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Toward income equity in rooftop solar adoption—the impact of policies and business models

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Abstract

High-income households are more likely to adopt rooftop solar photovoltaics (PV) than low- and moderate-income (LMI) households in the United States. Income-skewed adoption persists even as PV becomes more affordable and financially beneficial to LMI households. PV adoption inequity is an emerging energy justice issue, particularly considering that income-skewed deployment could increase LMI household electricity bills under typical rate structures. Further, PV adoption inequity could decelerate rooftop PV deployment. Here we show that policy and business model interventions could increase PV adoption equity. We find that LMI-specific financial incentives, PV leasing, and property-assessed financing increase PV adoption equity. We find that these interventions increase equity in existing markets and drive more installations into previously under-served markets with lower incomes. By shifting deployment patterns, these interventions could also catalyze peer effects and installer marketing efforts that increase LMI adoption among households that do not directly benefit from the interventions.

Keywords

solar; equity; energy justice; income inequality; technology adoption

Introduction

Rooftop PV has, until recently, largely been a status good in the United States. Early PV adopters tended to be high-income households willing to buy innovative green products without expectations for near-term financial returns [1]. As PV prices have declined, PV has become an economical good that yields direct financial benefits [2]. However, high-income households remain more likely to adopt than LMI households [3-5]. In 2018, a household earning more than $200,000 per year was about 4 times more likely to adopt PV than a household earning less than $50,000 (based on data defined in Methods). PV adoption inequity in an era of affordable PV reflects engrained deployment patterns that funnel PV systems into high-income areas. PV deployment has clustered around nodes of early adoption [6] skewed toward high-income areas [5]. Income-skewed clustering is reinforced by local peer effects [7], customer referrals [8], and localized installer marketing [9]. LMI households face various adoption barriers that reduce deployment clustering in LMI areas, such as cash constraints, lower home ownership rates, and language barriers [3, 10-12].

PV adoption inequity is an emerging energy justice issue [12]. Low-cost PV could reduce energy costs for LMI households who spend disproportionately more on energy [13]. However, historical deployment patterns limit LMI access to these benefits [2]. Further, under typical residential electricity rate structures there is a risk that income-inequitable PV adoption *increases* LMI energy bills [12]. Beyond questions of energy justice, PV adoption inequity may reduce or at least decelerate the realization of rooftop PV's clean energy benefits. By one estimate, LMI housing accounts for 42% of PV-viable rooftop space in the United States [14].

Numerous studies have documented trends in PV adoption income equity [2-4, 15]. This literature shows that PV adoption equity has generally increased over time [2, 4], that subsidies do not improve adoption equity [2], and that the emergence of PV financing business models may have improved equity [16-19]. However, while these studies have explored how income affects adoption, the literature has yet to explore what factors explain local differences in PV adopter income equity. In this study, we fill this research gap by exploring how five policy and business model interventions may affect PV adoption equity. We frame our paper around two novel research questions: 1) What interventions are associated with more income-equitable PV deployment? and 2) Do those effects reflect changing income distributions within existing markets or shifting deployment patterns into under-served markets with different income characteristics? In answering these research questions, we make three contributions to the literature. First, ours is the first study to our knowledge to leverage household-level PV adopter income data covering more than 70% of the U.S. residential PV market. Second, we provide novel analyses on several policy interventions, a number of which have received little attention in the literature to date. Third, our analyses yield relatively clear implications that could be used to design policy measures to increase PV adoption equity.

PV policy and business model interventions

We evaluate five PV policy and business model interventions that could affect PV adoption equity: financial incentives; LMI-targeted financial incentives; system leasing; property-assessed clean energy financing; and Solarize campaigns. Our study is not comprehensive in that some potentially relevant interventions—such as green bank loans—are excluded, largely due to data limitations. Further, our analysis is restricted to policies that affect residential rooftop PV systems. Community solar—a model where multiple customers subscribe to the PV output of a single PV array—is outside the scope of our study.

Incentives. Financial incentives could increase adoption equity by reducing adoption costs. Most states and many local jurisdictions have offered up-front rebates or ongoing production-based incentives. These incentives have generally declined over time or, in some cases, expired [20]. Research to date suggests that PV incentives are ineffective at increasing adoption equity [12, 15, 21].

