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The role of peer influence in rooftop solar adoption inequity in the United States

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ABSTRACT

Individual demand for emerging technologies can be influenced by the demand of other individuals within defined peer groups. These so-called peer effects have been demonstrated in emerging clean energy technologies such as rooftop solar. To date, peer effects have disproportionately driven solar adoption among relatively affluent households. Here, we use household-level income estimates of rooftop solar adopters to explore how peer effects drive adoption for low-income households. We find evidence of peer effects for both high- and low-income households and find that peer effects are generally stronger within than across income groups. Our results indicate that peer effects translate to adoption less frequently among low-income households. These results suggest that low-income peer effects are mitigated by barriers to low-income adoption. Heterogeneous peer influence is another demand shifter that explains the inequitable adoption of emerging technologies.

1. Introduction

Small-scale consumer technologies such as rooftop solar photovoltaics (PV) could play key roles in electric grid decarbonization and climate change mitigation (Dietz et al., 2009; O'Shaughnessy et al., 2022b). Rooftop PV deployment depends on the idiosyncratic adoption decisions of millions of individual households. Understanding the factors that shape rooftop PV demand and adoption has thus driven a growing body of research (Sintov and Schultz, 2015; Alipour et al., 2020; Schulte et al., 2022). Most of this work applies a rational actor model, modeling PV demand as a function of various incentives that drive adoption decisions. Another prominent adoption model is based on interpersonal influence within peer groups, or simply peer influence (Axsen and Kurani, 2012; Xiong et al., 2016; Wolske et al., 2020). Peer influence plays a prominent role in models of how technologies diffuse into society (Rogers, 2003; Van den Bulte and Stremersch, 2004). The literature has identified numerous potential mechanisms through which peers can influence technology diffusion, such as through sharing experience (i.e., learning) (Foster and Rosenzweig, 1995), reducing the uncertainty associated with new products (Van den Bulte and Stremersch, 2004), word-of-mouth communication, persuasion (Wolske et al., 2020), and visible adoption actions (e.g., PV systems installed on street-facing rooftops) (Bollinger et al., 2022). In practice, peer influence is identified through peer effect models estimating the impacts of peer demand on individual demand (Pratkanis, 2007; Graf-Vlachy et al., 2018). Several studies find evidence of peer effects in early rooftop PV adoption (Bollinger and Gillingham, 2012; Graziano and Gillingham, 2015; Moezzi et al., 2017; Palm, 2017; Mundaca and Samahita, 2020; Balta-Ozkan et al., 2021; Bollinger et al., 2022).

More recently, an emerging body of research explores the factors that explain heterogeneous rooftop PV adoption across income levels (Sunter et al., 2019; O'Shaughnessy et al., 2021). As is common for emerging technologies, low- and moderate-income (LMI) customers adopt rooftop PV less frequently than more affluent customers (Attanasio and Pistaferri, 2016; Forrester et al., 2022). Inequitable PV adoption could pose challenges to long-term deployment (Welton and Eisen, 2019), and policymakers are increasingly exploring ways to drive LMI adoption (Carley et al., 2021). LMI PV adoption research has largely focused on socioeconomic barriers that prevent LMI households from adopting clean energy technologies (Mueller and Ronen, 2015; Lukanov and Krieger, 2019; Brown et al., 2020). Some previous work posits a potential role for peer influence in LMI adoption (Wolske, 2020; Wolske et al., 2020), and potential differences in peer influence according to area income levels (Bollinger and Gillingham, 2012). No study, to our knowledge, quantifies peer influence on LMI adoption based on household-level income estimates.

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In this paper, we fill this gap by exploring the role of peer influence in rooftop PV adoption for low-income households. Filling this gap is important for three reasons. First, of economic interest, peer influence represents an under-studied factor shaping customer demand. Insofar as peer influence varies across income levels, heterogeneous influence could provide an additional factor to explain consumption inequality. Second, peer influence research based on early adoption may mischaracterize the long-term role of influence during mass diffusion, given that early adopters tend to be more affluent than later adopters (Van den Bulte and Stremersch, 2004; Attanasio and Pistaferri, 2016), and that social influence tends to be stronger among groups with shared characteristics (Marsden and Friedkin, 1993; Wolske et al., 2020). Third, heterogeneous peer effects could have implications for LMI PV policy. Specifically, peer effects have been used to argue for seeding policies: interventions to support early adoptions to foment peer-influenced deployment (Zhang et al., 2016). Seeding deployment among LMI households may be one approach for promoting LMI adoption of emerging clean energy technologies (Sunter et al., 2019; Carley and Konisky, 2020). However, the potential efficacy of LMI seeding may be over- or under-estimated based on peer effects for mostly affluent early adopters. An accurate understanding of LMI peer influence requires confirming results from prior research hold across income levels or differentiating peer effects across income levels.

