Analyzing the Effectiveness of State-level Solar Policy Incentives in the Western United States

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Abstract

This paper investigates the effects of solar deregulation and policy incentives on county-level, residential photovoltaic (PV) panel installations from 2004 to 2016. I use a Tobit regression model to analyze policy impacts in California, New Mexico, Oregon, and Texas. Results run counter to conclusions from previous studies on solar PV adoption in the Northeast United States. I find that property tax incentives and residential installation deregulation have much stronger effects on adoption rates than both cash rebates and sales tax waivers.[1][2] Average marginal effects indicate that policy incentives have substantial impacts on installment rates. Property tax waivers, for example, appear to more than double residential installations, signaling highly effective incentives that should be investigated by non-participating state governments.

Keywords:

Solar PV
Renewable energy policies
Incentives for solar PV
Residential solar PV

JEL Classification:

Q42
Q48

[1] I would like to thank Dr. Jean-Francois Houde for critiques during office hours and Dr.'s Christine Crago and Ilya Chernyakhovskiy for an excellent paper that provided the basis for my research.
[2] Special thanks to Dr. Naim Dargouth at Lawrence Berkeley National Laboratory. Naim provided solar panel installation data weeks prior to public release, which allowed this project to be completed within the given timeframe.

I. Introduction

I.I. Motivation

The climate crisis is growing in severity and this has led to calls for more sustainable energy alternatives in recent years. Of all renewable energy sources, solar energy has taken center stage as many states push for easily implementable, low-cost alternatives to traditional carbon fuel sources. This push has come in the form of massive financial incentive packages aimed at lowering the installation costs of residential and commercial solar panel projects. California, alone, has allocated over one billion taxpayer dollars to solar initiatives since 2000 and every continental state now has some form of renewable energy benefits. The scale and variability of these benefits across the United States provides a rich set of data to analyze. Moving into the 20th year of large solar initiatives, it is time to re-examine the effectiveness of these policies to see if installations are being maximized in an efficient and timely fashion. In addition, it is important to understand whether policies have differing impacts across the United States.

I.II. Research question

I answer the following questions in this paper: "how strongly have state-issued financial incentives impacted the growth of residential solar panel installations in the Western United States? And which incentives seem to be most effective?" Previous papers have shown positive causal effects for policy incentives ranging from sales tax write-offs to net metering, but financial policy incentives are only half of the story. Regulatory incentives in the form of solar access deregulations have largely been left out of the literature and I argue that these policies have a significant effect on installations.[3] Studying financial and regulatory incentives, together, allows for a more complete picture of the variables driving solar installations.

I.III. Main contributions

My paper contributes to existing literature across several dimensions. Primarily, it reconciles impacts of solar financial incentives with solar deregulation, which, to my knowledge, have yet to be included together in an analysis. I also apply familiar estimation techniques in an entirely new geographic area by looking at policy incentives in the western United States, as opposed to the northeast. This reapplication allows for a more robust debate on internal versus external validity in the realm of public policy. As I will show in the results section, there is good reason to believe that geographic location plays a significant role in the effectiveness of solar policy incentives. Possible reasons for this difference will be hypothesized.

I apply a logarithmic transformation to the dependent variable—Kilowatts of solar panel power added—which allows for an elasticity interpretation of policy effectiveness.[4] By utilizing a logarithmic transformation, I quantify the effect of policies in terms of the percent change in kilowatts added, which is I feel is a more meaningful metric than net kilowatts added. Other studies have used log transformations, but not in the context of deregulation.

[3] Crago & Chernyakhovskiy (2017) do not include regulatory variables in the analysis.
[4] Cameron and Trivali (2010)

I.IV. Methodology & Robustness Check

A times-series, Random Effects (RE) Tobit model was the estimator of choice for a few reasons. First, a mass point of observations with "zero kilowatts added" existed in the dataset, to which Crago & Chernyakhovskiy (2017) selected a Tobit model to deal with this problem. I keep the same method of estimation to allow for more apt comparisons between my results and Crago's results. Second, timestamped installation data was not completely reliable. It is unclear when exactly a solar system was purchased, when refunds were received, and when the system itself was installed. This eliminated the possibility of a Poisson regression, which is largely reliant on the average time between events being precisely known (Woolridge 2010).

The robustness of the final model was checked using different covariates for three separate random-effects Tobit regressions. I will show later in the paper that results are robust to noise in the data generated by the addition of meaningless covariates. Robustness was further verified by using bootstrapped standard errors, which slacken the assumption of a normal distribution in each explanatory variable. Again, I maintained a similar structure to Crago and Chernyakhovskiy (2017) for comparative purposes, since no clear improvements could be made.

