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Rooftop solar incentives remain effective for low- and moderate-income adoption

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Abstract

Financial incentives for rooftop solar photovoltaic (PV) adoption have declined in the United States over time by policy design. Incentive phase-down can efficiently promote early adoption and avoid ineffective payments to late adopters. However, incentive phase-down may exclude low- and moderate-income (LMI) households from realizing the same financial benefits from PV adoption as high-income early adopters. Here, data from two state-level LMI PV incentive programs are analyzed to test whether incentives still drive PV adoption among LMI households. As a first order approximation, the analysis suggests that incentives drove adoption that would not otherwise have happened in about 80% of cases. To the extent that policymakers prioritize PV adoption equity as part of the emerging energy justice policy agenda, the results suggest that ongoing incentive support for LMI adoption may be merited.

Keywords

rooftop solar; low-income adoption; energy justice

1. Introduction

Most U.S. states have directly incentivized rooftop PV adoption through rebates, ongoing production-based incentives, and net metering (NC CETC 2018). These incentives played an instrumental role in driving early PV adoption (Kwan 2012, Sarzynski, Larrieu et al. 2012, Hughes and Podolefsky 2015, Crago and Chernyakhovskiy 2017). Rebates and production-based incentives have generally phased down or out by policy design (Nemet 2019). Incentive "phase-down" design is theoretically efficient (Alizamir, de Véricourt et al. 2016, Ding, Zhou et al. 2020). Phase-down ensures that larger incentives are available when technology prices are high. Large initial incentives can reduce the risks associated with adopting new, expensive, and relatively unproven technologies, thus fostering early adoption. Early adoption, in turn, can support scaling and learning-based technology cost reductions. As technology costs and prices decline, phase-down ensures that incentives also decline to avoid inefficient payments to free riders, i.e., individuals who would have adopted PV even without incentives. Further, phase-down can prevent individuals from inefficiently delaying their own adoption to wait for higher expected net benefits in the future.

Incentive phase-down poses equity and energy justice problems due to the inequitable adoption of PV. Relatively high-income households have been significantly more likely to adopt PV than low- and moderate-income (LMI) households (Barbose, Forrester et al. 2021). As a result, state-funded incentives have predominantly benefited high-income early adopters (Borenstein 2017). Like most emerging technologies, LMI households have and will continue to become more likely to adopt PV as prices decline (O'Shaughnessy 2021). LMI households could therefore increasingly benefit from these incentives, particularly given evidence that LMI households face similar motivations to adopt PV as high-income households (Wolske 2020). However, these incentives are phasing down or out just as PV prices have brought PV adoption within financial reach for LMI households.

At least 46 state and local programs have emerged in the United States to support LMI PV adoption (Paulos, Forrester et al. 2021). A common approach in these programs is to provide means-tested financial incentives for income-qualifying households. In some cases, LMI incentives have persisted even as other incentives have phased down. For instance, California set aside 10% of funds for LMI incentives under the state's California Solar Initiative (Navigant 2015). While other incentives have phased out, LMI households remain eligible for incentives under California's set-aside for LMI customers. However, it is important to recognize that despite the recent proliferation of LMI PV incentive programs, LMI incentive programs are much smaller than state programs for broader PV adoption. Only about 1% of rooftop PV systems installed to date have been supported by means-tested incentives (O'Shaughnessy, Barbose et al. 2021).

LMI incentives could theoretically address some of the equity challenges posed by incentive phase-down. Beyond questions of equity, LMI incentives may still be a necessary component of nascent LMI rooftop PV markets. That is, whereas incentives without income eligibility requirements may simply subsidize PV adoption that would have occurred otherwise, means-tested incentives may still drive PV adoption that would not have occurred without the incentive support. However, research on the impacts of these programs is scarce. Several LMI incentive programs have implemented program evaluations, however these evaluations generally only document program effort (e.g., number of incentives distributed) without rigorously estimating program impacts (Paulos, Forrester et al. 2021). These simple evaluations do not provide insights into the degree to which LMI incentives drive LMI PV adoption.

This study fills this research gap on the role of means-tested incentives in LMI rooftop PV markets. The study builds on the available literature, summarized in Section 2, in two ways. First, the effects of specific programs are analyzed using case studies based in California and Connecticut. In doing so, the magnitude of program impacts can be placed in the context of program size to assess the degree to which these incentives drive new adoption. The analysis suggests that most distributed LMI incentives in these case studies drove adoptions that would not have otherwise occurred, providing evidence that incentives remain a key factor in supporting LMI adoption. Second, I explore the hypothesis that LMI incentives generate spillover impacts by increasing PV adoption among LMI households that do not receive incentives themselves.