2. Theoretical background

The peer concept can be applied to any group with shared social bonds. Clean energy peer effects research most frequently defines peer groups in spatial terms by analyzing adoption within defined geographic areas (Wolske et al., 2020). Our model will similarly define peers as households living in the same Census tracts, as discussed further in Section 3.

In economic terms, the theory of peer influence asserts that individual demand curves are a function of the demand of other individuals within a defined peer group, that is:

$$Q_{j,g} = D\left(p, Q_{\neq j,g}, X\right) \tag{1}$$

Where $Q_{j,g}$ is the demand of individual j in peer group g, D is a demand function, p is the price of a good, $Q_{\neq j,g}$ is the demand of other individuals in the peer group, and X is a set of other relevant demand shifters. The estimated impacts of $Q_{\neq j,g}$ on $Q_{j,g}$ is known as a peer effect.

Bollinger and Gillingham (2012) (B&G) develop an approach to consistently estimate rooftop PV peer effects while addressing common identification challenges such as self-selection into peer groups. The approach leverages the fact that PV system installations occur days, weeks, or months after an adoption decision. That is, an installation at time t represents an adoption that occurred at time $t-l$, where l is the lag between adoption and installation. Under the assumption that rooftop PV peer influence occurs for installed systems, B&G model adoption decisions as a function of cumulative installations, also known as the installed base:

$$a_{gt} = \alpha + \beta b_{gt} + X\gamma_{gt} + \varepsilon_{gt} \tag{2}$$

Where a_{gt} is adoption in group g at time t, β is the coefficient of interest estimating peer effects, b_{gt} is the installed base, and X is a vector of relevant price and demand shifters. Importantly, the installed base is itself effectively a lagged version of adoption:

$$b_{gt} = \sum_{t=0}^{t} \sum_{j=1}^{J} a_{jt-l} \tag{3}$$

Where J is the total number of potential adopters in group g. Given the structure of b_{gt}, serial autocorrelation in the error term could bias the estimator β. To see why, suppose that the error term is autocorrelated with an order $v \geq l$, so that a_t is a function of ε_{t-l}. Given that $b_t = \sum_0^t \sum_0^J a_{jt-l}$, b_t is correlated with ε_{t-l}, such that $E\left[b_{gt}, \varepsilon_{gt}\right] \neq 0 \forall v \geq l$. As a result, an identifying condition in this model is that the lag l exceeds the order of autocorrelation. Fortunately, lag times between PV adoption and installation are substantial, taking around 39 days at the median according to data described further below. B&G find that PV installation lags easily satisfy this condition in their sample, as we likewise demonstrate in our sample.

Note that the theoretical model does not identify a specific influence mechanism. For instance, rooftop PV peer effects can occur because adopters actively share information about PV with peers or because non-adopters passively observe their peers install PV (Xiong et al., 2016; Bollinger et al., 2022). Similarly, while we discuss potential mechanisms, our empirical model is not based on a specific influence mechanism.

3. Data & methods

Our primary data source is a set of United States rooftop PV installation records compiled by BuildZoom, an online platform connecting households with service contractors. To analyze peer effects across income levels, we use customer addresses to match the PV records to modeled household-level income estimates generated by Experian. We define peer groups at the U.S. Census tract level, that is, we measure how installations within Census tracts affect adoption decisions in the same tracts. We use data from the U.S. Census American Community Survey for Census tract population estimates. We eliminate tracts with daily adoption rates of >10% as extreme outliers or possible data errors. Our analysis data set comprises 801,534 records for systems installed from 2010 to 2020 with valid observations for permit application and issuance dates in 20,624 tracts (see Fig. 1) that could be matched to household-level income estimates,[1] or N = 82,867,232 tract-day observations. Table 1 provides summary statistics for adoption rates and installations in the tract-level panel data.