I.V. Findings

My findings deviate substantially from my basis paper and I will provide an in-depth explanation of these observed differences in the results section of the paper. In brief, I conclude that property tax waivers and solar installation deregulation are the two largest drivers of residential adoption in the western United States. In addition, I find that cash rebates and sales tax waivers do not have a significant effect on net kilowatts of solar power added at the county-level. The former conclusion is especially interesting since my basis paper argued that cash rebates have the strongest positive effect of all policy incentives.

Marginal effects analysis suggests that homeowners, on average, do not respond to small increases in electricity prices. Changes in population density and total county-level installations in the previous year also have negligible effects on net kilowatts added. Counterintuitively, unemployment appears to be positively correlated with the dependent variable. I will discuss possible explanations for this result in later sections.

I.VI. Roadmap of Paper

The order of my paper is standard to most academic literature in Economics. In Section II, I review the contemporary sources related to my topic and related to my methodology of choice. Following the literature review, I explain the sources used to compile my dataset, the assumptions that were made when modifying data, and discuss limitations that can be addressed in future work. In Sections III and IV, I discuss my research methodology and results, respectively. I address required assumptions in the methodology section and detail my findings in the results section. Finally, Section V concludes the paper with advice for future policy and areas for future research.

II. Literature Review

The basis for my research stems from Christine Crago and Ilya Chernyakhovsky's *"Are policy incentives for solar power effective? Evidence from residential installations in the Northeast" (2017).* Their paper investigates the impact of cash rebates, solar renewable energy credits, sales tax waivers, and third-party ownership on residential installations in the Northeast United States. They analyze state policy at the county level between 2004 and 2012 for much of the eastern seaboard using an IV Tobit with random effects. They determine that cash rebates have the strongest, positive impact on installations, while all other variables of interest have insignificant and negligible impacts on installations. The Northeastern United States is a unique market for solar installations since inconsistent sunlight throughout the year lessens effectiveness of panels. Crago and Chernyakhovskiy attempt to control for fluctuations in sunlight using a variable called *solar resource*, which is a measure of sunlight intensity calculated on a per-county basis. I choose to omit this variable for a few reasons. Primarily, solar intensity is a much different measure than days of sunlight per year. Intensity is based on the number of photons that reach the Earth's surface, which is not directly correlated with days of sunlight.[5] Because intensity varies depending on latitude, longitude, and seasonal weather patterns, it is difficult for me to believe a solar PV consumer can accurately factor intensity into their net present value calculations. Intensity can also vary substantially across large counties, which becomes a problem when working with aggregated data. California counties such as Mendocino, Humboldt, and Monterey are each over 100 miles in length, so an individual installing panels at the north end of a county may be exposed to significantly different solar intensity as opposed to a south end resident. For these reasons, solar intensity was left out of my analysis.

Studies conducted by Kwan (2012), Hughes and Podolefsky (2015), and Burr (2014) restrict themselves to investigating the impact of a single policy. Crago and Chernyakhovskiy (2017) extend the analysis to multiple policies, but do not include property tax incentives. To my knowledge, I am unfamiliar with any research that analyzes the impact of property tax incentives on solar installations, so I attempt to shed light on the effectiveness of property tax policies in the western United States. It is important to note that states analyzed in prior research may not have offered property tax incentives, which makes a one-to-one comparison of my results to others' results difficult. What my analysis does add to the literature is an understanding of how property tax incentives function alongside other previously researched policy incentives.

III. Data

III.I Overview

The data in this paper spans a 13-year period from 2004 to 2016 and is indexed across counties from 4 states located in the southwestern/western United States: California, Oregon, New Mexico, and Texas. Several

[5] https://earthobservatory.nasa.gov/features/EnergyBalance/page2.php

modifications were made to the dataset received from Naim Dargouth at Lawrence Berkeley National Lab.[6] Initially, the data provided detailed, individual installations for commercial, residential, industrial, and military consumers in dozens of states. The kilowatt size of these installations ranged from as small as 1 KW (Kilowatt) to several MW (Megawatts). All installations greater than 10 KW were dropped from the dataset, and I restricted observations to residential installations within the 4 states of interest during 2004-2016. The 10 KW threshold was first established in Kwan (2012) as an upper limit for residential systems. A zipcode-to-county crosswalk was utilized to index each observation at the county level. Observations that could not be matched with a unique county were dropped from the dataset.[7] Individual observations were then aggregated at the county-level to generate "total KW added" and "total installations" on a per-year basis.

