Social Learning and Solar Photovoltaic Adoption: Evidence from a Field Experiment*

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Abstract

A growing literature points to the importance of social interactions and nudges in influencing economic outcomes. This study investigates a large-scale behavioral intervention designed to actively leverage social learning and peer interactions to encourage adoption of residential solar photovoltaic systems. Municipalities receive a municipality-chosen solar installer, group pricing, and an informational campaign driven by volunteer ambassadors. We find a treatment effect of 33 installations per municipality, an increase of over 100 percent, and no evidence of harvesting or persistence. The intervention also lowers installation prices. Additional randomized controlled trials show the importance of selection into the program and the lack of importance of group pricing. Our results suggest that this program may improve social welfare through economies of scale and lowered consumer acquisition costs.

Keywords: non-price interventions; social learning; renewable energy; solar photovoltaic panels; technology adoption; natural field experiment
JEL classification codes: D03, L22, Q42, Q48

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1 Introduction

Economists have become increasingly interested in behavioral interventions or "nudges" that encourage actions that are privately or socially beneficial. Such interventions often involve information provision that includes social comparisons or pro-social appeals. This is especially true for the voluntary provision of public goods, such as environmental public goods (Allcott and Rogers, 2015; Ferraro and Price, 2013). At the same time, there is mounting evidence that social interactions themselves, through social learning and peer effects, influence the adoption of environmentally-friendly technologies (e.g., Bollinger and Gillingham, 2012).

This study uses a natural field experiment to examine a rapidly expanding behavioral intervention in the United States designed to leverage the power of social interactions to promote a fast-growing renewable energy technology: solar photovoltaic (PV) installations. The "Solarize" program is a community-level behavioral intervention with several key pillars. Treated municipalities who receive the intervention choose a single solar PV installer. In order to become this chosen installer, installers submit bids with a discount group price that is offered to all consumers in that municipality during the program. The intervention begins with a kick-off event and involves roughly 20 weeks of community outreach. Notably, the primary outreach is performed by volunteer resident "solar ambassadors" who encourage their neighbors and other community members to adopt solar PV, effectively providing a major nudge towards adoption. This social interaction-based approach parallels previous efforts to use ambassadors as "injection points" into the social network to promote adoption of agricultural technology (**?**Vasilaky and Leonard, 2011) and behavior conducive to improving public health (Kremer, Miguel, Mullainathan, Null and Zwane, 2011; Ashraf, Bandiera and Jack, 2015) in developing countries.

In this paper, we ask several questions that shed light on consumer and market behavior under the influence of a large-scale behavioral intervention. Is such a program effective at increasing adoption of solar PV and lowering installation prices? Do these effects persist after the intervention? Are there spillovers or positive treatment externalities to nearby communities (Miguel and Kremer, 2004)? How cost-effective is the program for meeting policy goals, and is it welfare-improving? These research questions have important policy relevance, for Solarize or similar interventions are currently being implemented in many states, and many others have expressed interest in the program to help meet climate and energy goals.[1] In fact, there is even a well-publicized program guidebook for policymakers interested in implementing a Solarize program (Hausman and Condee, 2014).

We establish the effectiveness of the Solarize program using two randomized controlled trials (RCTs) and a set of trials with rolling control groups, as in Sianesi (2004) and Harding and Hsiaw (2014). We first examine municipalities that apply to join the program, for these are the marginal municipalities that would first join if the program is expanded elsewhere. Using a difference-in-difference strategy, these municipalities are compared to a randomly-drawn control group and a larger control group of towns that applied to join the program later. For the treated municipalities, we find that the treatment leads to 27 additional installations over the course of a campaign on average, an increase of over 100 percent from the counterfactual. Post-treatment, we find no evidence of either a harvesting effect (e.g., as occurred with the well-known Cash for Clunkers program (Mian and Sufi, 2012)) or further induced installations, which might be possible due to continued social learning or peer effects.

We find that the program lowers the equilibrium price during the Solarize campaigns by roughly \$0.64 per watt (W) out of a mean price of roughly \$4.61/W in the control municipalities.[2] Moreover, our results suggest the presence of treatment externalities that lower the equilibrium price in neighboring Census block groups by \$0.15/W, but only increase installations by 1 to 2 installations per municipality. In fact, our treatment effect results do not change when including the price as a covariate, indicating that the discount pricing in the treatment is only a small part of the explanation for the effect.

[1] Implementing states include Oregon, Washington, California, Colorado, South Carolina, North Carolina, Ohio, Pennsylvania, New York, Rhode Island, Massachusetts, and Vermont.

[2] All dollars in this paper are 2014\$.

Our second RCT involves randomly selected municipalities across CT, rather than municipalities that applied to participate in the program. Nearly all of the municipalities we approached agreed to join the program. The estimated treatment effect is roughly half of the treatment effect estimated for the municipalities that opted-in, both in terms of installations and prices. This finding provides guidance for policymakers who would consider scaling up such a program beyond the municipalities that self-select by applying.

We examine the mechanisms underlying the effectiveness of the treatment in two ways. First, we examine an installer-led program, called "CT Solar Challenge," (CTSC) that included all of the central tenets of the Solarize program except the involvement of state government and the competitive bidding process. This allows us to test the hypothesis that competitive bidding is necessary for the effectiveness of the campaign. We estimate a small treatment effect of CTSC leading to more installations, but no effect on prices in the first six months, and only an effect afterwards when it was clear that CTSC was not bringing in many new leads. This result provides insight into the age-old question in economics of how the institutional structure of markets influences pricing and other market outcomes. Second, we survey participants in the Solarize program, and find that measures related to social influence, such as "speaking with friends and neighbors" or "interactions with the social ambassador," are rated as extremely important factors in the decision to install solar. This, combined with the finding that including price does not change the estimated treatment effect on installations, provides suggestive evidence that the Solarize behavioral intervention works primarily by *leveraging* social interactions.

Our results have clear policy implications. Behavioral interventions based on information, word-of-mouth, persuasion, and other non-price approaches have become increasingly popular for encouraging prosocial activities, and community-based interventions are perhaps the latest vanguard of this movement among practitioners (McKenzie-Mohr, 2013). With billions of dollars spent each year by electric and natural gas utilities on energy conservation (Gillingham and Palmer, 2014) and billions more by federal and state governments on promoting adoption of solar PV (?), evaluating the effectiveness, persistence, and cost-effectiveness of these rapidly expanding community-based programs is important for policy development.

In our setting, we find that each additional installation due to the program costs roughly \$900 in funding, which can be compared to typical estimates of installers' customer acquisition costs of \$0.48/W (Friedman, Ardani, Feldman, Citron, Margolis and Zuboy, 2013), amounting to \$1,500 to \$3,000 for an installation, or an average consumer savings of \$4,627 from the program. The cost-effectiveness per ton of CO_2 reduced depends on assumptions about the future carbon intensity on the New England electric grid. Assuming the 2012 CT carbon intensity from EIA (2014) remains constant into the future, this implies a cost-effectiveness estimate of \$32 per ton of CO_2 avoided based only on the direct costs of the intervention. If the CT carbon intensity decreases rapidly with increased natural gas use or if other subsidies are included, this estimate may be significantly higher. From an economic efficiency standpoint, we deem it quite likely that by acting as a "nudge" to encourage prosocial behavior, the Solarize programs increase social welfare.

This paper is organized as follows. Section 2 describes the empirical setting, our hypotheses, and our randomization. Section 3 presents our dataset and descriptive summary statistics, while section 4 describes our estimation strategy. Section 5 presents the results and section 6 the cost-effectiveness calculations. Section 7 concludes with a discussion of implications for policy.

2 Research Design

This paper leverages a unique experimental setting to test several hypotheses about the rapidly-expanding Solarize behavioral intervention. Before moving to these hypotheses, it is useful to first provide some background on solar PV in CT and the Solarize program itself.

2.1 Empirical Setting

CT has a small, but fast-growing market for solar PV, which has expanded from only three installations in 2004 to nearly 5,000 installations in 2014. Despite this, the cumulative number of installations remains a very small fraction of the potential; nowhere in CT is it more than 5 percent of the potential market and in most municipalities it is less than 1 percent.[3] The pre-incentive price of a solar PV system has also dropped substantially in the past decade, from an average of $8.39/W in 2005 to an average of $4.44/W in 2014 (Graziano and Gillingham, 2015).

Despite being in the Northeastern United States, the economics of solar PV in CT are surprisingly good. While CT does not have as much sun as other regions, it has some of the highest electricity prices in the United States. Moreover, solar PV systems in CT are eligible for state rebates, federal tax credits, and net metering.[4] For a typical 4.23 kW system in 2014, we calculate that a system purchased with cash in southern CT would cost just under $10,000 after accounting for state and federal subsidies and would have a internal rate of return of roughly 7 percent for a system that lasts the expected lifetime of 25 years (See Appendix Appendix A for more details on this calculation and some sensitivity analysis).

Thus, solar PV systems display the properties of a classic new technology in the early stages of the process of diffusion (e.g., Griliches, 1957). From a private consumer perspective, solar PV systems are very often an ex ante profitable investment. This is important in the context of this study, for it indicates that Solarize campaigns are nudging consumers towards generally profitable investments. There of course will be heterogeneity in the suitability of dwellings for solar PV and we are careful to focus on the potential market based on satellite imaging from Geostellar (2013). We will discuss the social welfare implications in Section 6.

[3] Estimates based on authors' calculations from solar installation data and potential market data based on satellite imaging from Geostellar (2013). The potential market data is focused on the shading of households, but accounts for the possibility of some ground-mounted systems. Ground-mounted systems are more expensive and they make up only 2 percent of the systems.

[4] Net metering allows excess solar PV production to be sold back to the electric grid at retail rates, with a calculation of the net electricity use occurring at the end of each month. Any excess credits remaining on March 31 of each year receive a lower rate.

