treatment mean). Assuming each of the 923 customer hours of lost power would have had average energy use as reported by the Energy Information Administration in 2020, this estimate implies that the policy led to between \$150 and \$51,000 in lost consumer surplus at the average distribution circuit. Together these results suggest that the liability regulation effectively encouraged ignition prevention behavior in this setting.

Although shifting fire liability costs onto electric utilities effectively increased their precautionary behavior, the greater reliance on shutoffs may have burdened consumers if they affected individuals with a high value of electricity use. Since consumers whose medical or life-supporting devices rely on electricity likely have a high value of their electricity use, I estimate how San Diego Gas and Electric's use of shutoffs changes by the share of total customers that rely on electricity for medical needs. San Diego Gas and Electric publicly reports the number of customers with medical devices that rely on electricity by census tract, so I estimate an aggregated version of equation 1.4 on a daily panel of census tracts in California. The results of this analysis are reported in figure A.8 and figure A.9. These estimates suggest that shutoff use increased the most in census tracts with the highest share of customers relying on electricity for their medical needs and for life support. Because these customers have a high value of energy use, San Diego Gas and Electric's increased use of shutoffs following the policy change was likely costly for consumers.

To validate the empirical model above, I estimate the effect of increasing electric utilities' share of liability costs by circuit-level ignition risk and daily weather conditions in Appendix A. The utilities' stated criteria for power shutoffs suggest wind speed and humidity are two prominent drivers of ignition risk. As expected, I find that shutoff use increased almost 200-fold at the circuits with the highest risk of ignition. Furthermore, I find that wind speed and humidity are also significant predictors of shutoff use. These results help to provide confidence that the empirical model above is correctly specified and captures utilities' ignition prevention behavior.

1.5 How does the level of liability that a firm faces affect precaution?

1.5.1 Empirical Framework

Precaution and Threatened Property Values

According to the theory developed in section 1.3, utilities' use of shutoffs should respond (either positively or negatively) to the liability cost they bear. One way to test this hypothesis would be to estimate a linear model that relates the probability of a shutoff at circuit i on day t (y_{it}) to the total replacement cost of structures near circuit i ($Value_i$).

$$y_{it} = \nu Value_i + \varepsilon_{it} \tag{1.6}$$

Under the conditional independence assumption, ν identifies the effect of liability on firms' use of shutoffs. However, the conditional independence assumption is unlikely to hold in this example because unobserved determinants of shutoffs such as the moisture content of vegetation, regional weather conditions, and the presence of critical energy infrastructure are likely correlated with structure replacement costs. To overcome this challenge and isolate the effect of structure replacement cost on shutoffs, I use daily changes in wind direction to create exogenous variation in structure replacement costs that would be threatened by an ignition, if it occurred. Since power line-ignited fires are more likely to occur during periods of extreme wind speeds (Syphard and Keeley (2015)), wind direction is likely to be a relevant determinant of whether a region is threatened by a wildfire on any given day, t. Furthermore, since, on average, daily variation in wind direction is uncorrelated with both power shutoffs and property values the conditional independence assumption likely holds.

Following a procedure implemented by Missirian (2020) in a different context, I use reported wind conditions from stations operated by Pacific Gas and Electric, Southern California Edison, and San Diego Gas and Electric to determine which zip codes are downwind of each circuit. Figure A.6 displays this process. I compute the horizontal ("U-wind") and vertical ("V-wind") wind vectors by multiplying the wind speed by the sine or cosine of the wind direction (in radians). After converting the horizontal and vertical wind vectors from meters per second to degrees latitude or longitude per second, I can compute how far away an object would travel if it could remain airborne for one second (the end of the blue arrow in Figure A.6). Finally, I scale the horizontal and vertical wind vectors up by an estimate of how long a lit ember could remain airborne if picked up by the wind from Albini et al. (2012).[24]

I use circuit level changes in wind direction across days to assign which zip codes lie downwind of a utility's power lines. I choose to use zip codes as the unit of analysis for several reasons. First, it uses borders which are determined by the California government, rather than boundaries that I have chosen myself. Second, using zip codes allows me to control for other characteristics that are important determinants of utilities' shutoff use such as population and total energy use which are not available at finer geographic scales using publicly available data. Finally, the data on structure replacement costs is available for the universe of parcels in California at the zip code level, but may be missing at more granular levels of aggregation. In a robustness analysis, I re-estimate the relationship between structure replacement cost and shutoff use using only variation in wind direction within twenty kilometers of power lines, finding similar results to the aggregate zip code analysis.

Figure A.7 shows how I determine downwind structure replacement cost in the empirical analysis using an example of 13 zip codes from San Diego County in California. The tan zip code in the center of both panels contains three distribution circuits and the black circles represent the centroid of each circuit. In my empirical framework, I define each tan zip code in my sample as an "origin" zip code. All of the white and yellow zip codes lie downwind of the origin zip code at some point during 2018 and 2020. I define

[24]Albini et al. (2012) estimate that the maximum spotting distance for a wind driven fire is 10 kilometers. Assuming that wind speeds are at the third quartile observed across my sample between 2018 and 2020 (6.7 meters per second), the 10 kilometer estimate implies that an ember could remain airborne for up to 24 minutes. Estimates are robust to other assumptions of how long a lit ember could remain airborne (such as 5 minutes).

these zip codes as "destination" zip codes because they are the set of possible destinations where an ember could land if picked up at a circuit in the origin zip code. Each black line points in the direction that the wind is blowing, and its end point is how far away from each circuit a lit ember could travel given observed wind speed and direction. When the black line intersects with a zip code, I define the zip code as downwind of the origin zip code. Therefore, in panel (a) the three yellow destination zip codes to the north of the origin are downwind, while the following day (shown in panel (b)) the three destination zip codes to the west are downwind. I estimate the relationship between the total replacement cost in the downwind zip codes and shutoff use at circuits in the origin zip code. As a result, this strategy uses daily variation in liability that is driven by exogenous changes in wind speed and direction. Equation 1.7 formally presents this research design.

Since there may be underlying static characteristics about each zip code, such as geography, that correlate with shutoff declaration and threatened property values, I construct a paired data set of origin and destination zip codes and control for a pair fixed effect following Kuhn et al. (2011). For each day t and origin zip code o the data file contains a set ($N(o)$) of neighboring destination zip codes indexed by d which are ever downwind of zip code o between 2018 and 2020. By including pair fixed effects, ν_{od}, this strategy accounts for time invariant characteristics of pairs that may be correlated with structure replacement cost and power shutoffs, such as vegetation moisture. Furthermore, I include a calendar day fixed effect which accounts for day-specific unobserved heterogeneity which impacts all zip code pairs, such as seasonality or statewide climatic factors.

$$y_{jodt} = \beta_1 Value_{jd} \times DW_{jodt} + \beta_2 Value_d + \beta_3 DW_{jodt} + \beta_3 X_{jot} + \beta_4 X_{jdt} + \nu_{od} + \delta_t + \gamma_{jt} + \varepsilon_{odt} \tag{1.7}$$

Where y_{odt} is a binary variable indicating whether a shutoff is in effect in zip code o which is ever upwind of zip code d on day t. $Value_d$ is the logged and de-meaned total (or average) structure replacement cost in zip code d and DW_{odt} is equal to one if zip code d is downwind of zip code o on day t. The model includes time-varying covariates (X_{ot}, X_{dt}) which are specific to zip codes o and d respectively and include average daily wind speed, temperature, specific humidity, and maximum wind speed. In order to allow the effect of the climatic controls to non-linearly impact the outcome, I bin each control variable into septiles. I also control for the 2020 wildfire hazard potential interacted with the downwind indicator and the share of 2010 zip code population living in the wildland-urban interface interacted with the downwind indicator to control for day-to-day variation in characteristics of the downwind landscape such as vegetation and slope. Finally, I include utility by year fixed effects (γ_{jt}) which account for annual changes in utilities' plans to prevent ignitions.

Since I de-mean the structure replacement cost in equation 1.7, β_3 is the change in shutoff likelihood when a zip code with average structure replacement cost is downwind. The coefficient of interest β_1 measures the average percentage point change in the likelihood of a power shutoff with respect to a one percent increase in downwind structure replacement cost. Furthermore note that while the coefficient β_2 captures the effect of non-threatened property values, it is not estimated because the replacement cost is collinear with the pair fixed effects. Under the conditional independence assumption, β_1 and β_3 identify the causal effect of down wind structure replacement cost on the probability of a shutoff.

Causal identification of the relationship between potential liability and power shutoffs in equation 1.7 relies on exogenous changes in wind speed and direction across days. To provide suggestive evidence that the daily variation in wind conditions is as good as randomly assigned, I compare average socioeconomic and demographic characteristics of destination zip codes by downwind status in Table A.3. The average characteristics for not-downwind and downwind zip codes are shown in columns 1 and 2 while the difference in means as a percent of a standard deviation is presented in column 3. While downwind and not-downwind zip codes are statistically different across nearly all characteristics, all differences are small, accounting for less than 8% of a standard deviation for all observed variables. For example, although the median replacement cost of structures in downwind zip codes is around $1,000 more than in non-downwind zip codes, this is less than 2% of the average replacement cost. Furthermore, the empirical framework in equation 1.7 includes a pair fixed effect which controls for all time-invariant characteristics about zip codes.

Model 1.7 estimates how threatened property values impact the probability of shutoff declaration, but does not explore how this effect is distributed across each zip code's socioeconomic status. The next part of the analysis addresses this question by separately interacting threatened property values with an indicator variable equal to one if a non-zero share of each zip code o's (or d's) 2010 population lives in a census tract designated as a disadvantaged community by the California government (denoted by DAC_o and DAC_d respectively).

$$\begin{aligned} y_{odt} = {} & \gamma_1 DAC_o Value_d DW_{odt} + \gamma_2 DAC_d Value_d DW_{odt} + \gamma_3 Value_d DW_{odt} \\ & + \gamma_4 DAC_o DW_{odt} + \gamma_5 DAC_d DW_{odt} + \gamma_6 DW_{odt} + \gamma_7 X_{ot} + \gamma_8 X_{dt} + \nu_{od} + \delta_t + \varepsilon_{odt} \end{aligned} \tag{1.8}$$

The parameters of interest γ_1 and γ_2 measure the percentage point impact of threatened property values relative to days when they are not threatened and on days when zip code o is disadvantaged or zip code d is disadvantaged respectively. A positive estimate of γ_1 would suggest that higher threatened property values in non-disadvantaged zip codes increase the probability of a power shutoff in disadvantaged zip codes. Similarly, a positive estimate of γ_2 would imply that higher threatened property values in disadvantaged zip codes increase the probability of shutoff declaration in non-disadvantaged zip codes. Standard errors are again clustered at the calendar week level to allow for correlation in shutoff use across circuits within a week.

1.5.2 Additional Data Used in Intensive Margin Analysis

Replacement Cost I use the same parcel-level replacement costs from Zillow ZTRAX to compute the total and median replacement cost in each zip code. Table A.2 reports that the average total replacement cost across all zip codes in the sample is nearly $7 billion dollars, while the average of the zip code-level median replacement cost is $53,000 dollars.

Vegetative Cover I use the discrete Wildfire Hazard Potential index to capture underlying vegetative conditions in the areas surrounding distribution circuits in California.[25] Values of the WHP index indicate wildfire risk and range from 1 (very low)

[25] Dillon, Gregory K; Gilbertson-Day, Julie W. 2020. Wildfire Hazard Potential for the United

to 5 (very high). The WHP index is intended to guide strategic long-term management of vegetation and is based on vegetation and fuels data from LANDFIRE 2014. As a result, the WHP reflect vegetative conditions at the end of 2014. Future work will utilize vegetation data from more recent products such as LANDFIRE 2018.

Wildland Urban Interface Since utilities' decision to declare a shutoff event could be impacted by whether a circuit is located in an area that is high fire risk, I obtain the boundaries of the Wildland Urban Interface (WUI) from the California Department of Forestry and Fire Protection's Fire and Resource Assessment Program. The WUI is defined as an area with dense housing adjacent to vegetation that can burn in a wildfire.[26] Because the property value analysis is estimated at the zip code level, I compute the share of total 2010 zip code population living within the WUI.

Potential Liabilities In order to identify structures that would be threatened by a potential ignition, I use daily variation in wind direction at the centroid of the area where each distribution circuit operated by Pacific Gas and Electric, Southern California Edison, or San Diego Gas and Electric in California overlaps with a zip code. In the dataset construction I refer to zip codes with a distribution circuit as "origin" zip codes and zip codes that lie downwind of the origin zip code on any given day as "destination" zip codes. Using data on the daily average wind direction and maximum wind speed described above, I assign destination zip codes to each origin zip code for each day of the sample. I describe this process in detail below.

As shown in figure A.6, I use two results from trigonometry to calculate the vertical and horizontal wind vectors in degrees of latitude or longitude per second.[27] After converting the vertical and horizontal wind vectors to degrees of longitude per second

States (270-m), version 2020. 3rd Edition. Fort Collins, CO: Forest Service Research Data Archive. https://doi.org/10.2737/RDS-2015-0047-3

[26]Specific housing density and vegetation thresholds for WUI classification can be found here.

[27]The vertical wind vector is given by $x \sin\theta$ and the horizontal wind vector is given by $x \cos\theta$, where x is the wind speed in meters per second and θ is wind direction measured from the x-axis in radians.

and latitude per second respectively, I multiply each vector by a measure of how many seconds a lit ember can stay airborne from Albini et al. (2012).[28] I then use the scaled-up vertical and horizontal wind vectors to compute where an ember would land if it were picked up by the wind at each distribution circuit. Finally, I connect the start and end points with a line and assign a zip code as downwind if it intersects with that line. As shown in table A.2, the daily wind speeds across distribution circuits during the sample period range between 24 and 88 miles per hour.

Disadvantaged Community Definition I use the California Office of Environmental Health Hazard Assessment's definition of a disadvantaged community in the CalEnviroscreen 2018 update to assign disadvantaged status to each zip code. This definition classifies the census tracts with CalEnviroScreen 3.0 scores in the top 25% of all tracts in California as disadvantaged communities. The CalEnviroScreen score accounts for pollution exposure, environmental conditions, health factors, and socioeconomic factors which could magnify the negative effects of pollution exposure. Since disadvantaged status is assigned at the census tract level, I compute the share of total 2010 population in each zip code that lives in a disadvantaged tract. For the main analysis I assign disadvantaged status to any zip code with more than 50 percent of its population living in a census tract designated as disadvantaged.

Summary Statistics Table A.2 reports the summary statistics for relevant variables that I use in this analysis. On the most active day of power shutoffs in my sample there were 80 concurrent power shutoffs. However, shutoff events are very infrequent at the daily level, occurring on average 0.8% percent of total zip code-days in the sample. The last row of table A.2 shows that there are 539 zip codes in California that ever experience

[28]Albini et al. (2012) report a maximum spotting distance of 10 kilometers for wind driven fires. To convert this estimate to seconds that an ember is airborne, I multiply 10,000 meters by the inverse of the third quartile of wind speed in the sample yielding an estimate of about 18 minutes. This estimate means that at it would take a lit ember 18 minutes to travel 10 kilometers at the third quartile of wind speed in the sample.

an event between 2018 and 2020. The average replacement cost is substantial at around \$6.8 billion and there is significant variation across zip codes with a standard deviation of \$6.6 billion.

I construct the final sample by dropping all days that do not fall under the minimum criteria for a shutoff event used by Pacific Gas and Electric in 2021 as shown in figure **??**.[29] I make this sample restriction based on the reported wind speeds and humidity in origin zip codes rather than destination zip codes because this reflects climate conditions around the power lines themselves. I further drop months where no shutoff events occur between 2018 and 2020 since these months do not help identify the coefficient of interest from model 1.7. The final sample consists of a daily panel of 13,039 unique origin-destination zip code pairs.

1.5.3 Results

In settings where firms' assets are significantly less than their liability costs from an accident, it may be optimal for firms to declare bankruptcy (Shavell (1986)). A common solution to this problem posed by regulators is to cap firm liability, providing incentives for precaution without leading to bankruptcy. However, because prior estimates of firms' precautionary response to liability are from one point in the distribution of potential liabilities, regulators have limited information about which level to place the cap on damages. The estimates in this section leverage firms' full distribution of potential liabilities from power line-ignited fires, allowing me to non-parametrically estimate their precautionary response to liability.

Table A.5 reports the main results from regression model 1.7. The coefficient of interest is reported in row 1 and is interpreted as the percentage point change in power

[29] The minimum criteria are wind speeds greater than 20 mph and relative humidity less than 30%. Pacific Gas and Electric has many other criteria for declaring a shutoff, but these are the minimum criteria that I can observe using the publicly available climate data.

shutoff declaration probability that results from a 1 percent increase in the replacement cost of downwind structures relative to days when the properties are not downwind. Since I de-mean the replacement cost of structures, the estimate in row 2 reflects the change in shutoff likelihood when a zip code with average total (or mean) replacement cost lies downwind. Columns 1 and 2 report the estimate of firms' precautionary response to liability with total and mean zip code replacement cost as the independent variable of interest. Both specifications include controls for daily maximum wind speed, maximum temperature, average relative humidity, and cumulative precipitation in the origin and destination zip codes. In addition, both specifications include origin-destination zip code pair fixed effects and calendar day fixed effects.

The estimate in column 1 suggests that, on average, utilities' are 0.02 percentage points (100%) more likely to use a power shutoff when a region with 10% higher total zip code replacement cost lies downwind. Assuming that baseline total replacement cost is at the average level I observe in the sample ($6.8 billion) implies that shutoff use increases by 100% when potential liabilities increase by $680 million. However, the positive relationship between total liability and shutoff use could reflect utilities' increased willingness to undertake precaution when densely populated regions lie downwind. The estimate in column 2 shows that firms consider liabilities independently of population, suggesting that utilities use shutoffs 160% more when the mean downwind zip code structure replacement cost is about $6,000 higher.[30]

Although prior work suggests that the relationship between liability and precaution should be nonlinear, the estimates in table A.5 assume a linear relationship. I relax this linearity assumption by binning total (or mean) zip code replacement cost by decile and re-estimating equation 1.7. Figures A.10 and A.11 report the resulting estimates of

[30]In Appendix A, I estimate robustness specifications that explicitly control for population. While I find that downwind population is a relevant determinant of shutoff use, it does not alter the estimates in table A.5.

downwind total and mean replacement cost on power shutoff use. The estimates in figure A.10 suggests that shutoff use increases in total structure replacement cost until liability exceeds \$10 billion (the eighth decile of total replacement cost), after which it begins to decrease. Similarly, the response of shutoffs to average zip code replacement costs in figure A.11 is increasing until mean liability cost exceeds \$85 thousand (the eighth decile of mean replacement cost). Shavell (1986) posits that as the ratio of liability to assets increases, the firm will eventually begin to take fewer precautions to prevent an accident. The estimates I report above are consistent with this prediction. Utilities take greater precautions until their total liability from a potential ignition exceeds \$10 billion and then begin to take less precautions.

Because utilities direct shutoffs to areas with higher structure replacement costs, there may be systematically more precaution taken in high socioeconomic status communities that tend to have greater property values. I explore this possibility in Appendix A and find that because low socioeconomic status communities tend to live in low ignition risk areas in this setting, there is not relationship between firms' response to liability and socioeconomic status. In other settings where high and low socioeconomic status communities live in high risk areas at similar rates, there may be systematic distributional consequences of liability regulation.

1.5.4 Robustness

Factors such as vegetation conditions near power lines, extent of interaction between housing and wilderness, and energy consumption patterns could drive utilities' use of shutoffs. If these factors are also correlated with structure replacement costs, then the estimated relationship between shutoffs and potential liability could be biased. In table A.6, I estimate several modifications of regression model 1.7 to test the robustness of the main result in table A.5. Column 1 replicates the main estimate from table A.5. Column 2 adds a control for the share of total population in destination zip code d living in the WUI interacted with the downwind indicator, DW_{odt}. This covariate captures daily changes in the number of structures near vegetation that is likely to burn in the event of a fire. In column 2, I also control for the average WHP index in each destination zip code interacted with the downwind indicator. This covariate measures the conduciveness of vegetation in the downwind zip code to spreading fire. Column 3 adds separate controls for monthly electricity usage in zip codes o and d respectively. These additional covariates capture patterns in electricity usage that are relevant to firms' shutoff decision.

The empirical model in equation 1.7 uses daily changes in downwind structure costs to estimate the relationship between shutoffs and liability. However, evidence suggests that utilities monitor forecasted wind conditions in addition to current conditions. As a result, utilities may base their shutoff decisions on their expectation of which regions will be downwind in the upcoming days. To account for this behavior, I define a destination zip code as downwind if it lies downwind of the origin zip code at any time in the next five days. For example, the downwind indicator, DW_{odt}, is set equal to one if a destination zip code is downwind anytime over the next five days (between day t and day $t + 5$). I report the results from this specification in column 4 of table A.6. The main estimate of interest is positive, significant, and of a similar magnitude in all specifications.

Since the empirical framework in equation 1.7 is specified at the zip code level, it may include properties that are located far away from high-ignition risk circuits. If utilities only consider structures that are very close to high-risk power lines (and therefore very likely to be destroyed if an ignition occurs), then the zip code analysis could be misspecified. In appendix A, I address this by estimating 1.7 at the circuit level. I do this by using daily variation in the replacement cost of structures that lie downwind of power lines and are located within 20 kilometers of a circuit. Using this local variation, I estimate similar effects to the main result from table A.5.

1.6 Discussion and Conclusion

1.6.1 Short Run Welfare

In this section I derive the short run welfare change resulting from a decrease in the pass through rate of liability costs. Consumer surplus is the sum of the value of electricity consumption, total payments to the utility, and rental payments from the utility to the household.

$$CS = \bar{p}Q(1-L) - \beta(\gamma k' + \nu)(1-L) + rk' \tag{1.9}$$

Where the first term is the consumer's dollar valuation of their electricity consumption, term two is the consumer's payment to the utility, and term three is the utility's rental payment to the household. Producer surplus is denoted by the utility's profit function. Since total welfare is the sum of consumer and producer surplus, I can write the change in welfare from a change in the rate of capital return as the sum of the change in consumer and producer surplus. Since this model has a constant marginal cost, producer surplus is zero. By including producer surplus in the welfare derivation, I therefore obtain an upper bound on the short-run welfare change.

$$WF(\nu') - WF(\nu) = PS(\nu') - PS(\nu) + CS(\nu') - CS(\nu) \tag{1.10}$$

$$WF(\nu') - WF(\nu) = \beta[\theta(k')\bar{d} - (\bar{p} - p)\mathrm{Q}][P(L=1 \mid \nu') - P(L=1 \mid \nu)] + \tag{1.11}$$

$$\beta\bar{d}[\theta(k'(\nu')) - \theta(k'(v))](1 - P(L=1 \mid \nu) \tag{1.12}$$

Where $\bar{p}$ is the consumers' maximum willingness to pay per kilowatt hour and $\nu' < \nu$ is the capital return after the 2017 CPUC rate case decision. Since this study estimates the short-run response of firms to the change in the share of liability cost they bear, I assume that the third term is zero so that the welfare estimates reflect short run variation. There are three parameters that characterize the short run welfare change from an increase in the share of liability born by firms in equation 1.10: (1) the change in probability of shutoff event following an increase in the share of liability born by firms $(P(L = 1 \mid \nu') - P(L = 1 \mid \nu))$, (2) expected damages $(\theta(k')\bar{d})$, and (3) consumers' maximum willingness to pay for electricity $(\bar{p})$.

There are several important caveats to the welfare change represented in 1.10. In the model, consumers value their home at its replacement cost and receive a payment from the utility equal to the home replacement cost if the structure burns down. As a result, consumers in this model do not care whether their home burns down. In practice, consumers may have a value of their home which exceeds the replacement cost, causing consumer surplus to potentially increase when firms use more shutoffs. Thus, the welfare change in equation 1.10 is likely larger (in absolute terms) than a more detailed model that incorporates intrinsic home values.

Another caveat to keep in mind is that I am assuming the adjustment of defensive capital cannot occur in the short term (making term three in equation 1.10 zero. Since the sample includes three post-policy years and the utilities have extensive networks of power lines, the extent of defensive capital investment is limited in this setting. However, future analyses of defensive capital's impact on the likelihood of ignition would be very useful.

In order to compute (1), I use the estimates by ignition risk presented in Figure A.1. This strategy assigns the same probability change to all circuits that are in the same decile of ignition risk. Then, I use parcel-level assessed values from the Zillow ZTRAX dataset and the Risk to Potential Structures index created by Scott et al. (2020) to compute (2) as described in the data section. To account for the likelihood that an ignition occurs at each circuit, I obtain modeled ignition probabilities from San Diego Gas and Electric's data filing as part of its 2021 Wildfire mitigation plan. Multiplying damages by the likelihood of ignition yields expected damages (2). In order to compute (3), I multiply the average historic energy use at each circuit (described in the data section) by estimates of the value of lost load per kilowatt hour of energy use from a 2019 value of service study conducted by Southern California Edison.

Since the empirical literature on the value of lost load is still young, I first estimate the per kilowatt hour value of lost load required for there to be a welfare change of zero at each circuit. Figure A.12 plots the number of circuits by the value of lost load necessary for welfare to remain the same following the policy. For most circuits in the sample, the maximum value of lost load required for a non-negative welfare change is less than \$3 per kWh. The average value of lost load necessary for welfare to remain unchanged across all circuits is \$0.3 per kWh and the median is \$0.01 per kWh. The smallest estimate of the value of lost load conducted for Southern California Edison customers is \$1.90 for residential customers, implying that the observed change in liability likely leads to a reduction in welfare.

To calculate a conservative estimate of the short run welfare change at each circuit, I assume that consumers' value of electricity is \$0.22 per kilowatt hour, the average retail price of electricity in California. Figure A.13 plots a histogram of the estimated welfare change at each circuit in millions of dollars. The short run welfare effect is negative at nearly every circuit in the sample, suggesting that the value of lost electricity use at each circuit during power shutoffs outweighs the reduction in expected damages. Adding the welfare effects across circuits implies that the regulatory change reduced welfare by between \$17 million and \$7 billion depending on the value of lost load used in the calculation. In the next section I provide a short discussion of the results, explore policy implications, and suggest directions for future research.

1.6.2 Conclusion

In summary, I find that utilities increase their use of shutoff events following an increase in the share of liability for power line-ignited fire damages and that this policy change reduced welfare in the short term by leading utilities to over-utilize shutoff events. The theoretical framework outlined in section 1.3 suggests that the observed increase in blackouts crowds out other types of ignition prevention, such as burying power lines underground. I further provide evidence that utilities increase shutoff event use at the circuits with the highest likelihood of fire ignition and on days when threatened downwind property values are higher. There are several key implications for policymakers from this paper: First, these results suggest that policymakers can increase utilities' ignition prevention effort by increasing the share of liability for fire-related damages that they bear. Second, the policymaker can influence which ignition prevention efforts the utility undertakes by clearly defining which strategies will allow the utility to avoid a negligence ruling. In the California context, the 2017 rule change and subsequent rule amendments did not clearly specify what utility actions (or lack thereof) would lead them to be negligible for fire damages. This lack of clarity may have led utilities to use shutoff events as a signal that they are not acting negligently, leading to an overuse of blackouts at the expense of longer term mitigation investments. Third, since utilities appear to direct precautionary effort towards regions with higher threatened property values, policymakers should be wary of potential distributional consequences of liability regulations.

There are several areas where future research can extend this analysis to further inform our knowledge of liability regulations and how they impact firm precaution in the power line-ignited fire setting. First, future research should explore whether the circuits with the highest welfare loss from an increase in liability are located in areas with a large share of disadvantaged community members. For example, if expected damages are low and the VOLL is high in disadvantaged communities, then this implies that increasing the share of liability on firms is regressive in this setting. Second, future studies should take a longer term view of the impact of liability regulation on utilities' ignition prevention behavior. Researchers could do this by collecting data on other measures of ignition prevention, such as burying power lines, which utilities can take in the long term. Although liability regulation has a negative welfare impact in the short term, it could be beneficial in the long term if it encourages precautionary activities that both reduce the likelihood of ignition and the probability that a power shutoff occurs. Finally, more work is needed to identify which ignition prevention strategies most effectively reduce the likelihood of a fire caused by power lines. In particular, cost benefit analyses may need to be revised to account for the fact that capital investments both reduce the probability of ignitions *and* blackouts in the future.

Chapter 2

Causal Effects of Renewable Portfolio Standards on Renewable Investments and Generation: The Role of Heterogeneity and Dynamics

2.1 Introduction

Most industrialized countries now have commitments, or in a few cases laws, with targets to reach carbon neutrality status by 2050 or 2060. A central strategy to reach this target common across countries is the decarbonization of the electricity generation sector through expanding renewable resources. In the United States, the renewable portfolio standards (RPS), a state-level policy imposing standards for renewable electricity sales in a state, is one of the most prominent policies implemented to date with the goal of incentivizing decarbonization of the electricity sector. Beginning with Iowa in 1991, thirty states and Washington D.C have now enacted RPSs; these states represent more than 70 percent of the US population and 64% of total generation capacity in 2019.[1] As the U.S. federal government works toward its stated goal of 100% carbon-free electricity by 2035, many of the proposed federal policies mimic state-level RPS in how they displace fossil fuel use in electricity generation, reduce greenhouse gas emissions, and ensure reliable operation of the electrical grid.[2] Given the centrality of RPS to U.S. decarbonization goals, it is imperative to provide a better understanding how such policies affect the deployment of renewable electricity generation sources.

While RPSs have been designed and enacted to increase the share of renewable electricity supplied and sold in states adopting them, there is still limited consistent empirical evidence about their efficacy and whether RPS cause investments in renewable capacity. Two key issues complicate the identification of the causal effect of RPS on renewables. First, RPS policies are not randomly assigned across states, and previous studies suggest that political ideology, underlying renewable resource potential, labor market conditions, and interest group pressure are strong predictors of RPS adoption (Lyon (2016)). Second, due to significant differences in policy design and renewable resource endowments, RPS policies are likely to have dynamic and heterogeneous effects across states and have been adopted in a staggered manner since the mid 1990s (see Figure B.1 below).

Causal identification in this setting is complicated by difficult to quantify state-specific characteristics such as political ideology and natural resource endowment which may correlate with both RPS implementation and the deployment of renewable energy generation. Further, national-level policies that are correlated with RPS implementation, such as the U.S. federal government Production Tax Credit also create challenges for causal identification. To address these identification concerns, virtually all of the prior empir-

[1] To date, more than 70 proposals for a national portfolio standard have been introduced but none has become law (Congressional Research Service, 2020).

[2] https://www.whitehouse.gov/briefing-room/statements-releases/2021/04/22/fact-sheet-president-biden-sets-2030-greenhouse-gas-pollution-reduction-target-aimed-at-creating-good-paying-union-jobs-and-securing-u-s-leadership-on-clean-energy-technologies/

ical literature on the impact of RPSs uses panel data regressions with state and year fixed effects, often labelled as two-way fixed effects (TWFE) or sometimes difference-in-differences (DD) models. Recent econometric research has shown that in settings with heterogeneous treatment effects (like in the case of the RPS policy adoption), TWFE or DD estimators identify a weighted average of treatment effect parameters which may not correspond to the overall average treatment effect on the treated (ATT) (Sun and Abraham (2020), de Chaisemartin and D'Haultfœuille (2020), Borusyak and Jaravel (2017), Goodman-Bacon (2021)).

We address these challenges by using the most comprehensive data available on RPS policies and renewable electricity capacity investments and generation in the U.S. and present new evidence on the causal effect of RPS policies on renewable electricity capacity investments and generation by renewable resource using state-level data for 1990-2019. We exploit the long time-series of data, combined with the staggered timing of RPS adoption across states to derive heterogeneity-robust estimates of the causal effect of RPS on renewables capacity and generation using the econometric methods recently developed in Callaway and Sant'Anna (2021). This approach, we believe, provides the first *source-specific* panel data evidence on the efficacy of RPS that is robust to treatment effect heterogeneity, a pervasive feature of such programs.

