Promotional Campaign Duration and Word-of-Mouth in Durable Good Adoption

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Abstract

Intensive promotional marketing campaigns can be used to introduce products to consumers with the goal of increasing awareness, consideration, purchase, and word-of-mouth (WOM). In this paper, we study the role of campaign duration on solar photovoltaic adoption using a large-scale field experiment, in which we randomly assign communities to campaigns with shorter durations, increasing the marketing intensity to maintain the same total resources per campaign. In our context, a nonprofit partner conducts the campaign in cooperation with an installer selected by the community. We combine detailed campaign information with administrative data on solar adoptions and a large-scale survey of adopters to assess

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the effect of campaign duration on the number of campaign events, prices paid, product adoption, and potential determinants of adoption during the campaign. We find that shortening the length of the campaign did not affect installer behavior, price, or product adoption *during* the campaign but led to significantly fewer leads and less WOM, resulting in significantly lower post-campaign adoption rates.

Keywords: Durable good adoption, promotional campaign duration, word-of-mouth, field experiments.

1 Introduction

Motivating risk-averse consumers to adopt new products and technologies is a challenge for firms. The task of spurring adoption by mainstream customers after adoption by the "early adopters" has been referred to as "crossing the chasm" (Rogers 2014). Resource-intensive promotional campaigns are often utilized in an attempt to achieve this, by increasing consumer awareness of the product or reducing the risk associated with product adoption. These campaigns can consist of multiple elements, including advertising, product demonstrations, and price discounts. Given the extent to which new products fail to achieve widespread adoption, the design of these campaigns can have critical implications in both the short and long run. In this paper, we study the role of campaign duration on solar photovoltaic panel adoption by residential consumers, both during the campaign and afterwards.

In addition to the mix of promotional activities, campaign duration is a critical decision made by managers. Shorter, higher-intensity campaigns can have both positive and negative effects. One advantage of shorter campaigns is that the time limits on any corresponding price deals can signal value (Inman et al., 1997), especially when overpayment concerns are active, such as in the case of durable goods with uncertain payoffs (Srivastava and Lurie, 2004; Dutta, 2012). In terms of potential drawbacks, we know that information from peers can help alleviate risk (Punj and Staelin, 1983; Risselada et al., 2014; Kraft-Todd et al., 2018) but that information transmission via word-of-mouth (WOM) takes time to operate. This can be especially true in group pricing settings, which can led to adoption inertia, with the greatest uptake at the end of the deal due to the revelation of information regarding the final price (Kauffman and Wang, 2001; Kauffman et al., 2010; Luo et al., 2014). We propose that shortening campaigns (especially when combined

with group pricing) can lead to detrimental long-term effects; although a corresponding increase in marketing intensity might lead to comparable product adoption during the campaigns, we expect a reduction in marketing leads and WOM from the shortened campaigns, which will lead to lower long-run adoption rates.

To study the role of promotional campaign duration on durable good adoption, we ran a large-scale field experiment in which we randomly assigned communities to "Express" campaigns, which had shorter durations than the standard "Classic" campaigns, and increased the marketing intensity accordingly to maintain the same total resources per campaign. The context is the Solarize Connecticut (Solarize CT) program, in which grassroots marketing efforts are utilized to increase solar adoption. The Solarize CT program leverages WOM with the use of "solar ambassadors" (volunteer residents who speak to their neighbors about solar) and town events, and offers group pricing through a single contractor so that as more people commit to installing solar during the campaign, the price is lowered for everyone.[1] We used detailed administrative data on all solar adoptions in Connecticut in order to measure both the short-run and long-run impact of the campaigns on adoption. We found that both campaign durations led to the same number of installations per household during the period of the campaign.

The same was not true for the generated leads and WOM. To test the effect of the shorter campaigns on leads and WOM, we first collected the names of all leads generated at all events during the campaigns, which provided a total count of unique individuals who demonstrated an interest in adopting solar. Despite the similar adoption rates across campaign durations during the campaigns, the Express campaign generated fewer overall leads. We also followed the literature (Punj and Staelin, 1983; Newman and Staelin, 1972; Lovett and Staelin, 2016) by conducting

[1]The program typically involves five price tiers, with tier five providing the lowest price if enough people adopt.

a large-scale survey after the campaigns concluded, which directly asked the entire set of leads and solar adopters how they learned about the Solarize program and the importance of different sources of information in their adoption decisions.[2] We found that respondents considered WOM to be less important in the Express campaigns than in the standard "Classic" campaigns, and respondents were less likely to have heard about Solarize via WOM channels.