During the time period of this study, the CT solar market had 89 installers, ranging in size from small local companies to large national installers. The state rebates, disbursed by the CGB, began in 2006 at \$5.90 per W and declined to \$1.75 per W by the end of 2014. The incentives were held constant during the time periods covered by the treatments in this study. The CT solar market has been slow to adopt third party-ownership (e.g., solar leases or power purchase agreements) and most systems have been purchased outright.[5] Regardless of ownership, the state rebates are nearly always taken by the installer and passed on to consumers.[6]

2.2 The Solarize Program

The Solarize program in CT is a behavioral intervention with several components, each motivated by theory. At its core, the program focuses on facilitating social learning and peer influence. Peer influence has been demonstrated to speed the adoption of many new technologies and behaviors, including agricultural technologies (Foster and Rosenzweig, 1995; Conley and Udry, 2010), criminal behavior (Glaeser, Sacerdote and Scheinkman, 1996; Bayer, Pintoff and Pozen, 2009), health plan choice (Sorensen, 2006), retirement plan choice (Duflo and Saez, 2003), high student performance (Sacerdote, 2001; Duflo, Dupas and Kremer, 2011), foreclosure choices (Towe and Lawley, 2013), contraceptive adoption (Munshi and Myaux, 2006), and even welfare participation (Bertrand, Luttmer and Mullainathan, 2000). Bollinger and Gillingham (2012) and Graziano and Gillingham (2015) find evidence of neighbor or peer influence on the adoption of solar PV technology in California (CA) and CT respectively.

The first critical component to the Solarize program is the *use of volunteer promoters or ambassadors* to provide information to their community about solar PV. There is growing evidence on the effectiveness of promoters or ambassadors in driving social learning

[5]As of 2014, roughly 37 percent of all systems installed were third party-owned, and these third party-owned systems were distributed across CT and not concentrated in any particular municipalities.

[6]Gillingham and Tsvetanov (2016) estimate the pass-through of state rebates in the CT solar market and find that only roughly 16 percent of the rebates are captured by firms.

and influencing behavior (**?**Vasilaky and Leonard, 2011; Kremer *et al.*, 2011; Ashraf *et al.*, 2015). Why might volunteer community members be effective in Solarize? There is a robust economic literature on the importance of trust and trustworthiness in influencing economic outcomes by reducing transactions costs and building social capital (Arrow, 1972; Knack and Keefer, 1997; Fehr and List, 2004; Karlan, 2005). Furthermore, there is evidence that trust is enhanced by social connectedness (e.g., Glaeser, Laibson, Scheinkman and Soutter, 2000; List and Price, 2009). Since the Solarize campaigns are based at the community-level, social connectedness is more likely to be high. Moreover, since the ambassadors are volunteers, they may be more likely to be seen as more trustworthy by other community members.

The second major component to the Solarize program is the *focus on community-based recruitment*. In Solarize, this consists of mailings signed by the ambassadors, open houses to provide information about panels, tabling at events, banners over key roads, op-eds in the local newspaper, and even individual phone calls to neighbors who have expressed interest by the ambassadors. Jacobsen, Kotchen and Clendenning (2013) use non-experimental data to show that a community-based recruitment campaign can increase the uptake of green electricity using some (but not all) of these approaches. Kessler (2014) shows that public announcements of support can increase public good provision, which perhaps may apply to the ambassadors in this setting.

The third major component is the *group pricing discount* offered to the entire community based on the number of contracts signed. This provides an incentive for early adopters to convince others to adopt and to let everyone know how many people in the community have adopted. In this sense, it is intended to build social pressure and create a social norm around solar PV in the community. There is strong evidence from consumer decisions about charitable contributions that indicates consumers are more willing to contribute when others contribute (Frey and Meier, 2004; Karlan and List, 2007; DellaVigna, List and Malmendier, 2012). Moreover, there is building evidence demonstrating the effectiveness of social norm-based informational interventions to encourage electricity or water conservation (Allcott, 2011; Allcott and Rogers, 2015; Ferraro, Miranda and Price, 2011; Ferraro and Price, 2013; LaRiviere, Price, Holladay and Novgorodsky, 2014). The choice to install solar PV is a much higher-stakes decision than to contribute to a charity or conserve a bit on electricity or water, so it is not obvious that effects seen in lower-stakes decisions apply. However, Coffman, Featherstone and Kessler (2014) show that provision of social information can have an important impact even on high-stakes decisions such as undertaking teacher training and accepting a teaching job.

The fourth major component is the *limited time frame* for the campaign. Such a limited time frame may provide a motivational reward effect (Duflo and Saez, 2003) for the price discount would be expected to be unavailable after the campaign. Recent reviews (Gneezy, Meier and Rey-Biel, 2011; Bowles and Polania-Reyes, 2012) suggest that monetary incentives can be substitutes for prosocial behavior, but by providing a prosocial reward that helps all, it is quite possible that the two are complements in this situation.

Thus, the program is designed as a package that draws upon previous evidence on the effectiveness of social norm-based information provision, the use of ambassadors to provide information, social pressure, prosocial appeals, goal setting, and motivational reward effects for encouraging prosocial behavior.

Facilitating the Solarize program in CT is a joint effort between the a state agency, Connecticut Green Bank (CGB), and a non-profit marketing firm, Smartpower.[7] A standard timeline for the program is as follows:

1. CGB and Smartpower inform municipalities about the program and encourage town leaders to submit an application to take part in the program.
2. CGB and Smartpower select municipalities from those that apply by the deadline.
3. Municipalities issue a request for group discount bids from solar PV installers for each municipality.
4. Municipalities choose a single installer, with guidance from CGB and Smartpower.

[7]The programs were funded by the CGB, The John Merck Fund, The Putnam Foundation, and a grant from the U.S. Department of Energy.

5. CGB and Smartpower recruit volunteer "solar ambassadors."
6. A kickoff event begins a 20-week campaign featuring workshops, open-houses, local events, etc. coordinated by Smartpower, CGB, the installer, and ambassadors.
7. Consumers that request them receive site visits and if the site is viable, the consumer may choose to install solar PV.
8. After the campaign is over, the installations occur.

Randomization occurs in this study in stage 2. The treatments are staggered into four rounds for logistical reasons. Each round has several municipalities included. In the first round, nine municipalities submitted applications and only four were selected, allowing for a randomization at the application level (Step 2 in the timeline above). In the subsequent rounds, there was never more than one or two additional applicants, so randomization was not possible.

We also ran a second field experiment where the municipalities were randomly selected from a list of all non-Solarize municipalities in CT. Smartpower then approached these selected municipalities and were able to convince all but one to apply to take part in the program. Everything else about the program was identical to the other chosen towns.

Finally, a for-profit installer, Aegis Solar, created and funded the non-profit CTSC to contact municipalities and encourage them to participate in a very similar campaign. Three municipalities agreed to participate in the initial set of CTSC campaigns that began during a similar time frame as the second round of Solarize. These CTSC campaigns are modeled after Solarize, as Aegis Solar took part in the first round of Solarize and thus was very familiar with the program. One very important difference is that there was no competitive bidding process (stages 3 and 4 were removed). A second difference is that CGB and Smartpower were not involved.

Table 12 lists the timeline of the treatment campaigns in this study. Figure 1 provides a map of the 169 municipalities in CT, illustrating the 34 treated municipalities in this study.[8]

[8]Some contiguous municipalities are run as joint campaigns, such as Mansfield and Windham in order to

2.3 Hypotheses

Based on the previous literature and the design of the study, we had five hypotheses about the Solarize program:

1. The intervention will substantially increase installations due to the combination of program features designed based on theories in the literature.
2. The intervention will lead to lower prices due to the group pricing discount, which is made possible by economies of scale and lower consumer acquisition costs.
3. The intervention will lead to treatment externalities to adjacent municipalities through word-of-mouth, which would lower prices and increase installations.
4. The intervention will be more effective at increasing installations in municipalities that select into the program, but will also increase installations and lower prices in randomly selected municipalities that agree to join the program.
5. Without competitive bidding, and with an institutional structure that may not be as conducive to trust (Bohnet and Huck, 2004), the CTSC will not lead to lower prices and will be less effective.

3 Data

3.1 Data Sources

The primary data source for this study is the database of all solar PV installations that received a rebate from the CGB, 2004-2015. When a contract is signed to perform an installation in CT, the installer submits all of the details about the installation to CGB in order for the rebate to be processed. As the rebate has been substantial over the the past decade, we are confident that nearly all, if not all, solar PV installations in CT are included

reduce costs. However, both municipalities still receive the full treatment.

in the database.[9] For each installation the dataset contains the address of the installation, the date the contract was approved by CGB, the date the installation was completed, the size of the installation, the pre-incentive price, the incentive paid, whether the installation is third party-owned (e.g., solar lease or power-purchase agreement), and additional system characteristics.

The secondary data source for this study is the U.S. Census Bureau's 2009-2013 American Community Survey, which includes demographic data at the municipality level. Further, we include voter registration data at the municipality level from the CT Secretary of State (SOTS). These data include the number of active and inactive registered voters in each political party, as well as total voter registration (CT SOTS, 2015).[10] Finally, we use data on the potential market for solar PV in CT based on a satellite imaging analysis that removes multi-family dwellings and highly shaded dwellings (Geostellar, 2013). These estimates are made at the county-level, so we create a municipality-level estimates by using the fraction of owner-occupied dwellings in each municipality in a county from the Census.

3.2 Summary Statistics

We convert our dataset to the municipality-month level, calculating the count of newly approved installations in each municipality-month. This conversion is performed for two reasons. First, the treatment itself is at the municipality-level, so the conversion facilitates easy interpretation of the coefficients. Second, the contract approval date is usually within a few weeks of the contract signing date. Data at a higher level of temporal disaggregation is very likely to contain significant measurement error, but we expect that this is less of an issue with monthly-level data. For each municipality-month, we also calculate the remaining potential market size for solar PV, which is the Geostellar (2013) potential market size minus the cumulative installations up to that month.

[9] The only exception would be in three small municipal utility regions: Wallingford, Norwich, and Bozrah. We expect that there are few installations in these areas.

[10] Unfortunately, we cannot separate out the green party from other minor parties such as the libertarian party, so we simply focus on the Republican and Democratic parties.

For many municipality-months in CT, there are no installations, so the average price is missing. This is a common issue in empirical economics with several possible solutions. We take a simple approach: we impute the missing prices based on the average price in the county in that month, and if that is not available, the average price in the state in that month. Since this may underestimate the actual prices offered, but not taken, by consumers in these municipalities, we also perform a robustness check that imputes the price with the highest price in the county or state. We find little difference in our results, likely due to the fact that during the time periods we are most interested in, when the Solarize programs were implemented, a high percentage of municipalities had at least one installation per month.