LMI incentives. Several states offer means-tested incentives to households under certain income thresholds, which we will refer to as LMI incentives. LMI incentive programs tend to be relatively small, generally on the order of 1% of all incentives distributed [22]. The effects of LMI incentives on adopter income distributions have not been studied. These potential effects are somewhat predictable and tautological. By definition, LMI incentives accrue to LMI households. Insofar as at least some incentive recipients would not otherwise have adopted, LMI incentives should increase LMI adoption. Nonetheless, an analysis of LMI incentives is still valuable for answering our second research question: whether LMI incentives increase LMI adoption in existing markets or shift deployment patterns into under-served LMI areas.

Leasing. Prospective adopters in most states have the option to lease rather than buy PV (for simplicity, we use the term “leasing” to refer to any purchase where a third party owns the system, including power purchase agreements). Leasing can significantly reduce the up-front adoption costs that impede LMI adoption [16-19]. While leasing is not a policy measure *per se*, it generally must be authorized through rules allowing non-utility companies to sell electricity to retail electricity customers. Further, not all installers offer leasing. Indeed, a relatively small number of high-volume installers account for the majority of leased systems in the United States [23].

Property Assessed Clean Energy Financing (PACE). PACE allows homeowners to finance PV through property tax payments. PACE is relatively accessible to LMI customers given that—compared to other loans—PACE has more lenient qualification criteria, PACE entails little or no up-front costs, and PACE loans can be transferred upon sale of the property. Some studies have shown that PACE increases PV adoption [24-26], though the studies did not test the effects on LMI adoption in particular. Residential PACE is only available in California, Florida, and Missouri at the time of writing, of which only California is a major residential rooftop PV market.

Solarize. A Solarize campaign is a community initiative to recruit a coalition of prospective PV adopters. Solarize campaigners negotiate with one or a few installers to make bulk PV purchases on behalf of campaign participants. Bulk purchasing discounts—possibly on the order of 20-30% [27]—could make Solarize campaigns an effective model to increase adoption equity. Further, Solarize campaigns can overcome informational barriers associated with LMI adoption [27, 28].

Estimating adoption income equity and bias

We define PV adoption equity as the degree to which PV adopter incomes reflect the incomes of the broader local population. To estimate adoption equity, we compare modeled household-level income estimates for PV adopters with county household median incomes based on U.S. Census data (for details see Methods). We restrict the study period to 2010-2018 to identify relatively recent trends in PV adoption equity. The final cleaned data set represents 1,007,459 residential rooftop PV systems installed on single-family homes in 18 states. We used the data to generate five variables representing the interventions (Table 1).

Table 1. Policy and Business Model Intervention Variables

Variable	Description	Mean (SD)
Incentives	Value of all financial incentives ($/W) including up-front rebates and the net present value of ongoing incentives, excluding federal investment tax credits	$0.22/W (0.53)
LMI incentive	Dummy variable for LMI incentive-supported systems	0.01 (0.1)
Leasing	Dummy variable for leased systems	0.42 (0.49)
PACE	Dummy variable for PACE-financed systems	0.03 (0.17)
Solarize	Dummy variable for Solarize systems	0.01 (0.08)

Our primary metric is the difference between adopter incomes and county household median incomes, which we will refer to as *adopter income bias*. For robustness, we present results for income bias defined at the zip code (more granular) and state (less granular) levels in the Supplementary Information. As expected, adopter income bias is disproportionately positive: about 81% of adopters earned more than the county median income. The mean adopter income bias is about $64,000, meaning that PV adopters earned $64,000 more per year, on average, than county median incomes.

The effects of the interventions on adopter income bias

We test the effects of the various factors on adopter income bias through a fixed effects regression in the following form:

$$bias = I\gamma + X\beta + Y + S \tag{1}$$

Where *bias* is PV adopter income bias, I is a vector of the interventions defined in Table 1, X is a vector of control variables comprising market and demographic variables, Y is a year fixed effect, and S is a state fixed effect. We use county-clustered robust standard errors. γ is the coefficient of interest; it represents the mean difference in bias between intervention-supported and other systems while controlling for other factors. These differences provide evidence of effects on adopter income bias under two assumptions. First, adopter usage of different interventions represents revealed preferences for those interventions. For instance, a negative coefficient on the leasing variable shows that lower-income adopters are more prone to leasing than higher-income adopters. The assumption, then, is that lower-income *households* have stronger latent preferences for leasing than higher-income *households*. Second, at least some households that prefer intervention-supported systems would forego adoption in the absence of the interventions (for details, see Methods).