I dropped all counties that experienced 10 or fewer total installations from 2004 to 2016, which alters the interpretation of results slightly.[8] Previous studies opted to keep all counties in the dataset, however western states are unique in that some areas are nearly inhospitable. Counties that had less than 10 installations over the period also had negligible populations. Thus, the number of installations each year can be attributed more to randomness than any specific policy incentive because the sample population is so small. Dropping specific counties means results should be interpreted as the effect of policy incentives on counties who are already installing residential solar panels. In effect, this is identical to the interpretation given in my basis paper. A sample visual of my data structure can be seen below:

Data Structure Example

County	*Year*	*Prop. Tax*	*Elec. Price*
6001	2004	0	9.59
↓	↓	↓	↓
6001	2016	1	14.53
6024	2004	0	10.53
↓	↓	↓	↓
6024	2016	1	16.98

Figure 1: Visual example of data structure

My dataset contained 1378 observations, which is comparable to the 1750 used in Crago and Chernyakhovskiy (2017). Dependent variable instances equal to zero were classified as censored within the Tobit framework and represented 475 out of 1378 observations. This ratio of 34% censored to 66% uncensored observations is coincidentally identical to the dataset in my basis paper.

[6] Dr. Naim Dargouth contact info: https://emp.lbl.gov/staff/naim-darghouth

[7] In some cases, zipcodes can fall into multiple counties. Observations that could not be pinned down to a specific country were removed, which was less than 1% of the data.

[8] Eight counties were dropped using this criterion.

III.II Policy Variables

Policy variables included in this study are *Property Tax*, *Sales Tax*, *Rebate*, *Access*, and *Net Metering*. All variables were collected from the DSIRE online database, which keeps an up-to-date record of renewable energy policies at federal, state, and county levels. Each policy was classified as a dummy variable where 1 indicated a policy in effect and 0 indicated no policy. *Access* refers to state deregulation of residential solar PV installation and only appears in Texas. Deregulation, in this case, means a slackening of rules prohibiting installations. Beginning in 2011, homeowner's associations (HOA's) were no longer able to prevent community residents from adopting solar technology. The three other states in my model all deregulated solar prior to 2004, so *Access* strictly applies to Texas. Even with the lack of variability, deregulation serves as an interesting case study when compared to other causal variables. At least three incentives from *Property Tax, Sales Tax, Rebate, and Net Metering* were implemented in each state, which provides a stronger case for external validity of their coefficients.

Many state-level policies were implemented throughout the year, which presents some bias in the coefficients since county-level observations vary by year. For example, if a policy was implemented in June 2011 for county I.D. 6017, then the entire year would be assigned a policy indicator equal to one. Mid-year policy implementation causes the model to misattribute solar panel additions to policy. Still, I chose to maintain a yearly times-series structure for a few reasons. First, instances of mid-year policy implementation were not rampant in the data. Most policies were effective in either the first or last quarter of the calendar year. If a policy was put into effect in February 2009, then 2009 was assigned an indicator equal to one. On the other hand, if a policy became effective after September 2009, then 2009 was assigned an indicator equal to zero while 2010 was assigned an indicator equal to one. Using this method of assignment, I was able to substantially mitigate bias caused by over or under classification. The second reason I maintained a yearly structure was due to data sparsity. The count of installations in states besides California did not exceed 1000 on a per-year basis until 2011. Indexing the data on a quarterly basis would have shrunk the sample size and increased variance. Balancing bias and variance is important to the validity of this analysis, so I felt maintaining a yearly index was most appropriate.

III.III Market Variables

My dependent variable is *Ln(KW Added)*, which captures the log of cumulative KW of solar power added at the county-level. *KW Added* (untransformed) was contained in the dataset I received from Berkeley Laboratory. Transforming this variable was not completely straight-forward as 475 observations in the dataset were equal to zero prior to the transform. I corrected for this issue by adding 0.01 to every *KW Added* observation and then censoring the data at -4.6051 after taking logs. By applying a minor linear transformation and censoring out to four decimal places, I did not adversely affect the distribution of the dependent variable or misclassify observations in the censored category. I would like to re-emphasize that the dependent variable is not necessarily correlated to total installations per year because residential solar PV systems vary continuously from less than 1 KW to 10 KW, while installations are a discrete variable.

Another market variable I include is *time trend*, which is an indicator of year. This variable roughly captures the effect of decreasing solar installation costs that have occurred between 2004 to 2016. There was no reliable source of installation costs available at the county-level and fixed effects cannot be added into times-series Tobit models without introducing bias, so including a time trend was my best effort at capturing installation cost changes. I assume that costs do not vary by geographic location and decrease linearly on a yearly basis.

The final two market variables I use are *Electricity Price and Ln(Unemployment)*. Electricity prices were gathered from the U.S. Energy Information Administration online data browser.[9] Prices vary at the state-level and were converted from nominal dollars to 2004 base year adjusted dollars. State-level data presents limitations since electricity prices vary based on utility provider. However, there is not much evidence to suggest that electricity prices vary substantially from county to county.[10] Unemployment data were gathered at the county-level in the form of 12-month net changes using August as the base month. Net changes were preferred over yearly rates because county-level data was not seasonally adjusted. By comparing net changes over a 12-month period, the effect of seasonality is diminished but not eliminated. A logarithmic transformation was applied to allow for elasticity interpretations of marginal effects.