We find that, on average, RPS policies increase wind generation capacity by 600-700 MW but have no significant effect on investments in solar generation capacity. Relative to the average installed wind capacity in 2019 among ever-adopting states, the point estimates imply that wind generation capacity increased by 21% as the result of RPS, a sizeable increase. After modifying our empirical strategy to allow for treatment effect dynamics, we find that the impact of RPSs on wind capacity investments ramps up slowly: most of the capacity additions occur 5 years after RPS implementation. We also examine the possibility of policy spillover where the introduction of an RPS in one state leads to a change in capacity mix in the neighboring states. We find weak and limited evidence that RPS policies cause the mix of electricity generators to change in unregulated neighboring states. Overall our findings underscore the importance of accounting for dynamic responses to RPSs, of allowing for differential effects of RPSs on wind and solar investments, and of incorporating the most recent data available, since installed renewable capacity both increased by 40% (wind) and 164% (solar) between 2015 and 2019.[3] As we explain below, these important considerations distinguish our paper from the previous literature.

Due to the now long history of RPS policies, dating back to 1991, and its significance for the electricity generation sector and for decarbonization goals, a sizable literature examines the impact of RPSs on renewable generation capacity investments, carbon emissions, and electricity prices. Overall, the evidence from the previous literature on the impact of RPS policies on the deployment of renewable electricity generation is mixed. Several studies (e.g., Shrimali et al. (2015) and Yin and Powers (2010)) find a positive relationship between RPS requirements (or compliance) and renewable electricity generation using a difference-in-differences type empirical strategy, and highlight the importance of controlling for state-specific features of both determinants and characteristics of policies across states. At the same time, other studies find little or no evidence of an effect of RPS policies and deployment of renewable generating capacity (e.g., Greenstone and Nath (2023), Fullerton and Ta (2022), Feldman and Levinson (2023), and Upton and Snyder (2017)). Notably most of this previous literature on RPS policies and the deployment of renewable electricity deployment implicitly relies on a staggered adoption empirical design with state and year fixed effects in an attempt to derive credible estimates.[4] As we argue below, the assumptions necessary to lend a causal interpretation

[3]Depending on the measure considered, the stringency of RPS requirements increased by 22% and 38% between 2015 and 2019.

[4]Some exceptions are Upton and Snyder (2017) who use the synthetic control method,

standard TWFE (DD) estimates from this previous literature are not valid in the setting RPS policy adoption.

Recent work by Hollingsworth and Rudik (2019) and Feldman and Levinson (2023) estimate the impact of RPSs on renewables deployment using an empirical framework that accounts for interstate sales of electricity via wholesale electricity markets. For each megawatt hour of electricity it generates, a renewable source creates one Renewable Energy Credit (REC). To achieve compliance with RPSs, utilities can purchase electricity from a renewable source (and its associated RECs) or directly purchase RECs which are sold separately from underlying electricity (commonly referred to as "unbundled" RECs). Because of interstate sales of RECs, RPSs may incentivize investments in renewables outside of the regulated state. Feldman and Levinson (2023) explicitly account for interstate trade by using states' net total in-state and out-of-state demand for RECs following the implementation of an RPS. Using an instrumental variables framework, Feldman and Levinson (2023) find that RPSs have an ambiguous impact on renewables investments.[5] As we discuss below, since renewables investments take time to occur, it is important to consider both interstate demand for RECs and dynamic effects when studying the impact of RPS policies.

Several recent papers also study the effects of RPS policies on electricity prices, emissions, and renewables deployment using analytical general equilibrium models (Bento, Garg and Kaffine (2018), Fullerton and Ta (2022)). While these papers generally conclude that more stringent RPS policies unambiguously increase the price of electricity, they

Greenstone and Nath (2023) who use an estimator proposed by Sun and Abraham (2020) as a robustness check, and Feldman and Levinson (2023) who use an instrumental variables approach.

[5]Finding an excludable instrument is difficult in this setting because it requires exogenous variation in the stringency of an RPS that is uncorrelated with state-specific, time-varying unobservables. Feldman and Levinson (2023) use a number of instruments in their analysis including (among other variables) out-of-state supply of RECs, sector-specific gross domestic product, and an indicator for which political party controls the state legislature.

have ambiguous predictions for the effect of RPSs on renewables deployment.[6] Fullerton and Ta (2022) show that the effect on renewable capacity investments depends on state-specific transmission costs and natural resource endowments. For example, states with larger intermittent resource endowments (such as high wind class) may actually reduce renewable generation by increasing RPS stringency because the policy reduces demand for all electricity through higher retail prices. Thus a detailed empirical analysis is necessary to resolve the theoretical ambiguity.

We contribute to the empirical literature estimating the impacts RPS policies in four key ways. First, we bring in recent data on renewable capacity investments up to 2019 while other papers only consider the 2000s and the mid-2010s. The latter 2010s period (i.e., past 2015) is critical to properly measure the impact of RPS policies on renewables, since as Figure 1 shows, the net RPS requirement at the average state in the U.S. doubled between 2015 and 2019, reflecting dramatic increases in each state's standard. Thus, the post 2015 period meaningfully affects the impact of RPSs on renewable generation because it is a period where the intensity of each state's policy dramatically increases. Notably, the recent studies by Greenstone and Nath (2023) and Fullerton and Ta (2022) only consider data up to 2015 and so their estimates are likely biased downwards as a result.

Second, unlike the recent literature, we analyze the impact of RPS policies on wind and solar separately. This distinction is important since pooling the analysis across solar and wind generation essentially confounds the marked differences in declining cost trends and innovation across solar and wind renewables in the U.S. (Wiser et al 2022; Bolinger, Seel, Warner, and Robson 2022). Our results clearly shows differential impacts of RPS

[6] Using a coarsened exact matching algorithm, Wolverton, Shadbegian and Gray (2022) show that plants in RPS states faced electricity prices that were 2% higher than comparable plants in non-RPS states.

policies on wind and solar investments.[7] Third, our analysis provides new insights by documenting the dynamic impacts of RPS policies in the longer-term, up to 11 years after policy implementation, which is made possible by our newly compiled data sets and longer time framework. This consideration is particularly important because most installations of utility-scale solar generating capacity have occurred since 2010 (see Figure 3 below) and most of the previous research has studied the impacts of RPS over the 2000s and early 2010s periods. Fourth, our paper leverages the new Callaway and Sant'Anna (2021) estimator that is robust to treatment effect heterogeneity in presence of staggered treatment adoption. Treatment effect heterogeneity is an innate feature of RPS programs due to differences in policy design and underlying renewable resource endowments in each state. An additional consideration that emerges from the recent econometric literature is that the standard TWFE/DD estimator may provide a biased estimate of the average treatment effect on the treated in presence of treatment heterogeneity. This is a critical concern in this setting since virtually all the previous literature uses DD methods, which calls into question the validity of the resulting empirical evidence.

The rest of this paper is organized as follows. Section 2.2 provides background on RPS policies and their implementation in the U.S. since 1991. Section 2.3 describes the data used in our analysis. Section 2.4 presents the empirical strategy and section 2.5 describes our results. Finally, section 2.6 concludes.

2.2 Details on RPS Programs in the United States

RPS requires retail electricity suppliers to provide a minimum percentage or amount of their retail load using eligible renewable electricity generation sources. Although RPS

[7] Feldman and Levinson (2023) estimate the effects of RPSs on wind and solar investments separately, but do not estimate dynamic effects which are important for understanding changes in investment behavior.

policies exist in 30 states and the District of Columbia as of 2021, their design differs significantly across states. Most significantly, minimum percentages or "targets" differ both in magnitudes and time frames across states. Furthermore, states differ in their eligibility requirements for existing renewable generation sources, exemptions for publicly owned utilities, enforcement mechanisms, incentives for specific renewable generation technologies, and compliance tracking systems. In the U.S., RPS policies apply to 58% of total retail electricity sales as of 2021 (Barbose (2021)).

Figure B.1 shows the history of RPS adoption over time. While Iowa became the first state to adopt a mandatory RPS in 1991, most RPS states implemented their programs between 2000 and 2009. Typically, each state's annual percentage requirement increases gradually over time until it reaches its mandated goal. For example, California's RPS mandates that 60% of retail electricity sales come from renewable generation sources by 2030 and has interim targets of 44% by 2024 and 52% by 2027 (DSIRE (2021)). These time-varying targets within adopting states underscore the importance of examining the dynamic effects of the policy. Figure B.2 plots the mean, 95th percentile, and 5th percentile of observed statutory RPS targets across all RPS states in the U.S. between 2000 and 2019. The statutory RPS targets have increased over time as more states adopt RPS policies and update existing legislation. While the average RPS target in 2000 was near 0 percent, the average statutory RPS target in the U.S. exceeded 20 percent in 2019. Although the RPS percentage requirement for each state may appear stringent, the effective standard may be much lower because some states allow existing renewable generation to qualify for compliance. For example, although California's standard was 20% of total retail electricity sales in 2010, it's effective standard was approximately 17% of sales after accounting for eligible existing generation. Such variation in how "constraining" RPS mandates are may introduce lags between the time a policy is first adopted and the time detectable impacts on renewable investments incentivized by the policy are made. Consequently, we focus on estimating the causal effect of implementing any RPS legislation on renewables deployment using a method that accounts for potential dynamic impacts, and also consider variation in standard stringency in a robustness analysis.

While all states with RPS policies mandate that a share of retail electricity sales come from renewable generation sources, they often differ in what sources are considered renewable. The list of designated technologies always includes wind and solar electricity generation, but often states differ in their classification of sources such as hydroelectric and nuclear generation as renewable. Furthermore, some states such as California exempt publicly owned utilities from the RPS standard, while others such as Colorado set separate, lower standards for publicly owned utilities.

States further differ in how the RPS policy encourages renewable development. Some states mandate that a certain percentage of the renewable generation used to comply with the RPS policy come from specific technologies. For example, Delaware's solar carve-out currently stipulates that solar generating sources comprise at least 2.25% of renewable generation used for RPS compliance. Additionally, some states such as Delaware enforce RPS policies by charging a fee (typically termed an 'Alternative Compliance Payment') for each unit of renewable generation that would be required to bring a utility into compliance with the standard. Other states such as California allow regulators to levy financial penalties on non-compliant utilities.

Most states monitor compliance with RPS policies using Renewable Energy Credits (RECs) which certify that a given unit of electricity qualifies to meet the standard. Typically, RECs are issued by regional authorities that encompass multiple states and issue a unique serial number for every megawatt-hour of generation produced by registered compliant generators. While some trading of RECs may occur across regions, most RECs used for RPS compliance occurs within a region. We exploit this fact to explicitly model spillovers in wind and solar capacity additions from RPS-adopting states to non-RPS states using an approach similar to Hollingsworth and Rudik (2019).

As this brief overview highlights, RPS policies may appear straightforward, but in practice there is a large degree of heterogeneity across states in how they are implemented. This complexity requires sophisticated econometric methods in order to identify causal effects, as we demonstrate below.

2.3 Data and Preliminary Analysis

In order to estimate the impact RPS policies on the deployment of utility-scale renewable electricity generation installations, we compile state-level panel data set on the relevant outcomes, policy variables, and predictors of renewable investments (Table 2.5). While many of the underlying data are recorded at the sub-state level (e.g., the county where wind turbines are located), we organize all the data at the state-level given that RPS policies are implemented by states. This section describes the data sources and presents summary statistics and preliminary analyses.

Figure B.1 illustrates the timing of RPS policy adoption across states, focusing on the continental U.S. using data from Barbose (2021). This adoption will constitute the primary treatment indicator we consider in the empirical analysis. Each box represents a year, and are marked in gray once a state adopts the policy. For example, Alabama has yet to adopt an RPS policy, while Arizona enacted it in 2002. By the end of 2019, 27 states had enacted RPS policies, with Iowa being the earliest adopter (1992) and Vermont being the latest (2015). Since no state has disadopted these policies during our sample period, there is a large degree of autocorrelation in the 'treatment status', which we address using cluster-robust inference in the empirical analysis.

Data on operating capacity by source is obtained from the Energy Information Administration (EIA) Form 860, which contains generator-level information at electric power plants with at least 1 MW of combined nameplate capacity. For this study, we use information on installed capacity in wind and solar, which we complement with the same information for coal or gas units (all recorded in MW). Importantly, Form 860 includes information on all operable generators in a given year, as well as the list of retired generators (along with their year of retirement). For operable and retired generators we observe the first year of operation, which allows to reconstruct a complete history of the total cumulative installed capacity (henceforth 'installed capacity') over time, by source (wind, solar, coal, and gas) from 1990-2019.

Figure B.3 reports the national trends in installed utility-scale wind and solar electricity capacity. The deployment of capacity for both renewable resources follows a similar pattern, with wind installations beginning to emerge in the early 2000s, while utility-scale solar takes off around 2010. Growth in capacity appears roughly linear, reaching 100,000 MW for wind and 38,000 MW for solar by the end of the sample period in 2019. Many factors have contributed to the diffusion of these renewable technologies in addition to RPS policies, including reduction in levelized costs of operation, and federal and state-level production tax credits and other localized incentives (Hitaj 2013). The econometric methods detailed below are designed to control for the influence of those other factors.

We also analyze the impact of RPS adoption on actual generation of electricity by source. Data on generation are obtained from EIA Form 906 which reports annual data on generation at the power plant level. Other auxiliary data sources are described in the Data Appendix. Table 3.1 presents summary statistics tabulated for the 29 states that adopted an RPS policy during the period 1990-2019 and the 11 states that never adopted RPS. Columns (1) and (2) report sample averages while Column (3) reports the RPS state minus non-RPS state difference in means, with stars indicating statistical significance testing the null hypothesis of "no difference" based on an OLS linear regression with standard errors clustered by state. Panel A shows that on average, RPS states have marginally better infrastructure and wind speed endowments, with an additional 0.03 km of transmission per square km of state area, and average wind speed that is 0.3 meter per second higher. Solar irradiance, measured in kWh per square meter per year is a measure of total energy received from sun and a key determinant of solar electricity potential. The data in Table 3.1 indicates solar irradiance is weakly smaller in RPS states. The small magnitude of the differences reported in Panel A and the lack of statistically significant differences indicate that natural resource endowments do no appear to be a key driver of RPS policy enactment.

Panel B shows (as expected) that RPS states have higher levels of wind and solar capacity installed, on average, than non-RPS states, although the difference is only statistically significant for wind capacity. On average over 1990-2019, total installed wind capacity is 553 MW in states that ever-adopted an RPS. during that period, compared to 192 MW for states that never adopted the policy. At the same time, we note that coal and gas capacity is lower in states that adopt RPS policies. These differences in capacity by source are mirrored in the average generation by source in Panel C. RPS states produce more renewable electricity and less fossil-fueled electricity on average over 1990-2019, but none of the differences are statistically significant.

Panel D reports sample averages for various potential predictors of investments in renewables, including state-level GDP per capita, state-level electricity price and consumption, and League of Conservation Voters (LCV) scores for each state's senator and house of representative members.[8] This correlational analysis reveals marked differences between states adopting RPSs and states never adopting them. GDP per capita is notably higher in RPS states. Electricity prices are also higher in RPS states, by $0.03

[8]We obtain annual LCV scores between 1993 and 2013 for each state from Hollingsworth and Rudik (2019) and annual scores between 2014 and 2019 directly from the LCV website. The LCV describes its scoring methodology in the following way: "Annual scores are based on a scale of 0 to 100 and calculated by dividing the number of pro-environment votes cast by the total number of votes scored except for excused absences."

per kWh on average, as is total electricity consumption.[9]. Not surprisingly, RPS are more likely to be adopted in states that score higher in the LCV score index; the RPS - non-RPS difference is roughly 30 points and statistically significant. RPS states also have a higher fraction of counties that are designated 'Non-Attainment' for one or more criteria air pollutant. Finally, RPS compliance is also listed as 94% for RPS states. Since states that ever adopt RPS legislation differ from those that never adopt on a number of important observable margins, our preferred estimates will use not-yet-treated states (as opposed to never treated) as a control group. We test the sensitivity of our estimates to this choice in the robustness analysis.

2.4 Empirical Approach

2.4.1 Estimating Impact of RPSs with Staggered Adoption and Treatment Effect Heterogeneity

The primary goal of this paper is to estimate the causal effect of RPS policies on the deployment of renewable electricity capacity investments and generation using a staggered adoption research design. In order to estimate the impact of RPS policies on the various outcomes of interest, the previous literature has typically used a difference-in-differences design with a two way fixed effects (TWFE) estimator with state and year fixed effects (Yin and Powers (2010), Shrimali et al. (2015), Hollingsworth and Rudik (2019), and Greenstone and Nath (2023)). The canonical regression equation for such models is:

$$y_{it} = \beta RPS_{it} + X_{it}'\theta + \gamma_i + \delta_t + \varepsilon_{it} \tag{2.1}$$

Where in the context our study, y_{it} denotes utility-scale wind or solar electric capacity

[9]Prices are in adjusted to 2019 dollars and represent the electricity price for all end use sectors

installed (or generation) in state i at year t, RPS_{it} is a binary variable taking a value of one for all years following RPS implementation, and X_{it} is a vector of state-specific time varying control variables. The state fixed effects (γ_i) capture time-invariant characteristics of each state, such as underlying wind class, that determine renewable capacity installations and correlate with the probability that each state implements an RPS policy. Similarly, the year fixed effects (δ_t) control for annual shocks that are common to all states, and may be correlated with both renewable capacity installations and the probability of implementing an RPS policy. For example, the year fixed effects account for changes in the federal production tax credit, helping us to isolate the causal impact of RPS policies alone on the deployment of renewable electricity generation. The coefficient of interest, β is the average treatment effect on the treated (ATT) of an RPS policy on the outcomes (utility-scale wind and solar capacity and generation).

OLS estimation of equation (1) is straightforward. However, recent advances in econometric research show that, in the presence of treatment effect heterogeneity (i.e., where β can vary over time or across cross-sectional units), the standard TWFE estimator identifies a weighted average of group-time specific treatment effects which may not correspond to the overall ATT (Sun and Abraham (2020), de Chaisemartin and D'Haultfœuille (2020), Borusyak and Jaravel (2017), Goodman-Bacon (2021)). As explained earlier, due to important differences in RPS policy design across states and advancement in renewable generation technologies over time, it is reasonable to expect sizable treatment effect heterogeneity in this setting. For example, California's initial RPS target was 11.85% for all utilities while Missouri's was 2% and included a carve out for solar electricity generation.

In order to address the issues with the TWFE estimator, we use the estimator proposed by Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021) to estimate the impact of RPS policies. While researchers have proposed several different estimators which are robust to treatment effect heterogeneity, the Callaway and Sant'Anna (2021) estimator is well suited to staggered adoption research designs with a binary treatment indicator as in our setting.[10] Furthermore, we employ the estimator proposed by Callaway and Sant'Anna (2021) because it provides a flexible framework for aggregating group-time specific treatment effect parameters into dynamic treatment effects.

This estimator requires defining "adoption cohorts" which are groups of units that become treated at the same time. Due to limited overlap in RPS implementation years across adopting states (Figure B.1), we define adoption cohorts using 3 year windows, so that all states implementing RPSs between 1998-2000, 2001-2003, 2004-2006, and 2007-2009 are assigned to the same cohort. [11] In our setting, the estimator computes the treatment effect for each RPS adoption cohort by differencing each cohort's outcomes in a post implementation year t with its outcome in the year prior to implementation (akin to the pre/post difference for the treatment group in standard DD estimation), and then computing the same difference for a control group that is not treated as of year t (akin to the pre/post difference for the control group in standard DD estimation). For example, $ATT_{g,t}$ denotes the average treatment effect on the treated for all states that implemented a RPS policy in year g at post-treatment time t relative to the year before treatment, $g-1$. Adoption cohort-specific control groups are constructed by estimating a propensity score for each untreated state using baseline covariate values. The set of possible comparison groups for cohort g could be all of the states that never adopt an RPS policy during the sample period or the set of states that have not yet adopted a RPS policy at year g. We choose to use the set of *not yet treated* states to construct the control groups in the preferred analysis because (as documented in Table 3.1) ever

[10] See Sun and Abraham (2020), Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020), Strezhnev (2018), Ben-Michael, Feller and Rothstein (2021), Imai, Kim and Wang (2019), Borusyak and Jaravel (2017) for other proposed estimators.

[11] Due to our estimation procedure requiring 9 years of pre-RPS adoption observations, we cannot estimate treatment effects for the state of Vermont, the only state to adopt an RPS in 2010 or after.

treated and never treated states differ on a number of relevant characteristics. However, estimates using the never treated comparison group are similar to our main results, as shown in Table **??**.

Estimation of the $ATT_{g,t}$ parameters in our setting relies on four assumptions. First, the data structure must be a panel or a repeated-cross section of states. Second, conditional common trends holds between the treated and not-yet-treated groups, conditional on covariates. Third, treatment follows a staggered adoption design (e.g., the treatment is binary, and never reverts back from "1" to "0"). Fourth, there is some overlap on baseline covariates between the treatment and control groups. Assumptions 1 and 3 are trivially satisfied in our setting since our sample consists of a balanced panel of states from 1990 to 2019 and we treat each RPS policy as irreversible. While assumption 2 is impossible to formally test since it involves unobserved counterfactuals, we provide evidence that it is plausible by estimating pre-treatment period event study coefficients. Finally, to address assumption 4, we use the outcome regression estimand proposed by Callaway and Sant'Anna (2021) because there is limited covariate overlap between RPS states and their not-yet-treated counterparts, leading to imprecise inference procedures when using inverse probability weighting and doubly robust estimators (Khan and Tamer (2010)).

We control for state-level endowment characteristics which previous research has suggested influence renewables deployment such as: wind potential (wind speed), solar irradiance, and total length of electricity transmission lines. Wind potential and solar irradiance capture a state's latent potential for renewable electricity generation, while the length of transmission lines measures potential grid access for renewable generation sources. Furthermore, we control for a set of time-varying state level socioeconomic characteristics including gross domestic product (GDP) per capita, House and Senate League of Conservation Voting (LCV) scores, and electricity price per kilowatt hour of electricity in 1990 (the first year in our sample). The House and Senate LCV scores rank representatives and senators based on their environmental voting record. We use these variables to capture the underlying degree of pro-environmental attitudes in each state.

To conduct inference and compute standard errors, we use the multiplier bootstrap procedure described in Callaway and Sant'Anna (2021) which constructs simultaneous confidence intervals for the $ATT_{g,t}$ parameters. We cluster standard errors at the state level to allow for correlation in renewable capacity adoption within each state over time. Since some adoption cohorts are small, we group states that adopt RPS policies into 3-year bins corresponding to years 1998-2000, 2001-2003, 2004-2006, and 2007-2009.

To facilitate the interpretation of our results, we summarize the $ATT_{g,t}$ parameters in 4 ways using the did R package from Callaway and Sant'Anna (2021). The parameter 'Overall ATT (cohort)' correspond to the average effect of RPS policies experienced by all states that ever implement an RPS.[12] Similarly, we report the 'Overall ATT (year)' parameter, which corresponds to the average effect of implementing an RPS policy for states that have implemented an RPS for at least 11 years. This parameter first averages the heterogeneous effect of RPS policies across adoption-cohort groups within each time period for those states that we observe at least 11 years of post-implementation data, before averaging these parameters across time periods.[13] The last two average treatment effect parameters are the same as Overall ATT (year), except that they are computed separately for post-implementation years 1-5 and 6-11 respectively. This provides a simple metric to gauge dynamic effects of RPS policies on renewable capacity investments and

[12]Callaway and Sant'Anna (2021) recommend computing an overall ATT by first averaging the adoption-cohort, time specific treatment effects $\beta_{g,t}$ across post-implementation time periods for each cohort and then averaging the adoption cohort specific treatment effects.

[13]We chose to balance the panel of states when estimating the dynamic treatment effects because it prevents the treatment effect from being driven by changes in the composition of the RPS ever-adoption group over time Callaway and Sant'Anna (2021). This results in Iowa and Vermont (two RPS states) to drop from the main estimation sample. We evaluate the robustness of the results to this sample construction choice below.

generation.

2.4.2 Estimating the Impact of RPS Intensity

Since the binary treatment indicator ignores differences in RPS targets across states, we construct a continuous measure of treatment intensity measure following Feldman and Levinson (2023), Greenstone and Nath (2023), and Hollingsworth and Rudik (2019). Recall that utilities can comply with RPSs by generating renewable electricity themselves (creating RECs), purchasing renewable electricity (and associated RECs) from suppliers, or purchasing RECs that are unbundled from their underlying renewable electricity. Feldman and Levinson (2023) measure RPS intensity by calculating the total demand for RECs in each state. Since utilities can (in many cases) purchase RECs from out-of-state suppliers, total demand for RECs is composed of in-state and out-of-state demand.

Net in-state demand is the gross statutory RPS requirement less eligible renewable generation produced in the year before an RPS was passed.

$$\textit{Net-RPS}_{it} = max(0, RPS_{it} - EligibleRenewables_{i,\tau i-1})$$

where the subscript i denotes a state, t a year, and τi is the year of RPS passage in state i. Since states cannot demand negative quantities of RECs, we assume that in-state demand for RECs is zero whenever states' eligible renewable generation in the year prior to policy enactment exceeds the RPS requirement.

Net out-of-state demand for RECs is the sum of the RPS goal in the states where state i can sell RECs to less those states' contemporaneous renewables generation.

$$\textit{Net-Out-of-State-REC-Demand}_{it} = \sum_{j \in TP_i} max(0, RPS_{jt} - Renewables_{jt})$$

where TP_i is the set of states to which state i is permitted to sell RECs. To identify TP_i for each state we use data on REC trading networks from Hollingsworth and Rudik (2019).[14] To calculate each state's total net REC demand, we then add its net in-state demand to its net out-of-state demand for RECs.

$$\textit{Total-Net-REC-Demand}_{it} = \textit{Net-RPS}_{it} + \textit{Net-Out-of-State-REC-Demand}_{it}$$

$\textit{Total-Net-REC-Demand}_{it}$ reflects state i's demand for RECs net of existing renewable generation from within its own borders and from states to which it can sell RECs. Since the estimator proposed by Callaway and Sant'Anna (2021) is specific to staggered adoption settings for a binary treatment, we create a discrete treatment indicator equal to one in all periods after $\textit{Total-Net-REC-Demand}_{it}$ exceeds its sample average level. Defining treatment in this way means that there could be anticipatory treatment effects if renewable generation capacity responds to below-average levels of $\textit{Total-Net-REC-Demand}_{it}$. We also consider an alternative treatment definition where we create a discrete treatment indicator equal to one in all periods after the first time that $\textit{Total-Net-REC-Demand}_{it}$ is positive.

2.5 Results: Impact of RPS Policies

2.5.1 Wind Capacity and Generation

The empirical analysis begins by analyzing the impact of RPS implementation on wind outcomes. Table B.2 reports the results for installed wind capacity (Panel A)

[14] Since Hollingsworth and Rudik (2019) collect data on REC trading networks between 1993 and 2016, we assume that these trading networks did not change from 1990 to 1993 and 2017 to 2019.

and wind generation (Panel B). The estimates in column (1) include no additional controls (besides the adoption cohort and year fixed effects implicitly accounted for by the Callaway and Sant'Anna (2021) estimator). Column (2) adds the 'natural endowments' controls (wind potential, solar irradiance, and total length of transmission lines in the state, see Table 3.1 for details), and column (3) adds the 'socioeconomic' controls (GDP per capita, House and Senate League of Conservation Voting scores, and the price per kilowatt hour of electricity in 1990). Standard errors for all estimates are computed using a multiplier bootstrap method with clustering at the state level (Callaway and Sant'Anna (2021), Kline and Santos (2012), Belloni et al. (2017), Chernozhukov, Fernández-Val and Luo (2018)).

The preferred estimates in column (3) indicate that, on average, implementing an RPS policy increases installed wind capacity by 586 MW on average, across all states that ever adopted an RPS at any point during our sample period (Overall ATT (cohort)). This is a large effect, corresponding to 21% of the average installed wind capacity in 2019 among RPS states. Overall, across the estimates in columns (1) to (3), the size effect of the estimated ATT of RPS policy implementation ranges from 10% to 21%. Converting our preferred estimates to reflect the average percentage point change in the share of total capacity or generation attributable to wind resulting from a 1 percentage point increase in the RPS target implies that the share of capacity increase by 0.33 percentage point.[15] The average impact of RPSs for the group of states for which we have at least 11 years of pre and post-implementation data (Overall ATT (year)) is of similar magnitude, implying that RPS policies lead to 714 MW in capacity additions. Decomposing the effect by post-implementation event time suggests that most of the increase in wind capacity investment occurs 6-11 years after RPS implementation (1,000 MW on average), as opposed to 197

[15] These results are consistent with estimates from Yin and Powers (2010) and Shrimali et al. (2015) who find that the share of electricity generated by renewables increases by 0.6 and 0.3 percentage points respectively.

MW in years 1-5. While all these estimates are positive, indicating that RPS policies were important contributor to the development of in-state wind electricity installation, it should be noted that the statistical significance is sensitive to the chosen specification, with the column (1) and column (3) estimates being statistically different from 0 at the 5% significance level, while those in column (2) are not.

The results for annual wind generation are shown in Panel B and are generally mirror those for installed capacity. The Overall ATT (cohort) estimate is column (3) is 3,110 GWh while Overall ATT (year) is 3,710 GWh. The impact again is larger for years 6-11 after RPS implementation (compared to years 1-5), implying that, on average, RPS states increase annual wind generation by 5,350 GWh relative to their not-yet-treated counterparts. This effect corresponds to 136% of the mean wind generation and even 9% of mean coal generation among ever-adopting RPS states in our sample, again underscoring the importance of RPSs as drivers of renewables deployment. The statistical significance of the estimates of the impact of RPS policies on wind generation follows a similar pattern as those for wind capacity. The estimates in column (3), with the full set of natural endowments and socioeconomic controls are generally statistically significant the at the 5% level, while the column (2) estimates are qualitatively similar, but imprecisely estimated.

Figures B.4 and B.5 present the unconditional dynamic treatment effects for wind capacity investments and generation, respectively. Each point represents an event time-specific treatment effect which has been computed by averaging the group-time specific effects across adoption cohort groups, following the approach in Callaway and Sant'Anna (2021). Again, these are average effects for the subset of states for which we have 11 years of pre- and post-implementation data. We color-code the point estimates to reflect the pre-RPS adoption period (gray) and post RPS adoption period (orange). The corresponding 95% confidence intervals are represented by the length of the tickers. The pre-RPS adoption treatment effect estimates to the left of 0 on the horizontal axis are small and provide supporting evidence for the parallel trends assumption for wind capacity investments across the treated and not-yet-treated groups. In the case of wind generation (Figure B.5), the pre-RPS adoption estimates also support the parallel trend assumption. The combined evidence in Figures B.4 and B.5 indicate that the estimates of the impact of RPS policies in Table B.2 can be interpreted as credible estimates of the ATT of the policy.

The post-RPS adoption treatment effect estimates confirm the results in Table B.2: RPS policies cause wind capacity investments and generation to increase in the post-policy adoption period.[16] All the post-adoption point estimates are statistically significant at the conventional level for wind capacity investments, and 8 out of 11 are for wind electricity generation. In terms of dynamics, the treatment effects appear to grow roughly linearly with post-adoption time. Importantly, through the 11 years of post-adoption data we have, the estimated impact of RPS on capacity investments and generation show no sign of reverting back to a null effect. This indicates that RPS policies created long-lasting change to the electricity sector of the states adopting them.

