A Harsh Sun? Staggered Difference-in-Differences Analyses of Community Solar Adoption's Impacts on Residential Energy Expenditure

by

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Abstract

Net metering policies that allow solar panel adopters to "sell" their unconsumed energy production back to the grid are driving debates in the solar industry. Experts question the efficiency and equitability of such policies due to their implications on utility pricing behavior as well as their inaccessibility for marginalized communities. While community solar systems facilitate increased access to solar adoption, little research attempts to explore the implications of these systems on electric utility retail pricing. In this paper, I use traditional and alternative difference-in-differences (DID) analyses to estimate the effect of county-level community solar implementations on household electricity expenditure and residential retail electricity prices. Utilizing American Community Survey and National Renewable Energy Laboratory datasets, I find significant increases in residential retail electricity prices following community solar integrations. However, there is no evidence to suggest that these retail price increases result in higher overall electricity expenditures for households in adopting counties nor do they occur across all treated county groups. Results indicate that changing utility behavior in the form of price increases may be of little impact on aggregated household expenditures. Further research is necessary to explore the direct impacts of utility pricing reactions on non-adopting households and better understand the household impacts of solar energy transitions.

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1 Introduction

As global energy systems increasingly shift away from fossil fuel-based forms of energy supply, solar photovoltaic (PV) technologies serve as an important player in global and domestic energy transitions. In the US, solar PV capacity, or the maximum output of power generation, grew from just 0.34 gigawatts (GW) in 2008 to 97.2 GW in 2021 (*Solar Energy in the United States*, 2021). Yet, with such rapid adoption there is an observed disparity in the demographic distribution of adopters and the technology's potential impacts on household well-being (Sunter et al., 2019; O'Shaughnessy et al., 2021; Barbose et al., 2021). Traditional net metering policies that historically uplifted solar development specifically come under debate in states like California, and were threatened with banishment in the EU due to their potential contributions to cost-shifting and distributive injustices among solar adopters and non-adopters (Canary Media, 2022; Clastres et al., 2019). More specifically, non-adopters, who tend to be household renters, of lower income levels, less educated, and of racial minority groups may face less optimal levels of well-being as a result of increased solar development (Barbose et al, 2022). While new efforts in the form of community solar or shared solar systems provide an avenue to reduce these disparities in adoption (Feldman et al, 2015), little research explores these systems' contributions to pricing behavior and the larger net metering debate.

I address the following question in this paper: how does county-level community scale solar adoption affect county-level residential electric retail pricing and household electricity expenditures? Using two-way fixed effects and staggered difference-in-differences estimators by Callaway and Sant'Anna (2021), I take advantage of National Renewable Energy Laboratory (NREL) project and retail electricity price data along with American Community Survey household samples to answer such questions. Despite initial violations of necessary parallel trends assumptions, I find that residential electricity prices increased for counties that adopted community solar projects under altered assignment of comparison groups and when controlling for demographic characteristics. While the effects of these price increases are insignificant under household expenditure impacts, they suggest a behavior of utilities to increase electricity rates in accordance with increased utilization of solar photovoltaic adoption.

I also ask the following supplemental question: how is community solar implementation associated with county-level demographic factors like race, income, education, and policy integrations? I find that counties installing community solar on average have higher levels of educational attainment, higher levels of household income, and a smaller proportion of their population identify as Black. This aligns with common residential rooftop adoption demographic findings (Sunter et al., 2019, Barbose et al., 2022). However, these characteristics do not define project subscribers, rather the communities in which these projects are located.

I structured this paper as follows: Section 2 outlines underlying mechanisms behind my investigation of adoption impacts; Section 3 describes my data collection and aggregation methods; Section 4 provides the specifications of my empirical methods; Section 5 reports the results of my estimators; Section 6 concludes this paper, outlines its limitations, and guides future research.

2 An Efficient and Equitable Energy Transition?

2.1 Energy Insecurity

Energy burden, or the proportion of a household's income spent on utility bills, tends to be correlated with demographic characteristics. National low-income and rural low-income households spend 7.2% and 9% of their annual incomes on energy utilities, respectively, outpacing the national average of 3.3% (Drehobl & Ross, 2016). Home ownership is another indicator of insecurity, as renters face a higher than average median energy burden of 4.0%. These disparities are worrying in light of rising energy prices; increasing retail prices are associated with climate change as well as potential infrastructure changes resulting from the energy transition (Carley & Konisky, 2020). The nominal residential retail price of electricity was projected to rise from 12.87 cents per kilowatt hour (kWh) in 2018 to 14.32 cents/kWh in 2022 (*Short-term Energy Outlook*, 2021). Therefore, there is a relevant concern for changing household expenditure priorities as a result of new energy needs, especially in the context of the renewable energy transition and shifts away from fossil fuels.

2.2 Residential Solar PV Adoption

PV technology broadly consists of four main system scales: residential, community, commercial, and utility. Of note for this paper are community systems in relation to residential solar consumption, opposed to commercial or other non-residential beneficiaries of solar power. Residential solar generally defines solar installations that are rooftop-based and produce electricity that is generally consumed by a singular household who owns or leases the system. Compared to other scales, like utility systems, residential solar has a much higher lifetime cost per unit of energy production — defined as the levelized cost of energy (LCOE) (*The Future of Solar Energy*, 2015).[1] Although uncompetitive, 2.3 million residential rooftop systems were installed in the US as of 2019 (Barbose et al., 2021). To incentivize further adoption, 43 states participate in net-metering practices that allow for consumers subscribed to, or owners of a solar PV system, to count any added generation to the grid on their electricity bill (Stokes & Breetz, 2018, p. 13; Wan, 1996).[2] Despite such benefits, residential solar tends to face adoption challenges due to its unaffordable fixed costs, reliance on property ownership, and household infrastructure challenges (Carley & Konisky, 2020; Crago, 2021).

According to EnergySage (2021) the cost of a 5-kW system in a state like Virginia is $14,450 with an average payback of 12.89 years — an unlikely investment for households unable to meet such payments, especially for those with potentially high discount rates unwilling to invest in the long-term. Issues also stem from owner and tenant split incentives (Barbose et al., 2021). Landlords may not face direct incentives to pay for infrastructure investments that could increase a unit's energy efficiency and subsequent bill savings. Households without suitable roof space, like in an apartment complex, lack the ability to install rooftop panels for their individual apartment (Carley & Konisky, 2020, p. 4). Over 35% of Department of Housing and Urban Development low-income projects from 1995-2015 consisted of multi-unit complexes with 21-50 housing units (Cook & Bird, 2018, p. 10). As a result, adoption is unsurprisingly anticorrelated with existing demographic

[1] See Ray, D. (2021). *Lazard's Levelized Cost of Energy Analysis—Version 15.0. 21*.
[2] Net metering will be discussed at length in Section 2.4.

disparities already within the residential solar PV space (Barbose et al., 2021; O'Shaughnessy et al., 2021, Heeter et al., 2021a).

Barbose et al. (2022) analyzed the demographic state of household adoption and found that the median rooftop adopter income was $115k per year compared to the national median of $63k in 2020. Not only are residential adopters associated with higher incomes, but they are also statistically more educated and are more-likely to live in majority-white census tracts (Barbose et al., 2021, p. 5; Sunter et al., 2019). Rooftop solar PV has the potential to benefit households through net-metered bill reductions. Yet, such benefits are largely available to those who are less energy insecure (Sunter et al., 2019, p. 74). Given these challenges, states also look to other, larger PV systems, like community or shared solar, that overcome the limitations of rooftop-mounted systems conceptually allowing for greater access to solar adoption.

2.3 Community Scale Solar

Community scale solar programs represent centralized, on or off-site systems that allow for multiple energy consumers to subscribe to or purchase shares of produced energy.[3] "Participants who finance the development of a [community solar] project receive compensation for electricity generated by their share in the project, typically through so-called 'virtual net metering' (VNM) schemes. VNM allows subscribers to receive economic returns for electricity sold to a utility generated from the share of the solar project to which they are subscribed" (Chan et al., 2017, p. 2). Such systems allow households to consume solar energy when it would otherwise be infeasible due to previously discussed barriers. Community scale solar systems conceptually have cost benefits because they are owned and operated jointly, resulting in shared installation costs among consumers, or by a third-party who bears the fixed cost investments of the system (Feldman et al., 2015; National Renewable Energy Laboratory, 2011). Likewise, community scale solar does not require households to have property-ownership or suitable rooftop infrastructure to consume

[3] These systems are also broadly referred to as "community shared solar" or "community solar."

energy because the system is usually located offsite or on the rooftop of a multi-unit building and fed into a utility's existing services (Feldman et al., 2015).[4]

Community solar is looked to as an alternative method of transitioning households to solar PV adoption. Yet, such opportunities do not entirely reduce concern for cost externalities resulting from mass adoption. According to Crago (2021), existing disparities in solar distribution and increasing adoption raise further concern for cross-subsidizations in energy markets. Researchers describe this to be when solar adopting homes, who are more likely to be energy secure and of a higher-incomes, receive benefits by selling electricity to the grid while non-adopter, and more likely energy burdened, lower-income homes pay for costlier energy (p. 10).

2.4 Cost Shifts and Solar Adoption

While disparate adoption of solar PV is not necessarily inherent to the technology, its potential facilitation of existing energy insecurities is of concern (O'Shaughnessy, 2021). Previously mentioned net metering programs are arguably the most effective policy effort to drive the adoption of distributed solar generation.[5] While a benefit for households looking to reduce their electricity bills and their emissions impacts, net metering practices may also "dampen demand for grid supplied power and thereby cut into utilities' profits" (Rule, 2015, p. 119). Compensation for volumetric rates in US dollars per kilowatt-hour ($/kWh), the retail rates we see per consumption of electricity, often cover the variable and fixed costs accrued by utilities in addition to fixed charges on consumer's bills (Darghouth et al., 2022; Rule, 2015). Depending on the scope of adoption within a given utility territory, the impacts of such reductions in demand may be manageable. However, large-scale adoption of distributed solar PV may lead utilities to compensate for such profit losses by petitioning to increase their retail rates of electricity or fixed consumer charges. Retail price increases may incentivize more households to switch to distributed solar for its

[4] In comparison to residential rooftop solar with a minimum estimated LCOE of $147/MWh, the minimum estimated LCOE of community solar is $63/MWh, a much more competitive cost with other energy supply technologies like coal ($65/MWh) (Ray, 2021). Third-party providers may therefore be incentivized by community solar's cost effectiveness to make investments in the space.

