Title

The effect of residential solar on energy insecurity among low- to moderate-income households

Authors
Yozwiak, Madeline
Barbose, Galen
Carley, Sanya
et al.

Publication Date
2025-03-05

DOI
10.1038/s41560-025-01730-y

Energy Markets & Policy
BERKELEY LAB

The Effect of Residential Solar on Energy Insecurity among Low-to-Moderate Income Households

Madeline Yozwiak[1], Galen Barbose[2], Sanya Carley[3], Sydney P. Forrester[2], David M. Konisky[1], Trevor Memmott[4], Cristina Crespo Montañés[2], Eric O'Shaughnessy[2]

[1]O'Neill School of Public and Environmental Affairs
2Lawrence Berkeley National Laboratory
3University of Pennsylvania, Philadelphia, PA, 19104, USA;
4University of Massachusetts Boston

March 2025

This work was funded by the U.S. Department of Energy's Office of Energy Efficiency and Renewable Energy (EERE) under Solar Energy Technologies Office (SETO) Agreement Number 38444 and Contract No. DE-AC02-05CH1123.

Title: The Effect of Residential Solar on Energy Insecurity among Low-to-Moderate Income Households

Authors: Madeline Yozwiak[1*], Galen Barbose[2], Sanya Carley[3], Sydney P. Forrester[2], David M. Konisky[1], Trevor Memmott[4], Cristina Crespo Montañés[2], Eric O'Shaughnessy[2]

Affiliations: [1]O'Neill School of Public and Environmental Affairs, Indiana University, Bloomington, IN, 47401, USA; [2]Lawrence Berkeley National Laboratory, Berkeley, CA, 94720, USA; [3]University of Pennsylvania, Philadelphia, PA, 19104, USA; [4]University of Massachusetts Boston, Boston, MA, 02125, USA.

Corresponding Author: Madeline Yozwiak, myozwiak@iu.edu

Abstract: Each year, millions of Americans experience energy insecurity, or the inability to afford enough energy to meet their basic needs. This study evaluates whether residential rooftop solar can serve as a preventative solution to energy insecurity among low-to-moderate income households. Using a national, matched sample of solar and non-solar households, based on detailed and address-specific data, we find that solar leads to large, robust, and salient reductions in five indicators of energy insecurity. Moreover, the benefits of solar "spill over" to improve a household's ability to pay other energy bills. The results suggest that rooftop solar may be an effective tool for policymakers who seek to reduce energy insecurity.

Introduction

Energy insecurity–or the inability to secure sufficient energy for one's needs–impacts millions of U.S. households each year. Researchers often measure energy insecurity by households that report being unable to pay their bills, maintain comfortable temperatures, or avoid utility disconnection; according to the Energy Information Administration's (EIA) most recent Residential Energy Consumption Survey (RECS), around 33 million households experience energy insecurity in the United States.[1] Households facing energy insecurity often resort to coping strategies to limit their energy expenditures to avoid forgoing other household necessities and reduce the threat of utility disconnection.[2] These strategies include keeping homes at uncomfortable and potentially dangerous temperatures[3,4] and choosing between adequately heating homes or purchasing enough food for adequate nutrition, referred to in the literature as the "heat or eat" dilemma.[5]

Scholars have identified several leading predictors of energy insecurity, including inefficient housing conditions, household race and ethnicity, income, and having vulnerable populations in the home, such as young children or medically compromised individuals.[6–8] Furthermore, research finds that energy insecurity is a chronic condition for many. Households that experienced energy insecurity once—such as struggling to pay their bills or facing utility disconnection—are more likely to face these situations on a recurring basis, even after controlling for other observable predictors of energy insecurity.[9] In other words, past energy insecurity can lead to future energy insecurity, and these conditions may yield a perpetual cycle that is difficult to break.

Despite the cyclical nature of energy insecurity, most strategies identified in the literature that relieve energy hardship primarily provide short-term relief to individuals and households, rather than seek to prevent insecurity from reoccurring in the future. For example, existing policy mechanisms to address energy insecurity include bill assistance from the government, such as that provided by the Low Income Home Energy Assistance Program (LIHEAP),[10] bill payment plans offered by utilities,[11] and protections from disconnections.[12–14] To date, the main preventative solution that has been studied is weatherization assistance, in which income-qualified households receive energy efficient upgrades and home sealing and insulation.[15] Because weatherization improves the quality of the home, it can act as a long-term reduction in the household's energy use and utility bills, thereby increasing the likelihood the household is able to meet their energy needs in the future.

An additional preventative solution that has not been well-studied in the energy insecurity literature is rooftop solar. Like weatherization, solar panels reduce the amount of electricity a household purchases from the utility, creating a long-term decrease in their utility bills.[16–18] In addition, because a solar array is typically sized to generate around 80-100% of a household's annual electricity use, the decrease can be quite large. As policy solutions, both weatherization and rooftop solar create split incentive problems for renters and may require supporting policy measures to effectively reach low-income individuals who do not own their home.[19] While the potential impact of rooftop solar on energy insecurity has been broadly theorized in the literature,[7] empirical analysis that demonstrates its efficacy is lacking. To our knowledge, the only study that directly analyzes the impact of solar on energy insecurity is Riley et al.,[20] which explores changes in service disconnections among a small group of solar adopters in rural Australia. The study finds that voluntary service disconnections—an energy insecurity coping strategy—were common in the population before solar adoption but completely absent post-adoption.

In a parallel literature, we note that recent research has established that rooftop solar can reduce energy burden, defined as the percentage of income dedicated to energy expenses, and that larger percent reductions are observed among households with low-to-moderate incomes even while taking the cost of adoption into account.[21] Our study on the effect of rooftop solar on energy insecurity directly complements this work in three ways. First, energy insecurity questionnaires are able to identify households that employ behavioral responses to afford their bills, such as rationing energy use, which can mechanically lead to a lower and misleading estimate of energy burden. Second, energy insecurity measures qualitative dimensions of hardship, such as health and physical comfort in the home. Finally, because we obtain data on energy insecurity via survey, we are able to obtain self-reported socio-economic information from households, in contrast to the estimated demographic variables used in prior research such as Forrester et al.[21] Understanding the effect of rooftop solar on both metrics thus provides complementary evidence for policymakers who seek to alleviate hardship.[22]

In this paper, we evaluate whether installing solar affects energy insecurity in the US. We do so in a large, national sample of 2,618 households across 35 states, and we collect data on five measures of energy insecurity to provide a complete picture of how rooftop solar affects households' experiences. Our sample is comprised of primarily low-to-moderate income (LMI) households, who are more likely to experience energy insecurity and the target population of most policy interventions.[7] Using data from Lawrence Berkeley National Laboratory, CoreLogic, and Experian, we create a matched sample of primarily LMI households with and without solar in the United States and compare their self-reported experience of energy insecurity through an original survey. The design of our study enables us to estimate a plausibly causal effect of solar adoption on the incidence of energy insecurity. We find that rooftop solar adoption reduces energy insecurity across all five measures in the analysis. With solar, households are better able to pay their energy bills; less likely to receive a disconnection notice, reduce their energy consumption or forgo expenses to afford energy bills; and more likely to keep their home at a comfortable temperature. The effects are precisely estimated and robust to multiple sensitivity tests. Given this evidence, we propose that rooftop solar can be considered an effective, preventative solution to energy insecurity—providing an additional tool for policymakers seeking to address the issue.

Characteristics of survey respondents

From January to March 2023, we sent surveys on experiences of energy insecurity to a matched, national sample of primarily LMI households with and without solar installed on their homes. Central to our research design is the similarity of solar and non-solar households: we use a robust series of data sets to identify households without solar that can serve as the counterfactual for households with solar.

In other words, non-solar households benchmark the levels of energy insecurity that solar households would have felt, but for the installation of solar panels. Our design enables us to estimate an unbiased, plausibly causal treatment effect if the two arms of the survey are sufficiently similar, on average, across characteristics predictive of adopting solar panels and experiencing energy insecurity.

Among the 2,608 respondents in our main sample, we find no major differences within demographic and housing characteristics of solar and non-solar households, as we show in Table 1. We assess balance using a standardized mean difference, Cohen's *d* (Methods). The units of Cohen's *d* are standard deviations, such that a value of 0.10 indicates that solar and non-solar mean are 0.10 standard deviations apart. We selected this statistic because, unlike a *t*-test, it is a property only of the sample and not a function of sample size.[23] In the matching literature, a common rule of thumb is that the means of covariates should fall no more than 0.25 standard deviations apart.[23–26]

Table 1 shows that solar and non-solar respondents are balanced across mean age, gender, number of individuals in the household, race, age of the home, estimated electricity price, and state. For characteristics where we observe a small statistical difference among respondents, solar households are more likely to earn above $150,000 per year, to have attended graduate school, and to own and live in larger houses. However, the standardized mean difference for all covariates falls below 0.25, and only one (square footage) is above 0.20. This suggests that any small differences in observed characteristics between survey arms, such as income, can be appropriately adjusted for using covariates in a regression.[25] Finally, while the sample contains respondents from 35 states, most of the sample resides in California, Florida and Texas, reflecting the relatively large sizes of the residential solar markets in these regions. Within states, nearly all (90%) of the sample is located in counties where there are both solar and non-solar respondents, as illustrated in Figure 1.