In addition to fewer leads and lower levels of WOM during the campaign, post-campaign adoption rates were significantly lower in the Express campaigns than in the Classic campaigns. Furthermore, the lower adoption rates in the Express campaigns can be fully explained by the reduction in leads and WOM. Customers in all campaigns were granted the lowest price tier by the end of the campaign, tier five, and we found no effect of campaign type on the realized transaction prices. Installers also reported no differences in their marketing strategies across campaign types. Furthermore, it is not the case that the higher post-campaign adoption rates in Classic are simply due to the higher number of leads that are later converted to adoptions; the post-campaign solar adopters are for the most part *not* the leads that were collected during the campaign. It could be that these non-adopting leads also share WOM, which is one explanation for why adoption rates might be lower in Express post-campaign. Another explanation for the lower adoptions rates is that solar adopters are more likely to share WOM if they themselves learned about solar via WOM.[3]

This paper offers a substantive contribution by demonstrating the importance of campaign duration in the promotion of durable goods by experimentally manipulating the length of the campaigns. The campaigns serve both to spur product adoption and to seed WOM. We find

[2]Email addresses for leads were collected directly at events, and email addresses for all solar adopters were included in the administrative data.

[3]We prefer the latter explanation, but cannot distinguish between the two from the data.

that by shortening these types of campaigns (with the same total resources deployed), firms can achieve similar outcomes along the first objective, but risk the second. The decline in the post-campaign adoption rates that can be attributed to the lower level of WOM has implications for managers, who often use campaign adoptions as a primary Key Performance Indicator (KPI) to measure campaign success. We find that WOM is an even more important indicator of success in the long-term.

The rest of the paper is organized as follows: In Section 2, we discuss the related literature and in Section 3, we discuss the institutional details. We describe the data in Section 4, and in Section 5, we show that both campaigns led to the same number of campaign adoptions, but the Express campaign led to fewer leads and less WOM. In Section 6, we show that the post-campaign adoption rates are lower in the Express towns, and provide evidence that this can be explained due to the reduction in leads and WOM. We provides a discussion regarding the implications for marketers in Section 7, and Section 8 concludes.

2 Background

The literature in marketing is mixed with regards to its predictions about the effect of promotion length on product adoption. Previous work indicates that a shorter campaign may even be beneficial since shorter deal duration can lead to greater purchase likelihood (Simonson, 1992; Dhar and Nowlis, 1999; Suri and Monroe, 2003). Dhar and Nowlis (1999) study the effect of time pressure on choice and find that time pressure decreases choice deferral when the choice involves high conflict, a situation that often occurs with expensive, durable goods, indicating that the shorter campaign might still be effective in leading to similar adoption levels. Time pressure decreasing

choice deferral is also consistent with Simonson (1992), if time pressure increases the anticipatory regret of making the wrong choice, and Suri and Monroe (2003) find that in situations where there is a high motivation to process information, an increase in time pressure will increase the perception of quality and decrease the perception of monetary sacrifice.

These studies demonstrate that promotions of shorter duration may lead to a greater purchase likelihood. However, it should be noted that these were all laboratory studies with hypothetical choices between products, and that the no-purchase option was not included. In the case of solar, the most important decision is whether to buy now or not, and the choice of installer is often secondary (in the case of the Solarize campaigns, there is only one installer offering the discounted price through the program).[4] Furthermore, Simonson (1992) finds that consideration of choice error (leading to anticipatory regret) may not accelerate purchases if there are other external factors, such as knowing the timing of discounts, as in our setting.

Perhaps just as importantly, shortening the campaigns might lead to less WOM, which takes time to operate. This would lead to a detrimental effect of shortening the campaigns on adoption. This is especially true if adoption is not accelerated, as predicted by Surasvadi et al. (2016), who use a model of strategic forward-looking consumers to show that consumers only purchase with group pricing after a certain time threshold. None of these aforementioned papers consider WOM effects. Previous literature has shown the importance of peer effects in the diffusion of residential solar (Bollinger and Gillingham, 2012), and thus this concern is especially relevant in this domain.