2.5.2 Solar Capacity and Generation

Next, we examine how RPS legislation has impacted solar capacity and generation. Table B.3 is configured as Table B.2 and presents the ATT estimates for solar capacity and generation in panels A and B respectively. While most of the estimates of the impact of RPS policies on solar capacity investments and generation are positive (as expected), they are smaller than their wind counterparts and lack statistical precision. The preferred estimates in column (3) imply that, on average, implementing an RPS increases

[16]Note that the Overall ATT (year) estimates in Table B.2 are just a weighted average of the event-time estimates from Figures B.4 and B.5.

solar capacity by 28 MW in ever-adopting RPS states. The overall ATT (year) estimate similarly implies that wind capacity increases by 44 MW following RPS implementation. While statistically insignificant, the estimated impact of RPS on solar electricity generation range between 87 and 117 GWh. As is the case with wind energy, much of the estimated RPS impacts on solar capacity additions and generation occur between 6 and 11 years after RPS implementation. The estimated standard errors for all estimates in column (3) are large relative to the ATT estimates such that the 95 % confidence intervals for the RPS impact on solar energy all include zero.

Figures B.6 and B.7 display the estimated dynamic treatment effects for solar capacity and generation respectively, computed in the same way as its counterpart in Figures B.4 and B.5. The pre-implementation estimates provide suggestive evidence that the parallel trends assumption holds between treated and not-yet-treated states. Furthermore, the post-implementation estimates (shown in orange) are small and indistinguishable from zero, confirming the results in Table B.3.

The estimate for generation in column 3 of Table B.3 implies that solar generation increased by 18% relative to the mean level of solar generation for ever-adopting states, although the lack of precision makes this conclusion tenuous. One likely explanation for the imprecise estimates of the effect on solar capacity and generation is that growth in solar capacity investment was limited prior to 2010. Figure B.3 shows that while most of the wind capacity investment in the U.S. has occurred since 2000, similar increases in solar capacity investment did not meaningfully accumulate until 2010. This is consistent with evidence from Wiser, Barbose and Holt (2010) who suggest that wind generation proved more economically attractive and lower risk than solar in many regions of the U.S., leading to earlier investment in wind. For context, most solar electricity generation farms had installed capacity of 5 MW or less as of 2019 in the U.S.[17]

[17]https://www.eia.gov/todayinenergy/detail.php?id=38272

2.5.3 Robustness

We check the robustness of our estimates to a number of additional model specifications in Table **??**. The rows alternate between installed capacity (MW) and annual generation (GWh). All specifications control for both the natural endowment and socioeconomic covariates. Rows 1 and 2 replicate our preferred estimates of ATT (Year) from Tables 2 and 3, while all other rows depart from the baseline specification in one of five possible ways: whether the panel of treated states is balanced for 11 pre- and post-treatment periods, whether the control group is the set of never treated states or not yet treated group (as in the preferred estimates specification of Tables B.2 and B.3), and finally modelling the RPS policy as a binary indicator for RPS adoption (legislation) or as a binary indicator for RPS intensity being above the sample average. Rows 3 and 5 replicate our preferred specification using the control group of states that have never adopted an RPS policy with installed capacity and generation as outcomes, respectively. The results for wind using the never-treated group as the control are slightly larger than our preferred estimates, and remain statistically significant at the 95% confidence level. For solar energy, the estimates are virtually identical using the never-treated and not-yet-treated groups as control groups and the results remain statistically insignificant.

Rows 4 and 6 replicate the preferred estimates using an unbalanced panel of treated states rather than the subset of treated states for whom we observe 11 years of pre- and post-treatment data. Using an unbalanced panel substantially increases the estimated treatment effect for both the capacity and generation outcomes. Consistent with the prior evidence, only the ATT estimates for wind are statistically significant at the 5% level. Callaway and Sant'Anna (2021) note that the estimates using the unbalanced panel of treatment units should be interpreted with caution since they may be driven by changes in the composition of treated units over time. For this reason, we find it reassuring that the unbalanced panel results are qualitatively similar to our preferred estimates based on the balanced panel.

Rows 7 through 10 use the treatment variable defined in section 2.4.2 which accounts for each state's net in-state and out-of-state demand for RECs rather than the policy implementation date. In general these estimates are qualitatively similar to the preferred estimates in rows 1 and 2. In rows 7 and 8 treatment is a binary indicator equal to one for all years after a state's total net REC demand is positive for the first time. The estimates imply that, on average, solar capacity increases by 114 MW and wind capacity by 671 MW after states face positive net REC demand. For generation, the results suggest once total net REC demand is positive, solar generation increases by 369 GWh and increases wind generation by 3,480 GWh. Rows 9 and 10 define treatment using a binary indicator equal to one for all years after a state's total net REC demand exceeds its sample average. Consistent with the dynamic effects shown in figure B.4, we find effects that are larger in magnitude than those from our preferred specification. We find that after total net REC demand exceeds its average, wind capacity and generation increase by 1,120 MW and 5,620 GWh respectively. Similar to the results in Tables B.2 and B.3, the estimates for wind capacity and generation are more precise than those for solar capacity and generation.

In summary, our results suggest that, on average, RPSs have played an important role in spurring wind energy investments (and generation) across the United States. The evidence for a causal effect on solar sources is weaker and generally statistically insignificant. It takes time for RPSs to impact installed wind capacity, with most additions occurring five or more years after policy implementation. As pointed out by Hollingsworth and Rudik (2019) and Feldman and Levinson (2023), RPSs can influence renewables investments in states that can sell RECs to RPS states.

2.6 Discussion and Conclusion

Renewable portfolio standards are the most prominent policy lever to stimulate investments in renewable electricity in the United States. Despite their more than 30-year long history, RPSs remain controversial and debates continue to surround their efficacy in leading the low-carbon transition in the electricity sector. This paper provides a careful evaluation of the impact of RPSs on renewable electricity capacity investments and generation, using modern panel data econometric methods suited for the analysis of staggered policy adoption with heterogeneous effects and the most up-to-date data available. These considerations are critical as they overturn results from the recent literature evaluating the impacts of RPS programs.

The results of this study point to 3 ways by which RPS legislation have changed the composition of electricity generation in the U.S. First, RPS legislation dramatically increased wind capacity investments and generation and this increase persists up to eleven years after policy implementation. Second, dynamic responses to the policy, which had not been considered in the previous literature are important: RPS policies take time to affect renewable capacity installations and generation, with much of our estimated effect occurring 6-11 years after the policy's initial implementation. Third, we find no evidence that RPS legislation had any effect on solar capacity investment or generation. One caveat on this last finding is that due to the timing of utility-scale solar deployment in the U.S., our sample of data is not as well-suited to test the effect of RPSs on solar investments.

We can use our estimates to infer the contribution of RPS policies on total wind capacity installed (we ignore solar due to the small ATT estimates and the lack of statistically significant evidence). The estimated ATT of RPS on capacity 11 years post RPS implementation (relative to the year prior to implementation) is an increase of approximately 1000 MW (Table 2). Applying this estimate to the 29 states with RPS legislation as of 2019 implies that 29 GW, almost 30% of the current aggregate wind capacity is a result of RPS policies. While this is admittedly a simple and crude calculation, it nevertheless highlights the key role RPS played in developing the wind sector in the United States.

The empirical analysis also highlights the importance of explicitly accounting for the considerable heterogeneity in RPS legislation across states in empirical analyses. Amongst papers in the previous literature, our findings most closely resemble the results from Yin and Powers (2010) and Shrimali et al. (2015), both of which only find a positive effect on renewable generation after controlling for aspects that differ across states' RPS policies. This is a reassuring result that helps to reconcile the wide variety of prior estimates of RPS policies' impact on renewable generation. Our estimates also build on the prior literature by separately identifying RPS policies' effect on wind and solar generation. Despite evidence from Wiser, Barbose and Holt (2010) that wind generation was more economically feasible than solar in most regions of the U.S. prior to 2010, most prior research has grouped wind and solar generation together as an outcome.[18]

The U.S. and many other advanced economies are at a turning point where detailed and aggressive decarbonization plans are established. The Clean Energy Standard proposed by President Biden in 2021 shares many features with RPSs as they have been implemented by U.S. states since 1991. Taken together, the evidence presented in this paper indicates that a national Clean Energy Standard may promote investments in wind and solar production capacity and actual generation of renewable electricity. An important topic for future research is whether these investments will be sufficient for the energy

[18]Another working paper by Fullerton and Ta (2022), find no effect of RPS policies on generation from wind and solar power using the same estimator from Callaway and Sant'Anna (2021). One possible explanation for the discrepancy between our estimates and theirs is that they are not separately estimating the effect on wind and solar power.

sector to reach targets of zero emissions by 2035.

Chapter 3

Can the Low-Carbon Transition Energize Labor Markets? Evidence from Wind Electricity Investments in the U.S.

3.1 Introduction

Over the last decade, the prospects for 'green jobs' and 'green new deals' have become central to climate change and economic development policy debates worldwide, holding the joint promise of economic progress and environmental preservation. Most of these proposed policies are green energy transition policies that set targets for the decarbonization of the energy sector, particularly the electricity sector. For example, the Green New Deal resolution (H.Res.109) of 2019 calls for the production of "100 percent of the country's electricity from renewable and zero-emissions" sources through a "fair and just transition for all workers and communities."

Beyond policy debates in Washington D.C, the creation of a low-carbon energy sector alongside a suite of new green jobs has become a central objective of development strategies set forth by international agencies (OECD, World Bank, UNEP, Asian Development Bank), many countries, and even states and provinces (e.g., California's Senate Bill 100 mandates that renewable energy and zero-carbon resources supply 100% of electric retail sales to end-use customers by 2045). In practice, many proposed low-carbon energy policies are embedded in government programs that mandate and/or subsidize the development of biofuels, the weatherization of homes, and job training programs for workers to learn green job skills. The remarkable deployment of renewable electricity capital, including utility-scale and residential-scale wind and solar projects, is the most prominent example of such policy to date, having led to significant increases in renewable electricity generation capacity in China, Germany, and many other countries.

In the United States, a low-carbon energy transition is already underway. Deployment of wind turbines began significantly in the 1990s and now amounts to more than 55,000 turbines, located in more than 500 counties, for close to 100GW of total capacity. Likewise, utility-scale solar plants have grown rapidly in recent years, totaling more than 1,000 plants (including own-generation on industrial and commercial sites) and 32GW of capacity. The growth of this renewable generation capacity reflects a myriad of factors, including decreasing levelized costs, federal and state-level tax credit policies, renewable portfolio standards, and climatic drivers such as wind speeds and annual sunshine hours. As Figure 3.1 shows, the location of renewable capacity has been concentrated in specific regions of the United States, in particular, the Southeastern region has seen almost no investments in wind capacity.

An important motivation underlying low-carbon energy transition policies is the expectation that they will create a significant number of new, high-quality jobs in environmental industries: from solar and wind turbine installation and maintenance, to energy retrofitting, to pollution-control technicians, to environmental engineers. Notably, such jobs are typically held by medium to high-skill workers. At the same time, the expansion in the green job sectors comes at the cost of job displacement in the fossil-fuel extraction and production sectors. Such displacements may contribute to furthering economic inequality between higher-skill workers and lower-skill workers, which has increased rapidly in recent decades. Understanding the implications of renewable energy transition policies for labor market outcomes such as wages, job creation and destruction, and long-term employment, is of fundamental importance to inform the design optimal green energy transition policies.

In particular, this paper addresses two key related questions: (1) Can such policies create new and long-lasting labor market opportunities, and (2) What types of workers and industries will benefit from these policies? These questions are especially important given that green energy transition policies are typically justified on the basis of jointly reducing emissions in the energy supply sector while at the same time creating sustained employment opportunities.

To answer these questions, we estimate a Two Way Fixed Effects (TWFE) model that relates cumulative wind capacity to regional economic outcomes (including employment, monthly earnings, GDP, and per capita income) using an annual panel of counties observed between 2000-2019. To address concerns that the TWFE estimator may be biased in contexts with treatment effect heterogeneity (Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020), Borusyak, Jaravel and Spiess (2021), Callaway and Sant'Anna (2021), Sun and Abraham (2020)), we also estimate the relationship between regional economic conditions and wind generation investments using a matching estimator proposed by Callaway and Sant'Anna (2021) and a stacked difference-in-differences estimation strategy used by Deshpande and Li (2019), Cengiz et al. (2019), and Flynn and Smith (2022).

Our TWFE model estimates the relationship between employment and wind energy investments using detailed information from administrative datasets compiled by the U.S. Census Bureau and the Energy Information Administration (EIA). The Census' Quarterly Workforce Indicators (QWI) provide information on employment and average monthly compensation at the county-by-quarter of year level. Employment and compensation information are collected by states through administrative records sources such as social security and federal tax data, and shared with the federal government. The QWI also report employment and compensation by worker sex, age, educational attainment, race, and ethnicity at the county level, allowing us to examine which workers are most impacted by wind investments. Finally, the QWI reports labor market information by two digit NAICS industry, which we use to study which sectors of the economy are impacted by wind investments. The EIA reports data on every planned and operational electricity generation source in the U.S. that has nameplate capacity greater than 1 MW through its Form 860 database. For this project, we collected the location, year of initial operation, and nameplate capacity for all utility-scale wind generators.

The results of our study suggest that increasing county-level wind capacity by 1 GW positively impacts regional economic conditions, raising overall employment by 4.6%, average monthly earnings by 5.8%, GDP by 13%, and per capita income by 5.3%. While these effects seem quite large, a typical annual wind capacity addition is 0.1 GW, implying that the regional economic effects of a single wind installation are modest. In fact, the estimated elasticities of employment, earnings, GDP, and per capita income are 0.004, 0.005, 0.013, and 0.005 respectively for an average county with any wind investments during our sample period. While the regional economic effects are modest for an average county in our sample, we show that wind projects benefit regional economic conditions once counties have accumulated more than 0.2 GW of wind generation capacity and more than 10 years after a county's first wind installation. Taken together, these results suggest that wind investments modestly impact regional economies and such impacts take time to accumulate.

We also estimate how workers from different demographic categories are affected by wind projects through employment opportunities and income. Our estimates suggest that the labor market benefits of wind energy investments are borne by workers who identify as Male and either African American or White. We find the largest employment effect for African American workers (an increase of 18% resulting from a 1 GW change in wind capacity), although this estimate is imprecisely measured due to the relatively small number of individuals identifying with this racial category in counties with wind projects.

Finally, we show that much of the observed increase in employment and monthly earnings is concentrated in the construction and utilities sectors, suggesting that much of the overall labor market benefits are directly related to wind turbine construction and maintenance. This finding supports the BLS' occupational labor market projections which suggest that the "Wind Turbine Technicians" occupation will experience a 44% increase in employment between 2021 and 2031.[1]

Our research contributes to long-standing literature in environmental economics on the connection between energy markets (or regulations in energy markets) and labor markets. This literature is motivated in part by informing policymakers about the impact of regulations on labor markets outcomes, mostly on jobs or unemployment rates (e.g. Linn (2010), Martin et al. (2014), Yamazaki (2017), Yip (2018), Curtis (2018), Hafstead and Williams (2020)). Debates surrounding climate policy and carbon pricing have also prompted new research on the impact of changes in energy prices (in particular electricity prices) on employment and earnings (e.g. Deschenes (2011), Kahn and Mansur (2013),

[1] See here for more information. The BLS reports that there are 11,100 "Wind Turbine Technicians" in the U.S. as of 2021 and their employment is projected to increase by 4,900 workers by 2031.

Cox et al. (2014)). The literature on the relationship of green energy and labor markets is not as well developed.

Green energy transition policies involve projected changes in electricity generation mix that can achieve a given carbon target by a future date. As such, there is no concurrent data that can be examined to assess their impacts on labor markets. Instead, the literature concerned with employment effects of green electricity transitions in the in energy modelling research community has mostly relied on projections from project-level case studies of wind or solar plants or on input-output model estimates and extrapolations.

Aldieri et al. (2020) provides a recent review of the literature on the job effects of wind energy projects based on input-output and/or modelling tools. The authors observe a wide range of job effects, from 0.5 to 15 jobs created per MW installed, but overall conclude that "job creation (associated with wind projects) seems to be limited". A key advantage of the input-output method is that it can be readily implemented, with relatively small data requirements, to answer a host of important questions. At the same time, it is important to note the inherent limitations of input-output type modelling: Most prominently, input-output models rely on assumptions of constant return to scale and ignore the possibility of factor substitution and relative price adjustment.

While there is relatively large 'modelling' literature related to the questions of renewables and employment (Aldieri et al. (2020) reviews 20 published papers), the empirical counterpart literature is limited. To the best of our knowledge, only 2 published empirical studies (Brown et al. (2012) and Gilbert, Gagarin and Hoen (2023)) examine the employment effects of renewable project investments.

Brown et al. (2012) uses data on the placement of wind turbines from the National Renewable Energy Laboratory and county-level employment and per capita income from the Bureau of Economic Analysis. Using various cross-sectional regression methods, the authors find that increases in wind capacity lead to increases in both per capita income and employment. While it provides an important first step in empirically analyzing the connection between renewables and employment, there are several limitations to the study. First, it is now dated, with a sample period ending in 2008 and thus lacking more than a decade of growth in the wind electricity sector. Further, it only considers two labor market outcomes (employment and per capita income), so impacts at the worker characteristics and industry levels are not provided. Another limitation is that the analysis only considers the Great Plains region, which is not necessarily representative of labor markets in other areas of the US. Finally, the empirical methodology is not robust to the presence of unobserved local labor market shocks that are concurrent with the turbine installation projects.

Our analysis is closely related to recent work by Gilbert, Gagarin and Hoen (2023), who examine the relationship between wind capacity investments and labor market outcomes at the establishment and worker levels between 2000 and 2015. While Gilbert, Gagarin and Hoen (2023) focuses on the overall local labor market impacts of wind installations, this paper examines how wind projects change broader regional economies by estimating effects on county-level GDP and per capita income in addition to labor market outcomes. Furthermore, Figure 3.2 shows that roughly one-third of all wind capacity investments in the U.S. from 2000-2019 have occurred after 2015, suggesting that analyses excluding this period may exclude a meaningful period in renewable energy development in the U.S.. Finally, our analysis is the first to examine which workers are affected the most by wind capacity installations in the U.S., identifying the distributional consequences of wind installations.

This paper advances the existing literature in three ways. First, our analysis examines the impact of wind energy installations on employment over a longer time period than prior work, including the years 2015-2020 which accounts for 30% of the increase in wind generation capacity across the U.S. since 2000. The longer panel of data allows us to estimate long run effects of wind installations on county-level employment, supplementing prior work which focuses on shorter-term impacts of renewable energy investments on local economies.

Second, this analysis is the first to quantify the aggregate regional economic impact of wind energy projects by showing sizeable increases in GDP and per capita income following wind capacity additions. While prior work has used input-output modelling to quantify the impact of renewables investments on regional economic activity, this is the first study that does not rely on common assumptions of those models.

Finally, this paper provides evidence of the distributional and occupational impacts of wind energy investments in local economies, informing current policies to decarbonize the U.S. electrical grid. Although previous work has examined the overall labor market implications of wind energy development, none has studied how such development impacts the composition of local labor markets.

3.2 Data and Descriptive Statistics

The key variables analyzed in the paper are county-level employment rates and average monthly earnings combined with county-level installed wind electricity generation capacity over the period 2000-2019.

Employment and earnings data. We compile annual county-level data on employment and average monthly earnings from the Quarterly Workforce Indicators (QWI). The QWI is a job-level data set linking workers to their employers and derived from the Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee microdata. The sample frame covers about 95% of U.S. private sector jobs, and excludes self-employed workers. The tabular data provides county-level employment counts for various demographic groups (by gender, age, and education levels), as well as by industry up to the 4-digit level. Due to severe data disclosure limitations, we use 2-digit industry classifications in the analysis below. We also use monthly average earnings at the county level. In order to define an annual, county-level employment rate (employment over population ratio), we draw on the National Cancer Institute SEER database to obtain annual county-level population counts.

Since fourteen states opt out of reporting labor market data to the Census Bureau through its LEHD microdata at some point during our sample period, we have employment and earnings information by county, sector, and worker demographic characteristic for 38 states.[2] We assess whether this data restriction impacts our baseline results by collecting employment and earnings data at the county-year level from the Bureau of Labor Statistic's Quarterly Census of Employment and Wages (QCEW). Although the QCEW does not report labor market outcomes by worker demographic characteristics, it includes information from all 50 U.S. states during our sample period. We show that our estimates are the same whether we use the QWI or QCEW data source, suggesting that the data restrictions in the QWI do not meaningfully impact our estimates.

GDP and income per capita. To comprehensively measure the impact of wind generation investments on local economic conditions, we collect annual county-level GDP and income per capita from the Bureau of Economic Analysis (BEA). The BEA uses an income approach to compute county-level GDP by adding employee compensation, net taxes on production and imports (taxes less subsidies), and gross operating surplus. GDP is calculated with data from a number of public and private sources including the EIA, BLS, and other sources.[3] Per-capita income is largely based on administrative records from various governmental programs (for example, unemployment insurance and

[2] The 14 states are Alabama, Arkansas, Arizona, Kentucky, Massachussetts, Mississippi, New Hampshire, Wyoming, Ohio, Michigan, New York, Oklahoma, Vermont, and Wyoming.

[3] See the BEA's GDP methodology for more information.

Medicaid) and mid-year population counts from the Census Bureau.

Installed wind capacity. Data on installed operating wind electricity capacity are taken from the Energy Information Administration (EIA) Form 860, which contains generator-level information at electric power plants with at least 1 MW of combined nameplate capacity. The structure of the Form 860 database includes information on all operable generators in a given year, as well as the list of retired generators (along with their year of retirement). This information allows us to reconstruct the complete history of the total cumulative installed wind capacity over time. Since the EIA reports the date at which a specific wind turbine begins to generate electricity that is distributed on the grid (date of commercial operation) of wind, we adjust the date of operation by minus 2 years to include construction phase of wind turbine installations. Overall there are 55,000 wind turbines that operated between 2000 and 2019, part of 1,110 installations ('wind farms'), located in 421 counties.

Additional control variables. Since counties with wind electricity investments may meaningfully differ from counties that never receive wind investments, we control for annual trends in cumulative solar generation capacity, working-age population, non-attainment status under the Clean Air Act Amendments (CAAA), and scores measuring "pro-environmental" voting behavior by Congresspersons in each state. We collect annual trends in cumulative solar generation capacity from the same EIA form 860 database that we use to track wind generation capacity investments. As with wind generation, the Form 860 data report the date a utility-scale solar generator becomes operational.

Since the descriptive statistics reported in Table 3.1 suggest that wind generation investments are more likely to be made in more rural, less dense regions of the U.S., we also control for working age population at the county-level. Furthermore, controlling for county-level population also accounts for net migration in each county, which is likely correlated with variation in employment. We obtain population counts of all individuals between ages 20 and 69 for each county and year of 2000-2019 from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program.

A long literature documents the negative relationship between environmental regulation and employment at regulated facilities (Greenstone (2002),Walker (2013),Curtis (2018),Yamazaki (2017),Yip (2018)). Since a county's regulatory status may be correlated with investments in wind generation, we collect data on counties' regulatory status under the Clean Air Act Amendments from the EPA's Green Book database.

Finally, we collect data measuring each state Congressperson's voting behavior on environmental issues between 2000 and 2019 from the League of Conservation Voters (LCV). The LCV scores every Congressperson on a scale of 1 to 100 (100=most pro-environment) based on their voting behavior on high-profile environmental legislation.[4]

Sample restrictions. Using the QWI database, we can construct a balanced panel of 2,701 counties for which we have valid employment data over 2000-2019. We also defined a "preferred sample" by excluding counties with population density above 1000 persons per square kilometer, and excluding counties with average wind speed below 5.4 meters per second and normalized length of transmission lines below the 10th percentile of the distribution. The preferred sample of 2,334 counties is designed to avoid using counties that are highly urbanized or have too weak potential for wind generation as controls for the treated counties. In a robustness analysis we show that making this sample restriction has little impact on our estimates of the relationship between wind generation investments and local economic outcomes.

Descriptive Statistics. Figure 3.2 shows the trends in total installed wind capacity over time in the continental U.S., as well as annual additions to capacity. The steady deployment of wind electricity capacity is evident over the last 2 decades, with total wind capacity growing from 2 to 102 gigawatt (GW) between 2000 and 2019. This growth

[4]More information on the methodology used to compute LCV scorecards can be found at their website.

represents an average annual growth rate of 25% and reflects average annual additions of 5.3 GW, ranging from a low of 0.4 in 2004 and a high of 12.8 in 2012. As of 2019, 15% of the continental U.S. population resided in county with at least one wind installation.

Figure 3.1 displays the geographical distribution of wind farms in the U.S. as of 2019. Specifically, we show the installed capacity by county in 2019 across 6 levels: no installations (gray), and increasing shades of blue, where darker blue indicates larger capacity. The highest category, > 1000 MW, is among others observed in Kern county, CA, Gillian county, OR, Carson, Kenedy, Nolan, Scurry, and Taylor counties, TX, and Klickitat county, WA. Two other notable patterns are revealed in this map: the near absence of wind installation in the East South Central and Southern Atlantic regions (essentially all states south and eastern of Missouri), and the importance of wind installations in the Great Plains region (with the addition of counties in California, Iowa, and Illinois).

Employment in industries related to wind energy investments has increased over time, facilitating the dramatic increase in wind capacity across the U.S. between 2000 and 2019. Figure 3.3 plots national employment as reported in the BLS QCEW between 2001 and 2019 for 6 digit NAICS codes related to the manufacturing, construction, and operation of wind electricity generation facilities.[5] Figure 3.4 displays the average annual compensation per worker in 2019 dollars for industries related to wind energy generation. Employees engaged in the operation of wind energy installations earn around $108,000 per year, which is more than their counterparts engaged in manufacturing ($92,000) and

[5]Unfortunately, employment directly related to the manufacturing and construction of wind facilities was reported together with other types of employment for every sector except for "Wind Power Generation". Prior to 2011, the BLS reported wind power generation employment together with employment in the solar and tidal electricity generation industries under "Other Power Generation." The sector "Power System, Renewables Construction" includes workers involved in the construction of wind projects as well as workers involved in stringing power lines, building solar generation structures, constructing power plants, and other energy-related construction. "Turbine Manufacturing" includes wind, steam, hydraulic, and gas turbine manufacturing employment.

construction ($70,000). However, in real terms the gap between compensation in wind power construction, turbine manufacturing, and wind power generation has shrunk over time.

Figures 3.3 and 3.4 highlight an important limitation of this study. The analysis in this paper estimates the impact of wind energy investments in the surrounding local economy where the wind turbines are located. As a result, our estimates capture construction and operation employment associated with wind energy investments, but not the workers engaged in the manufacture of parts for wind turbines. Since this manufacturing employment occurs outside of the local economy where a wind turbine is located, we are unable to quantify how demand for wind turbine parts has reshaped local economies surrounding wind generation-associated manufacturing plants.

Table 3.1 presents some summary statistics, comparing counties that at any point between 2000 and 2019 had an operating wind installation ("ever-wind counties", in column 3) against counties that never had operating capacity ("never-wind counties", in column 4). For completeness we also report unconditional summary statistics for the full sample (column 1), and the preferred sample (column 2). The top panel of Table 3.1 shows the sample mean of various attributes of the renewable sector (installed capacity and its determinants), while the lower panel reports average socio-economic characteristics measured in 2000, at the onset of the deployment of wind electricity in the U.S.

Average wind capacity is 93 MW in the ever-wind counties, which reflects in part higher average wind speeds, compared to never-wind counties. Ever-wind counties also have more installed solar capacity than never-wind counties even if solar irradiance (a key predictor of solar electricity potential) is on average virtually the same in both groups.

The differences between counties with wind installations and those without are more pronounced when examining baseline characteristics in 2000. Ever-wind counties have notably lower population and are less densely populated, by a factor of 2 to 3. Per-capita income are similar, but wind projects are more likely to be developed in counties with lower median housing values, used here as a proxy for land values. Electricity prices in 2000 (measured at the state-level) are uncorrelated with wind investments, while counties scoring higher on the 2000 League of Conservation Voters (LCV) scores for House representatives are more likely to have wind installations. Counties with any wind investments have 40% lower average GDP than their never-wind counterparts, suggesting a negative correlation between wind investments and productivity. Finally, the 1995-1999 change in county-level unemployment rate is similar across ever-wind and never-wind counties, suggesting that employment trended similarly in both groups prior to our sample period.

3.3 Empirical Methodology

This paper estimates the causal effect of wind electricity investments on labor market outcomes. Given the staggered nature of these investments, we begin the analysis with a simple a TWFE regression, as shown in equation 3.1:

$$Y_{it} = \alpha + \beta WCAP_{it} + \psi X_{it} + \mu_i + \delta_{s(i)t} + \varepsilon_{it} \tag{3.1}$$

Where Y_{it} denotes the average log employment (or log earnings) and $WCAP_{it}$ is the two-year lead of operating wind capacity in county i during year t (scaled in GW). We use the two-year lead of wind capacity as treatment because the EIA data on wind capacity report the date that a wind installation starts generating electricity ("operating date"). Leading the operating date by two years enables us to capture the construction phase associated with wind projects. Analysis of industry reports suggested a high degree of uncertainty in the time from wind turbine construction to operation, with estimates ranging between 6 months and 2 years depending on resource allocation to construction. We chose 2 years as this timeframe likely captures the construction phase for most wind energy projects in our sample. The coefficient of interest is β, which measures the percent change in the outcomes following a 1 GW increase in wind generation capacity.

Because wind capacity investments may be correlated with other determinants of local labor market conditions such as population, we include state-by-year fixed effects, county fixed effects, and a vector of controls (X_{it}) to the empirical model. The state-by-year effects control for annual shocks to local economic conditions such as changes in state-level fiscal policy while the county effects capture underlying time-invariant characteristics of the county, including potential for wind development and location.[6] In addition, we also control for changes in counties' regulatory status under the Clean Air Act Amendments over time and the log population between ages 20 and 69. Since labor market conditions tend to be correlated within each county over time, we conduct inference using standard errors clustered by county and year (Cameron, Gelbach and Miller (2011)).

Naturally, we present several estimates of β below, for various specifications of the controls and fixed effects. Further, we make use of the information contained in QWI and estimate models that allow for a potentially different effect of wind electricity investments on labor market outcomes across gender, age, and education groups. We also estimate separate models for log employment rates and average earnings by industry category.

The identifying assumption in this "staggered adoption" research design is that the average labor market outcomes of counties that received and did not receive wind farms would trend similarly in the absence of the investment. Below, we estimate the dynamic effect of wind capacity installation on employment and earnings using a distributed lag

[6]The state-by-year effects also account for federal and state-level changes in incentives for wind generation development during our sample period, which have been shown to be an important driver of renewable energy installations by Hitaj (2013) and Yin and Powers (2010).

model and provide evidence supporting this assumption. Another challenge to identification in this setting are general equilibrium spillover effects of wind investments to the labor markets in neighboring counties that do not receive investments. We address this concern by re-estimating our main specification at the commuting zone level instead of the county level. Since commuting zones are aggregated versions of counties created to approximate the local economy where people live and work, this analysis should capture to a first order spillover effects to non-treated counties.