2. Background

In the early years of the U.S. rooftop PV market (~2000-2010), relatively high-cost PV was disproportionately adopted by high-income households willing to buy innovative green products at a premium (Wolske, Stern et al. 2017). PV adoption has become more equitable over time but remains inequitable at the current stage of diffusion. In 2019, only about 21% of PV adopters earned less than 80% of their area's median income (Barbose, Forrester et al. 2021), a threshold commonly used to define LMI households in LMI programs. O'Shaughnessy et al. (2021) estimate that a household earning more than $200,000/year was about 4 times more likely to adopt PV than a household earning less than $50,000/year in 2018.

A direct consequence of inequitable PV adoption is that a majority of the financial benefits of PV adoption—including state-funded incentives—have flowed to high-income households (Borenstein 2017). Incentive phase-down therefore generally implies that LMI households will be shut out from many of the financial benefits realized by high-income early adopters (Baker 2021). Beyond this historic inequity, persistent PV adoption inequity poses at least three potential policy issues. First, inequitable adoption represents a missed opportunity to use rooftop PV deployment as a pathway to alleviate LMI energy burdens, that is, the disproportionately high shares of income that LMI household dedicate to energy expenses (Bednar and Reames 2020). Second, inequitable PV adoption could—and arguably already has—undermine public opinion of rooftop PV and drive regulatory reforms that ultimately stymy future PV deployment (Welton and Eisen 2019). Third, nearly half of PV-viable rooftop space in the United States is located on LMI buildings (Sigrin and Mooney 2018). Inequitable PV adoption could therefore ultimately reduce or at least delay the realization of the clean energy benefits of rooftop PV.

By the early 2010s, several states began to address inequitable PV adoption through a variety of policy measures. The most common approach was to set aside a portion of state incentives for income-qualifying households. Paulos et al. (2021) identify 46 programs that explicitly support LMI PV adoption. O'Shaughnessy et al. (2021) show that these LMI incentive programs can increase PV adoption equity by increasing LMI adoption rates. Further, the authors posited that LMI incentives could have beneficial spillover impacts by increasing LMI adoption even among households that do not receive incentives.

The potential for LMI incentives to spill over into broader adoption has strong support in the social influence literature. That literature shows that households are more likely to adopt PV when their neighbors have done so (Wolske, Gillingham et al. 2020). These so-called peer effects imply that PV installations have spillover impacts by increasing the likelihood of future adoptions in the area. In the context of LMI incentives, an incentive-supported system installed in a low-income area could catalyze peer effects that cause other LMI households to adopt PV.

This study builds on the findings in O'Shaughnessy et al. (2021) in two ways. First, while that study explored LMI incentives from three programs in a single national sample, I analyze two specific case studies and contextualize the results within the sizes of those specific programs. Second, for the first time to the author's knowledge, I directly test the spillover hypothesis by exploring changes in adoption patterns in areas that receive LMI incentives among households that did not receive the incentives.

3. Data and Methods

This study is framed around two research questions:

1. Do LMI incentives continue to drive new adoption among LMI households?
2. Do LMI incentives generate spillover impacts by increasing LMI adoption in under-served areas among households that do not receive incentives?

The primary data source is the publicly-available *Tracking the Sun* (TTS) data set published by the Lawrence Berkeley National Laboratory (Barbose, Darghouth et al. 2020). TTS comprises system-level data collected from various PV programs covering more than 70% of the U.S. PV market, making it the most comprehensive U.S. rooftop PV data set. A public version of the data set is available for download from https://emp.lbl.gov/tracking-the-sun. The TTS data set was augmented with modeled household-level income estimates from Experian. The raw data used for inputs to this study comprise 1,269,783 residential PV systems installed from 2010 to 2019 in 23 states and Washington, DC. The TTS data include identifiers for systems that received LMI incentives. Two of the programs have sufficiently large coverage in the data for evaluation purposes: the California Single-Family Affordable Solar Homes program; and the Connecticut Solar for All program (Table 1).