[5] Distributed solar generation refers to solar production near the point of consumption. A utility scale solar array would be a centralized system since most of its consumers are likely not living in proximity to the system. See *Distributed Generation of Electricity* (2021).

consumption benefits, leading to higher prices, and further incentivizing more solar development in what Rule (2015) referred to as a "death spiral" for utilities (p. 119). As a result, utilities across the nation have advocated for a variety of policy mechanisms to manage profit losses: fixed monthly fees specific to solar users, increases to fixed monthly charges for utility customers, reductions in the compensatory rate paid to solar prosumers, and limits on distributed generation development (Rule, 2015; California Public Utilities Commission, 2022). In addition to combating profit loss, such advocacy stems from a common argument surrounding the "fairness" of net metering policies toward non-adopter utility consumers who are paying for these rate increases without the added benefits of net metering. These cost shifts are particularly of interest due to the disparate adoption of solar panels among low-income, racial minorities, and renter households (Rule, 2015, p. 129, Borenstein, 2017; Barbose et al., 2022; Wan, 1996).

Conceptually, net metering may create cross-subsidies in which, assuming two groups pay equal retail rates, one group (adopters) do not pay the full cost of a service for a utility to supply electricity and as a result, unmet costs are spread across all consumers (non-adopters). However, cross-subsidies are not unique to solar integrations. Utilities often impose direct and indirect cross-subsidization through discounted electricity rates to low-income communities and for rural areas where it is costlier to deliver energy (Faulhaber, 1975; Rule, 2015). In the case of solar PV adoption, some argue that the rate at which prosumers are compensated for their solar generation exceeds its avoided cost or the value of that produced energy to the grid. While true cost-shift effects are debated, research that measures the potential cost impacts of distributed solar development, specifically among community shared solar integrations, is limited. In order to provide a greater understanding of the energy transition's impacts on households, I attempt to measure the implications of increasingly utilized, and arguably more accessible, community shared solar systems on residential retail electricity prices and household energy expenditures.

2.5 Review of Literature Frameworks

Researchers are increasingly focused on developing sound policy solutions for solar PV transitions. O'Shaughnessy et al. (2021) studied the impacts of residential solar-based policies that incentivize uptake by low-to-moderate income (LMI) households.[6] The authors measure a number of financing incentives' impacts on adoption bias, the difference between average adopter income in a given area and that area's median income. They then conduct a staggered difference-in-differences analysis that looks at quarterly changes in adoption beginning with the first quarter of an incentive's introduction.[7] However, O'Shaughnessy et al. (2021) explicitly excluded analyses of community scale solar and solely focus on household adoption of residential systems.

In addition to O'Shaughnessy et al. (2021), there are a number of separate analyses and reports dedicated to similar understandings of household adoption. Sunter et al. (2019) found that Black and Latino-majority census tracts installed 69% and 30% less rooftop PV in comparison to tracts with no racial majority, respectively. A number of reports from the Lawrence Berkeley National Laboratory (LBNL) and NREL also contain useful analyses of solar PV adoption and market trends. Heeter et al. (2021) conducted a brief analysis in their report of community solar that estimated the impacts of subscription models on household energy burden.

Likewise, academic as well as industry reports contribute to the incidence of cross-subsidization occurring from increased adoption of solar PV integrations (Satchwell et al., 2014; Johnson et al., 2014; Clastres et al., 2019). Satchwell et al. (2014) employed a model that found average electricity rates rose anywhere from 0.1% to 2.7% depending on region-specific, solar PV market penetration over a 20-year period. Likewise, over this 20-year period, utility revenue reductions were greater than utility cost reductions in high penetration levels (10% of total retail sales).[8] Johnson et al. (2014) found that while residential solar PV adopters experienced a 58% drop in bill charges, non-adopters experienced only a 1% increase in bill charges over a 15-year modelling period, easing

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[6] These are households whose income is less than 80% of an area's median income

[7] O'Shaughnessy et al. (2021) utilize staggered treatment difference-in-differences models of Callaway and Sant'Anna (2021) to conduct their analyses. Their paper will be discussed at length in Section 4.

[8] Satchwell et al. (2014) note that a 10% sales penetration at the time of writing was non-existent. Roughly 2% of sales were allocated to utilities in 2014 (p. viii).

potential concerns for substantial cost shifts. Clastres et al. (2019) estimated the level of cross-subsidies occurring in France under changes in the ratio of produced solar energy that is either consumed or sold by a household. They find that while cross-subsidizations exist, they are relatively insignificant due to the relative penetration of solar in the country's overall energy market. As a result, analyses of community solar impacts may be hindered by project penetration in local energy markets. While a community may adopt a project, the relative size of that project may have more or less of an impact on utility behavior.

This research paper aims to build upon the work of O'Shaughnessy et al. (2021), and other researchers dedicated to solar demographics and cross-subsidy research, to estimate the impacts of system adoption on energy expenditure (Barbose et al., 2021; Heeter et al., 2021; Satchwell et al., 2014; Johnson et al. 2014; Clastres et al., 2019). This differentiates from analyses of rooftop residential solar by looking at the adoption impacts of community scale solar. Given the hypothetical and observed benefits of community scale solar, it stands to reason how impactful these systems are on cross-subsidization debates. As will be discussed, I focused my research around three central data categories: community solar installation data, electricity price data, and demographic data — all of which found influence from the previously mentioned research.

3 Data Collection & Aggregation

3.1 Solar Installation

I collected community solar project location and capacity data from the NREL *Sharing the Sun Community Solar Project Data December 2021 Update* (Chan et al., 2022).[9] The *Sharing the Sun* dataset contains information on individual project name, location by state and "city", capacity (kW-AC), utility provider, and year of implementation from 2006-2019. Data after 2019 was removed to maintain consistency with demographic and price data. The current dataset observes these measures across 2,028 projects located in 39

[9] The National Renewable Energy Laboratory has hosted a dataset of community solar projects since July, 2018. The December 2021 update can be found here: https://data.nrel.gov/submissions/185.

states, including Washington, DC. A public-use Homeland Infrastructure Foundation-Level Data (HIFLD) collective of cities, counties, and state names, along with their respective FIPS codes, allowed for the identification of county locations for *Sharing the Sun* projects.[10] This was necessary to match the dataset with county-level electricity cost and demographic data described in the following sections.

Table 1

Community Solar Project Characteristics

Project Characteristics	
Total Capacity (*kW-AC*)	2,140,013
Median Capacity	1,000
Mean Capacity	1,718.89
Max Capacity	81,000
Min Capacity	2.31
Median Year of Interconnection	2017

Note. AC refers to alternating current.

Outside of geographic matching, there are a number of limitations with the dataset. NREL acknowledges that the list is not comprehensive and may contain errors among localities. I identified any projects with naming errors, mostly misspelled or missing locality names, using the United States Geological Survey during my initial data consolidation.[11] Likewise, the dataset only contains information on project locations and not project subscriber locations. This limited my empirical analysis to measure precise impacts on the households who subscribe to a given project. Rather, I estimate the project integration impacts on the county hosting a project. It is also of note that projects are not identified to either a regulated or deregulated market system. Under a regulated system,

[10] Data is available here: https://hifld-geoplatform.opendata.arcgis.com/datasets/geoplatform::cities-and-towns-ntad/about.
[11] Listed cities often contained unincorporated localities requiring verification through the United States Geological Survey National Map Corps: https://www.usgs.gov/core-science-systems/ngp/tnm-corps.

utility prices are set by a public commission and may not be directly impacted by increased project adoption (*Electricity Explained*, 2022). This will be discussed further in Section 6 and Appendix C.

Table 1 describes the general characteristics of the selected dataset. Total project capacity sits around 2 gigawatts (GW) for the selected projects. I also identified a skew toward larger projects above 1 megawatt (MW) as mean capacity sits around 2 MW. This is especially visible considering the range of project sizes as the largest 81 MW project, which is located in Almyra, Arkansas, is 35,000 times as large as the smallest listed project located in Vancouver, Washington. Table 2 describes the total capacities of the top four largest community solar adopting states by capacity. Minnesota has the most generation capacity with 668,267 kW (668 MW). However, its average capacity is only 2,169 kW compared to a state like Florida with an extremely low number of projects but a higher average project size around 11,230 kW. This is representative of a broader trend with some states installing many smaller projects and others hosting a small number of large-scale projects.

Table 2

Top Four States by Total Capacity (kW-AC)

States with Most Capacity (kW-AC)	
Minnesota (*N*=308)	668,267.05
Massachusetts (*N*=293)	454,263.17
New York (*N*=162)	188,259.01
Florida (*N*=13)	145,987.38

Note. The following table describes the top four states by total installed capacity for the years 2006 to 2019.

3.2 Household Energy Expenditure

I collected household-level average monthly electricity cost data from the American Community Survey (ACS) 5-year Public Use Microdata Sample estimates (PUMS) for the end-years 2009 to 2019. The use of ACS PUMS data was influenced by the Department of Energy's Low-Income Energy Access Data (LEAD) Tool, which utilizes 2018 PUMS 5-year estimates to visualize energy expenditure and energy burden data at the census tract level.[12]

Conventional ACS estimates consist of 1, 3, and 5-year samples. 5-year ACS estimates define data collection over a 60-month period and allow for data collection in Census Bureau defined areas of less than 65,000 people. All year values represent the end-year of the sample period, so the sample for the 2009 end-year represents data collected from 2005 to 2009, 2010 for the years 2006 to 2010, etc. Therefore, the estimates are period estimates and not point-in-time estimates.[13] Conventional 1-year ACS samples are limited to data collection in geographic areas with populations of 65,000 people or more in a given 12-month period. This limited the ability to measure demographic information for less populated areas of the country and was especially challenging given the scale of project implementation. Only 87 of the 417 aggregated counties with projects have at least 65,000 people within their boundaries. Therefore, ACS 5-year estimates provide a solution to these accessibility challenges at the expense of current estimations.

