Long-Run Effects of Competition on Solar Photovoltaic Demand and Pricing *

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Abstract

The relationship between competition and economic outcomes is a first order question in economics, with important implications for policy and social welfare. This study presents the results of a field experiment examining the impact of exogenously-varied competition on equilibrium prices and quantities in the market for residential solar photovoltaic panels. We alter the specifications of a large-scale behavioral intervention by allowing either one or multiple firms to operate through the program in randomly-allocated markets. Our findings confirm the classic result that an increase in competition lowers prices and increases demand, both during the intervention and afterwards. Using the campaign to exogenously shift the long-run number of competitors, we estimate an elasticity of between -0.11 and -0.14 for the effect of the number of competitors on equilibrium prices after the campaigns conclude. The persistence of these effects in the post-intervention period highlights the value of facilitating competition in behavioral interventions.

Keywords: pricing; imperfect competition; diffusion; new technology; energy policy.

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1 Introduction

The study of price competition in imperfect markets has been a fundamental part of industrial organization (IO) since Bertrand (1883).[1] In a comprehensive survey of studies from the 1970s, Weiss (1974) documents a weak, positive relationship between market concentration measures and firm profits (42 of 46 studies found such a relationship, but some found a negative relationship). However, although causal effects of competition and concentration on equilibrium price and quantity are often the desired objects of interest, there are issues in interpreting the results of these studies in this way, as discussed in Schmalensee (1989). It has long been known that there is a bias in many previous estimates of the effect of competition on prices and other market outcomes. These studies focus identification on cross-sectional variation, ignoring the endogeneity of market structure. In a study of price-cost margins in 284 manufacturing industries, Domowitz, Hubbard, and Petersen. (1986) use panel data to demonstrate the bias resulting from focusing solely on such cross-sectional variation.

In his Handbook chapter, Bresnahan (1989) discusses the following shift in the study of market power away from the the structure-conduct-performance paradigm (Bain 1951) using variation across industries to industry-specific studies ranging from banking (Berger and Hannan 1989) to grocery (Cotterill 1986) to airlines (Evans and Kessides 1993). These studies can account for idiosyncratic industry characteristics by using time-series, within-industry variation to identify the effect of of concentration on market outcomes, such as prices and firm profits. Much of the literature in this "new empirical industrial organization" (NEIO) has focused on estimation of market power using oligopolistic models, allowing for the calculation of unobserved marginal costs. And recent advances in estimation techniques have allowed more complex structural models reconciling theory and empirics.

But what allows for identification of the effect of concentration on outcomes? In a study of the airline industry, Graham, Kaplan, and Sibley. (1983) find that the coefficients on their concentration measures change substantially when they are treated as endogenous.[2] Bresnahan (1989) outlines five classes of identification arguments: (i) comparative statics in demand, (ii)

[1] The concept of competition in economics has payed a fundamental role since Adam Smith. However, the explicit and systematic analysis of competition did not begin before 1871 (Stigler 1957).

[2] Exogeneity cannot be rejected but the statistical test they use has low power.

comparative statics in cost, (iii) supply shocks, (iv) econometric estimation of MC, and finally, (v) comparative statics in industry structure. The first two rely on functional form assumptions, the presence of valid instruments (unless endoegneity of market structure is ignored), and the assumption that price and quantity are the only endogenous instruments.[3] The third again relies on structural assumptions, such as those on optimal input expenditures or pricing. The fourth approach is attractive if such shocks are available, but this is often not the case. And finally, the fifth approach assumes price is again predicted by concentration measures, and is subject to the same critiques as the inter-industry studies, now levied at the variation that arises across local markets rather than across industries.

In his review, Bresnahan concludes that "Only a very little has been learned from the new methods about the relationship between market power and industrial structure." More recently, Angrist and Pischke (2010) lament the lack of design-based studies in the field of IO. Einav and Levin (2010) respond to this general criticism of the field by noting that including model structure has yielded many insights, and that the trade-off between acceptable identifying variation and the quality of the measured instrument as a proxy for the object of interest depends on the research question. They also note that "Every researcher would like to first define an object of interest and then design the perfect experiment to measure it."

This is the approach we take. There are clear, inherent issues in identifying causal relationships between competition and market outcomes. However, such relationships are still of principle interest to economists and policymakers. Energy, housing, and health are some specific domains in which the role of concentration is of paramount importance, both because it may result partially from the regulatory environment and also because the welfare implications are enormous. In the housing market, Favara and Imbs (2015) use an exogenous exogenous expansion in mortgage credit to identify its effect on prices, but they still use HHI as an exogenous control variable. Dai, Liu, and Serfes (2014) study the effect of concentration in the airline industry on price dispersion using variation in route-level HHI. They instrument for HHI using route end-point Metropolitan Statistics Area (MSA) populations and the total passengers enplaned on a route as done in Borenstein and Rose (1994) and Gerardi and Shapiro (2009), but this assumes (somewhat dubiously) that the only effect of these variables on price dispersion is through firm

[3] Evans and Kessides (1993), for example, uses share rank an as an IV for share, while treating HHI as exogenous.

entry. And Trish and Herring (2015) study the effect of Insurer-Employer, Hospital, and Insurer-Hospital HHI all on insurance premiums, taking variation in these concentration variables as exogenous. In residential PV, Gillingham, Deng, Wiser, Darghouth, Barbose, Nemet, Rai, and Dong (2016) actually document that in areas of higher concentration, prices are actually lower, indicating that endogeneity bias can even reverse the sign of the expected effect (this could easily be due to economies of scale and/or learning-by-doing, as found in Bollinger and Gillingham (2016)).

We extend this literature by experimentally varying the amount of compeition in order to identify the causal effect of competition on equilibrium prices and quantities in the realm of residential solar PV. We do so by experimentally varying the number of competitors allowed to operate within a large-scale behavior intervention, Solarize Connecticut, CT. While in the standard program, municipalities choose a single solar PV installer, alternative treatment designs include three or more installers. This unique program feature allows us to test for the impact of increased competition directly. For our estimation, we rely on detailed administrative data managed by the Connecticut Green Bank (CGB) which includes the universe of all grid-connected solar PV installations in CT and contains detailed information on installation date, system size, system type, installer, price, and incentives. We find that having multiple selected installers for the campaign leads to an average price drop during the campaigns about twice the size as during the single installer campaigns. We further find that the increased competition during the campaigns leads to long term increases in the number of active installers by 2-4 installers (an increase of 50-100%) and a long term price decline of \$0.18/W - \$.28/W. This is a decline of between \$0.08/W and \$0.09/W per extra installer, an elasticity between -0.11 and -0.14.

This study relates to several strands of literature. We test a classic IO theory by evaluating the impact of market structure on equilibrium outcomes (Tirole 1988, Geroski 1995). Despite the recent advances in empirical IO (Einav and Levin 2010) allowing for the estimation of structural models of competition (Bresnahan and Reiss 1991), such models still rely on specific structural assumptions in order to uncover the impact of competition on market outcomes.[4]. Other studies rely on lab experiments to uncover the impact of competition on market outcomes (Dufwenberg

[4]For example, in the airline (Berry 1992, Goolsbee and Syverson 2008), car and car rental (Bresnahan and Reiss 1990, Singh and Zhu 2008), and supermarket industries (Jia 2008, Holmes 2011)

and Gneezy 2000, Aghion, Bechtold, Cassar, and Herz 2014). Although such approaches can help test different compeitive theories in a lab environment with student lab subjects, we would expect very different results in real market settings in which the decision makers are actual firms with long-term, economically meaningful ramifications tied to their decisions.

This paper makes two important contributions. First, by conducting a large-scale field experiment (Harrison and List 2004), we exploit the exogenous increase in the number of installers to test for the impact of competition on equilibrium prices and quantities under different settings, following the empirical strategy promoted by Angrist and Pischke (2010).[5] To the best of our knowledge this is the first attempt to answer this classical question in economics in the non-development context, allowing the use of detailed administrative data. The use of randomized field experiments to analyze the impact of competition on market outcomes is usually not possible given the high costs, logistical challenges, and political feasibility of manipulating competition. Second, as solar PV panels are heavily subsidized, our study makes an important policy contribution, pointing towards the role of supply side market power when it comes to technology diffusion.[6] Effective market design that aims at increasing competition will lead to lower equilibrium prices and higher quantities and hence will result in larger adoption of solar panels with benefits for greenhouse gas abatement.

The paper is structured as follows. The next section describes the experimental setting. In Section 3, we develop a simple model to highlight the anticipated effects from increased competition. In Section 4, we describe the data, followed by descriptive evidence of the effects of competition in Section 5. Section 6 introduces the main regression model and 7 discusses the results. We include a general discussion in Section 8, and Section 9 concludes.

2 Experimental Setting

With the support of the CGB, The John Merck Fund, The Putnam Foundation, and a grant from the U.S. Department of Energy, six rounds of Solarize CT have been run. These campaigns are

[5]The present paper also relates to the growing literature in field experiments in economics and behavioral policies in energy markets (see for example Allcott and Mullainathan 2010, Allcott and Rogers 2014)

[6]Evidence from post-campaign surveys point to the price as the most important factor influencing the solar purchase decision.

very resource intensive, costing on the order of $30,000 per campaign. The Solarize campaigns have several key pillars. Treated municipalities choose a solar PV installer with whom to collaborate throughout the campaign. In order to be selected and following a request-for-proposals (RFP), 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. 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 (see Gillingham and Bollinger 2017, for details).

Each of the rounds included Solarize Classic campaigns, but in two of the rounds–the third (R3) and fifth (R5) – we experimented with two additional treatments. In the first field experiment, we examined the impact of campaigns with more than one focal installer on the price and quantity of solar installations during the campaign and in the post-campaign period. During round 3 (R3) we randomly assigned towns to either be in the single focal installer 'Classic' version of Solarize, or the 'Choice' version, that chooses up to three installers per participating municipality. A total of 11 towns were assigned to the Classic program, while 6 towns received the Choice treatment. The Classic and Choice campaigns started with a time lag of up to two month due to implementation constraints.

The second experiment occurred during round 5 (R5). In this experiment, we randomized across 11 municipalities: seven Classic Solarize towns, which had a single installer, and four 'Online' campaigns which used an Internet platform (EnergySage) in which a multitude of vetted installers actively bid for customers.[7] Table A.1 provides a list of all towns and campaign dates.

As Solarize installers receive extraordinary attention during the program duration, they end up with temporal market power.[8] We exploit this exogenous increase in the number of competitors that will enter consumers' consideration sets to test for the effect of competition on

[7]The Online campaigns did not involve a focal installer, rather interested customers had to sign up to the webpage EnergySage where they received price bids from competing installers.

[8]Even though the number of active installers are fairly comparable in the pre-campaign period for the three treatments, selecting a single installer in Classic resulted in an average market share of 89.9% during the five-month campaign window in the first intervention period (round 3) and 68.3% in a second intervention (round 5). Other installers are not excluded from the market but are at a significant disadvantage during the program.

equilibrium outcomes. As Solarize campaigns have been rolled out in a consecutive fashion in several rounds, for each treatment variation (Choice and Online), we have a set of municipalities that receive the Classic treatment concurrently. Furthermore, we can compare the outcomes in the treated municipalities to a group of control towns with similar observable characteristics that engage in green energy programs, but have not been part of Solarize.

We use the set of Clean Energy Communities (CEC) that had not yet participated in a Solarize program or the CT Solar Challenge (an installer-run version of Solarize) as a natural control group and examine the validity of these control groups below. The CT Clean Energy Communities are towns that have expressed particular interest in clean energy and have formed a committee or task force on the topic. Nearly all Solarize towns are drawn from the CT Clean Energy Communities.

2.1 Program Details

The Solarize CT program is a joint effort between a state agency, the Connecticut Green Bank (CGB), and a non-profit marketing firm, SmartPower. The first critical component to the Solarize program is the selection of a focal installer. Solarize campaigns are usually built around one trusted installer that collaborates with the volunteers and the non-profit in the implementation of the town events.

The second major 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 and influencing behavior (Kremer, Miguel, Mullainathan, Null, and Zwane 2011, Vasilaky and Leonard 2011, BenYishay and Mobarak 2014, Ashraf, Bandiera, and Jack 2015, Kraft-Todd, Bollinger, Gillingham, Lamp, and Rand 2017).

The third component 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, ads in the local newspaper, and even individual phone calls by the ambassadors to neighbors who have expressed interest.[9]

[9] 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.

A final 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. With the group pricing comes a limited deal duration for the campaign. The 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. In an additional R5 condition, Gillingham and Bollinger (2017) found no significant effect of group pricing on average prices or the number of installations.

The Solarize program is thus 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. A more detailed description on the Solarize program can be found in Gillingham and Bollinger (2017). The standard timeline for a Solarize 'Classic' 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.
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.

3 Model

We develop a stylized model to guide our empirical analysis, which will also help with interpretation of the results. The two main effects of a Solarize campaign are to increase demand and to decrease (almost eliminate) installers' customer acquisition costs. These effects lead to shifts in both the demand and supply curves during the campaign. After the campaign, the installer once again is responsible for its own customer acquisition and the supply curve shifts back. In addition, demand falls in the post-period, but not necessarily entirely back to the pre-campaign demand curve. For example, the installers may have picked the low-hanging fruit of customers most excited about solar. If some harvesting occurred, we expect post-campaign prices and quantities to be lower.

In the case of several installers (Choice and Online), the number of installers in consumers consideration sets is larger due to the experimental manipulation. If this effect on consideration set sizes remains in the post campaign period, we would expect increases in consumer price sensitivity (for a partial installer) as shown in Ellison and Ellison (2009) in the context of search engine rankings. This would lead to lower optimal long run prices for installers, as the below model demonstrates.