Table 1. Case Study Descriptions

State	Program Description	Number of Incentives Represented in Data
California	The California Single-Family Affordable Solar Homes program provides up-front financial	8,305

	incentives for income-qualifying households. The current incentive level is $3/W, enough to offset the majority of system and installation costs.	
Connecticut	The Connecticut Green Bank offers LMI incentives for third-party owned systems. Income-qualifying households can sign subsidized 20-year leases for PV systems.	1,375

This study is an ex-post program evaluation. As a result, this analysis must rely on econometric methods designed to control for potentially confounding factors. See Abadie and Cattaneo (2018) for a review of program evaluation methods, which include conditioning on observed variables, differences-in-differences models, instrumental variables, and regression discontinuity design. The primary objective is to isolate incentive-supported adoption that would not have occurred in the absence of the programs. For the purposes of this study, model design was based on two constraints. First, the LMI incentive programs were implemented in a staggered fashion. That is, LMI incentives were gradually distributed over time at different rates in different areas. Second, while LMI incentive program eligibility is discontinuous at the income thresholds, that discontinuity cannot be exploited using modeled household-level incomes. While modeled household-level data are valuable (Tidemann, Engerer et al. 2019), all modeled data include some modeling error, and that modeling error in this case obviates the use of a regression discontinuity design.

The impacts of LMI incentives on LMI PV adoption rates are analyzed using a differences-in-differences (DiD) model. To account for the staggered implementation of the programs, I use the staggered differences-in-differences model described in Callaway and Sant'Anna (2020). That model, known specifically as a group-time model, measures the impacts of some intervention on groups of observations that began the intervention at the same time. In the context of this study, the group dimension is defined at the zip code level, while the time dimension is defined by quarters. Each "group" is a cohort of zip codes that began receiving LMI incentives in the same quarter. For instance, suppose LMI incentives were first distributed in the first quarter (Q1) of 2018 in zip codes W and X and in Q2/2018 in zip codes Y and Z. The model treats W and X as one group distinct from zip code Y and Z. The group-time model was implemented in the following form:

$$r_{g,t} = \sigma_{g,t} + \tau_{g,t} G_g + \rho_{g,t} T_t + \beta_{g,t} G_g T_t + \gamma X + \varepsilon_{g,t} \tag{1}$$

Where $r_{g,t}$ is the LMI adoption rate in group g in quarter t, G_g is an indicator variable for the group of zip codes that first began receiving incentives in quarter g, T_t is an indicator variable for quarter t, $G_g T_t$ is an interaction of the two terms, and X is a vector of controls for median income, number of households in each zip code, the non-LMI adoptions rate, and the percentage of rooftop space that could viably host PV (see definitions in Table 2). Consistent with income eligibility criteria in many federal and state programs, I define LMI households as those earning less than 80% of their county's median income. I used the DiD package in R to implement the group-time models (Callaway and Sant'Anna 2020).

Table 2. Model Variable Definitions and Summary Statistics*

Variable	Description (Source[s])	California Case Study Mean (SD)	Connecticut Case Study Mean (SD)
LMI adoption rate (dependent variable)	Estimated installs by households earning less than 80% of the county median income per quarter per 1,000 households (TTS/Census)	0.51 (1.3)	0.37 (0.76)
Zip code median income	Median household income (Census)	48,998.12 (14,776.56)	43,820.34 (16,614.85)
LMI share of population	Estimated percentage of households in zip code earning less than 80% of county median income (TTS/Census)	0.55 (0.1)	0.68 (0.1)
Zip code population	Number of households in zip code (Census)	10,943.78 (6,684.91)	7,925.12 (4,883)
Non-LMI adoption rate	Estimated installs by households earning more than county median income per quarter per 1,000 households (TTS/Census)	1.15 (2.23)	1.02 (3.76)
PV-viable rooftop space	Percentage of rooftop space in zip code that could viably host PV (Google Project Sunroof)	85.69 (8.65)	83.61 (8.95)

* Case study summary statistics are based on R10 sample

The group-time model measures heterogenous DiD effects across groups of zip codes and across quarters. Because the two programs use household income eligibility criteria rather than location-based criteria there are no clearly defined treatment groups in either program. To increase the robustness of the analysis, I explore three thresholds to identify treatment groups:

- *R0 sample*: Treatment group includes all zip codes where at least one adopter received an LMI incentive
- *R1 sample*: Treatment group includes all zip codes where at least 1% of adopters received an LMI incentive during the study period
- *R10 sample*: Treatment group includes all zip codes where at least 10% of adopters received an LMI incentive during the study period.

The different sample definitions entail tradeoffs between sample size and the extent to which LMI incentives were distributed (Table 3). The R0 and R1 samples are considerably larger in both cases because many zip codes receive a small number of LMI incentives. However, less than 10% of PV adopters in those samples received incentives after incentive distribution began. In contrast, around 19% and 28% of adopters received incentives in the R10 samples in California and Connecticut, respectively, though the small sample sizes compromise statistical power. Given that the R10 sample may provide a more accurate reflection of program impacts, I generally focus on the R10 samples in the Results section.