The effect of solar on energy insecurity

We find that solar reduces energy insecurity in LMI households across a range of measures, presented in Figure 2 and Table 2. For each outcome, the survey questions prompted respondents to report incidence over the prior three months; because households responded between January and March, our survey captures household experiences of energy insecurity during the late fall and early winter months. The intercept of the model is the average incidence of the outcome among non-solar households. In our sample, 13.3% of non-solar households report being unable to pay an electricity bill; 8.0% received a disconnection notice due to nonpayment of an electricity bill; 74.1% reduced their consumption to save money on an energy bill; 22.6% reduced or forwent expenses for basic household necessities, like food or medicine, to pay an energy bill; and 33.1% always or often kept their home at an uncomfortable temperature.

The coefficient on solar represents the absolute change in likelihood that a household experiences a given measure of energy insecurity, after installing solar; all outcomes are binary and the regression is specified as a linear probability model. Normalizing the coefficient by the incidence in the control arm provides the percentage change in how often the outcome occurs with solar. Our results suggest that solar leads to a 5.9 percentage point reduction in the likelihood an individual is unable to pay their electricity bill (44% less often, relative to the control arm); a 3.7 point reduction in the likelihood they receive a disconnection notice (46% less often); an 11.2 point reduction in the likelihood they reduce their energy consumption to save money on energy costs (15% less often); a 7.6 point reduction in the likelihood they forgo expenses on household necessities to pay an energy bill (34% less often); and a 6.6 point reduction in the likelihood their home is kept at an uncomfortable temperature (20% less often). All coefficients are significant at the 1% level, with standard errors clustered at the state level.

The magnitude and precision of these coefficients is unaffected by the inclusion of all available demographic and housing characteristics for each household as covariates, shown in Figure 2 (“Main, with Covariates”) and Table 3. This suggests that our treatment effects are unlikely to be driven by a small difference in observed covariates between solar and non-solar households. However, a limitation of this approach is that it implicitly makes a structural assumption about the relationship between the covariates and the outcome (i.e., that it is linear). In the Supplementary Table 2, we show that the treatment effect estimates are robust when we add flexibility to the model and include the estimated propensity score as the covariate. In addition, we find no meaningful difference in the treatment effect estimates when re-weighting the control arm, using inverse propensity score weights, to better mirror the observed distribution of characteristics among solar respondents (Supplementary Table 3). Details regarding both specifications are provided in the Methods section. Finally, we show that the effects are not sensitive to unobservables at the county-level by including a county fixed effect in Supplementary Table 4.

The estimated treatment effects of solar on energy insecurity are further robust to several alternate sample definitions, presented in Figure 2. First, we add respondents from our pilot survey (“With Pilot Respondents”; Supplementary Table 5) and include recategorized households that self-reported having or not having the technology (“Recategorize”; Supplementary Table 6). These “miscategorized” households are dropped in our main specification (Tables 2 and 3; Methods). Second, we impose a stricter requirement for comparability at the sub-state level by limiting respondents to those counties where we have replies from both treatment and control households (“Counties with Both”; Supplementary Table 7). Finally, we test whether our effects are driven by higher income individuals by excluding respondents who self-report income above 110% of AMI (“Drop High Income”; Supplementary Table 8). To be conservative, all regressions on the alternate samples include covariates. The magnitude of the treatment effects, across alternative samples, remain stable. Some precision is lost in the specification without higher income households, which we attribute to the reduction in sample size (N=1061), but we note that the estimated effect of solar on two of the outcomes (ability to pay electricity bill and to keep a comfortable temperature in the home) is larger for the lower-income households in the sub-sample.

While the direct benefits of solar are limited to electricity bills, we also find evidence that the benefits of solar “spill over” to enhance a household’s ability to pay other types of energy bills. For example, among those households with both natural gas and electricity service, we find that solar is associated with a 4.3 percentage point decrease in the likelihood of being unable to pay a natural gas bill, as presented in Table 4 without covariates and Supplementary Table 9 with covariates. This is equivalent to a 43% reduction over the mean incidence of 10% in the control arm.

Heterogeneity by how households pay for their solar panels

We expect that the amount a household saves on electricity expenditures, after installing solar, will mediate the magnitude of the effect of rooftop solar on energy insecurity. While we do not observe savings for solar households directly—which requires data on a household’s solar production, electricity usage, and electric utility tariff, pre- and post-solar—we do ask respondents to recall information on the economics of their solar system and how much they typically pay for electricity. These results are presented in Supplementary Table 10 and Table 5, respectively. Supplementary Table 10 shows that majority of households (54%) claimed the federal investment tax credit (ITC) on their purchase, though our questions on the receipt of incentives had a high rate of non-response (38%). Most households report paying for their installations over time using a loan (40%) or a lease (16%), and a minority report receiving their panels through an incentive program (9.6%). These responses indicate that the LMI households in our sample primarily paid for their solar panels through private investment, similar to the overall rooftop solar market.

Table 5 shows that households with solar report spending about $65 less per month on electricity, which is 36% lower than the control mean of $183. On first glance, the magnitude of electric utility bills for solar households may appear to be high. Because rooftop solar systems are typically sized to produce or offset 80-100% of a household's annual average electricity use, we anticipate a similar reduction in electric utility bills, after going solar. (We note that the actual reduction depends on both the rate structure of the utility tariff and state-specific net metering policy, and savings can be reduced by the use of fixed fees or demand charges.) Our reported difference (38%) is lower and aligns, instead, with the expected monthly savings: households pay less to the utility but add an additional loan or lease payment to pay for their installation. As a result, we believe households may have reported their bills inclusive of solar lease or loan payments, in response to our survey question ("How much are your average monthly bills for electricity?").

Finally, in Table 6, we explore if the way in which a household financed their solar panels moderates the effect of solar on energy insecurity. We categorize households into three groups: those who report paying for their system upfront; those who pay monthly, via a lease or a loan; and an other category, such as households who did not respond to the finance questions or reported receiving their installation for free. Because financing structure (upfront or monthly) affects how solar impacts household cash flows in the near-term and total savings in the long-term, there is strong reason to expect the effect of solar on energy insecurity may vary across these groups. Formally, we include F-tests to assess if the treatment effects are statistically different between adopters that paid upfront or monthly for their panels.

Interestingly, our results are mixed. We observe no difference in the effect of solar by payment timing in three of the five energy insecurity outcomes, but in two–the likelihood a household reduces consumption (column 3) and that they forgo expenses (column 4)–we find upfront customers perceive significantly different and larger effects of solar. This may run counter to intuition; to pay upfront, a household must make a large, one-time investment that can take years to be recouped through bill savings. That solar is associated with greater reductions in certain energy insecurity measures for these households may suggest that the monthly cash flows, after installing solar, could influence households' felt experience of energy insecurity. For example, households who pay monthly will typically save money immediately, with no money down, but they also add a new loan or lease bill, after going solar. In contrast, households who pay upfront experience only reductions in their electricity bills, on a monthly basis, once the large initial payment has been made.

One concern about these findings is that households who pay upfront may be, on average, wealthier than households who pay monthly, confounding our estimate of the effect of solar. While we do find that a larger share of households who report paying for their system upfront also self-report income in the highest bracket of more than $150,000 per year (Supplementary Figure 1), we note that the results in Table 7 include income as a control and that the results are consistent when we exclude households with the highest incomes from the sample (Supplementary Table 11). Both suggest that the choice of payment mechanism influences the effect of solar on a household's likelihood to reduce consumption or to forgo expenses. However, because we had a high rate of non-response on the financing questions overall, we caution that the results in Table 7 should be viewed as suggestive and preliminary findings.

Discussion

Using address-specific data on solar adoption and housing characteristics along with estimated, household-level demographics, we created a matched sample of primarily LMI households with and without solar power across 35 US states. Households across our survey arms are similar across a large range of characteristics, including geography, income, race, education, family size, gender and household type. The strong balance across observable characteristics supports a plausibly-causal interpretation and provides empirical evidence on the effect residential solar on U.S. household energy insecurity to the literature.

We find that the installation of solar panels leads to large decreases in households' experience of energy insecurity. Solar households are 44% less likely to report being unable to pay their electricity bills, 46% less likely to receive a disconnection notice from their electricity provider, 15% less likely to reduce their energy consumption to save money on energy costs, 34% less likely to forgo necessary expenses to pay an energy bill, and 20% less likely to keep the home at an uncomfortable temperature. These effects are robust to different model and sample specifications. We further find that the effect of solar spills over from electricity and improves a household's ability to pay their natural gas bills.

Future research can expand our findings in several directions. First, our survey captured household energy insecurity during the early winter, with a sample weighted towards warmer climates, such as California, Texas and Florida. However, energy insecurity in these regions is likely higher during the summer, when air conditioning leads to greater energy expenditures. Similar to energy demand, solar generation also varies with the weather and time of year, with generation peaking in the summer months. As a result, it is possible that our estimated effects based on a winter survey understate the full annual reduction in energy insecurity due to solar in these regions. Future work may seek to quantify how the effects of solar on energy insecurity varies over the course of the year or across regions.