Table 2 shows summary statistics for key variables in the data. The number of installations is always much below the potential market size. The pre-incentive price is \$6.79/W on average, although as mentioned above, it drops to closer to \$4.50/W on average by 2014. For comparison, the average CT rebate in the data is just over \$2/W on average, but also drops by 2014 to under \$1/W on average by 2014.

3.3 Descriptive Evidence on Solarize

Figures 2 and 3 provide descriptive evidence of the remarkable effect of the classic Solarize program described above on the cumulative number of installations over time. As is clear from the figures there is a slow growth in installations over the decade prior to Solarize, and then an extremely rapid growth during the program. After the program, some municipalities continued growing faster than they had before, while others seemed to return to growth rates similar to those prior to the campaigns.

Figures 2 and 3 examine the stock of installations, while Figure 4 illustrates the flow of installations per month by the round of the program. In addition to plotting the average installations over time in the municipalities in each of the four rounds, it also plots the average number of installations per municipality in non-Solarize Connecticut Clean Energy Communities (CEC). Municipalities in CT can choose to be designated a Clean Energy

Community by setting up an energy task force that promotes renewable energy or energy efficiency in the municipality (e.g., see Jacobsen *et al.*, 2013). These municipalities are the first target for recruitment of Solarize municipalities and tend to be similar to Solarize municipalities both in terms of observable demographics and interest in solar PV.

One striking feature in Figure 4 is the similarity between the average number of installations per municipality in the CEC sample and the Solarize municipalities prior to the program, and the CT statewide average, which is also plotted. Then during each program, there is dramatic growth in the number of installations.

Figure 5 shows monthly average solar PV prices over time in the same groups as the previous figure. Prior to the program, it is difficult to discern any differences in prices between the groups. However, during each campaign, the effect of the discount pricing is visible. For example, prices are noticeably lower for Round 1 Solarize municipalities during Round 1. The same is true for Round 2, but slightly less noticeably in the final two rounds. Note that while there is group pricing, there is some variation in final pre-incentive prices due to allowed cost adders for difficult roof configurations or more expensive panels than standard.

4 Estimation Strategy

4.1 A Simple Model of Solar PV Adoption

Consider consumer i considering purchasing a solar PV system in municipality m at time t. Let the indirect utility for this purchase be given by

$$u_{imt} = \beta T_{mt} - \eta p_{mt} + \mu_m + \delta_t + \xi_{mt} + \epsilon_{imt},$$

where T_{mt} is the Solarize treatment (i.e., treated municipality interacted with the treatment period), p_{mt} is the post-incentive price of the solar PV system (i.e., inclusive of the Solarize group discount and state rebate), and μ_m and δ_t are individual effects for municipality and time. μ_m and δ_t can be represented by dummy variables; μ_m captures municipality-level unobservables, such as demographics and environmental preferences. These municipality-level unobservables are assumed to be time-invariant over the relatively few years covered by our sample. δ_t is a vector of two dummy variables, for both the pre-treatment period and the treatment period.

Under the assumption that ϵ_{imt} is an i.i.d type I extreme value error, we have the following model at the municipality market level:

$$\ln(s_{mt}) - \ln(s^0_{mt}) = \beta T_{mt} - \eta p_{mt} + \mu_m + \delta_t + \xi_{mt}, \tag{1}$$

where s_{mt} is the market share of solar PV,[11] and s^0_{mt} is the share of the outside option (i.e., not installing solar PV). Note that $\ln(s_{mt}) - \ln(s^0_{mt})$ is the log odds-ratio of the market share in a municipality. β is the coefficient of interest, and in an RCT setting, can be interpreted as the average treatment effect (ATE).

This approach models the treatment as changing the utility from installing a solar PV system. For example, a positive coefficient can be viewed as the utility gain from information acquisition about solar PV. Of course, it may also be due to knowledge that other community members will see the installation, a "warm glow" from contributing to the community program, or even additional utility from "getting a good deal" through the program. We will discuss these mechanisms in section 5.

Since price is endogenous due to simultaneity of supply and demand for solar PV systems, we also estimate a version excluding price. This specification without price can be thought of as estimating the combined treatment effect of the behavioral intervention and the group pricing. We also estimate a specification that instruments for price using electrician and roofer wages at the county-month level (BEA, 2015). After conditioning on municipality

[11] The market share is defined as $s_{mt} = \frac{q_{mt}+1}{P_m - \sum_{\tau<t} q_{m\tau}}$, where q_{mt} is the number installations and P_m is the size of the potential market for solar PV based on the satellite imaging. The outside option share is defined as $s^0_{mt} = 1 - s_{mt}$.

fixed effects, which subsume income, these instruments should not enter into demand, and yet can be expected to shift marginal costs.

4.2 Pricing

The adoption model in (1) lends itself to a classic difference-in-differences treatment effects approach as long as there is a suitable control group. We employ a similar difference-in-differences estimation to examine the treatment effect on the pre-incentive price:

$$p_{mt} = \gamma T_{mt} + \mu_m + \delta_t + \varepsilon_{mt}. \tag{2}$$

The estimated treatment effect in the price equation is the effect of the program on the equilibrium price: since Solarize may affect supply as well as demand, we can not separate the contributions of each.

4.3 Identification and Control Groups

Identification of the coefficients in both (1) or (2) relies on the parallel trends assumption and the stable unit treatment value assumption (SUTVA). The parallel trends assumption requires that the control group would have had an identical trend to the treatment group had the treatment not been implemented. If this assumption holds, then any time-varying unobservables will be captured through the trends in the control group. This assumption only holds with a valid control group. As described above, in the first round of Solarize, we are able to randomize among the municipalities that applied to participate in the program. Thus, the control group for this round consists of the randomly non-selected municipalities. Many of these non-selected municipalities did receive the treatment in later rounds. The key identification assumption in this RCT is that the randomization is valid.

For the other rounds, our primary results rely on rolling controls, in the spirit of Sianesi (2004) and Harding and Hsiaw (2014). In other words, we assume that the exact round that a municipality applies for is as-good-as random, so that municipalities that apply to join the program in later rounds are a good control group for the earlier rounds. The rolling controls can be used for all four rounds, including the fourth, for there is also fifth round that is not included in this study.[12] Since the process of municipalities applying involves chance contacts between town leaders and Smartpower or CBG, as well as time in the town leaders' schedule to apply, we believe that the timing of the application is quite plausibly random.

To provide evidence in support of the parallel trends assumption, we can examine the comparability of the treatment group to the control group. A first way to examine this is to look at the pre-trends in both groups. Figure 4 provides convincing evidence that in the pre-period, nearly all of the municipalities in CT are the same prior to any Solarize treatments, for all have very few adoptions of solar PV. A statistical test of the differences in the mean monthly adoptions between any of the treatment groups and the control group fails to reject that the difference is zero in the pre-period.

Another common way to provide evidence in support of the parallel trends assumption is to examine the balance of demographics across the control and treatment groups. Table 3 shows that there is a considerable degree of balance across a wide range of demographics and voter registration variables for the pooled sample with all four rounds of Solarize included. In fact, for all of the variables examined, we fail to reject the null of zero difference in a two-sided pairwise t-test of differences in means.

For the set of treated towns that are randomized across all municipalities in CT, the primary control group we use is the set of all municipalities in CT that did not receive a prior treatment. The CTSC municipalities began their campaigns at a similar time as Round 2 of the Solarize program, so the primary control group we use for this campaign is the same as the future controls used for Round 2. For all campaigns, we perform a set of robustness checks using propensity score matching approaches to confirm our results.

[12] There are additional experiments performed during round 3 and round 5 that provide an even larger pool of municipalities that opted-in to the treatment. In these additional experiments, municipalities are randomized in the treatment they are provided, with one of the treatments being the classic treatment described above.

Even if the control groups are chosen carefully, SUTVA must hold. This requires stable treatments, which we are confident of, and non-interference. For example, the treatment in one municipality may spill over and affect a control municipality. Figure 4 provides descriptive evidence that treatment spillovers are unlikely to have a dominant effect, for there is no discernable change in any of the municipalities treated in a future round in the early round. Yet spillovers may still lead to an underestimate of our treatment effect if they exist. We address this possibility with a robustness check that drops all adjacent municipalities from the control and a discussion of spillovers beyond adjacent municipalities.

5 Results

5.1 Treatment Effects

We begin by estimating (1) by ordinary least squares (OLS). Table 4 presents the primary results, which are simplified to not include the system price. Columns 1 through 5 present the results by round, while column 6 presents the pooled results. The dependent variable is the log odds-ratio, so the treatment effect coefficients, while highly statistically significant, are not easily interpretable. The bottom panel converts these coefficients into the average treatment effect on installations per municipality.[13] Just below this, on the bottom row of the table, is the raw number of installations per treatment town during the treatment period.

These results indicate that in Round 1 the estimated average treatment effect is 53 additional installations on average per municipality out of an average of 67.5 installations during the treatment period in the treated municipalities. The result using rolling (future) controls in column 2 is nearly the same as the result in column 1, providing further justification for the empirical approach using future controls.[14] Consistent with Figure 4, we see a smaller treatment effect in Rounds 2 and 3 and a slightly larger one again in Round 4. The pooled

[13] See Appendix 2 for the details of this calculation.

[14] Note that for the pooled models, the time variable is converted from calendar months to months from the start of the treatment. This allows for a clean pooled regression.

sample brings together the all four rounds, using the randomized control group from round 1 and the future control groups from the other three rounds. The results indicate an average treatment effect of roughly 27 additional installations per municipality involved in the program, and given that there were roughly 50 installations per treatment municipality, this implies an increase of over 100 percent over what would have happened otherwise.

These results show a highly statistically significant treatment effect with robust standard errors clustered at the municipality level. One possible concern about the inference in these results is that in columns 1 through 5, the number of clusters is relatively small. Bertrand, Duflo and Mullainathan (2004) perform simulations indicating that the cluster-correlated Huber-White estimator can lead to an over-rejection of the null hypothesis when the number of clusters is small, with 50 being a common benchmark. While the raw data suggests that we are almost certain to have a strong effect, and we are most interested in the results in column 6, we do consider another form of inference. Rosenbaum, Duflo and Mullainathan (2002) generates consistent hypothesis tests using randomized inference, an approach taken in Bhushan, Bloom, Clingingsmith, Hong, King, Kremer, Loevinsohn and Schwartz (2007). Appendix 3 follows this approach and continues to show high statistical significance of the treatment effect.