Table 3. Treatment Sample Summary Statistics

	California		Connecticut	
	Treatment Zip Codes	% Receiving Incentives*	Treatment Zip Codes	% Receiving Incentives*
R0 sample	523	1.8%	115	8.7%
R1 sample	253	4.8%	95	9.4%
R10 sample	38	18.5%	16	28%

* Based on LMI incentive shares in quarters after incentives were first distributed in each zip code

The pool of potential controls includes all remaining zip codes. For each case study, I identified control groups comprising equivalent numbers of zip codes using propensity score matching. The propensity score matching was implemented using the MatchIt package in R (Ho, Imai et al. 2011) using the following formula:

$$incZip = \alpha_0 + \alpha_1 \%CMI + \alpha_2 income + \alpha_3 pv + \alpha_4 roof + \epsilon \quad (2)$$

Where $incZip$ is a dummy variable for zip codes that received LMI incentives, $\%CMI$ is the percentage of the population earning less than the respective county median income, $income$ is the zip code's median household income, pv is the zip code's per-capita PV penetration, and $roof$ is the percentage of rooftop space in the zip code that is viable for hosting PV (based on data from Google Project Sunroof). Using the matched data, balanced panel data sets were generated for the 40 quarters from 2010 to 2019 for the R0, R1, and R10 samples in each case study.

The key identifying assumption in the group-time model, as in other DiD models, is that trends in the treatment and control groups would have been the same without the LMI incentives. Pre-trend testing is one method to increase confidence in the assumption of parallel trends, though pre-trend tests do not discard the untestable possibility of non-parallel post trends (Kahn-Lang and Lang 2019). In this case, I examine the 4 quarters prior to LMI incentive implementation in each of the case studies and split that period into two sub-periods: the 2 quarters immediately prior to program implementation, and the 2 quarters preceding that period. A DiD model was implemented between those sub-periods. If the trends are indeed parallel, one would expect an insignificant coefficient on the interaction term between the treatment group and the time period. Table 4 presents the results of the pre-trend tests for the three samples in each state. The interaction terms are insignificant in all cases except the R0 sample for Connecticut. I therefore focus most of our discussion of the results on the R10 sample.

Table 4. Pre-Trend Test Results

	CA **R0; R1; R10**	**CT** **R0; R1; R10**
treatment	0.06*; 0.07*; 0.21	1.9*; 0.3; 0.58
	(0.02; 0.03; 0.14)	(0.07; 0.2; 1.56)
quarter	0.008; 0.01; -0.004	0.008; 0.02*; 0.10
	(0.005; 0.008; 0.03)	(0.02; 0.01; 0.05)
treatment x quarter	-0.004; -0.007; -0.005	0.10*; -0.03; -0.04
	(0.007; 0.01; 0.05)	(0.03; 0.02; 0.07)
N	2,092; 948; 152	460; 368; 56

* p<0.05

4. Results

The group-time model produces heterogenous estimates for program impacts for group at every time. Unlike conventional DiD models, there is no single metric upon which to evaluate program impacts. I therefore present results in terms of several metrics and visualization methods:

- *Aggregate treatment effects*: The average of all group-time effects across every group and time over the study period.
- *Average first quarter effects*: Every group in the treatment group began receiving LMI incentives in some quarter in the data. I refer to the first quarter in which a group began receiving incentives as simply the *first quarter*. Average first quarter group-time effects are presented across all groups. First quarter impacts are of particular interest in that LMI incentives may have strong initial impacts when incentives first "arrive" in a given area.
- *Average group-time effects*: To visualize the heterogeneous group-time effects, I take the average estimated impacts across groups based on the number of quarters elapsed from the first quarter. For instance, if a group first began receiving incentives in Q1/2014, then Q1/2014 has an *elapsed* value of 0, Q2/2014 has an elapsed value of 1, Q3/2014 has an elapsed value of 2, and so on.

Visualizing results in terms of *elapsed quarters* allows a comparison of the initial versus lagged impacts of incentives on LMI adoption rates.

- *% positive*: Finally, results are presented for the percentage of groups for which the models estimated positive first quarter effects and positive group-time effects in subsequent quarters.

I begin by exploring the first research question on whether incentives directly increase LMI adoption rates in Section 4.1. I then turn to the second research question on whether incentives generate spillover impacts in Section 4.2

4.1 Direct impacts

Table 5 presents metrics to summarize the results of the staggered DiD models in both case studies across the three treatment sample thresholds. The metrics in Table 5 are aggregated statistics of all the group-time effects. Figure 1 provides a visualization of the average group-time effects by elapsed quarters. Again, elapsed quarters refer to the number of quarters from when incentives were first distributed in each zip code. Figure 1 illustrates how the average group-time effects spike in the first elapsed quarter, corresponding to zero on the x-axis. In each scenario and state, group-time effects decline after the initial quarter then steadily grow over time through the remainder of the study period.