We aim to distinguish the effects of the interventions on adoption equity in existing markets from the effects from shifting deployment into under-served markets with lower incomes. To distinguish these effects, we tested models with and without controls for zip code-level median household incomes. By controlling for local income levels, we isolate local effects on income bias. By removing these controls, we allow the model to capture effects generated due to the differential use of the interventions in areas with different income levels. Further, we identified *conventional* markets as zip codes in the top quartiles of cumulative per-capita adoption and median household income, both with respect to other zip codes in the respective states, and tested the model limited to data for systems installed in those markets.

Table 2 presents the model results of the three model variations. Negative coefficients represent variables associated with less adopter income bias, that is, higher usage among adopters closer to county median incomes. The model suggests that LMI incentives, leasing, and PACE financing are associated with less adopter income bias in each variation. In contrast, incentives are associated with higher adopter income bias and the results for Solarize are not robust. The lacking or potentially perverse effect of incentives on adoption equity is consistent with previous results [2]. While we return to incentives and Solarize in the Conclusions, we focus the remainder of our analysis on LMI incentives, leasing, and PACE.

Table 2. Regression Results (Y=adopter income bias x$1,000)

	(1) No local income control	(2) Local income control	(3) Conventional markets only
Incentives	6.14*	5.44*	3.64
	(1.04)	(1.23)	(2.66)
LMI incentives	-63.93*	-43.64*	-40.62*
	(4.649)	(3.41)	(8.86)
Leasing	-14.75*	-11.27*	-11.15*
	(1.124)	(1.05)	(1.47)
PACE	-14.79*	-8.67*	-8.77*
	(2.406)	(1.43)	(1.25)
Solarize	7.36*	1.43	-4.16
	(2.92)	(1.72)	(2.83)
Price	-1.083	0.91	0.07
	(1.47)	(1.55)	(2.99)
Electricity rate	101.0*	155.4*	270.6*
	(24.03)	(46.6)	(77.78)
Market density	-0.07	-0.242	0.11
	(0.20)	(0.30)	(0.45)
Installer density	-4.51*	-0.008	-14.65
	(1.76)	(2.80)	(10.34)
Local income (zip median)		0.86*	0.75*
(x1000)		(0.04)	(0.07)
Income inequality (GINI)	194.5*	295.7*	180.9*
	(42.12)	(56.29)	(84.91)
Median home-ownership costs	12.02*	-12.58*	-15.07*
	(2.56)	(3.34)	(5.99)
% moved pre 1990	-0.79*	-1.02*	-1.09
	(0.26)	(0.40)	(0.70)
Year fixed effects	x	x	x
State fixed effects	x	x	x
R^2	0.06	0.15	0.11
N	1,007,459	1,007,459	193,229

* $p<0.05$

The models provide evidence that LMI incentives, leasing, and PACE increase adoption equity in existing markets. Model (2) suggests that the interventions increase adoption equity while controlling for local income. Model (3) suggests that the three interventions increase adoption equity in relatively high-income conventional markets. At the same time, excluding the control for local income in Model (1) augments the estimated effects of all three interventions. The augmented results suggest that at least some of the effects derive from shifting deployment patterns.

Shifting deployment patterns

The shifting deployment hypothesis would be supported by evidence that adopters in under-served markets use the interventions more frequently than in conventional markets. To explore whether the data satisfy this condition, we identified *under-served* markets as zip codes in the bottom quartiles of cumulative per-capita adoption and median household income, each with respect to other zip codes in the respective states. The condition is sufficient for deployment shifting under two assumptions: 1) at least some intervention-supported systems would not have been installed without the interventions; and 2) the share of intervention-supported systems in under-served markets that would have been installed regardless is equal to or less than the share in conventional markets (for details, see Methods).

Adopters in under-served markets use all three interventions more frequently than adopters in conventional markets (LMI incentives: t=27.0; leasing: t=35.4; PACE: t=9.2) (Figure 1). These results are robust to multiple variations on how we define conventional and under-served markets (see Supplementary Information). To illustrate how these results translate to shifting deployment patterns, we calculated the predicted number of systems that would have been installed in each market if intervention-supported systems followed the same deployment patterns as other systems. To emphasize how these shifts affect LMI adoption in particular, we limit the analysis to adopters that earned less than county median incomes. The numbers of intervention-supported systems consistently exceed predictions in under-served markets and fall below predictions in conventional markets (Figure 2). In the case of LMI incentives, the shift is strong enough to fully offset historic deployment patterns, such that more LMI incentives flowed to under-served than to conventional markets. In the case of PACE, this shift nearly equalizes deployment among customers earning less than county median incomes between under-served and conventional markets.