III.IV Demographic Variables

Population density and *housing density* are variables that have been used as controls in the existing literature (Crago 2017, Hitaj 2012). I collected these county-level data from the Bureau of Economic Analysis; both variables calculated density using a square mile metric.[11] Logarithmic transformations were applied to both variables to deal with highly skewed distributions. For example, a 2017 estimate of Los Angeles County population density is 2490 persons per square mile, while sparsely inhabited locations such as Inyo county have 2 persons per square mile.[12]

III.V Environmental Preference Variables

Costa and Kahn (2013) show that democratic party affiliation plays a role in support of renewable energy and climate change policies. As a result, solar PV installations may occur more frequently in counties that have higher populations of registered democratic voters. To control for environmental preferences generated by party affiliation, I collected presidential voting data and linearly interpolated to fill in missing observations between elections. Data sources are from Secretary of State websites listed in the Appendix.

III.VI Omitted Variables

Several variables were either omitted from my final Tobit model or were omitted entirely from the dataset. Solar insolation or *solar resource* (Crago 2017) was left out due to reasons explained in Section II. To recap, insolation, at best, weakly contributes to net-present value calculations of a solar PV system. I assume that insolation

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[9] https://www.eia.gov/electricity/data.php
[10] https://www.eia.gov/energyexplained/electricity/prices-and-factors-affecting-prices.php
[11] https://www.bea.gov/
[12] https://www.opendatanetwork.com/

is not correlated with any of my covariates (Eq. 1), which means that its effect will be captured by the error term. For these reasons, I choose to exclude insolation from my analysis.

$$E[insolation|X] = 0 \tag{1}$$

Another deviation from my basis paper was the exclusion of *hybrid vehicle registrations (%)*. The data I needed to fill this variable was not readily available and I argue that the time-range of my sample lessens the significance of hybrid registrations. The market has become increasingly saturated with hybrids and plug-in electrics following 2012, which was the upper bound for Crago (2017).[13] Growing popularity of these vehicles among the general public lessens their correlation with environmental preferences. For these reasons I omit vehicle registration data. Finally, *income per capita, number of homes,* and *population* were included in the composite dataset, but left out of my final Tobit estimations. I exclude these variables for reasons that I will cover in Section V.III.

IV. Methodology

IV.I Model

The model I chose to conduct my analysis is a times-series Tobit with random effects at the county level and bootstrapped standard errors, predominantly consistent with my basis paper. Equation 2, below, details the linear specification I implement:

$$lnY_{it} = \alpha_{it} + P_{it}\beta + M_{it}\gamma + D_{it}\delta + E_{it}\theta + \vartheta_i + \epsilon_{it} \tag{2}$$

My dependent variable, lnY_{it}, is the log of total kilowatts of residential solar PV power added in county i at year t. The intercept, α_{it}, is an important change from my basis paper as I allow it to fluctuate by county and by year. Crago and Chernyakhovskiy (2017) interpret their intercept, α, as "average additional solar PV capacity installed across all counties and time periods, holding all explanatory variables constant at zero". By adding a time and county index, I interpret the intercept as the number of installations in county i at year t, holding all explanatory variables equal to zero. My specification may permit the effect of *solar insolation* to be absorbed into the model, since fixed effects cannot be added without causing bias.[14]

The variables on the right-hand side of Equation 2 are my independent variables. Coefficients on each variable require scaling in order to interpret an average marginal effect (AME)—I discuss the reasoning behind this transformation and its shortcomings in Section IV.V. Causal variables are contained in the policy vector, P_{it}, which include: *Property Tax, Sales Tax, Access, Net Metering,* and *Rebate.* Vectors M_{it}, D_{it}, and E_{it}, contain control variables for market characteristics, demographic characteristics, and environmental preferences, respectively. Finally, ϑ_t and ϵ_{it} represent a county level fixed effect and an additive error term. As noted, ϑ_t does not appear in the estimated

[13] https://afdc.energy.gov/data/
[14] See Section IV.II

model and is absorbed into both the error term and intercept. It is within reason to believe county-level fixed effects bias results, but my inclusion of an intercept is an attempt to isolate this bias.

The linear specification in Equation 2 is subject to "censoring", which would be more aptly described as an adjustment due to a corner solution. I detail the conditions under which my dependent variable was transformed below:

$$lnY_{it} = \begin{Bmatrix} lnY_{it}^{*}, & if\ Y_{it}^{*} > 0 \\ -4.60517, & if\ Y_{it}^{*} = 0 \end{Bmatrix} \tag{3}$$

Taking the log of the dependent variable also normalizes its distribution to match normalization of heteroskedastic independent variables. There were outliers of *KW Added* at the county-level in areas that contained large cities and a high density of *KW Added* at the year-level after 2012.