To see this more formally, we turn to the Bass (1969) model:

$$f(t) = dF(t)/dt = p + qF(t)(1 - F(t)) \tag{1}$$

[4]This can be evidenced by the large nest parameter found in previous research that estimates a nested Logit model of solar demand (Bollinger and Gillingham, 2018).

in which $f(t)$ is the proportion of adoptions at time t, $F(t)$ is the cumulative proportion of adopters, p is the coefficient of innovation, and q is the coefficient of imitation, which may be due to WOM. When fitting the Bass model to empirical adoption curves, the coefficient of imitation, q, picks up a variety of effects, including WOM. That said, if WOM is reduced we would expect to see a reduction in q. We conceptualize the coefficient of innovation, p, as something that can be affected by the campaign intensity. Since both p and q can be affected by manger decisions, to explain the mechanism behind our findings, we also look to verify that our manipulation does not change installer behavior.

The solution to this differential equation is:

$$F(t) = \frac{1 - \exp(-(p+q)t)}{1 + \frac{q}{p}\exp(-(p+q)t)} \tag{2}$$

If, for example, the Express campaign were able to double both p and q (with t half the length), then $F(t)$ is unaffected. In other words, the two types of campaigns will lead to the same number of adoptions, and adoption patterns over time, during the campaigns. Adoptions in the Express campaigns at any time t will be the same as in Classic at time $2t$, and we would be able to attribute the same proportion of adoptions to p versus q for both campaigns.

If instead the Express campaigns increase p more than q, as we would expect, since we do not anticipate social interactions increasing at twice the rate in the Express campaigns[5], then the campaigns can be just as effective, but fewer people will have heard about solar as a result of the imitation effect (the q). The adoption pattern over the course of the campaign would look different as a result. A high q would lead to substantial curvature in the adoption rate over

[5]Berger and Schwartz (2011) note that offline interactions usually occur naturally, at which point the content within the interactions is determined.

time, as past adopters encourage others to adopt. This would not be the case in an accelerated campaign that does not also experience a significant acceleration in social interactions leading to more WOM.

If the Express campaigns do lead to fewer potential adopters discussing the campaigns (less WOM), then the Express communities will then have different "initial conditions" in the post campaign period, even if there is a similar number of adoptions during the campaigns. Since we find that the Express campaigns generated fewer leads and WOM during the campaign than the Classic campaigns, one might expect this to lead to lower adoption rates in the post-campaign period if WOM begets more WOM, as shown by Chae et al. (2016).[6]

Given the expected differences in demand response across campaigns of different durations, we might expect firms to behave differently in the shorter campaigns, either through their marketing efforts or their pricing behavior. However, given our context, in which the campaigns are run by the nonprofit rather than by the solar installers, we are able to ensure consistency in many firm behaviors. In other contexts, firms might have the incentive to lower prices more in the beginning of the shorter campaigns to help spur initial adoption (increasing the "p" in the Bass model) and thus still leverage WOM. In the Solarize program, however, all installers are tied to the five-tier pricing system, and all marketing activities are coordinated by the nonprofit. Installers will be present at town events and workshops, but the nonprofit organizes them. In the next section, we describe the campaigns in more detail.

[6] Agent-based models have also shown that in a contagion model with recovering nodes (i.e. consumers no longer affecting peers), diffusion slows (Fibich, 2017). If the end of the campaign results in solar adopters and non-adopting leads no longer spreading WOM about solar, then the shorter campaign would lead to less WOM during the campaign which would then lead to less WOM (and resulting adoption) in the long-run.

3 Program Details

The Solarize CT program is a joint effort between a state agency, the Connecticut Green Bank (CGB), and a nonprofit marketing firm, SmartPower. Five rounds of Solarize CT in 58 towns were run between 2012 and 2015. During that time period, the number of homes with solar grew from about 800 to over 12,500. Previous work has shown that the Classic Solarize campaigns played a major role in this expansion, although it was not clear what elements of the campaigns were most critical to their success (Gillingham and Bollinger, 2017). That said, the Solarize campaigns are designed explicitly to leverage peer effects to foster the transfer of information about the benefits of solar technology.