Second, the effects of solar on energy insecurity may also vary the longer a household has the technology installed. We effectively measure the near-term impact of solar soon after households adopt. Our survey was administered in early 2023, and all solar households installed their panels in 2021. Long-term effects may vary for several reasons: household energy usage can increase after installing solar[27,28], solar panel production degrades about 0.5% per year, and up-front installation costs for panel owners can affect near-term perceptions of energy insecurity, to name just a few. Further research is needed to establish if the effects we measure are indicative of a long-term, persistent reduction in energy insecurity among adopting households.

Third, the treatment effect of solar on energy insecurity may also vary with the dose of savings achieved. This directly relates to policy considerations, due to the role of incentives in how much a household pays for a solar installation. For example, most households in our sample report financing their system privately using a lease, loan or by paying upfront. In contrast, some states and local governments offer free or heavily subsidized solar installations to LMI households.[29] Interestingly, we do not observe a different effect of solar on three out of five energy insecurity outcomes between households who paid upfront versus monthly. However, the results are preliminary and mixed. Further research is needed to understand the degree to which reductions in energy insecurity are sensitive to the magnitude of financial savings with solar, in order to inform policymakers who seek to design cost-effective interventions. In addition, policymakers focused on energy insecurity may also be interested in the specific effect of rooftop solar among the most economically vulnerable households, relative to the distribution of incomes captured in our sample. Due to the small share of the most economically vulnerable households among existing solar adopters, future work may seek to use a different research design, such as a randomized control trial, to explore the specific treatment effect in this sub-group.

Finally, we note that our results are specific to the policy and technology context at the time of the survey. Several broader trends within the U.S. energy system—such as changes to rate design and net metering policy,[30] as well as electrification[31]—can affect both the savings from rooftop solar adoption and the incidence of household energy insecurity. Similarly, while solar-only systems compose most of our sample (94%), growing shares of households are adopting solar plus storage systems. (We did not include a question about storage on our survey, due to space constraints. Instead, we can infer which households have storage using the Tracking the Sun database; we consider the value an estimate because the storage variable is not reported by all regions. We find that 100 of our solar respondents, of which 64 are in California, had both solar and storage installed. This is equivalent to 6.1% of the solar arm in the main sample.) It is likely that solar plus storage systems affect energy insecurity differently than solar-only systems, due to the added up-front costs of batteries, the potential for additional bill savings for households in certain regions, and the ability of solar-plus-storage systems to provide back-up power during outages while solar-only systems cannot. The connection between an increase in power outages related to extreme weather,[32] subsequent changes to energy insecurity, and possible technological trends, such as the rise of solar plus storage, is not well-understood and could be fruitful to explore.

Overall, our results reveal that rooftop solar is a potentially effective solution to reducing energy insecurity. As an intervention, solar is distinct from measures such as bill assistance and utility disconnection protections, which focus on families already experiencing hardship and do not address the underlying causes of energy insecurity. Rooftop solar is perhaps most similar to weatherization because both reduce net load for the home and thus act as a preventative solution to energy insecurity. With solar, households reduce the electricity they purchase from the utility by the amount of solar electricity consumed by the home, as well as receiving credits for solar generation exported to the grid, resulting in savings on utility bills through the lifetime of the solar system. However, a solar array will typically be sized to offset a larger share of a household's energy usage, on the order of 80-100%. Solar panels are also likely to come at a higher cost than weatherization, on the order of $25-29,000 before incentives. (We note that wealthier households tend to install larger solar systems[33]. The estimate here assumes a system size of 6.1-6.8 kW, which represent the median system size for individuals earning less than $50k and $50-100k per year, respectively, across all states in 2022[33]. The size is then multiplied by the median install price for systems that are between 6-7 kW—$4.2/W, in $ 2022[34]—to obtain the stated range of $25,620 to $28,560.) In addition, because generation from solar panels depends on the weather, the financial savings for a household due to solar fluctuates across months, leading to a variable effect on energy insecurity. Establishing the relative efficacy of each policy option, as well as possible combinations—such a household installing solar after weatherization improvements—is a promising area for future research.

A challenge in reaching LMI households for weatherization and rooftop solar is that both solutions can favor individuals who live in and own single-family homes rather than individuals living in multi-family units and renters. Renting creates split incentives where building owners pay the cost of upgrades while tenants realize the benefits.[19]. While experts estimate that a significant share (42%) of the total technical potential for rooftop solar generation in the U.S. is located on buildings occupied by LMI households, the majority of this LMI potential—nearly 60%—is located on renter-occupied and multifamily buildings.[35] In other words, rooftop solar may be a possible intervention for millions of LMI households, but it is unlikely to serve as a standalone solution for certain LMI communities, especially renters and multi-family building occupants. A key question for the literature is how the energy insecurity benefits of solar may differ in alternative adoption models such as community solar, which expands solar access to renters and those in multi-family housing.[36]

Our study further adds to a small but growing literature on the role of rooftop solar in a just energy transition. This literature consists of countervailing narratives. First, research has shown that rooftop solar has thus far disproportionately benefited wealthy households,[37,38] perpetuating existing inequities in the energy system. On the other side, research has shown that rooftop solar can provide significant benefits to disadvantaged and vulnerable populations, including economic[16,17] and energy security benefits,[20] and how targeted policies and programs can be designed to accelerate and amplify those benefits.[36,39] Our research adds to the latter body of literature by showing how rooftop solar can yield a further benefit for low-income and other vulnerable populations in the form of reduced household energy insecurity, primarily among those who own and occupy their home.

Energy insecurity is a pernicious and pervasive problem, with demographically inequitable incidence in the United States.[1] In this paper, we present plausibly causal evidence that residential, rooftop solar directly reduces households' experience of energy insecurity. The effects are significant and robust. Policies that support rooftop solar are traditionally justified on their environmental benefits and the ability to drive learning-based cost reductions. Our findings in this paper suggest that the technology can also be treated as a way to reduce energy insecurity. This introduces one more much-needed tool to policymakers' toolboxes.

Methods

Overview of matched survey design

In this study, we compare the incidence of energy insecurity among primarily LMI households with solar to a matched sample of households without solar. The causal interpretation of our approach rests upon the degree to which the two groups are similar on important covariates, such that the households without solar form an appropriate counterfactual for the households with solar. More formally, this is equivalent to asserting that the conditional independence assumption holds.[40,41] This assumption is violated if there remain unobserved differences between groups, correlated with both the treatment and outcome, that lead to omitted variable bias. A strength of our study is the detailed information we are able to obtain on both households' demographics and the physical characteristics of their home from a combination of data sources, which substantially lessens the risk of bias.

Our methodology proceeded in four steps. First, we selected the characteristics on which to assess the similarity of households with and without solar. Second, we developed the survey instrument to gather information on households' experience of energy insecurity. Third, we selected an initial matched sample of similar households to whom we sent the survey. We sent an initial pilot via email and the final survey by paper mail. The final survey was in the field for three months, between January and March 2023, so that our energy insecurity outcomes are indicative of households' experiences in the early winter. Fourth, we analyzed the final survey responses among the 2,608 households who responded (63% solar, 37% non-solar), ensured the balance among arms remained, and estimated the treatment effect. Each step is discussed in detail below.

Before proceeding, one unique aspect of our research design is important to clarify, relative to the large literature on matching.[41–43] The outcome data we study was obtained using a survey and not observed at the beginning of the study. This naturally aligns with experimental best-practices for matching, because it ensures our final sample was selected only on the basis of observable characteristics and not selectively chosen to obtain a desired treatment effect result.[44] However, it also creates the possibility that the balance among the respondents could differ from the balance among all the households who were sent the survey. Unlike other matching studies, we cannot resample in this case, because doing so would require administering a second survey to obtain outcome data. Instead, we will parametrically adjust for any small observed differences by including those characteristics as covariates in a regression.[23,25]

Characteristics on which to assess similarity between solar and non-solar households

In our study, we used three types of data on households: predicted income and demographics, obtained from Experian; empirical characteristics of the property and solar adoption, obtained from CoreLogic and Lawrence Berkeley National Laboratory;[34] and self-reported data, in response to our survey. The predicted and empirical data were used to determine which households were contacted to complete the survey. Once households completed the survey, we use their self-reported demographics in our analysis. The empirical, physical characteristics of the home—square footage and age—are obtained from CoreLogic, based on the respondents' physical address.

To create the initial samples of households to whom surveys were sent, we considered a set of variables that are predictive of treatment assignment and are correlated with both treatment and likelihood to experience energy insecurity. Including predictors of both treatment receipt and the outcome of interest helps avoid omitting characteristics that, if systematically different between solar and non-solar households, could bias our results. We created our initial samples using six variables: predicted income; predicted race; geography; and physical characteristics of the home (age and square footage). We predicted the race or ethnicity of each using the wru algorithm from Imai and Khanna,[45] which uses Bayesian analysis to predict race from public voter registration data. In our setting, a few determinants of solar adoption—such as retail electricity rates and incentives—are not measured directly. Rather, we initially addressed these factors by the way in which geographic balance is enforced: control households are limited to those within the same counties as adopters, where such conditions are likely to be similar. As a robustness check, after survey responses were received, we estimated the electricity price for each respondent in the final analysis; the process is explained in the final paragraph below. Finally, we limited the initial sample to those households who are predicted to be owner occupied, because solar adoption is significantly more difficult for renters.