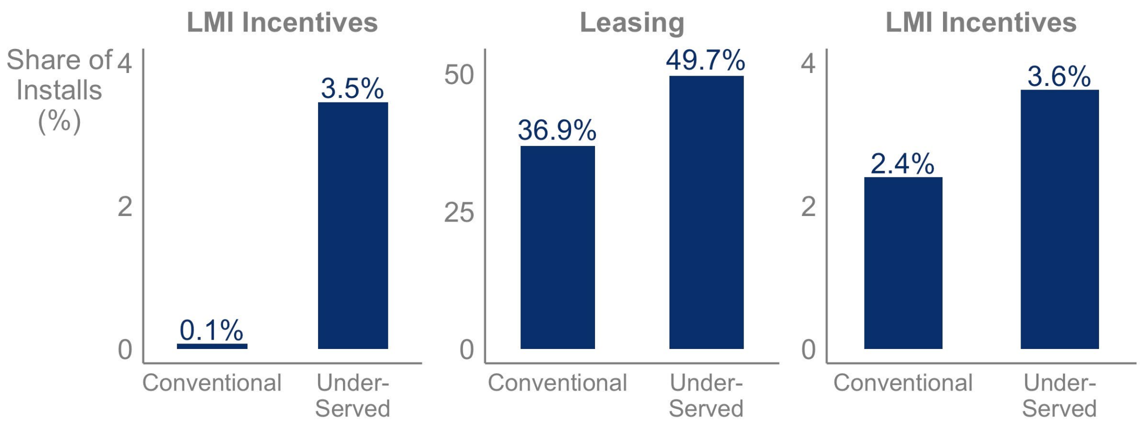

Figure 1. Shares of installs using interventions in conventional and under-served markets

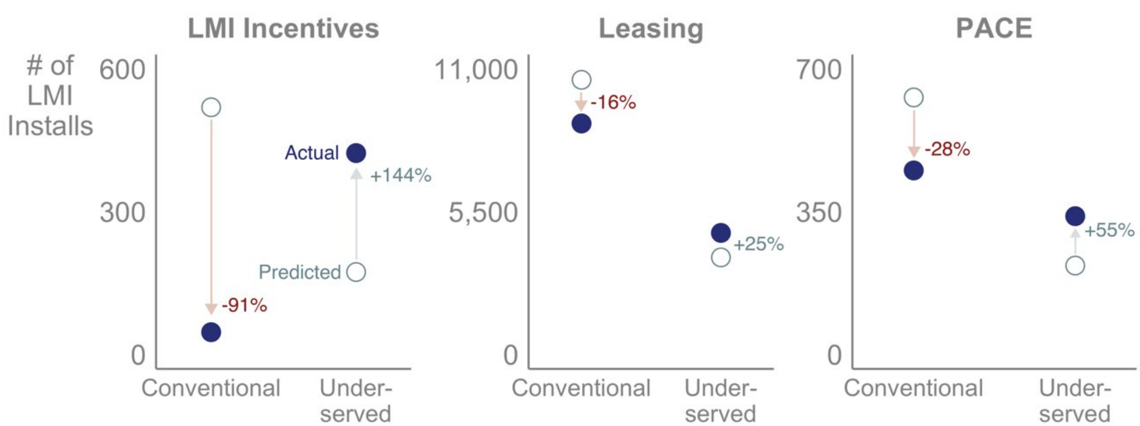

Figure 2. Predicted and actual number installs supported by interventions in conventional and under-served markets. Limited to customers earning less than county median income. For details, see Methods.

Spillover impacts

By shifting deployment patterns into areas with lower incomes, the interventions could catalyze peer effects and installer marketing to drive PV clustering in previously under-served markets. These effects could have spillover impacts by increasing LMI adoption even among households that do not directly benefit from the interventions. The spillover hypothesis is an area for further research. Here, we provide a brief exploratory analysis of potential spillover impacts from LMI incentives in Connecticut.

Connecticut began offering LMI incentives in 2015, though the data suggest that the program began in earnest in 2016. These incentives have accrued disproportionately to LMI areas, especially to under-served urban areas. The data suggest that *non*-recipient systems exhibit similar deployment shifts as recipients. The share of non-recipient systems installed in under-served markets increased from 3.6% before 2016 to 6.6% from 2016 to 2018 (t=8.5). About 66% of non-recipient *systems* in under-served markets were installed within 1 kilometer of an LMI incentive recipient *household* from 2016 to 2018, compared to 57% of non-recipient systems before 2016 (t=3.6). Further, adopter income bias declined more rapidly among non-recipients in under-served markets than in conventional markets after the incentive program began (Figure 3). It is difficult to isolate the role of LMI incentives in these results, and other factors could explain at least some of these trends, such as exogenous shifts in installer strategies or city-level programs to increase LMI adoption. Nonetheless, these trends accord with the spillover hypothesis. Spillover impacts—and how they could be leveraged—may be a rich area of future research.