The BuildZoom data include dates for when each record applied for and received applicable local permits. The B&G model and similar approaches use these or similar dates (e.g., incentive reservation dates) to proxy adoption decision and installation dates. Recent research suggests that these proxies misrepresent adoption timelines (O'Shaughnessy et al., 2020; O'Shaughnessy et al., 2022a). We use data compiled by the National Renewable Energy Laboratory (NREL) (NREL, 2023) to extrapolate more accurate PV adoption and installation dates from the permit application and issuance dates, respectively, from the BuildZoom data sample. We implement two approaches to extrapolate more accurate adoption and installation dates.

3.1. Discrete dates

Our first approach uses discrete daily adoption and installation dates, largely building on the B&G specification. We use median timelines from the NREL data to identify imputed dates for adoption decisions and installations based on permit application and issuance dates, respectively, from the BuildZoom data. Under this approach, an adoption occurs in tract g on day d if a household applied for a PV permit in tract g on date $d+16$, and an installation occurs if a PV permit was issued on date $d-23$. We define the adoption rate a_{gd} as the percentage of households adopting as a share of households in tract g that had not yet adopted as of date d, and we define the installed base b_{gd} as the cumulative number of installations in tract g as of date d.

Following B&G, we implement another discrete date approach based on the first difference of the installed base, which equates to the daily

[1] Income estimates were available for about 98% of all BuildZoom records with valid addresses, permit application, and permit issuance dates that met the defined tract criteria.

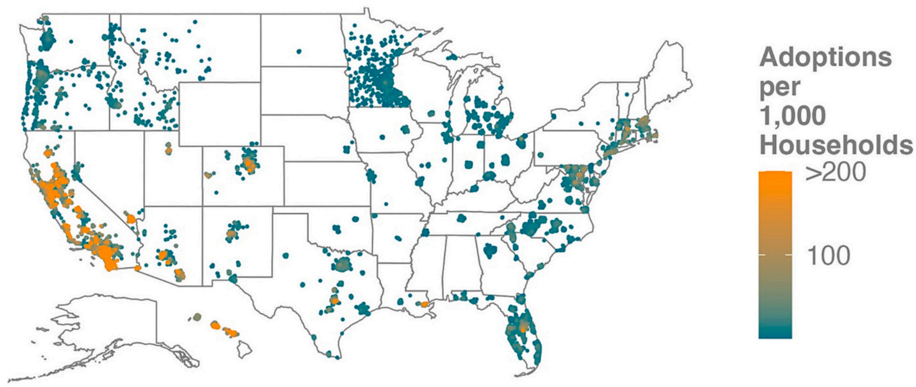

Fig. 1. Census tracts in study sample with cumulative rooftop PV adoption rates (per 1000 households).

Table 1
Summary statistics for tract-level panel data (N = 82,867,232).

Variable	Mean	SD.	Min	Max
Adoption rate (per household in 10^{-6})	5.92	83.99	0	83,333.3
LMI adoption rate (10^{-6})	1.78	43.97	0	82,987.6
Non-LMI adoption rate (10^{-6})	4.14	68.21	0	68,376.1
Installs	0.01	0.13	0	113
LMI installs	0.003	0.06	0	112
Non-LMI installs	0.007	0.10	0	72

All units are at the tract-day level.

installed rate. However, one issue with differencing daily adoption rates is that most days have no adoptions, such that differencing adoption rates yields many negative values following adoptions. As a result, models with first-differenced daily adoption rates are sensitive to the assumptions used to impute adoption and installation dates. Instead, we model daily adoption rates as a function of daily installations (see Section 3.3). For simplicity we refer to models using the first-differenced installed base (daily installs) as discrete date *deltas* models to distinguish these from the discrete date installed *base* approach and from a pure first-differenced model.

3.2. Continuous probabilities

As an alternative to the discrete dates approach we build continuous probability distributions for the expected number of daily adoptions and installations. The benefit of the continuous probability approach is that it captures the impacts of installations on adoption decisions over the course of the ensuing days and weeks.