The ACS PUMS sample is bound to Public Use Microdata Areas (PUMA) which comprise around 100,000 people. A PUMA is bound to state-lines, and generally follows county or census tract lines. PUMAs contain samples of households within smaller counties (less than 65,000 people) unlike the ACS and these localities are aggregated under a single PUMA when their population is under 100,000 people. As a result, a PUMA may contain multiple counties, or vice versa if a county contains a multiple of 100,000 people. While period collection for the PUMS follows similar methods as the conventional ACS, 1-year PUMS estimates only sample 1% of PUMA households compared to 5% of households under the 5-year estimates. I chose to use the 5-year estimate for its larger sample and to maintain continuity with data collected from the conventional 5-year ACS estimates. The variable of interest, average monthly household electricity costs, are collected at the PUMA level. I discuss the specific aggregation and crosswalk methodologies from PUMA to county-level measurements in Appendix B.

[12] The Department of Energy LEAD Tool can be accessed here: https://www.energy.gov/eere/slsc/maps/lead-tool
[13] 3-year estimates were discontinued in 2015, so they were not applicable to this analysis.

The collected electricity cost data excludes households whose electricity costs are combined with their monthly rent and who did not record any electricity costs for the collected period. All end-year cost values were further adjusted for inflation using the Bureau of Labor Statistics (BLS) *Consumer Price Index for All Urban Consumers: Electricity in the US City Average* (2022).[14] All reported dollar values are in constant 2013 US Dollars. I also adjusted individual 5-year estimates to measure constant end-year dollars.[15]

3.3 Residential Electricity Retail Pricing

I collected panel data of electricity retail prices from a NREL organized dataset of utility rates and their zip codes from the Open Energy Data Initiative (OEDI) for the years 2013 to 2019. The dataset is a consolidation of rates from utilities recognized by the US Energy Information Administration's (EIA) *Annual Electric Power Industry Report, Form EIA-861*[16] and the Hitachi Energy Velocity Suite (Open Energy Data Initiative, 2020; *U.S. Electricity Companies and Rates: Look-Up by Zip code*, 2020).[17] OEDI datasets define utilities by investor owned utilities (IOU) and non-investor owned utilities (Non-IOU). Non-investor owned utilities consist of publicly owned utilities and cooperatives. IOUs and Non-IOUs were aggregated together due to the mixed ownership structure of the observed community solar projects. The initial dataset consisted of residential rates in $/kWh for utility providers by zip code and state. I used aggregation methods similar to those used with ACS PUMS monthly electricity cost data to define county-level electric retail data. I relied on the MCDC for zip code and county crosswalks through the Geographic Correspondence Engine to conduct similar aggregations.

Table 3 describes my main outcomes of interest across sample periods and treatment groups. Across both outcomes and treatment groups, overall real expenditures and retail prices increased. In the case of household electricity costs, expenditures increased around $45 monthly, with a significant difference in price changes between 2009

[14] BLS Consumer Price Index data can be collected here: https://fred.stlouisfed.org/series/CUSR0000SEHF01
[15] For instance, for the end-year 2009, household costs from 2005 to 2009 were adjusted to 2009 US Dollars.
[16] The EIA-861 form is an annual census of US electric utilities from 1990-2020. See *Annual Electric Power Industry Report* (2021).
[17] Data for 2019 utility retail prices can be found here: https://data.openei.org/submissions/4042. Data for 2013 to 2018 utility retail prices can be found here: https://catalog.data.gov/dataset?publisher=National%20Renewable%20Energy%20Laboratory

and 2019 for the two treatment groups. Households in never treated counties saw on average a change of $47 in monthly expenditures compared to treated counties' change of $43. In the case of retail prices, neither treatment group significantly differed in the $/kWh rate paid for consumption from 2013 to 2019. These price increases are indicative of existing understandings of energy pricing, but may be a result of multiple factors including increases in extreme temperatures and prices of alternative fuel sources, like natural gas and coal. These factors will also be discussed in Section 6 in reference to my findings.

Table 3

Summary Statistics for Outcomes of Interest by Treatment Group

Summary Statistics			
	Year	Household Electricity Cost ($/mo)	Residential Retail Price ($/kWh)
Treated	Starting Year (2009 or 2013)	120.22 (1.232)	0.114 (0.00124)
N= 4,345	2019	163.39 (1.368)	0.129 (0.00139)
	Difference	43.17 (0.876)	0.015 (0.00081)
Not Treated	Starting year (2009 or 2013)	126.83 (0.497)	0.108 (0.00065)
N= 30,202	2019	173.54 (0.493)	0.121 (0.00069)
	Difference	46.71 (0.355)	0.014 (0.00056)
	Difference b/w groups	3.542 (1.001)	0.00159 (0.00152)
	P-value	<0.001	0.107

Note. The p-values were calculated under a t-distribution. Household electricity costs cover the years 2009 to 2019, retail prices cover 2013 to 2019.

3.3 Demographic Data

I collected additional income and other demographic data through the US Census Bureau American Community Survey 5-year sample. The collected data are described in Table 4. All demographic variables align with the previous discussion of ACS 5-year estimates. All year values represent the end-year of the sample period.

Table 4 describes the demographic characteristics of "not-yet-treated" units across comparison groups.[18] Not-yet -treated groups consist of county units that are never treated and those that will eventually be treated. Column (1) reports characteristics across nationwide observations in county cross sections that did not yet receive a treatment and that will never receive a treatment. Column (2) reports these demographics for posttreatment units of county panels. Treated counties on average contain more people, are of higher incomes, have higher levels of educational attainment, and less black residents than their not-yet-treated counterparts. While not an analysis of project subscribers, this aligns with general adoption characteristics emphasized by researchers (Sunter et al., 2019; Barbose et al., 2022). It is of note that households are more likely to be renters and more likely to identify as Latino compared to untreated counties. This contrasts with underrepresentation of Latinos among traditional residential adopters (Barbose et al., 2022). However, these statistics neither characterize the subscribers to these systems nor do they identify these characteristics relevant to other neighboring counties or greater state characteristics. Appendix A.1 contains density charts of county-level characteristics by treatment conditions and visual analyses to further this analysis.

[18] The utilization of "not-yet-treated" units will be explained in Section 4.

Table 4

Demographic Characteristics of Counties by Treatment Group

	Demographic Characteristics		
	Not-yet-treated	Treated	p-value
	(1)	(2)	(3)
Housing Characteristics			
Population	91,411	224,677	<0.001
Households	34,882	85,610	<0.001
% Owner Households	72	70	<0.001
Income Characteristics			
Median Income (2013 $)	47,405	59,309	<0.001
GINI Coefficient	0.440	0.440	0.500
Educational Attainment			
% Less than Highschool	15	11	<0.001
% Highschool Educated	35	31	<0.001
% Some College	29.9	31.3	<0.001
% Bachelor's Degree	13.1	17.3	<0.001
% Graduate Degree	6.9	9.8	<0.001
Racial Characteristics			
% White	84	85	0.007
% Black	9	7	<0.001
% Asian	1.21	1.91	<0.001
% American Indian	1.88	1.19	<0.001
% Native Hawaiian	0.09	0.11	0.005
% Other	2.05	2.89	<0.001
% Hispanic or Latino	8	11	<0.001
% Not Hispanic or Latino	92	89	<0.001

Note. All values are averages of county panel data. The p-values are calculated under a t-distribution.

4 Empirical Methodology

My approach in this paper centers around commonly practiced and newly researched difference-in-differences (DID) models to estimate the effect of community solar adoption on retail electricity rates and household electricity expenditure under staggered treatment adoption designs. Opposed to standard DID models,[19] treated counties do not contain the same post and pre-treatment periods because these counties are exposed to adoption across a range of years. As a result, the methodology of this paper most closely follows empirical specifications with more than two time periods.

The measurement of effect will follow three separate estimation models. The first is a "static" two-way fixed effects (TWFE) DID model. Given the variation in treatment timing, the models will utilize the time-based fixed effects to account for heterogeneity across years. Likewise, a county fixed effects variable will account for time-invariant heterogeneity. The second is an estimate of group-specific effects, or the estimator of the effect of project implementation by a projects' year of interconnection. The third follows an event study estimator that attempts to measure the treatment effect of project adoption with varying lengths of exposure to projects. The two latter models follow the specifications of Callaway and Sant'Anna (2021) and take influence from their application in O'Shaughnessy et al. (2021). This paper will delineate from prior literature as it attempts to analyze the impacts of community solar on household expenditure, as opposed to rooftop solar adoption.

4.1 "Static" Two-Way Fixed Effects Difference-in-Differences Model

$$y_{it} = \gamma_i + \lambda_t + \beta^{DD} D_{it} + \mu X_{it} + \epsilon_{it} \tag{1}$$

where y_{it} is the outcome of interest, γ_i is a county fixed effect, λ_t is a year fixed effect, D_{it} is a dummy variable equal to 1 if county i hosts a community solar project in year t. X_{it} is a vector of county- and year-level control variables for county population, median county income, proportions of ACS-designated racial identities, educational attainment, and percentages by household ownership. The coefficient β^{DD} is the post-treatment difference-in-difference estimator for the effect of project adoption on y_{it}.

[19] See Callaway and Sant'Anna (2021), p. 2. Common DID designs follow two groups of units with clearly defined pre and post-treatment periods that allow for measurement of changes in an outcome of interest before and after one group is exposed to the treatment. Under a counterfactual assumption that both groups would have experienced similar changes in the outcome of interest, researchers can measure an average effect of the treatment on the treated (ATT).

While two-way fixed effects (TWFE) are widely used in analyses of difference-in-differences models, recent econometric literature highlights a number of theoretical problems with TWFE in the application of staggered DID models. Specifically, when treatment implementations occur at different times across different groups and such treatment effects are dynamic or unequal. This paper's setting is particularly concerning, as counties do not all receive the same level of adoption. As described in Table 1, community solar projects range in size from smaller 2 kW projects to 80,000 kW (80 MW) projects. The hypothetical influence of these varying treatment intensities may cause unequal treatment effects across counties. It may also be the case that parallel trends do not hold until conditioned on observed covariates, as seen with the variations reported in Table 4 (Callaway & Sant'Anna, 2021; Goodman & Bacon, 2021; Sun & Abraham, 2021; Baker et al., 2022).