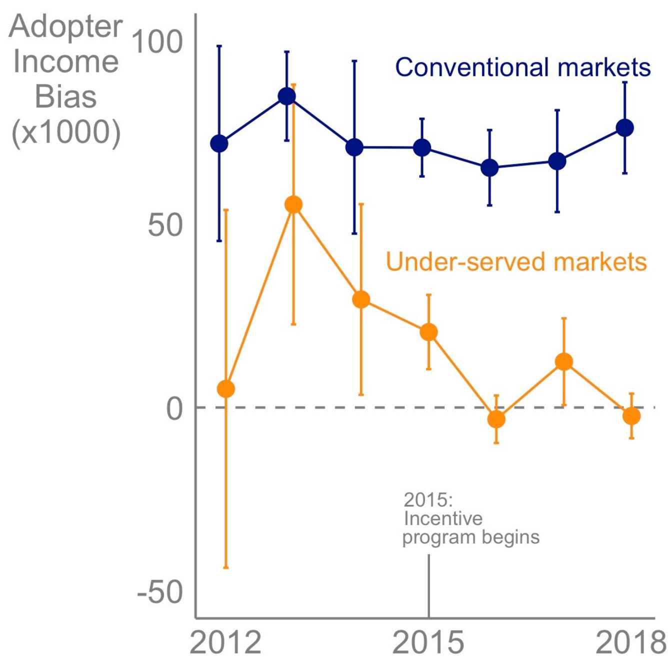

Figure 3. Mean adopter income bias among non-recipients in under-served and conventional markets in Connecticut, with 95% confidence intervals.

Market and demographic factors

The data also suggest several relationships between PV adoption equity and market and demographic factors (Table 3). We close our discussion by highlighting one potentially insightful result among the control variables. The model suggests that adopter income bias is higher in areas with greater income inequality. This result is partly mathematical: in areas with high income inequality, the distance between median and high-income households is greater, creating more space for adopter income bias. However, we posit a second explanation. Areas with high income inequality also tend to have high levels of income segregation, i.e., geographic clustering of households by income levels [29]. Income segregation could increase adoption inequity by bolstering the forces that drive PV deployment clustering in high-income areas. For instance, peer effects may have shorter range in wealthy gated communities than in open neighborhoods with mixed income levels. We propose the effects of income segregation on PV deployment clustering as an additional area for future research.

Table 3. Statistically Significant Market and Demographic Variable Results
Based on model with local income control

Variable	Coefficient Sign	Interpretation
Electricity rate	+	Adopter income bias is higher in areas where adopters pay higher electricity rates. Electricity rates could have two divergent effects on LMI adoption: 1) Higher electricity rates correlate with higher living costs that impede LMI adoption; 2) Higher rates provide stronger incentives for adoption. This result suggests that the first effect dominates the second in the data. Another possibility is that LMI households in areas with high electricity rates are more likely to pay subsidized rates, which would reduce their incentives to adopt.
Median income	+	Adopter income bias is higher in high-income zip codes than in low-income zip codes. This result is largely mathematical: adopters in high-income zip codes are more likely to earn more than the county median income than adopters in low-income zip codes, all else equal.
Income inequality	+	Adopter income bias is higher in areas with greater income inequality. High income inequality creates conditions for greater adopter income bias. High income inequality also correlates with high income segregation, which may exacerbate adopter income bias in areas where LMI households are physically segregated from high-income households.
Home-ownership costs	-	Adopter income bias is lower in areas with higher home-ownership costs. This result largely reflects the correlation between home-ownership costs and median incomes—when excluding the income variable, the sign on this effect flips positive, suggesting that high home ownership costs hinder LMI adoption. Holding income constant, a

		hypothesis for this negative coefficient is that LMI households in areas with higher home-ownership costs are more accustomed to making large capital investments in their homes.
% moved before 1990	-	Adopter income bias is lower in areas with more long-term resident occupants. This result suggests that LMI adoption is higher in areas where LMI households are less likely to move and have to sell their home.

Conclusions

High-income households are more likely to adopt rooftop PV than LMI households in the United States. Persistent PV adoption inequity in an era of affordable PV reflects engrained patterns of deployment that funnel PV systems into high-income areas. PV adoption inequity is an emerging energy justice issue. Further, the under-utilization of LMI rooftop space could significantly reduce or at least decelerate the realization of the benefits of rooftop PV as a clean energy resource.