Table 5. Staggered DiD Results

Metric	California			Connecticut		
	R0	R1	R10	R0	R1	R10
Aggregate treatment effect	0.33*	0.27*	0.32	0.28*	0.36*	0.47*
	(0.09)	(0.09)	(0.52)	(0.08)	(0.10)	(0.14)
Average first quarter effect	0.12	0.29*	0.99*	0.30	0.42	0.55
	(0.07)	(0.12)	(0.46)	(0.18)	(0.30)	(0.29)
% positive in first quarter	69%	67%	75%	63%	65%	88%
% positive in subsequent quarters	75%	68%	68%	68%	78%	76%
N	41,840	18,960	3,040	9,200	7,360	1,280

* p<0.05

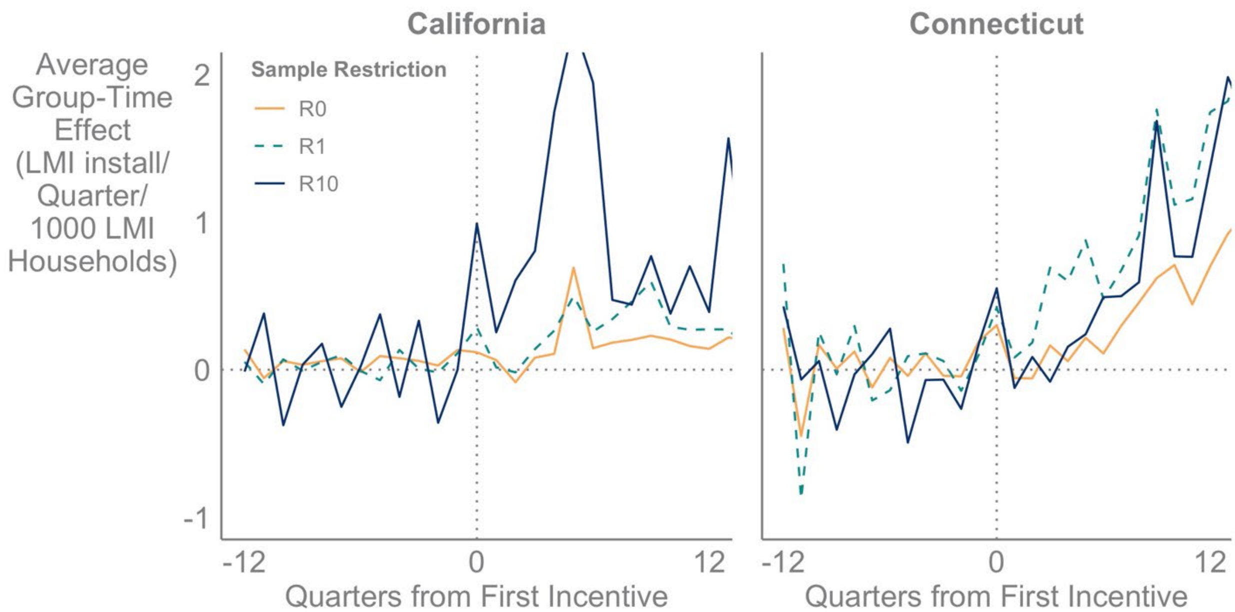

Figure 1. Average group-time effects by elapsed quarters

Focusing first on the aggregate treatment effects, the models suggest that program impacts are on the order of 0.3 to 0.5 additional LMI installs per quarter per 1,000 households. These results are generally robust though statistically insignificant in the

R10 sample of the California case study. Recall that the R10 threshold entails a significant reduction in sample size (see Table 3), reducing the statistical power of the R10 samples, but may more accurately describe program impacts by identifying zip codes where more adopters received incentives. Focusing on the R10 results, based on the average zip code-level populations of LMI households in each case study, the effects translate to roughly an additional 1.9 and 2.4 LMI installs per zip code per quarter in California and Connecticut, respectively. Extrapolating over the whole R10 sample and study period, the aggregate treatment effects suggest that the incentives drove about 2,300 and 490 additional LMI adoptions in California and Connecticut, respectively. These estimates compare to totals of 2,544 and 623 LMI incentives distributed in the R10 California and Connecticut treatment groups. As a first order approximation, the model therefore suggests that around 80% of LMI incentive-supported adoptions were additional to any LMI adoption that would have occurred in these case studies.