4.2 Group-Specific Effects

In acknowledgement of potential biases arising from project adoption under a TWFE model, I incorporate Callaway and Sant'Anna's (2021) identification, aggregation, and estimation methods to measure more robust and flexible causal effects. Under the assumption that treatment is irreversible and anticipation for treatment is negligible,[20] the unconditional treatment effect of a specific treatment group is estimated under the following regression:

$$y_{it} = \alpha_1^{g,t} + \alpha_2^{g,t} \cdot G_g + \alpha_3^{g,t} \cdot 1\{T = t\} + \beta^{g,t} \cdot \left(G_g \times 1\{T = t\}\right) + \epsilon^{g,t} \tag{2}$$

where y_{it} is the outcome of interest for a county in treatment group period g and time period t, where $t = 1, \dots, \mathcal{T}$. Under a model estimating average monthly household electricity costs, $\mathcal{T} = 11$. Under a model estimation of the retail price of electricity, $\mathcal{T} = 7$. No units are treated when $t = 1$. G_g is a dummy variable that is equal to one if a unit is treated in period g. Treatment is determined when a county implements at least one

[20] I assume households and utilities do not actively change their monthly average electricity costs and retail prices in anticipation of a project coming online.

project in a given year.[21] $1\{T = t\}$ is a dummy variable equal to one if a unit is observed at time period t. When the assumption that unconditional parallel trends based on a comparison of "not-yet-treated" county observations holds,[22] $\beta^{g,t} = ATT(g,t)$. $ATT(g,t)$ is the true "group-time average treatment effect" such that,

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0) | G_g = 1] \tag{3}$$

where $ATT(g,t)$ is the expected difference between the outcome of interest for treated counties at time t and the counterfactual outcome at time t had such counties never been treated. If I include covariates such that parallel trends are conditional, I have the following model:

$$y_{it} = \tilde{\alpha}_1^{g,t} + \tilde{\alpha}_2^{g,t} \cdot G_g + \tilde{\alpha}_3^{g,t} \cdot 1\{T = t\} + \tilde{\beta}^{g,t} \cdot \left(G_g \times 1\{T = t\}\right) + \tilde{\pi} \cdot X_{it} + \tilde{\varepsilon}^{g,t} \tag{4}$$

where X_{it} is a vector of panel controls including population, median income, percentage of population identifying as white, percentage of population 25 years and older with a high school degree, and an index of income inequality. Following the summary statistics in Table 4, it is reasonable to assert that the distribution of covariates between treated and untreated counties are not equal and therefore necessary to condition on should we assume parallel trends among our treatment groups.

The estimation of $ATT(g,t)$, assuming conditional parallel trends with a comparison to county observations who are not-yet-treated, follows Callaway and Sant'Anna's (2021) doubly robust estimation method.[23] The aggregation of group-time treatment effects into a singular parameter undergoes a simple average:

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[21] The treatment condition for counties does not differentiate between counties with many projects and counties with only a single project. This was due to the varying number of projects and overall capacities that accumulated in counties overtime. Future researchers should attempt to implement treatment intensity within their analysis.

[22] See Callaway and Sant'Anna (2021), p. 8-9. "Not-yet-treated" comparisons consist of all never treated and eventually treated units that have not yet received treatment. This comparison is particularly useful when analyzing observations solely among eventually treated counties.

[23] See Callaway and Sant'Anna (2021), p. 20, for a detailed breakdown of the doubly robust estimator for not-yet-treated comparison groups.

$$\theta_{gs}(\tilde{g}) = \frac{1}{\mathcal{T} - \tilde{g} + 1} \sum_{t=\tilde{g}}^{\mathcal{T}} ATT(\tilde{g}, t) \tag{5}$$

where $\theta_{gs}(\tilde{g})$ is the aggregated group-specific effect for units treated in period $\tilde{g}$ for all post-treatment periods. For an overall aggregation of treatment effects across all groups, the following aggregation is employed:

$$\theta_{gs}^{O} = \sum_{g \in \mathcal{G}}^{\mathcal{T}} \theta_{gs}(g) P(G = g | G \leq \mathcal{T}) \tag{6}$$

where θ_{gs}^{O} is the average effect of county adoption of community solar projects across all counties that adopted a project. $P(G = g | G \leq \mathcal{T})$ is a weight giving preference to larger groups, or time periods that saw the more project interconnections. In addition to group-specific estimates of average treatment on treated individuals, I employed event study specifications in Callaway and Sant'Anna (2021) to visualize pre and post-treatment estimations of treatment exposure effects on retail electricity prices and average monthly household electricity costs.

4.4 Event Study/Dynamic Treatment Effects

Callaway and Sant'Anna's (2021) aggregations also allow for the measurement of treatment effects based on county exposure to project implementations.

$$\theta_{es}(e) = \sum_{g \in \mathcal{G}} 1\{g + e \leq \mathcal{T}\} P(G = g | G + e \leq \mathcal{T}) ATT(g, g + e) \tag{7}$$

$$\theta_{es}^{O} = \frac{1}{\mathcal{T} - 1} \sum_{e=0}^{\mathcal{T}-2} \theta_{es}(e) \tag{8}$$

where e represents the time of exposure for treatment periods such that $e = t - g$. $\theta_{es}(e)$ is the average effect of adopting a community solar system e periods after adopting a project for all groups that are observed for exactly e periods. $1\{g + e \leq \mathcal{T}\}$ is a binary variable that allows the function to consider just average group-time treatment effects and $P(G = g | G + e \leq \mathcal{T})$ allows for a group-size weighted average of the group-time $ATT(g, t)$. $\theta_{es}(e)$ can be further aggregated to measure the average effect across all events where θ_{es}^{O} is a simple average of all estimated event times. The numerical and graphical results of these estimators are reported in Section 5.

5 Results & Discussion

In the following section, estimation results are broken down into two parts: the first are TWFE DID and group-specific effects as described in Callaway and Sant'Anna (2021); the second is an event study describing doubly robust ATT estimates resulting from treatment exposure for counties adopting at least one project. All analyses are conducted with not-yet-treated unit comparisons on a national and county-level.

5.1 Difference-in-Differences and Group-Specific Effects

Table 5 reports the results of the estimated effects of the project adoption on average monthly household expenditures for counties under traditional TWFE DID and staggered treatment group-specific effects estimates. These are outlined in Equation (1) and Equations (2), (4), (5), and (6), respectively. Column (1) identifies estimates under unconditional models utilizing a national comparison. I find significant negative effects under both the TWFE and group-specific aggregated effects. However, testing of parallel trends suggest a significant violation of the assumption that national, not-yet-treated control units and treated units would have followed parallel trends. As a result, the effect of these estimates is likely biased, invalidating their causality.[24] The potential differences in county characteristics, when referring to those listed under Table 4, may be a potential

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[24] Under a violation of the parallel trend assumption, the expected or average outcome of treated and comparison groups may not follow similar trends overtime. This would lead to a bias in the measurement of the post-treatment effect β^{DD} and $\beta^{g,t}$.

source of deviation in assuming parallel trends. Column (2) reports the conditional estimates, which still violate the assumptions of parallel trends between comparison groups. To further test for potential deviations among national samples and treated counties, I reduce all comparisons to eventually treated counties. Columns (3) and (4) report unconditional and conditional treatment estimates utilizing not-yet-treated county units as a comparison. Under altered comparisons, the once significant effects reported under column (1) are insignificant. Parallel trends are weakly validated under a conventional 5% significance level. These results, although limited, acknowledge the insignificant impacts of community solar systems given average system sizes on household electricity costs. It may be the case that current adoption and system sizes are not large enough to cause any significant changes in utility revenue and therefore pressure to increase retail prices. However, this is disputed in Table 6. It is further important to note that these estimates do not distinguish between adopter and non-adopter households, limiting the ability to measure any potential cost-shifts that may occur. The data may be confounded by other potential factors, like weather conditions, existing installed residential solar systems, and regulatory indicators that may impact variations in household costs and even consumption across units. The similarities between adopting counties may eliminate such variations seen at larger scopes of comparison.[25]

[25] The vignette for Callaway and Sant'Anna (2021) can be found here: https://bcallaway11.github.io/did/index.html

Table 5

Two-Way Fixed Effects and Group-Specific Effects for Average Household Electricity Expenditure

	Average Monthly Cost of Electricity ($/mo)			
	National		County	
	(1)	(2)	(3)	(4)
TWFE	-2.251** (0.877)	-1.454** (0.651)	0.003 (0.624)	0.0832 (0.516)
Group-Specific Effect	-0.860*** (0.2365)	-0.320 (0.233)	0.287 (0.427)	0.493 (0.463)
$g = 2010$	-7.145 (6.566)	---	-4.020 (4.887)	---
$g = 2011$	-6.034*** (1.676)	-2.768 (1.927)	-3.276* (1.682)	-1.041 (1.939)
$g = 2012$	-2.225 (1.503)	1.487 (1.461)	-0.342 (1.912)	1.690 (2.260)
$g = 2013$	-3.728 (2.271)	-0.583 (2.046)	-1.931 (2.207)	-0.977 (1.706)
$g = 2014$	0.031 (1.090)	0.923 (1.108)	0.604 (1.170)	0.554 (1.153)
$g = 2015$	1.301* (0.707)	1.360* (0.704)	1.557** (0.744)	1.547* (0.846)
$g = 2016$	-1.531*** (0.487)	-1.155** (0.545)	-0.752 (0.615)	-0.985 (0.641)
$g = 2017$	-0.237 (0.3303)	-0.0110 (0.308)	0.920** (0.466)	1.108** (0.485)
$g = 2018$	-0.612 (0.497)	-0.509 (0.485)	0.797* (0.484)	0.811 (0.607)
$g = 2019$	-1.633*** (0.427)	-1.629*** (0.457)	---	---
N	34,444	31,289	4,164	3,781
Parallel Trends	0.000	0.001	0.000	0.110
Controls	No	Yes	No	Yes