Let consumer i's utility for solar PV from installer j at time t follow a random utility model as follows:

$$u_{ijt} = \alpha P_{jt} + Xjt\beta + \epsilon_{ijt} \tag{1}$$

The share of consumers purchasing a solar installation from firm j is given by:

$$s_{jt} = \int \Phi_{\epsilon_j|\epsilon_{-j}} \left(\alpha p_{jt} + Xjt\beta - \max_{k \in \mathbb{C}} u_{ikt} \right) d\Phi_{\epsilon_{-j}}(\epsilon_{i-jt}) \tag{2}$$

in which $\Phi_{\epsilon_{-j}}$ is the cumulative distribution function of shocks for installers other than j in consideration set $\mathbb{C}$, and $\Phi_{\epsilon_j|\epsilon_{-j}}$ is the resultant conditional distribution of ϵ_{ijt}.[10]

A profit maximizing installer sets price to maximize:

$$\pi_{jt} = (p_{jt} - c_{jt}) s_{jt} M_t \tag{3}$$

in which c_{jt} is the marginal cost of installation and M is the market size. Given the large treatment effects found in (Gillingham and Bollinger 2017), we conceptualize the main demand effect

[10]This specification allows for an arbitrary correlation structure in the error terms across installers and across time.

of Solarize programs as increasing the number of consumers considering solar, i.e. an increase in market size M, which would occur if the potential market expands beyond "innovators" to "early adopters".[11]

The first order pricing condition is:

$$p_{jt} = c_{jt} + \frac{s_{jt}}{\frac{\partial s_{jt}}{\partial p_{jt}}} \tag{4}$$

The effect of the Solarize programs affect both these terms. On the cost side, the Solarize program shifts customer acquisition costs from the installers to SmartPower. This leads to a shift to the right for the supply curve during the campaigns, leading to a further increase in quantity and decrease in price. We expect this to affect all participating installers. Post-campaign, installers are again responsible for their own customer acquisition costs. We would expect c_{jt} to return to similar levels as in the pre-campaign period, although there could be modest long-term effects as well if there are capacity constraints, local learning-by-doing, or permanent declines in customer acquisition costs.

The second affect occurs in the markup term. The main demand effect of increasing M during a Solarize program leads to higher demand and higher profits. The optimal price will be unaffected if installer share and share elasticities are unaffected, although both may be affected if the increased market size leads to a change in the utility parameters for the representative consumer. However, both will certainly be affected by increasing the number of Solarize installers in the Choice and Online campaigns. Given (2), $\frac{s_{jt}}{\frac{\partial s_{jt}}{\partial p_{jt}}}(.)$ is a negative, decreasing function in the expected utility difference between the installer, j, and the alternatives. Thus increasing the size of the consideration set (in Choice and Online relative to Classic) decreases the optimal markup. Intuitively, the fact that the installers must split the market leads to lower markups and lower resultant prices. After the program concludes, if the effect of introducing more competition into the market leads to a sustained increase in consumers' consideration sets, we would expect a long term effect leading to lower post-campaign equilibrium prices.

In sum, the effects of Solarize during the campaigns will include 1) A decrease in customer

[11] An alternative would be to assume that the Solarize program increases the utility of solar, rather than the market of potential adopters, but the implied monetary equivalent of the utility increase needed to explain the treatment effects found in Gillingham and Bollinger (2017) make this explanation far less likely. The implications of this model hold regardless of which interpretation is preferred.

acquisition costs; 2) An increase in market size M during the campaign (and possible a change in the preferences of the representative consumer); and 3) an increase in consideration set sizes from Choice and Online, which leads to lower optimal markups during and post campaign for the Choice and Online towns, relative to the Classic towns.

After the campaigns conclude, we might expect a change in marginal cost for focal installers through capacity constraints, local learning-by-doing, or permanent declines in customer acquisition costs. If this were the case, there would be a long-term price effect in the post-period for all campaigns. There is no reason to expect a different cost impact for Choice and Online relative to Classic.

It could also be the case that any long-term effects would be related to the markup term. The increase in consideration set sizes from Choice and Online leads to lower optimal markups during and post campaign for the Choice and Online towns, relative to the Classic towns. It would also lead to price effects for all installers in the market, whereas if the effect of adding competition worked through long-term effects on marginal costs, we would expect the focal installers to have larger long-term price effects.

If the optimal makeup does change due to the increased competition, in addition to the price effect, shares for installers will be smaller, and there will be more competition in the marketplace and less market concentration, all testable implications. The lower prices will also lead to higher post-campaign demand relative to the single installer campaigns, in which prices may return to pre-campaign levels or even increase if capacity constraints lead to temporary increase in the marginal cost c_{jt}. One reason for such long lasting effects on consideration sets include word-of-mouth (WOM) in which consumers refer their installers to others', or through a decrease in market level marginal costs for installers who have previously operated within that market.

We illustrate these effects in Figures 1 and 2, using simple linear demand and supply curves for illustration purposes only. In Figure 1(a), we show the shifts in demand and supply from a Classic program which results in a dramatic increase in equilibrium quantity and a price effect, as determined by the relative changes to the supply and demand curves. In the post period, the installer once again is responsible for its own customer acquisition and so we draw the supply curve as the same as in the pre-campaign level, as shown in Figure 1(b) which would be the case if there was no change on the optimal markups (and no large effect on marginal costs in the short term for the focal installer, leading to lower short term competition). Of course there might be some long-term supply shift, but it is an empirical question.

In Figure 2, we show the expected effects of a Choice or Online campaign. All of the focal installers experience the reduction in their acquisition costs. For simplicity, we assume the same effect of the campaign on total demand in Classic. But because of the increased size of consumers' consideration sets, the installers will price lower relative to the Classic program, and they will split the total demand during the campaign. The resultant equilibrium price is lower than in Classic, and the quantity is higher as well due to the greater shift in the aggregate supply curve. In the post period (Figure 2(b)), increased consideration set sizes will lead to lower equilibrium post-solarize prices as well, and higher quantities relative to Classic because of a permanent shift to the right of the supply curve.

Our model leads to clear testable implications: if allowing multiple installers to operate within the Solarize program does indeed increase consumer consideration sets sizes by increasing the number of considered installers, then we should see long lasting effects of these short-term campaigns. The post campaign effects include an increase in the number of active installers, a decrease in HHI, a decrease in price for all installers, and an increase in demand (all relative to that found in the Classic Solarize program).

4 Data

The primary data source for this study is the database of all solar PV installations that received a rebate from the CGB, 2004-2016. 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 in the database.[12]

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. so-

[12]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.

lar lease or power-purchase agreement), and additional system characteristics. For Online, we also have the bid data for the 327 customer requests (call for bids) submitted by potential solar customers through the *EnergySage* platform.[13]

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 (SOTS 2015). We match the installation data with the demographic and voter registration data at the census tract level. Finally, campaign participants are sent an online survey after the conclusion of the campaign.[14]

We standardize the time period that we include in our analysis relative to the Solarize campaign starting dates. For each campaign, we consider 24 month pre Solarize, the five month Solarize campaign, and 12 month post campaign period. This approach allows us to compare the treatment effects across campaigns relative to the Solarize interventions, and allows us to test for persistent market outcomes (see also Allcott and Rogers 2014). For round 3 we consider the full set of untreated clean energy communities as control towns.[15] For round 5, we exclude the 10 largest towns, resulting in 45 control municipalities. This adjustment ensures that we are indeed comparing similar towns as treatment towns in later rounds have been slightly smaller. As robustness, we verify that the choice of control towns does not impact our main results.

In order to assess the trends in prices and quantities, Figure 3 shows the cumulative uptake of installations as well as the mean prices (third-party owned installations excluded) for the entire sample period in the three groups. Both treatment groups as well as the control group have parallel pre-treatment trends.[16] The appendix (Figure A.1) also presents histograms of the main dependent variables. Town demographics for treatment and control groups one year prior to the

[13]Note that in Online the conversion rates have been lower than in other type of Solarize campaigns, about 20% (67 contracts signed).

[14]Due to the community engagement, response rates for solar adopters were unusually high (close to 40%). In addition we surveyed non-adopters, as well as performed personal interviews with the group of solar ambassadors.

[15]CT Clean Energy Communities make a "Municipal Pledge" to save energy and voluntarily purchase renewable energy.

[16]In line with the visual inspection of the pre-treatment trends, we test for equal means of the first-differences one year prior to Solarize and do not find evidence for statistic differences between the groups.

Solarize intervention are presented in Tables A.2 and A.3. The tables show that the distribution of key demographic variables across treatment and control towns is indeed very similar.[17]

5 Descriptive Evidence

5.1 The effect on the level of competition

Using the exogenous change in the number of selected Solarize installers leads to important experimental variation in the amount of competition in the market. Table 1 provides evidence on the average market concentration and the number of active installers per municipality in five-month intervals relative to the Solarize campaign timing. The median number of bids in Online is 3.01 with standard deviation 1.36, and so the number of active Solarize installers is quite similar for both Choice and Online.

Focusing first on the period 'during Solarize', we find that both Choice and Online lead to a significant increase in active installers per town relative to the Classic campaigns. This is in line with the experimental design, allowing for a larger number of selected installers. As expected, we also see that the number of active installers remains higher in the five-month period after the policy intervention.

In order to first assess the differential effect of adding more competition on market concentration, we look at the means of the normalized Herfindahl-Hirschman Index (HHI) in the same time periods. While Choice and Classic municipalities show very similar market concentration in the pre-period (panel a), the single focal installer in Classic leads to a dramatic increase in concentration (an HHI of 0.63 compared with 0.19 in Choice towns). As predicted, we find that Choice towns have more active installers in the post period relative to Classic towns, which also leads to lower market concentration.

R5 reveals slightly different numbers. As the overall market has matured importantly in 2014-15, we see a larger number of active installers to begin with. Yet, similar to the case of Choice, Online leads to a significantly larger increase in the number of active installers during the campaign compared to Classic. This results in lower market concentration. On the other

[17] Statistical differences for key demographics can be only found for household income in Table A.2, and for the share of homeowners and number of registered republican voters in Table A.3.

hand, the market concentration indices in the post-period are very similar for Online and Classic. This could be a floor effect, which results from how low the concentration is in the post-period for both campaign types. Online still leads to a significantly larger increase in the number of active installers post-campaign relative to Classic.

5.2 The effect on shares and prices

In terms of market shares, the shares of focal installers increased in R3 Classic by 28% and in Choice by 81%, relative to their pre-Solarize shares. The greater competition in Choice clearly led to a larger post-campaign increase in focal installer market share. SolarCity shares increased similar for both campaign types, by 23% and 25%, respectively.

In R5, we see a similar effect from introducing more competition during the program. In Classic, the post campaign share for the focal installers increased 52%, relative to 175% for Online installers. Unlike in R3, SolarCity shares *decreased* as a result of both, by 31% and 42%, respectively. In contrast, SolarCity shares increased for the control group in R5. Thus, the increase in focal installer share during R5 for both Classic and Online came at the expense of the now established market leader, SolarCity.

Descriptive evidence of the heterogeneous impact of distinct Solarize interventions on system prices are given by Figure 4 which shows the mean price of solar installations by the type of system financing. Solar installations are either purchased, financed though a loan, or installed with third-party ownership either through a lease or power purchasing agreements (PPA). Since the type of financing can have an important impact on the cost per Watt of a solar installation, Figure 4 compares average prices by type of financing. The figure shows the average price as well as standard deviation for R3 (Choice vs. Classic vs. Control) and R5 (Online vs. Classic vs. Control) in five-month intervals relative to the Solarize campaign. Panel (a) reveals that both Classic and Choice led to an important drop in prices during the campaign for Lease, Loan, and Purchase products. Moreover, purchase prices remain low in the post-campaign period. PPA installations, on the other hand, show very little price movement. Panel (b) of the same figure compares prices for R5 and tells a similar story. Overall we find that the Solarize campaigns lead to an important price decrease during the campaign, with a larger drop for Choice and Online compared to Classic. These insights are in line with our theoretical predictions. The effect for post-campaign prices is not as clear when comparing unconditional means.

Figure 5 provides insights on the pricing of Solarize installers versus competitors in the same towns; clearly, the focal installers in Solarize towns are more competitive to begin with.[18] However, the price differences can be partially explained by the financing composition of these groups, as it is mainly focal installers that sell purchased and financed installations, while some competitors engage in the PPA market.[19] Comparing differences within groups over time, an increase in competition seems to be not only related to a larger drop during the campaign, but a prolonged drop in the post-period. Finally, the price of non-focal installers in Solarize towns reflect closely the price of solar installations in control towns.

Another explanation for the post-campaign price differences is that is that the type of Solarize campaign affects the composition of product financing or size of installations. To assess this, Figures A.3 and A.4 present the mean financing shares as well as size distributions for the different campaigns. While Figures A.3 shows clearly that Solarize lead to more purchase-financed installations during the campaign, post-campaign financing shares are similar across all campaign types and the control group. More importantly, different type of Solarize campaigns did not lead to different financing shares.

Fig A.4 shows that the distribution of system sizes in the post period in R3 was not affected by Solarize, although system sizes were slightly larger during the campaigns. In R5, the sizes are slightly larger in the pre period. In our regression framework, we condition on the type of system financing, system size, and system mounting when estimating the main price effect.

6 Empirical Specification

To quantify the equilibrium impact of increasing competition on equilibrium prices, we regress the installation-level cost of solar PV on treatment dummies, a rich set of individual controls as well as time and municipality fixed-effects. The regression equation is:

$$\mathrm{Price}_{imt} = \alpha + \delta_s T_{mt} + \gamma_s P_{mt} + \boldsymbol{\beta X} + \theta_m + \psi_t + \epsilon_{imt} \tag{5}$$

[18] As installers actively bid for towns this insight is in line with the Solarize design.

[19] A.2 in the appendix shows the shares by type of financing.

where Price_{imt} represents the price ($ per kilowatt) of solar installation i, in municipality m, at time period t. T_{mt} is a treatment dummy variable and P_{mt} is a post-treatment dummy.