The NREL data suggest that contract signature to permit application (contract-apply) timelines are roughly described by a left-skewed beta distribution, and that permit issuance to installation (issue-install) timelines are described by a right-skewed beta distribution. The beta distributions are highly skewed. To reduce the impacts of extreme durations, we cap each distribution based on the 95th percentiles of contract-apply and issue-install durations, 79 and 129 days respectively. With this restriction, NREL provided beta distribution parameters for both durations, using observations for 276,215 contract-apply durations and 297,630 issue-install durations. The contract-apply durations are described by the beta distribution $\alpha = 0.94, \beta = 2.76$, and the issue-install durations are described by the distribution $\alpha = 0.67, \beta = 2.14$. We use these distributions to calculate the expected number of adoption decisions and installations for any given day at the Census tract level, using observed dates for permit applications and issuance from BuildZoom. We then sum all probabilities at the tract level to derive expected values for the number of adoptions and installations in a tract on any given day:

$$\widehat{a}_{gd} = \sum_{d=1}^{D}\sum_{h=1}^{H} p(a)_{hd}, \widehat{\Delta b}_{gd} = \sum_{d=1}^{D}\sum_{h=1}^{H} p(\Delta b)_{hd} \tag{4}$$

Where $\widehat{a}_{gd}$ and $\widehat{\Delta b}_{gd}$ are the expected number of adoption decisions and installations in tract g on day d, respectively, $p(a)_{hd}$ is the probability that household h decided to adopt on day d, $p(\Delta b)_{hd}$ is the probability that household h installed a PV system on day d, and H is the total number of households in tract g.

3.3. Model specifications

We implement three variations of the following basic model structure, using fixed effects to identify the impact of the installed base on adoption rates:

$$a_{gd} = \alpha + \beta b_{gd} + T_g + GQ_{gd} + \zeta_d + \varepsilon_{gd} \tag{5}$$

Where a_{gd} is the observed adoption rate based on imputed dates (discrete dates) or the expected number of adoption decisions (continuous probabilities), b_{gd} is the observed installed base (discrete date base), the observed daily installs (discrete date deltas), or the expected daily installs (continuous probabilities), T_g is a tract fixed effect, GQ_{gd} is an area-quarter fixed effect, and ζ_d is a set of time fixed effects for year-month, day-of-month, and day-of-week. The area-quarter fixed effect is defined at the Census place level, a geographic area roughly the size of a city. The tract fixed effects are included to control for demand shifters that vary across tracts, such as area income levels and rates of home ownership. The area-quarter fixed effect controls for exogenous regional covariation between adoption and installation rates unrelated to peer influence, such as the introduction of new local incentives or the opening of a new installation business. Note that we exclude the tract fixed effect from the discrete date deltas model given that the tract and area-quarter fixed effects statistically absorb most of the tract-level variation in daily adoption and installation rates (Δb_{gd}). Including both fixed effects depletes the statistical power of the model and renders most coefficients insignificant. Still, the coefficient signs robustly support the two primary conclusions discussed in Section 5 using any combination of tract and area-quarter fixed effects (see Supplementary Table S1).

We test peer effects within and across individual income levels by identifying records associated with LMI and non-LMI households. In social assistance programs, LMI households are typically identified using thresholds based on area median incomes defined by the Department of Housing and Urban Development. For our purposes, we identify LMI households as those earning <100% of area median income. We chose the 100% threshold for statistical purposes given that we cannot similarly bifurcate the total number of households in tracts by area median incomes. By using 100% as a threshold, roughly half of households are defined as LMI and half as non-LMI, allowing for easier comparison across LMI and non-LMI adoption rates. The results are robust to alternative criteria for identifying LMI households (see Supplementary Table S2). We then implement the following specifications:

$$a_{igd} = \alpha + \omega b_{igd} + T_{tg} + GQ_{gd} + \zeta_d + \varepsilon_{gd} \quad (6)$$

$$a_{igd} = \alpha + \gamma b_{igd} + \mu b_{\neq igd} + T_g + GQ_{gd} + \zeta_d + \varepsilon_{gd} \quad (7)$$

Where a_{igd} is the observed (discrete dates) or expected (continuous probabilities) adoption rate among households in income group i (LMI or non-LMI), b_{igd} is the observed or expected install variable for income group i, and $b_{\neq igd}$ is the observed or expected install variable for the opposite income group. Note that the coefficient ω reflects peer effects from installs at all income levels on adoption rates in a specific income group, γ reflects within-group peer effects (e.g., LMI on LMI), and μ reflects across-group peer effects.