IV.II Assumptions, Specifications, Estimation

Standard assumptions were made in the estimation of the RE Tobit model as taken from Woolridge (2010). Equation 4a illustrates the assumption that the error term for every county *i* in year *t* is mean zero given independent variables for that county in year *t*. Equation 4b is a stronger assumption that states the error term is also normally distributed. There is no reason to suspect either of these conditions are violated.

$$E[\epsilon_{it}|X_{it}] = 0 \tag{4a}$$

$$\epsilon_{it}|X_{it} \sim N(0, \sigma^2) \tag{4b}$$

Equations 5a-c detail another set of necessary assumptions for the RE Tobit estimator. Equation 5a says that the error term inserted into the RE Tobit model is composed of two parts: 1) ν_i, which represents county-level effects that are unobserved (random) and fixed over time and 2) ϵ_{it}, which is a county-level, time-variant error. Stemming from 5a, equation 5b highlights the assumption that county-level random effects are not correlated with county-level covariates in any time period. Both 5a and 5b are more so explanations of a concept rather than assumptions, but equation 5c draws a hard line by assuming that covariates are strictly exogenous. 5c requires the belief that the realization of all covariates in time period *t* do not impact the realization of covariates in time period *t+1*, which is a very strong restriction. In my data environment, however, I argue this assumption should hold for most the covariates included. The only breakdown may occur with *Democratic Vote (%)*, since performance of a candidate elected in year *t* will more than likely play a role in the year *t+1* vote. I tread with caution when interpreting marginal effects for *Democratic Vote (%)* considering this plausible serial correlation. I will note that Autor et. Al. (2017) show increasing polarization in United States elections from 1992 to 2012, which would have a downward effect on serial correlation. Further, linear interpolation over presidential elections mitigates transitory voter shocks that may occur on a year-to-year basis.

$$\mu_{it} = \nu_i + \epsilon_{it} \tag{5a}$$

$$\nu_i|X_{it} \sim N(0, \sigma_\nu^2) \tag{5b}$$

$$cov(v_i, X_{it}) = cov(v_i, X_{it+1}) = 0 \tag{5c}$$

I estimate coefficients in my Tobit model using maximum likelihood (MLE), which "accounts for non-linearity due to the high proportion of corner solutions".[15] Nonlinear squares (NLS) was also an option for estimation, but Woolridge recommends MLE for data with corner solutions. The general formula for MLE is displayed below in equation 6.

$$E[Y|X] = \Phi\left(\frac{X\beta}{\sigma}\right)X\beta + \sigma\varphi\left(\frac{X\beta}{\sigma}\right) \tag{6}$$

I considered using a semi-parametric estimator developed in Honoré (1992) to include fixed effects within my Tobit estimator, but contemporary research has shown that Honoré's methods can lead to inaccuracies when interpreting marginal effects and convergence problems for asymptotic standard errors. Greene (2004) investigates these issues in detail and concludes that RE Tobit models outperform modified FE Tobit models in the case of binary dependent variables.[16]

IV.III Tobit Coefficient Scaling & Marginal Effects

In Section V, I discuss results of my tobit estimation that appear in Table 1 by interpreting coefficients as average marginal effects (AMEs). In order to use this method of interpretation, Woolridge (2010) suggests scaling tobit coefficients by a scalar adjustment factor, ξ, listed below in equation 7.

$$\xi = n^{-1}\sum_{i=1}^{n}\Phi\left(\frac{x_i\hat{\beta}}{\hat{\sigma}}\right) \tag{7}$$

$$\widehat{AME}_J = \hat{\beta}_j\xi \tag{8}$$

The scalar adjustment factor plays a role in minimizing inflated coefficients that stem from nonlinear tobit modelling. A pre-requisite for calculating the adjustment factor are bootstrapped standard errors, which help mitigate randomness in the covariates. I use equations 7 and 8 to calculate the marginal effects for all variables, manually, as a check of Stata generated output. Woolridge suggests calculating the adjustment factor in a slightly different manner for binary covariates, but also stated that the method in equation 7 will suffice.

[15] Crago and Cheryakhovskiy (2017)

[16] "If the analysis focused on a dummy variable, as it might in the analysis of a treatment effect, then either of the alternative estimators (RE or pooled) would dominate the MLE/FE – *in spite of the presence of the fixed effects in the data*." – Greene (2004)

V. Results & Discussion

V.I Policy Significance

Table 1, listed to the right, contains my regression results for three separate models with standard errors in parentheses. Column one is an ordinary least squares estimation, column two is a pooled tobit estimation and column three is a random effects tobit indexed by county and year; I will discuss how covariates were selected in Section V.III. My results indicate that *property tax*, *access,* and *net metering* are significant at 1%, 5%, and 5% levels, respectively. *Rebate* and *Sales Tax* do not appear to have a significant effect on kilowatts of solar PV added in the states analyzed in this model. Coefficient values, alone, do not provide insight into elasticities, but Table 1 output suggests that policy incentives operate in a much different manner in the western United States than in the northeast. It is interesting to see such strong significance in *property tax* since this variable was either not included or not available for analysis on policy incentives in northeast states.