3.3.1 Dynamic Effects of Wind Capacity Investments

Our two way fixed effects research design relies on the assumption that the average labor market outcomes of counties that received and did not receive wind farms would trend similarly in the absence of treatment. While we cannot directly test this assumption, we estimate the following distributed lag model specification to provide suggestive evidence that it is plausible. One challenge in estimating the distributed lag model is that we know the date a wind installation starts generating power, but we do not know when construction started. Since increased employment associated with wind turbine construction would occur before power is generated, specifying the year before the installation starts producing power as the reference year would violate our identifying assumption. Following industry reports, we choose a reference year 2 years before a wind investment starts producing electricity. Our preferred specification models the logged outcomes as a function of 5 leads and lags of wind capacity as shown in equation 3.2

$$Y_{it} = \sum_{j=-5}^{5} \gamma_j WCAP_{i,t-j} + \psi X_{it} + \mu_i + \delta_{s(i)t} + +\varepsilon_{it} \tag{3.2}$$

Where the parameter of interest, γ_j, measures the contemporaneous effect of wind capacity investments on outcomes in county i at j years following (or prior to) a capacity addition. Following Schmidheiny and Siegloch (2020), we cumulatively sum the distributed lag coefficients of interest according to equation 3.3 to obtain the standard event study estimates, which are reported in Figure 3.5.

$$\beta_z = -\sum_{k=z+1}^{-1} \gamma_k \text{if } j \leq -20 \text{if } j = -1 \sum_{k=0}^{z} \gamma_k \text{if } j \geq 0 \tag{3.3}$$

Note that moving from the distributed lag model to the event study coefficients requires a normalization on the coefficient corresponding to the period 2 years before treatment adoption (β_{-2}) to be zero. Because we allow for 2 years of anticipation effects to capture changes in construction sector employment prior to wind capacity deployment, event study coefficient β_{-1} represents the cumulative change in capacity starting one period prior to a capacity addition. As demonstrated by Schmidheiny and Siegloch (2020), this dynamic model is equivalent to an event study with end points binned at 5 periods before and after treatment. By binning event time in this way, we are assuming that the effect of wind capacity installations is constant more than five periods after (or before) treatment occurs.

Figure 3.5 reports the event study coefficients and their associated 95% confidence intervals associated with equation 3.3. As described above, we normalize the event-time coefficients to 2 years before a wind installation begins operation to account for construction employment. Each coefficient represents the cumulative effect of a wind capacity installation on overall county-level employment at each event time period. None of the pre-wind installation coefficients are statistically distinguishable from zero individually or when pooled together (the p-value of a joint test of significance of the pre-period coefficients is 0.6), supporting the identifying assumption of our baseline TWFE research design: that employment in counties with and without wind installations would trend similarly in the absence of treatment. Figure 3.5 also suggests that wind capacity investments have an immediate, positive effect on employment that persists up to 5 years after the investment occurs. The persistence of the effect in local economies likely reflects continued construction of additional wind turbines and employment associated with the ongoing operation and maintenance of wind projects.

3.3.2 Heterogeneous Treatment Effects

Recent research has shown that the TWFE estimator may be biased for the average treatment effect on the treatment in the staggered adoption setting in the presence of treatment effect heterogeneity. (Borusyak, Jaravel and Spiess (2021), Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021), Sun and Abraham (2020)). In order to investigate whether this issue is impacting our central estimates, we re-estimate our main specification using two methods that explicitly account for treatment effect heterogeneity. First, we estimate the effect of wind energy investments on employment using the matching estimator proposed by Callaway and Sant'Anna (2021).

This estimator requires defining "adoption cohorts" which are groups of units that become treated at the same time. Since the estimator proposed by Callaway and Sant'Anna (2021) is specific to contexts with a binary treatment, we define treatment as an absorbing state which "turns on" 2 years prior to a county's first wind capacity addition. The estimator computes the treatment effect for each wind capacity adoption cohort by differencing each cohort's outcomes in a post- year t with its outcome 2 years prior to treatment (akin to the pre/post difference for the treatment group in standard DD estimation), and then computing the same difference for a control group that is not treated as of year $t-2$ (akin to the pre/post difference for the control group in standard DD estimation). For example, $ATT_{g,t}$ denotes the average treatment effect on the treated for all states that implemented a RPS policy in year g at post-treatment time t relative to 2 years before treatment, $g-2$. Adoption cohort-specific control groups are constructed by estimating a propensity score for each untreated county using baseline covariate values. The set of possible comparison groups for cohort g is of the counties that never add wind capacity during the sample period.

Estimation of the $ATT_{g,t}$ parameters in our setting relies on four assumptions. First, the data structure must be a panel or a repeated-cross section of counties. Second, conditional common trends holds between the treated and not-yet-treated groups, conditional on covariates. Third, treatment follows a staggered adoption design (e.g., the treatment is binary, and never reverts back from "1" to "0"). Fourth, there is some overlap on baseline covariates between the treatment and control groups. Assumptions 1 and 3 are trivially satisfied in our setting since our sample consists of a balanced panel of counties from 2000 to 2019 and we treat each county's first capacity addition as an irreversible treatment. While assumption 2 is impossible to formally test since it involves unobserved counterfactuals, we provide evidence that it is plausible by estimating pre-treatment period event study coefficients. Finally, to address assumption 4, we use the outcome regression estimand proposed by Callaway and Sant'Anna (2021) because there is limited covariate overlap between counties with wind generation capacity and their never-treated counterparts, leading to imprecise inference procedures when using inverse probability weighting and doubly robust estimators (Khan and Tamer (2010)).

As in our baseline TWFE specification, we control for dynamic county-level characteristics such as: solar electricity generation capacity, the log of the working age population, and nonattainment status. To conduct inference and compute standard errors, we use the multiplier bootstrap procedure described in Callaway and Sant'Anna (2021) which constructs simultaneous confidence intervals for the $ATT_{g,t}$ parameters. We cluster standard errors at the county level to allow for correlation in wind capacity adoption within each county over time. To facilitate the interpretation of our results, we summarize the $ATT_{g,t}$ parameters using the did R package from Callaway and Sant'Anna (2021). The parameter in column 2 of Table 3.7 corresponds to the average effect of wind capacity additions averaged across all adoption cohorts and time periods.

Since the treatment in our baseline specification is cumulative wind capacity, we also employ a "stacked difference in differences" (SDID) estimating equation following Cengiz et al. (2019), Deshpande and Li (2019), Fadlon and Nielsen (2021), and Flynn and Smith (2022) which allows for a continuous treatment. Overall, we find estimates from the basic TWFE and the SDID to be very similar, alleviating concerns that the TWFE is biased in our setting. The similarity between our TWFE estimates and SDID estimates is likely due to the large number of never-treated units in our sample. One source of bias is that the TWFE estimator includes "bad" comparisons between treated and already-treated units. Goodman-Bacon (2021) shows that when there are many never-treated units in the sample, the TWFE estimator places more weight on "good" and less weight on "bad" comparisons.

To proceed with the SDID method, we first match each of the 393 counties across the U.S. that ever receive a wind capacity investment to never-treated counties within the same state. This results in a yearly panel of 393 stacks, each consisting of one treated county and many never-treated control counties. We then estimate the two-way fixed effects regression as in equation 3.4.

$$Y_{ijt} = \alpha + \beta WCAP_{ijt} + \psi X_{ijt} + \delta_{ist} + \mu_j + \varepsilon_{ijt} \tag{3.4}$$

Where Y_{ijt} is the log employment (or earnings) in county i during year t that is part of stack j. The inclusion of stack and stack-state-year fixed effects ensures identification is driven by variation in wind capacity investments within each stack over time. In this setting, identification of the causal effect of wind installations on the outcomes requires the parallel trends assumption holds on average across all stacks. We include the same controls as in equation 3.1 and report robust standard errors clustered by county and year.

3.4 Results

3.4.1 Total employment and average earnings

Tables 3.2 and 3.3 report the estimates of the impact of wind capacity investments on the log total employment (employment for all demographic groups and industries) and log average monthly earnings. The estimates in column (1) include county and year fixed effects and column (2) adds a control for log population aged 20-69. Column (3) replaces the year fixed effects with state-year effects. In column (4), our preferred specification, we add an indicator for non-attainment status and county-level cumulative utility-scale solar generation capacity. Finally, in column (5) we control for community zone by year fixed effects instead of state-by-year. This permits the inclusion of controls that vary at the state-year level: the League of Conservation Voters (LCV) scores (scaled from 0 to 1).

The preferred estimates in column (4) indicate that a one GW wind capacity investment increases log employment by 0.046 log points, with a standard error of 0.014. This roughly corresponds to a 4.6% increase in total employment in response to a GW increase in installed wind capacity. Across all specifications considered in Table 3.2, the point estimates range from 3.5 to 5.3%, and all would be judged statistically significant at the 5% level. Notably, the employment impact of wind investments which accounts for time-varying confounding variables specific to each commuting zone is similar (and within one standard error) of the estimate in column (4).

Table 3.3 continues the analysis for log average monthly earnings and is designed exactly as Table 3.2. The evidence in Table 3.3 points to a positive impact of wind electricity investments on earnings as well. The point estimates range from 0.035 to 0.097 in log earnings points and all have confidence intervals that exclude a null effect. The preferred estimates in column (4) imply that each GW increase in capacity increase average monthly earnings by approximately 5.7%.

So far we have estimated wind investments' impact on local labor markets, but such investments could also affect the overall productivity of a regional economy. We examine this impact in Tables 3.4 and 3.5 which extend the employment and earnings analysis to log GDP and log income per capita respectively. The estimates in Table 3.4 suggest a positive effect of wind investments on local economic productivity, although the point estimate decreases significantly in magnitude and is statistically indistinguishable from zero at the 95% confidence level. Point estimates range from 4.4% to 16.6%, and all are statistically significant a the 5% level, except the point estimate in column (5). The preferred estimate in column (4) suggests that, on average, a GW increase in wind capacity increases county-level GDP by 13.1%.

While GDP captures the affect of wind capacity additions on local economic productivity, it may be possible that the increase in GDP we estimate in Table 3.4 is entirely driven by increasing incomes. We explore this possibility in Table 3.5 which replicates the GDP analysis with county-level logged income per capita as the outcome. If the estimated change in income per capita is of a similar magnitude of the change in GDP, then our results suggest that wind investments largely impact regional productivity by increasing incomes. The estimates in Table 3.5 suggest that wind capacity additions increase per capita income, and suggest that wind investments impact GDP partly through income and partly through direct productivity channels. Estimates range between 1.4%

and 9.2% and all are significant at the 5% confidence level, except for the specification with commuting zone by year effects. Our preferred estimate in column (4) suggests that a 1 GW increase in wind capacity is associated with a 5.3% increase in per capita income. Notably, the impact on per capita income is generally more than 1 standard deviation smaller than the impact on GDP (although most 95% confidence intervals overlap), suggesting that wind investments impact productivity directly and through income channels (although ultimately more evidence is needed to show this result conclusively).

Our analysis suggests that a 1 GW increase in wind capacity improves local economic conditions by raising employment, monthly earnings, GDP, and per capita income. However, 1 GW (1,000 MW) is a sizeable increase in wind capacity that greatly exceeds the size of any individual wind project or the typical average annual wind capacity addition for any county in our sample. Since the median annual capacity addition for any county in our sample is 100 MW, we rescale our results to provide estimates with a clearer interpretation. Our preferred estimates from column (4) of Tables 3.2, 3.3, 3.4, and 3.5 suggest that increasing wind capacity by 100 MW raises employment by 0.5%, earnings by 0.6%, GDP by 1.3%, and per capita income by 0.5%. These results point to a modest effect of wind capacity investments on overall local economic conditions. Applying the estimates for a 100 MW increase in wind capacity to the average county in our sample, we find increases of 178 workers, \$19 in average monthly earnings, \$45 million in GDP, and average annual income per capita by \$151. The estimated employment return to wind generation investments in this paper is about four times larger than that found in prior work by Brown et al. (2012) who estimates a OLS regression model with state fixed effects. However, relative to recent work by Gilbert, Gagarin and Hoen (2023) suggests that our estimates could provide a lower bound for the impact of wind capacity investments on employment at the local level.

The elasticities of employment, earnings, GDP, and per capita income with respect to wind capacity investments points to a small, but robust positive relationship. Our estimates imply that increasing wind capacity by 1% would increase employment in an average county with any wind investments during our sample period by 0.004%. The corresponding elasticities for average monthly earnings, GDP, and per capita income are 0.005%, 0.013%, and 0.005% respectively. Although the average effect of wind capacity investments is small, the effect is largely driven by counties with greater than the fourth quartile of cumulative wind generation capacity (248 MW). Table 3.9 decomposes our baseline result by quartile of wind capacity investment. Each coefficient reports the effect of having cumulative capacity in a given quartile on employment relative to county-years with no capacity investments. The preferred specification in Column (4) suggests that employment increases by 2.2% when cumulative capacity exceeds 248 MW, relative to county-years with no wind capacity.

Lastly, we explore how the employment effect we estimate changes over time for male and female workers in Table 3.10. Each row in Table 3.10 presents estimates for a specified time span of years following a county's first wind installation. Both columns replicate our preferred specification in Column (4) of Table 3.2 for the sample of male and female workers. Our estimates suggest that the employment gains from wind investments largely accrue to male workers and increase over time, with a 3.1% increase in employment more than 10 years after a county's first wind installation. Taken together, the results in Tables 3.9 and 3.10 suggest that the regional economic benefits of wind installations scale positively with installation size and may not accumulate in the short run.

We test the robustness of our baseline results using a variety of different specifications and estimands to estimate the relationship between wind capacity and regional economic activity. Our baseline specification relies on the assumption (commonly referred to as the Stable Unit Treatment Value Assumption or SUTVA) that wind investments in treated counties do not affect counties without wind installations. Since local economies may encompass several counties, the SUTVA may be violated in our context. To provide suggestive evidence of the validity of SUTVA in this context, we re-estimate our baseline preferred specification on a panel of commuting zones observed between 2000 and 2019. Since commuting zones are constructed by the Census Bureau to measure clusters of counties with strong commuting ties, these units are a better representation of local labor markets than county borders. Table 3.6 reports the estimates from the commuting zone-level analysis. All specifications include county and state-by-year effects, log population between ages 20 and 69, solar capacity, and a binary indicator of non-attainment status. Columns (1) through (4) present the estimated relationship between wind capacity and log employment, log monthly earnings, log GDP, and log per capita income, respectively. The estimated effect of a 1 GW increase in wind capacity is slightly larger for employment (9%) and earnings (10%) relative to the county-level specifications and both are marginally insignificant at the 5% confidence level. The estimates for GDP and income per capita are significant at the 5% confidence level, with similar sign and nearly double the magnitude of the estimates from the county-level regressions. Taken together, estimates from Table 3.6 support the validity of the SUTVA in this context.

As described in section 3.3.2, a recent literature in applied econometrics has documented that TWFE estimator is biased in the presence of heterogeneous treatment effects. Although work by Goodman-Bacon (2021) suggests that concerns regarding the bias of the TWFE estimator are more limited in settings with many never treated units, we use the estimator developed by Callaway and Sant'Anna (2021) to assess whether such bias is a concern in our setting. Column (1) of Table 3.7 reports the estimates from our preferred baseline specification in column (4) of Table 3.2, using a binary indicator for whether a county has any non-negative amount of wind capacity as treatment. Column (2) presents the estimated effect of having any wind capacity on log employment using the estimator proposed by Callaway and Sant'Anna (2021). While there are several ways to aggregate the Callaway and Sant'Anna (2021), estimator across capacity installation cohorts and event time periods, we choose to take the simple average of the cohort-time treatment effect estimates. The TWFE (column (1)) and heterogeneity robust (column (2)) estimates of wind capacity's effect on employment are 2.3% and 1.3% respectively and both estimates are significant at the 5% confidence level.

Since the data on labor market outcomes from the Census QWI excludes 14 states, in Table 3.8 we test the robustness of our baseline employment results using data from the BLS QCEW program which includes all U.S. states. All specifications replicate our preferred specification with county and state-by-year effects, log population aged 20-69, solar capacity, and an indicator for non-attainment status. Columns (1) and (2) present estimates of wind capacity's impact on employment using the QWI data with our preferred sample and the full sample of U.S. counties, while columns (3) and (4) display the same effects using the QCEW. Regardless of sample restriction or data source, all estimates are very similar (about 4.6%) and significant at the 5% confidence level.

3.4.2 Employment and average earnings by demographic group

Given the significance of wind generation in future plans to decarbonize the U.S. electrical grid, it is important to understand which workers benefit from investments in wind installations. We use the data on worker demographic characteristics from the Census QWI data set to estimate our preferred specification relating wind capacity to employment and earnings by worker sex, educational attainment, race and ethnicity, and two digit NAICS sector. All specifications replicate our preferred specification from Column (4) of Table 3.2 with county and state-by-year effects, logged population between ages 20 and 69, and a binary indicator for whether a county is in non-attainment under the U.S. Clean Air Act. Overall, our estimates suggest that wind capacity investments' impact on employment is most pronounced for male and black workers in the construction and utility sectors.

We start by estimating the effect of wind installations on employment by worker sex and age in Figure 3.6. The employment effect is larger for males overall than females and implies that employment increases by 7% in response to a 1 GW wind change in wind capacity. While it appears that younger workers receive the largest employment benefits of wind installations, all estimates are statistically indistinguishable from each other. Figure 3.7 reports estimates of wind investments' employment impact by worker sex and education. Once again, the estimates suggest that the employment effect is largest for male workers, implying that male employment increases by 7% in response to a 1 GW change in wind capacity.

Estimates by worker race and ethnicity are presented in Figure 3.8. The QWI reports 6 different categories of worker race (White, Black or African American, Asian, Native American or Alaskan Native, Native Hawaiian or other Pacific Islander, and two or more races) and 2 categories of ethnicity (Latino and not Latino). For conciseness, we report the estimates for workers identifying as African American and White by ethnicity in Figure 3.8. We estimate that individuals who identify as Black, non-Hispanic experience an 18% increase in employment following a 1 GW increase in wind capacity, an effect about 4 times larger than our baseline results in Table 3.2 which is statistically different from 0 at the 5% confidence level. However, we note that the point estimate for workers who identify as Black, non-Hispanic is imprecisely estimated, reflecting the relatively small number of individuals identifying as Black working in counties with any wind installations in our sample.[7] The estimate for individuals who identify as white implies a 4% increase in employment resulting from a 1 GW increase in wind capacity, which

[7] Our sample indicates that on average, 1,578 workers identify as Black in counties with any wind installations while 21,128 identify as white.

closely mirrors our baseline result in Table 3.2. Finally, the employment effect is similar for workers identifying as White, non-Latino and individuals identifying as White, Latino.

Lastly, Figure 3.9 reports the estimated employment effect of wind installation by 2 digit NAICS industry code.[8] Each coefficient is separately estimated by industry. Only the estimated employment effect for the construction sector is significant at the 5% confidence level, implying that a 1 GW change in wind capacity increases construction employment by 20%. We also find suggestive evidence of an increase in utility sector employment, although the estimate is imprecisely estimated, reflecting the variety of electricity generation sources contained in this sector (e.g. coal, natural gas, solar, wind, etc.). The industry-level results highlight that much of the overall employment effect we estimate is directly related to construction and maintenance activities from building and operating wind installations.

3.5 Conclusion

Using administrative data on labor markets and operational electricity generators across the U.S., this paper estimates the relationship between wind energy investments and regional economic conditions. The results suggest that wind installations have a modest, but persistent effect on regional economic conditions, with small, positive elasticities of employment, average monthly earnings, GDP, and per capita income that are two times larger than previous estimates from Brown et al. (2012) and significantly smaller than recent work by Gilbert, Gagarin and Hoen (2023). We find meaningful differences in which worker receive the estimated regional economic benefits from increases in wind capacity, with the largest estimated employment and earnings changes for male workers who identify as White, Black, and Latino. Construction, maintenance, and operation of

[8] While the QWI reports employment by detailed 4 digit NAICS code, we choose to use the more aggregated 2 digit sector codes due to significant data suppression in the more detailed industry data.

wind generation facilities appears to drive much of the earnings and employment effect we estimate, with the largest impact for the construction and electric utilities sectors.

This paper also demonstrates that the regional economic benefits accumulate within a year of a wind generator becoming operational and remain constant up to five years after installation. Although we find that the regional economic benefits accrue in the short run, the results are driven by the set of counties in the top quartile of cumulative wind capacity investments as of 2019 and more than 10 years after a county's first wind installation. Taken together, these results suggest that wind investments will have the largest immediate impact on regional economic conditions in counties which already have a significant amount of wind generation.

The estimates from this paper indicate that policies such as the U.S. Inflation Reduction Act (IRA), which incentive the development of wind energy projects will have a moderate positive impact on regional economies. One initial technical report by researchers at the Princeton ZERO Lab suggests that the IRA will increase total on-shore wind generation capacity in the U.S. by up to 150 GW more than in the policy's absence by 2035 (Jenkins et al. (2022)). Naively applying our estimate to this prediction to the average county with any wind investments in our sample implies that the IRA will increase employment by 164,000 and GDP by $44 billion between 2022 and 2035 through incentives for onshore wind generation.

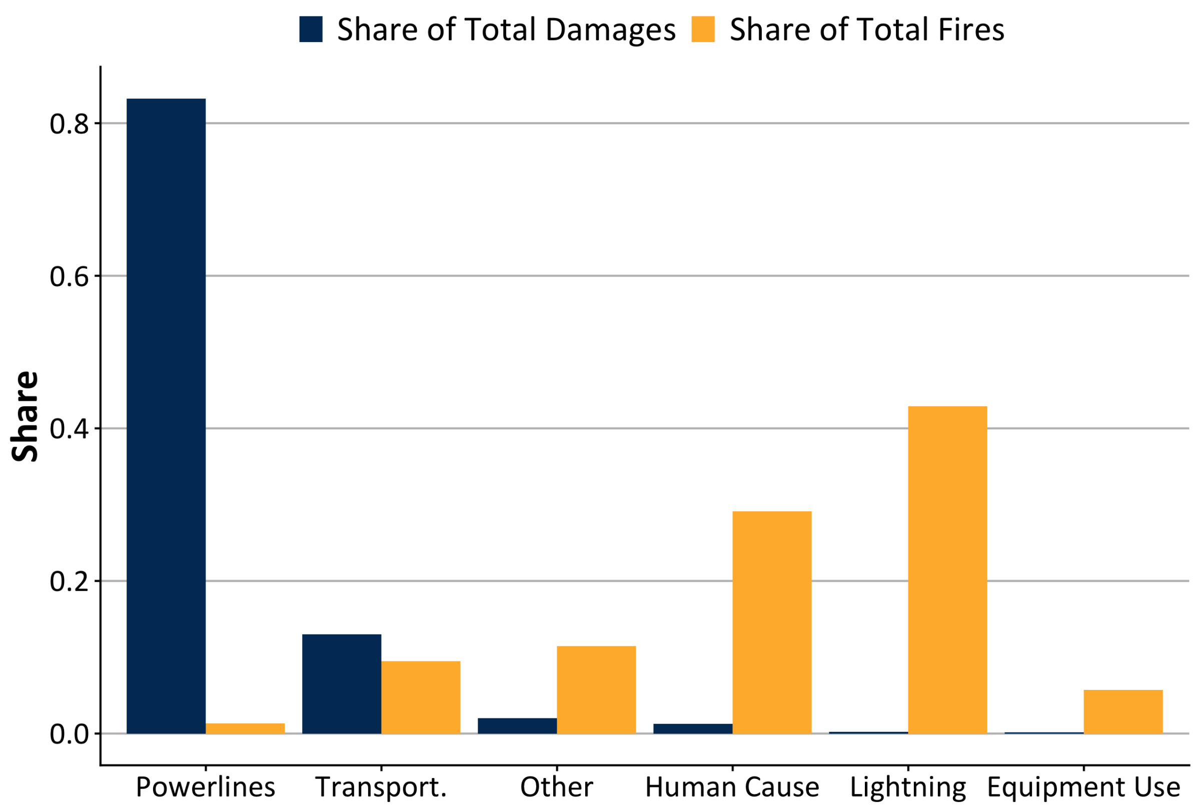
Several areas of research would build on this study to provide a complete analysis of the local economic impact of decarbonization policies. Our analysis does not quantify the economic impact of manufacturing activity or electrical infrastructure development related to on-shore wind investments. Furthermore, this paper considers one part of the suite of policies countries have considered to achieve decarbonization of their economies. Our analysis suggests that input-output models may incorrectly measure the true direct impacts of decarbonization policies, underscoring the need for further empirical study of how such policies impact local economies.

Appendix A

Appendix for “The Precautionary Consequences of Wildfire Liability: Evidence from Power Shutoffs in California”

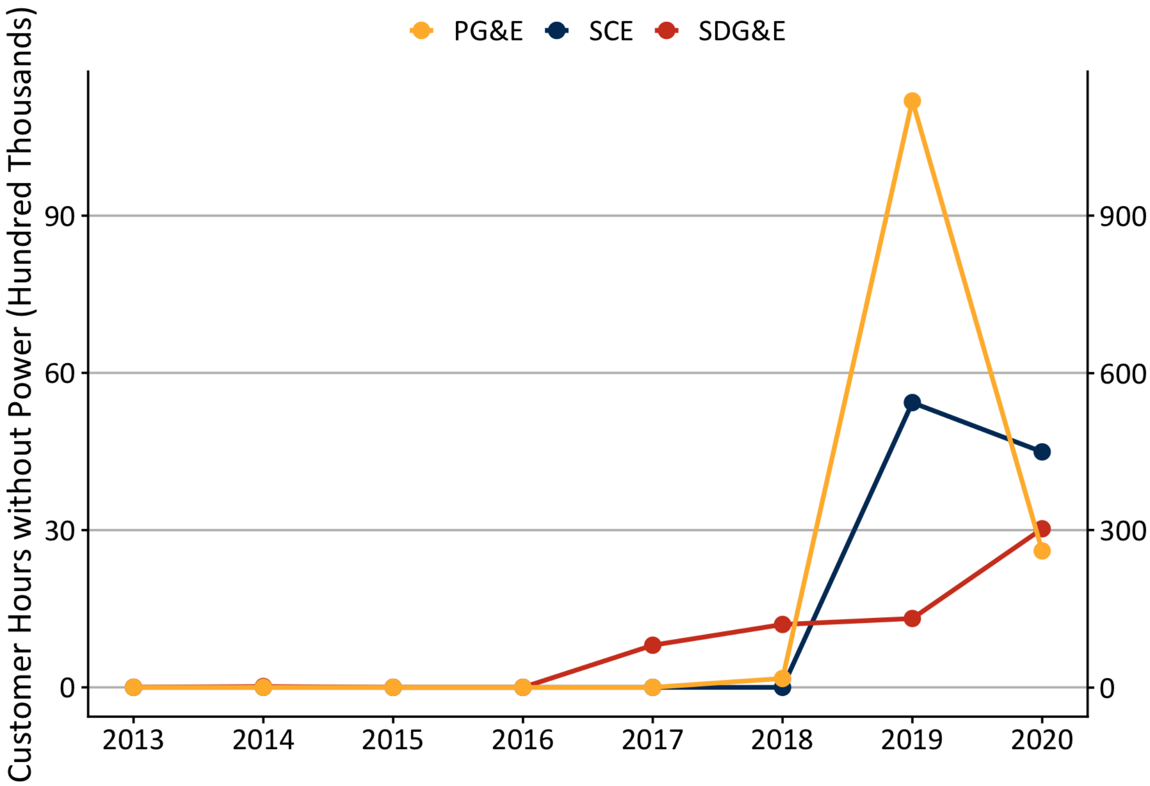
A.1 Figures

Figure A.1: Share of Wildfire Ignitions (1910-2016) and Damages (2008-2019) by Source



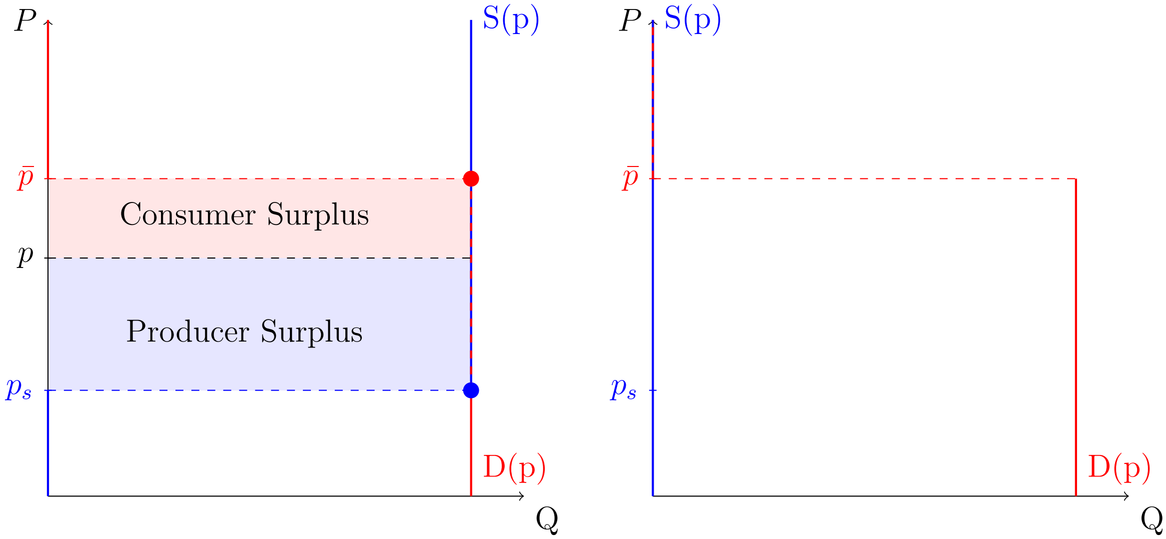
Notes: Share of total wildfire ignitions in California by cause of ignition between 1910 and 2016 are shown in yellow. The "Other" category includes fires caused by arson, debris, smoking, camping, playing with fire, railroads, lumber, equipment, and vehicles. Data are from Keeley et al. (2018). Share of total wildfire damages by ignition cause between 2008 and 2019 in California are shown in blue. Damages are defined as the replacement cost of homes destroyed by wildfire. The "Other" category includes fires caused by arson, debris, smoking, camping, playing with fire, railroads, lumber, equipment, and undefined cause. Data were collected by the author from CalFire historical wildfire activity data, also referred to as "redbooks."

Figure A.2: Total Customer Hours Impacted by Shutoff Events



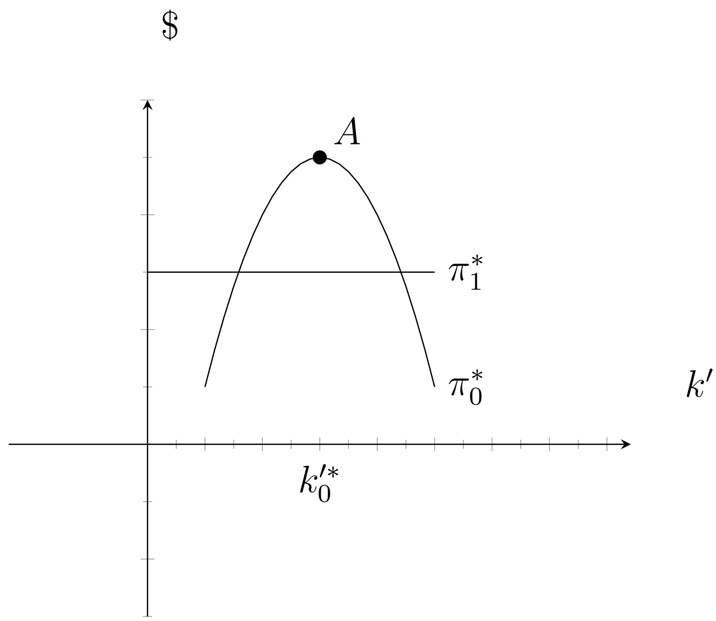
Notes: Total customer hours computed by the author from public safety power shutoff post event reports. Customer hours include commercial and residential customers served by California's three largest privately-owned utilities, Pacific Gas and Electric, Southern California Edison, and San Diego Gas and Electric. Reports are available from the California Public Utility Commission.