The effects of the treatments during the campaigns and afterward, δ_s and γ_s, depend on the type of Solarize campaign, indicated with the s subscript. The regression includes municipality and month fixed-effects (FE) to account for both time-invariant differences across municipalities and aggregate shocks to the CT solar market. The control vector $\mathbf{X}$ includes observable characteristics of the solar installations such as system size, type of system mounting and system financing.[20] All standard errors are clustered at municipality level, in order to account for error correlation within the same municipality over time. We estimate equation (5) for each round separately. We are interested in comparing the treatment effects and post-treatment effects for Choice and Classic in the R3 regression as well as Online and Classic in R5.

To better study the adjustment dynamics, we estimate a variation of (5). Using an event study design (similar to Gallagher 2014), we introduce a full set of time dummies that is allowed to vary by group (Classic, Choice or Online, Control) and estimate the price impact relative to the Solarize intervention.[21]All results are relative to the two-month period pre-intervention.[22]. Besides, these alteration, the regression model contains all additional variables that have been used in specification (5).

6.1 Quantity regression

An additional outcome of interest are the equilibrium quantities sold in each market. For this purpose, we aggregate our data at the monthly level and estimate a classical difference-in-difference (DiD) estimator to test for the impact of competition on quantities sold. The quantity

[20]For robustness, we experiment with different time fixed-effects (quarterly, annual combined with monthly dummies). The main findings are robust to the choice of FE.

[21]As there are certain month with zero installations, we include one separate dummy for each two-month interval. Moreover, as there have been few installations in the first months of our sample, we group these early installations (24 month to 13 month prior to the campaigns) into a single dummy.

[22]This category is omitted from the regression.

model is estimated both by ordinary least squares using a difference-in-difference mode[23]:

$$\mathrm{Inst}_{mt} = \alpha + \delta_s T_{mt} + \gamma_s P_{mt} + \boldsymbol{\beta X} + \theta_m + \psi_t + \epsilon_{mt} \tag{6}$$

where Inst_{mt} is the total number of new solar installations in municipality m, at time t. As in (5), the model includes both municipality and month fixed-effects (FE). Our main interest again lies in the comparison of treatment (δ_s) and post-treatment (γ_s) coefficients. To better understand the dynamics, we follow the same event study design approach as outlined above. Moreover, as the main dependent variable in (6) is count data, we estimate the model both by ordinary least squares and by fitting a negative binomial count data regression.

7 Results

7.1 Impact of competition on equilibrium prices and quantities

7.1.1 Prices

The main regression results from equation (5) for both R3 and R5 are displayed in Table 2. Column 1 estimates the model without control variables, while column 2 includes controls for system financing, system size, and system mounting. The reference category are purchase-financed installations.

We find that controlling for individual system covariates is important to obtain precise (unbiased) treatment effects, which highlights the importance of using detailed micro-data in the analysis. Focusing on the R3 results in column 2 with size, mounting and financing controls, we show that while Solarize Classic leads to a price decrease of about 29 cents per watt installed, the price impact of Choice is considerably larger at 49 cents per Watt. While the post-campaign dummies are not significant relative to the control group, an F-test establishes that the post-campaign prices after Choice are significantly lower than after Classic ($p = .027$). One explanation in the suggestive increase in prices relative to the control is short term capacity constraints as result of the campaign due to frictions in labor supply, which is consistent with anecdotal evidence we have collected from speaking with installers.

[23] We will show robustness to the use of a negative binomial count model.

Columns 3 and 4 display the main results for R5, comparing Online and Classic. Without the controls, we find similar results as in R3, but controlling for system size, mounting and financing shows that the interventions in R5 did not have a price impact during the campaign. Again, whether there is an equilibrium price effect is an empirical question and is determined by the relative shifts of the supply and demand curves. This indicates that the demand curve shifted more in R5, or the supply curve shifted less. Similar to Choice in R3, we find that there is a significant effect in the post campaign period with Online leading to lower prices than Classic ($p = 0.04$).

As it has been pointed out, the CT solar market has evolved importantly between R3 and R5 and new types of financing were beginning to increase their market shares. As the overall market is more competitive, we see smaller price drops due to Solarize. It is thus not surprising that the additional competition led to only an 18 cents per kW drop in Online relative to Classic in the post-solarize period, in contrast to the 30 cent difference between Choice and Classic in R3.

Although our outcomes of interest, the price of solar installations, are at the individual level, since Solarize campaigns are randomized at municipality level we use clustered standard errors in order to not overstate the precision of our estimates. That said, our results still rely on asymptotic arguments ti justify the normality assumption. We can address this concern using randomization inference (Fisher 1935, Rosenbaum 2002). Randomization inference (RI) has found increasing attention in studies dealing with small sample size (see for example Bloom, Eifert, Mahajan, McKenzie, and Roberts 2013, Cohen and Dupas 2010). The main advantage of RI is that it does not need any asymptotic arguments or distributional assumptions.

In order to test for the causal impact of the Solarize treatment, we group our data in different sub-samples to perform each pairwise comparison for Classic, Choice (or Online), and the control group, for example keeping only Solarize Classic and Control municipalities for Round 3. We then randomly assign treatment status at the municipality level and re-estimate our original regression model (5). The two coefficients of interest are the main treatment effects and the effects in the post-intervention period.[24] In Table 3, we report the estimated coefficients using each subsample with the clustered standard errors, as well as the results of the randomization inference. These findings confirm our main results, showing that Choice and Online towns led to significant lower prices both during and post-campaign compared to the single installer case. As an alternative, we also address the small sample sizes using the wild cluster bootstrap, discussed more in the robustness checks.

We perform a series of additional robustness checks concerning our main results. In particular, we limit our sample to cash-purchased installations only.[25] We provide additional robustness checks for the impact of focal installers as well as the main economic specification. Finally, we experiment regarding pre- and post-periods and concerning the group of included control municipalities. Our main results are robust to these robustness checks. We further confirm the significance of our findings using the wild cluster bootstrap which also does not rely on asymptotic arguments.

7.1.2 Quantities

In order to better understand the equilibrium price response to increased competition, we need to also assess what happens to quantities. To that purpose we estimate model (6), aggregating the data at municipality-month level. Columns 1 and 2 of Table 4 shows the main treatment effects for Classic and Choice in R3. While column 1 does not control for any additional covariates, column 2 includes the share of financing to control for compositional effects. In both specifications, we find that Choice leads to 1.3 to 1.5 additional monthly installations during the campaign (approximately 7 installations for a five-month campaign), which represents an increase of roughly 25% when evaluated at the mean number of monthly installations during Classic.[26]

If we assume that the shift in the demand curves are comparable across campaigns in R3,

[24] We simulate 1000 data draws and perform a left-tail test, comparing the simulated coefficients to our original estimates. The p-value is given by the number of simulations that lead to smaller treatment effects as the original sample divided by the number of simulations.

[25] The cost information for leasing products as well as loan-financed installations might contain measurement errors as this information cannot be perfectly observed by the CT Green Bank.

[26] The total number of installations in Choice are displayed in Table A.1. On average, there have been 6.86 new installations per month of Solarize.

then the price and quantity changes in Choice vs. Classic in R3 give us two points on the demand curve (exploiting the experimental shift in the supply curve). The implied price elasticities are -4.6 during the campaign and -3.6 after the campaign. These are higher than that found by Gillingham and Tsvetanov (2015), implying that the campaigns may have indeed expanded the market to include more than just the less-elastic "innovators" in the market. Since all elasticities are estimates for the marginal consumers, this is clear evidence that the marginal consumer has changed as a result of the campaign. This also supports our proposed interpretation of the quantity treatment effects from Solarize found in Gillingham and Bollinger (2017) as an expansion of the market.

These points are subject to the caveat that the quantity effects are imprecisely estimated. Even though the points estimates are large, we cannot reject the null hypothesis of equality of coefficients. We also find evidence that Classic leads to some harvesting (negative and significant coefficient in both specifications in the post period), while this is not true for Choice. We hence find that increasing competition in the Choice campaigns leads to more product sales during the campaigns and more importantly there is limited evidence for harvesting in the post-campaign period. It could be that this is also reflecting the impact of the long-term price effect we found for Choice relative to Classic in the post periods.

Columns 3 and 4 of Table 4 present the estimation results for Classic and Online. While Classic led to about 5.5 additional installations during each campaign month , Online was responsible for only 3 additional installations. Note that the overall market expansion from R3 to R5 has led to more solar uptake in the state, so that the quantity effects for Classic are comparable across rounds. Given the very different nature of the online campaign, the demand curve shifts clearly are different for Online and Classic, making the elasticity calculation impossible. One obvious question is why did Online lead to little additional sales if prices were lower? We explore this question further using the survey data below.

7.2 Effect persistence

Another way to look at the main cost dynamics without making any assumptions about the precise campaign timing[27] is to use the time dummy approach as explained in Section 6. Figure 6 plots the individual estimates for two-month intervals for the three groups in R3, Classic, Choice, and Control, relative to the Solarize timing. The figure shows both point estimates and the 95 % confidence bands. The two month period prior to the Solarize intervention are omitted from the regression. Panel (a) shows clearly that while prices in Control towns have been not affected by the Solarize intervention, both Classic and Choice led to a drop at the beginning of the campaign period. However, while prices in Classic stayed similar relative the baseline, Choice stayed at a lower level for the entire year post-period. Panel (b) shows the same dynamic for Classic versus Online in R5. round 5. Although prices co-moved for most of the periods (prices in Online were slightly lower during the campaigns), Online prices dropped even further around the five-month post-campaign point, possibly due to the free entry of competitors.

In line with the price regression, we estimate a variation of model (6), including separate two-month time dummies for each of the campaigns to better analyze the dynamics (i.e. estimating time x campaign type interactions). Figure 7 plots the point estimates and 95% confidence intervals for the impact of Solarize on equilibrium quantities. Panel (a) shows clearly that in the pre-intervention period, Classic and Choice towns had very similar uptake rates, and only three months into the campaign do people in the Solarize Choice municipalities start installing more solar panels. This larger effect is persistent in the post-period, and only one year after the campaign do the number of new installations converge. Panel (b) of the same figure shows the quantity dynamics for R5. In line with our main regression, we find that Online leads to less additional uptake. Interestingly, the main difference between the campaigns occurs in the last months of the campaign, indicating that the single installer in Classic did a better job in converting sales leads.[28] The post-campaign period was unaffected (in terms of quantity) by the

[27] Some installations might happen after the official end of the Solarize campaign, but might still receive the price-benefits of Solarize.

[28] The particular rough winter 2014/15 led to cancellation of site visits and to postponement of scheduled events. This partially explains the lower uptake in R5 compared to R3. Moreover, in Classic, the single installer had a greater ability to make a sales push in the last months of the campaign and to recover sign-ups. The setup of Online did not allow for this direct sales interaction.

campaigns. Finding no evidence for a significant quantity response in the pre-treatment time dummies provides a first robustness-check ruling out consumer anticipation of the campaigns.

7.3 Effects by installer

Our proposed mechanism behind the differences in prices between Classic programs and those with more competition is that when there are more program installers, consideration sets are increased (permanently). This leads to a smaller difference in consumer utility in the post-campaign period between the focal installer(s) and the non-foal installers, because the focal installers in Choice and Online have to split the market, which leads to smaller optimal markups (relative to having a single focal installer). It is not clear whether the observed price decrease has been driven by focal installers alone or if the Solarize campaigns have led to an overall price changes in campaign municipalities. Table 5 shows the additional price effects for focal installer(s), interacting the treatment dummies in main specification (5) with an indicator variable for the focal (selected) Solarize installer.[29]

As before, columns 1 and 2 show the main effects for R3; although no individual point estimates are significant, the size and sign are in line with the main results. The large standard errors can be explained by the small number of focal-installer sales in the pre-and post-Solarize periods (see Table A.1) as well as heterogeneity across markets.[30] In R3, the results are suggestive that prices decline more for the focal installers during the campaigns, which is what we would expect given the reduction in customer acquisition costs. The results also suggest that prices decline more in Choice relative to Classic both during the campaign an in the post period. However in contrast to the effects during the campaign, after the campaigns there is no discernible difference in the prices for focal and non-focal installers. This is more consistent with our proposed explanation of increased competition in the post-Solarize period than persistent declines in costs for the focal installers.

For R5, prices are lower in the post period for the non-focal installers but are actually higher for the focal installers. Part of this could be explained by more binding capacity constraints, but

[29] The number of active focal installer in Choice and Online can be found in Table A.1.

[30] Running the main regression only on the sub-sample of installations done by focal installers leads to a highly singular covariance matrix.

we also see that the price increase by focal installers in the post-campaign period is smaller for Online than for Classic. The most likely explanation is that in R5, the utility of the focal installers increase relative to the other adoption alternatives, leading to an increase in market power and optimal markup. This increase is smaller in Online because all the installers that participated in the program benefited in this way.

A persistent cost decline for focal installers would have implied lower prices for the focal installers after all the campaigns. We see no difference in R3, and higher prices for focal installers after R5. The larger price declines for campaigns with more competition (Choice and Online) also indicate that the likely explanation for the long term price effects is from changes in markups, not costs. To explore why prices actually increase after R5 for the focal installers, it is helpful to examine the consumers' likely alternatives, not purchasing solar or purchasing from the main non-focal installer, SolarCity.

In many locations in R3, the Solarize campaigns coincided with the entry of SolarCity, the largest provider of solar energy services in the US and started targeting the residential solar market in Connecticut in mid 2012. SolarCity is the main provider of PPA's, offering solar installations at zero down payment. If SolarCity focused on specific Solarize municipalities, it would affect the post-campaign market structure.

SolarCity was active in 11 of 16 Solarize markets prior to the start of R3, and all but one at the start of R5. In Figure 8, we compare the mean SolarCity market shares in the pre- and post-campaign periods for different campaigns.[31] In both campaign types and the control group, the share of SolarCity increases from the pre-campaign to post-campaign period. In R5, this is also true for the control group, but the opposite is true for the campaigns. This indicates that the Solarize campaigns in R5 lowered the market share of SolarCity, again providing evidence for a persistent effect on the focal installers' market power which is reflected in their prices.[32] In accordance with our random utility model, the campaigns increased the market power for the focal installers relative to the non-focal alternatives, leading to the increase in optimal price post-campaign, a smaller increase for Online in which consumers still have the choice between multiple participating installers.