In Eq. (7), the hypothesis is that within-group peer effects are stronger than across-group effects, i.e., $\gamma > \mu$. We do not specify the mechanism through which within- and across-group effects vary. Under peer influence theory, the hypothesis $\gamma > \mu$ stems from the idea that individuals are more strongly influenced by peers with shared identities. For instance, LMI households may be more strongly influenced by installations by other LMI households facing comparable budget constraints. Another factor that could explain $\gamma > \mu$ is the local clustering of households with comparable income levels, also known as income segregation (Reardon and Bischoff, 2011). Spatial income segregation is largely a macro-scale phenomenon, such that income is mostly segregated over broad geographic areas (e.g., between neighborhoods) and less segregated over small geographic areas (e.g., between blocks) (Reardon and Bischoff, 2011). Still, income segregation within tracts (our geographic unit of analysis) means that peer effects among households with similar income levels may be stronger because those households tend to live closer to one another. This second mechanism establishes an indirect effect of income on peer effects due to proximity.

3.4. Identification

Breusch-Godfrey tests show that serial autocorrelation exists in all specifications, but F-test comparisons of models in tracts with large markets show that the order of autocorrelation is substantially shorter than the minimum imputed lag time of 39 days, thus satisfying the identification condition.

3.5. Robustness checks

We implement robustness checks to explore two sources of uncertainty in our models. First, our models are built on uncertain assumptions for contract-apply and issue-install timelines. For simplicity, our preferred specifications use national median durations and probability distributions for these timelines. The NREL data show that these timelines vary substantially over space, partly due to differences in state and local permitting regulations. To test the robustness of our results to our timeline assumptions we implement separate models in the five states with the most coverage in the NREL data: Arizona, California, Massachusetts, Nevada, and New York. We estimate separate models based on state-level median durations and probability distributions. The estimates of these robustness checks are provided in Supplementary Information, Tables S3-S7. Second, our models use fixed effects to control for other PV demand shifters, consistent with the approach first developed by B&G. As a robustness check, we implement alternative specifications that directly control for three demand shifters: local income levels, rates of home ownership, and the PV hosting capacity of rooftops. We present the results of the models with direct controls for demand shifters in Supplementary Information, Table S8.

4. Results

We use our data and refined peer effects models to explore three research questions treated in the three following sub-sections. First, we estimate population-level peer effects regardless of household income levels. Second, we explore the relative magnitudes of peer effects in different income groups to understand whether LMI peer effects are quantitatively distinct from non-LMI peer effects. Third, we explore peer effects within income groups (e.g., the effects of LMI installations on LMI adoption rates) and across income groups (e.g., the effects of non-LMI installations on LMI adoption rates). Specifically, we explore the hypothesis that within-group peer effects are stronger than across-group peer effects.

The coefficients in the discrete date specifications can be interpreted as an increase in probability (x10^{-6}) of adoption per household in a tract given a unit-change in the installed base (base model) or an additional installation on the same day (deltas model). The coefficients in the continuous probability model can be interpreted as changes in the number of adoptions per additional installation in a tract on any given day. For simplicity, we convert some discrete date results into estimated impacts in terms of percentage point changes in the probability of adoption based on an average-sized tract (1780 households). For instance, a discrete date coefficient of 9×10^{-6} equates to a roughly 1.6 point change in the probability of adoption in an average-sized tract ($9 \times 10^{-6} \times 1780$). We present the percentage point results in brackets in the result tables. All results are presented with tract-clustered standard errors.

4.1. Rooftop solar peer effects at all income levels

Table 2 presents estimated impacts of peer effects for all income levels, based on Eq. (5) in Section 3.3. The discrete date base model suggests that an additional installation increases the probability of adoption by around 0.1×10^{-6}, comparable to a coefficient of 0.13×10^{-6} from a similar specification in B&G. The discrete date deltas model suggests that an installation on a given day increases the probability of adoption on that day by around 10.3×10^{-6} per household, or an increased adoption probability of around 1.8 percentage points in an average-sized tract. For comparison, B&G estimated an effect of around 0.8 percentage points. The difference may be partly due to differences in model specifications and our assumptions around imputed dates as well as sample differences. B&G study peer effects from 2001 to 2011, a period when the PV industry operated at a significantly smaller scale than during our study period (2010–2020).

Table 2
Rooftop solar peer effects for all households.

	Discrete Date Base (x10^{-6})	Discrete Date Deltas (x10^{-6})	Continuous Probability
Installed base	0.11*	10.38*	0.50*
	(0.01)	(0.72)	(0.01)
	[0.02]	[1.8]	
Tract FE	X		X
Area-quarter FE	X	X	X
Year-month FE	X	X	X
Day-of-month FE	X	X	X
Day-of-week FE	X	X	X
N	82,867,232	82,867,232	82,867,232
Adjusted R^2	0.04	0.02	0.65

(tract-clustered standard errors in parentheses) [discrete date coefficients converted to percentage point terms based on average-sized tracts presented in brackets].