Market controls also present substantially different coefficients than the results displayed in my basis paper. I find that *electricity price* is not a significant determinant of *log KW Added*, while *time trend* is significant at the 0.1% level. A concern with *time trend* is its correlation with the dependent variable, since year increases linearly and *KW Added* increases monotonically in all four observed states. Due to this correlation, I do not take the coefficient

Table 1: Model Selection

	(1) OLS	(2) Pooled Tobit	(3) RE Tobit
Indep. Vars.			
Rebate	-0.197	-0.798	-0.979
	(0.37)	(0.59)	(0.54)
Property Tax	1.344***	3.768***	4.112**
	(0.36)	(0.94)	(1.23)
Sales Tax	0.291	-0.0260	0.109
	(0.50)	(0.62)	(0.66)
Access	0.656	2.093**	1.678*
	(0.43)	(0.85)	(0.86)
Net Metering	1.608***	2.867***	1.365*
	(0.32)	(0.45)	(0.58)
Time Trend	0.402***	0.523***	0.615***
	(0.05)	(0.09)	(0.09)
Electricity Price	0.0212	0.0644	-0.056
	(0.09)	(0.17)	(0.17)
Ln(Pop. Dens)	1.376**	2.278***	2.031
	(0.50)	(0.89)	(1.97)
Ln(Housing Dens.)	-0.890	-1.607*	-1.474
	(0.52)	(0.95)	(2.11)
Ln(Unem.)	2.166***	3.345***	3.901***
	(0.26)	(0.40)	(0.76)
Democratic Vote (%)	3.850***	5.258***	7.748***
	(0.77)	(1.20)	(1.88)
Lag(Installs)	0.001	0.001	0.000
	(0.00)	(0.00)	(0.00)
Constant	-817.8***	-1071.0***	-1255.9***
	(102.57)	(181.58)	(178.45)
var(Ln(KW Added))	X	20.83***	X
		(1.02)	
sigma_u	X	X	2.590***
			(0.17)
sigma_e	X	X	3.727***
			(0.23)
Observations	1378	1378	1378
Rsq	0.452	X	X
BIC	7272	6120	5882

Standard errors reported in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

value (or its marginal effects) too seriously. Its only purpose was to effectively capture decreasing costs of solar PV installations, which I feel was successful given the significance level.[17]

V.II Average Marginal Effects & Discussion

In addition to calculating Tobit coefficients, I calculated the average marginal effect for each variable included in my final RE Tobit model. These values can be compared directly to OLS estimates listed in Table 1, but, due to the nature of Tobit regressions, these marginal effects should be interpreted as an upper bound (Woolridge 2010). True coefficient values likely fall between the estimates in Table 2 and the OLS estimates found in the previous subsection, if we assume the model is correctly identified.

Table 2: Average Marginal Effects

	(1) RE Tobit
Policy Vars.	
Rebate	-0.386 (0.257)
Property Tax	1.620*** (0.428)
Sales Tax	0.043 (0.315)
Access	0.6613* (0.320)
Net Metering	0.538* (0.268)
Market Vars.	
Ln(Unem)	1.537*** (0.259)
Lag(Installs)	0.000 (0.000)
Time Trend	0.243*** (0.033)
Electricity Price	-0.019 (0.060)
Demographic Vars.	
Ln(Pop. Dens.)	0.800 (1.12)
Ln(Housing Dens.)	-0.581 (0.727)
Env. Pref. Vars.	
Democratic Vote (%)	3.053 (0.776)
Observations	1378

Standard errors reported in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The logarithmic transformation of the dependent variable permits an elasticity interpretation of average marginal effects, which is a proxy for average treatment effect (ATE) in binary cases. Of the significant policies, implementation of a property tax is associated with a 162% increase in kilowatts of solar power added by residential consumers on average. *Access*, which prohibits HOA regulations on solar panel installations, is associated with a 66% increase in KW added on average. Finally, *net metering* is associated with a 53% increase on average. The magnitudes of these results are shocking at first, but they are not far off from estimates found in my basis paper (albeit for different policies.) There is likely correlation with omitted policy variables at federal and local levels, which causes upward bias on state policy coefficients. But even accepting upward bias, it is hard to pass off the strength of the results replicated in three different estimation methods.