Note. Standard errors are listed in parentheses. Data are collected from 2009-2019. The row $g = 2010$ is not listed under (2) and (4) due to missing covariate data for 2010. Columns (1) and (2) use all not-yet-treated observations as a control group from nationwide counties. Columns (3) and (4) use not-yet-treated county observations as controls, allowing for better parallel trend assumptions. $g = 2019$ observations are not included in (3) and (4) since by $t = 2019$ there would be no untreated county observations.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6 reports TWFE and group-specific effects of community solar adoption on residential retail prices of electricity. Columns (1) and (2) report that on average, adoption of community solar increases retail prices of electricity charged to households under TWFE specifications. This effect is insignificant under county-level comparisons. Group-specific aggregations vary between geographic comparisons and conditional estimates. There are improved assumptions of parallel trends when moving from columns (1) to (4) such that columns (3) and (4) report an insignificant p-value that does not reject parallel trends under the unconditional and conditional county models. The column (4) group-specific effect reports that on average a county that implements a community solar project sees a $0.002 per kilowatt hour ($/kWh) increase in the residential retail price of electricity. Counties that adopted projects in 2015 drove this aggregated increase, as we see residential retail prices for these counties rose by almost $0.01 per kWh. For an average household that consumes 893 kWh of electricity in a month,[26] a cent difference in an area's residential retail price would lead to an increase of around $108 in a household's annual electricity expenditures. My discussion of cross-subsidization in Section 2 support this estimate, as utilities may look to improve their revenue potential through consumer wide price increases due to reduced income from net metering customers. This is in line with Johnson et al. (2014) who estimated only modest increases in non-adopter expenditures. This may further highlight the potential biases within the reported insignificant and negative expenditures estimated in Table 5. The opposite effects of adoption on direct household expenditures and retail prices may also contain a number of omitted considerations like household consumption changes and county energy efficiency improvements. Likewise, rises in electricity prices may be due to external price increases in other fuel sources — like natural gas — that are correlated with shifts to renewable energy adoption, regulatory policies, and even the presence of other solar systems which are not considered in this analysis. These are discussed more in Section 6.

[26] See *Frequently Asked Questions* (2021).

Table 6

Two-Way Fixed Effects and Group-Specific Effects for Residential Retail Electricity Price

	Residential Retail Price ($/kWh)			
	National		County	
	(1)	(2)	(3)	(4)
TWFE	0.00335*** (0.00103)	0.00274** (0.00109)	0.00051 (0.00067)	0.00070 (0.00067)
Group-Specific Effect	0.001** (0.0004)	0.0006 (0.0005)	0.0011 (0.0009)	0.002** (0.0008)
$g = 2014$	0.0029 (0.0025)	0.0029 (0.0027)	0.0017 (0.0026)	0.0019 (0.0028)
$g = 2015$	0.005*** (0.0011)	0.0046*** (0.0012)	0.0058*** (0.0014)	0.0063*** (0.0015)
$g = 2016$	0.0003 (0.0006)	-0.0005 (0.0006)	0.0010 (0.0013)	0.0008 (0.0011)
$g = 2017$	0.0003 (0.0006)	0.0001 (0.0006)	0.0009 (0.0009)	0.0002 (0.0009)
$g = 2018$	0.000 (0.0007)	-0.0008 (0.0008)	0.0003 (0.0011)	0.0011 (0.0009)
$g = 2019$	-0.0002 (0.0013)	-0.0007 (0.0012)	---	---
N	21,748	21,736	2,606	2,606
Parallel Trends	0.001	0.017	0.170	0.286
Controls	No	Yes	No	Yes

Note. Standard errors are listed in parentheses. Data are collected from 2013-2019. Columns (1) and (3) are unconditional models. Columns (2) and (4) are conditional on demographic characteristics. Columns (1) and (2) use all not-yet-treated columns as a control group from nationwide counties. Columns (3) and (4) use not-yet-treated county observations as controls, allowing for better parallel trend assumptions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2 Event Study: Exposure to System Adoption

Tables 7 and 8 report the exposure-specific and aggregated event study estimates from Equations (7) and (8). Total exposure periods are limited to $e = 9$ for household expenditure estimates and $e = 5$ for retail price estimates. In Table 7, columns (1) and (2) follow similar invalidations of causality due to rejection of both unconditional and conditional parallel trends. The parallel trend assumptions in column (4) are met under a conventional 5% significance level. The aggregated effect of treatment exposure on household expenditure is insignificant, but significantly large for counties with the longest exposure to project adoption. These trends align with the reported estimates in Table 5. Outside of shared bias and invalidation of parallel trends, both tables report generally negative impacts of adoption on expenditure with increased validity when restricting comparisons to eventually treated county groups. Figure 1 visualizes the event study plot reported in column (4). No significant pre-treatment effects are reported for project adoption. Individual event times are shown to not be significantly different than zero, as visualized with the 95% confidence bands of each estimate. It is also important to note the fanning of confidence bands with event time. This is likely due to the increasing number of recorded projects in the collected panel data. Fewer projects were installed in earlier periods, leading to a smaller sample of observations for projects experiencing treatment over increased lengths of exposure. Counties that experienced treatment for at least $e = 7$ periods saw on average a decrease of $9 in average monthly household electricity expenditures, however, such effects may be biased due to a limited number of treated counties. In the Appendix, Figures 7, 8, and 9 visualize the event study estimates under the violation of parallel trends for columns (1), (2), and (3). Despite violations of parallel trends, these figures continue to show pre-treatment trends centered around zero.

Table 7

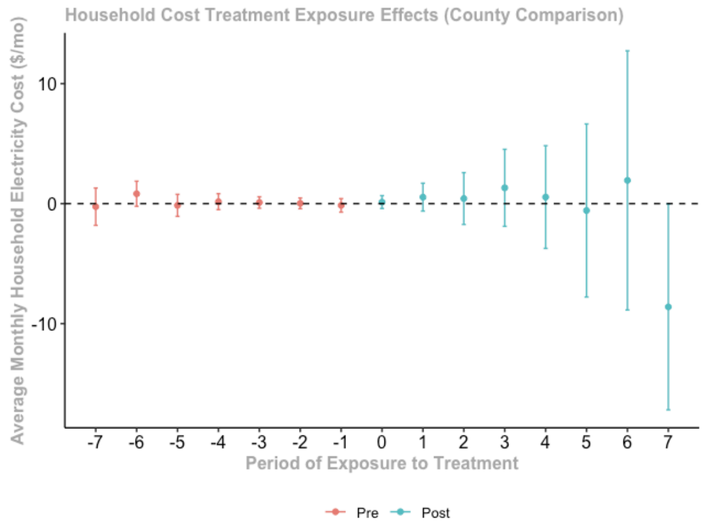
Treatment Exposure Effects for Average Household Electricity Expenditure

	Average Monthly Cost of Electricity ($/mo)			
	National		County	
	(1)	(2)	(3)	(4)
Event Study	-4.049*** (1.327)	-0.856 (0.8693)	-1.565 (1.513)	-0.534 (1.138)
$e = 0$	-0.4533*** (0.1452)	-0.309** (0.142)	0.039 (0.185)	0.132 (0.190)
$e = 1$	-0.497** (0.226)	-0.119 (0.242)	0.335 (0.393)	0.541 (0.456)
$e = 2$	-0.717** (0.350)	-0.072 (0.343)	0.049 (0.632)	0.426 (0.769)
$e = 3$	-1.392*** (0.525)	-0.286 (0.536)	0.630 (0.957)	1.319 (1.144)
$e = 4$	-0.619 (0.838)	1.152 (0.835)	-0.922 (1.332)	0.549 (1.580)
$e = 5$	-2.835** (1.246)	0.574 (1.258)	-3.308* (1.837)	-0.571 (2.600)
$e = 6$	-6.753*** (1.564)	-1.667 (1.688)	-2.798 (2.057)	1.937 (3.960)
$e = 7$	-7.539*** (1.881)	-1.987 (1.930)	-4.447 (2.945)	-8.604*** (3.084)
$e = 8$	-10.012*** (2.973)	-4.992* (2.948)	-3.663 (7.674)	---
$e = 9$	-9.671 (7.214)	---	---	---
N	34,444	31,289	4,164	3,781
Parallel Trends	0.000	0.001	0.000	0.110
Controls	No	Yes	No	Yes

Note. Treatment exposure effects are measured under Event Study where *e* represents the number of periods a unit is exposed to the treatment. Standard errors are listed in parentheses. Columns (1) and (3) are unconditional models. Columns (2) and (4) are conditional on demographic characteristics. Columns (1) and (2) use all not-yet-treated columns as a control group from nationwide counties. Columns (3) and (4) use not-yet-treated county observations as controls to attain conditional parallel trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1

Conditional event study of average effect by length of exposure for average household electricity expenditure in county comparison



Note. Event study estimates were found to be valid due to sufficient parallel trends.

Under Table 8, nationwide comparisons continue to be biased due to the rejection of parallel trends outlined in Section 5.1. However, columns (3) and (4) of the county-level comparison report validation of parallel trends. Under the conditional event study, I estimate that on average, the exposure of a county to community solar adoption leads to an increase of $0.004 per kWh in the residential retail price of electricity. This is much larger than the estimated increases in Table 6. The aggregated estimate also appears to be driven by counties with at least 2 to 3 periods of exposure, which is consistent across columns (3) and (4).