Figure A.3: Demand, Supply, and the Shutoff Decision for an Example Firm



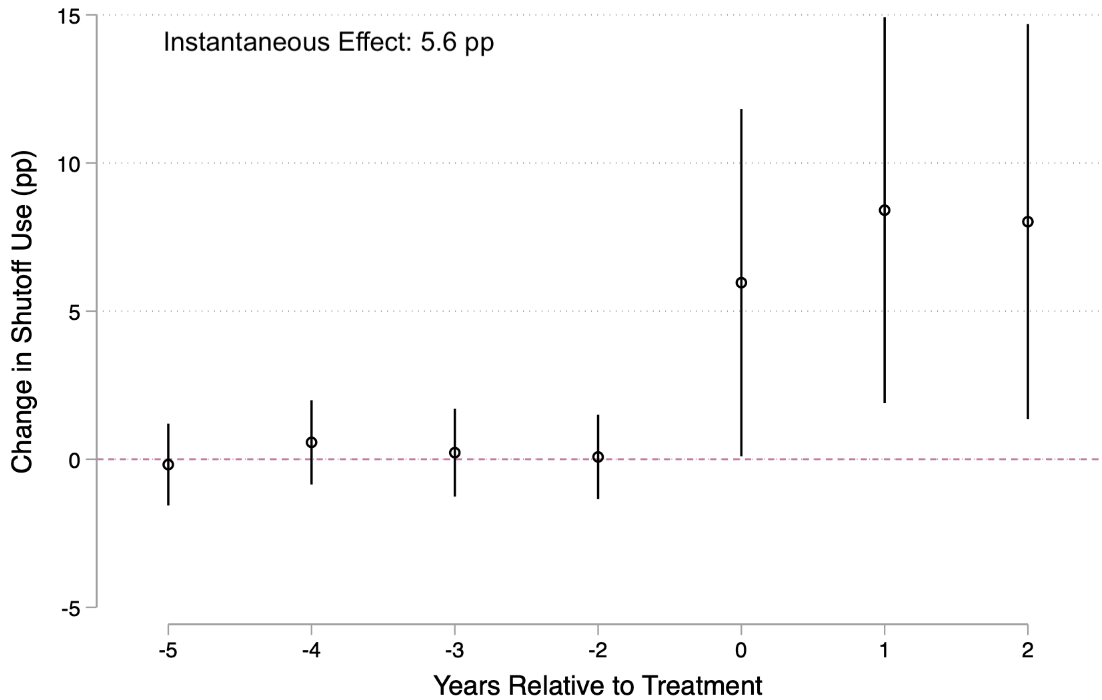
Notes: Supply and demand curve for an example firm when the firm provides electricity (left) and uses a power shutoff (right). Consumers' maximum willingness to pay for electricity is $\bar{p}$ and the firm's shutdown price is p_s.

Figure A.4: Firms supply electricity when $\pi_0 > \pi_1$



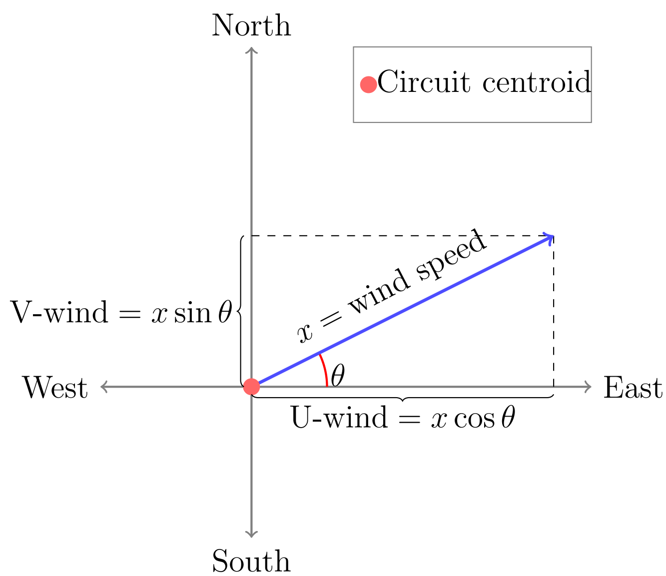
Notes: Solution to the firm's problem. Defensive capital investment is on the x-axis and dollars of profit is on the y-axis. The firm does not use a shutoff whenever its earns higher expected from supplying electricity (π_0) than from using a shutoff (π_1).

Figure A.5: Effect of 2017 Rule Change on Power Shutoff Use



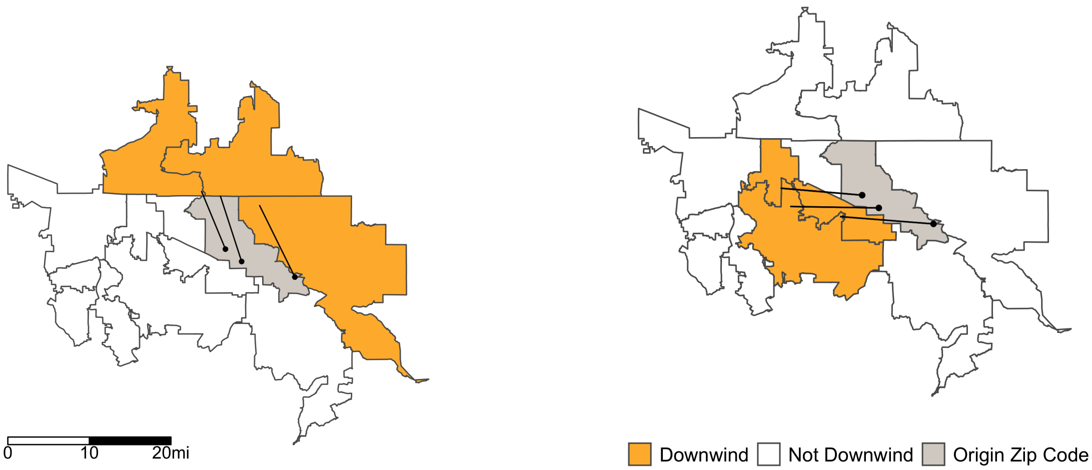
Notes: Estimated effect on shutoff use (in percentage points) of shifting liability for power line-ignited fire damages from electricity consumers to utilities. The x-axis plots event time in years relative to the 2017 liability rule change. The coefficient for period "-1" is excluded in this figure because it is zero by construction. Coefficient estimates are plotted with their 95% confidence intervals. The figure is created by estimating an event study version of regression model 1.7 on a daily panel of distribution circuits operated by San Diego Gas and Electric between 2013 and 2020. Standard errors are clustered at the high fire threat district by calendar week level to allow for correlation in Shutoff use across circuits with similar ignition risk during the same week.

Figure A.6: Description of Downwind Assignment



Notes: Figure shows how to compute U and V wind vectors from station-level wind speed (x) in meters per second and direction (θ) in radians. U and V wind vectors are scaled up by 18 minutes, the amount of time it takes for a lit ember to travel 10 km at wind speeds of 9.5 meters per second, and converted to degrees latitude and longitude to compute where a lit ember would land if picked up by the wind at the circuit centroid.

Figure A.7: Example of Regions that are Downwind of Power Lines Across Days

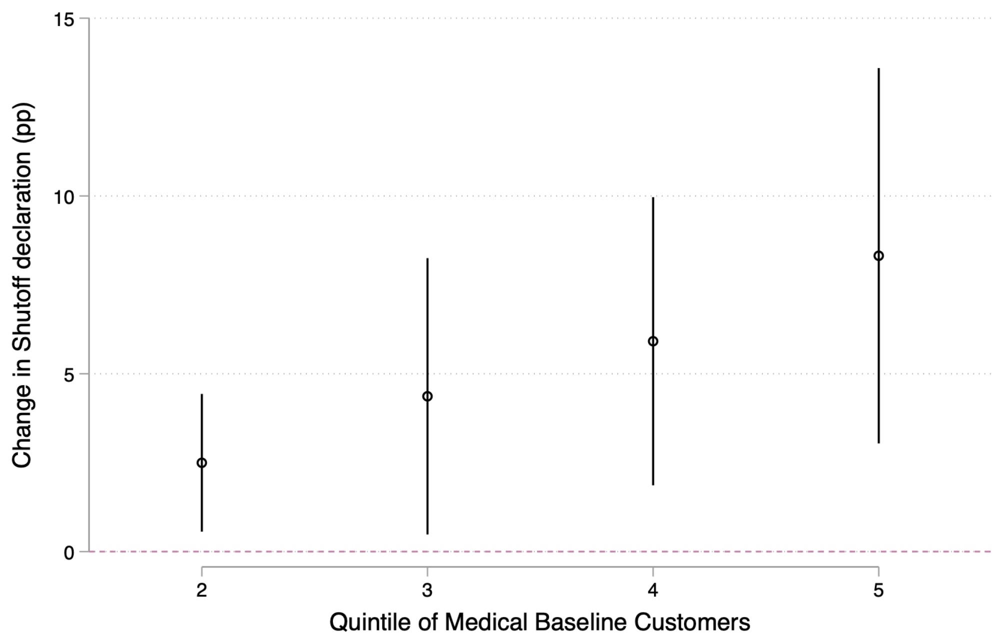


(a) Oct. 10, 2019

(b) Oct. 11, 2019

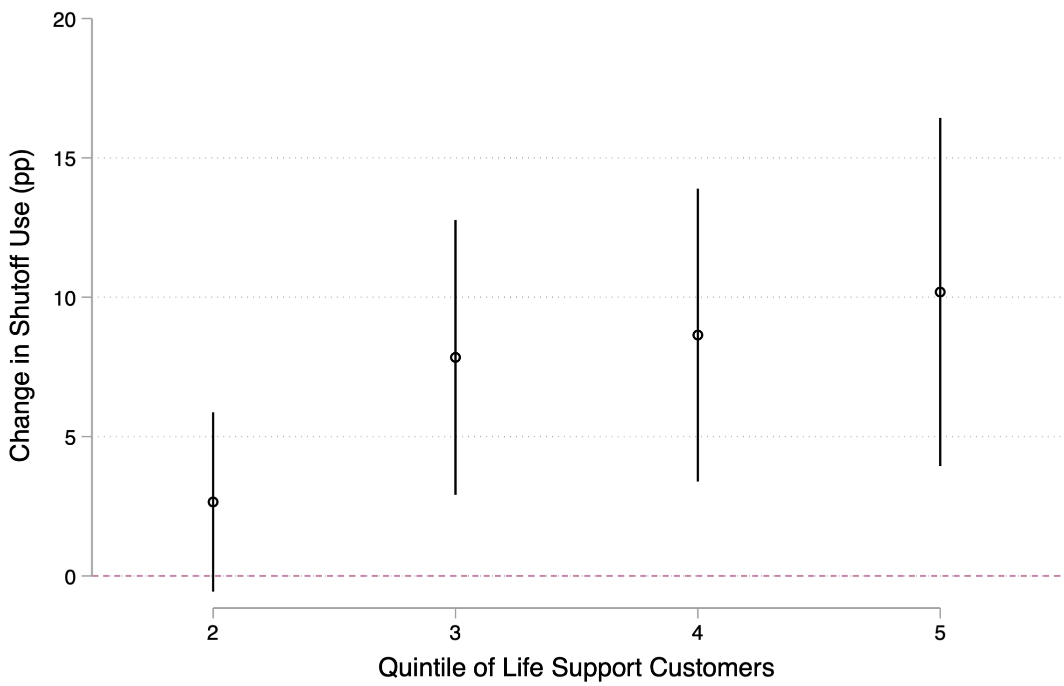
Notes: Daily variation in which zip codes are downwind of zip code 95917 (shown in tan) on October 10 and 11, 2019. The yellow and white shaded zip codes are the set of zip codes that are downwind of 95917 on any day between 2018 and 2020. The yellow zip codes are downwind of 95917 on a given day and the white zip codes are not downwind on the day shown. The black dot is the centroid of an electrical distribution circuit in zip code 95917 and the black line indicates the maximum daily wind direction and speed at the circuit on the day shown. The black line is using maximum daily wind speed and direction, an estimate of how far the wind can carry a lit ember from Albini et al. (2012), and several trigonometric identities. I calculate the total structure replacement cost for the yellow and white zip codes and changes in liability are generated by variation in wind direction and speed across days.

Figure A.8: Effect of 2017 Rule Change on Shutoff Declaration by Share of Medical Baseline Customers



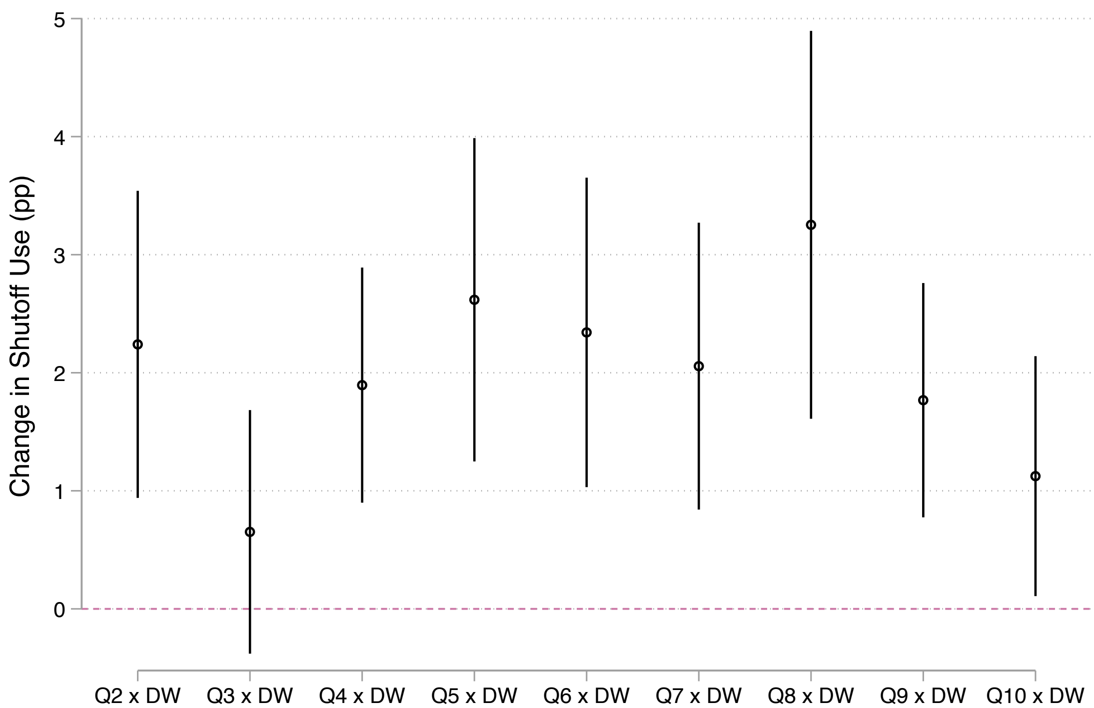
Notes: Estimated effect on shutoff use (in percentage points) of shifting liability for power line-ignited fire damages from electricity consumers to utilities by quintile of medical baseline customer share. A customer self selects into medical baseline status by notifying San Diego Gas and Electric of a qualifying medical condition or device. The share is calculated as the Coefficient estimates are plotted with their 95% confidence intervals. Each coefficient is interpreted relative to the estimated effect on days in the lowest septile of each climate condition. The figure is created by estimating a version of regression model 1.7 where treatment is interacted with binned climate conditions on a daily panel of on a daily panel of census tracts containing distribution circuits operated by San Diego Gas and Electric between 2013 and 2020. Daily climate conditions are from weather stations operated by San Diego Gas and Electric along their power lines. Standard errors are clustered at the high fire threat district by calendar week level to allow for correlation in shutoff use across circuits with similar ignition risk during the same week.

Figure A.9: Effect of 2017 Rule Change on Shutoff Declaration by Share of Life Support Customers



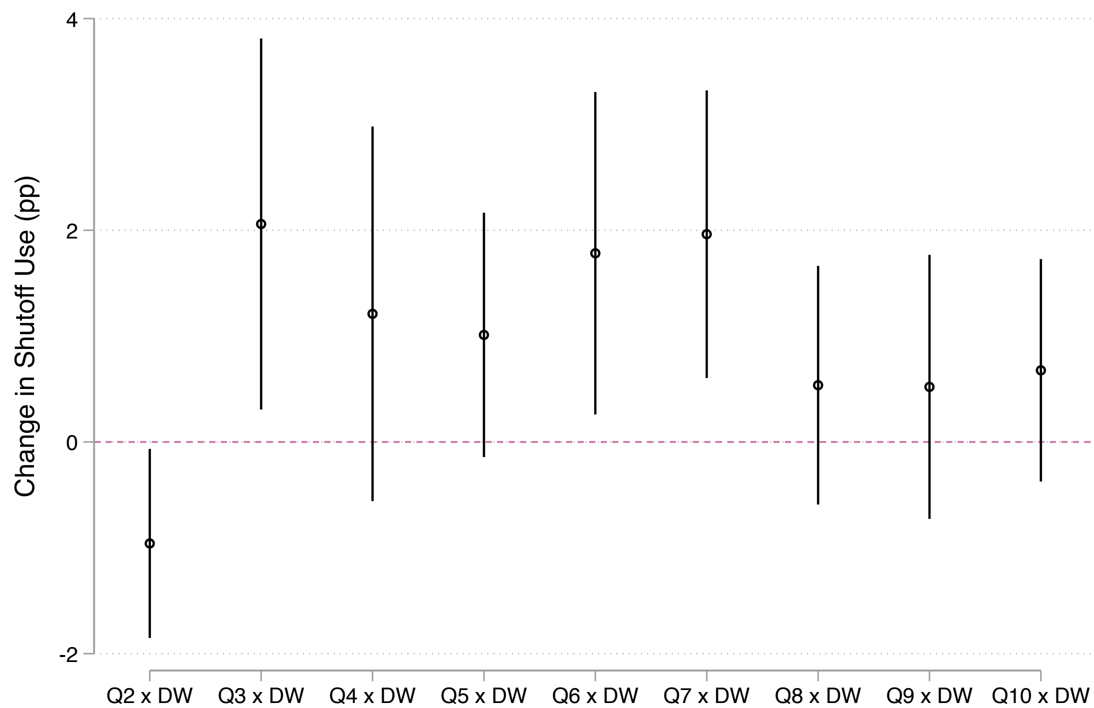
Notes: Estimated effect on shutoff use (in percentage points) of shifting liability for power line-ignited fire damages from electricity consumers to utilities by quintile of life support customer share. A customer self selects into medical baseline status by notifying San Diego Gas and Electric of a qualifying medical condition or device. The share is calculated as the Coefficient estimates are plotted with their 95% confidence intervals. Each coefficient is interpreted relative to the estimated effect on days in the lowest septile of each climate condition. The figure is created by estimating a version of regression model 1.7 where treatment is interacted with binned climate conditions on a daily panel of census tracts containing distribution circuits operated by San Diego Gas and Electric between 2013 and 2020. Daily climate conditions are from weather stations operated by San Diego Gas and Electric along their power lines. Standard errors are clustered at the high fire threat district by calendar week level to allow for correlation in shutoff use across circuits with similar ignition risk during the same week.

Figure A.10: Results by Decile of Total Zip Code Replacement Cost



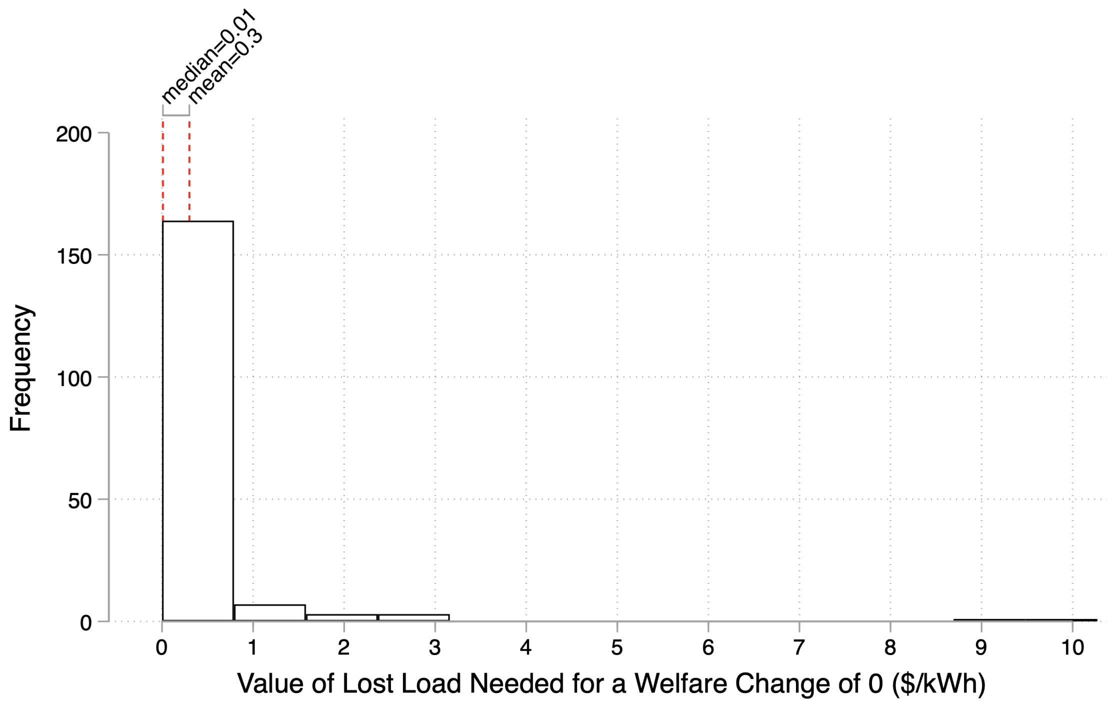
Notes: Wind and climate data is taken from weather stations operated by utilities in California and interpolated to each distribution circuit centroid using inverse distance weighting. Replacement costs are taken from the Zillow ZTRAX dataset. The underlying data consists of pairs of upwind, ever-downwind zip codes for selected days during January and April-December 2018-2020. Only days with wind speeds greater than 20 mph and relative humidity less than 30% are included in the sample. The outcome is a binary variable equal to 1 if there is an active shutoff in origin zip code o. The variables of interest are indicator variables for whether the total replacement cost in each destination zip code d is in one of ten bins on days when it lies downwind of zip code o. The excluded category is decile one, so all estimates represent the impact of threatened property values in each decile relative to the first decile. Controls include daily average temperature, wind speed, humidity, and maximum wind speed binned by septiles for each origin zip code o and destination zip code d. Standard errors are clustered at the high fire threat district by calendar week level.

Figure A.11: Results by Decile of Mean Zip Code Replacement Cost



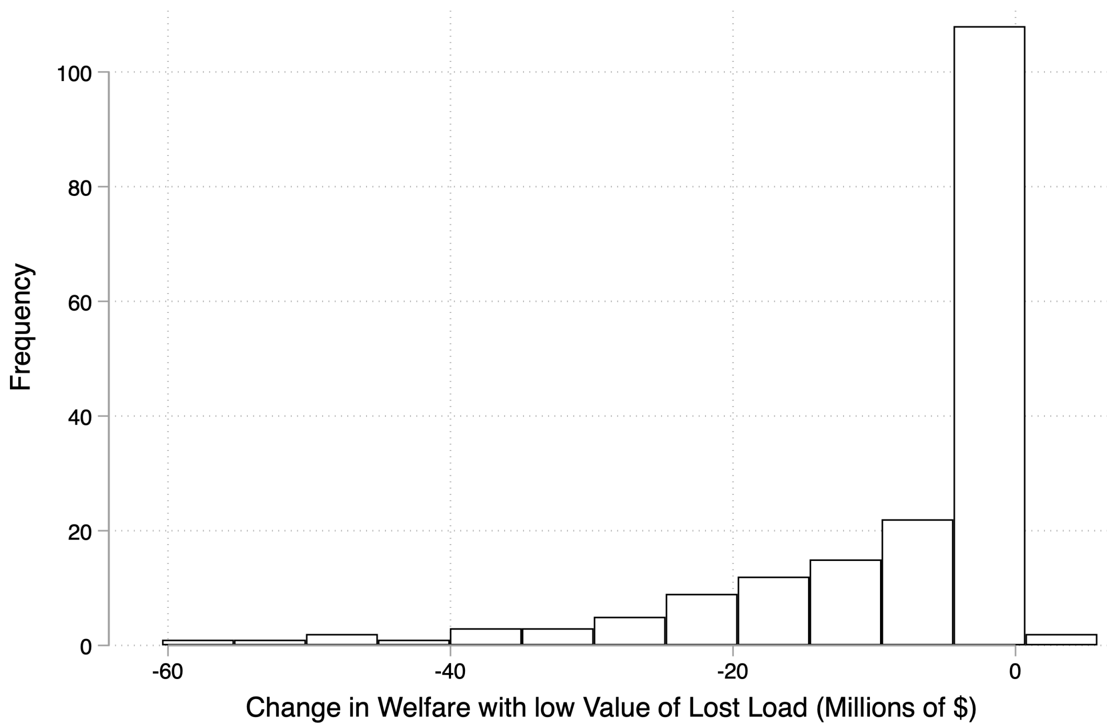
Notes: Wind and weather data is taken from weather stations operated by utilities in California and interpolated to each distribution circuit centroid using inverse distance weighting. Mean replacement costs are computed from the Zillow ZTRAX dataset for each zip code. The underlying data consists of pairs of upwind, ever-downwind zip codes for selected days during January and April-December 2018-2020. Only days with wind speeds greater than 20 mph and relative humidity less than 30% are included in the sample. The outcome is a binary variable equal to 1 if there is an active shutoff in origin zip code o. The variables of interest are indicator variables for whether the median replacement cost in each destination zip code d is in one of ten bins on days when it lies downwind of zip code o. The excluded category is decile one, so all estimates represent the impact of threatened property values in each decile relative to the first decile. Controls include daily average temperature, wind speed, humidity, and maximum wind speed binned by septiles for each origin zip code o and destination zip code d. Standard errors are clustered at the high fire threat district by calendar week level

Figure A.12: Value of Lost Load Needed for a Welfare Change of 0 at Each Circuit



Notes: This figure plots the value of lost load, or consumer's maximum willingness to pay for electricity, required for the observed shift of liability onto utilities to be welfare neutral. Values are computed by computing equation 1.10 for each circuit operated by San Diego Gas and Electric between 2013 and 2020. The change in the likelihood of shutoff use at each circuit is taken from the estimated coefficients in figure A.1. Ignition probabilities at each circuit are from San Diego Gas and Electric's internal model of circuit-level ignition risk. Energy usage at each circuit is computed from zip code level energy usage statistics reported by San Diego Gas and Electric. The author assigns energy usage to each circuit based on its share of total power line length in a given zip code. Future versions of this paper will use restricted access circuit-level energy use data. Damages are computed as the total commercial and residential property value within 20 kilometers of a circuit.

Figure A.13: Short Run Welfare Change at Each Circuit with a Low Value of Lost Load



Notes: This figure plots the short run welfare change for San Diego Gas and Electric customers following a 2017 policy change which increased utilities' share of liability costs from power line-ignited fires. Values are computed by computing equation 1.10 for each circuit operated by San Diego Gas and Electric between 2013 and 2020. The change in the likelihood of shutoff use at each circuit is taken from the estimated coefficients in figure A.1. Ignition probabilities at each circuit are from San Diego Gas and Electric's internal model of circuit-level ignition risk. Energy usage at each circuit is computed from zip code level energy usage statistics reported by San Diego Gas and Electric. The author assigns energy usage to each circuit based on its share of total power line length in a given zip code. Future versions of this paper will use restricted access circuit-level energy use data. Damages are computed as the total commercial and residential property value within 20 kilometers of a circuit.

A.2 Tables

Table A.1: Circuit by Day Panel Summary Statistics

	Mean (SD)	Min	Max	N
PSPS Likelihood (%)	0.02	0	100	2,960,650
	(1.43)			
Temperature (C)	23.74	0	46	2,960,650
	(5.11)			
Precipitation (mm)	0.71	0	165	2,960,650
	(3.56)			
Humidity (%)	64.74	3	100	2,960,650
	(16.44)			
Max Wind Speed (m/s)	7.43	0	96	2,960,650
	(1.86)			
Energy Usage (Millions kWh)	1.04	0	6	2,960,650
	(0.90)			
Property Value (Billions of $)	5.03	0	52	2,960,650
	(2.99)			
Expected Damages (Millions of $)	2.02	0	20	2,960,650
	(3.30)			
Probability of Ignition (%)	0.01	0	2	2,960,650
	(0.09)			
Installation Year	1969.24	1928	2019	2,815,192
	(20.15)			
N Circuits	88.00			

Notes: Statistics are computed for a daily panel of distribution circuits operated by San Diego Gas and Electric between 2013 and 2020. Shutoff event use data was collected from post-event reports submitted to the California Public Utility Commission. Daily temperature, humidity, and maximum wind speed are collected at 10 minute intervals from weather stations operated by San Diego Gas and Electric along their power lines. Energy usage data was collected from zip code level reports published on the San Diego Gas and Electric website. Property values were collected from the Zillow ZTRAX database. The probability of ignition was collected from a public data submission by San Diego Gas and Electric to the California Public Utility Commission in their wildfire management plan. Expected damages is the product of circuit-level ignition probabilities and property values at each circuit. "N Circuits" refers to the number of circuits that ever experience a shutoff event between 2013 and 2020.

Table A.2: Zip Code Panel Summary Statistics

	Mean (SD)	Min	Max	N
N PSPS	0.167	0	44	325,211
	(1.16)			
PSPS(0/1)	0.046	0	1	325,211
	(0.21)			
Replacement Cost (Billions)	6.799	0	37	325,211
	(6.64)			
Median Replace Cost (Thousands)	53.384	0	223	325,211
	(27.73)			
DAC Status	0.158	0	1	325,211
	(0.36)			
DW DAC Status	0.166	0	1	325,211
	(0.37)			
Temperature(F)	41.458	31	109	325,211
	(12.95)			
Humidity(%)	10.651	0	30	325,211
	(8.02)			
Wind Speed (mph)	24.496	20	88	325,211
	(4.52)			
Downwind Temp. (F)	45.030	30	113	324,965
	(14.48)			
Downwind Humid. (%)	14.517	0	100	324,965
	(12.49)			
Downwind Wind Speed (mph)	21.210	0	56	177,615
	(6.67)			
Downwind WHP Share	3.054	0	5	325,211
	(0.99)			
Downwind WUI Pop Share	0.006	0	1	325,211
	(0.04)			
Energy Use (GWh)	6.644	0	833	318,485
	(30.39)			
Downwind Energy Use (GWh)	6.880	0	833	320,130
	(31.81)			
N Zip Codes	562.000			

Notes: Wind and weather data is taken from weather stations operated by utilities in California and interpolated to each distribution circuit centroid using inverse distance weighting. Total replacement costs are taken from the Zillow ZTRAX dataset and converted to 2021 dollars. Median replacement costs are computed for each zip code from the parcel-level Zillow data.

Table A.3: Zip Code Characteristics by Downwind Status

	Mean (Not Downwind)	Mean (Downwind)	Difference (% of SD)
Replacement Cost (Billions)	5.4	5.6	-2.8***
Median Replace Cost (Thousands)	47.3	48.4	-4.1***
Share Disadvantaged	0.5	0.4	7.7***
Wildfire Hazard Potential	3.1	3.1	-0.9
Pop. Living in WUI (% of Total)	0.6	0.4	5.9***
Avgerage kWh Consumed	4,182.6	3,889.9	1.4***
N Houses	7,345.2	7,305.8	0.5
Total Population	20,039.4	19,705.6	1.6***
% White	58.8	60.2	-5.9***
% Black	3.2	3.1	1.0*
% Asian	6.1	5.6	4.9***
% Other Race	4.2	4.4	-4.8***
% Hispanic	27.7	26.7	5.0***
Employment	5,715.0	5,376.3	3.9***
Annual Payroll (Thousands)	264,031.3	253,718.2	1.7***
N Establishments	432.4	416.3	3.1***
N Medicare Beneficiaries	2,992.5	2,967.2	0.9*
N Medical Devices	111.1	113.4	-1.9***
Temperature(F)	38.1	38.4	-6.1***
Relative Humidity (%)	12.8	13.1	-3.2***

Notes: Wind and weather data is taken from weather stations operated by utilities in California and interpolated to each distribution circuit centroid using inverse distance weighting. Total replacement costs are taken from the Zillow ZTRAX dataset and converted to 2021 dollars. Median replacement costs are computed for each zip code from the parcel-level Zillow data. Disadvantaged community (DAC) status comes from the CalEnviroScreen 3.0 update and is computed as the share of total 2010 zip code population living in a census tract categorized as a DAC. Wildfire hazard potential is an index varying from 1 (low) to 5 (very high) which quantifies the relative potential for wildfire that may be difficult to control (Dillon and Gilbertson-Day (2020)). The share of 2010 population living within the wildland urban interface is computed using data from Radelof et al. (2017). All population data is from the California Department of Finance. Employment, payroll, and the number of establishments are collected from the 2013 Census zip code business patterns database. Medicare beneficiaries and the number of medical devices that rely on electricity are collected from the U.S. Department of Health and Human Services emPOWER Map 3.0.