Within our survey, we asked households to report a larger set of demographic characteristics than we used when creating the initial samples. In addition to self-reported race and annual income, we collected data on the age, gender, household size, educational attainment, home ownership, and type of dwelling for each respondent in our survey. These characteristics are identified as predictive of solar adoption and the experience of energy insecurity within the literature, but difficult to obtain precisely from third-parties when creating our samples. We use this larger set of self-reported characteristics in our analysis. It provides a high degree of precision when comparing the similarity of treatment arms and a greater ability to control for confounding factors in robustness tests. A verifiable assumption of our methodology is that, if non-response is random among our initial sample, the final, smaller sample of survey respondents should remain balanced.

We also note that we limited characteristics to those that are unlikely to be affected by the treatment itself. An example of a control that we do not include is estimated home value, which is directly affected when a homeowner installs solar. This is especially important because we do not have pre-treatment information for solar adopters. For example, the characteristics we include are either immutable or highly stable aspects of an individual (e.g., age, gender, other members in the household, race) or the physical dwelling (e.g., state of location, age, square footage), and solar is unlikely to affect those variables that can change for an individual over time (e.g., income, education, home ownership). Supplementary Table 12 further shows that our results are not sensitive to these last three variables.

In the final analysis, we also include the estimated electricity price for each respondent. As noted above, while this characteristic was not measured when we matched our sample, it can be an important determinant of both solar adoption and energy insecurity, and it is crucial to ensure our results are robust to it; it is thus included in our balance table and subsequent regressions. To estimate the price paid by each respondent, we first identified the specific utility that serves their street address.[46] We then used the annual residential revenue and sales from that utility, as reported in EIA Form 861,[47] to calculate the average residential price. The resulting price, while an estimate and not self-reported, is highly specific to the location of each respondent. In the event multiple utility territories served a respondent, we selected the utility with the highest customer count, as reported in Form 861; and for 8 respondents that were served by a utility that did not report sales and revenue in Form 861, we selected the utility that served the most customers in their county.

Design of survey instrument

In our survey instrument we asked respondents to answer questions regarding the incidence of energy insecurity, their demographics, and the economics of their solar system, if installed. The study and survey instrument were authorized under Institutional Review Board #15996 by Indiana University on September 8, 2022 and January 11, 2023. The survey was administered by the IU Survey Research Center. We obtained informed consent from all survey respondents.

The energy insecurity questions draw on questions typically asked in the Residential Energy Consumption Survey and the American Community Housing Survey.[1,48] We include six questions about household's experience of energy insecurity; each question began with, *"In roughly the past three months…":*

1. "…was there ever a time your household could not pay an [energy type] bill?"
2. "…did your household receive a disconnection notice, shutoff notice, or non-delivery notice due to an unpaid [energy type] bill?"
3. "…did your household get disconnected or lose service for [energy type]?"
4. "…how often have you had to reduce your energy consumption to save money on your energy bill?"
5. "…how often were you able to keep a comfortable temperature in your home (not too hot or too cold)?"
6. "…has your household had to reduce or forgo expenses for basic household necessities, such as medicine or food, in order to pay an energy bill?"

For questions 1-3, households answered these outcomes for each energy source used in the home (e.g., electricity, natural gas, heating oil). Three concern financial indicators of insecurity (inability to pay a bill, given notice of disconnection due to non-payment, or being disconnected), and three center on behavioral adaptations households can employ to reduce their energy expenditures. Questions 4-5 are worded to specifically to identify energy-limiting behavior,[3] rather than environmentally-motivated reductions in consumption. All questions ask households to report their experience over the prior three months. We selected three months in order to capture a sufficiently long period in which energy insecurity events may have occurred, but short enough that households could recall the specifics of their energy bills and household expenditures. The full survey instrument is available in the "Supplementary Notes" section of the Supplementary Information file.

After implementing the survey, we realized that the wording of the third question, regarding disconnections, may have been confusing to respondents. Based on the survey results, in which respondents reported being disconnected or losing service in states with active disconnection moratoria, such as California, we believe that the phrase "lose service" may have been confused with experiencing a power outage. Thus, we excluded this outcome from our analysis, in order to avoid misleading results, and encourage future research to test this variable with more specific phrasing.

Selection of survey sample

To select the sample to whom the survey was sent, we identified LMI households who adopted solar in 2021, assessed their demographic and property characteristics, identified similar households without solar, and then sampled from both pools of households until we obtained two groups that were balanced, on average. This goal—match non-solar to solar, based on average characteristics—formed the basis of our sample selection process, though the practical execution involved multiple steps. In this process, we prioritized the internal validity of our sample over its external validity. That is, we did not design our sample to be nationally representative of all 2021 LMI solar adopters, but rather sought two groups of households that were equivalent, on the mean, based on best available data. Supplementary Table 13 shows the geographic distribution of our sample versus all adopters predicted to be LMI in 2021. It illustrates that our sample under-represents LMI adopters in California and over-represents adopters in other states.

We define LMI as 110% of area median income (AMI). Income as a percent of AMI measures an individual's relative affluence; because solar adoption is weighted towards areas with higher absolute incomes, such as California, the relative threshold allows us to better identify LMI individuals with solar. We set the threshold at 110% because we found the sample of individuals meeting a lower cutoff, such as 80% AMI, to be too small for our anticipated survey response rate and required statistical power. We used the Solar Demographics Database maintained by Lawrence Berkeley National Lab to identify 105,681 single-family, residential households who adopted solar in 2021 and whose estimated income, from Experian, fell at or below 110% of AMI, based on HUD estimates from FY 2021.[49] We limited our sample to households who adopted in 2021, the last year of available data in Tracking the Sun[34] at the time of survey implementation, to ensure all households were exposed to solar for a similar amount of time and to increase the likelihood a solar household could recall their experience, prior to solar panels, which we use as a robustness check for pre-trends across groups. This group formed the pool from which we selected solar households.

We then identified a pool of possible non-solar households. This occurred in two steps, based on data availability. First, we identified the counties in which our LMI solar households were located. We used data from CoreLogic to predict the race of the title-owner of each single-family, residential address in those counties and excluded any with solar installed, based on the Tracking the Sun Database. We selected 133,334 non-solar households that matched the solar households in distribution across counties and had similar mean predicted race and housing characteristics. Second, we purchased predicted income information for these households from Experian. This gave us two pools of solar and non-solar households with complete data for characteristics on which we sought to balance.

We implemented a pilot survey via email in November and December 2022. From our two pools of solar and non-solar households, we identified 66,667 solar households and 66,667 non-solar households (133,334 total) that were balanced on mean predicted race, predicted income, geography (share in each state, limited to counties in which there are adopters), and housing characteristics (home age and square footage). We then purchased emails using Melissa. Melissa is a data service that identifies an email address for individuals, using physical addresses and other identifiable information. Using the quality of the Melissa and Experian data, we further limited the sample to those for whom we had high confidence that the household was owner-occupied and that the purchased email and estimated income were tied to the individual on title for the property. Finally, we sampled at random within each state to identify the 59,959 households to whom the survey was emailed. This represented our pilot sample.

We administered the final survey via mailing in January 2023. We identified a smaller sample of 25,000 households, randomly selected within-state from the 59,959 to whom the survey was emailed until we reached balance across arms. Responses were collected between January and March 2023. In Supplementary Figure 2, we show that the timing of responses was evenly distributed by treatment arm during these months. Our main results use only mail respondents, though we include sensitivity checks to include the pilot sample, finding that our results remain robust. A total of 3,190 individuals responded to both the pilot and final survey; of this, 2,860 were obtained through the mailer (90% of respondents, equivalent to a 11.4% response rate) and 330 (10% of respondents, equivalent to 0.01% response rate) were obtained through email.

Among respondents, we dropped households who self-reported that we had categorized them in the wrong treatment arm. Specifically, among all our respondents, 207 control households reported having installed solar, and 59 solar households reported that they did not have panels installed on their home. We kept households who did not self-report solar or non-solar status, respectively. This is a conservative choice. Based on those who did self-report, it is more likely that households we thought did not have solar adopted the technology, versus mis-identifying solar households. In theory, this type of error in the control arm would bias our coefficient estimates downward, such that our estimates understate the true effect of solar on energy insecurity. We include sensitivity checks to recategorizing these households into their self-reported treatment arm and found our results robust to this alternative coding. Finally, we excluded households for whom we only had respondents from one treatment arm within the state. In our main sample, this affected 16 households in Hawaii, Iowa, Michigan, Nebraska, and Maine. The resulting sample has a total of 2,608 households, with 1,639 in the treatment arm (79%) and 969 in the control arm (21%).

We note that our sample was selected from households whose estimated income was at or below 110% of AMI. In our final sample of respondents, some individuals' actual, self-reported income fell above this threshold. The range of self-reported income is 9-384% of AMI for the solar arm and 8-362% for the control arm; there are 698 solar and 327 non-solar households whose self-reported income is above 110% of AMI. Similarly, we selected our sample from addresses that were predicted to be owner-occupied. As a result, the majority of respondents are homeowners and a minority rent (Table 1). As shown in Supplementary Table 1, our results are not sensitive to the inclusion of renters.