Table 5 presents the same results as in Table 4, but includes the price. The results are nearly identical, and the price coefficient is negative and statistically significant. This provides some first evidence on the mechanisms underpinning the program. While demand does increase with lower prices, the treatment effect remains largely the same conditional on price, suggesting that the other elements of the Solarize package of interventions are more important than the discount group pricing. Table 6 shows that instrumenting for price with electrician and roofer wages also does not change the results. Taken together, the findings in Tables 4, 5, and 6 strongly confirm hypothesis 1 in section 2.

Figures 6, 7, 8, and 9 show the treatment effect coefficients over time for each round. These indicate a pre-treatment effect that is statistically indistinguishable from zero, a dramatic treatment effect during the treatment, and a post-treatment effect indistinguishable from zero.[15] One might have expected a harvesting or intertemporal shifting effect, as in Mian and Sufi (2012) for cars after the Cash-for-Clunkers program. On the other hand, if the program "seeds" installations in a municipality, leading to additional future word-of-mouth and neighbor effects (Bollinger and Gillingham, 2012), one might expect a continued future increase in installations. Given the limited post-treatment time available for the later rounds, we interpret our result of no post-treatment effect as suggestive and worthy of further study after a sufficient amount of time post-treatment.

Table 7 presents the results from estimating (2) by OLS on each round and the pooled sample, just as in Table 4. The results indicate a considerable discount given to residents of municipalities participating in the Solarize program. The discount declines substantially over the rounds, beginning around $1/W (out of $4.86/W in the control municipalities) to $0.35/W and $0.52/W in Rounds 3 and 4 (with a similar pre-incentive price to Round 1). In the data, we can see that many more cost adders are used in the later programs, perhaps allowing installers to profit more from the programs. In column 6, the pooled results indicate an average decrease in price from the campaign of $0.64/W, with a control average price of $4.61/W. These results strongly support hypothesis 2 in section 3.

5.2 Treatment Externalities

If the Solarize programs lead to additional installations through word-of-mouth, we might expect nearby communities to also experience some treatment effect, since social networks extend across municipal borders. Such spillovers or treatment externalities have been exhibited in other field experimental setting (e.g., Miguel and Kremer, 2004) and can contribute positively to the cost-effectiveness of the program.

Table 8 estimates the model in (1), only the treatment now is a municipality adjacent to a Solarize campaign municipality interacted with the campaign. The control group for

[15] Note that future municipalities that receive the treatment in the time frame covered are not included in the control group.

each round consists of all municipalities that have never received a Solarize program or CTSC program. The results suggest a very small treatment externality effect on adoptions. Moreover, it is only statistically significant in Round 1 and in the pooled specification. The average treatment effect per adjacent municipality is 1.19 for Round 1 and 2.14 for the pooled sample. While not a strong effect, this provides weak evidence in support of hypothesis 3 in section 3 with respect to adoptions.

Table 9 estimates the model (2), with the adjacent municipalities as the treated and all non-program municipalities as controls. Again, the results suggest a small treatment externality effect. The results are statistically significant in columns 1, 2, and 5. The coefficient in the first row for the pooled sample indicates that being an adjacent municipality to a Solarize program municipality lowers the average price of a PV system by \$0.15/W. This result is intuitive: if some consumers in the adjacent municipalities hear about the discount pricing from their neighbors and succeed in negotiating a similar discount, the average price would decline. This result again provides evidence supporting hypothesis 3 in section 3.

5.3 How Important is Selection into the Program?

The results shown above are useful for understanding the effect of the marginal municipality most likely to select into the program in the future. But the treatment effect may be heterogeneous and one would expect the marginal municipalities to have a larger treatment effect than the average municipality. To understand the importance of selection into the program, Table 10 shows the results of providing the Solarize treatment to randomly-drawn municipalities in CT. These results provide insight into the effectiveness of the program if it is scaled up significantly or moved to less-enthusiastic locales.

The results in Table 10 indicate a smaller, but still statistically significant, average treatment effect than in Table 4. The estimated coefficients suggest that these randomized Solarize programs led to 12.5 additional installations per municipality on average. This is not surprisingly considerably less than the number of additional installations in the Solarize Round

4 program, suggesting a very strong selection effect. Figure 10 shows the treatment effect over time for these randomly chosen programs.

Column 3 in Table 10 presents the price regression results. The coefficient on the treatment effect indicates that on average the program lowers prices by $0.35/W. While less than the price decline in the concurrent Round 4, this price decline is actually similar to the price decline in Round 3. These results confirm hypothesis 4 in section 3 and indicate that while the Solarize program can still be effective in randomly selected municipalities, selection matters and a stronger effect can be expected when municipalities opt-in on their own.

This sheds further light on the mechanisms underlying the effectiveness of the program. Selecting into this program generally is the result of one or two key ambassadors or town leaders who are particularly interested in promoting solar PV to their community. Having these key promoters at the center of a campaign is the primary difference between the campaigns in the randomly drawn municipalities and the municipalities that selected into the program.

5.4 Connecticut Solar Challenge

The CTSC program also helps to better understand the mechanisms underlying the effectiveness of the Solarize treatment. By not having explicit price competition at the bidding stage of the process and not having involvement of Smartpower and CGB, CTSC provides a useful example of how Solarize could work if it is run in the private market.[16]

Table 11 shows the results of the CTSC. Columns 1 through 3 include only the first 6 months after the kick-off event as part of the treatment effect. The control municipalities are the same as for Solarize Round 2, for the CTSC municipalities selected into the program. The coefficients on the treatment effect in columns 1 and 2 are positive and statistically significant, indicating an average treatment effect of just under 7 additional installations on average due to the program (out of an average of 11 installations that occurred). These

[16]Although Aegis Solar created and funded CTSC, it is technically a non-profit organization.

results indicate that the CTSC program was less effective at increasing installations than even the randomly selected municipalities. The price results are even more interesting. In column 3 we see that the treatment effect on prices is near zero and actually *positive*. Without competition at the outset of the program, CTSC led to slightly higher prices than in any of the control towns.

Columns 4 through 6 in Table 11 include variables for a post-six month period when Aegis Solar actually did provide greater discount pricing, perhaps after noting that sales were slow in the first six months. This four-month post-treatment period led to an additional 7 installations on average per municipality and a discount of $0.46/W. Interestingly, after this four-month post-treatment period, the CTSC municipalities remained officially part of CTSC, but actually had a slightly *negative* (not statistically significant) treatment effect. These findings confirm the hypothesis 5 in section 3.

Why did these results differ from the results from the concurrent Solarize Round 2? CTSC attempted to use the same package of interventions, but without the competition and without CGB and Smartpower involvement. Competition at the bidding stage clearly translates into the lower prices. But prices cannot explain the entire difference in the number of installations, for prices are controlled for in columns 2 and 5. Aegis Solar was also extremely effective in Solarize Round 1, so it is unlikely that the difference is due to a lower quality installer who did not know how to run the Solarize intervention. This leads to a final possibility: that trust in the program is a critical element. CGB and Smartpower, along with the competitive bidding process provided potential customers more trust in the process, potentially explaining the larger treatment effects.

5.5 Further Insight into Mechanisms

To more deeply understand the mechanisms driving the treatment effects, we survey solar PV adopters after each Solarize round. This survey was performed through the Qualtrics survey software and was sent to respondents via e-mail, with 2 iPads raffled off as a reward for responding. The e-mail addresses came from Solarize event sign-up sheets and installer contract lists. Approximately 6 percent of the signed contracts did not have an e-mail address. All others we contacted one month after the end of the round, with a follow-up to non-respondents one month later. The adopter response rate is 35.6, 45.2, 45.7, and 42.5 percent in each round respectively, for an overall response rate of 42.2 percent (496/1,175). This is a high response rate for an online survey, a testament to the enthusiasm of the adopters in solar and the Solarize program.

Two questions provide the most insight into the mechanisms underlying the effectiveness of the program. One question provides 14 possible factors that influence the decision to install solar PV through the Solarize program and asks "Rate the importance of each factor in your decision to install solar PV," with the following possible answers: extremely important, very important, somewhat important, not at all important. Figure 12 shows the responses to each of the 14 factors. What is most notable are the factors that the highest percentage of respondents rated as "extremely important." These factors all have a social learning element to them: "town information event," "friend or neighbor's recommendation," "recommendation of someone you interact with in your town," and "seeing solar on another home or business." All of these also have a high percentage of respondents rating the factor as "very important." This survey result provides suggestive evidence that the Solarize behavioral intervention may be working exactly as intended: by fostering social learning.

The second useful question asks: "What was the single most important for the decision to install solar?" This question is useful for further disentangling the effect of discount pricing from other factors. If the Solarize program worked primarily by acting as a sale, then we would expect most respondents to say that the discount pricing was the most important factor in the decision to install solar PV. Figure 13 shows that this factor is indeed important, with 32 percent of the responses, but that two-thirds of the respondents had other reasons. In fact, the second and third largest responses, "concern for the environment" and "lower my monthly utility bill" are factors that should not have been specific to the Solarize program.

This suggests that information provision–highlighting how solar can improve the environment and/or lower monthly utility bills–is a key part of the program. Since these two responses make up over 40 percent of the total responses, these findings may underscore the importance of information provision and social learning in the program.

5.6 Robustness and Falsification Tests

We perform an extensive set of robustness and falsification tests to confirm the primary results including the following:

- Using the Connecticut Clean Energy Communities that are not in Solarize as a control group.
- Using nearest-neighbor propensity score matching based on demographics and voting registration variables to create control groups.
- Dropping all adjacent municipalities to confirm the SUTVA assumption.
- Using the highest, rather than average, price in each town or county to impute missing prices.
- Estimating the price regressions on the subsample for which price is nonmissing.
- Using randomized inference, rather than clustered standard errors for estimations that have a small number of clusters.
- Performing a placebo/falsification test where the treatment is assumed to be an earlier time period than it actually is.