Table A.4: Effect of Liability Regulation on Shutoff Probability and Customer Hours without Power

	Shutoff Indicator (1)	Customer Hours (2)
Treated x Post 2017	5.64***	923.47***
	(1.56)	(245.21)
Controls	x	x
Circuit FE	x	x
Month FE	x	x
Mean of Dep. Var	0.07	1.33
Bootstrap 95% CI	[1.9,9.4]	[376.3,1,506.5]
Observations	50,809	50,809

Notes: All columns estimate the change in shutoff use following a reform that increased the share of liability born by firms. The outcome is a binary variable equal to 1 when there is an active shutoff event at circuit i on day t. Column 1 reports the estimate from a regression with no controls. Column 2 adds nonlinear controls for daily climate conditions at circuit i. Column 3 adds circuit fixed effects and column 4 adds month fixed effects. Post 2017 is a binary variable that takes a value of 1 for all days following November 30, 2017. Standard errors are clustered at the calendar week level to allow correlation in shutoff declaration across circuits within a week. The average value of the outcome conditional on wind speeds being in the highest septile observed during the sample period is also reported.

Table A.5: Effect of Total Zip Code Replacement Cost on the Probability of a Shutoff

	Total Value (1)	Mean Value (2)
Value x DW	0.02***	0.04**
	(0.01)	(0.02)
DW	0.04**	0.04**
	(0.02)	(0.02)
Controls	x	x
Pair FE	x	x
Day FE	x	x
Mean of Dep. Var	0.025	0.025
1 SD Effect	0.286	0.224
Bootstrap 95% CI	[0.005,0.038]	[0.004,0.078]
Observations	505,656	505,656

Notes: Wind and weather data is taken from weather stations operated by utilities in California and interpolated to each distribution circuit centroid using inverse distance weighting. Total replacement costs are taken from the Zillow ZTRAX dataset and converted to 2021 dollars. The underlying data consists of pairs of upwind, ever-downwind zip codes for every day during January and April-December 2018-2020. The outcome is a binary variable equal to 1 if a shutoff event is active in origin zip code o. Value measures the total cost of replacing structures in each destination zip code d and DW is a binary variable equal to 1 when zip code d is downwind of zip code o on day t. Controls include daily average temperature, relative humidity, precipitation, and maximum wind speed binned by septiles for each origin zip code o and destination zip code d. Standard errors are clustered at the high fire threat district by calendar week level.

Table A.6: Robustness Analysis Estimates

	Main Model (1)	WUI Controls (2)	Usage Controls (3)	5 Day Treatment (4)
Value x DW	0.02***	0.02**	0.02**	0.02**
	(0.01)	(0.01)	(0.01)	(0.01)
DW	0.04**	0.09**	0.10**	0.02
	(0.02)	(0.04)	(0.05)	(0.01)
Controls	x	x	x	x
Pair FE	x	x	x	x
Day FE	x	x	x	x
Mean of Dep. Var	0.025	0.025	0.025	0.025
1 SD Effect	0.286	0.224	0.264	0.198
Observations	505,656	505,656	498,324	505,563

Notes: Column 1 replicates the main estimate from column 4 of table A.5. Column 2 adds controls for the share of total population in zip code d living in the Wildland Urban Interface. Column 3 adds controls for monthly zip code electricity usage in zip codes o and d separately. Column 4 assigns a destination zip code (d) as downwind if it is downwind anytime in the next 5 days (from t to $t + 5$). Wind and weather data is taken from weather stations operated by utilities in California and interpolated to each distribution circuit centroid using inverse distance weighting. Total replacement costs are taken from the Zillow ZTRAX dataset and converted to 2021 dollars. The underlying data consists of pairs of upwind, ever-downwind zip codes for every day during September-December 2019-2020. The outcome is a binary variable equal to 1 if a shutoff is active in origin zip code o. Value measures the total structure replacement cost in each destination zip code d and DW is a binary variable equal to 1 when zip code d is downwind of zip code o on day t. Standard errors are clustered at the high fire threat district by calendar week level.

A.3 Additional Results and Robustness Checks

A.3.1 Extensive Margin

Estimates by Ignition Risk

For this analysis, I measure ignition risk using San Diego Gas and Electric's modelled probability of ignition at each circuit as reported in its 2020 Wilfire Mitigation Plan. This measure captures the likelihood of ignition at each circuit operated by San Diego Gas and Electric as of 2020. Unlike the measure of ignition risk used in the main analysis, the modelled probabilities are *ex-post* because they reflect conditions after the 2017 policy change.

I bin the circuits by ignition probability into 11 categories: one category for circuits with an ignition probability of 0 and one category for each decile of the ignition probability conditional on it being positive. Figure A.1 plots the coefficients from a modified version of equation 1.4 where treatment is interacted with the 11 mutually exclusive indicator variables representing different risk percentiles. Since the 0 ignition probability category is excluded, each coefficient reflects the treatment effect at a specified decile of ignition risk relative to the treatment effect at circuits with no ignition risk. The coefficients in figure A.1 increase with wildfire risk, suggesting that San Diego Gas and Electric's increase in precaution following the policy was largely concentrated at circuits with high ignition risk. At the highest risk circuits, power shutoffs increased (on average) by around 12 percentage points, a more than 170-fold increase relative to the pre-period mean.

Precaution and Climatic Conditions

In order to test how changes in daily climate conditions influence precautionary activity, I use the daily panel of distribution circuits operated by San Diego Gas and Electric between 2013 and 2020. I examine how the change in shutoff use following the 2017 rule change differs by daily maximum wind speed, relative humidity, temperature, and cumulative precipitation. Based on the IOUs' explanation of ignition risk in their Wildfire Mitigation Plans, maximum wind speed and humidity should be particularly important predictors of power shutoff use because fire risk is elevated during periods of high wind speed and low humidity. Equation A.1 presents how I model the relationship between shutoff declaration (y_{imt}) and climate characteristics using a fixed effects framework with climate variables binned into septiles.

$$y_{imt} = \beta_0 + \sum_{k=1}^{7} \beta_{2k} X_{kimt} + \sum_{k=1}^{7} \beta_{3k} X_{kimt} Post_{mt} + \gamma_i + \delta_m + \nu_t + \varepsilon_{imt} \tag{A.1}$$

Where X_{kimt} is a vector of climate variables including maximum daily wind speed, daily average relative humidity, cumulative precipitation, and temperature each binned into septiles. The model conditions on fixed effects for each circuit (γ_i), month (δ_m), and calendar day (ν_t). Finally, the coefficients of interest, β_{3k}, capture how the percentage point change in power shutoff declaration following the 2017 rule change varies by daily climate conditions. Standard errors are clustered at the week by high fire threat district zone level to allow for correlation in utility decision making across all circuits with similar ignition risk during the same week.

Estimates by Daily Weather Conditions

Figure A.2 plots the estimates from Equation A.1 by septile for each of the four daily climate variables (maximum wind speed, relative humidity, temperature, and cumulative precipitation). In Panel (a) the coefficients imply that the increase in shutoff declaration following the 2017 rule change is increasing in maximum daily wind speed, with power shutoffs increasing by approximately 0.6 percentage points on days with wind speeds in the top septile. Panel (b) shows that San Diego Gas and Electric's increase in shutoff event use following the 2017 rule change was also decreasing in relative humidity. These results are reassuring, since utilities present wind speed and humidity as two primary drivers of ignition risk in documents submitted to the regulator. Panel (c) provides evidence that San Diego Gas and Electric's power shutoff event use increased more on cooler days following the 2017 rule change. Finally, Panel (d) shows that there is no clear relationship between daily cumulative precipitation and shutoffs.

A.3.2 Intensive Margin

Analysis Using Local Variation in Replacement Cost

To alleviate the concern that the estimates of potential liability's effect on shutoff use may be spuriously driven by the replacement cost of structures that are not close to a distribution circuit, I estimate a modified version of equation 1.7 which uses local variation in wind direction around each circuit. Figure A.4 provides an example of the methodology for this circuit-level analysis. As shown in figure, A.4, I create 10 and 20 kilometer buffers around each circuit, and divide each buffer into quarters to create 8 potentially downwind regions around each circuit. I then compute the total and median structure replacement cost in each region and use daily variation in wind direction and speed to generate changes in potential liabilities across days just as in the zip code analysis. Finally, I estimate a modified version of 1.7 at the circuit level that controls for daily weather conditions at each circuit, circuit-region fixed effects, utility-year fixed effects, and calendar day fixed effects.

Table A.1 reports the results of this analysis. The coefficient in column 2 implies that the likelihood of a shutoff increases by 0.05 pp (208% relative to the mean) when the median downwind structure replacement cost increases by 10%. Reassuringly, this effect is very similar to the effect of potential liability on precaution from the zip code analysis.[1]

[1]The effect in column 1 is no longer significant, but this is unsurprising because I had to drop all

Estimates by Zip Code Socioeconomic Status

Since I find that utilities use shutoffs more in regions with higher structure replacement cost, which is positively correlated with socioeconomic status, there could be distributional consequences associated with liability regulation. For example, utilities may be more likely to use a shutoff in a low socioeconomic status community in response to higher potential liability in a downwind high socioeconomic status community. To test for distributional impacts, I re-estimate equation 1.7 and decompose the effect by whether the origin or destination zip code has an above-average share of its 2010 population living in a census tract defined as a disadvantaged community by the California state government.

Table A.2 reports the estimated relationship between potential liability and shutoff use by socioeconomic status. Row 1 reports the effect when a high socioeconomic status community lies downwind of a low socioeconomic status community, while row 2 reports how shutoffs respond to potential liability when a low socioeconomic status community lies downwind of a high socioeconomic status community. The estimates in rows 3 and 4 reflect the relationship between shutoffs and liability when both the upwind or downwind zip codes are high socioeconomic status. The results suggest that the relationship between potential liability and shutoff use is driven by zip codes of high socioeconomic status, providing no evidence of distributional consequences in this setting.

of the parcels in the ZTRAX data that did not have geocoordinates or that were geocoded to zip code centroids. As a result, the total replacement cost is no longer accurate. I am in the process of manually geocoding these parcels.

A.4 Heterogeneous Treatment Effects

I use observed data on power shutoff use from three large investor owned utilities in California called Pacific Gas and Electric, Southern California Edison, and San Diego Gas and Electric to estimate the relationship between potential liability and precaution. Because each utility has different exposure to ignition risk in its service territory and varying experience with ignition prevention historically, there are likely heterogeneous treatment effects across firms in the sample. For example, over half of Pacific Gas and Electric's service territory lies within regions of heightened ignition risk while 35% of San Diego Gas and Electric's service territory is in high risk areas.[2]

Recent econometric research has shown that in settings with heterogeneous treatment effects (like in the case of the California's electric utility industry), two-way fixed effects or difference in differences estimators identify a weighted average of treatment effect parameters which may not correspond to the overall average treatment effect on the treated (Sun and Abraham (2020), de Chaisemartin and D'Haultfœuille (2020), Borusyak and Jaravel (2017), Goodman-Bacon (2021)). Furthermore, recent work has pointed out that many environmental policies have different effects across units and over time (Steigerwald, Vazquez-Bare and Maier (2021)).

Since heterogeneity across firms is the primary source of treatment effect heterogeneity in this setting, I re-estimate equation 1.7 by firm. As a result, the regression model identifies three parameters of interest: the response of shutoffs to liability for Pacific Gas and Electric, San Diego Gas and Electric, and Southern California Edison. Following Steigerwald, Vazquez-Bare and Maier (2021), the overall effect of liability on shutoff use can be estimated by taking a weighted average of the three coefficients of interest, where each weight is the group's proportion of the sample.

[2]See Pacific Gas and Electric and San Diego Gas and Electric's 2020 wildfire mitigation plans for a detailed breakdown of their service territories by ignition risk.

$$\hat{\beta}_\lambda = \sum_g \lambda_g \beta_{FE}^{\hat{g}}$$

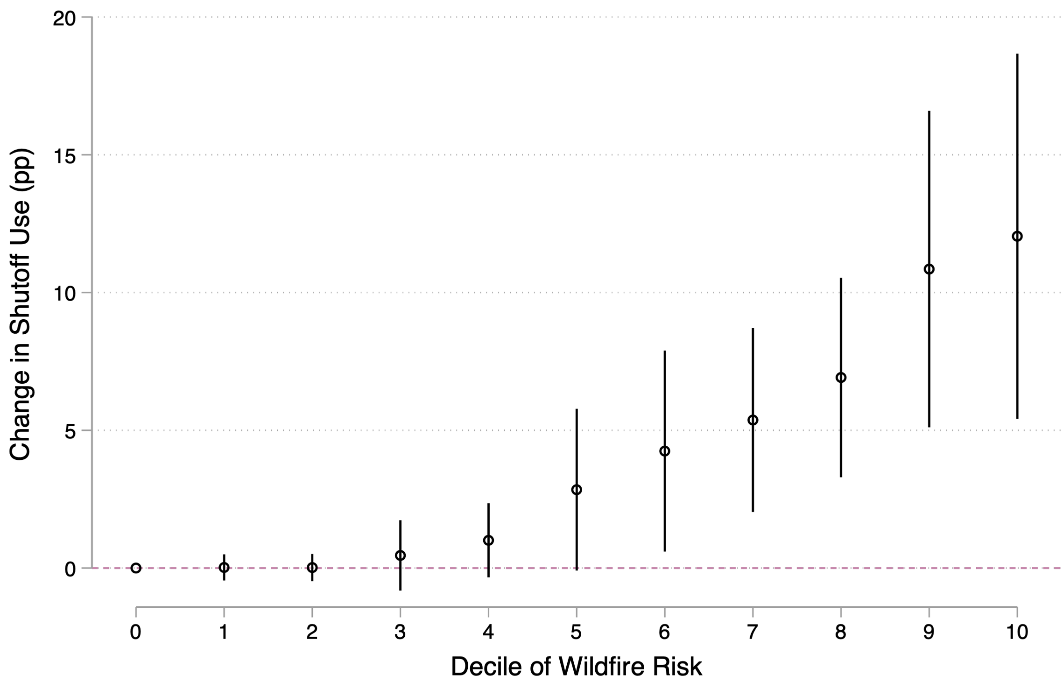
Where λ_g is the fraction of observations in the sample that are part of group g.[3] Since the cluster-robust variance estimator is sensitive to heterogeneity in between-cluster variation (Carter, Schnepel and Steigerwald (2017)), I compute the effective number of clusters using the *summclust* Stata command. There are 20 effective clusters in this setting, suggesting that using the wild cluster bootstrap procedure recommended by Cameron, Gelbach and Miller (2008) is warranted. I report the bootstrapped 95% confidence interval for the overall effect of potential liability on shutoff use in table A.3.

The results of the heterogeneity analysis are presented in table A.3. Columns 1 and 2 report estimates for the effect of total and average structure replacement costs on the likelihood of a power shutoff. The coefficients of interest in columns 1 through 3 suggest that most of the relationship between potential liability and shutoff use is driven by San Diego Gas and Electric and (to a lesser extent) Pacific Gas and Electric. The overall effect of structure replacement cost on shutoffs is reported as the "Pooled Estimate". Reassuringly, the pooled estimates are of a similar magnitude as the main estimates in table A.5 and both are statistically different from zero at the 95 percent confidence level.

[3] In this setting the weights are 0.39 (Pacific Gas and Electric), 0.39 (San Diego Gas and Electric), and 0.22 (Southern California Edison).

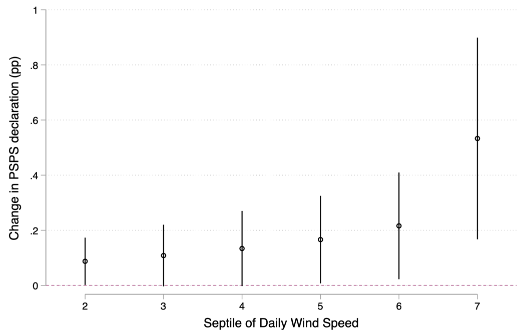
A.5 Appendix Figures

Figure A.1: Effect of 2017 Rule Change on Shutoffs by Circuit Ignition Risk

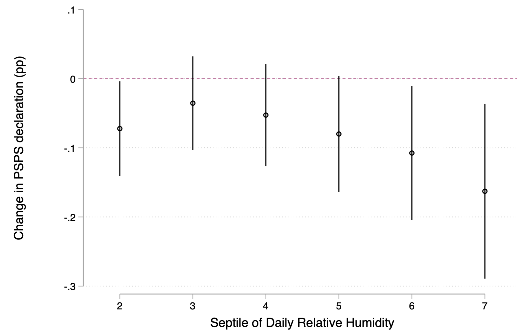


Notes: Estimated effect on shutoff use (in percentage points) of shifting liability for power line-ignited fire damages from electricity consumers to utilities by decile of circuit ignition risk. Coefficient estimates are plotted with their 95% confidence intervals. Each coefficient is interpreted relative to the estimated effect at circuits with no ignition risk. The figure is created by estimating a version of regression model 1.7 where treatment is interacted with binned circuit ignition risk on a daily panel of distribution circuits operated by San Diego Gas and Electric between 2013 and 2020. Ignition risk is from an internal model created by San Diego Gas and Electric. Standard errors are clustered at the high fire threat district by calendar week level to allow for correlation in shutoff use across circuits with similar ignition risk during the same week.

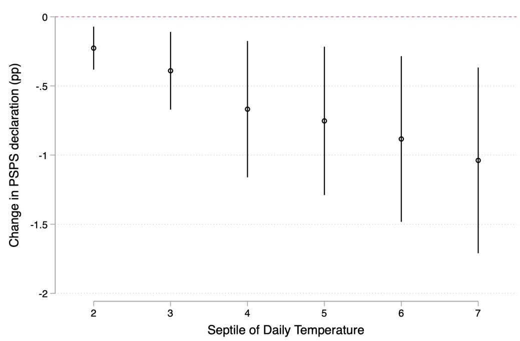
Figure A.2: Effect of 2017 Rule Change on Shutoff Declaration by Daily Weather Conditions



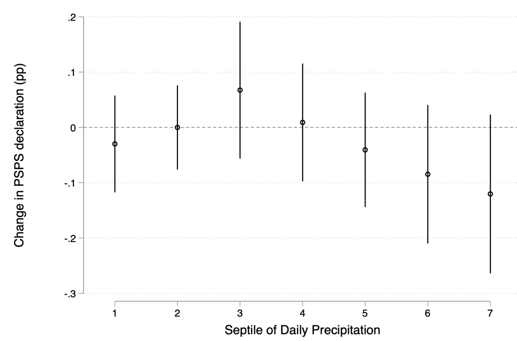
(a) Maximum Wind Speed



(b) Relative Humidity



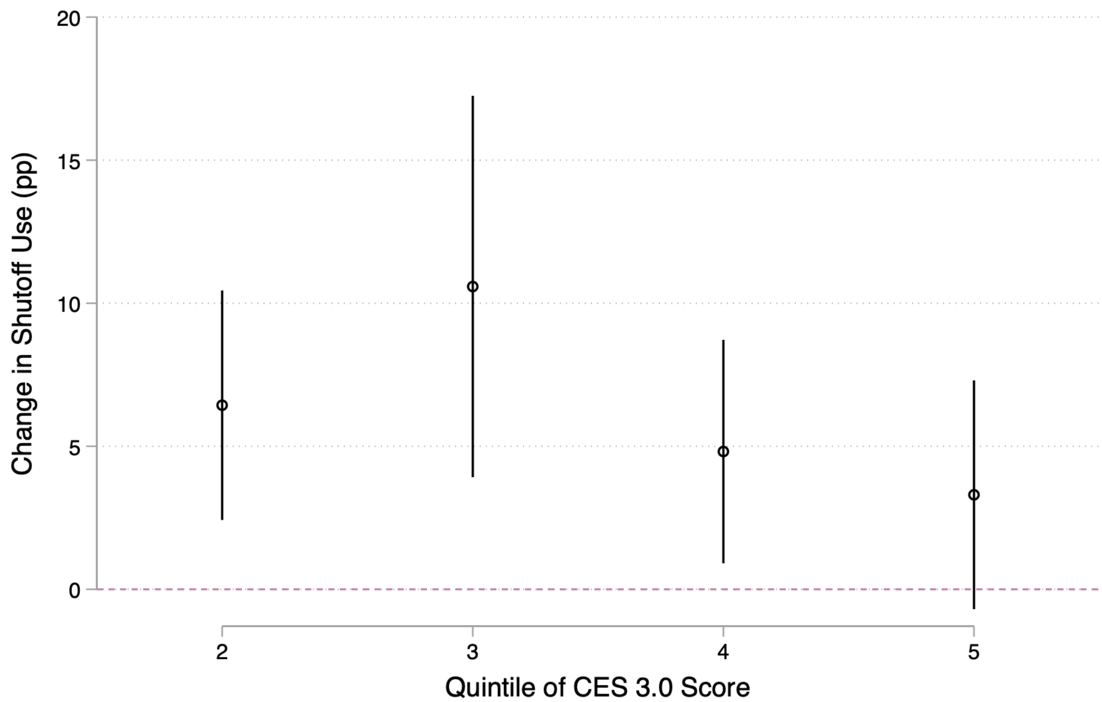
(c) Temperature



(d) Cumulative Precipitation

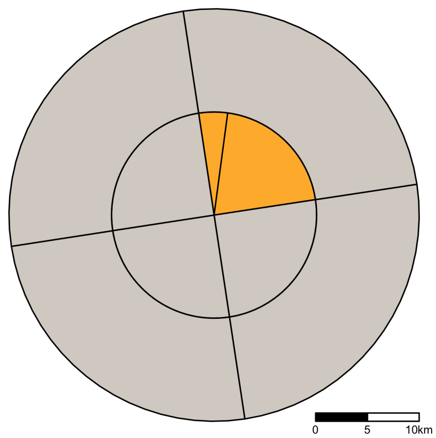
Notes: Estimated effect on shutoff use (in percentage points) of shifting liability for power line-ignited fire damages from electricity consumers to utilities by septile of daily climate conditions. Coefficient estimates are plotted with their 95% confidence intervals. Each coefficient is interpreted relative to the estimated effect on days in the lowest septile of each climate condition. The figure is created by estimating a version of regression model 1.7 where treatment is interacted with binned climate conditions on a daily panel of distribution circuits operated by San Diego Gas and Electric between 2013 and 2020. Daily climate conditions are from weather stations operated by San Diego Gas and Electric along their power lines. Standard errors are clustered at the high fire threat district by calendar week level to allow for correlation in shutoff use across circuits with similar ignition risk during the same week.

Figure A.3: Effect of 2017 Rule Change on Shutoff Declaration by Socioeconomic Status

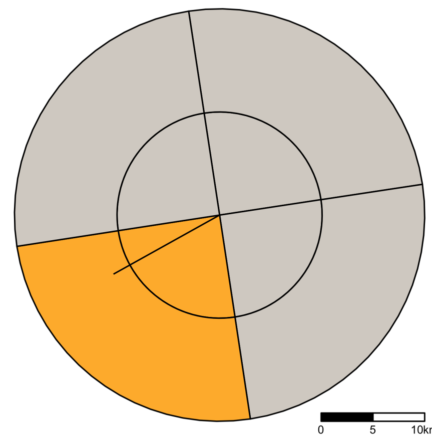


Notes: Estimated effect on shutoff use (in percentage points) of shifting liability for power line-ignited fire damages from electricity consumers to utilities by quintile CalEnviroscreen (CES) 3.0 score. The CES score is a composite index used by the California state government to rank census tracts by pollution exposure, demographic characteristics, and socioeconomic characteristics. The top 25% of census tracts based on the CES 3.0 score are defined as disadvantaged. Coefficient estimates are plotted with their 95% confidence intervals. Each coefficient is interpreted relative to the estimated effect at circuits in census tracts that are the least disadvantaged (lowest CES score). The figure is created by estimating a version of regression model 1.7 where treatment is interacted with binned CES scores on a daily panel of census tracts containing distribution circuits operated by San Diego Gas and Electric between 2013 and 2020. Daily climate conditions are from weather stations operated by San Diego Gas and Electric along their power lines. Standard errors are clustered at the high fire threat district by calendar week level to allow for correlation in shutoff use across circuits with similar ignition risk during the same week.

Figure A.4: Example of Daily Variation in Replacement Cost at the Circuit Level



(a) Sep. 23, 2019



(b) Sep. 24, 2019

Notes: Daily variation in which regions are downwind of a circuit operated by Pacific Gas and Electric on September 23 and 24, 2019. The centroid of the circuit is the center of the circle, and it is encircled by 10 and 20 kilometer buffers. Each buffer is divided into 4 regions, creating 8 possible downwind regions for each day between 2018-2020. Yellow shaded regions are downwind of the circuit on each day, while the tan regions are not downwind. The black line indicates which direction the wind is blowing and its length indicates how strongly the wind is blowing. The black line is created using maximum daily wind speed and direction, an estimate of how far the wind can carry a lit ember from Albini et al. (2012), and several trigonometric identities.

A.6 Appendix Tables

Table A.1: Effect of Replacement Costs on Shutoff Probability at the Circuit Level

	Shutoff Indicator (1)	Customer Hours (2)
Value x DW	0.05**	269.25*
	(0.02)	(142.88)
Controls	x	x
Pair FE	x	x
Day FE	x	x
Mean of Dep. Var	0.024	450.459
1 SD Effect	1.788	9,646.735
Observations	105,273	105,273

Notes: Estimates are from a regression of a binary variable equal to one if there is an active shutoff event at circuit i on day t on the total (column 1) or median (column 2) replacement cost in regions that are downwind of circuit i on day t. Both regressions control for septiles of maximum wind speed, maximum temperature, average relative humidity, and cumulative precipitation at circuit i on day t. Furthermore, both regressions include calendar day and circuit-downwind region pair fixed effects. Each circuit has 8 potentially downwind regions as shown in figure A.4. Standard errors are clustered at the high fire threat district by calendar week level.

Table A.2: Effect of Total Replacement Cost on the Probability of a Shutoff by Socioeconomic Status

	Total Value (1)
DAC_o x Value x DW	-0.015 (0.015)
DAC_d x Value x DW	-0.004 (0.019)
DW	0.041** (0.018)
Value x DW	0.024*** (0.009)
Controls	x
Pair FE	x
Day FE	x
Mean of Dep. Var	0.025
1 SD Effect	-0.191
Observations	505,656

Notes: Wind and weather data is taken from weather stations operated by utilities in California and interpolated to each distribution circuit centroid using inverse distance weighting. Total replacement costs are taken from the Zillow ZTRAX dataset and converted to 2021 dollars. Disadvantaged community status is taken from the CalEnviroScreen 2018 data release. The underlying data consists of pairs of upwind, ever-downwind zip codes for every day during January and April-December 2018-2020. The outcome is a binary variable equal to 1 if a shutoff event is active in origin zip code o. Value measures the total cost of replacing structures in each destination zip code d and DW is a binary variable equal to 1 when zip code d is downwind of zip code o on day t. I code an origin zip code as a disadvantaged community if more than 50% of its population lives in a census tract designated as a DAC by the California government. Controls include daily average temperature, relative humidity, precipitation, and maximum wind speed binned by septiles for each origin zip code o and destination zip code d. Standard errors are clustered at the high fire threat district by calendar week level.

Table A.3: Effect of Structure Replacement Cost on Shutoffs by Firm

	Total Value	Mean Value
	(1)	(2)
Value x DW x PGE	0.01	0.01
	(0.01)	(0.02)
Value x DW x SDGE	0.04***	0.15***
	(0.01)	(0.04)
Value x DW x SCE	-0.01	-0.06
	(0.02)	(0.04)
DW x PGE	-0.01	-0.01
	(0.02)	(0.02)
DW x SDGE	0.07**	0.09**
	(0.03)	(0.03)
DW x SCE	0.04	0.03
	(0.03)	(0.03)
Controls	x	x
Pair FE	x	x
Day FE	x	x
Mean of Dep. Var	0.025	0.025
Pooled Estimate	r(estimate)	r(estimate)
Bootstrap 95% CI of Pooled Estimate	[00,0.029]	[00,0.090]
Observations	505,656	505,656

Notes: Wind and weather data is taken from weather stations operated by utilities in California and interpolated to each distribution circuit centroid using inverse distance weighting. Total replacement costs are taken from the Zillow ZTRAX dataset and converted to 2021 dollars. The underlying data consists of pairs of upwind, ever-downwind zip codes for every day during January and April-December 2018-2020. The outcome is a binary variable equal to 1 if a shutoff event is active in origin zip code o. Value measures the total cost of replacing structures in each destination zip code d and DW is a binary variable equal to 1 when zip code d is downwind of zip code o on day t. The variables PGE, SDGE, and *Southern California Edison* equal one for observations from Pacific Gas and Electric, San Diego Gas and Electric, and Southern California Edison. Controls include daily average temperature, relative humidity, precipitation, and maximum wind speed binned by septiles for each origin zip code o and destination zip code d. Standard errors are clustered at the high fire threat district by calendar week level. The "Pooled Estimate" is a weighted average of the estimates in rows 1, 2, and 3 where the weights are the utility's proportion of observations in the sample. Since there are 20 effective clusters in this analysis, I construct a bootstrapped 95% confidence interval for the pooled estimate following Cameron, Gelbach and Miller (2008).