[31] Table A.1 shows the market shares of SolarCity in R3 and R5 by town.

[32] We test for equality of market shares using an ANOVA analysis and can reject the null hypothesis of equal market shares across treatments in round 3 and 5.

7.3.1 Survey data

In order to help asses the impact of the Solarize programs, we surveyed solar PV adopters, as well as non-adopters, after each Solarize round.[33] 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 approximately one month after the end of the round, with a follow-up to non-respondents one month later. The overall response rates for adopters was 42.2 percent (496/1,175). This high response rate is a testament to the enthusiasm of the adopters in solar and the Solarize program.

The survey includes several questions concerning customer satisfaction with the solar installation (quality measure) as well as concerning the Online platform. First, we find that on average around 80-90% of adopters report 'being very satisfied or satisfied with their installation', independent of the type of campaign. The very high satisfaction consumers had with their installer during the program, coupled with the peer effects shown in Bollinger and Gillingham (2012) more generally for solar, and in Gillingham and Bollinger (2017) for the Solarize CT programs provides an explanation for why consumer consideration sets would see a long-lasting increase as a result of the 20-week Solarize Choice and Online programs, leading to the long-term price declines. If consumers share their positive experiences with others in the community, all focal installers would be expected to maintain a market presence after the campaign, which is indeed what we find.

For the Online campaigns, we explicitly asked about the role of the platform in leading to larger consideration sets (we did not ask in Choice although the inclusion of multiple focal installers throughout the campaigns at events, etc. ensures it.). We found that 88% of survey participants said they 'liked the option of having distinct installers to choose from', providing strong support for our proposed mechanism. The respondents also reported that the 'website was a useful source of information' (76%) and that 'installers responded timely' to requests (84%).[34] Helping to explain the small treatment effect for quantities in Online compared to Classic, respondents noted that although the website itself was 'well organized and easy to use', only about

[33]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.

[34]Percentage for individuals that either agree or strongly agree. N= 25, only Online campaign.

one third (36%) thought that 'bids from different installers were easy to compare'. This finding is in line with anecdotal evidence that Online led to some confusion for potential adopters.

For each contract signed we observe the chosen installer and detailed product features (system size, solar panel brand, inverter type and brand, total cost, type of financing, estimated annual production, as well as installation date). In total, there were 13 competitors participating in the Online bidding with very heterogeneous bidding behavior.[35] Overall customers appear to be price sensitive. In the case of two or more competing offers, 31% of customers decide for the cheapest option (cost per Watt (pre-incentive) / system size), and close to 30% go with the second cheapest option.[36]

7.3.2 Single installer vs. group pricing

One other difference between the Classic program and the Choice and Online programs is the presence of group pricing. However, the addition of the group pricing cannot explain our results. Gillingham and Bollinger (2017) analyze the impact of Solarize Classic campaigns that did not have group pricing that were implemented during R5. They found that the campaigns without group pricing (which had relatively modest price declines) were just as effective as the Classic campaigns. We use the data from these additional campaigns to perform randomization inference to further test for equality of the Classic campaigns with and without group price. The results are in Table A.4. There are no significant differences in campaign effectiveness that result from the presence of group pricing.

7.4 Robustness

We perform a series of additional robustness checks and find similar results across all of them:

- Wild cluster bootstrap standard errors as developed in Cameron, Gelbach, and Miller (2008). See Tables A.5 and A.6

[35] While one installer bid on all 67 projects, two installers on about 2/3 of all projects, and the rest presented bids on 1/3 or a significant lower share. Interestingly the firm with most quotes did only win 6% of all contracts. Other large bidders have been more successful in their conversion rates.

[36] Note that installations can be heterogeneous in many dimensions such as brand or efficiency. The small sample makes it difficult to pin down the precise factors influencing consumer decisions.

- Type of fixed effects (quarter and annual FE with month-of-year dummies)
- Log-transformed main dependent variable
- Robustness regarding the precise starting and end date of individual Solarize campaigns.[37]
- Sample selection: pre-and post Solarize time-periods included in the estimation. Limit the sample to one year pre-intervention and to seven month post.

We also provide some robustness concerning sample selection in Tables A.7 and A.8. In column 1 we estimate the main price regression (5) on purchase-financed installations only.[38] Column 2 limits the sample to rooftop installations with a system size smaller or equal 10 kilowatt. We additionally control for system financing. The results for these sub-samples are very much in line with the main results from Table 2. We find a significant and larger price decreases for Choice and Online compared to Classic during the campaign. We also additional evidence in line with the hypothesis that Choice and Online affect prices in the post-campaign period.

For the quantity regressions, the main dependent variable, namely the number of new solar installations, is heavily skewed (see Figure A.1. For robustness, we estimate model (6) using a negative binomial model count data model. The results are presented in Tables A.9 and A.10 for round 3 and round 5 respectively. The tables show the incidence-rate-ratios (IRR), which can be interpreted as arrival rates. A coefficient of one hence means translate into a zero treatment effect. For robustness, columns 1 and 2 present two distinct estimation methods.[39] [40] We find that the qualitative results are in line with our main estimates.

[37] As we do only observe the date at which the solar installation received approval by the CT GreenBank, but not the precise installation date, some installations dated post-Solarize should still be counted to the campaigns. We use data from installer to recover the average time gap between completion of installation and "approval date" of the installation by the Green Bank. Our results are robust to the precise timing assumptions.

[38] While our database contains price information on all type of financing, the complexity of lease and loan products can lead to inaccuracies in reported costs, especially if the financing products have been offered through a installer contract. Replicating our main results for purchase-financed installations only is hence an important sign of robustness.

[39] The correct model selection in negative binomial models with high degree of FE is an active area of research. See for example Guimarães (2008), http://www.stata.com/statalist/archive/2012-02/msg00399.html.

[40] As additional robustness, we estimate model (2) with the subsample of rooftop installations only and experiment with weighting according to market size (number of residential buildings) and the number of installations. The qualitative results are robust.

8 Discussion

We find that an increase in competition during Solarize (Choice & Online) leads to a price drop about twice the size compared to the single installer case (Classic). Focusing on purchase-financed installations only, the price difference between Choice and Classic in round 3 is about 30 cents per Watt, while for Online and Classic (round 5) the difference is about 15 cents per Watt. These differences cannot be explained by changes in product composition, i.e., changes in the type of product financing, size of installations, etc.

Focusing on quantities sold, we find that Choice leads to 6.1 additional installations per month of campaign, while Classic results on average in 4.5 new installations per month. While we find some evidence for harvesting in Classic towns, Choice did not lead to a significant drop in installation quantities in the post-campaign period. This effect can be explained by two main factors. First, several Solarize installers might be able to generate more attention throughout the campaign, resulting in additional sales in the post-campaign period. Second, single Solarize installers might be faced with capacity constraints and they might be unable to meet demand in a timely manner in the post-period. The quantity response in Online are smaller than in Classic (3.2 installations versus 5.5 installations respectively), which can be explained by demand side factors. In Online, we find that a larger share of potential customers do not sign a purchase contract. Choice overload and comparability of bids are potential reasons for the lower sales conversion. The overall market in round 5 has expanded importantly with the entry of the largest solar energy service provider in the United States, SolarCity, and the provision of new type of product financing.

In line with our model predictions, we find that the higher degree of competition during Solarize Choice and Online leads to long-term price declines in the post-campaign period. The increased competition during the program leads to long term increases in the number of active installers by 2-4 installers (an increase of 50- 100%) and a long term price decline of $0.18/W - $.028/W. This equates to between $0.08/W and $0.09/W per extra installer, an elasticity of 0.11-0.14. The fact that a temporary campaign can lead to these long term prices declines, increasing consumer surplus, has large implications for government-business partnerships, specifically the need to still foster competition within such initiatives.

Although the use of online platform did lead to a smaller quantity increase than Choice did during and after the campaigns (relative their respective concurrent Classic campaigns), as consumers become more familiar with using such platforms, and as the number of participating installers increase, we would expect downward pressure on prices to continue. From a logistical standpoint, the EnergySage platform allows for a more open market environment and the inclusion of all firms that want to participate (assuming they meet certain criteria), whereas the Choice programs were logistically challenging and limited in the number of installers they could include. Online platforms could easily see larger demand effects if prices can be made more comparable across options (something we were able to ensure in the Classic and Choice program through the use of the RFP bid process).

The main limitation of this study is the sample size. Logistical and cost considerations are the reason for the small number of towns in the experiment.[41] However, these sample sizes are not uncommon in the development economics literature, in which entire communities must be randomly assigned as a unit, rather than randomization occurring at the household unit.[42] Such market-level randomization is necessary given the desired object of study, namely equilibrium price and quantity effects of competition. That said, our findings are very robust to alternative specifications, and the very large lift that results from the campaigns leads us to estimate statistically significant results, even with the smaller sample size and after clustering standard errors at the town level.

Despite the detailed information on individual campaigns (events, post-campaign surveys with ambassadors and participants, installer surveys, etc.), with the small sample it is hard to fully explain the mechanism for the equilibrium results and heterogeneity across the campaigns with revealed preference data alone, since we have limited degrees of freedom. The final estimates include any effects that result from coordination between the town, installers, Smartpower, and in the case of Online, the EnergySage platform. The effect of adding competitors may have also affected the effectiveness of ambassadors[43] and the communication strategy of installers

[41]The cost of the 28 towns in R3 and R5 alone exceeded $800,000.

[42]The paper most related to ours is Busso and Galiani (2014), in which the authors test for the impact of increased competition on market outcomes in the development context. The authors rely on random variation from the implementation of a conditional cash transfer program in the Dominican Republic. They find that six months after the intervention, product prices in the treated areas had decreased by about 6%, while product quality and service quality was not affected by the entry of new competitors.

[43]In another paper, Kraft-Todd, Bollinger, Gillingham, Lamp, and Rand (2017) analyze the impact that personality

with the town (e.g. a single focal installer might have been able to provide a clearer message than several competing firms). However, the combination of the large-scale field experiment with the extensive survey data do allow us to further examine the mechanisms of the interventions. The very high satisfaction of consumers with their installers and the importance of WOM lead us to believe that market power through WOM effects leading to larger consideration set sizes in the post-campaign periods lead to the long term effects.

9 Conclusion

This paper provides new evidence for a classic question concerning the equilibrium price and quantity impacts of competition. Taking advantage of experimental variation in the number of competitors allowed to operate within a large-scale marketing campaign (Solarize Connecticut), our findings confirm the classic result that an increase in competition lowers prices, and hence increases consumer surplus. We also find limited evidence that increased competition leads to larger product adoption that is persistent in the post-treatment period. These findings have important policy implications; government has increasingly worked through business partnerships to achieve policy goals in domains such as energy, health, education, and crime prevention. Although such partnerships may help achieve the end objectives, this paper highlights the risks when such partnerships are exclusive because competition remains critical in reducing costs in the long run.

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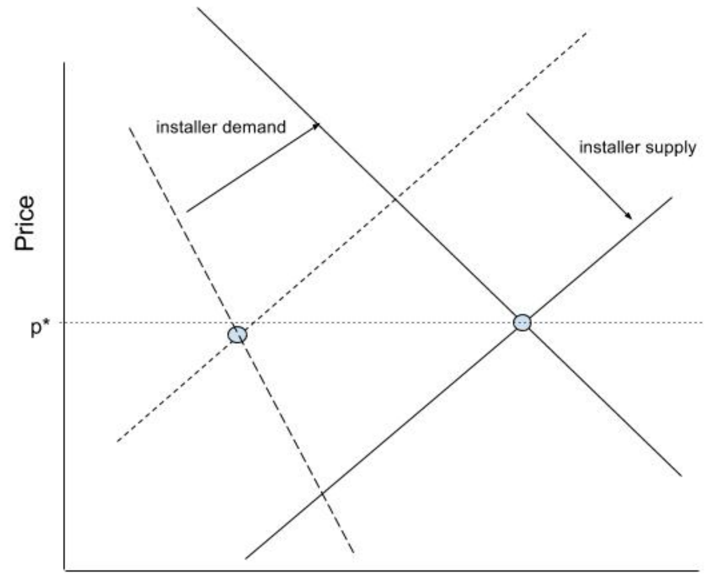
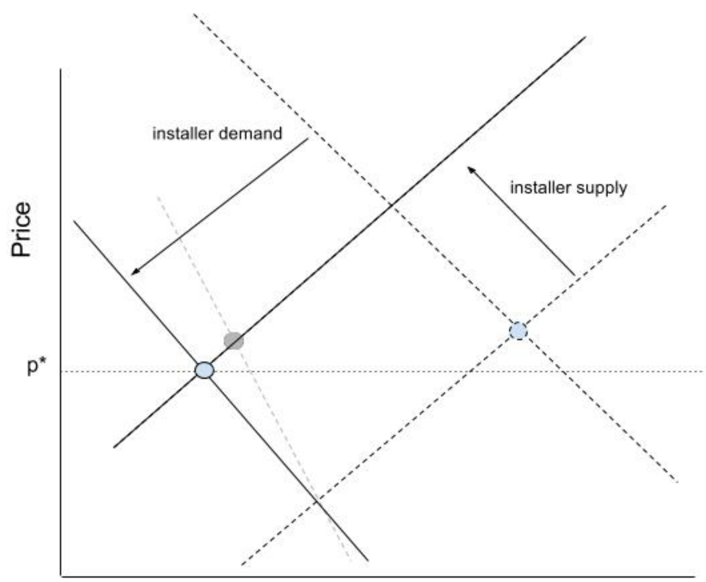
Figures & Tables