We find evidence that LMI incentives, leasing, and PACE financing increase PV adoption equity. The results suggest that these interventions increase adoption equity in existing markets and shift deployment into under-served markets with lower income levels. By shifting deployment patterns, these interventions could potentially catalyze local peer effects and increase LMI adoption in under-served areas even among households that do not directly benefit from the interventions. In contrast, our results corroborate previous findings that incentives do not improve and indeed may exacerbate PV adoption inequity. Further, our results suggest that Solarize campaigns have, at least historically, had little impact on PV adoption equity. However, particularly in the case of Solarize, program design changes could yield adoption income equity benefits. For instance, Solarize campaigns could set minimum targets for LMI participation.

Our findings have broad policy implications, but we posit that these implications can be synthesized as follows. Our results suggest that a variety of policy measures could disrupt traditional PV deployment patterns and increase PV adoption equity. Specifically, absent policy intervention, historical patterns of PV deployment will likely—to a degree, at least—continue to funnel PV systems into high-income areas. Policies that explicitly remove barriers to LMI adoption can break those historical patterns and shift PV deployment into under-served areas. By shifting deployment, these policies may also catalyze peer effects and installer marketing to generate self-sustaining increases in adoption in previously under-served areas.

Methods

Several studies have explored trends in PV adoption income equity [2, 3, 5, 15]. Our approach builds on the existing literature in three ways. First, similar to the references in [5, 15], our data comprise a relatively broad geographic area rather than focusing on a single state such as California. Second, we use modeled household-level income estimates rather than area-level income estimates used in the existing literature. Third, our methods go beyond describing PV adopter income distributions to evaluate factors that explain differences in income distributions across geographies.

Our primary source for PV adopter income data is the U.S. Lawrence Berkeley National Laboratory's *Tracking the Sun* (TTS) data set. TTS compiles system-level data collected through various state- and utility-level PV interconnection and incentive programs. The full TTS data set includes data for more than 70% of all residential PV systems installed in the United States [20]. Modeled PV adopter income data were obtained from household-level annual income estimates provided by Experian. Experian uses a proprietary algorithm to estimate household-level income. We validated the Experian data by establishing that the algorithm consistently modeled lower household incomes for adopters that received LMI incentives (see Supplementary Information). We derive general population income data from various 5-year U.S. Census American Community Survey data sets: Income in the Past 12 Months (ACS S1901); Gini Index of Income Inequality (ACS B19083); and Comparative Housing Characteristics (ACS CP04).

Of the five interventions, the incentive, Solarize, and leasing variables were based on values provided in TTS. The LMI incentive variable was generated for the three states in the data that offered LMI incentives during the study period as follows:

- *California*: LMI incentive data are published under the state's Single-family Affordable Solar Housing program. According to those data, a single installer (Grid Alternatives) was responsible for all systems installed under the program. The number of records associated with Grid Alternatives in TTS during the study period (N=8,384) closely aligns with the number of records reported by the program over the same timeframe

(N=8,573). We therefore assume that all records associated with Grid Alternatives received an LMI incentive.

- *Connecticut*: Over the study period, a single installer (Posigen) was eligible to install systems under the state's Solar For All program. All systems installed by Posigen were assumed to have received an LMI incentive.
- *New York*: LMI incentive data are published by the New York State Energy Research and Development Authority. These records were matched to TTS using unique program identifiers.

We identified records potentially associated with PACE financing by matching data from the California Alternative Energy and Advanced Transportation Financing Authority with home addresses available from Zillow.

To empirically estimate adoption equity, we calculated an adoption income bias metric equal to the difference between adopter incomes and county median incomes. Let ρ denote the magnitude of some intervention and $\mathbb{B}$ denote adopter income bias. Our goal is to establish a sign for $\mathbb{B}'(\rho)$, where $\mathbb{B}'(\rho) < 0$ would provide evidence that an intervention reduces income bias. Ideally, we would identify this effect through some type of differencing model. However, there are no sharp intervention discontinuities to allow for such an approach. As a second-best approach, we rely on the relative observed use of the interventions by adopters at different income levels.

To motivate this approach, consider that the use of any intervention is discretionary: the adopting household either chooses to use the intervention or does not—though this decision may be assisted by installers and constrained by policy terms (e.g., income criteria for LMI incentives). We will show that the relationship between adopter income bias and the observed use of the interventions provides evidence of the latent effects of the interventions on adopter income bias under two assumptions:

- *Revealed preferences*: An adopter's use of an intervention ρ accurately reveals an underlying preference of that household for the characteristics of adoption with ρ rather than without.
- *Additionality*: At least some households (not necessarily all) have sufficiently strong preferences for ρ such that these households would forego adoption in the absence of ρ.