Sales Tax and *Rebates* do not influence county-level installations in any significant way. It is highly unlikely that solar panel rebates deter consumers from investing, so I ignore the negative value and assume that the impacts of both policies are negligible. I find the preceding conclusions interesting, because they suggest that there is something about property tax waivers in the western United States that is far more enticing to consumers than sales tax waivers or cash rebates. I explain this result due to the size of property tax waivers being offered. California offers a property tax write-off equivalent to 100% of the system value, which far exceeds sales tax waiver values or cash rebates.[18] If I replicated this study, I would include interaction terms to better capture marginal effects with each policy. Implementing property tax incentives and net metering options does not automatically

[17] Intuitively, we expect individuals to respond to the sticker price of a solar system in the same manner as any other good or service. If *time trend* was not significant, then I would be concerned with the entire analysis.
[18] https://programs.dsireusa.org/system/program/detail/558

bolster installations, but the magnitude of coefficients imply that state policies are working well, even if a specific elasticity cannot be pinned down.

In terms of market variables, a 1% increase in unemployment is associated with a 1.53% increase in kilowatts added. It is difficult to intuit a reason as to why this result is observed in the data and I cannot make any definitive claims without conducting additional research. One possible explanation is that the rise of solar power occurred just before the Great Recession, so the initial wave of installations was linked to years of increasing unemployment. However, Figures 2 and 3 show that solar installations began to spike in the years following the Great Recession when unemployment fell by more than 10% in some states.[19] The most likely explanation for this strange elasticity is that seasonally adjusted data was not used. I had hoped that using a 12-month net change with August as the base month would suffice, but it wound up presenting errors in analysis.

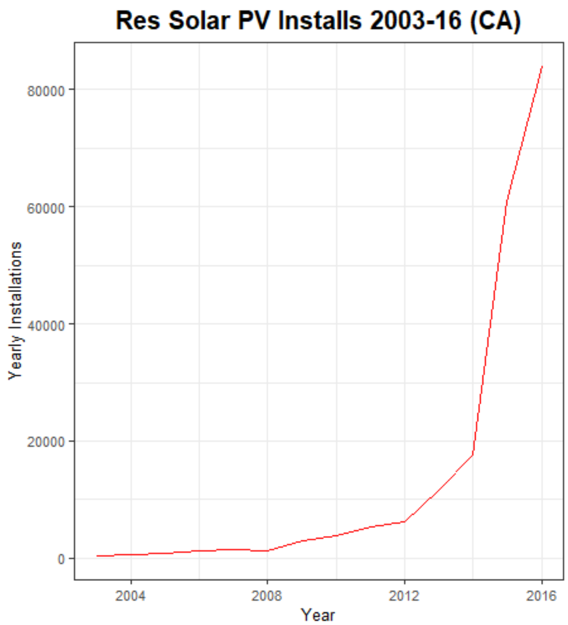

Figure 2: Residential solar PV installations in California from 2003 to 2016

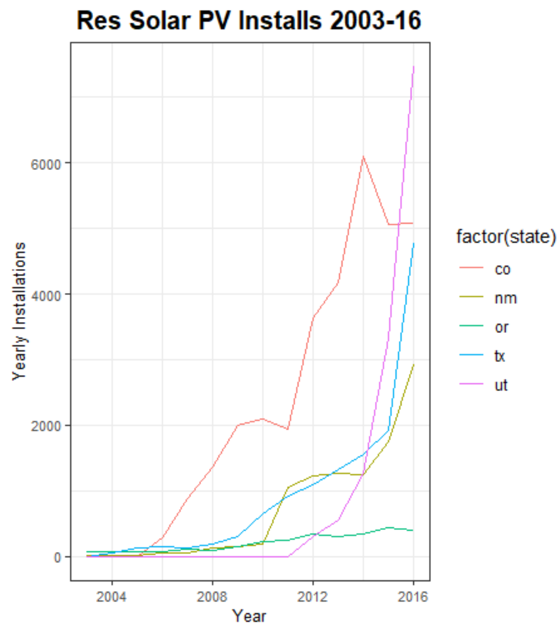

Figure 3: Residential solar PV installations in various western states from 2003 to 2016.

Electricity price does not appear to be correlated with KW of solar power added, which differs from results in Crago (2017), but there is some intuition, here. Real electricity prices (in 2004 USD) likely do not change drastically enough for consumers to alter behavior. It may be more interesting to replicate the analysis and leave prices in nominal form—stark differences would provide insight into residential consumer decision-making.

Demographic variables do not have any significant effect on panel installations which is a sharp departure from previous literature. My basis paper estimated that a 1% increase in housing density is correlated with 6.5% additional kilowatts of solar power, while a 1% increase in population density is correlated with a 3.7% decrease in kilowatts of power. I attribute this contrast to geographic and climatic variance between the northeast and the west.