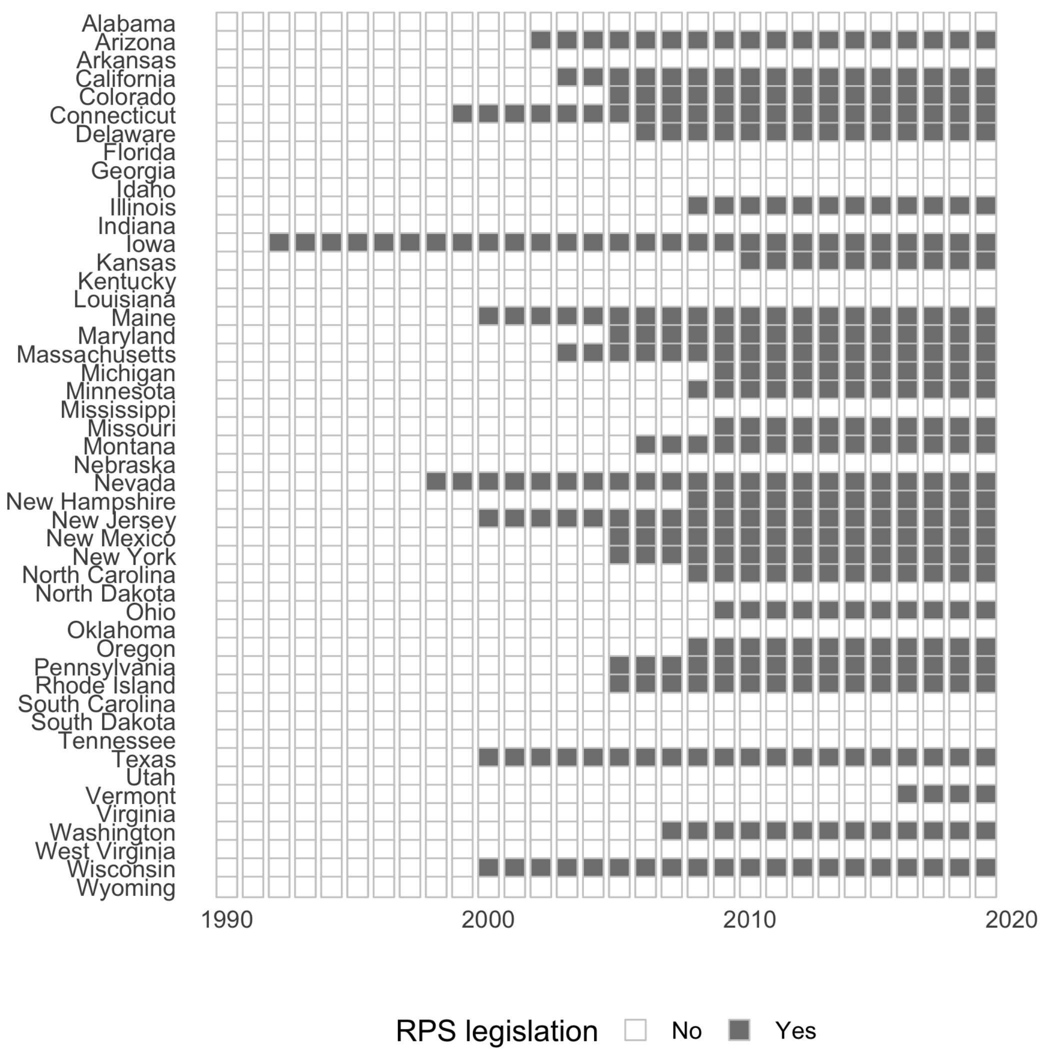
Appendix B

Appendix for "Causal Effects of Renewable Portfolio Standards on Renewable Investments and Generation: The Role of

Heterogeneity and Dynamics"

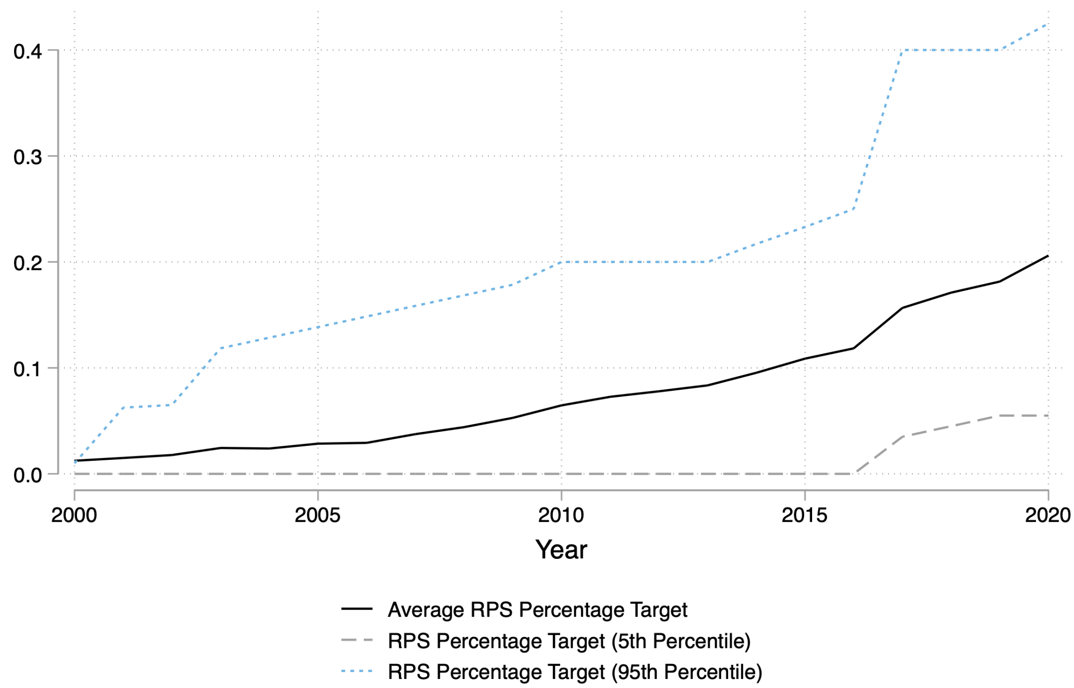
B.1 Figures

Figure B.1: Year of RPS Adoption by State



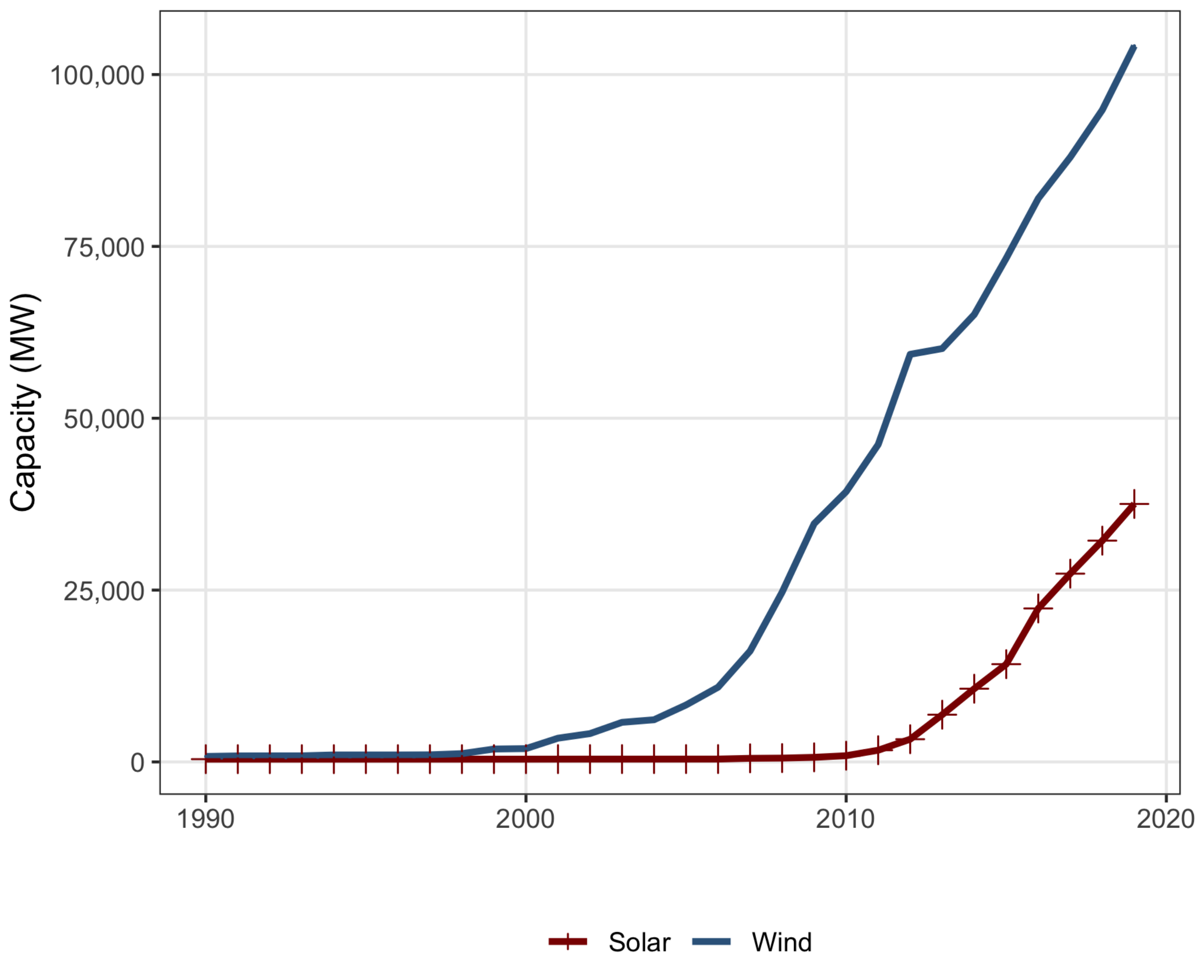
Notes: Each box is shaded gray starting in the first year that a state adopts any RPS policy. Information on RPS adoption date was taken from Greenstone and Nath (2020).

Figure B.2: Nominal RPS Percentage Targets Over Time



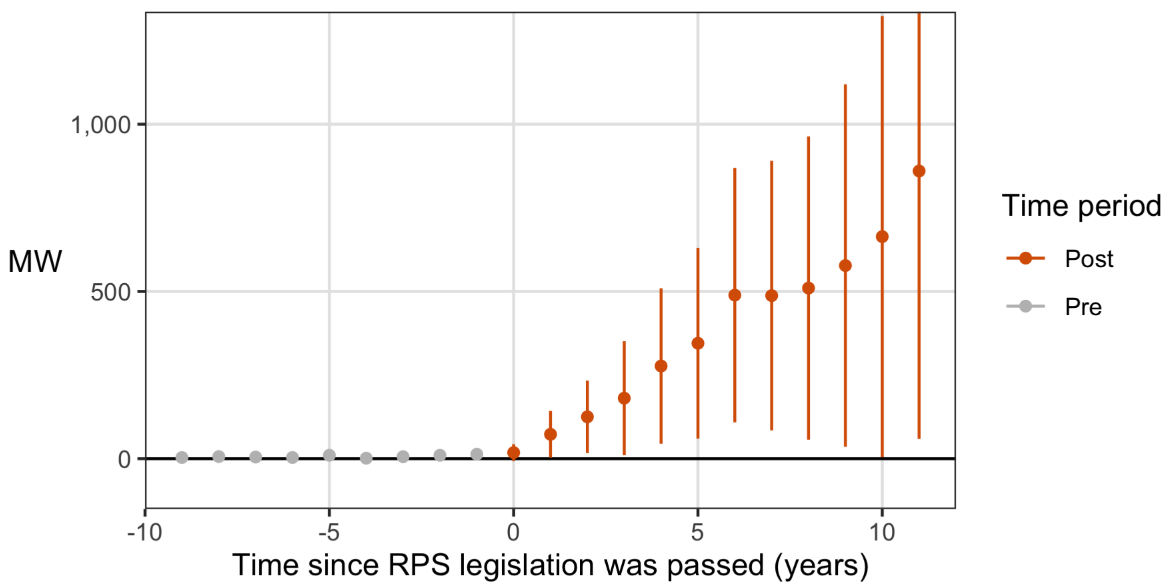
Notes: This figure shows the nominal RPS percentage targets over time based on data reported in Barbose (2021). Nominal RPS percentage targets measure the percent of applicable retail electricity sales required to be generated by renewable sources. Since the definition of a renewable resource, type of regulated entity (e.g. public vs. privately owned utilities), and incentives for certain types of renewable generation differ considerably across states, comparison of targets across states is inadvisable. This figure shows that targets have increased in stringency over time and vary widely across states.

Figure B.3: Annual Renewable Electricity Generation Capacity (MW)



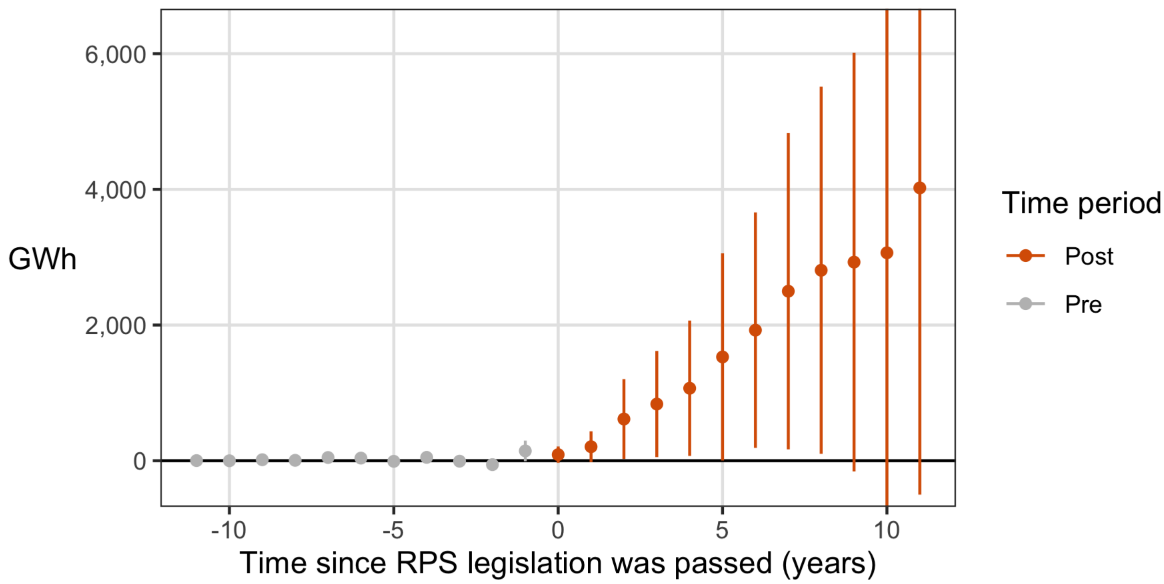
Notes: The blue (red with '+'s) lines plot the level of installed wind (solar) generation capacity in the continental U.S. annually between 1990 and 2019. Information on capacity installations by generation source was taken from the EIA Form 860 database.

Figure B.4: Estimated Dynamic Treatment Effects of RPSs on Installed Wind Capacity (MW)



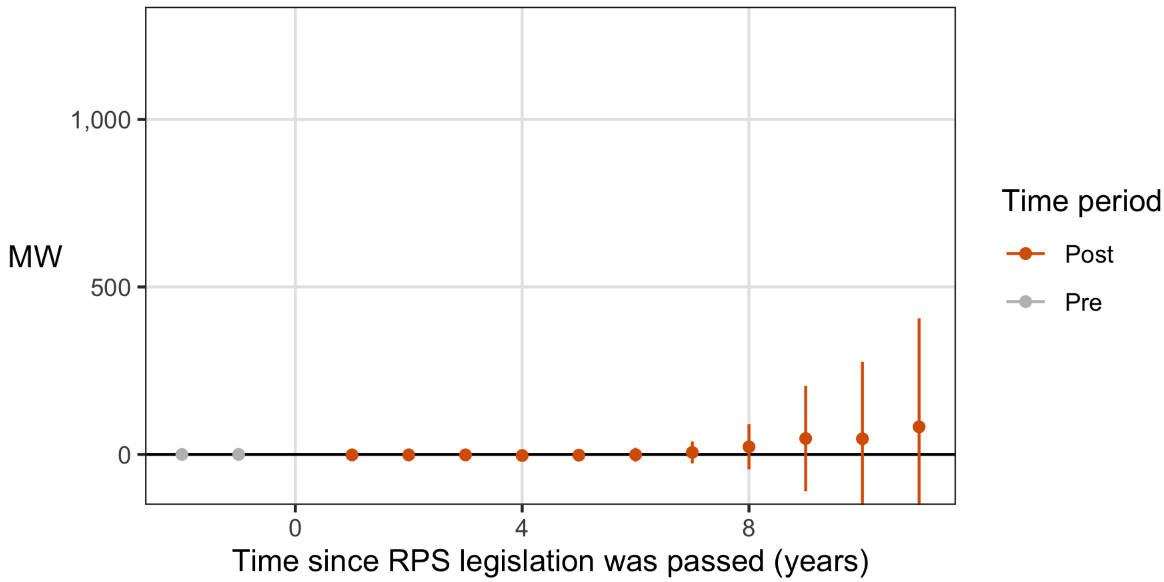
Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period (t). The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the state level. We include time invariant controls for wind potential, solar irradiance, length of transmission lines per square kilometer of state area, 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price per kilowatt hour of electricity in 1990 at the state level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown in gray.

Figure B.5: Estimated Dynamic Treatment Effects of RPSs on Wind Electricity Generation (GWh)



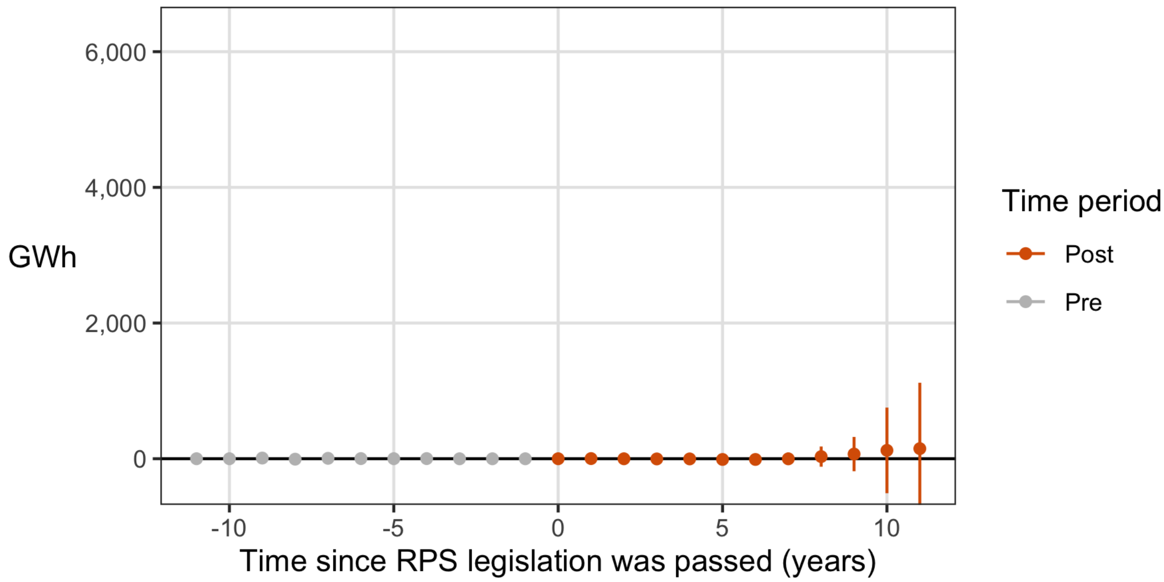
Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period (t). The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the state level. We include time invariant controls for wind potential, solar irradiance, length of transmission lines per square kilometer of state area, 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price per kilowatt hour of electricity in 1990 at the state level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown in gray.

Figure B.6: Estimated Dynamic Treatment Effects of RPSs on Installed Solar Capacity (MW)



Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period (t). The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the state level. We include time invariant controls for wind potential, solar irradiance, length of transmission lines per square kilometer of state area, 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price per kilowatt hour of electricity in 1990 at the state level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown in gray.

Figure B.7: Estimated Dynamic Treatment Effects of RPSs on Solar Electricity Generation (GWh)



Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period (t). The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the state level. We include time invariant controls for wind potential, solar irradiance, length of transmission lines per square kilometer of state area, 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price per kilowatt hour of electricity in 1990 at the state level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown in gray.

B.2 Tables

Table B.1: Summary Statistics

	(1) RPS states	(2) Non RPS states	(3) Difference
Number of states	30	19	11
A. Infrastructure & Endowments			
Transmission lines (km per kmˆ2)	0.16	0.14	0.02
Wind speed (meter per second)	6.3	6.1	0.2
Solar irradiance (kWh / mˆ2 /year)	4.3	4.6	-0.2
B. Installed Capacity (MW)			
Wind	785.0	518.8	266.6
Solar	166.5	40.9	125.56
Coal	6,415.4	7,291.0	-875.7
Gas	8,480.1	6,953.7	1,526.3
Total	20,795	19,192	1,603.1
C. Generation (GWh)			
Wind	4,095	2,971	1,124
Solar	599	131	467
Coal	71,857	80,294	-8,436
Gas	37,512	28,911	8,601
Total	78,747	74,377	4,369
D. Other Predictors			
GDP per capita	57,143	47,787	-11,356**
Electricity price (all end-use, $ / kWh)	0.12	0.09	0.03***
Electricity consumption (Bil. kWh)	72.9	62.4	10.5
House LCV score	56.4	27.4	29.0***
Senate LCV score	61.7	27.9	33.8***
Fraction counties non-attainment	0.53	0.17	0.36***

Notes: RPS states adopted any type of RPS legislation between 1990 and 2019 while Non-RPS states have never adopted any type of RPS legislation. Only states in the continental U.S. are included in the sample. All dollar dominated variables are in 2019 constant dollars. Column 3 reports the mean difference between RPS and non-RPS states for each variable and the stars indicate a significant difference across groups at the 0.05, 0.01, and 0.001 significance levels (*** $p<0.001$, ** $p<0.01$, * $p<0.05$).

Table B.2: Estimated ATT of RPSs Impact on Installed Wind Capacity and Generation

	(1)	(2)	(3)
Panel A: Capacity (MW)			
2* Overall ATT (cohort)	380*	264	586**
	(158)	(155)	(218)
Overall ATT (year)	394*	307	710*
	(198)	(218)	(282)
1-5 years post	195*	129	197*
	(77)	(79)	(98)
6-11 years post	596*	417	1000**
	(262)	(275)	(353)
Panel B: Generation (GWh)			
Overall ATT (cohort)	1790*	1160	3110**
	(910)	(797)	(1220)
Overall ATT (year)	1740	1340	3700*
	(1050)	(1090)	(1550)
1-5 years post	838*	521	980*

	(361)	(376)	(443)
6-11 years post	2870*	1880	5350**
	(1470)	(1490)	(2040)
Controls			
Endowments		Yes	Yes
Sociopolitical			Yes
Observations	810	810	780

Table B.2: **Notes:** Overall ATT (cohort) corresponds to the average effect of RPS policies experienced by all states that ever implement an RPS. Overall ATT (year), corresponds to the average effect of implementing an RPS policy for states that have implemented an RPS for at least 11 years. "1-5 years post" and "6-11 years post" are equivalent to Overall ATT (year), except that they are computed separately for post-implementation years 1-5 and 6-11 respectively. Standard errors are computed using a multiplier bootstrap procedure and clustered at the state level following Callaway and Sant'Anna (2021) (*** p<0.001, ** p<0.01, * p<0.05). Column 1 reports the unconditional estimates. Column 2 adds natural endowment controls for wind potential, solar irradiance, and length of transmission lines. Column 3 further introduces sociopolitical controls including 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price of electricity in 1990. Panel A reports estimates for megawatts of installed wind capacity as the outcome and panel B reports estimates for gigawatt-hours of wind electricity generation as the outcome.

Table B.3: Estimated ATT of RPSs Impact on Installed Solar Capacity and Generation

	(1)	(2)	(3)
Panel A: Capacity (MW)			
2* Overall ATT (cohort)	16.3	48.3	29.7
	(34.2)	(36.2)	(38)
Overall ATT (year)	50.1	71	43
	(51.3)	(49.4)	(54.4)
1-5 years post	-1.84	4.7	2.64
	(3.73)	(3.62)	(4.56)
6-11 years post	34.1	92.7	57.3
	(70.3)	(70.5)	(76.9)
Panel B: Generation (GWh)			
Overall ATT (cohort)	28.7	142	92
	(84.1)	(94.7)	(108)
Overall ATT (year)	119	195	114
	(138)	(129)	(149)
1-5 years post	-2.89	14.5	10.4

	(1)	(2)	(3)
	(9.92)	(10.7)	(13.9)
6-11 years post	59.8	272	175
	(174)	(183)	(226)
Controls			
Endowments		Yes	Yes
Sociopolitical			Yes
Observations	810	810	780

Table B.3: **Notes:** Overall ATT (cohort) corresponds to the average effect of RPS policies experienced by all states that ever implement an RPS. Overall ATT (year), corresponds to the average effect of implementing an RPS policy for states that have implemented an RPS for at least 11 years. "1-5 years post" and "6-11 years post" are equivalent to Overall ATT (year), except that they are computed separately for post-implementation years 1-5 and 6-11 respectively. Standard errors are computed using a multiplier bootstrap procedure and clustered at the state level following Callaway and Sant'Anna (2021) (*** $p<0.001$, ** $p<0.01$, * $p<0.05$). Column 1 reports the unconditional estimates. Column 2 adds natural endowment controls for wind potential, solar irradiance, and length of transmission lines. Column 3 further introduces sociopolitical controls including 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price of electricity in 1990. Panel A reports estimates for megawatts of installed solar capacity as the outcome and panel B reports estimates for gigawatt-hours of solar electricity generation as the outcome.

2.3 Data Appendix

Table 2.5: Variable metadata

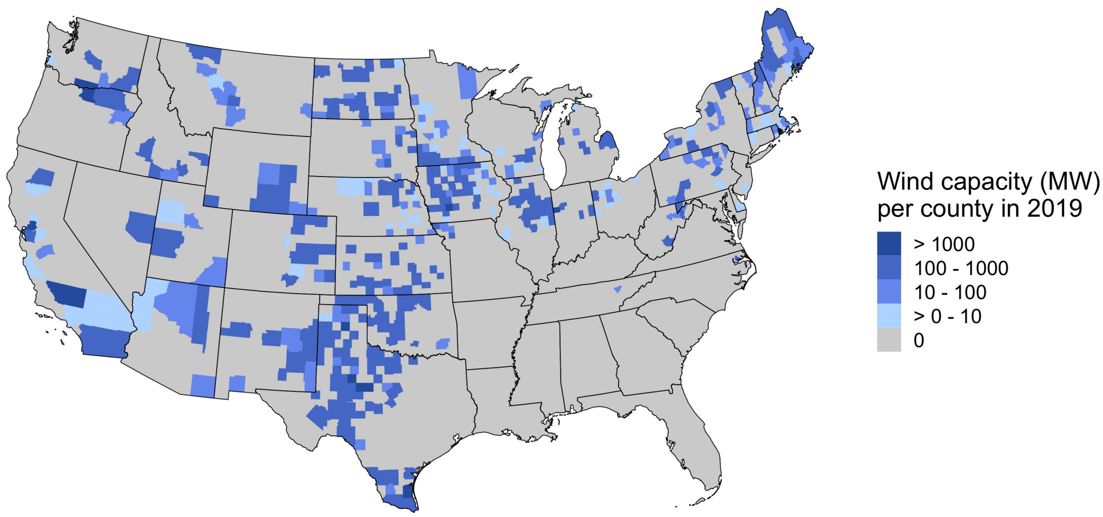
Variable	Units	Source
Transmission lines	km per kmˆ2	Homeland Infrastructure Foundation-Level Data (HIFLD)
Wind speed	meters per second	NREL Wind Integration National Dataset (WIND)
Solar irradiance	kWh / mˆ2 /year	NREL Physical Solar Model version 3 Global Horizontal Irradiance Multi-year Annual Average
Installed capacity	MW	EIA Form EIA-860
Generation	GWh	EIA Form EIA-906
GDP per capita	$ per person	Bureau of Economic Analysis (BEA) dataset SAGDP2N
Electricity price	all end-use, $ / kWh	EIA State Energy Data System (SEDS)
Electricity consumption	Bil. kWh	EIA State Energy Data System (SEDS)
House LCV score	Scale $[0, 100]$	League of Conservation Voters (LCV) Scorecard
Senate LCV score	Scale $[0, 100]$	League of Conservation Voters (LCV) Scorecard
Fraction counties non-attainment	Share $[0, 1]$	Environmental Protection Agency (EPA) Greenbook

Chapter 3

Appendix for "Can the Low-Carbon Transition Energize Labor Markets? Evidence from Wind Electricity Investments in the U.S."

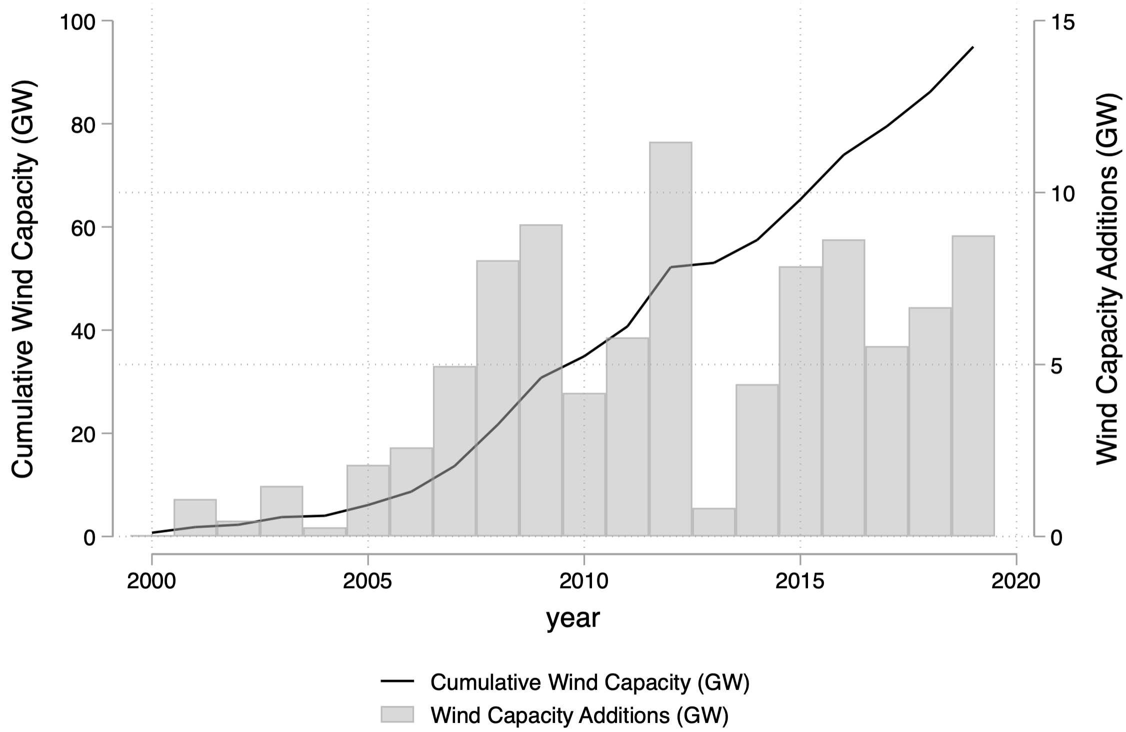
3.1 Figures

Figure 3.1: Active Wind Generation Capacity by County in 2019



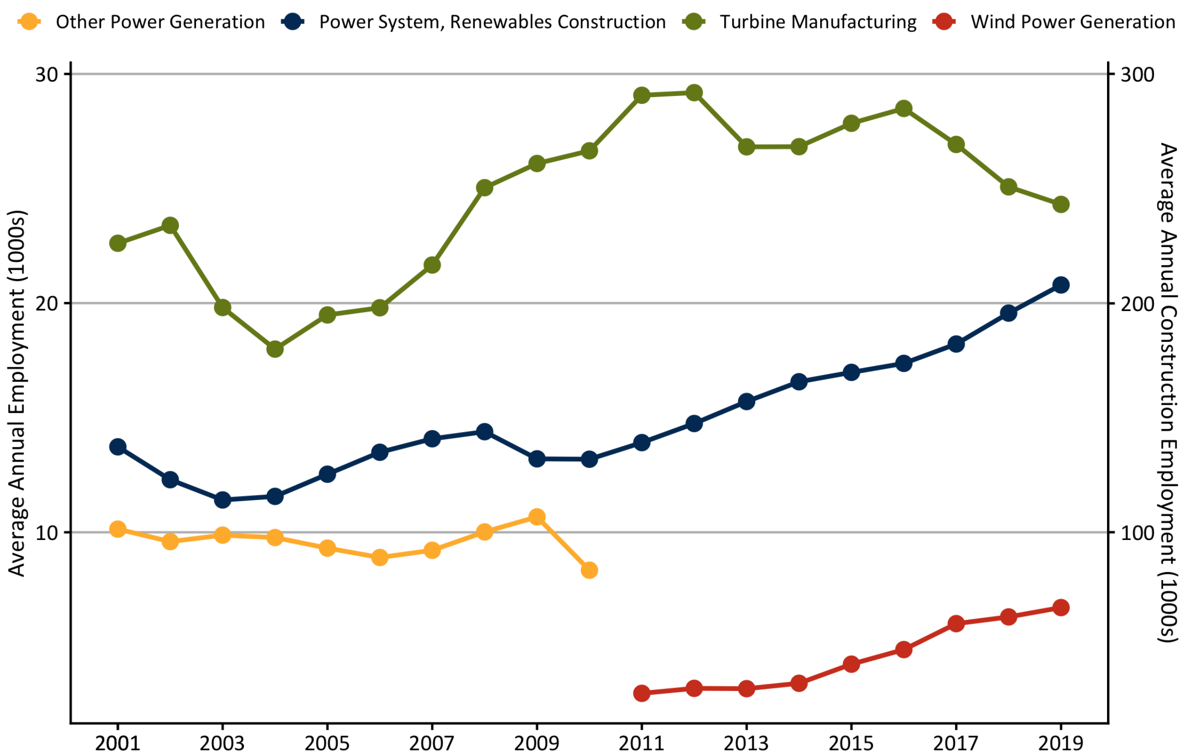
Notes: Data are from the EIA 860 database.