Moving to the first-quarter effects, the model suggests that initial program impacts range from 0.1 to 1 additional LMI adoption per quarter per 1,000 households. The first-quarter effects are generally larger than the aggregate treatment effects, suggesting that program impacts are strong initially but diminish over time, as illustrated by the first-quarter spikes in average group-time effects in Figure 1. Again, the results are generally robust, though the first-quarter effects in the Connecticut case study are only weakly significant. Further, the model suggests that first-quarter effects were positive in most zip codes in both case studies. That is, though LMI incentives have heterogeneous impacts, the incentives increase LMI adoption in most zip codes where incentives are received. The R10 samples yield the highest values for the % positive metric given that this sample represents zip codes that received greater shares of incentives. Based on the R10 samples, LMI incentives increased LMI adoption rates in 75% and 88% of zip codes in California and Connecticut, respectively.

Comparing the results across the case studies reveals strikingly different temporal patterns. In California, the model suggests that incentives sharply increase LMI adoption in initial quarters but that these effects decline in intensity over time. In contrast, in Connecticut, the model suggests weaker initial impacts and stronger lagged impacts that occur several quarters after incentives were first distributed. These distinct temporal patterns in program impacts represent similarly distinct underlying patterns in program implementation. In California, incentives have been gradually distributed across time and space (Figure 2). In contrast, incentives in Connecticut appear to have been distributed in waves, with most incentives being distributed a full year after incentives were first distributed in any given zip code. Put another way, Connecticut issued incentives in a more staggered fashion. These temporal patterns in incentive distribution help explain the distinct results in the two programs.

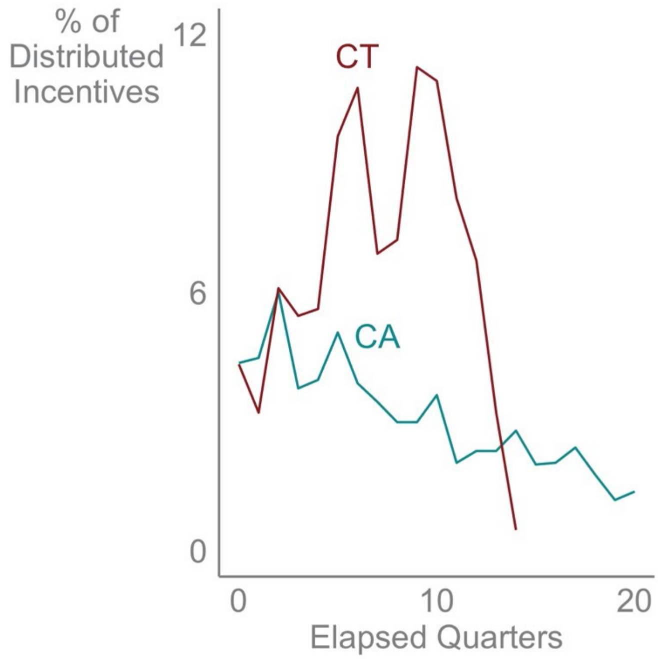

Figure 2. Incentive distribution patterns. This figure shows the percentage of all incentives distributed in each case study by elapsed quarters.

4.2 Spillover impacts

The results from Section 4.1 indicate that LMI incentives drive LMI adoption that would not have otherwise occurred. The social influence literature suggests that these additional adoptions should affect the adoption decisions of the neighbors and peers of those LMI adopters. It follows that the effects of LMI incentives should spill over into additional socially-influenced adoptions. This spillover hypothesis has a strong theoretical basis founded in a robust literature on the role of social influence in PV adoption (Wolske, Gillingham et al. 2020). Beyond social influence, LMI incentives could generate spillovers by changing installer marketing patterns (O'Shaughnessy, Barbose et al. 2021). By jumpstarting dormant LMI markets, LMI incentives could convince installers to begin marketing in previously under-served areas (Baker 2021), though this hypothesis has yet to be rigorously tested.

Detecting and measuring these spillover impacts is a methodological challenge (Angelucci and Di Maro 2015). Further, while there are strong prior reasons to expect spillover effects to exist, there are also strong priors to expect these effects to be small. Bollinger and Gillingham (2012), for instance, estimate that a PV installation in a typical zip code increases the probability of an additional installation in that zip code by about 0.8 percentage points. Based on the aggregate treatment effects, Bollinger and Gillingham's estimate suggests that the case study incentive programs increased the probability of adoption by around 1.4 percentage points in California and about 1.9 points in Connecticut. Based on the adoption rates in the samples, this translates to an expected impact of about 0.01 additional LMI installs per quarter per 1,000 households in both case studies, based on the R10 sample. Statistical power tests suggest that sample sizes of at least 30,000 observations would be required to detect such an effect.