We find our results to be highly robust to all of these tests and we include each of these in Appendix 2.

6 Cost-effectiveness and Welfare Implications

As described in section 2, solar PV is quite likely beneficial from a private consumer perspective for most, if not all, adopters in CT. But are the Solarize programs cost-effective at meeting policy goals? Are they socially beneficial? Answering these questions, and particularly the question of welfare, requires many assumptions, so our estimates should be viewed as a rough benchmark.

A first calculation can be made from a policymaker's perspective. On June 3, 2015, the Connecticut state legislature approved H.B. 6838, which sets a goal for residential solar PV in Connecticut of 300 MW of installed capacity by 2025, for the previous goal had been exceeded several years early. So from a policymaker perspective, the cost-effectiveness of the program in meeting these goals is of interest. For rounds 1 and 2, the funding for running all of the programs is $100,000 from foundations, $100,000 from CT taxpayers (CGB), $72,000 worth of CGB staff time, and roughly $32,000 in installer expenses. Dividing these costs by the number of induced installations translates into roughly $900 per installation. One benchmark to compare this to is the consumer acquisition costs for installers. These costs turn out to be roughly $1,500 to $3,000 per installation. Another benchmark to compare this to is the estimated decrease in the price of the systems of -$0.64. For a 6 kW system, this translates to $3,840 of a discount due to the Solarize program.

Moving to the social benefits, the water becomes murkier. Understanding the full social welfare effects requires understanding the social benefits of an installation of solar PV today. This requires understanding the reduced pollution externalities from offsetting fossil fuel generation, the social cost of public funds from raising the revenue to fund the program, the consumer welfare benefits from installing solar PV (including any warm glow), and even any spillover benefits from learning-by-doing in the technology.

7 Conclusions

This paper contributes to the literature in on pro-social behavioral interventions. The Solarize program, which draws upon several theoretical and empirical findings in behavioral economics, is expanding rapidly and one could imagine being applied to other technologies.

We find a very strong treatment effect from the program: an increase in installations by 27 per municipality and lowered pre-incentive equilibrium prices by \$0.65/W. Our research delves into the mechanisms underlying this result, highlighting the importance of social learning and information provision, especially by ambassadors, as being a key factor underlying the success of the program. The program is surprisingly cost-effective, although the full social welfare implications are likely context-specific and depend on the social benefits of installing solar PV.

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Appendix A Private Economics of Solar PV in Connecticut

This appendix provides details on the calculations for the private economics of solar PV in Connecticut, based on data from 2014. As solar PV prices have dropped since then, one would expect the private economics to have improved further in 2015 and 2016. As mentioned above, only a small fraction of the solar PV systems installed in CT as of 2014 were third party-owned. The remainder were either purchased with cash or financed. Such financing is possible through a home equity loan, a personal loan, or a CT solar loan (a product that was available for a short time from the CGB). We cannot observe whether consumers make an all-cash purchase or finance with a loan that is not the CT solar loan. Only 30 installations in our dataset were done with the CT solar loan, so this was not an important factor in our data.

The average system size in CT in 2014 is 4.23 kW, which is large enough to generate most of the electricity for a typical residential home. This system will produce 4,736 kW annually.[17] In 2014, the initial cost of a system is $4.54 per watt.[18] This implies a system cost of $19,187.28. The state rebate in late 2014 is $1.25/W, which corresponds to $5,287.50. Assuming that the purchaser has sufficient taxable income to take the full federal investment tax credit, this would imply a tax credit of $4,169.93. Thus, the post-incentive cost comes out to $9,729.85. The lifespan of a solar PV system is widely considered to be 25 years. About half-way through the lifespan of the system, the inverter must be replaced. While the future cost may be less, the cost in 2014 of a new inverter for a system this size is $3,315.21.[19] The electricity rates in CT are roughly $0.16/kWh on average. We assume that these electricity rates increase by 2 percent annually, consistent with EIA projections.[20].

The following analyses ignore warm-glow benefits to consumer utility, and also assume no additional maintenance costs outside of the replacement of the inverter.

[17] See http://pvwatts.nrel.gov/.

[18] See http://www.energizect.com/sites/default/files/uploads/Residential_Solar_Investment_Program_Market_Watch_Repor

[19] See http://www.greentechmedia.com/articles/read/new-report-tough-times-ahead-for-pv-inverter-incumbents.

[20] See http://www.eia.gov/forecasts/steo/report/electricity.cfm .

Cash Purchase

The simplest case is an all-cash purchase. Given the assumptions above, the internal rate of return on the 25-year investment is 7 percent. Given a 5 percent discount rate, the net present value of the investment is $1,816, while at a 7 percent discount rate, the investment is roughly break-even. The payback period for the investment is roughly 14 years. Thus, from a private perspective, the investment is a reasonable investment for the typical household purchasing solar PV in CT, albeit one with a relatively long payback period.

Financing

It is likely that many, if not most, consumers used some financing for their purchase of the solar PV system. For illustrative calculations, we assume a conservative 7 percent interest rate, a loan term of 20 years, with monthly payments. Under these assumptions, the payback period is very quick, due to the state rebate and the federal tax credit. For example, at the end of the first year, upon receipt of the state rebate nad tax credit, the net revenue from the system is over $9,000. After this year, the net annual revenue becomes negative for the remainder of the loan, but the cumulative cash flow remains positive for the remainder of the lifespan of the panels.

Other Options

Other options include power purchase agreements and solar leases. The economics of these depend greatly on the contract details. Illustrative calculations suggest that neither of these options are as attractive on a net present value basis as financing or an outright cash purchase. However, these options require little or no upfront investment and put the burden of maintenance on the installing firm, rather than the residential owner.

Further sensitivity analyses with different assumptions about the growth in electricity rates do not change the primary results significantly, unless it is assumed that electricity rates will decrease over time, rather than increase.

Table 1: Timeline of Campaigns

	Start Date	End Date	Treated Towns
Round 1	Sept 2012	Jan 2013	4
Round 2	Mar 2013	July 2013	5
Round 3	Sept 2013	Feb 2014	11
Round 4	Apr 2014	Sept 2014	7
Select	Apr 2014	Sept 2014	4
CT Solar Challenge	Mar 2013	(Sept 2013)	3

Notes: These are approximate dates; there are some individual campaigns that began or ended slightly before or after. “Select” refers to the Solarize campaigns randomized across CT. The end date for CT Solar Challenge is unspecified and it appears that the campaign was extended beyond the six months listed here.

Table 2: Summary Statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Installation Count	0.48	1.72	0	65	20,496
Cumulative Installations	11.071	17.732	0	186	20,496
Potential Market Size	5918	7209	198	41930	20,496
Pre-incentive price ($2014/W)	6.79	1.90	1.62	16.57	20,327

Notes: Summary statistics for the full dataset covering 2004-2014. An observation is a municipality-month.

Table 3: Pooled Sample Balance Across Treatment and Control

	Treatment		Control		
	mean	std. dev.	mean	std. dev.	p-value
Population Density	777.5	903.5	978.0	1476.4	0.467
Median Household Income	93908	24228	93908	29915	0.312
Fraction Pop Over 65	0.158	0.040	0.148	0.032	0.272
Fraction White	0.874	0.125	0.872	0.149	0.925
Fraction Black	0.051	0.990	0.053	0.120	0.950
Fraction Families	0.706	0.069	0.717	0.085	0.523
Fraction Commute 60+ Miles	0.082	0.053	0.095	0.067	0.361
Fraction College Grads	0.490	0.050	0.485	0.057	0.710
Fraction Below Poverty	0.060	0.055	0.070	0.075	0.528
Fraction Unemployed	0.077	0.024	0.080	0.029	0.578
Fraction Detached Dwelling	0.778	0.148	0.767	0.179	0.782
Fraction Regist. Republican	0.243	0.076	0.237	0.056	0.727
Fraction Regist. Democrat	0.320	0.084	0.331	0.099	0.598

Notes: Demographic variables from the 2009-2013 American Community Survey. The sample is the pooled sample of all four rounds of Solarize Classic, covering 27 treatment towns. The control towns are based on the randomization in Round 1 and future rounds (rolling control). There are 27 distinct control towns, but many are used as controls for multiple rounds; thus there are 48 controls. The units for median household income are 2014$. The p-values are for a pairwise two-sided t-test of differences in means by group.

Table 4: Primary Treatment Effect Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Round 1		Round 2	Round 3	Round 4	Pooled
	random	future	future	future	future	rand/future
Treatment Effect	2.21***	2.04***	1.34***	0.95***	1.18***	1.31***
	(0.25)	(0.23)	(0.17)	(0.16)	(0.15)	(0.13)
During Campaign	0.08	0.26***	0.26***	0.32***	1.01***	0.41***
	(0.08)	(0.03)	(0.04)	(0.05)	(0.14)	(0.05)
Constant	X	X	X	X	X	X
Town FE	X	X	X	X	X	X
Month Dummies	X	X	X	X	X	X
R-squared	0.771	0.926	0.923	0.903	0.833	0.892
N	882	5292	5250	3808	2737	12677
Number of total towns	9	53	49	33	22	54
Number of treated towns	4	4	5	11	7	27
Ave. treat. effect per town	53.41	52.18	20.82	15.59	42.91	26.85
Ave. installs per treat. town	67.50	67.50	30.20	31.45	81.57	49.55

Notes: Dependent variable is the log odds-ratio of market shares. Unit of observation is town-month. "Random" refers to randomly selected control towns; "future" refers to future selected towns as controls. Confidence intervals in brackets based on the wild cluster bootstrap-t procedure from Cameron, Gelbach and Miller (2008). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 5: Results Including Price

	(1)	(2)	(3)	(4)	(5)	(6)
	Round 1		Round 2	Round 3	Round 4	Pooled
	random	future	future	future	future	rand/future
Treatment Effect	2.17***	2.02***	1.32***	0.95***	1.16***	1.30***
	(0.25)	(0.23)	(0.17)	(0.16)	(0.15)	(0.13)
During Campaign	0.03	0.22***	0.21***	0.26***	0.94***	0.35***
	(0.08)	(0.03)	(0.03)	(0.05)	(0.13)	(0.04)
Price Per Watt	-0.02***	-0.01***	-0.02***	-0.03***	-0.03***	-0.02***
	(0.007)	(0.002)	(0.002)	(0.004)	(0.005)	(0.003)
Constant	X	X	X	X	X	X
Town FE	X	X	X	X	X	X
R-squared	0.831	0.926	0.924	0.905	0.837	0.894
N	882	5292	5250	3808	2737	12677
Number of total towns	9	53	49	33	22	54
Number of treated towns	4	4	5	11	7	27
Ave. treat. effect per town	53.23	52.09	20.72	15.50	42.61	26.70
Ave. installs per treat. town	67.50	67.50	30.20	31.45	81.57	49.55