Assessing balance among respondents

The first step of our analysis is to assess the balance across covariates of the solar and non-solar households that responded to the survey. (Recall that we matched the households to whom we mailed the survey, using estimated characteristics, but analyze the smaller group of respondents, using their self-reported, actual characteristics.) We calculate standardized mean differences using Cohen's *d* statistic,[50] defined as:

$$d = \frac{\overline{x_t} - \overline{x_c}}{s_p} \quad (1)$$

Here, x_t is the mean of covariate x in the treatment group, x_c is the mean in the control group, and s_p is the pooled standard deviation. The pooled standard deviation is calculated using the following formula, where n_t and n_c represent the number of observations in the treatment and control arm, respectively:

$$s_p = \sqrt{\frac{\sum(x_i - \underline{x})}{n_t + n_c - 2}} \quad (2)$$

Cohen's *d* can be interpreted simply as a standardized difference of means. The units of the statistic are standard deviations of the covariate; for example, a value of 0.5 indicates that the treatment arm is located 0.5 standard deviations away from the control arm. Cohen suggested that 0.2 be interpreted as a small difference, 0.5 as a medium difference, and 0.8 as a large difference. Within the matching literature, a common rule of thumb is that covariates should be no more than 0.25 standard deviations apart, in order for the sample to be sufficiently balanced.[23–25] While this is only a rule-of-thumb, and the assessment of sufficiently balanced remains subjective for a matching study, we include it as context for the interpretation of the results.

Finally, we note that the choice of Cohen's *d* is intentional and meant to align with best practice within the matching literature.[26] For example, historically, many matching papers assessed balance using a *t*-test for the equivalence between means across arms. As pointed out by Imai et al.,[25] this test can be problematic, in part because the *t*-statistic is affected by sample size and can be misleading in small samples. In contrast, Cohen's *d* measures only a property of the sample, not the population, and is not sensitive to sample size, thus meeting the criteria laid out by Imai et al.[23] for appropriate statistics.

The majority of covariates are self-reported, and our main balance table (Table 1) reports all data available for each variable. However, we find no meaningful difference in balance when limiting the sample to those respondents who answered all demographic questions, which we display in Supplementary Table 14. The individuals in this table—respondents who did not skip any demographic questions in the survey—represent the sample on which the models inclusive of covariates are estimated, as described below.

Balance across mean covariate values helps assess if the conditional independence assumption likely holds. Separate but related, the second identifying assumption in our design is that the covariate values of treated and control households lie within a common support. This assumption is also referred to as the overlap assumption.[41] We are able to provide evidence in support of this assumption by collapsing the vector of covariates into a single propensity score, across solar and non-solar households, shown in Supplementary Figure 3. The estimated propensity score is obtained from a logit model of solar on the full vector of demographic and household characteristics captured in the survey. The figure illustrates the joint distribution of covariate values across survey arms are similar.

Estimation of treatment effects

The next step in our analysis is to estimate the treatment effect of solar on energy insecurity. Each energy insecurity outcome we study is coded as a binary variable; as a result, the linear regressions can be interpreted as linear probability models. Within the matched sample, assuming the two arms are sufficiently balanced on observables, the effect of solar on energy insecurity can be assessed by taking the difference in mean outcomes between the treatment and control arms. This is equivalent to estimating the linear regression,

$$y_i = \beta_0 + \beta_1 Solar_i + \epsilon_i (3),$$

where *y* is a binary or categorical energy insecurity outcome of interest, *i* indexes an individual respondent, and *Solar* is a binary variable equal to 1 if individual *i* has solar installed on their house and 0 otherwise. Standard errors are estimated assuming that ϵ_i is clustered at the state-level. The coefficient of interest is β_1, interpreted as the effect of solar on households' experience of energy insecurity. Because it is a linear probability model, β_1 can be interpreted as the percentage point change in absolute likelihood that a solar household experiences the given energy insecurity outcome, relative to households without solar. Because we created a matched sample of control households to treatment households' characteristics, β_1 is interpreted as an estimate of the average treatment effect on the treated.

The causal interpretation for β_1 rests upon the conditional independence assumption, or that, conditional on all observable characteristics used to create the matched samples, treatment assignment is as-if random. A straightforward way to provide partial evidence that this assumption holds is to estimate the same model inclusive of covariates,

$$y_i = \beta_0 + \beta_1 Solar_i + X_i' \alpha + \epsilon_i (4),$$

where X_i represents the full vector of demographic and household characteristics captured in the survey. If the estimates of β_1 are similar in the models with and without covariates, it provides partial evidence that the coefficient is unbiased. The evidence is partial because we can only control for observed covariates; a characteristic excluded from X_i and correlated with both y and $Solar$ represents a possible source of bias for β_1. The inclusion of covariates to the model is a way to adjust parametrically, after matching, for any remaining differences between covariates, as long as differences are sufficiently small such that regression techniques are appropriate.[25]

One limitation of the specification above is that it implicitly makes a structural assumption of a linear relationship between the covariates and outcome. Given this, we further test the robustness of our treatment effect estimates to two alternate specifications that utilize the propensity score. The estimated propensity score is obtained from a logit model of solar on the full vector of demographic and household characteristics captured in the survey. First, we include the propensity score as a covariate, specified as $\hat{e}$, to allow for greater model flexibility, relative to the linear specification,

$$y_i = \beta_0 + \beta_1 Solar_i + \alpha \hat{e_i} + \epsilon_i (5)$$

Second, we run a weighted regression using the inverse of the propensity score. The weights are designed to estimate an ATT and re-balance the control arm to better match the empirical distribution in the treatment arm.[51] We define the weights as follows, such that all treated responds receive a weight of 1,

$$w_{ATT} = Solar_i + \frac{\hat{e_i}(1 - Solar_i)}{(1 - \hat{e_i})} \qquad (6)$$

Threats to Validity

The key limitation of the research design is that we do not directly observe solar households' experience of energy insecurity, prior to their adoption of the technology. As a result, one concern may be that solar households in our sample may have experienced less energy insecurity than control households before going solar, even after controlling for all observed covariates, and that this selection drives our treatment effect results. To address this concern, we asked survey respondents to recall energy insecurity events in the years prior to 2021, which we show in Supplementary Table 15, in lieu of actual data from the pre-period. Because all our solar households adopted in 2021, this period represents a baseline before which the solar arm was untreated or had not installed panels. The analysis shows no statistical difference between solar and non-solar households' recollection of experiences of energy insecurity prior to 2021. This further supports the comparability of the two groups and lessens the risk of bias due to lower levels of energy insecurity among treated households, prior to solar adoption.

Similarly, we might be concerned that solar households will be more likely to respond if they are satisfied with their solar installation, leading us to over-state the effect of solar on energy insecurity due to selection on high-performing systems. Anticipating this possible source of bias, we first note that our recruitment materials did not mention solar energy, and questions specific to households' installations appeared after those on energy insecurity within the survey. Both the survey instrument and recruitment documents are available in the Online Appendix. We believe this helps ensure that households in the treatment arm were not aware that the survey specifically involved solar until after reporting our core outcomes. Second, we examined the characteristics of those households who responded to the survey and those who did not, in order to assess if there is any visible evidence of selection among respondents. Because we do not observe actual demographics of non-respondents, we are limited to comparing the two groups on estimated characteristics. The results are presented in Supplementary Table 16. We find that respondents are similar on the whole to non-respondents, among the predicted characteristics available. Based on estimated characteristics, respondents were slightly more likely to have a higher predicted income, to live in a house with a higher estimated value, and to be predicted to be white.

Finally, we note that our methodology is only able to control for observed covariates. While we believe our set of characteristics considered is comprehensive, an unmeasured variable that affects both solar adoption and energy insecurity can be a source of omitted variable bias. This is not unique to our study and is a limitation of any matching design using cross-sectional data.

Data Availability

The data collected by the survey and analyzed in the study are available in the Dataverse repository, https://doi.org/10.7910/DVN/4HUD1Q. Any identifiable information has been removed from survey responses.

Code Availability

The code used to analyze the survey data is available in the Dataverse repository, https://doi.org/10.7910/DVN/4HUD1Q.

Acknowledgements: This work was funded by the U.S. Department of Energy's Office of Energy Efficiency and Renewable Energy (EERE) under Solar Energy Technologies Office (SETO) Agreement Number 38444 and Contract No. DE-AC02-05CH1123, received by G.B. The Energy Justice lab at Indiana University and the University of Pennsylvania was funded through a sub-contract with Lawrence Berkeley National Laboratory, received by S.C. and D.M. The authors also acknowledge that access to CoreLogic data was supported by a grant from the Racial Justice Research Fund at Indiana University, and that high performance computing resources were provided in part by Lilly Endowment, Inc., through its support for the Indiana University Pervasive Technology Institute.

Author Contributions: S.C., D.M.K., G.B. conceived of the project. M.Y. led the analysis and writing. G.B., S.C., S.F., D.M.K., T.M., C.C.M. and E.O. contributed to the analysis and writing.