Let $\tilde{\rho}$ denote latent unobserved preferences and $\hat{\rho}$ denote the observed usage of interventions. Under the assumption of revealed preferences, an observed relationship between intervention usage and household income implies that both unobserved and observed preferences are functions of income, denoted inc. Under the assumption of additionality, the expansion or retraction of an intervention ρ would affect adoption equity by increasing or decreasing adoption in income brackets that prefer ρ. As a result, under these assumptions, an intervention ρ reduces adopter income bias if lower-income households *use* ρ more frequently than higher-income households:

$$\frac{\partial \hat{\rho}}{\partial inc} < 0 \ \rightarrow \ \frac{\partial \tilde{\rho}}{\partial inc} < 0 \ \rightarrow \frac{\partial \mathbb{B}}{\partial \rho} < 0 \tag{2}$$

Further, under the assumption of revealed preferences, it follows that an adopter's use or non-use of an intervention is at least one determinant of the adopter's income bias, that is $\hat{\rho}'(inc) \propto \mathbb{B}'(\hat{\rho})$. From Condition (2), it follows that:

$$\frac{\partial \mathbb{B}}{\partial \hat{\rho}} \propto \frac{\partial \mathbb{B}}{\partial \rho} \tag{3}$$

In words, under the assumptions of revealed preferences and additionality, the observed effects of intervention *use* on adopter income bias provide evidence of the latent effects of the intervention on adopter income bias. We leverage this fact to develop our regression model:

$$\mathbb{B} = \hat{\rho}\gamma + X\beta + Y + S \tag{4}$$

Where $\gamma = \mathbb{B}^{'}(\hat{\rho})$. Table 4 defines the control variables in X and basic descriptive statistics. We use year fixed effects (Y) to control for declining adopter income bias over time and state fixed effects (S) to control for unobserved geographic differences. We use county-clustered robust standard errors.

Table 4. Control Variable Definitions and Descriptive Statistics

Variable	Description [Source]	Mean (SD)
Market variables		
Installed price	County-year level mean installed system price ($/W), dropping outlier systems <$1/W or >$25/W [TTS]	$4.81/W (0.98)
Electricity rate	Average residential retail electricity rate ($/kWh) in the customer's county [OpenEI Utility Rate Database]	$0.17/kWh (0.03)
Market density	# of systems installed per 1,000 households in county-year [TTS/Census Data Set S1901]	10.14 (7.07)
Installer density	# of installers that installed at least 1 system in the county-year per 1,000 households [TTS/Census Data Set S1901]	0.46 (0.35)
Demographic variables		
Median income	Zip code-year level median household annual income (x1,000) [Census Data Set S1901]	$75.9 (27.5)
Income inequality (GINI)	The GINI index is a measure of income inequality ranging from 0 (perfect equality) to 1 (perfect inequality) [Census Data Set B19083]	0.46 (0.02)
Home-ownership costs	County-year level median monthly home-ownership costs (x1,000) [Census Data Set CP04]	$2.2 (0.52)
% moved pre 1990	County-year level % of housing where the owner moved in before 1990 [Census Data Set CP04]	12.9% (3.9)

Our second research question is the extent to which the interventions shift deployment patterns into under-served markets with lower income characteristics. Comparable to our approach for the regression, given the lack of sharp intervention discontinuities, as a second-best approach we explore the deployment shifting hypothesis by testing for differences in the relative use of the intervention in conventional and under-served markets. We defined conventional markets as zip codes in the top quartiles of PV adoption per capita (based on number of installations in TTS and Census population estimates) and zip code median incomes and under-served markets as zip codes in the bottom quartiles of both metrics. The deployment shifting hypothesis implies that the interventions increase the market share of under-served markets relative to conventional markets. Formally, an intervention ρ shifts deployment into under-served markets if:

$$\frac{\partial \frac{N_{us}}{N_c}}{\partial \rho} > 0 \tag{5}$$

Where N_{us} and N_c are the numbers of systems installed in under-served and conventional markets, respectively. We will show that the deployment shifting effect in Condition (5) is satisfied if adopters in under-served markets use the interventions more frequently than adopters in conventional markets under the following assumption:

- *Stable additionality*: At least some intervention-supported systems would not otherwise have been installed and the share of intervention-supported systems that would have been installed regardless does not vary across markets.