Figure 1: Equilibrium effects of Solarize Classic

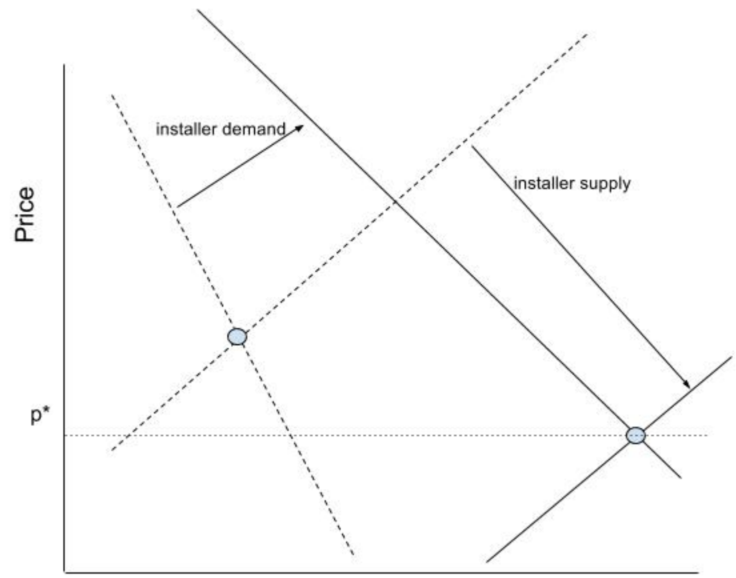


(a) During

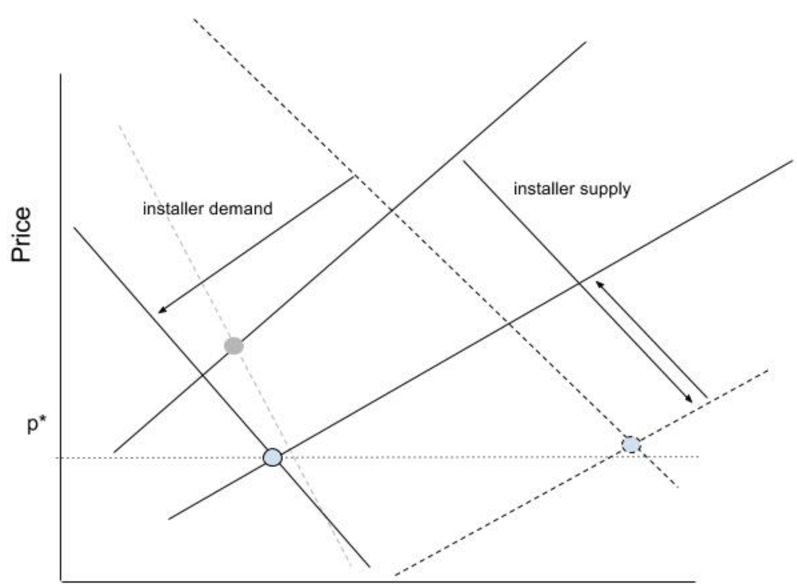


(b) Post

Figure 2: Equilibrium effects of Solarize Choice

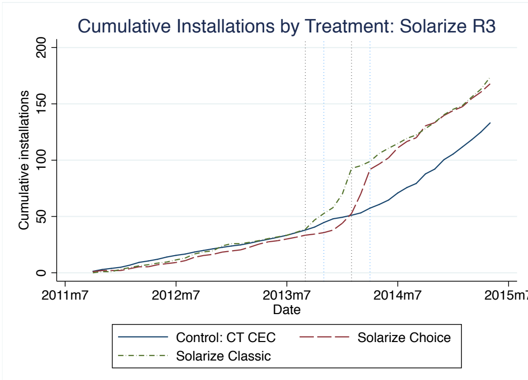


(a) During

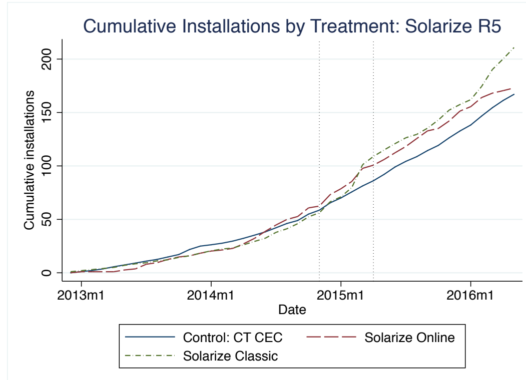


(b) Post

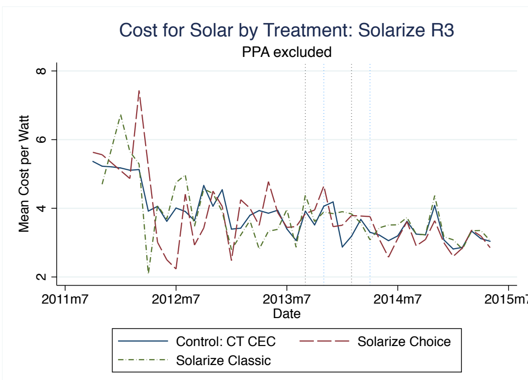
Figure 3: Pre-treatment trends. Comparison of treatment and control towns



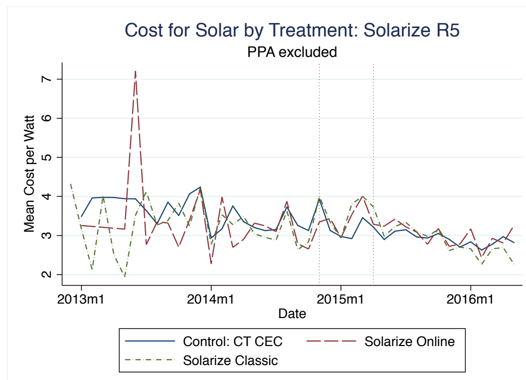
(a) Cumulative installations, R3



(b) Cumulative installations, R3



(c) Price of Solar, R3



(d) Price of Solar, R5

Note: Vertical lines indicate the start and end-date of Solarize campaigns. While the campaign timing was coordinated in R5, due to logistical reasons Choice started with a two month lag after Classic in R3.

Table 1: Market concentration: active installers and HHI

	None			**Choice**			**Classic**		
	Pre	During	Post	Pre	During	Post	Pre	During	Post
Numb. installers/town	2.400	3.447	4.678	4.680	6.172	7.742	4.088	4.328	4.775
HHI (normalized)	0.355	0.211	0.244	0.100	0.185	0.163	0.132	0.630	0.195

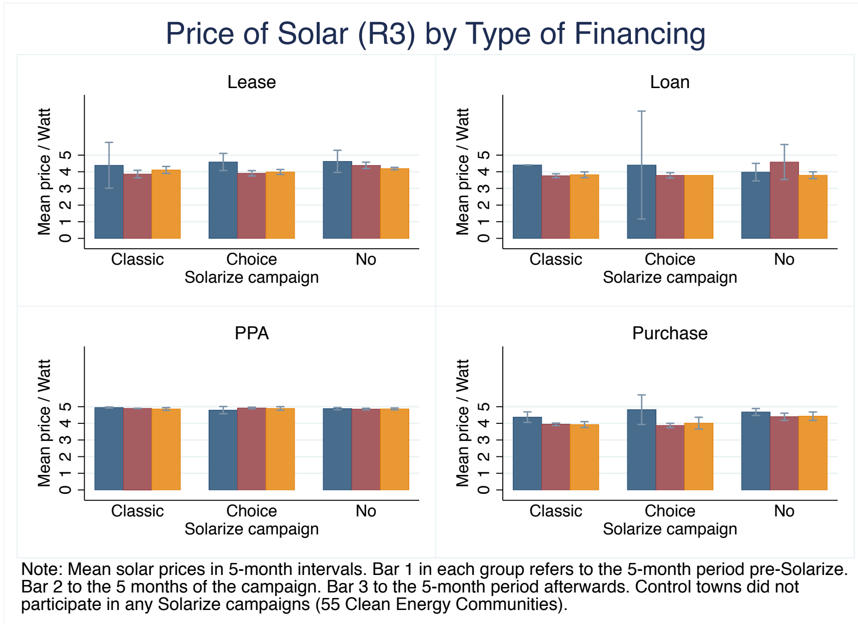
a) Round 3

	None			**Online**			**Classic**		
	Pre	During	Post	Pre	During	Post	Pre	During	Post
Numb. installers/town	6.501	5.201	6.063	6.421	9.288	8.935	5.102	5.975	5.537
HHI (normalized)	0.159	0.231	0.183	0.193	0.104	0.106	0.277	0.376	0.100

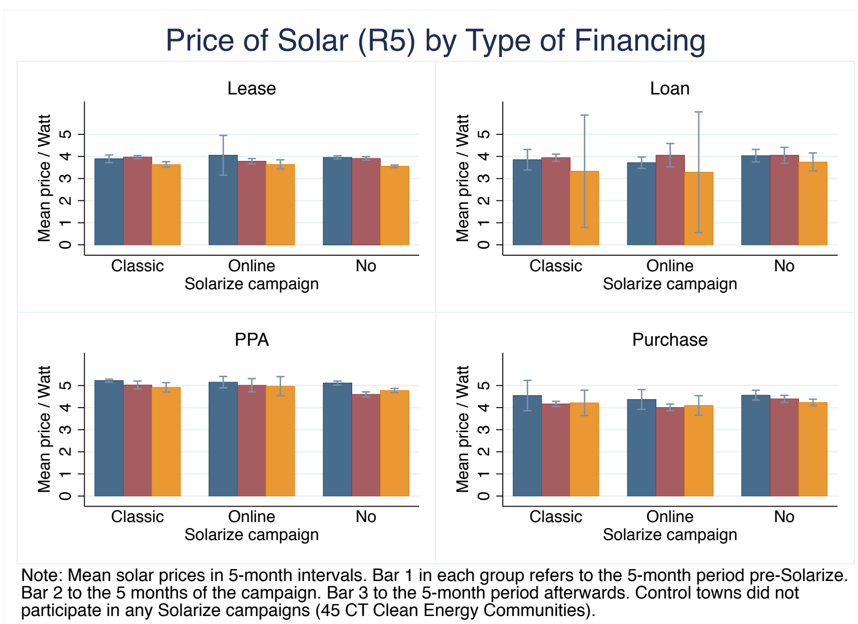
b) Round 5

Note: Active installers per municipality and mean of normalized Herfindhal-Hirschman Index (HHI) in five-month periods pre-, during- and post-Solarize.

Figure 4: Mean price of solar, by type of financing

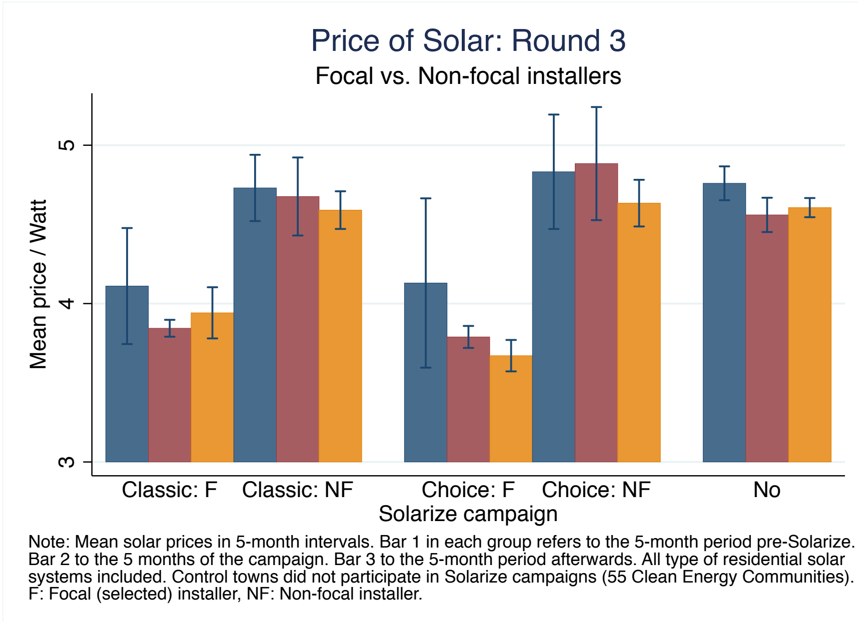


a) Round 3: Classic, Choice and Control

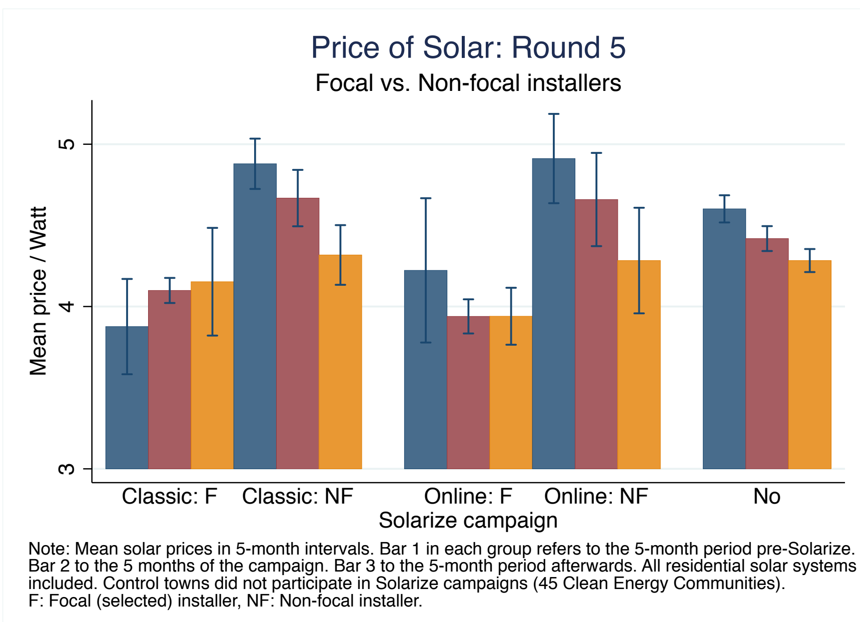


b) Round 5: Classic, Online and Control

Figure 5: Mean price of solar, by focal installer



a) Round 3: Classic, Choice and Control



b) Round 5: Classic, Online and Control

Table 2: Main price effects of Solarize

	R3: Classic & Choice				R5: Classic & Online			
	(1)		(2)		(3)		(4)	
Solarize Classic	-0.543^{***}	(0.127)	-0.292^{**}	(0.135)	-0.136^{*}	(0.071)	0.146	(0.088)
Solarize Choice/Online	-0.634^{***}	(0.095)	-0.491^{***}	(0.086)	-0.279^{***}	(0.073)	0.004	(0.077)
Post Classic	0.008	(0.113)	0.123	(0.121)	-0.190^{*}	(0.102)	0.004	(0.070)
Post Choice/Online	−0.106	(0.145)	−0.159	(0.105)	-0.256^{**}	(0.115)	-0.177^{**}	(0.084)
Observations	4255		4038		4402		4401	
R^2	0.151		0.308		0.169		0.405	
Month FE	Y		Y		Y		Y	
Municipality FE	Y		Y		Y		Y	
Controls	N		Y		N		Y	

Note: Estimation of model specification (5) by OLS. Control variables include system size, type of system mounting and type of system financing.