Competing Interests: The authors declare no competiting interests.

Tables

Table 1: Comparison of mean covariates across survey arms

Characteristic	Solar (*n*=1,639)	Non-Solar (*n*=969)	Difference	Cohen's *d*
Demographics:				
Age	55.12	56.78	-1.66	0.10
Gender (%)				
Male	66.26	62.63	3.64	0.08
Female	33.04	36.48	-3.44	0.07
Non-binary/Other	0.70	0.90	-0.20	0.02
Household Size (#)	66.26	62.63	3.64	0.08
Annual Income				
<$25	5.87	7.2	-1.33	0.05
$25-35	8.19	11.66	-3.47	0.12
$35-50	11.48	13.37	-1.89	0.06
$50-75	19.99	21.49	-1.5	0.04
$75-100	18.50	16.69	1.82	0.05
$100-150	17.41	17.14	0.27	0.01
>$150	18.57	12.46	6.11	0.17
Income as % of AMI	111.74	100.92	10.83	0.17
Education (%)				
Less than HS	1.65	3.39	-1.73	0.12
HS or equiv.	12.41	12.99	-0.58	0.02
Some college	22.22	24.07	-1.85	0.04
Associate	11.46	13.67	-2.21	0.07
Bachelor	28.07	25.99	2.08	0.05
Graduate	24.19	19.89	4.30	0.10
Race (%)				
Hispanic	20.92	22.56	-1.64	0.02
White	77.28	76.98	0.30	0.01
Black	10.09	11.16	-1.08	0.04
Asian	8.51	7.67	0.84	0.02
American Ind.	3.27	2.44	0.83	0.05
Pacific Isl.	0.98	0.58	0.40	0.03
Other	6.29	6.74	-0.46	0.02
Housing:				
Home ownership (%)				
Own	95.87	92.94	2.93	0.13
Rent	1.97	4.04	-2.07	0.13
Other	2.16	3.03	-0.87	0.06
Type of dwelling (%)				
Trailer or mobile	0.63	0.90	-0.26	0.03
Apartment	0.70	0.56	0.14	0.02
Det. single family	83.06	76.97	6.09	0.16
Att. single family	14.34	18.76	-4.42	0.12
Condo	1.27	2.81	-1.54	0.12
Year Built	1975	1974	0.60	0.03
Square Ft	1,805	1,687	118.33	0.21
Avg. Elec. Price (¢/kWh)	16.82	16.73	0.09	0.01
State (%)				
AR	0.12	0.10	0.02	0.01
AZ	5.86	6.71	-0.85	0.04
CA	31.67	28.38	3.29	0.07

Characteristic	Solar (*n*=1,639)	Non-Solar (*n*=969)	Difference	Cohen's *d*
CO	5.13	4.44	0.69	0.03
CT	2.32	2.27	0.05	0.00
DC	1.28	0.83	0.46	0.04
FL	11.41	10.84	0.57	0.02
GA	0.85	1.96	-1.11	0.10
ID	0.31	0.72	-0.42	0.06
IL	1.16	1.44	-0.29	0.03
IN	0.55	0.62	-0.07	0.01
KS	0.18	0.62	-0.44	0.07
LA	0.12	0.10	0.02	0.01
MA	2.38	1.55	0.83	0.06
MD	1.10	1.14	-0.04	0.00
MN	1.77	1.44	0.32	0.03
MO	0.73	0.93	-0.20	0.02
MT	0.06	0.10	-0.04	0.02
NC	3.11	3.20	-0.09	0.01
NJ	2.01	2.06	-0.05	0.00
NM	0.92	1.55	-0.63	0.06
NV	3.84	4.64	-0.80	0.04
NY	2.14	2.58	-0.44	0.03
OH	0.67	1.14	-0.46	0.05
OK	0.12	0.31	-0.19	0.04
OR	1.65	1.24	0.41	0.03
PA	0.55	0.93	-0.38	0.05
RI	0.73	1.34	-0.61	0.06
SC	0.79	0.62	0.17	0.02
TN	0.12	0.10	0.02	0.01
TX	10.31	10.11	0.20	0.01
UT	1.65	1.14	0.51	0.04
VA	2.26	2.68	-0.43	0.03
WA	1.89	1.86	0.03	0.00
WI	0.24	0.31	-0.07	0.01

Notes: See Methods for the formula used to calculate Cohen's *d*. Difference is defined as solar minus non-solar; it is negative when there are fewer respondents in the solar arm than non-solar. Year built, square footage, and state are obtained from CoreLogic. For the income as a percentage of area mean income (AMI) variable, the midpoint of households' self-reported ranges and self-reported family sizes were used in the numerator; the U.S. Department of Housing and Urban Development (HUD) AMI values from FY 2021 were used for the denominator; and households who did not report household size or income and those whose family size is larger than the HUD maximum of 8, for which no AMI estimate was available, were dropped. The average residential electricity price is estimated for each respondent, based on the utility which serves their mailing address and the utility's total sales and revenue in 2022[47]; further details are in Methods. All other information is self-reported by survey respondents.

Table 2: Estimated effect of solar on energy insecurity outcomes

	(1) Unable to pay bill	(2) Received disconnection notice	(3) Reduce energy consumption to afford bill	(4) Forgo expenses	(5) Keep comfortable temperature
Intercept	0.133 ***	0.080 ***	0.741 ***	0.226 ***	0.331 ***
	(0.014)	(0.011)	(0.032)	(0.015)	(0.027)
Solar	-0.059 ***	-0.037 ***	-0.112 ***	-0.076 ***	-0.066 ***
	(0.011)	(0.010)	(0.020)	(0.012)	(0.012)
Percentage change	*-44%*	*-46%*	*-15%*	*-34%*	*-20%*
Num. obs.	2546	2546	2608	2608	2608
R^2	0.010	0.006	0.013	0.009	0.005

Notes: All coefficients above are estimated using a linear probability model. Models (1) and (2) are specific to electricity bills. Standard errors are clustered at the state-level. '***' denotes significance at the 0.1% level, '**' at the 1% level, '*' at the 5% level and '.' at the 10% level. "Percentage change" divides the coefficient estimate for solar by the control group mean (the intercept).

Table 3: Estimated effect of solar on energy insecurity outcomes including covariates

	(1) Unable to pay bill	(2) Received disconnection notice	(3) Reduce energy consumption to afford bill	(4) Forgo expenses	(5) Keep comfortable temperature
Solar	-0.054 ***	-0.039 **	-0.109 ***	-0.062 ***	-0.058 ***
	(0.014)	(0.012)	(0.020)	(0.015)	(0.013)
Controls for:					
Demographics	✓	✓	✓	✓	✓
Housing	✓	✓	✓	✓	✓
Num. obs.	1848	1848	1892	1892	1892
R^2	0.163	0.115	0.140	0.161	0.113

Notes: All coefficients above are estimated using a linear probability model. Models (1) and (2) are specific to electricity bills. Standard errors are clustered at the state-level. '***' denotes significance at the 0.1% level, '**' at the 1% level, '*' at the 5% level and '.' at the 10% level. Controls include all variables shown Table 1 excluding only income as a percentage of area mean income (AMI) and state of residence. The sample size is reduced relative to Table 3 due to non-response on self-reported covariates.

Table 4: Spillover effects of solar on households' ability to pay other types of energy costs

	(1) Electricity	(2) Natural Gas	(3) Fuel Oil	(4) Wood	(5) Propane	(6) Other
Intercept	0.133 ***	0.101 ***	0.093	0.066 *	0.098 **	0.000 ***
	(0.014)	(0.013)	(0.047)	(0.027)	(0.030)	(0.000)
Solar	-0.059 ***	-0.043 **	-0.057	-0.066 *	-0.069 *	0.019
	(0.011)	(0.015)	(0.060)	(0.027)	(0.030)	(0.013)
Num. obs.	2546	1708	98	197	258	166
R^2	0.010	0.006	0.014	0.046	0.022	0.001

Notes: All coefficients above are estimated using a linear probability model. Standard errors are clustered at the state-level. '***' denotes significance at the 0.1% level, '**' at the 1% level, '*' at the 5% level and '.' at the 10% level. Respondents were asked the question, "In roughly the past three months, was there ever a time your household could not pay an [energy type] bill?"; each column represents a different energy type.

Table 5: Estimated effect of solar on average electricity bill amounts ($/month)

	(1) Bill amount	(2) With Covariates
Intercept	183.265 ***	
	(5.976)	
Solar	-65.302 ***	-63.313 ***
	(8.202)	(7.202)
Controls for:		
Demographics		✓
Housing		✓
Num. obs.	2335	1812
R^2	0.095	0.213

Notes: Standard errors are clustered at the state-level. '***' denotes significance at the 0.1% level, '**' at the 1% level, '*' at the 5% level and '.' at the 10% level. Controls include all variables shown Table 1 excluding only income as a percentage of area mean income (AMI) and state of residence. Survey respondents answered the question, "How much are your average monthly bills for electricity?" and answers were provided in increments of $25.