* $p < 0.05$.

The continuous probability model suggests that every two installations spawn roughly one peer-influenced adoption. The continuous probability peer effect is orders of magnitude greater than the discrete date effects. The greater magnitude was expected due to the structure of the continuous probability model, which better captures lagged peer effects. Still, the magnitude of the continuous probability coefficient merits a closer examination of what is being measured. We posit that the relatively large continuous probability peer effects reflect the central role of customer referrals in the U.S. rooftop PV industry. Customer referrals—financial incentives paid by installers to adopters who refer other customers—are the most common method of customer acquisition in the U.S. rooftop PV industry (Sigrin et al., 2022). Available data suggest that around half of rooftop PV sales derive from referrals (Mond, 2017) and that >70% of installers offer referral incentives (EnergySage, 2022). Assuming that most referrals occur locally and occur after systems have been installed, the peer effect coefficients include referral-driven adoptions. Valid arguments could be made for and against whether referrals reflect peer influence. On one hand, some referrals likely accelerate peer interactions that would have happened later, such that referrals are a form of subsidized influence. On the other hand, at least some referral-driven adoptions may not have occurred absent the referral incentive, such that the estimated peer effects overstate the true role of peer influence in driving adoption. The continuous probability results therefore provide an intuitive alternative metric for measuring peer influence, but the magnitudes of these "peer" effects are likely biased by financial incentives for referrals.

4.2. Rooftop solar peer effects by income level

Table 3 presents results for peer effects separately for LMI and non-LMI households, based on Eq. (6) in Section 3.3. All specifications suggest that peer effects are weaker for LMI than for non-LMI households. The discrete date deltas models, for instance, indicate that an additional installation increases the probability of an LMI adoption by around 1.3×10^{-6} or about 0.2 percentage points for an average-sized tract, compared to 9.1×10^{-6} or about 1.6 percentage points for non-LMI households. The continuous probability model similarly suggests that non-LMI peer effects are about twice as strong as LMI peer effects.

4.3. Rooftop solar peer effects within and across income levels

Table 4 provides results for estimated peer effects within and across income groups, based on Eq. (7) in Section 3.3. The models support the hypothesis that within-group peer effects are stronger than across-group effects. For instance, the discrete date deltas model suggests that within-group peer effects are about twice as strong than across-group peer effects. Specifically, an additional LMI installation is associated with a 0.5 percentage point increase in the probability of an LMI adoption, compared to a 0.1 point increase from non-LMI installations in average-sized tracts. Fig. 2 illustrates these results, showing how within-group peer effects are stronger than across-group effects, and that all effects are smaller for LMI than for non-LMI adoption.

All models suggest that peer effects from installs at any income level are weaker on adoption decisions among LMI than non-LMI households. Relatively weak LMI peer effects at least partly reflect the fact that LMI adoption rates are lower overall (see Table 1). To compare relative changes in adoption rates, Fig. 3 depicts the peer effect coefficients as ratios of background LMI and non-LMI adoption rates. That figure shows how the differences in results across income groups relative to background adoption rates are smaller than the unadjusted differences. Though the total relative effects remain substantially lower for LMI adoption, estimated within-group peer effects are more comparable across income groups when adjusting for background adoption rates.

5. Discussion & conclusions

We use household-level income estimates for rooftop PV adopters to explore whether peer effects vary across household income levels. Our two primary conclusions are that 1) peer effects are generally weaker among LMI households and that 2) peer effects are stronger within income groups, such that LMI installations have greater impacts on LMI adoption rates than do non-LMI installations.

The first conclusion requires a nuanced interpretation. Peer effect models only measure influence that results in adoption decisions. As a result, peer effects are partly a function of pre-influence adoption probabilities. Households with low pre-influence adoption probabilities require stronger influence to adopt and thus register peer effects than households with higher pre-influence adoption probabilities. All else equal, LMI households have lower pre-influence adoption probabilities due to various LMI adoption barriers, particularly budget constraints. Lower pre-influence adoption probabilities reduce LMI relative to non-LMI peer effects. Hence, our results should be interpreted to mean that peer influence is less likely to translate to adoption among LMI households, not that LMI households are less susceptible to influence. Indeed, a plausible hypothesis is that LMI households are more susceptible to influence. Starting from lower pre-influence adoption probabilities, peer influence may have relatively greater impacts on LMI households than on non-LMI households who may already be inclined to adopt PV. Future research could explore methodologies to measure influence more directly and how the magnitude of influence—not necessarily manifested in adoption—may vary across income levels.