Table 8

Treatment Exposure Effects on Residential Retail Electricity Price

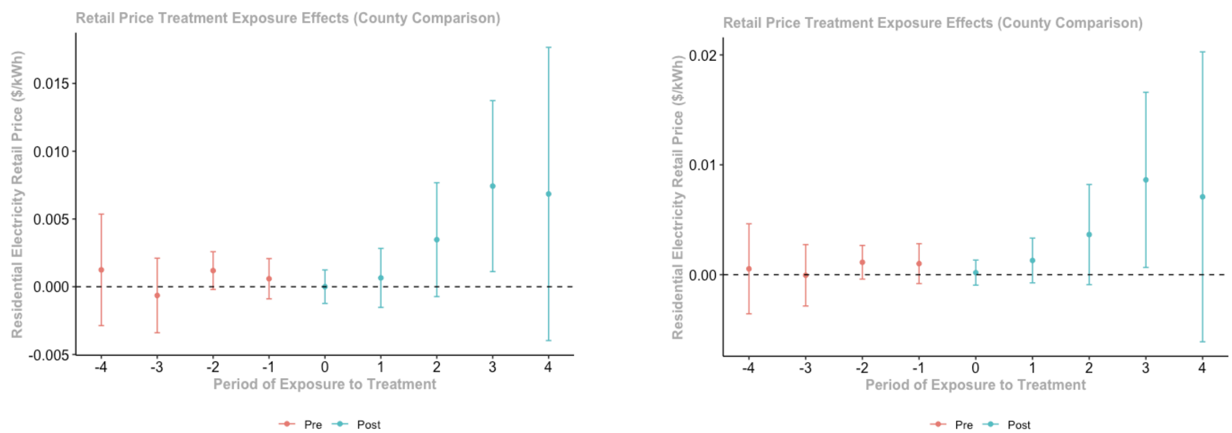
	Residential Retail Price ($/kWh)			
	National		County	
	(1)	(2)	(3)	(4)
Event Study	0.003*** (0.001)	0.0025*** (0.001)	0.0037** (0.0018)	0.0042** (0.002)
$e = 0$	0.0005 (0.0004)	0.0002 (0.0004)	0.0000 (0.0005)	0.0002 (0.0004)
$e = 1$	0.0011*** (0.0004)	0.0007* (0.0004)	0.0007 (0.0009)	0.0013 (0.0008)
$e = 2$	0.0013*** (0.0005)	0.0008 (0.0006)	0.0035** (0.0016)	0.0036** (0.0017)
$e = 3$	0.0028*** (0.0009)	0.0021** (0.0009)	0.0074*** (0.0025)	0.0086*** (0.0028)
$e = 4$	0.0060 (0.0016)	0.0056*** (0.0016)	0.0068 (0.0047)	0.0071 (0.0058)
$e = 5$	0.0062* (0.0035)	0.0059* (0.0035)	---	---
N	21,748	21,736	2,606	2,606
Parallel Trends	0.001	0.017	0.170	0.286
Controls	No	Yes	No	Yes

Note. e represents the number of periods a unit is exposed to the treatment. Standard errors are listed in parentheses. Columns (2) and (4) are conditional on demographic characteristics. Columns (1) and (2) use all not-yet-treated columns as a control group from nationwide counties. Columns (3) and (4) use not-yet-treated county observations as controls, allowing for better parallel trend assumptions. * p < 0.10, ** p < 0.05, *** p < 0.01

Figure 2 visualizes the county aggregations, highlighting further insignificant pre-treatment trends and positive treatment effects post-adoption. Increased exposure to the treatment also shows increased effects on retail price with an almost $0.01 per kWh increase in counties exposed for at least $e = 3$ periods. This further aligns with the utility mechanisms discussed in Section 2 and 5.1. Across the estimated effects of adoption on retail prices, there is a consistent trend of positive effects. There is a consistently stronger validation of parallel trends associated with county-level, not-yet-treated comparisons, supporting the potential omission of variables biasing the nationwide analysis. Likewise, the described differences in Table 3 highlight the strength of analyses concerning electricity rates as opposed to household expenditure. This is inevitably due to the dependence of household expenditures on electricity consumption opposed to the rate-based measure of electricity prices.

Figure 2

Event studies of average effect by length of exposure for residential retail price of electricity in county comparison group.



(a) Unconditional Retail Price

(b) Conditional Retail Price

Note. Event study estimates were found to be valid due to sufficient parallel trends.

6 Conclusion

In this paper, I contribute to the growing exploration of the impact of solar photovoltaic transitions on households. I do this through an investigation of the effects of county-level community solar system adoption on household expenditure and electricity prices. I take advantage of traditional and alternative empirical methods to measure treatment effects under staggered difference-in-differences designs. Under a two-way fixed effects difference-in-differences model, I find negative trends across nationwide comparison groups under estimations of household expenditure, although consistently significant and positive effects of adoption on residential electricity retail prices. Due to potential biases in TWFE estimates under staggered treatment designs, I employ the methodologies of Callaway and Sant'Anna (2021) to conduct estimates of group-specific and event study effects of adoption (Baker et al., 2022; Callaway & Sant'Anna, 2021; Sun & Abraham, 2020; Goodman-Bacon, 2021; Jakiela, 2021). Under a conditional framework and the restriction of comparison groups to eventually-treated county units, I find significant and positive effects on residential retail prices of electricity across both group and event study aggregations. I also find insignificant effects on average monthly household energy expenditure. The opposing estimations of insignificant or even negative effects of adoption on household expenditure compared with positive effects on residential retail prices highlight potential unconsidered factors in my analysis. If these price increases are present, research highlights a concern that energy insecurity within these counties may be exacerbated should energy insecure communities continue to face disparate benefits of solar adoption.

Variation in energy consumption, potentially through energy efficiency improvements across time and counties, is a plausible source of decreases in household electricity expenditures. It is also likely the case that the relative penetration of community solar in electricity markets is not significant enough to influence utility or household behavior. However, the noticeably positive impacts on retail prices under county comparisons acknowledge the hypothesized impacts of solar PV adoption on prices.

I note the consistent violation of parallel trends under the nationwide comparison, even when controlling for demographic characteristics which were found to vary across groups. As a result, causal estimates should not be inferred from nationwide comparisons, and should be taken lightly for county comparisons. The inclusion of potential confounding factors, such as installed residential capacity or energy market penetration, time and location variant temperature data, alternative fuel prices, as well as policy or regulatory controls, may be useful for improving the precision and unbiasedness of the estimated treatment effects.

The presence of other installed residential systems within a county should be considered for future research. If community solar adoption is highly correlated with other residential systems, it would be necessary to control for these systems to specifically isolate community solar adoption effects. Another potential factor, as mentioned in Section 3, is the indication of regulated and deregulated markets. If a county is subject to a regulated market where price changes are decided by a commission or some other government entity, then direct impacts of adoption are not plausible. Appendix C provides an analysis of household expenditure and retail price impacts when conditioning on an indicator of general state regulated or deregulated markets. Lastly, alternative fuels that contribute to electricity generation, like natural gas, may also influence changes in the $/kWh rate for households. The inclusion of these prices should be considered by future researchers. Despite my findings, applying full trust in the causality of the estimated effects of community solar adoption is not convincing given these limitations.

This thesis does not directly measure described cost-shifts or cross-subsidizations from one group to another (non-adopters to adopters). It instead indicates potential price changes that may affect all households within a given location. While this provides a potential concern for non-adopters, I cannot truly isolate such effects. Future research should attempt to measure the direct impacts of project implementation in order to contribute to expanding explorations of the efficiency and equity of solar energy transitions.

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Appendix A: Figures and Tables

A.1 Visualizations of Demographic Characteristics

Figure 3 visualizes the median household income of counties across treatment groups. Outside of the noted differences in average median income, both groups appear to have right ward skews in their distributions. This does not mean that treated county subscribers to projects also have higher levels of income. It is plausible that projects located in these higher income counties may focus on supplying energy to marginalized households. However, there is no indication that the alternative is also true.

Figure 3

Density Chart of County Median Income by Treatment Group

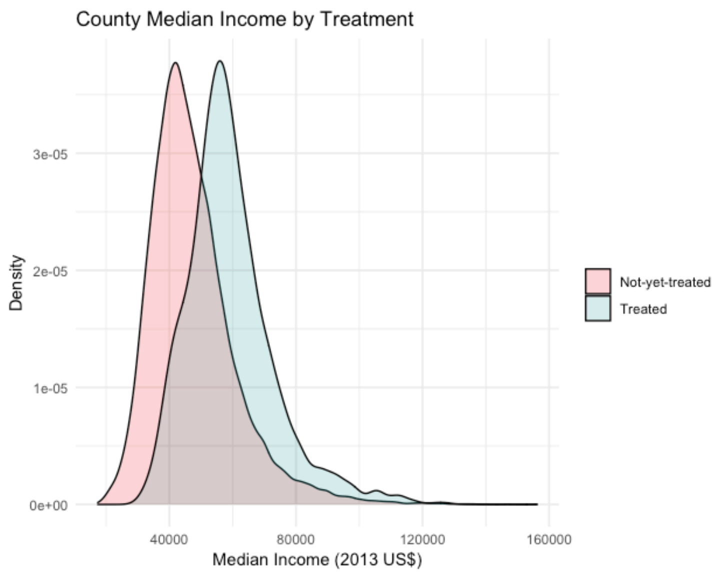


Figure 4 visualizes the percent of county households identified as being owned by their tenants. Although significantly different, treated and not-yet-treated counties do not see large variation in ownership. Treated counties have a higher proportion of renters, which may align with the goals of community solar in eliminating adoption barriers for renters. Without exact subscriber-level data, it is plausible that these projects do not specifically cater toward renter households in these counties. Both groups are slightly skewed to the left, noting higher renter levels in some counties.

Figure 4

Density Chart of Percent of Tenant Owned Households by Treatment Group

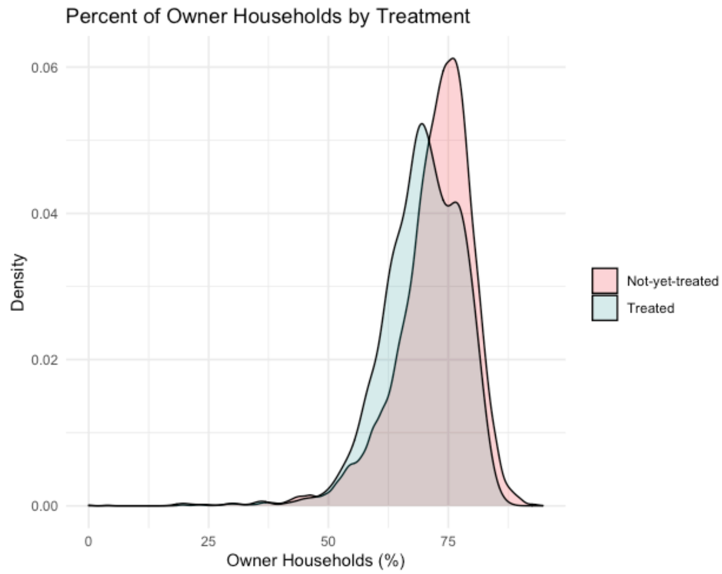


Figure 5 identifies the educational attainment of these households. While the percentage of the population 25 years or older with a high school degree is actually larger in not-yet-treated counties, the differences in educational attainment appear to be a result of treated counties' overall higher levels of educational attainment. Both counties have rightward skews in their proportions of Bachelor's Degree holders. However, treated counties show a much higher density of counties with 25 to 40% of their population attaining a college degree. Figure 6 supplements the insignificant findings of income inequality using the GINI coefficient. Both treatment groups sit around a coefficient of 0.44 with relatively symmetrical distributions. Further research should seek to understand subscriber-level demographics compared to overall county or other geographic level characteristics to better understand how community solar systems seek to alleviate common disparities in residential solar adoption.