Table 3: Robustness: Randomization Inference

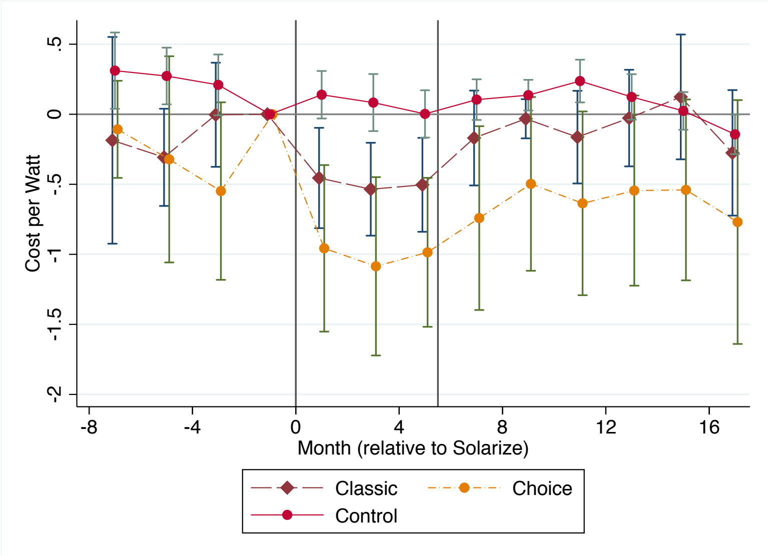
	Solarize				**Post Solarize**			
Sample	Coefficient	SE (clustered)	c	p-value	Coefficient	SE (clustered)	c	p-value
Round 3								
Classic & Control	-0.291	0.162	108	0.108	0.102	0.122	760	0.76
Choice & Control	-0.444	0.122	27	0.027	-0.123	0.099	270	0.27
Choice & Classic	-0.312	0.144	72	0.072	-0.246	0.140	59	0.059
Round 5								
Classic & Control	0.148	0.069	784	0.784	0.028	0.066	529	0.529
Online & Control	0.013	0.061	426	0.426	-0.156	0.108	168	0.168
Online & Classic	-0.112	0.071	60	0.06	-0.166	0.079	70	0.07
Pooled								
Classic & Competition	-0.354	0.074	17	0.017	-0.332	0.072	6	0.006

Random Inference with n=1000 Monte Carlo simulations. Treatment randomly assigned at municipality level.
Left tailed test statistic, c = number of cases in which the simulated regression coefficient is smaller than the true one.
P-values are calculated as p = c/n. The pooled sample combines the Classic municipalities from Round 3 and Round 5, and tests for the Null Hypothesis of equal treatment effects: Single installer (Classic) vs. Competition (Choice and Online).

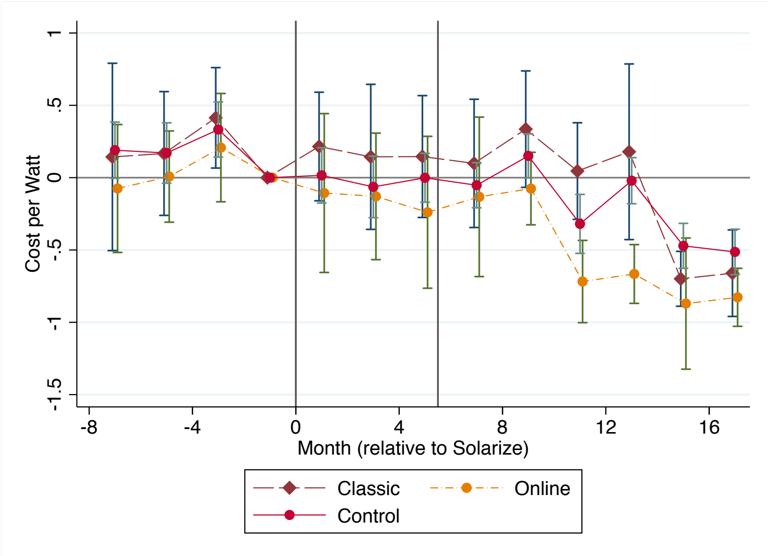
Table 4: Main quantity effects of Solarize: R3

	R3: Classic & Choice		R5: Classic & Online	
	(1)	(2)	(3)	(4)
Solarize Classic	4.517***	4.562***	5.484***	5.572***
	(1.028)	(1.060)	(1.208)	(1.120)
Solarize Choice/Online	6.080***	5.835***	2.922*	3.119**
	(1.186)	(1.230)	(1.530)	(1.446)
Post Classic	−1.381***	−0.925***	0.542	0.538
	(0.345)	(0.311)	(1.253)	(1.165)
Post Choice/Online	−0.860*	−0.420	−0.057	0.088
	(0.434)	(0.429)	(0.945)	(0.875)
Observations	4001	4001	3129	3129
R^2	0.386	0.459	0.486	0.535
Month FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y

Figure 6: Price dynamics



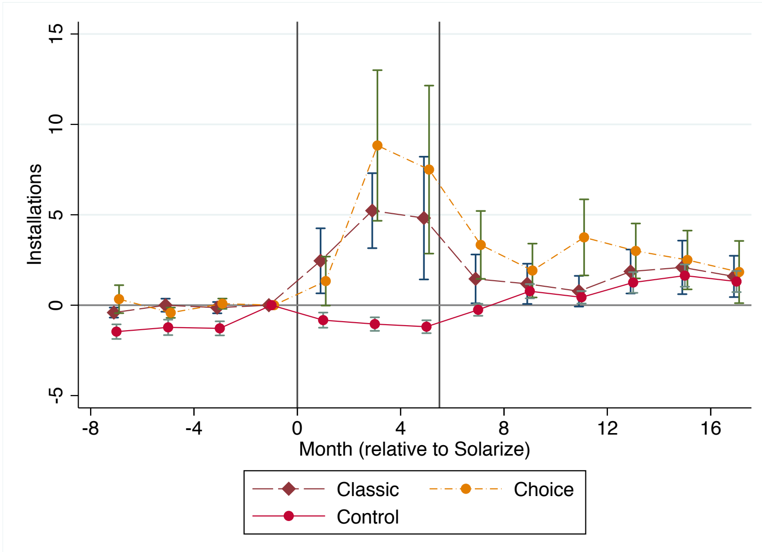
a) Round 3. Months relative to Solarize campaign.



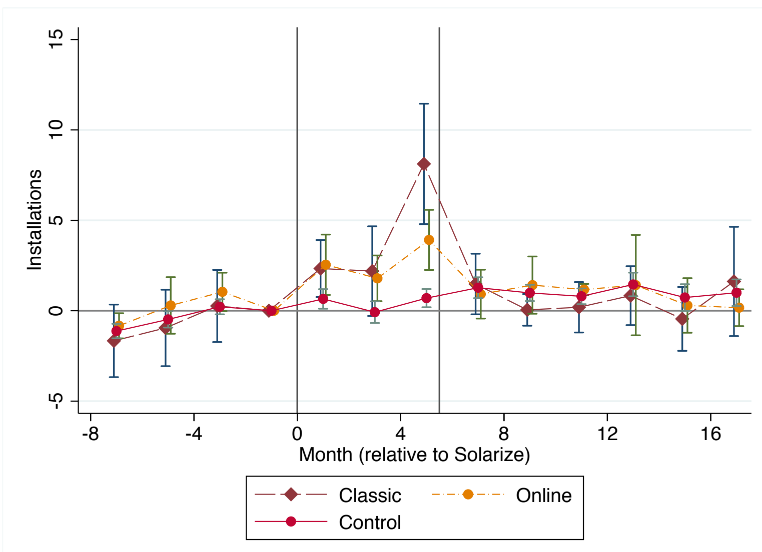
b) Round 5. Months relative to Solarize campaign.

Note: The figure displays point estimates and 95% confidence intervals for campaign $\times$ time dummies to show the dynamic impact of Solarize on equilibrium prices. Regression follows main specification (5) but includes a campaign $\times$ time dummy (2-month periods). Time t-1 omitted from the regression. The vertical lines indicate the duration of the Solarize campaigns.

Figure 7: Quantity dynamics



a) Round 3: Months since start of Solarize campaign.



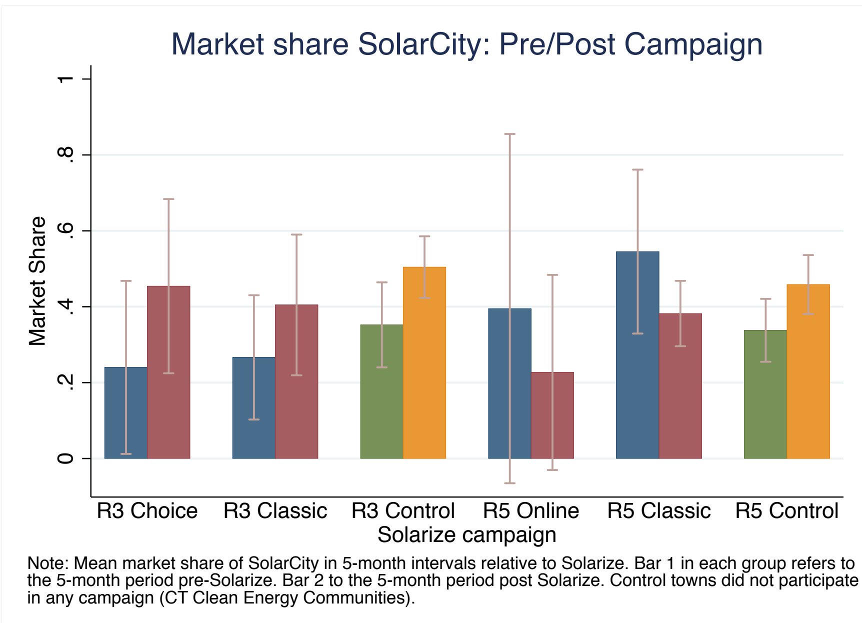
b) Round 5: Months since start of Solarize campaign.

Note: Plot displays coefficients and 95% confidence intervals for campaign × time dummies to show the dynamic impact of Solarize campaigns on equilibrium quantities. Regression follows main specification (6) but includes a dummy for every 2-month period. Time t-1 omitted from the regression. The vertical lines indicate the duration of the Solarize campaigns.

Table 5: Price effects by focal/non-focal installer(s)

	R3: Classic & Choice		R3: Classic & Online	
Solarize Classic	0.122	(0.233)	0.148	(0.115)
Solarize Installer	−0.396	(0.239)	-0.275^{**}	(0.115)
Solarize Classic × Solarize Installer	−0.224	(0.286)	0.241^{*}	(0.135)
Solarize Choice/Online	0.025	(0.119)	0.117	(0.150)
Solarize Choice/Online × Solarize Installer	−0.373	(0.290)	0.009	(0.135)
Post Classic	0.099	(0.124)	0.009	(0.135)
Post Classic × Solarize Installer	0.092	(0.228)	0.796^{***}	(0.177)
Post Choice/Online	−0.153	(0.104)	-0.257^{***}	(0.091)
Post Choice/Online × Solarize Installer	−0.008	(0.223)	0.391^{***}	(0.140)
Observations	4038		4401	
R^2	0.319		0.408	
Month FE	Y		Y	
Municipality FE	Y		Y	

Figure 8: SolarCity market shares



Note: SolarCity market shares: 5-month pre- and post-Solarize. Individual campaigns. Zero market shares imputed in case there are no sales in a given market (5-month interval $\times$ municipality).

10 Appendix

Description of the CT Solar Market

CT has a small, but fast-growing market for solar PV, which has expanded from only three installations in 2004 to nearly 7,200 installations in 2015. 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.[44] 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.[45] 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 25 years. 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.

During the time period of this study, the CT solar market had approximately 90 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.[46]

[44] Estimates based on authors' calculations from solar installation data and potential market data based on satellite imaging from Geostellar (2013).

[45] 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.

[46] 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.