Our second conclusion provides a rationale for targeted policies to seed PV installations on LMI rooftops. That is, insofar as enabling LMI PV adoption is a policy objective, our results suggest that seeding programs would be more effective if designed to seed installations on LMI rooftops or in LMI areas, specifically. Still, the fact remains that peer influence is less likely to translate to LMI adoption. The relative weakness of LMI peer effects shows the limitations of influence as a policy tool to promote

Table 3
Rooftop solar peer effects across at different income levels.

	Discrete Date Base ($x10^{-6}$)		Discrete Date Deltas ($x10^{-6}$)		Continuous Probability	
	Y = LMI	Y=Non-LMI	Y = LMI	Y=Non-LMI	Y = LMI	Y=Non-LMI
Installed base	0.01*	0.10*	1.29*	9.09*	0.10*	0.40*
	(0.001)	(0.006)	(0.13)	(0.67)	(0.004)	(0.01)
	[0.002]	[0.02]	[0.2]	[1.6]		
Tract FE	X	X			X	X
Area-quarter-year FE	X	X	X	X	X	X
Year-month FE	X	X	X	X	X	X
Day-of-month FE	X	X	X	X	X	X
Day-of-week FE	X	X	X	X	X	X
Adjusted R^2	0.01	0.03	0.01	0.02	0.38	0.63
N	82,867,232	82,867,232	82,867,232	82,867,232	82,867,232	82,867,232

(tract-clustered standard errors in parentheses) [discrete date coefficients converted to percentage point terms based on average-sized tracts presented in brackets].
* $p < 0.05$.

Table 4
Rooftop solar peer effects across and within income levels.

	Discrete Date Base (x10^{-6})		Discrete Date Deltas (x10^{-6})		Continuous Probability	
	Y = LMI	Y=Non-LMI	Y = LMI	Y=Non-LMI	Y = LMI	Y=Non-LMI
LMI installed base	0.10*	−0.02	2.99*	1.87*	0.23*	0.15*
	(0.01)	(0.02)	(0.30)	(0.41)	(0.01)	(0.007)
	[0.02]	[−0.004]	[0.5]	[0.3]		
Non-LMI installed base	−0.005*	0.12*	0.69*	11.64*	0.06*	0.48*
	(0.002)	(0.01)	(0.14)	(0.83)	(0.003)	(0.01)
	[−0.001]	[0.02]	[0.1]	[2.1]		
Tract FE	X	X			X	X
Area-quarter FE	X	X	X	X	X	X
Year-month FE	X	X	X	X	X	X
Day-of-month FE	X	X	X	X	X	X
Day-of-week FE	X	X	X	X	X	X
Adjusted R^2	0.01	0.03	0.008	0.02	0.39	0.63
N	82,867,232	82,867,232	82,867,232	82,867,232	82,867,232	82,867,232

(tract-clustered standard errors in parentheses) [discrete date coefficients converted to percentage point terms based on average-sized tracts presented in brackets].
* $p < 0.05$.

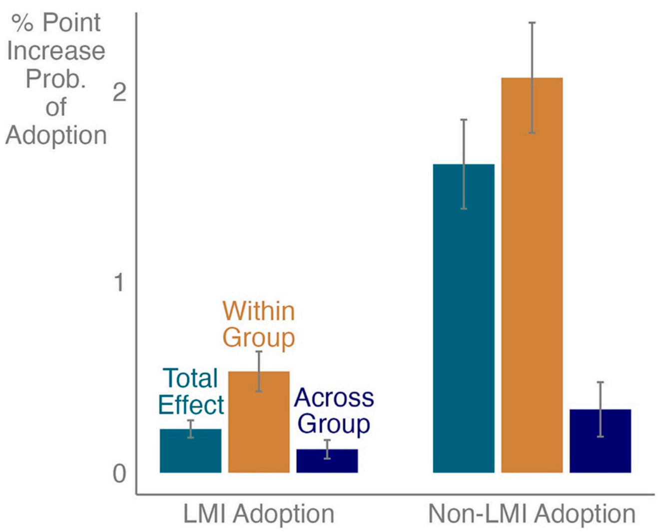

Fig. 2. Peer effects within and across income groups. Figure depicts percentage point increases in probabilities of adoption for peer effects across and within income groups. Estimates based on coefficients from discrete date deltas model and assuming a typical Census tract of 1650 households.