To formalize this proposition, let N represent the total number of systems installed without intervention support—including intervention-supported systems that would have been installed regardless. Let $\eta(\rho)$ denote the total number of additional systems installed due to the intervention. Let δ_{us} and δ_c represent the shares of unsupported systems installed in under-served and conventional markets respectively, and let θ_{us} and θ_c denote the shares of intervention-supported systems in under-served and conventional markets, such that the total number of systems installed in under-served markets is $N_{us} = \delta_{us}N + \theta_{us}\eta(\rho)$ and likewise for conventional markets $N_c = \delta_c N + \theta_c \eta(\rho)$. Under this framework, Condition (5) is satisfied if:

$$\frac{\theta_{us}}{\delta_{us}} > \frac{\theta_c}{\delta_c} \tag{6}$$

In words, condition (6) says that the intervention shifts deployment into under-served markets if the ratio of intervention-supported to unsupported systems is greater in under-served than in conventional markets. This condition can be further simplified to:

$$\frac{\eta_{us}}{N_{us}} > \frac{\eta_c}{N_c} \tag{7}$$

Where η_{us} and η_c are the numbers of intervention-supported systems in under-served and conventional markets, respectively. Condition (7) states that deployment shifting occurs if intervention-supported systems compose greater shares of systems installed in under-served relative to conventional markets, given the assumption of stable additionality. Estimates for all four variables in condition (7) are available from the data, allowing us to show that the data satisfy that condition, as depicted in Figure 1. We do not observe the number of systems that would have been installed absent the interventions. We therefore cannot directly test whether the data satisfy the assumption of stable additionality, though we have no strong priors that the assumption should not hold. If anything, it seems that the share of non-additional systems would be higher in conventional markets where higher-income customers rely less on the interventions. If so, η_c is *over*-estimated in the data relative to η_{us}. Hence, if the assumption of stable additionality is violated, it is likely violated in a way that the measured deployment shifts are under- rather than over-estimated.

We leverage the condition in Equation (7) to illustrate deployment shifting in Figure 2. We calculated predicted LMI deployment rates in under-served and conventional markets assuming that intervention-supported systems follow the same deployment patterns as unsupported systems, i.e., $\theta = \delta$:

$$\tilde{\eta}_{us} = \hat{\delta}_{us}\hat{\eta} \quad , \quad \tilde{\eta}_c = \hat{\delta}_c\hat{\eta} \tag{8}$$

Where $\tilde{\eta}_{us}$ and $\tilde{\eta}_c$ are the predicted number of intervention-supported systems in under-served and conventional markets, respectively, and $\hat{\delta}_{us}$, $\hat{\delta}_c$, $\hat{\eta}$ are estimated from the data. The data support the deployment shifting hypothesis by showing that $\hat{\eta}_{us} > \tilde{\eta}_{us}$ and $\hat{\eta}_c < \tilde{\eta}_c$ where $\hat{\eta}_{us}$ and $\hat{\eta}_c$ are the *observed* number of intervention-supported systems in under-served and conventional markets, respectively, as illustrated in Figure 1.

We note three data limitations. First, some TTS-generated variables differ in their geographic coverage based on how the various incentive programs report data. Specifically, the methods used to generate the PACE and Solarize dummy variables may not have yielded a complete identification of all systems associated with these programs, and the degree to which the methods comprehensively identified such systems may vary across the states. Second, the PV adopter incomes are modeled rather than observed. A data validation analysis suggests that the modeled estimates accurately capture household-level differences in income (see Supplementary Information). Details about the underlying structure of Experian's income model are proprietary and were not shared with the authors. However, we assume that the model uses local area incomes—among other inputs—to predict household incomes. Assuming the model accounts for area incomes, the model may tend to over-estimate the incomes of relatively low-income households in high-income areas and under-estimate the incomes of relatively high-income households in low-income areas.

The data validation indicates that the role of area incomes—if any—is small relative to household-level predictors. Nonetheless, as a precaution, we avoid relying on metrics that could be sensitive to this modeling bias. Specifically, it would be helpful to compare per capita LMI adoption rates in under-served and conventional markets. However, given the potential modeling bias in high-income areas, per capita LMI adoption rates could be under-estimated in conventional markets. For this reason, we based our analysis of deployment shifting on market shares estimated within the TTS data rather than income-based per capita adoption rates. A third limitation is that the modeled incomes are bounded from above at $250,000/year. As a result, the modeled PV incomes may under-estimate the true degree of bias toward higher incomes among PV adopters.

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