Figure 5

Density Chart of Percent of Population Identified as High School and College Graduates by Treatment Group

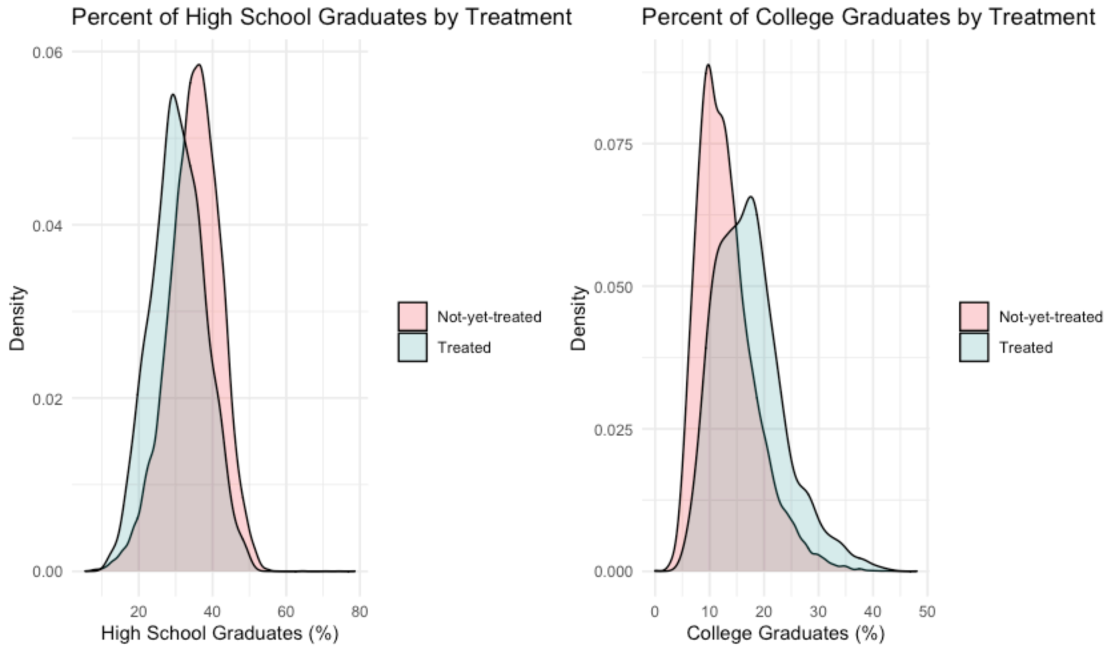
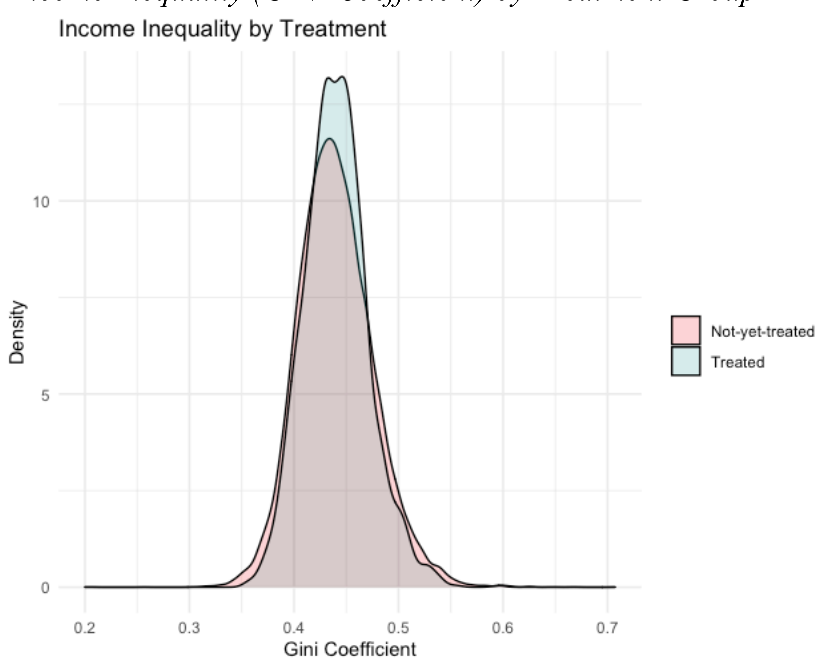


Figure 6

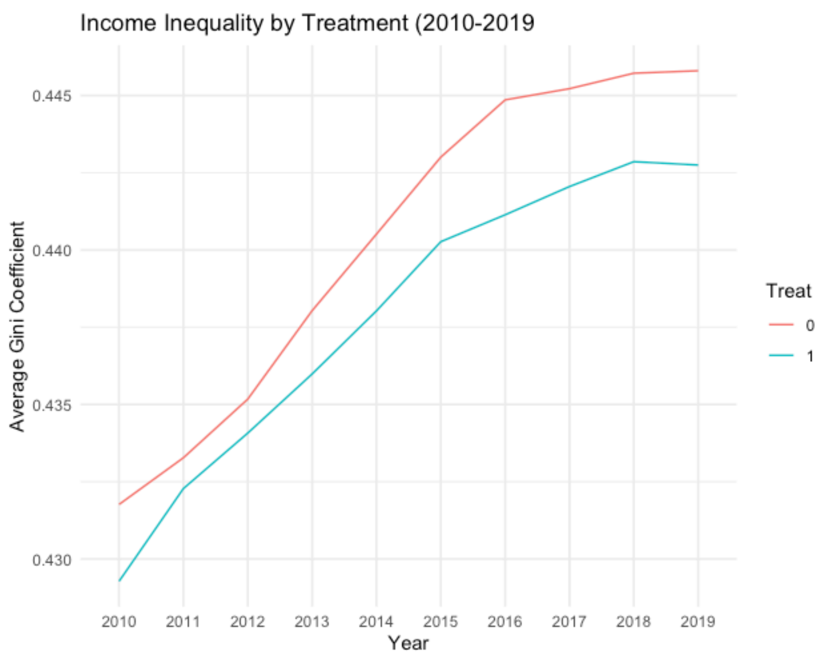
Density Chart of Income Inequality (GINI Coefficient) by Treatment Group



Income inequality across time highlights an interesting dynamic among non-adopting and adopting counties. Figure 7 shows a steady increase in average income inequality among both treated and never treated counties. There is also a divergence overtime, as the difference in inequality overtime appears to increase, but this difference is not significant as outlined in Table 4.

Figure 7

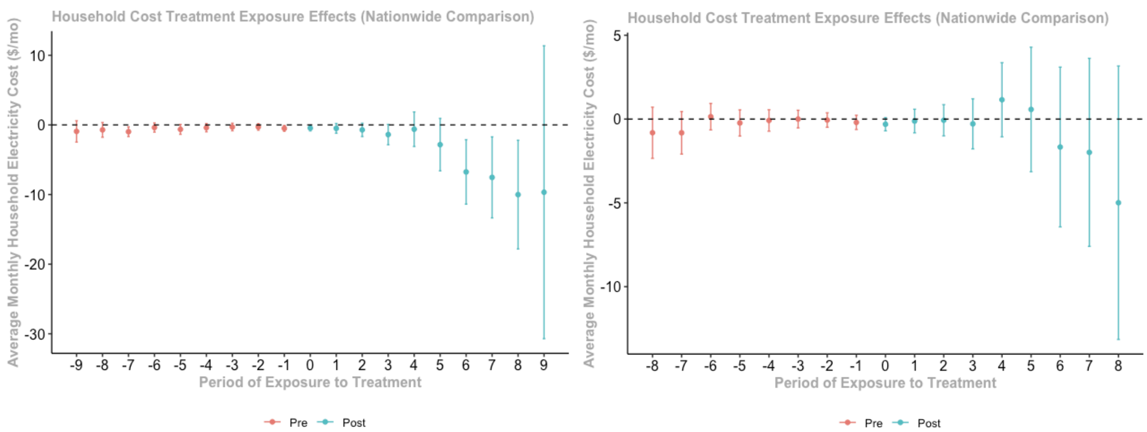
Panel of Income Inequality (2010-2019)



A.2 Event Study Visualizations for Invalid Parallel Trends Assumptions

Figure 8

Event Studies of Average Effect by Length of Exposure for Average Household Electricity Expenditure in Nationwide Comparisons.

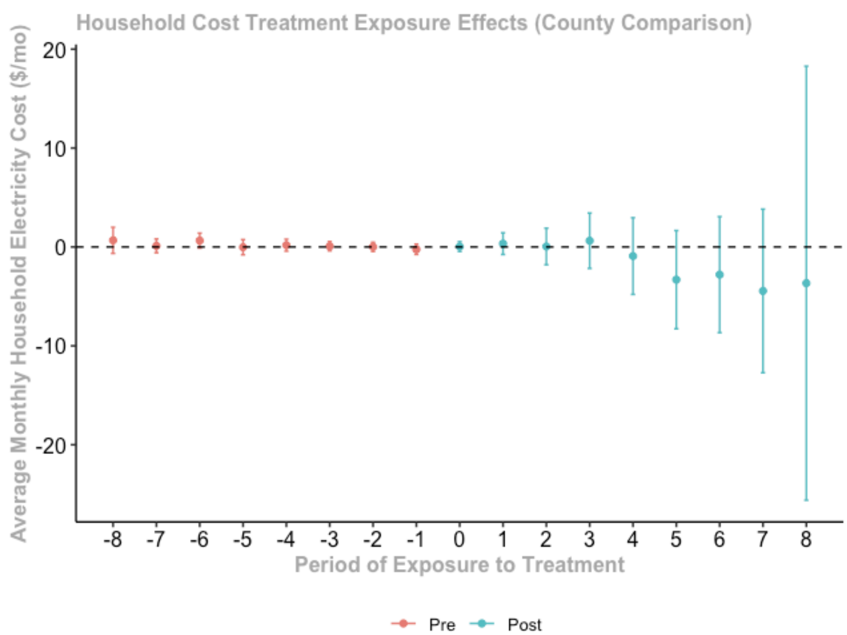


(a) Unconditional (b) Conditional

Note. Event study estimates were found to be invalid due to rejection of parallel trends.