Bearing in mind the methodological challenges, I attempt to analyze spillover impacts by repeating the staggered DiD analysis while excluding LMI incentive recipients. That is, I simply drop records for incentive-supported systems and otherwise repeat the process described in Section 3. The rationale is that spillover effects would be detected as changes in adoption patterns among LMI households that did not receive incentives themselves.

Table 6 presents the results of the staggered DiD models while excluding incentive recipients. The aggregate treatment effects are consistently positive across the models, consistent with the spillover hypothesis. However, the results are only statistically significant at $p<0.05$ in the R0 sample in California and the R0 and R1 samples in Connecticut. The lack of statistical significance is due, in part, to low statistical power stemming from the small sample sizes. Future research could further explore the spillover hypothesis with a more statistically powerful data sample or study design.

Table 6. Staggered DiD Results Excluding Incentive Recipients

	California			Connecticut		
Metric	**R0**	**R1**	**R10**	**R0**	**R1**	**R10**
Aggregate treatment effect	0.28*	0.16	0.05	0.17*	0.23*	0.12
	(0.09)	(0.10)	(0.37)	(0.08)	(0.08)	(0.12)
Average first quarter effect	-0.04	-0.02	0.21	0.04	0.03	0.03
	(0.06)	(0.08)	(0.37)	(0.18)	(0.21)	(0.07)
% positive in first quarter	43%	37%	42%	56%	47%	50%
% positive in subsequent quarters	73%	62%	56%	59%	74%	55%
N	41,840	18,960	3,040	9,200	7,360	1,280

* $p<0.05$

The R0 and R1 sample results are more difficult to interpret and are provided primarily as robustness checks. Recall that both sample treatment groups include large numbers of zip codes where relatively few households received LMI incentives, particularly in the R0 sample (see Table 3). The large estimated impacts in these samples are due largely to correlations between the treatment groups and the marketing patterns of a few large-scale installers, particularly in California. Large-scale installers install systems on LMI households more frequently than other installers (Barbose, Forrester et al. 2021), in part because large-scale installers are more likely to offer leasing models to LMI households (O'Shaughnessy, Barbose et al. 2021). As a result, the marketing patterns of large-scale installers can generate significant swings in LMI adoption rates as these installers move in and out of specific markets. For instance, from Q3 to Q4/2019 the number of LMI installs in one zip code in the R0 California treatment group jumped from 1 to 334, entirely driven by the entrance of a single large-scale installer into that zip code. When excluding the 10 largest-volume installers from the sample, the estimated impact in the R0 samples falls from 0.28 to 0.12 in California. The direct impact group-time results for the California R0 sample are similarly sensitive to installer exclusions. However, importantly, both the direct and indirect group-time results for the R10 samples are robust to the installer exclusions: the aggregate treatment effects are nearly identical, with estimates of 0.31 (SE=0.48) and 0.49 (SE=0.18) in the California and Connecticut R10 samples, respectively.

The temporal patterns of the non-recipient group-time effects likewise support the spillover hypothesis (Figure 3). Unlike the direct impacts test, group-time effects for non-recipients exhibit only a small bump in the first quarter in California and exhibit no trend whatsoever in Connecticut. In both cases, the non-recipient group-time effects hover around zero for several elapsed quarters and only become consistently positive 1 or 2 years after incentives are first distributed. These patterns are consistent with the spillover hypothesis. To the extent that spillovers occur, one would expect a significant lag between LMI incentive program implementation and spillover-driven adoption.