Notes: Dependent variable is the log odds-ratio of market shares. Unit of observation is town-month. "Random" refers to randomly selected control towns; "future" refers to future selected towns as controls. Confidence intervals in brackets based on the wild cluster bootstrap-t procedure from Cameron *et al.* (2008). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 6: IV Results Including Price

	(1) Pooled rand/future
Treatment Effect	1.30***
	(0.13)
During Campaign	0.36***
	(0.06)
Price Per Watt	-0.02**
	(0.01)
Constant	X
Town FE	X
R-squared	0.894
N	12677
Number of total towns	54
Number of treated towns	27
Ave. treat. effect per town	26.70
Ave. installs per treat. town	49.55

Notes: Dependent variable is the log odds-ratio of market shares. The price per watt is instrumented using the electrician wage and roofer wage. Both instruments are statistically significant at the 1% level in the first stage with a first-stage F-statistic of 240. Unit of observation is town-month. This estimation uses the randomly selected control group for round 1 and the future control groups for all other rounds. Standard errors clustered on town in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 7: Effect on Installed Prices

	(1)	(2)	(3)	(4)	(5)	(6)
	Round 1		Round 2	Round 3	Round 4	Pooled
	random	future	future	future	future	rand/future
Treatment Effect	-1.17***	-0.98***	-0.63***	-0.35***	-0.52***	-0.64***
	(0.29)	(0.07)	(0.06)	(0.11)	(0.15)	(0.07)
During Campaign	-2.52***	-2.71***	-2.54***	-2.62***	-2.22***	-2.48***
	(0.29)	(0.03)	(0.03)	(0.05)	(0.05)	(0.03)
Constant	X	X	X	X	X	X
Town FE	X	X	X	X	X	X
R-squared	0.131	0.113	0.087	0.101	0.070	0.092
N	882	5292	5250	3808	2737	12677
Number of total towns	9	53	49	33	22	54
Number of treated towns	4	4	5	11	7	27
Treat. town ave. price ($/W)	3.83	3.83	4.04	4.09	4.17	4.07
Control town ave. price ($/W)	4.86	4.69	4.67	4.43	4.67	4.61

Notes: Dependent variable is the average pre-incentive installed price (2014$/W). Unit of observation is town-month. "Random" refers to randomly selected control towns; "future" refers to future selected towns as controls. Standard errors clustered on town in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 8: Treatment Externalities: Effect on Installations

	(1) Round 1	(2) Round 2	(3) Round 3	(4) Round 4	(5) Pooled
Adjacent Town During	0.16**	-0.05	0.05	0.17	0.13**
	(0.07)	(0.05)	(0.06)	(0.12)	(0.06)
During Campaign	0.16***	0.19***	0.25***	0.87***	0.35***
	(0.02)	(0.02)	(0.02)	(0.05)	(0.02)
Constant	X	X	X	X	X
Town FE	X	X	X	X	X
R-squared	0.945	0.942	0.932	0.913	0.930
N	16072	16380	15008	14756	61320
Number of total towns	163	155	133	123	164
Number of adjacent towns	14	18	29	22	66
Ave. treat. effect per adj. town	1.19				2.14
Ave. installs per adj. town	3.57	1.94	4.67	18.45	7.54

Notes: Dependent variable is the log odds-ratio of market shares. Unit of observation is town-month. Standard errors clustered on town in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 9: Treatment Externalities: Effect on Prices

	(1) Round 1	(2) Round 2	(3) Round 3	(4) Round 4	(5) Pooled
Adjacent Town During	-0.19*	-0.25***	-0.07	0.01	-0.15***
	(0.11)	(0.06)	(0.05)	(0.12)	(0.03)
During Campaign	-2.68***	-2.45***	-2.65***	-2.28***	-2.51***
	(0.03)	(0.02)	(0.02)	(0.05)	(0.01)
Constant	X	X	X	X	X
Town FE	X	X	X	X	X
R-squared	0.108	0.081	0.098	0.067	0.088
N	16072	16380	15008	14756	61320
Number of total towns	163	155	133	123	164
Number of adjacent towns	14	18	29	22	66
Adj. town ave. price ($/W)	4.64	4.55	4.37	4.64	4.52
Control town ave. price ($/W)	4.71	4.77	4.42	4.62	4.64

Notes: Dependent variable is the average pre-incentive installed price (2014$/W). Unit of observation is town-month. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 10: Results for Solarize Randomized Across CT

	(1) installs	(2) installs	(3) prices
Treatment Effect	0.55***	0.54***	-0.35***
	(0.13)	(0.13)	(0.11)
During Campaign	0.91***	0.84***	-2.26***
	(0.05)	(0.04)	(0.02)
Price Per Watt		-0.03***	
		(0.003)	
Constant	X	X	X
Town FE	X	X	X
R-squared	0.907	0.910	0.066
N	15,960	15,960	15,960
Number of total towns	9	53	49
Number of treated towns	4	4	5
Ave. treat. effect per town	12.70	12.50	
Ave. installs per treat. town	39.40	39.40	
Treat. town ave. price ($/W)			4.30
Control town ave. price ($/W)			4.62

Notes: Dependent variable is listed on the column heading; "installs" refers to the log odds-ratio of the market shares. Unit of observation is town-month. Standard errors clustered on town in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 11: Connecticut Solar Challenge Results

	(1)	(2)	(3)	(4)	(5)	(6)
	installs	installs	prices	installs	installs	prices
Treatment Effect	0.61***	0.61***	0.01***	0.61***	0.61***	0.006
	(0.11)	(0.12)	(0.07)	(0.13)	(0.13)	(0.07)
During Campaign	0.21	0.17***	-2.54***	0.24***	0.19***	-2.55***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Post Treatment Effect				0.49**	0.48**	-0.46**
				(0.23)	(0.04)	(0.18)
During Post Period				0.50***	0.45***	-2.67***
				(0.06)	(0.06)	(0.05)
Price Per Watt		-0.02***		-0.03***	-0.02***	-0.02***
		(0.003)		(0.004)	(0.002)	(0.003)
Constant	X	X	X	X	X	X
Town FE	X	X	X	X	X	X
R-squared	0.923	0.924	0.924	0.098	0.915	0.165
N	5007	5007	5007	5243	5243	5243
Number of total towns	47	47	47	47	47	47
Number of treated towns	3	3	3	3	3	3
Ave. treat. effect per town	6.94	6.94		7.07	7.07	
Post-period ave. treat effect				7.03	7.03	
Ave. installs per treat. town	11.33	11.33		11.33	11.33	
Treat. town ave. price ($/W)			4.73			4.73
Post-period ave. price ($/W)						4.14
Control town ave. price ($/W)			4.66			4.54

Notes: Dependent variable is the log odds-ratio of market shares or prices (2014$/W). Unit of observation is town-month. All towns use future (rolling) controls. Standard errors clustered on town in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 12: Appendix: Detailed Timeline of Campaigns

	Start Date	End Date
Round 1		
Durham	Sept 5, 2012	Jan 14, 2013
Westport	Aug 22, 2012	Jan 14, 2013
Portland	Sept 4, 2012	Jan 14, 2013
Fairfield	Aug 28, 2012	Jan 14, 2013
Round 2		
Bridgeport	Mar 26, 2013	July 31, 2013
Coventry	Mar 30, 2013	July 31, 2013
Canton	Mar 19, 2013	July 31, 2013
Mansfield	Mar 11, 2013	July 31, 2013
Windham	Mar 11, 2013	July 31, 2013
Round 3		
Easton	Sept 22, 2013	Feb 9, 2014
Redding	Sept 22, 2013	Feb 9, 2014
Trumbull	Sept 22, 2013	Feb 9, 2014
Ashford	Sept 24, 2013	Feb 11, 2014
Chaplin	Sept 24, 2013	Feb 11, 2014
Hampton	Sept 24, 2013	Feb 11, 2014
Pomfret	Sept 24, 2013	Feb 11, 2014
Greenwich	Oct 2, 2013	Feb 18, 2014
Newtown	Sept 24, 2013	Feb 28, 2014
Manchester	Oct 3, 2013	Feb 28, 2014
West Hartford	Sept 30, 2013	Feb 18, 2014
Round 4		
Tolland	Apr 23, 2014	Sept 16, 2014
Torrington	Apr 24, 2014	Sept 16, 2014
Simsbury	Apr 29, 2014	Sept 23, 2014
Bloomfield	May 6, 2014	Sept 30, 2014
Farmington	May 14, 2014	Oct 7, 2014
Haddam	May 15, 2014	Oct 7, 2014
Killingworth	May 15, 2014	Oct 7, 2014
Select		
Essex	Apr 29, 2014	Sept 23, 2014
Montville	May 1, 2014	Sept 23, 2014
Brookfield	May 6, 2014	Sept 30, 2014
Weston	June 24, 2014	Nov 14, 2014
East Lyme	May 22, 2014	Oct 14, 2014

Figure 1: Map of Solarize programs in Connecticut in this study.