Figure 9

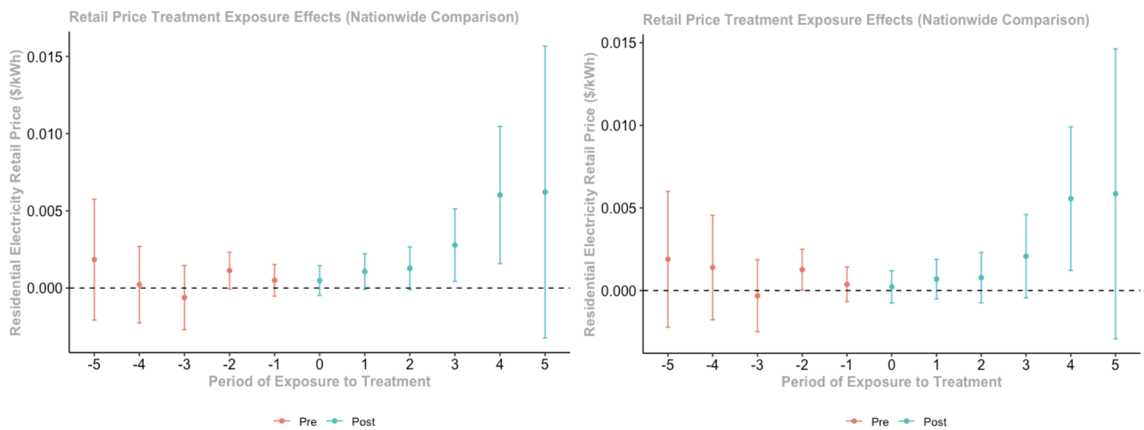
Unconditional Event Study of Average Effect by Length of Exposure for Average Household Electricity Expenditure in County Comparison



Note. Event study estimates were found to be invalid due to insufficient parallel trends.

Figure 10

Event Study of Average Effect by Length of Exposure for Residential Retail Price of Electricity in Nationwide Comparison.



(a) Unconditional (b) Conditional

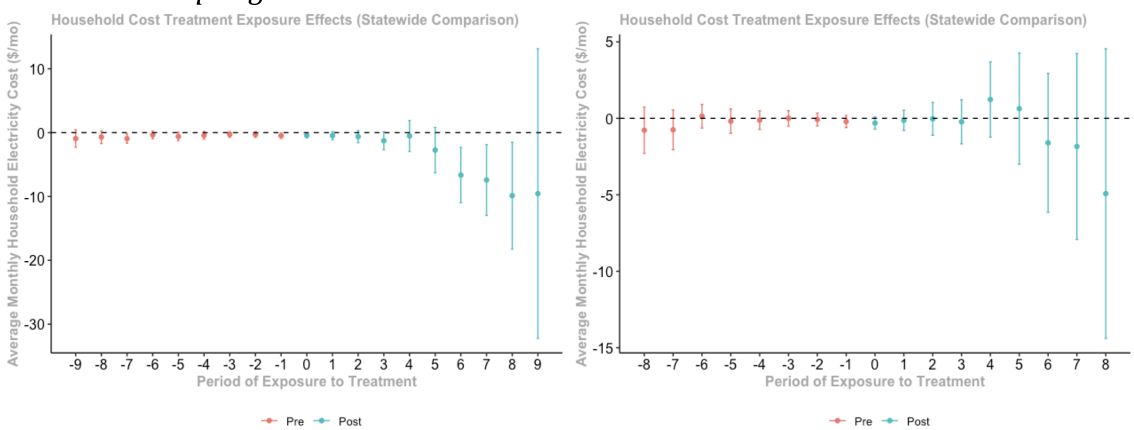
Note. Event study estimates were found to be invalid due to insufficient parallel trends.

A.3 Regression and Graphical Results Restricted to Adopting States

In addition to county comparisons, I conducted analyses that limited comparison groups to only states with treated counties. All statewide comparisons violated parallel trends, and therefore were not considered in the reported results. However, I provide their event study graphs.

Figure 11

Event Study of Average Effect by Length of Exposure for Average Household Electricity Expenditure in Adopting States.

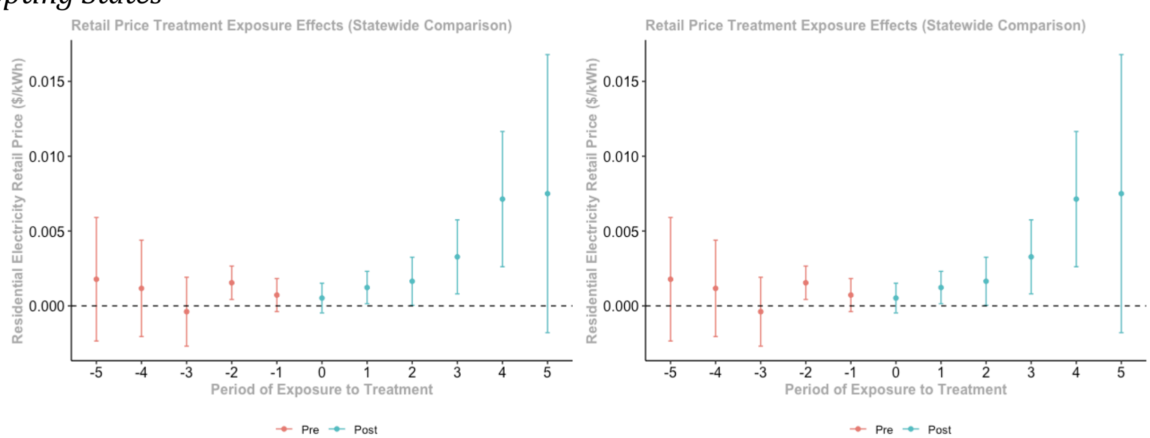


(a) Unconditional (b) Conditional

Note. Event study estimates were found to be invalid due to insufficient parallel trends.

Figure 12

Event Study of Average Effect by Length of Exposure for Residential Retail Electricity Price in Adopting States



(a) Unconditional (b) Conditional

Note. Event study estimates were found to be invalid due to insufficient parallel trends.

Appendix B: PUMA-County Crosswalk

I relied on the PUMA-to-county matching of the Missouri Census Data Center (MCDC) Geographic Correspondence Engine to allocate PUMA-based household electricity cost measurements to their respective county's (Geocorr 2018 – MCDC). The consolidation relied on population allocation factors using 2010 Census population counts described below:

$$\mathrm{A}_{cp} = \frac{Population_{cp}}{\sum_{c=1}^{l} Population_{cp}} \tag{B.3}$$

where c is a county index, p is a PUMA index, and l is the maximum number of counties in a given PUMA. A_{cp} is the allocation factor for county c in PUMA p. Average PUMA electricity costs are calculated as a weighted average of Census household weights. A further weighted average is taken to determine county weights for any counties that intersect two different PUMAs or multiple PUMAs contained within a single county for final county-level average monthly household electricity costs:

$$CountyWeight_c = \frac{\mathrm{A}_{cp} * TotalHouseholds_p}{\sum_{p=1}^{k} \mathrm{A}_{cp} * TotalHouseholds_p} \tag{B.4}$$

$$\#\ of\ County\ Households_c = \mathrm{A}_{cp} * TotalHouseholds_p \tag{B.5}$$

$$ElectricityCost_C = \frac{\sum_{c=1}^{C} CountyWeight_c * ElectricityCost_c}{\sum_{c=1}^{C} CountyWeight_c} \tag{B.4}$$

where $TotalHouseholds_p$ is the total number of households in PUMA p and $CountyWeight_c$ is the proportion of PUMA p households in county c for all households in PUMA p. $ElectricityCost_C$ is the final weighted average household monthly electricity cost for the completely aggregated county C. $ElectricityCost_c$ is the average monthly electricity cost for households in county c. $ElectricityCost_C$ is the fully aggregated weighted average of monthly electricity costs of households in county C.

Appendix C: Controlling for Regulation in Retail Price Analysis

The following section includes an additional control variable in the TWFE DID and aggregated models to indicate whether a state hosts fully regulated or deregulated energy markets.[27] Table 9 shows improved validation of parallel trends. However, the group-specific and event study estimates show reduced significance in aggregate. Counties first adopting in 2015 continue to show significant effects.

Table 9

DID Effect Estimates on Residential Retail Electricity Price with Deregulation Control

Residential Retail Price ($/kWh)			
TWFE	0.00070 (0.00067)		
Group-Specific Effect	0.0013 (0.0009)	**Event Study**	0.0036 (0.0022)
$g = 2014$	0.0016 (0.0030)	$e = 0$	0.0001 (0.0004)
$g = 2015$	0.0046** (0.0019)	$e = 1$	0.0009 (0.0008)
$g = 2016$	0.0005 (0.0013)	$e = 2$	0.0024 (0.0019)
$g = 2017$	0.0003 (0.0010)	$e = 3$	0.0072** (0.0036)
$g = 2018$	0.0012 (0.0008)	$e = 4$	0.0074 (0.0060)
N		2,606	
Parallel Trends		0.545	
Controls		Y	

[27] States were identified under the US EPA *Policies and Regulations (2022)*.

While it appears necessary in future applications to apply regulatory controls, the following estimations are inaccurate. Variation within state energy market policies is an important factor to consider, which this analysis does not do. Regulation is not necessarily static within a deregulated state. This is especially important considering that community solar projects are under a mix of potentially regulated IOUs and municipally-owned Non-IOUs (*US Electricity Markets 101,* 2022). Future research would need to identify the specific regulatory identity of individual utilities or project communities in order to truly control for price effects.