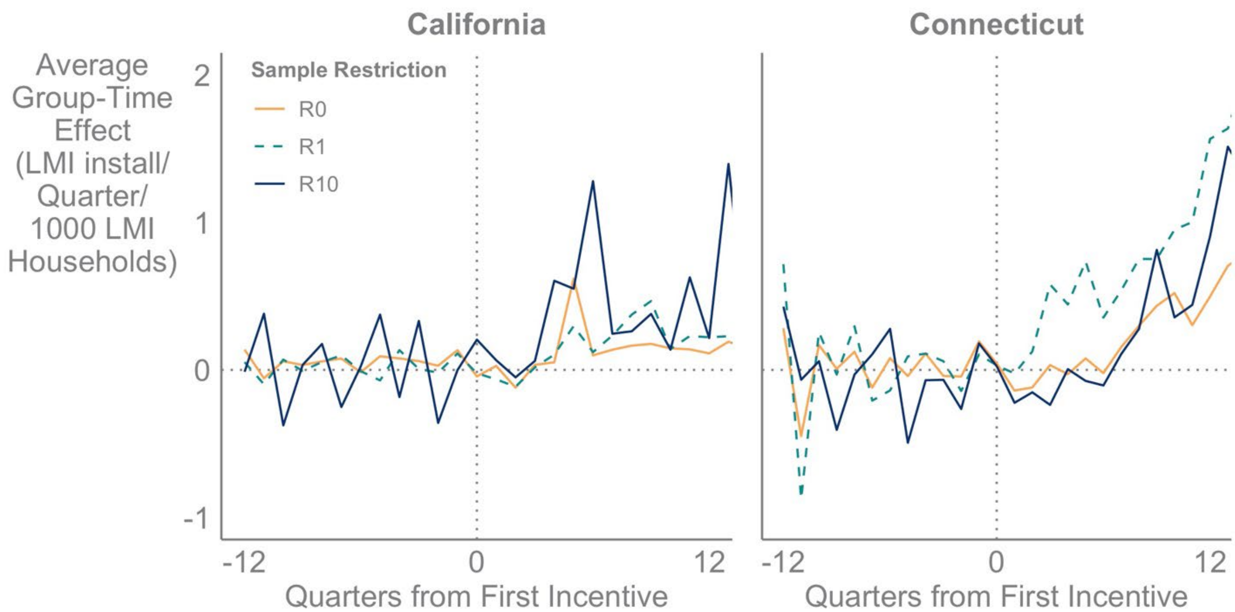

Figure 3. Average group-time effects excluding LMI incentive recipients

5. Conclusions and policy implications

Rooftop PV financial incentives have declined in the United States over time, by policy design. Incentive phase-down is an efficient design, ensuring cost-effective investments in early adoption and preventing inefficient payments to later adopters who would have adopted otherwise. However, the phase-down occurs just as PV prices bring adoption within financial reach for LMI households, which have been historically under-represented in the U.S. rooftop PV market. As a result, incentive phase-down may inequitably exclude LMI households from realizing the same financial benefits from PV adoption as high-income early adopters. A few dozen state and local programs continue to offer means-tested incentives to income-qualifying households, though these programs are small relative to the broader suite of programs that have supported rooftop PV adoption.

Incentive phase-down design is based, in part, on the assumption that falling prices would obviate the role of such incentives. While this assumption may hold for "conventional" PV adopters—namely relatively high-income households—it does not necessarily hold for previously under-served populations such as LMI households. The results indicate that LMI incentives still drive LMI adoption. As a first order approximation, the analysis suggests that around 80% of LMI incentive-supported systems would not have been installed without incentive support. The estimate accords with qualitative insights from the California incentive program. In a 2015 survey of California incentive recipients, only 36% of recipients reported that they would have adopted if they had been required to contribute to the system's cost (Navigant 2015).

Presumably, a smaller share of incentive recipients would have adopted if they have been required to pay the full system cost.

This finding has significant policy implications, particularly given the growing prominence of energy justice on public policy agendas (Carley, Engle et al. 2021). The results suggest that means-tested incentives could be an effective tool for driving PV adoption equity. One alternative to the conventional phase-down design would be to tighten income eligibility restrictions over time. For instance, many existing programs base eligibility on criteria related to area median incomes. Income eligibility can be adjusted downward as increasingly smaller shares of area median income as PV prices decline. Income-based phase-down could have similar efficiency objectives as conventional phase-down design. For instance, the pace of adjustments in income criteria could be designed to prevent ineffective payments and to prevent inefficient delays in adoption (Alizamir, de Véricourt et al. 2016, Ding, Zhou et al. 2020). In this way, tightening income eligibility requirements can help more LMI households benefit from those incentives while also addressing concerns related to excessive free riding.

Further, I find evidence that LMI incentive programs generate spillover impacts by increasing adoption among households that do not receive incentives. Though the effects are statistically insignificant—partly due to low statistical power—the spillover hypothesis is strongly supported by a robust literature on the key role of social influence in solar adoption. The implication for policymakers is that the impacts of LMI incentives on LMI adoption may be under-appreciated. Further, incentive programs could potentially be designed to leverage these spillover effects, such as by targeting incentives in LMI areas rather than setting eligibility based on household income.

This study adds to the body of evidence that LMI incentives and other interventions can increase PV adoption equity. Further research can explore specific program designs to determine which programs are more successful than others, and why. Future research can also evaluate LMI programs using a wider range of metrics, such as cost-effectiveness and impacts beyond adoption, particularly program impacts on LMI household energy burden. Finally, further research is required to determine the extent to which LMI incentives and other LMI PV programs drive spillover adoption. This particular vein of proposed research is critical. Overlooking spillover impacts could result in under-valuation of LMI incentive or other LMI PV programs. Factoring in such spillovers could support broader adoption of LMI PV programs.

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