Table 6: Estimated effect of solar on energy insecurity outcomes across ways in which the household pays for their solar panels

	(1) Unable to pay bill	(2) Received disconnection notice	(3) Reduce energy consumption to afford bill	(4) Forgo expenses	(5) Keep comfortable temperature
Solar – Monthly	-0.059 ***	-0.044 **	-0.089 ***	-0.065 ***	-0.072 ***
	(0.016)	(0.013)	(0.024)	(0.015)	(0.014)
Solar – Upfront	-0.069 ***	-0.042 ***	-0.194 ***	-0.102 ***	-0.061 **
	(0.013)	(0.009)	(0.029)	(0.014)	(0.018)
Solar – Unknown	-0.028	-0.021	-0.075 *	-0.015	-0.014
Payment Type	(0.017)	(0.018)	(0.031)	(0.030)	(0.023)
F-test of Monthly = Upfront:					
F value	0.931	0.058	14.936	5.388	0.622
P(>F)	0.335	0.809	0.000***	0.020*	0.431
Controls for:					
Demographics	✓	✓	✓	✓	✓
Housing	✓	✓	✓	✓	✓
Num. obs.	1848	1848	1892	1892	1892
R^2	0.164	0.115	0.145	0.164	0.114

Notes: All coefficients above are estimated using a linear probability model. Models (1) and (2) are specific to electricity bills. Standard errors are clustered at the state-level. '***' denotes significance at the 0.1% level, '**' at the 1% level, '*' at the 5% level and '.' at the 10% level. Controls include all variables shown Table 1 excluding only income as a percentage of area mean income (AMI) and state of residence. The sample size is reduced relative to Table 3 due to non-response on self-reported covariates. Households are categorized into "Unknown", "Monthly", and "Upfront" on the basis of two questions: (1) "How did you pay for the solar panels on your home?" and (2) "Did you take out a loan to help pay for your system?". Households are categorized as "Monthly" if they answered "I lease the solar panels" to (1) or if they answered "I bought the solar panels" to (1) and "Yes" to (2). Households are categorized as upfront if they answered "I bought the solar panels" to (1) and "No" to (2). Households are categorized as "Unknown" if they did not answer both questions, or if they answered, "I was gifted the solar panels", "I received the panels through an incentive program", or "When I moved into this house, it already had solar panels" in response to question (1), due to evidence households may have misunderstood the incentive program answer. The F-statistic has 1800 degrees of freedom for the restricted model, and the hypothesis test is one-sided.

Figures

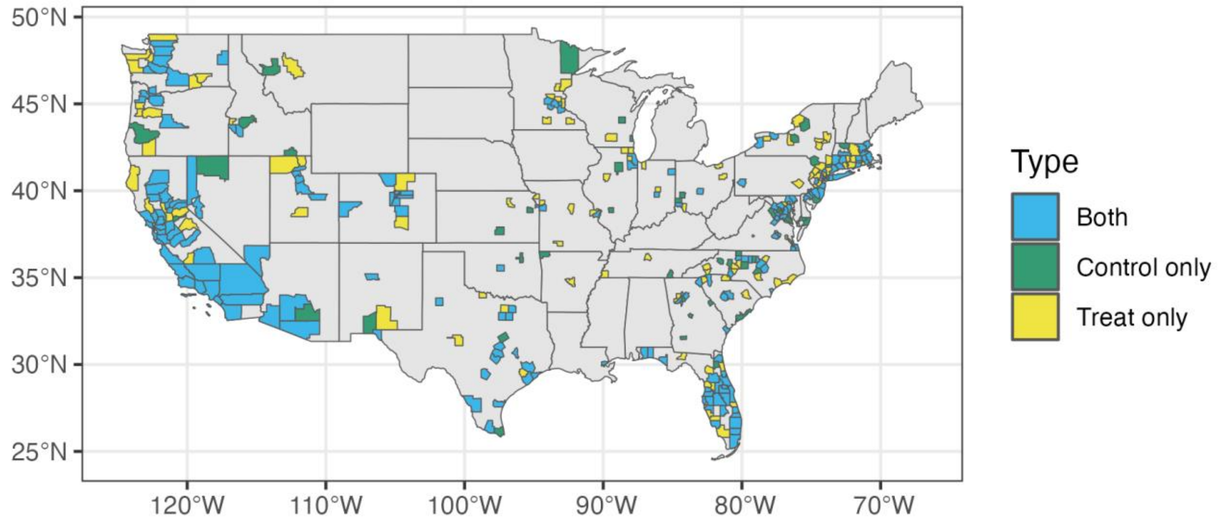

Figure 1: Geographic distribution of survey respondents in sample by county and treatment arm
Caption: “Both” indicates that there are both solar and non-solar survey respondents in the county. “Control only” indicates only non-solar households replied to the survey in that county, and “Treat only” indicates that only solar households responded in the county.

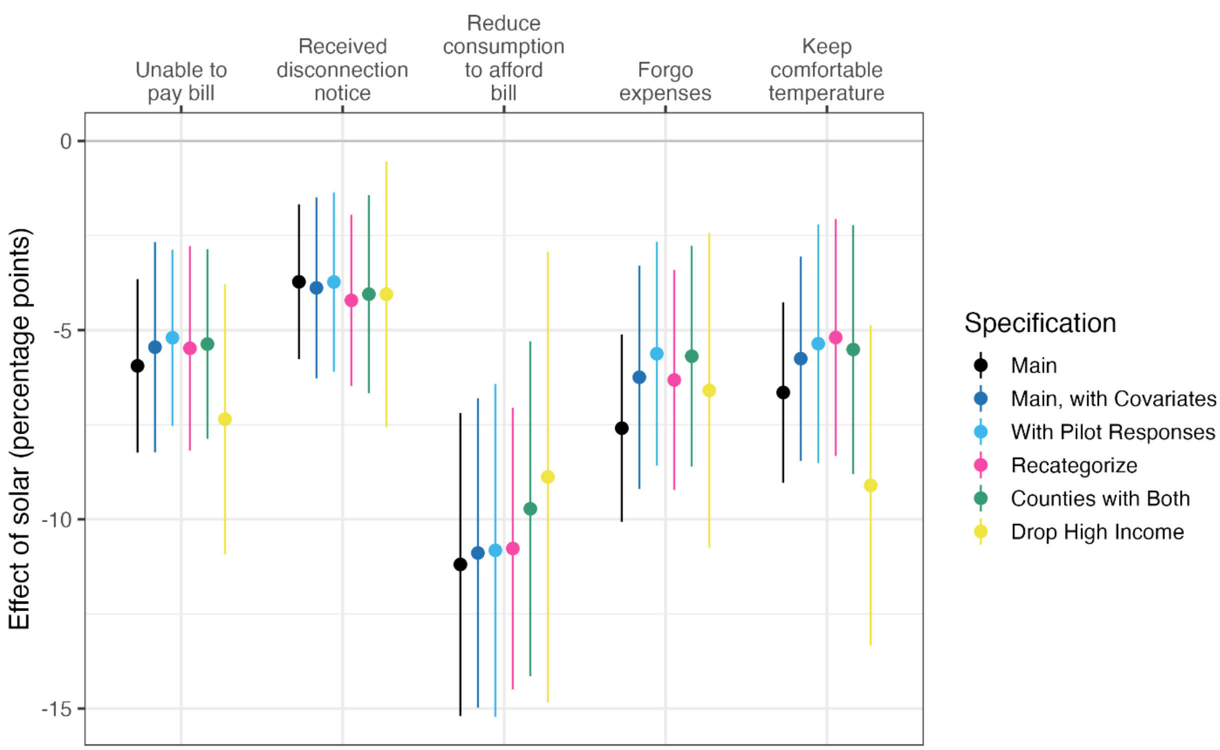

Figure 2: Estimated effect of solar on energy insecurity outcomes
Caption: Data are presented as the coefficient values (circle) +/- the 95% confidence interval (bars). Exact results for each specification are reported in Tables 2-3 and Supplementary Tables 5-8. The

outcomes “Unable to pay bill” and “Received disconnection notice” are specific to electricity bills. Standard errors are clustered at the state-level. All specifications but “Main” include covariates; these include all variables shown Table 1 excluding only income as a percentage of area mean income (AMI) and state of residence. Sample sizes are as follows. For the “Unable to pay bill” and “Received disconnection” outcomes, the “Main” sample contains *N*=2,546 observations; the “Main, with Covariates” sample contains *N*=1,848 observations; the “With Pilot Responses” sample contains *N*=2,049 observations; the “Recategorize” sample contains *N*=2,023 observations; the “Counties with Both” sample contains *N*=1,880 observations; and the “Drop High Income” contains *N*=1,027 observations. For the “Reduce consumption to afford bill”, “Forgo expenses”, and “Keep comfortable temperature” outcomes, the “Main” sample has *N*=2,608 observations; the “Main, with Covariates” sample has *N*=1,892 observations; the “With Pilot Responses” sample has *N*=2,097 observations; the “Recategorize” sample has *N*=2,068 observations; the “Counties with Both” sample has *N*=1,919 observations; and the “Drop High Income” has *N*=1,061 observations.