Table A.1: Solarize municipalities

ROUND 3

R3 Classic	Start Date	End Date	5 MONTH PRE Inst	5 MONTH PRE Share SolarCity	5 MONTH PRE Share Focal	DURING Inst	DURING Share SolarCity	DURING Share Focal	5 MONTH POST Inst	5 MONTH POST Share SolarCity	5 MONTH POST Share Focal
Ashford	9/24/13	2/11/14	7	57.14	14.29	23	8.7	91.3	7	57.14	14.29
Chaplin	9/24/13	2/11/14	3	0	33.33	14	0	100	2	100	0
Easton	9/22/13	2/9/14	4	50	25	12	0	100	5	40	40
Greenwich	10/2/13	2/18/14	6	16.66	33.33	38	5.26	94.74	9	44.44	44.44
Hampton	9/24/13	2/11/14	2	0	50	13	7.69	76.92	1	0	100
Manchester	10/3/13	2/20/14	11	36.36	0	47	6.38	82.98	21	66.67	33.33
Newtown	9/24/13	2/28/14	5	60	40	33	0	96.97	13	30.77	23.08
Pomfret	9/24/13	2/11/14	7	28.57	57.14	8	0	100	2	0	100
Redding	9/22/13	2/9/14	1	0	0	9	0	77.78	2	50	50
Trumbull	9/22/13	2/9/14	2	0	0	45	4.44	80	11	27.27	9.09
West Hartford	9/30/13	2/18/14	9	44.44	22.22	64	6.25	89.06	33	42.42	33.33
R3 Choice											
Cheshire	11/14/13	4/8/14	4	25	0 (0)	43	6.98	90.7 (3)	25	32	40 (3)
Columbia	11/4/13	3/25/14	1	0	0 (0)	27	3.7	92.59 (3)	6	33.33	0 (0)
Enfield	11/20/13	4/15/14	7	57.14	28.6 (2)	62	12.9	77.42 (3)	32	75	12.5(2)
Lebanon	11/4/13	3/25/14	0	0	0 (0)	37	5.41	94.59 (3)	6	66.67	16.67 (1)
Stamford	11/21/13	4/15/14	7	28.57	14.29 (1)	22	4.55	81.82 (3)	23	41.67	16.67 (1)
West Haven	11/13/13	4/8/14	6	33.33	0 (0)	15	6.67	86.67 (2)	29	20.69	27.59 (2)

ROUND 5

R5 Classic	Start Date	End Date	5 MONTH PRE Inst	5 MONTH PRE Share SolarCity	5 MONTH PRE Share Focal	DURING Inst	DURING Share SolarCity	DURING Share Focal	5 MONTH POST Inst	5 MONTH POST Share SolarCity	5 MONTH POST Share Focal
Burlington	11/19/14	4/9/15	13	23.08	0	58	8.62	77.59	12	41.67	16.67
East Granby	12/2/14	4/22/15	8	25	50	27	33.33	62.96	10	40	0
New Canaan	12/2/14	4/22/15	5	60	0	16	0	100	5	20	40
New Hartford	11/17/14	4/7/15	10	50	0	23	21.74	60.87	18	38.89	11.11
Suffield	12/2/14	4/22/15	22	72.73	13.64	61	19.67	60.66	23	47.83	8.7
Windsor	12/2/14	4/22/15	21	66.67	4.76	58	31.03	63.79	35	45.71	8.57
Windsor Locks	12/2/14	4/22/15	19	84.21	0	42	30.95	52.38	18	33.33	22.22
R5 Online											
Lyme	11/18/14	4/8/15	3	0	66.66 (2)	6	0	100(3)	1	0	100(1)
Old Lyme	12/4/14	4/24/15	8	37.5	37.5 (3)	24	8.33	75 (5)	9	22.22	44.44 (4)
South Windsor	11/10/14	3/31/15	33	66.67	18.18 (3)	43	27.91	60.48(6)	37	35.14	22.04 (5)
Woodstock	12/3/14	4/23/15	13	53.85	15.38 (2)	32	31.25	56.23(5)	15	33.33	26.67 (2)

Note: The above tables list all municipalities participating in Solarize round 3 (Classic and Choice) and round 5 (Classic and Online). They show the total number of installations (inst) in 5-month intervals relative to the Solarize campaign as well as the market shares of SolarCity and the focal (Solarize) installer(s). In the case of Choice and Online, the number of active focal installers is annotated in parenthesis. We label an installer as active if he has made at least one sale in the 5-month period in a given market.

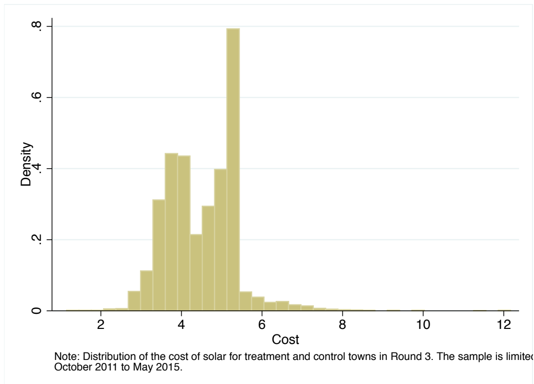
Table A.2: Sample balancing: Round 3. Municipality (means) for the pre-treatment year 2012.

	Population	Area	Income	Share White	Share Black	Share Asian	Share Homeowner	Share 5beds	Median Age	Number of new houses	Fees	Share Republican	Share Democrat	Share Not Registered	Cost per Watt	New Installations	Total Added capacity
Treatment																	
mean	32992.17	33.49	117362.50	85.36	5.90	4.88	76.21	9.61	43.46	804.73	473.60	25.05	33.30	40.55	5.281	5.29	27.30
sd	34444.76	13.24	31016.55	11.77	6.72	3.35	12.39	5.43	3.85	178.28	173.61	6.01	7.19	5.02	0.794	3.92	20.36
p50	26932.33	32.24	114317.10	92.34	2.15	3.65	78.13	9.15	44.29	762.93	420.88	25.29	31.42	40.11	5.285	4.00	22.30
max	125885.00	59.38	191353.80	96.89	22.94	10.22	90.21	22.26	49.48	1235.48	777.35	36.49	52.81	48.47	7.675	16.00	72.30
min	1683.41	12.04	73222.59	59.77	0.16	0.69	52.45	1.54	34.96	558.83	9.99	13.62	26.15	32.15	4.270	1.00	4.01
N	17	17	17	17	17	17	17	17	17	17	17	17	17	17	17	17	17
Control																	
mean	24804.58	28.07	94482.88	85.48	5.90	4.18	71.63	7.51	44.39	950.20	504.02	22.29	34.19	42.69	4.878	4.29	21.62
sd	30798.69	14.36	32684.61	14.65	8.54	2.82	15.04	5.56	5.66	293.74	182.49	7.16	9.99	6.25	0.613	3.29	18.01
p50	14048.29	23.55	92111.90	91.71	2.40	3.65	75.10	5.97	45.37	901.20	437.69	21.68	32.86	43.01	4.931	3.00	14.93
max	142456.80	60.27	201298.40	98.11	39.77	12.38	92.50	34.96	55.37	1786.77	1094.68	37.63	73.96	54.40	6.660	16.00	92.65
min	1313.05	7.67	38598.02	31.81	0.43	0.25	25.24	1.66	31.32	587.34	248.75	3.80	23.37	21.80	3.190	1.00	2.79
N	55	55	55	55	55	55	55	55	55	55	55	55	55	55	52	52	52
Total																	
mean	26737.76	29.35	99885.02	85.45	5.90	4.35	72.71	8.01	44.17	915.85	496.84	22.94	33.98	42.18	4.977	4.54	23.02
sd	31639.65	14.20	33541.59	13.94	8.10	2.94	14.51	5.57	5.28	276.87	179.70	6.97	9.37	6.02	0.679	3.45	18.62
p50	14408.93	26.68	92604.22	91.87	2.38	3.65	77.51	6.50	44.86	888.87	437.61	22.41	32.56	42.95	4.970	3.00	17.42
max	142456.80	60.27	201298.40	98.11	39.77	12.38	92.50	34.96	55.37	1786.77	1094.68	37.63	73.96	54.40	7.675	16.00	92.65
min	1313.05	7.67	38598.02	31.81	0.16	0.25	25.24	1.54	31.32	558.83	9.99	3.80	23.37	21.80	3.190	1.00	2.79
N	72	72	72	72	72	72	72	72	72	72	72	72	72	72	69	69	69

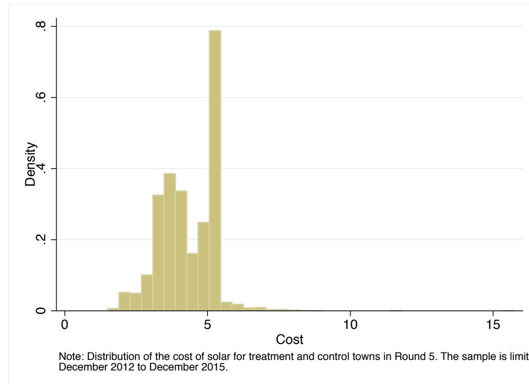
Table A.3: Sample balancing: Round 5. Municipality (means) for the pre-treatment year 2013.

	Population	Area	Income	Share White	Share Black	Share Asian	Share Homeowner	Share 5beds	Median Age	Number of new houses	Fees	Share Republican	Share Democrat	Share Not Registered	Cost per Watt	New Installations	Total Added capacity
Treatment																	
mean	13239.07	30.97	93764.69	84.11	7.65	5.25	81.84	10.93	46.25	847.96	492.08	28.99	29.59	39.81	4.648	6.45	37.93
sd	9308.06	14.15	28503.81	15.44	13.22	3.07	5.82	7.22	5.17	216.95	111.43	7.56	6.80	4.18	0.337	3.45	20.22
p50	10388.22	30.15	87689.51	86.25	3.56	3.31	82.94	9.16	44.80	836.93	472.00	29.66	27.52	39.80	4.695	6.00	32.72
max	32515.20	62.09	159172.40	95.98	46.44	10.06	94.09	26.54	58.99	1283.11	778.72	43.23	48.78	47.20	5.095	11.00	60.11
min	2586.59	7.93	48797.09	41.53	0.31	1.85	72.97	1.63	39.49	571.78	364.00	14.13	24.51	31.06	3.940	1.00	3.88
N	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11	11
Control																	
mean	12949.68	28.49	89486.90	89.25	3.42	4.73	73.18	8.96	46.46	1095.26	434.79	24.52	31.53	43.01	4.661	6.04	33.33
sd	10183.06	15.06	32963.19	6.47	3.56	3.35	10.86	7.17	5.32	415.95	137.76	6.02	6.36	5.24	0.504	5.78	31.65
p50	10445.95	23.55	80791.41	90.90	2.65	4.18	75.65	7.67	46.52	1002.12	392.51	23.35	30.96	43.57	4.674	4.00	21.59
max	34523.67	60.27	180719.30	98.46	23.70	14.14	91.69	44.44	60.46	2174.59	773.04	38.40	51.82	53.64	6.247	32.00	171.99
min	1283.64	7.67	42867.96	59.95	0.60	0.27	32.48	1.63	32.50	621.22	224.00	13.09	22.00	33.25	3.510	1.00	3.80
N	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45
Total																	
mean	13006.53	28.97	90327.18	88.24	4.25	4.84	74.88	9.35	46.42	1046.68	446.04	25.40	31.15	42.38	4.658	6.13	34.23
sd	9935.89	14.80	31936.18	9.00	6.69	3.27	10.61	7.16	5.24	395.98	134.04	6.53	6.43	5.18	0.473	5.38	29.65
p50	10417.09	26.68	82901.77	90.82	2.69	3.94	76.43	7.97	46.18	977.45	412.00	24.18	28.93	42.67	4.683	4.00	24.02
max	34523.67	62.09	180719.30	98.46	46.44	14.14	94.09	44.44	60.46	2174.59	778.72	43.23	51.82	53.64	6.247	32.00	171.99
min	1283.64	7.67	42867.96	41.53	0.31	0.27	32.48	1.63	32.50	571.78	224.00	13.09	22.00	31.06	3.510	1.00	3.80
N	56	56	56	56	56	56	56	56	56	56	56	56	56	56	56	56	56

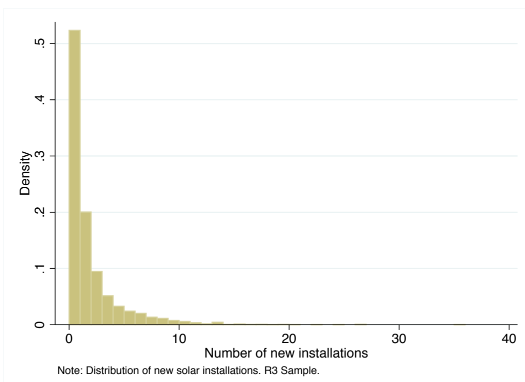
Figure A.1: Histogram of main dependent variables



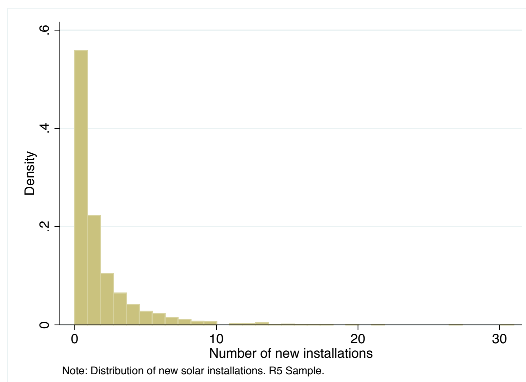
(a) Price of solar, sample R3



(b) Price of solar, sample R5



(c) Number of new installations, sample R3



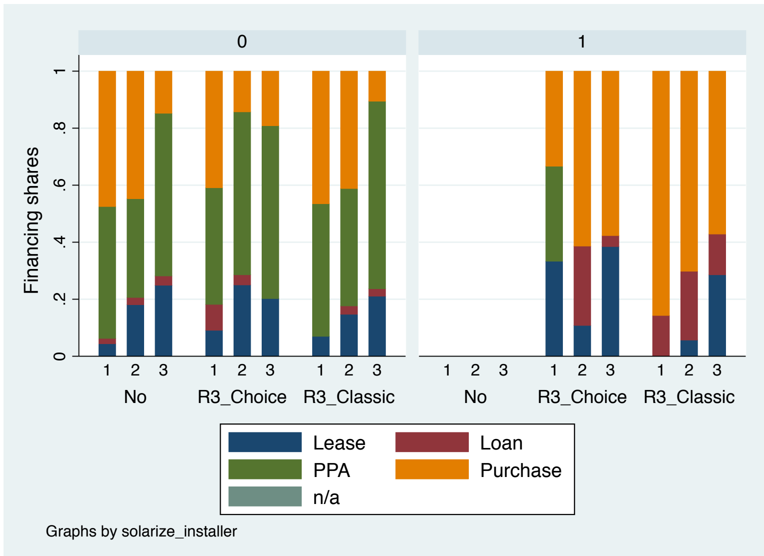
(d) Number of new installations, sample R5