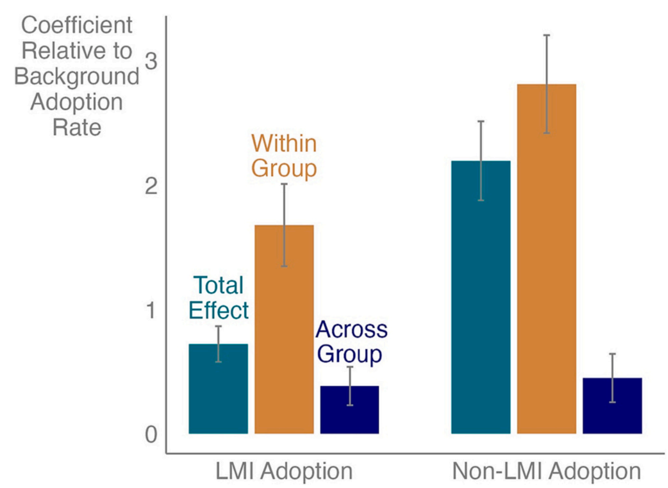

Fig. 3. Estimated peer effects at different income levels relative to background adoption rates. Plot depicts ratios between peer effect coefficients and background adoption rates as defined by a mean daily adoption from Table 1. Estimates based on coefficients from discrete date deltas model.

LMI adoption. Influence-driven policies such as seeding could prime LMI households to consider adoption. However, such interventions do not address LMI barriers to adoption such as budget constraints. To the extent that policymakers seek to accelerate LMI adoption of PV and other clean energy technologies, our results provide a rationale to pair targeted influence-driven policies with interventions that directly address LMI adoption barriers, such as incentives or access to low-cost financing.

Finally, from an economic perspective, our results further confirm that peer influence plays a role in shaping customer demand curves for emerging technologies. Peer influence is effectively a demand-shifter that, we have shown, itself varies with customer income levels. Heterogeneous peer effects provide one more mechanism to explain consumption inequality. More formally, returning to the theoretical model, we could write customer demand as a function of income:

$$Q_{j,g} = D\left(p, \beta(i) Q_{\neq j,g}, X(i)\right) \tag{8}$$

Where $\beta(i)$ is a peer effect that is itself a function of income and $X(i)$ is a vector of demand shifters that are likewise functions of or correlate with income. For most emerging technologies, including rooftop PV, both $\beta(i)$ and $X(i)$ shift low-income demand curves to the left, constraining low-income consumption. Future work could consider the role of peer effects in explaining the inequitable adoption of other emerging products and consumer technologies.

We conclude with a suggestion for further research. As noted throughout this paper, peer effect models generally do not specify the mechanism of peer influence. Heterogeneous influence mechanisms provide one potential explanation for differing magnitudes of peer effects across income levels. For instance, Wolske (2020) finds descriptive evidence that panel visibility and word-of-mouth communication may be more influential for LMI households than for higher-income households. If so, heterogeneous peer effects could be partly explained by heterogeneous building and urban designs in LMI areas. Further, insofar as influence mechanisms differ across income levels, policymakers could focus on the influence interventions that are most effective for LMI adoption decisions. The possibility of heterogeneous influence mechanisms is an area for further research.

Inclusion and diversity statement

One or more of the authors of this paper self-identifies as an underrepresented ethnic minority in science. One or more of the authors of this paper received support from a program designed to increase minority representation in science.

CRediT authorship contribution statement

Eric O'Shaughnessy: Conceptualization, Methodology, Investigation, Writing – original draft, Formal analysis, Data curation, Visualization. **Alexandra Grayson:** Conceptualization, Writing – original draft, Data curation, Writing – review & editing. **Galen Barbose:** Conceptualization, Writing – review & editing, Funding acquisition, Supervision.

Declaration of Competing Interest

None.

Data availability

Certain elements of this work are based on proprietary data that cannot be shared. Aggregated, non-proprietary data and scripts will be made available upon reasonable request from the authors.

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Appendix A. Supplementary data

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