Figure 3.2: Annual Trend in Wind Electricity Generation



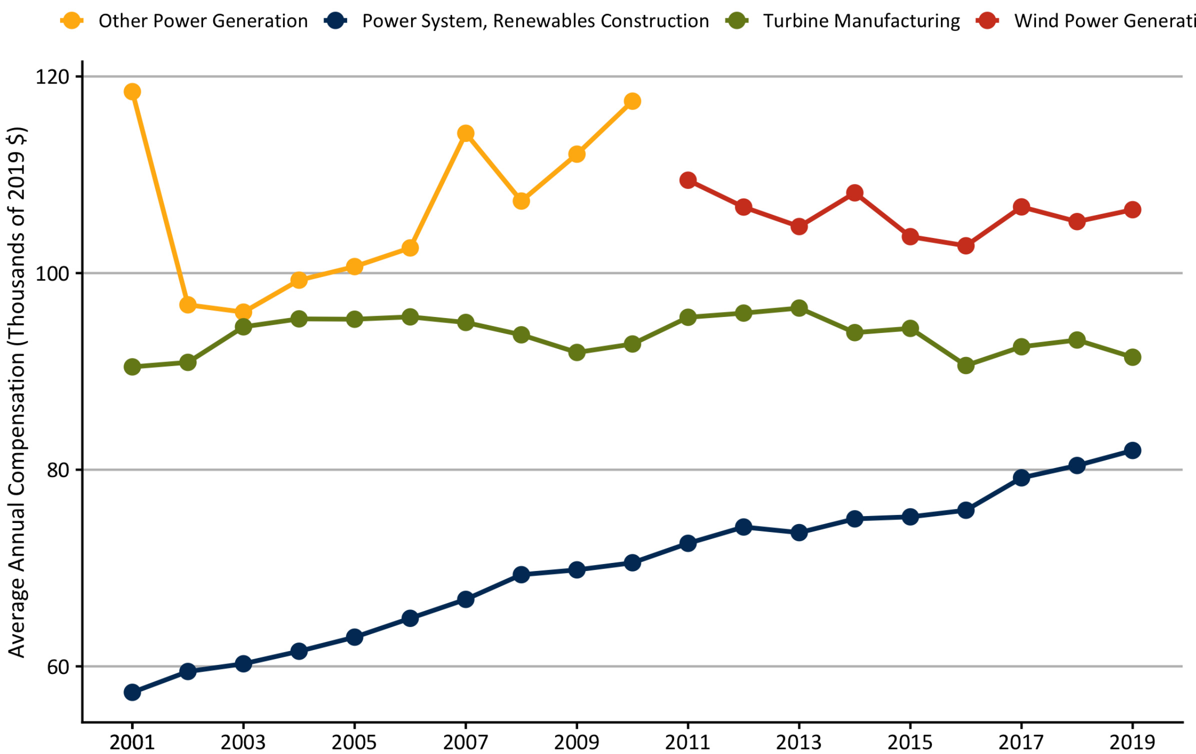
Notes: Data on wind capacity is reported in the Energy Information Administration form 860 database. The 860 data include wind generators with nameplate capacity exceeding 1 MW across the United States. Each capacity addition date refers to the date that a wind generator began operating. Our sample includes counties from 40 U.S. states: We exclude Alaska and Hawaii from our final sample because their energy sectors are meaningfully different to the rest of the U.S.. The 8 other excluded states (AL, AR, AZ, KY, MA, MS, NH, WY, OH, MI, NY, OK, VT, and WY) do not share labor market data with the Census Bureau for at least 1 year during our sample and are thus excluded.

Figure 3.3: Annual Employment in Industries Closely Related to Wind Investments



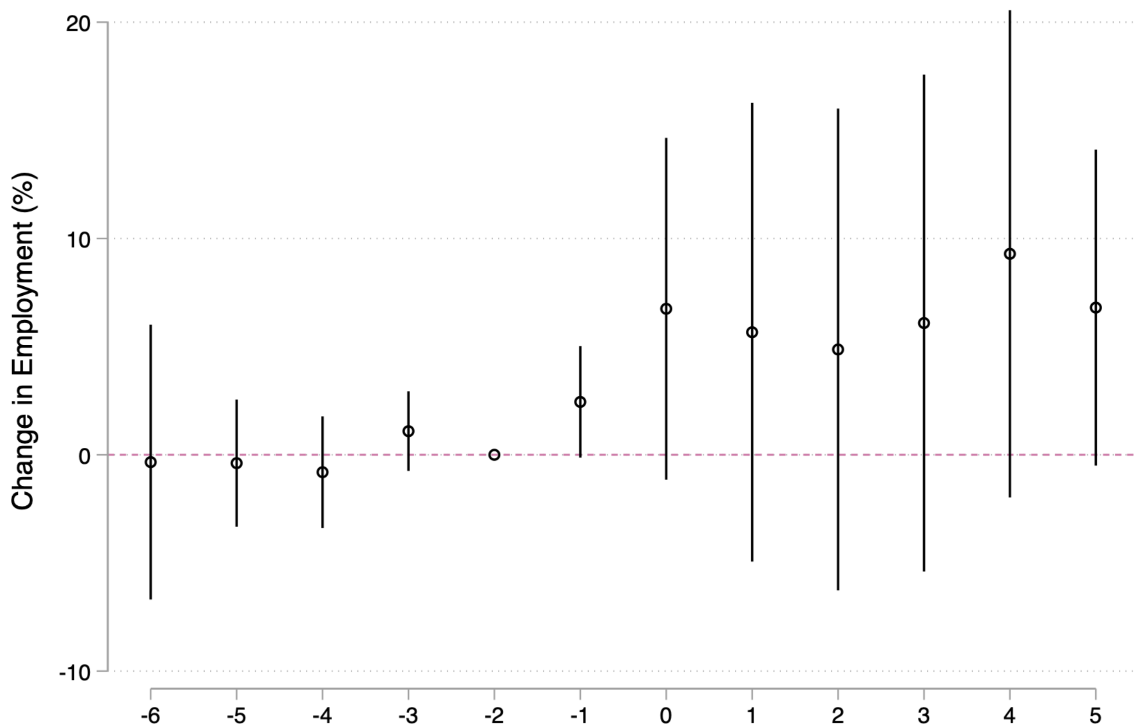
Notes: Data are from the Bureau of Labor Statistics QCEW database. Each data point represents the total employment in each industry across all 50 U.S. states. Prior to 2011, the BLS reported wind power generation employment together with employment in the solar and tidal electricity generation industries under "Other Power Generation." The sector "Power System, Renewables Construction" includes workers involved in the construction of wind projects as well as workers involved in stringing power lines, building solar generation structures, constructing power plants, and other energy-related construction. "Turbine Manufacturing" includes wind, steam, hydraulic, and gas turbine manufacturing employment.

Figure 3.4: Average Annual Compensation in Industries Closely Related to Wind Investments



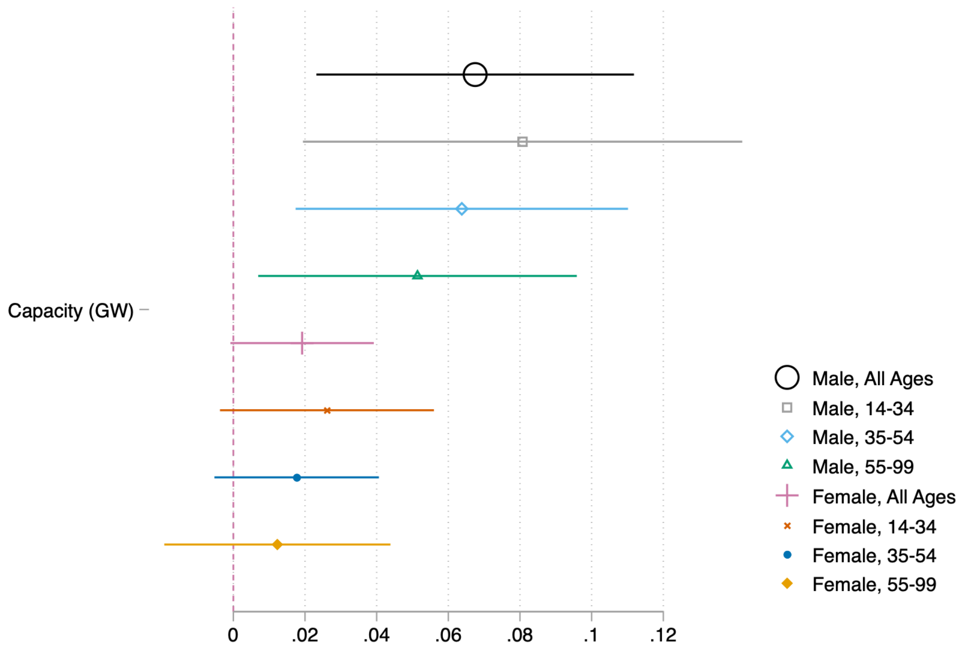
Notes: Data are from the Bureau of Labor Statistics QCEW database. Each data point represents the average annual compensation in 2019 dollars in each industry across all 50 U.S. states. According to the BLS, "Under most state laws or regulations, wages include bonuses, stock options, severance pay, the cash value of meals and lodging, tips and other gratuities. In some states, wages also include employer contributions to certain deferred compensation plans, such as 401(k) plans." Prior to 2011, the BLS reported wind power generation employment together with employment in the solar and tidal electricity generation industries under "Other Power Generation." The sector "Power System, Renewables Construction" includes workers involved in the construction of wind projects as well as workers involved in stringing power lines, building solar generation structures, constructing power plants, and other energy-related construction. "Turbine Manufacturing" includes wind, steam, hydraulic, and gas turbine manufacturing employment.

Figure 3.5: Cumulative Effect of Wind Capacity Investments on Log Employment



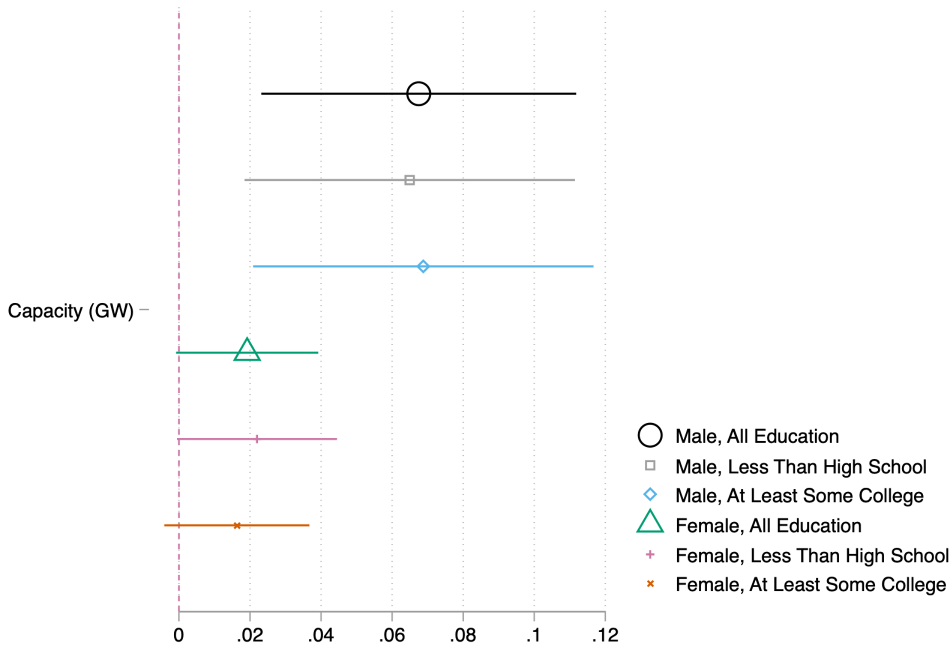
Notes: Estimates and their associated 95% confidence intervals are plotted against event time. Each estimate is computed by cumulatively summing coefficients from a distributed lag model (DL) according to Schmidheiny and Siegloch (2020). For example, the coefficient at t=4 is computed by adding the DL coefficients from t=0 to t=4. Thus, each estimate represents the total effect of a wind capacity investment on employment at event time t. Since t=0 is the date a wind investment begins generating power, we normalize event time coefficients relative to t=-2 to allow for construction-related changes in labor market conditions. The p-value from a joint test of significance of the pre-period coefficients (t=-6 to t=-3) is 0.6 and the estimate of wind investments' effects on employment from the baseline TWFE specification is 0.05.

Figure 3.6: Effect of Wind Capacity Investments on Log Employment By Sex and Age



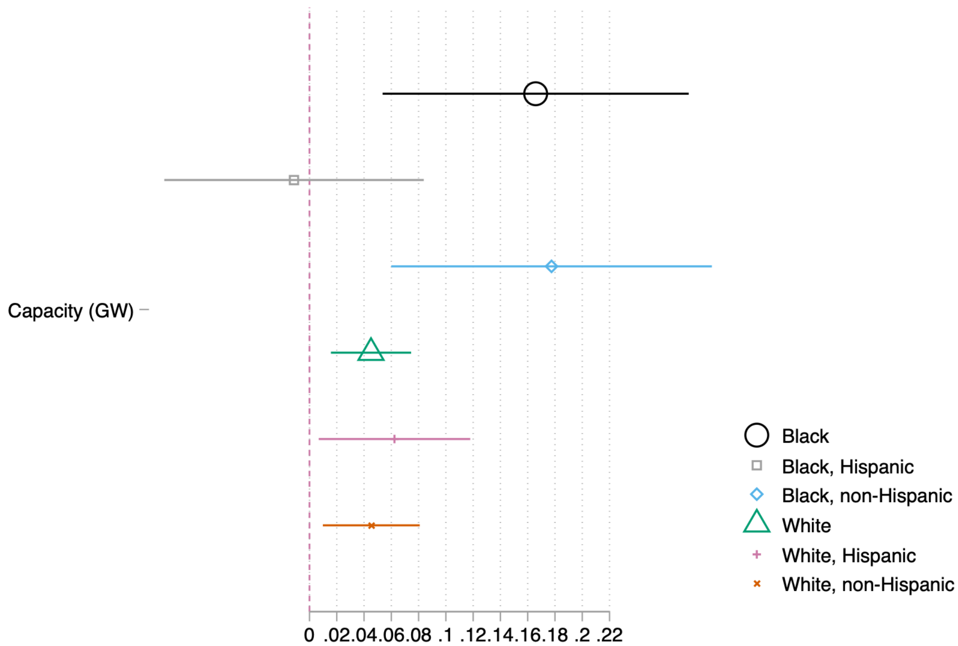
Notes: Each point estimate and associated 95% confidence interval is separately estimated with a two-way fixed effects regression model on an annual panel of counties in our preferred sample. Each regression model controls for county and state-by-year fixed effects, a binary indicator for whether a county is in non-attainment under the U.S. Clean Air Act Amendments, and the logged county-level population between ages 20 and 69. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)). The estimates reflect the percent change in county-level employment that is associated with a 1 GW increase in wind generation capacity. Labor market outcomes by county and worker demographic characteristics come from the U.S. Census Bureau's Quarterly Workforce Indicators database. Information on wind electricity generation investments across the U.S. comes from the U.S. Energy Information Administration Form 860 database.

Figure 3.7: Effect of Wind Capacity Investments on Log Employment By Sex and Education



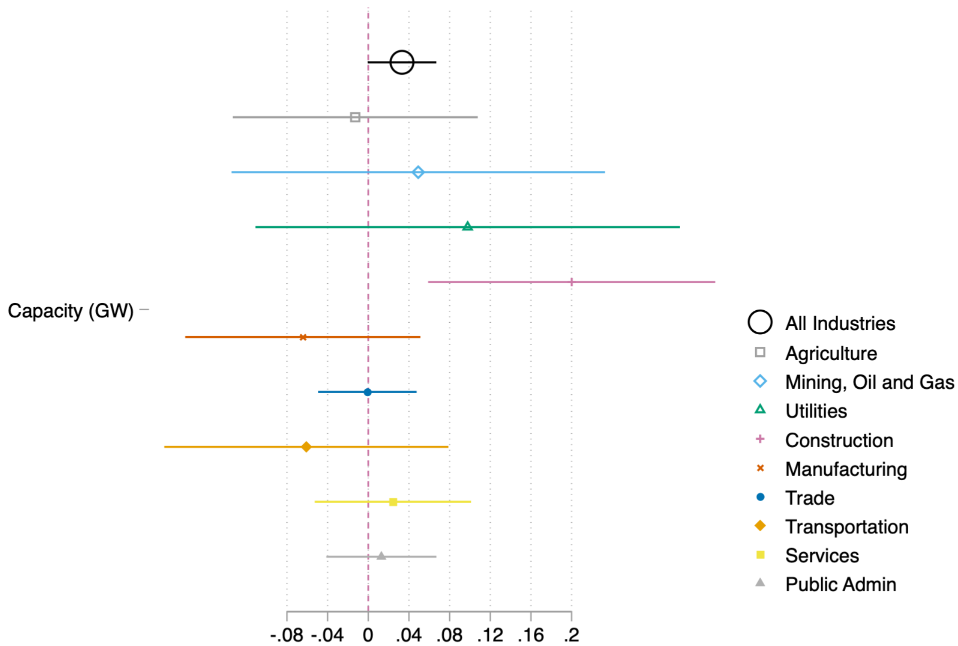
Notes: Each point estimate and associated 95% confidence interval is separately estimated with a two-way fixed effects regression model on an annual panel of counties in our preferred sample. Each regression model controls for county and state-by-year fixed effects, a binary indicator for whether a county is in non-attainment under the U.S. Clean Air Act Amendments, and the logged county-level population between ages 20 and 69. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)). The estimates reflect the percent change in county-level employment that is associated with a 1 GW increase in wind generation capacity. Labor market outcomes by county and worker demographic characteristics come from the U.S. Census Bureau's Quarterly Workforce Indicators database. Information on wind electricity generation investments across the U.S. comes from the U.S. Energy Information Administration Form 860 database.

Figure 3.8: Effect of Wind Capacity Investments on Log Employment By Race



Notes: Each point estimate and associated 95% confidence interval is separately estimated with a two-way fixed effects regression model on an annual panel of counties in our preferred sample. Each regression model controls for county and state-by-year fixed effects, a binary indicator for whether a county is in non-attainment under the U.S. Clean Air Act Amendments, and the logged county-level population between ages 20 and 69. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)). The estimates reflect the percent change in county-level employment that is associated with a 1 GW increase in wind generation capacity. Labor market outcomes by county and worker demographic characteristics come from the U.S. Census Bureau's Quarterly Workforce Indicators database. Information on wind electricity generation investments across the U.S. comes from the U.S. Energy Information Administration Form 860 database.

Figure 3.9: Effect of Wind Capacity Investments on Log Employment By Industry



Notes: Each point estimate and associated 95% confidence interval is separately estimated with a two-way fixed effects regression model on an annual panel of counties in our preferred sample. Each regression model controls for county and state-by-year fixed effects, a binary indicator for whether a county is in non-attainment under the U.S. Clean Air Act Amendments, and the logged county-level population between ages 20 and 69. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)). The estimates reflect the percent change in county-level employment that is associated with a 1 GW increase in wind generation capacity. Labor market outcomes by county and worker demographic characteristics come from the U.S. Census Bureau's Quarterly Workforce Indicators database. Information on wind electricity generation investments across the U.S. comes from the U.S. Energy Information Administration Form 860 database.

3.2 Tables

Table 3.1: Summary Statistics

	Full Sample	Preferred Sample		
		All	Ever-Wind	Never-Wind
Renewable Potential Characteristics				
Capacity Wind (MW)	14.9	15.7	93.0	0.0
	(90.89)	(85.25)	(189.69)	(1.05)
Capacity Solar (MW)	2.6	1.9	4.4	1.4
	(42.61)	(31.75)	(61.70)	(20.98)
Wind Speed (m/s)	6.4	6.5	7.0	6.4
	(0.77)	(0.65)	(0.50)	(0.63)
Solar Irradiance (kWh/m^2)	4.5	4.4	4.5	4.4
	(0.48)	(0.47)	(0.59)	(0.44)
Transmission Lines (km/km^2)	0.4	0.4	0.3	0.4
	(0.37)	(0.35)	(0.20)	(0.37)
Baseline Characteristics (2000)				
Population	94,469.5	77,468.4	56,987.9	81,615.2
Population Density	101.4	60.6	27.7	67.2
Employment	42,030.8	34,331.7	23,314.7	36,562.4
Median House Value ($ 2019)	122,810.1	117,585.0	100,383.6	121,067.8
Senate LCV Score	42.7	42.3	40.9	42.6
House LCV Score	37.7	37.8	41.8	37.0
Electricity Price ($ 2019/kWh)	0.09	0.09	0.10	0.09
County GDP ($ 2019 Millions)	2,508.4	1,933.8	1,249.8	2,074.8
Per Capita Income ($ 2019)	18,058.4	18,042.3	18,025.2	18,045.8
Unemployment Rate (1995-99)	5.3	5.2	4.8	5.3
N Counties	2,699	2,334	393	1,941
Observations	53,980	46,680	7,860	38,820

Notes: The preferred sample includes all counties where the population in 2000 normalized by county area is less than 1,000, wind speed is greater than 5.36 meters per second, and the length of transmission lines normalized by county area exceeds 0.04. Wind and solar capacity are reported in the EIA Form 860 database. County-level wind speed is reported by NREL in the Western Wind Dataset and county-level solar irradiance is reported by NREL in its National Solar Radiation Database. Data on transmission lines is reported by the Department of Homeland Security in its Homeland Infrastructure Foundation-Level dataset. County-level population is reported by the National Cancer Institute Surveilance, Epidemiology, and End Results (SEER) program. Employment is reported by the Census Bureau in its QWI database. Per capita income and median housing values for each county are collected by the IPUMS NHGIS program. Senate and House League of Conservation scores were collected from the LCV's website. Electricity prices are taken from the EIA's SEDS database. County-level GDP and income per capita are from the BEA. All dollar values are converted to 2019 dollars using the BEA's implicit price deflator.

Table 3.2: Impact of Wind Capacity Investments on Log Employment

	(1)	(2)	(3)	(4)	(5)
Wind Capacity (GW)	0.053**	0.080**	0.045**	0.046**	0.035*
	(0.017)	(0.022)	(0.014)	(0.014)	(0.016)
Log Population (20-69)		0.877***	0.890***	0.891***	0.789***
		(0.037)	(0.034)	(0.034)	(0.037)
Solar Capacity (GW)				-0.043	-0.180
				(0.034)	(0.095)
Non-attainment indicator				0.004	0.006
				(0.005)	(0.006)
Senate LCV Score					-0.000
					(0.000)
House LCV Score					-0.000
					(0.000)
County FE	x	x	x	x	x
Year FE	x	x			
State x Year FE			x	x	
CZ x Year FE					x
Mean Employment	35,499	35,499	35,499	35,499	35,499
Observations	46,680	46,680	46,680	46,680	45,440

Notes: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)).

Table 3.3: Impact of Wind Capacity Investments on Log Monthly Earnings

	(1)	(2)	(3)	(4)	(5)
Wind Capacity (GW)	0.097*	0.096*	0.057*	0.058*	0.035*
	(0.034)	(0.034)	(0.022)	(0.022)	(0.015)
Log Population (20-69)		-0.004	-0.009	-0.008	-0.051*
		(0.028)	(0.023)	(0.023)	(0.019)
Solar Capacity (GW)				-0.016	-0.028
				(0.014)	(0.036)
Non-attainment indicator				0.000	0.000
				(0.002)	(0.003)
Senate LCV Score					-0.000*
					(0.000)
House LCV Score					-0.000
					(0.000)
County FE	x	x	x	x	x
Year FE	x	x			
State x Year FE			x	x	
CZ x Year FE					x
Mean Monthly Earnings	3,154	3,154	3,154	3,154	3,154
Observations	46,680	46,680	46,680	46,680	45,440

Notes: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)).

Table 3.4: Impact of Wind Capacity Investments on Log County GDP

	(1)	(2)	(3)	(4)	(5)
Wind Capacity (GW)	0.166*	0.185**	0.130*	0.131*	0.044
	(0.060)	(0.064)	(0.053)	(0.053)	(0.040)
Log Population (20-69)		0.790***	0.872***	0.873***	0.714***
		(0.095)	(0.089)	(0.088)	(0.085)
Solar Capacity (GW)				-0.123	-0.205
				(0.064)	(0.099)
Non-attainment indicator				-0.008	-0.006
				(0.008)	(0.009)
Senate LCV Score					-0.000
					(0.000)
House LCV Score					-0.001
					(0.001)
County FE	x	x	x	x	x
Year FE	x	x			
State x Year FE			x	x	
CZ x Year FE					x
Mean County GDP	3,482,187	3,482,187	3,482,187	3,482,187	3,482,187
Observations	43,681	43,681	43,681	43,681	42,484

Notes: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)). The mean county-level GDP is reported in thousands of 2019 dollars.

Table 3.5: Impact of Wind Capacity Investments on Log Income per Capita ($2019)

	(1)	(2)	(3)	(4)	(5)
Wind Capacity (GW)	0.092**	0.088**	0.053*	0.053*	0.014
	(0.031)	(0.029)	(0.022)	(0.022)	(0.015)
Log Population (20-69)		-0.163**	-0.134**	-0.133**	-0.158***
		(0.044)	(0.035)	(0.035)	(0.033)
Solar Capacity (GW)				-0.034	-0.111***
				(0.017)	(0.020)
Non-attainment indicator				-0.003	-0.001
				(0.003)	(0.003)
Senate LCV Score					-0.000
					(0.000)
House LCV Score					0.000
					(0.000)
County FE	x	x	x	x	x
Year FE	x	x			
State x Year FE			x	x	
CZ x Year FE					x
Mean County Income per Capita	30,185	30,185	30,185	30,185	30,185
Observations	45,980	45,980	45,980	45,980	44,720

Notes: *** $p<0.001$, ** $p<0.01$, * $p<0.05$. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)).

Table 3.6: Impact of Wind Capacity Investments on Log Employment at the Commuting Zone Level

	Employment (1)	Earnings (2)	GDP (3)	Income per Capita (4)
Wind Capacity (GW)	0.086	0.102	0.361**	0.128*
	(0.045)	(0.051)	(0.098)	(0.047)
Log Population (20-69)	1.197***	0.114***	1.120***	0.067***
	(0.041)	(0.020)	(0.061)	(0.013)
Solar Capacity (GW)	-0.043	-0.034	-0.257	-0.020
	(0.024)	(0.032)	(0.173)	(0.020)
Non-attainment indicator	-0.018	0.018	0.025	0.038*
	(0.035)	(0.017)	(0.048)	(0.014)
CZ FE	x	x	x	x
MSA x Year FE	x	x	x	x
Mean Employment	38,828	3,151	3,670,552	29,973
Observations	11,120	11,120	10,564	11,120

Notes: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)).

Table 3.7: Impact of Any Wind Investment on Log Employment, Accounting for Heterogeneous Treatment Effects

	TWFE (1)	C&S (2)	SDID (3)
Wind Capacity > 0	0.023*		0.315***
	(0.008)		(0.078)
Log Population (20-69)	0.879***		1.049***
	(0.036)		(0.012)
Solar Capacity (GW)	-0.063*		-0.006
	(0.025)		(0.179)
Non-attainment indicator	0.005		-0.039
	(0.005)		(0.052)
Wind Capacity > 0		0.013*	
		(0.006)	
County FE	x		
Stack FE			x
State x Year FE	x		x
Mean Employment	35,499	35,499	33,374
Observations	46,680	45,720	17147026

Notes: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)).

Table 3.8: Impact of Wind Capacity Investments on Log Employment by Sample

	QWI,Preferred (1)	QWI, Full Sample (2)	QCEW, Preferred (3)	QCEW, Full Sample (4)
Wind Capacity (GW)	0.046**	0.041**	0.047*	0.043**
	(0.014)	(0.012)	(0.017)	(0.015)
Log Population (20-69)	0.891***	0.878***	0.921***	0.897***
	(0.034)	(0.034)	(0.032)	(0.031)
Solar Capacity (GW)	-0.043	-0.029	-0.026	-0.025
	(0.034)	(0.016)	(0.038)	(0.015)
Non-attainment indicator	0.004	0.004	0.005	0.001
	(0.005)	(0.004)	(0.005)	(0.004)
County FE	x	x	x	x
State x Year FE	x	x	x	x
Mean Employment	35,499	43,464	34,352	42,133
Observations	46,680	53,970	53,399	62,070

Notes: *** $p<0.001$, ** $p<0.01$, * $p<0.05$. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)).

Table 3.9: Impact of Wind Capacity Investments on Log Employment by Quartile of Wind Capacity

	(1)	(2)	(3)	(4)	(5)
Q1 Wind Capacity (<35 MW)	0.007	0.015	0.009	0.010	0.009
	(0.008)	(0.009)	(0.008)	(0.008)	(0.007)
Q2 Wind Capacity (<122 MW)	0.006	0.018	0.007	0.007	0.016
	(0.007)	(0.010)	(0.008)	(0.008)	(0.009)
Q3 Wind Capacity (<248 MW)	0.003	0.015	-0.006	-0.006	-0.003
	(0.009)	(0.009)	(0.007)	(0.007)	(0.007)
Q4 Wind Capacity (>248 MW)	0.027*	0.042**	0.021*	0.022*	0.016
	(0.011)	(0.013)	(0.009)	(0.009)	(0.008)
County FE	x	x	x	x	x
Year FE	x	x			
State x Year FE			x	x	
CZ x Year FE					x
Mean Employment	35,499	35,499	35,499	35,499	35,499
Observations	46,680	46,680	46,680	46,680	45,440

Notes: *** p<0.001, ** p<0.01, * p<0.05. Each quartile is defined as the quartile of wind capacity in counties with positive wind capacity. The reference category includes counties during years where they have zero wind capacity. Column (1) only includes county and year effects. Column (2) controls for log population between ages 20 and 69. Column (3) replicates Column (2) with state-by-year effects rather than year effects. Column (4) adds controls for solar capacity and county-level nonattainment status under the Clean Air Act. Column (5) replicates column (4) with commuting zone-by-year effects and state and house LCV scores. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)).

Table 3.10: Impact of Wind Capacity Investments on Log Employment by Sex and Treatment Duration

	Males (1)	Females (2)
1 Year	0.007	0.002
	(0.007)	(0.004)
2 Years	0.013	0.005
	(0.007)	(0.004)
2-5 Years	0.011	0.003
	(0.008)	(0.004)
5-10 Years	0.017	0.005
	(0.011)	(0.005)
10+ Years	0.031*	0.007
	(0.013)	(0.006)
County FE	x	x
State x Year FE	x	x
Mean Employment	17,848	17,652
Observations	46,680	46,680

Notes: *** p<0.001, ** p<0.01, * p<0.05. Each quartile is defined as the quartile of wind capacity in counties with positive wind capacity. The reference category includes counties during years where they have zero wind capacity. Column (1) only includes county and year effects. Column (2) controls for log population between ages 20 and 69. Column (3) replicates Column (2) with state-by-year effects rather than year effects. Column (4) adds controls for solar capacity and county-level nonattainment status under the Clean Air Act. Column (5) replicates column (4) with commuting zone-by-year effects and state and house LCV scores. Standard errors are two-way clustered by county and year (Cameron, Gelbach and Miller (2011)).

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