Table A.4: Robustness: Randomization Inference with and without Group Pricing

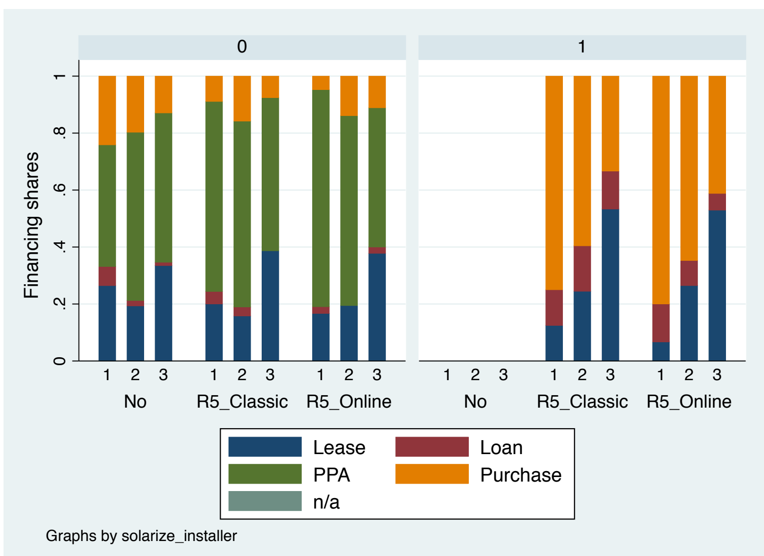
	Solarize				Post Solarize			
Sample	Coefficient	SE (clustered)	c	p-value	Coefficient	SE (clustered)	c	p-value
Round 5								
Online & Control	0.013	0.061	452	0.452	-0.156	0.108	170	0.17
Prime & Control	0.011	0.072	475	0.475	-0.129	0.122	203	0.203
Online & Prime	0.025	0.048	612	0.612	-0.002	0.151	500	0.5

Note: Randomization Inference with n=1000 Monte Carlo simulations. Treatment randomly assigned at municipality level.
Left tailed test statistic, c = number of cases in which the simulated regression coefficient is smaller than the true one.
P-values are calculated as p = c/n. The coefficients in "Online & Prime" refer to Online as treatment assignment.

Figure A.2: Financing shares by focal installer

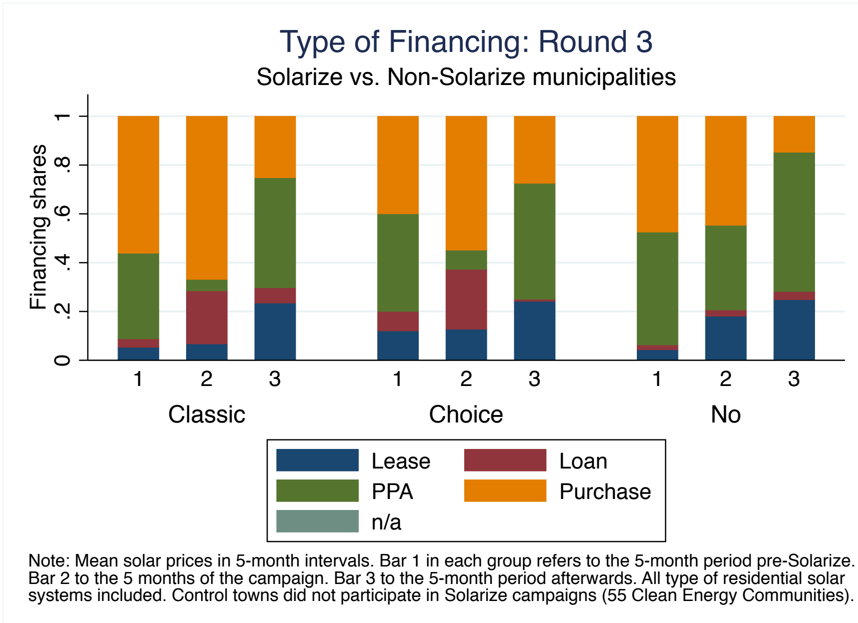


a) Round 3: Financing shares by type of campaign and focal installer.

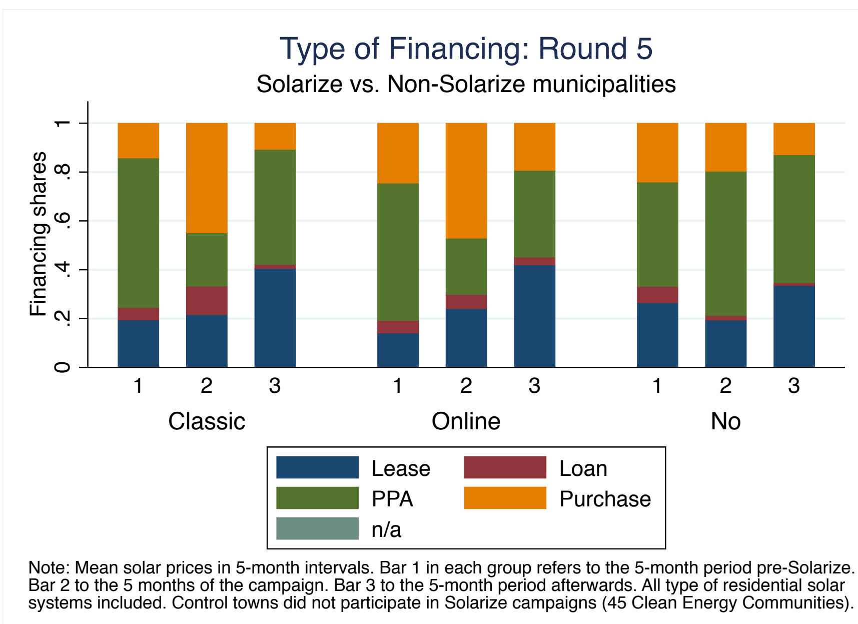


a) Round 5: Financing shares by type of campaign and focal installer.

Figure A.3: Mean financing shares

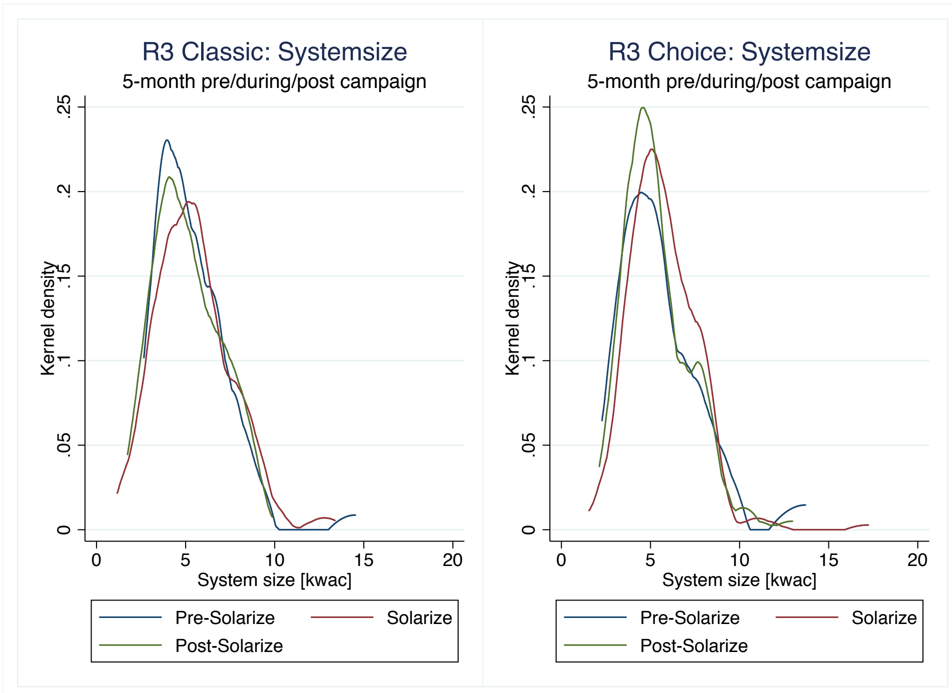


a) Round 3: Classic, Choice and Control

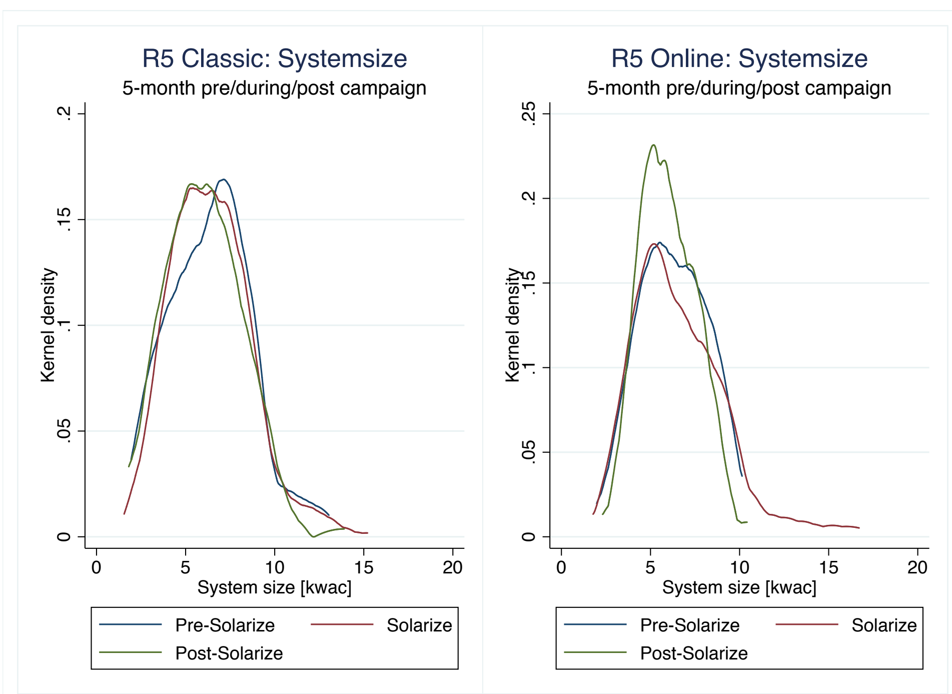


b) Round 5: Classic, Online and Control

Figure A.4: System size in Solarize municipalities



a) Round 3: System size in 5 month intervals relative to Solarize.



b) Round 5: System size in 5 month intervals relative to Solarize.

Table A.5: Robustness R3: Wild Bootstrap

	Coef	p-value	95% CI
SolarizeR3Classic	−0.286*	0.074	[-0.543, -0.016]
SolarizeR3Choice	−0.489***	0.002	[-0.657, -0.328]
PostR3Classic	0.124	0.306	[-0.112, 0.352]
PostR3Choice	−0.158	0.166	[-0.362, 0.039]
Observations	3976		
R^2	0.308		
Clusters	72		

Table A.6: Robustness R5: Wild Bootstrap

	Coef	p-value	95% CI
SolarizeR5Classic	0.146	0.168	[-0.025, 0.319]
SolarizeR5Online	0.004	0.977	[-0.135, 0.149]
PostR5Classic	0.004	1.000	[-0.125, 0.143]
PostR5Online	−0.177	0.150	[-0.336, -0.030]
Observations	4401		
R^2	0.405		
Clusters	56		

Note: Regression follows main regression model (5), controlling for month and municipality fixed effects as well as controls for system size, system mounting, and type of system financing. Standard errors obtained through wild cluster bootstrap as developed in Cameron, Gelbach, and Miller (2008) with 1000 simulations.

Table A.7: R3 (price): sample selection

	(1)	(2)
SolarizeR3Classic	−0.082	−0.283***
	(0.211)	(0.096)
SolarizeR3Choice	−0.708***	−0.531***
	(0.198)	(0.126)
PostR3Classic	0.135	0.146
	(0.253)	(0.106)
PostR3Choice	−0.304**	−0.158
	(0.150)	(0.132)
Observations	1185	3853
R^2	0.295	0.303
Month FE	Y	Y
Municipality FE	Y	Y

Table A.8: R5 (price): sample selection

	(1)	(2)
SolarizeR5Classic	−0.186	0.154
	(0.209)	(0.095)
SolarizeR5Online	−0.378**	0.075
	(0.178)	(0.096)
PostR5Classic	0.085	−0.006
	(0.169)	(0.062)
PostR5Online	−0.199	−0.122
	(0.155)	(0.125)
Observations	1008	4082
R^2	0.253	0.389
Month FE	Y	Y
Municipality FE	Y	Y

Note: Column 1 limited to purchase-financed installations. Regression controls additionally for system size and system mounting. Column 2 limits the sample to rooftop installations $\leqslant$ 10 kW and controls for system financing.

Table A.9: R3 (quantity): Negative Binomial

	(1)	(2)
SolarizeR3Classic	3.980***	5.449***
	(0.647)	(0.820)
SolarizeR3Choice	3.076***	5.484***
	(0.588)	(1.477)
PostR3Classic	0.325***	0.355***
	(0.044)	(0.065)
PostR3Choice	0.332***	0.466***
	(0.054)	(0.098)
Observations	4001	4001
Pseudo R^2		0.178
Month FE	Y	Y
Municipality FE	Y	Y

Table A.10: R5 (quantity): Negative Binomial

	(1)	(2)
SolarizeR5Classic	2.833***	3.112***
	(0.350)	(0.405)
SolarizeR5Online	2.346***	2.338***
	(0.396)	(0.352)
PostR5Classic	0.979	1.078
	(0.117)	(0.299)
PostR5Online	0.954	0.939
	(0.149)	(0.120)
Observations	3129	3129
Pseudo R^2		0.250
Month FE	Y	Y
Municipality FE	Y	Y

Note: Negative binomial estimation of model (6). The coefficients can be interpreted as incidence-rate-ratios (IRR). Column 1 estimates the model with XTNBREG and reports standard errors according to the observed variance-covariance matrix, while Column 2 uses the standard NBREG command with dummy variables to account for municipality fixed effects and reports municipality-clustered standard errors.