[19] https://fred.stlouisfed.org/series/CAUR

Finally, *democratic voting percentage* appears to be a sound representation of "warm glow" and/or environmental preferences. All else equal, democratic voters increasing by 1% in any given county is correlated with a 3.1% uptick in kilowatts of solar power. I am very confident in the accuracy of this variable due to the suppression of transitory voter shocks through linear interpolation and related research by Autor et. Al (2017). This conclusion leads me to suggest that federal solar policies should be focused in states with large democratic representation. Whether this style of public policy is "fair" or not is a debate left to another field.

V.III Robustness Checks

I implemented robustness checks using three different covariate selection procedures, which can be seen to the right in Table 3. Model 1 (Barebones) only used market variables along with all 5 policy variables of interest and garnered a BIC of 5978. Model 2 was filled with all covariates and achieved a lower BIC of 5900. Finally, Model 3—the optimal linear specification used in analysis—generated the lowest BIC of 5882.

Table 3: Robustness Check for Optimal RE Tobit Model

	(1) Barebones	(2) Bloated	(3) Optimal
Indep. Vars			
Rebate	0.919	-0.866	-0.979
	(0.51)	(0.69)	(0.60)
Property Tax	5.806***	4.180***	4.112***
	(0.97)	(0.89)	(1.17)
Sales Tax	1.026	0.287	0.109
	(1.01)	(0.82)	(0.95)
Access	1.672**	1.622*	1.678*
	(0.64)	(0.70)	(0.70)
Net Metering	1.289	1.300*	1.365*
	(0.66)	(0.59)	(0.56)
Lag(Installs)	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)
Time Trend	0.527***	0.603***	0.615***
	(0.08)	(0.09)	(0.07)
Electricity Price	-0.164	-0.113	-0.0563
	(0.20)	(0.17)	(0.14)
Income per Capita		0.000	
		(0.00)	
Population		-0.000	
		(0.00)	
Ln(Pop. Density)		2.349	2.031
		(1.74)	(1.73)
Homes		0.000	
		(0.00)	
Ln(Housing Dens)		-1.983	-1.474
		(1.92)	(1.87)
Ln(Unem)		3.835***	3.901***
		(0.60)	(0.56)
Democratic Vote (%)		7.274***	7.748***
		(1.39)	(2.00)
Constant	-1065.5***	-1231.2***	-1255.9***
	(165.76)	(173.93)	(145.13)
sigma_u	3.313***	2.531***	2.590***
	(0.23)	(0.25)	(0.18)
sigma_e	3.856***	3.729***	3.727***
	(0.28)	(0.24)	(0.25)
Observations	1378	1378	1378
BIC	5978	5900	5882

Standard errors reported in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3 shows that significant policy variables are robust to changes in explanatory covariates. Due to this fact, I conclude noise and endogeneity are not overly significant contributors to coefficient values observed in Model 3 and average marginal effects observed in Table 2. However, slackening the strict exogeneity condition means admitting bias in the causal variables, which was something I discussed at length in the prior section.

VI. Conclusion

In summary, my results indicated that property tax incentives are by far the most effective policy measures in the western United States followed by residential deregulation and net metering. Without data that is rich and varied in property tax waivers, I cannot draw any conclusions on an optimal rate, but can unequivocally state that property tax incentives significantly and substantially increase total kilowatts of solar PV added at the county-level. The difference between my elasticity estimates and those provided in Crago & Chernyakhovskiy (2017) are convincing evidence that northeastern states need to strongly consider adding property tax waivers to their menu of incentives. Renewable energy growth does not have the luxury of waiting for the market to transition as climate change continuously shrinks our available time to act. Even if the true elasticity of property tax waivers is 1/10th of my 162% estimated treatment effect, I would argue that implementation is still a worthy cause. As a country, the United States is leaving substantial untapped potential for renewable energy on the table.

VII. References

VII.I Articles/Books

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VII.II Electronic Resources

NASA Earth Observatory: https://earthobservatory.nasa.gov/features/EnergyBalance/page2.php

California Secretary of State Voting Data: https://www.sos.ca.gov/elections/voter-registration/voter-registration-statistics/

New Mexico Secretary of State Voting Data: https://www.sos.state.nm.us/voting-and-elections/data-and-maps/voter-registration-statistics/

Oregon Secretary of State Voting Data: https://sos.oregon.gov/elections/Pages/electionsstatistics.aspx

Texas Secretary of State Voting Data: https://www.sos.state.tx.us/elections/historical/70-92.shtml

Bureau of Labor Statistics Unemployment Data: https://data.bls.gov/lausmap

Bureau of Economic Analysis Population, Income, Housing Data: https://apps.bea.gov/

Lawrence Berkeley National Lab Data: Available upon request

Renewable Energy Policy Data: https://programs.dsireusa.org/

Electricity Price Data: https://www.eia.gov/electricity/data.php