References

1. U.S. Energy Information Administration. 2020 RECS Survey Data: Consumption & Expenditures. (2021).
2. Carley, S., Graff, M., Konisky, D. M. & Memmott, T. Behavioral and financial coping strategies among energy-insecure households. *Proc. Natl. Acad. Sci.* **119**, e2205356119 (2022).
3. Cong, S., Nock, D., Qiu, Y. L. & Xing, B. Unveiling hidden energy poverty using the energy equity gap. *Nat. Commun.* **13**, 2456 (2022).
4. Huang, L., Nock, D., Cong, S. & Qiu, Y. (Lucy). Inequalities across cooling and heating in households: Energy equity gaps. *Energy Policy* **182**, 113748 (2023).
5. Bhattacharya, J., DeLeire, T., Haider, S. & Currie, J. Heat or Eat? Cold-Weather Shocks and Nutrition in Poor American Families. *Am. J. Public Health* **93**, 1149–1154 (2003).
6. Memmott, T., Carley, S., Graff, M. & Konisky, D. M. Sociodemographic disparities in energy insecurity among low-income households before and during the COVID-19 pandemic. *Nat. Energy* **6**, 186–193 (2021).
7. Brown, M. A., Soni, A., Lapsa, M. V., Southworth, K. & Cox, M. High energy burden and low-income energy affordability: conclusions from a literature review. *Prog. Energy* **2**, 042003 (2020).
8. Hernandez, D., Aratani, Y. & Jiang, Y. Energy Insecurity among Families with Children. (2014) doi:10.7916/D89G5JX8.
9. Konisky, D. M., Carley, S., Graff, M. & Memmott, T. The persistence of household energy insecurity during the COVID-19 pandemic. *Environ. Res. Lett.* **17**, 104017 (2022).
10. Murray, A. G. & Mills, B. F. The Impact of Low-Income Home Energy Assistance Program Participation on Household Energy Insecurity. *Contemp. Econ. Policy* **32**, 811–825 (2014).
11. Eisen, J. B. COVID-19 and Energy Justice: Utility Bill Relief in Virginia Annual Survey of Virginia Law. *Univ. Richmond Law Rev.* **57**, 155–196 (2022).
12. Baker, S. H., Carley, S. & Konisky, D. M. Energy insecurity and the urgent need for utility disconnection protections. *Energy Policy* **159**, 112663 (2021).
13. Flaherty, M., Carley, S. & Konisky, D. M. Electric utility disconnection policy and vulnerable populations. *Electr. J.* **33**, 106859 (2020).
14. Memmott, T., Carley, S., Graff, M. & Konisky, D. M. Utility disconnection protections and the incidence of energy insecurity in the United States. *iScience* **26**, (2023).
15. Tonn, B., Rose, E. & Hawkins, B. Evaluation of the U.S. department of energy’s weatherization assistance program: Impact results. *Energy Policy* **118**, 279–290 (2018).
16. Cook, J. J. & Shah, M. *Reducing Energy Burden with Solar: Colorado’s Strategy and Roadmap for States*. https://www.osti.gov/biblio/1431421 (2018) doi:10.2172/1431421.
17. Agdas, D. & Barooah, P. On the economics of rooftop solar PV adoption. *Energy Policy* **178**, 113611 (2023).
18. Crago, C. L., Grazier, E. & Breger, D. Income and racial disparities in financial returns from solar PV deployment. *Energy Econ.* **117**, 106409 (2023).
19. Bird, S. & Hernández, D. Policy options for the split incentive: Increasing energy efficiency for low-income renters. *Energy Policy* **48**, 506–514 (2012).
20. Riley, B. *et al.* Connected: rooftop solar, prepay and reducing energy insecurity in remote Australia. *Aust. Geogr.* **54**, 325–346 (2023).
21. Forrester, S. P., Montañés, C. C., O’Shaughnessy, E. & Barbose, G. Modeling the potential effects of rooftop solar on household energy burden in the United States. *Nat. Commun.* **15**, 4676 (2024).
22. Baker, E. *et al.* Metrics for Decision-Making in Energy Justice. *Annu. Rev. Environ. Resour.* **48**, 737–760 (2023).
23. Imai, K., King, G. & Stuart, E. A. Misunderstandings Between Experimentalists and Observationalists about Causal Inference. *J. R. Stat. Soc. Ser. A Stat. Soc.* **171**, 481–502 (2008).

24. Cochran, W. G. The Effectiveness of Adjustment by Subclassification in Removing Bias in Observational Studies. *Biometrics* **24**, 295–313 (1968).
25. Ho, D. E., Imai, K., King, G. & Stuart, E. A. Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference. *Polit. Anal.* **15**, 199–236 (2007).
26. Stuart, E. A. Matching Methods for Causal Inference: A Review and a Look Forward. *Stat. Sci.* **25**, 1–21 (2010).
27. Qiu, Y., Kahn, M. E. & Xing, B. Quantifying the rebound effects of residential solar panel adoption. *J. Environ. Econ. Manag.* **96**, 310–341 (2019).
28. Deng, G. & Newton, P. Assessing the impact of solar PV on domestic electricity consumption: Exploring the prospect of rebound effects. *Energy Policy* **110**, 313–324 (2017).
29. Directory of State Low- and Moderate-Income Clean Energy Programs. *Clean Energy States Alliance* https://www.cesa.org/resource-library/resource/directory-of-state-low-and-moderate-clean-energy-programs/.
30. Proudlove, A. *et al. The 50 States of Solar: Q1 2024 Quaterly Report*. https://nccleantech.ncsu.edu/wp-content/uploads/2024/04/Q1-24_SolarExecSummary_Final.pdf/ (2024).
31. Savarani, S. & Spiegel-Feld, D. Equitable Electrification: Could City and State Policies Aggravate Energy Insecurity? *Environ. Law Report.* **52**, 10831–10846 (2022).
32. Do, V. *et al.* Spatiotemporal distribution of power outages with climate events and social vulnerability in the USA. *Nat. Commun.* **14**, 2470 (2023).
33. Forrester, S., Barbose, G., O'Shaughnessy, E., Darghouth, N. & Montañés, C. C. *Residential Solar-Adopter Income and Demographic Trends: 2023 Update*. https://emp.lbl.gov/publications/residential-solar-adopter-income-2 (2023).
34. Barbose, G., Darghouth, N., O'Shaughnessy, E. & Forrester, S. *Tracking the Sun: Pricing and Design Trends for Distributed Phovoltaic Systems in the United States (2024 Edition)*. trackingthesun.lbl.gov (2024).
35. Sigrin, B. & Mooney, M. Rooftop Solar Technical Potential for Low-to-Moderate Income Households in the United States. (2018) doi:10.2172/1434891.
36. O'Shaughnessy, E., Barbose, G., Kannan, S. & Sumner, J. Evaluating community solar as a measure to promote equitable clean energy access. *Nat. Energy* **9**, 955–963 (2024).
37. Borenstein, S. Private Net Benefits of Residential Solar PV: The Role of Electricity Tariffs, Tax Incentives, and Rebates. *J. Assoc. Environ. Resour. Econ.* **4**, S85–S122 (2017).
38. Lukanov, B. R. & Krieger, E. M. Distributed solar and environmental justice: Exploring the demographic and socio-economic trends of residential PV adoption in California. *Energy Policy* **134**, 110935 (2019).
39. O'Shaughnessy, E., Barbose, G., Wiser, R. & Forrester, S. Income-targeted marketing as a supply-side barrier to low-income solar adoption. *iScience* **24**, 103137 (2021).
40. Angrist, J. & Pischke, J.-S. *Mostly Harmelss Econometrics: An Empiricist's Companion*. (Princeton University Press, Princeton, New Jersey, 2009).
41. Imbens, G. W. Nonparametric Estimation of Average Treatment Effects Under Exogeneity: A Review. *Rev. Econ. Stat.* **86**, 4–29 (2004).
42. Stuart, E. & Rubin, D. Matching methods for causal inference: Designing observational studies. (2007).
43. Cunningham, S. *Causal Inference: The Mixtape*. (Yale University Press, New Haven, CT, 2021).
44. Rubin, D. B. Using Propensity Scores to Help Design Observational Studies: Application to the Tobacco Litigation. *Health Serv. Outcomes Res. Methodol.* **2**, 169–188 (2001).
45. Imai, K. & Khanna, K. Improving Ecological Inference by Predicting Individual Ethnicity from Voter Registration Records. *Polit. Anal.* **24**, 263–272 (2016).

46. Oak Ridge National Laboratory. Electric Retail Service Territories. Homeland Infrastructure Foundation Level Database (2023).
47. U.S. Energy Information Administration. Annual Electric Power Industry Report, Form EIA-861 detailed data files. (2022).
48. U.S. Census Bureau. American Community Survey. (2023).
49. U.S. Department of Housing and Urban Development. Methodology for Determining FY 2021 Section 8 Income Limits. (2021).
50. Cohen, J. *Statistical Power Analysis for the Behavioral Sciences*. (Routledge, New York, 1988). doi:10.4324/9780203771587.
51. Austin, P. C. & Stuart, E. A. Moving towards best practice when using inverse probability of treatment weighting (IPTW) using the propensity score to estimate causal treatment effects in observational studies. *Stat. Med.* **34**, 3661–3679 (2015).