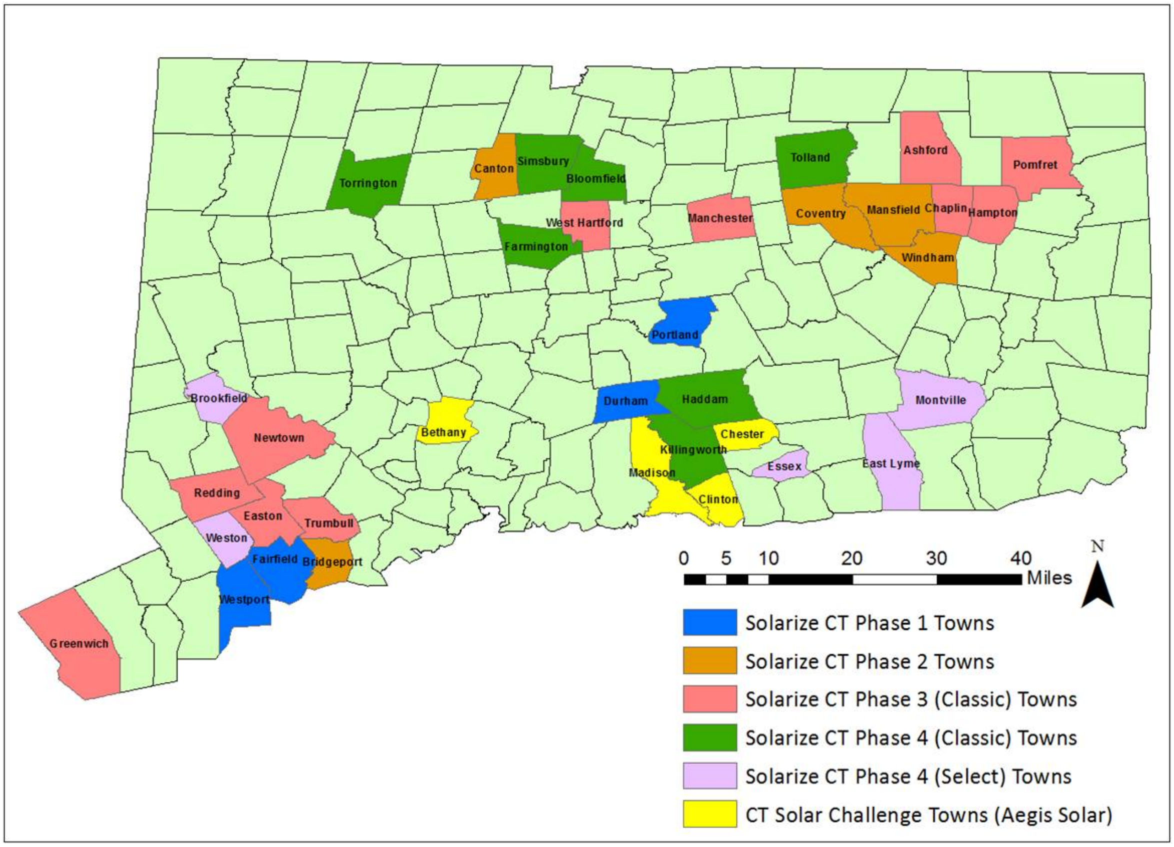

Figure 2: Cumulative installations in Solarize Rounds 1 and 2.

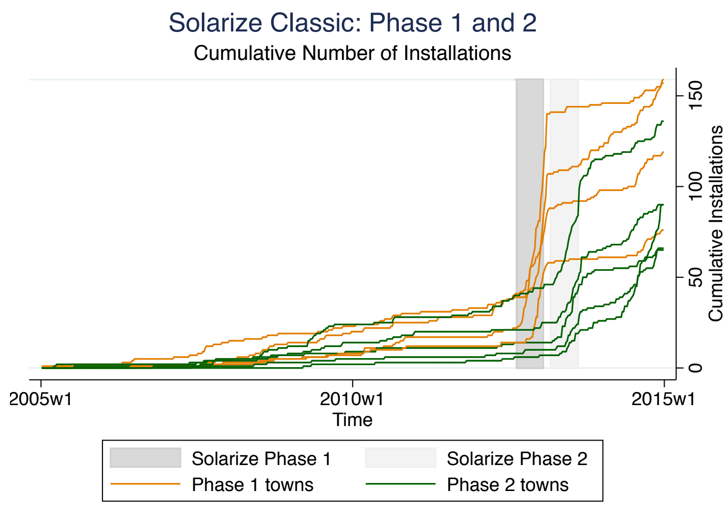

Figure 3: Cumulative installations in Solarize Rounds 3 and 4.

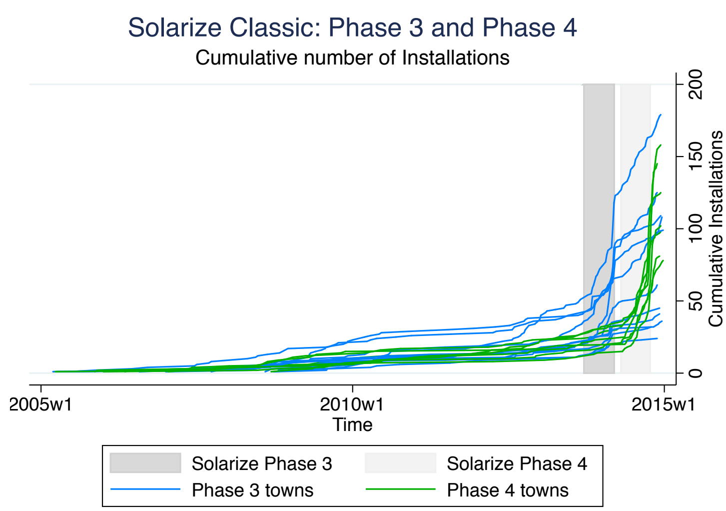

Figure 4: Monthly installations by round.

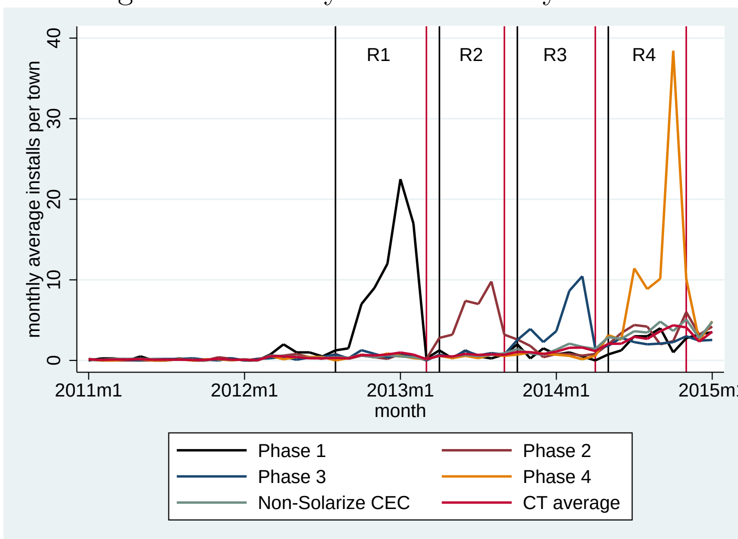

Figure 5: Monthly average prices by round.

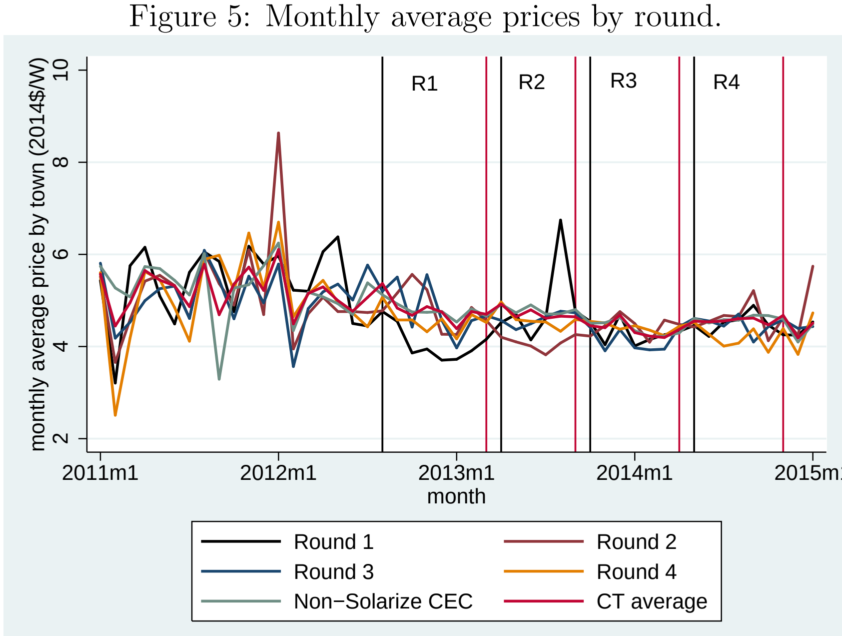

Figure 6: Treatment Effects Over Time.

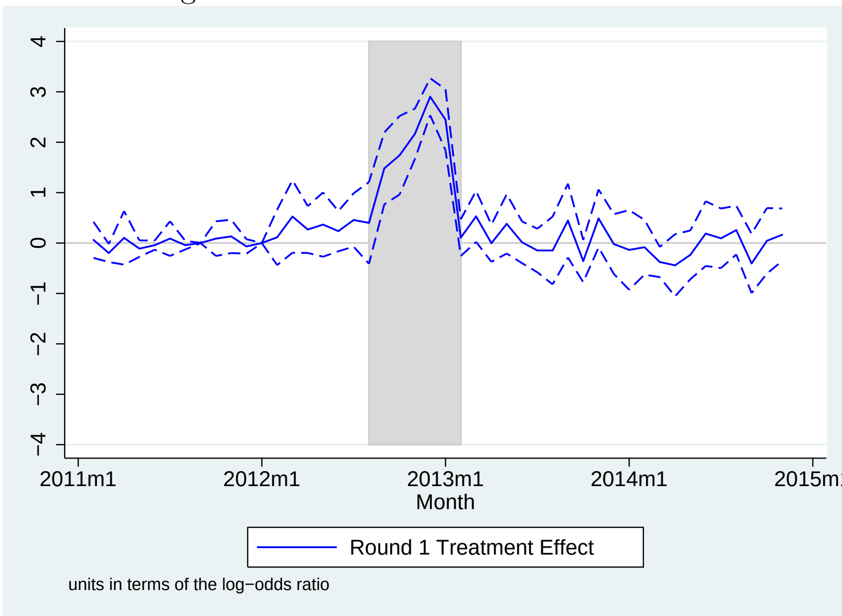

Figure 7: Treatment Effects Over Time.

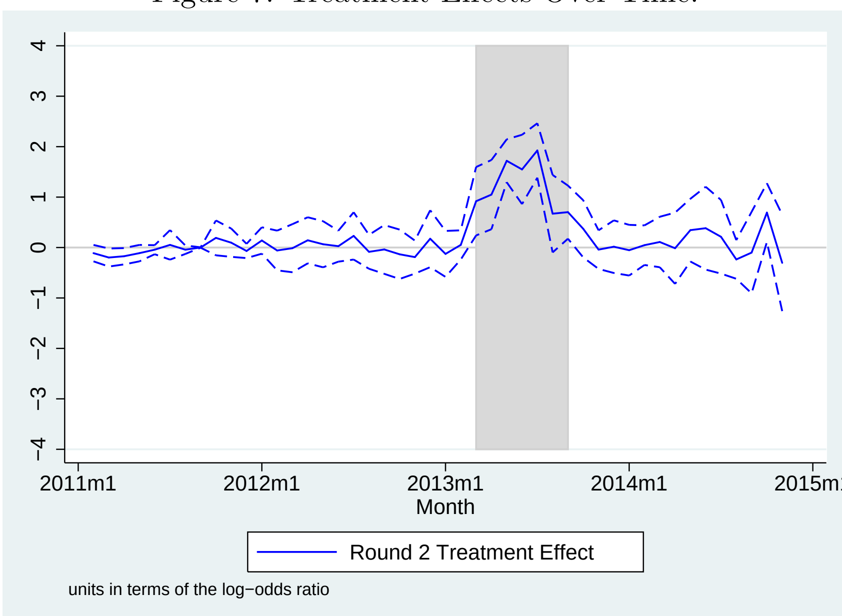

Figure 8: Treatment Effects Over Time.

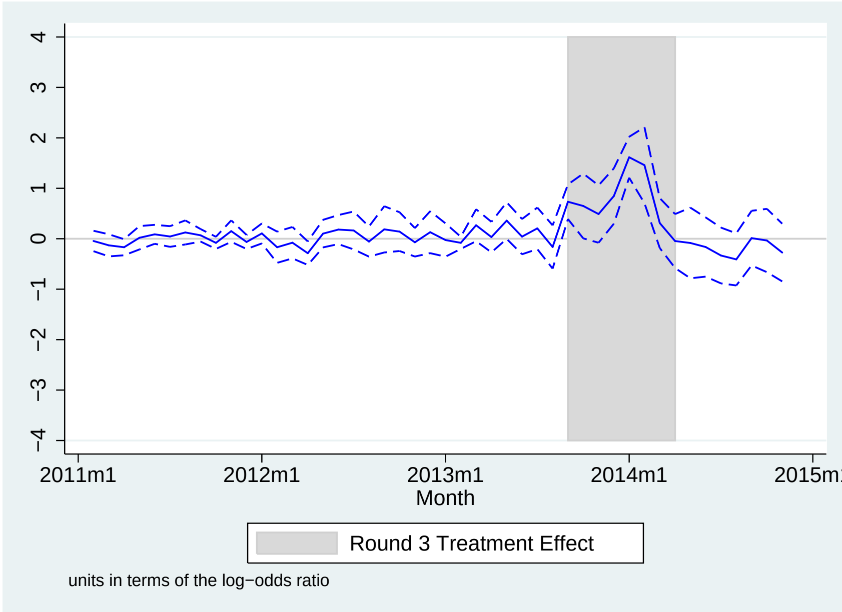

Figure 9: Treatment Effects Over Time.

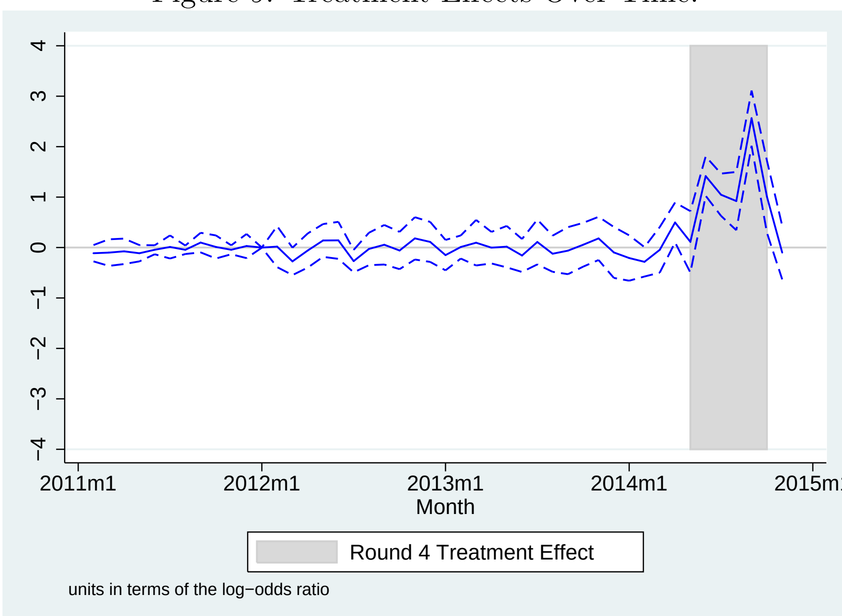

Figure 10: Treatment Effects Over Time.

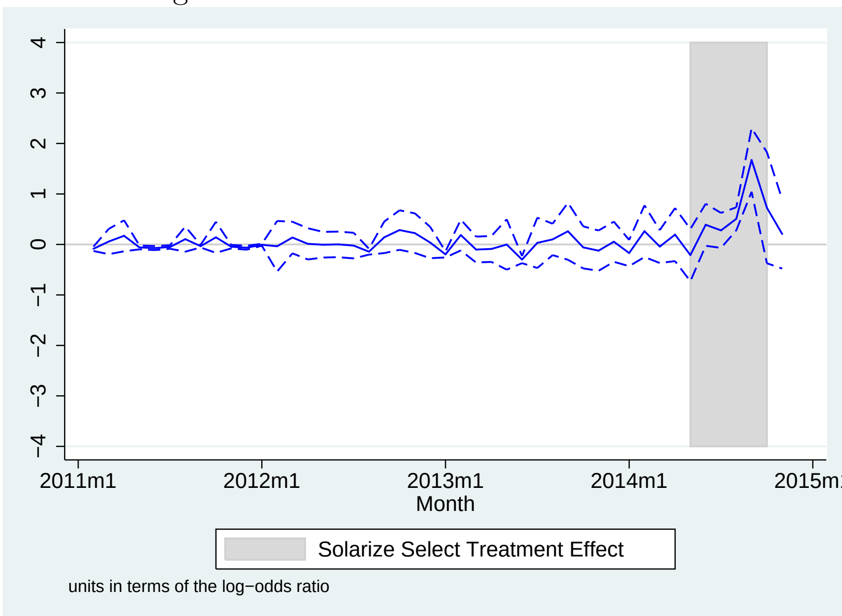

Figure 11: Treatment Effects Over Time.

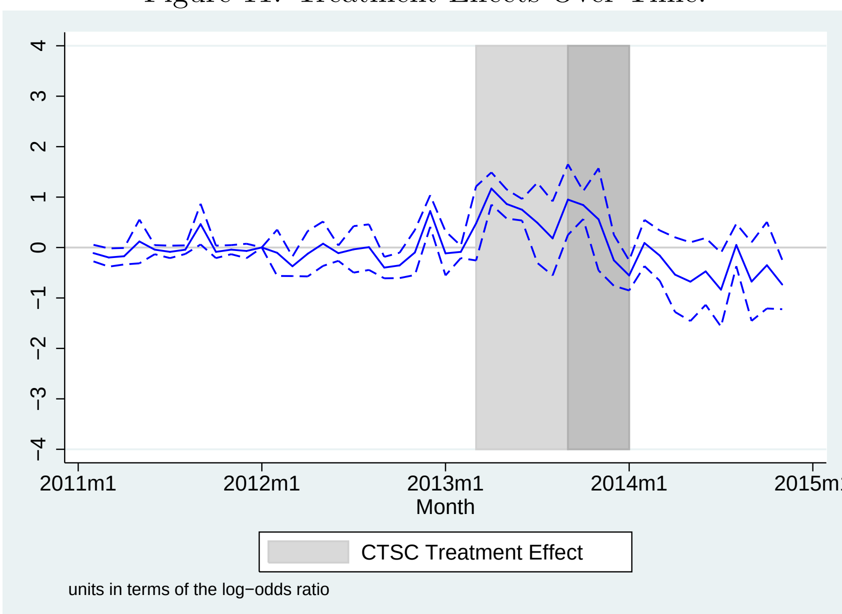

Figure 12: Survey Responses on Factors Influencing the Decision to Install Solar PV.

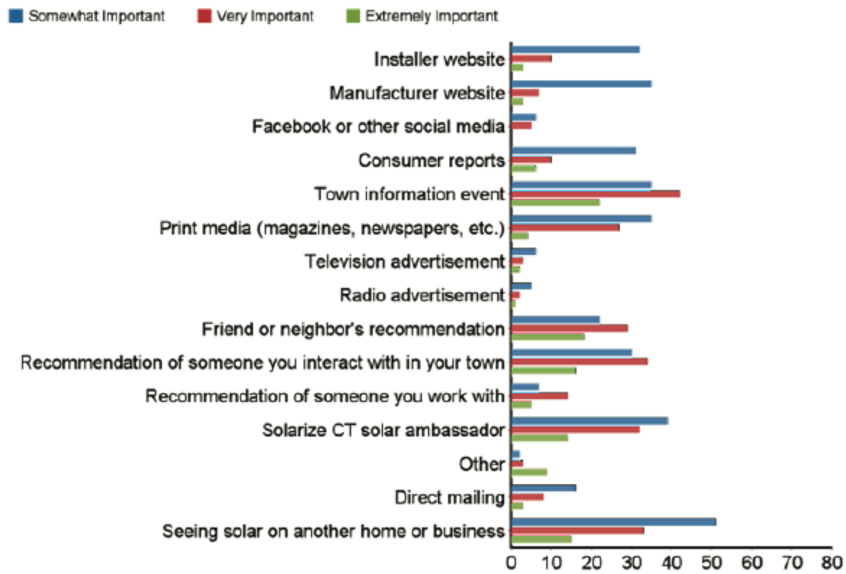

Figure 13: Survey Responses on the Most Important Reason to Install Solar PV.