The Solarize campaigns have several key pillars. Treated municipalities choose a solar PV installer with whom to collaborate throughout the campaign after a request for proposal (RFP) posted by the Connecticut Green Bank.[7] The process requires each installer to submit a bid with a discount group price, which 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 are knowledgeable opinion leaders that agree to encourage their neighbors and other community members to adopt solar PV, effectively providing a major nudge towards adoption. There is growing evidence of the effectiveness of promoters or ambassadors in driving social learning and influencing behavior (Nair et al., 2010; Kremer et al., 2011; Vasilaky and Leonard, 2011; BenYishay and Mobarak, 2014; Ashraf et al., 2015; Kraft-Todd et al., 2018).

A final component is the five-tier, group pricing discount offered to the entire community

[7] Jacobsen et al. (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.

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), as the price discount would be expected to be unavailable after the campaign. That said, across the campaigns, installers would offer a several week grace period, in which they would grant the final price tier for newly requested installations. All campaigns offered the lowest, tier-five pricing by the end of the campaign.

The standard timeline for a Solarize 'Classic' campaign 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.
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. The installations continue to occur after the end of the campaign and are usually completed within a few months.

In our randomized field experiment, we experimentally manipulated the duration of the cam-

paign, randomizing the intervention at the municipality level.[8] While the Solarize Classic campaign officially lasts 20 weeks, the Express version runs for 12 weeks. Furthermore, in all campaigns, installers allow for a grace period of several weeks after the campaign officially ends in which they will provide the Solarize promotional pricing. When the installers report the signed contracts to the CT Green Bank, they classify installations as Solarize and non-Solarize. The majority of installations in the four-week period after the official conclusion of the campaigns are classified as Solarize installations. We thus treat the Solarize Classic and Express campaign durations as 24 and 16 weeks, respectively, so as not to underestimate the treatment effects.

Our experiment consists of eleven Classic towns and five Express towns.[9] The high cost of each campaign (approximately $30,000) was the dominant factor limiting our experiment to 16 treatment towns. Elberg et al. (2017) face a similar challenge in their field experiment, which randomizes price promotions at the store level, as does Godes and Mayzlin (2009), who study the impact of WOM across 15 markets without market-level randomization. In the robustness checks, we provide evidence of small-sample robustness to show that out results are not dependent on asymptotic arguments.

4 Data

The primary data source for this study is a database of all solar PV installations that received a rebate from the CGB, 2004-2016. As the rebate has been substantial over the past decade, we are

[8] Some other variations in treatment design have taken place in order to test for the main underlying mechanisms. Besides the primary Solarize program described above, Solarize Classic, there were five treatment variations. In this paper we restrict attention to the Classic and Express campaigns.

[9] The discrepancy in the number of Express and Classic towns was a result of funding restrictions – one funding source was only for Classic towns. The timing of the campaigns is shown in Table A.1.

confident that nearly all, if not all, solar PV installations in CT are included in the database.[10] This dataset contains the address of the installation, the adopter's email address, the date the contract was approved by CGB, the date the installation was completed, the size of the installation, the pre-incentive price, the incentive paid, whether the installation is third party-owned (e.g., solar lease or power-purchase agreement), and additional system characteristics.

The second dataset includes lists of all activities facilitated by SmartPower in all the campaigns and all leads generated at any of these campaign events. SmartPower collected names and email addresses in order to ensure that no double counting occurred and in order to send the survey to the non-adopting leads as well.

The third data source for this study is the U.S. Census Bureau's 2009-2013 American Community Survey, which includes demographic data at the municipality level. Further, we include voter registration data at the municipality level from the CT Secretary of State (SOTS). These data include the number of active and inactive registered voters in each political party, as well as total voter registration (CT SOTS, 2015).

The fourth dataset was used to determine each town's market size. We rely on the U.S. Census Bureau's 2009-2013 American Community Survey and on 2010 official counts of the population and housing units to define the potential market size in each municipality. We calculate the market size of suitable homes by constructing the potential market for solar PV in CT, based on a satellite-imaging analysis that excludes unprofitable sites, determined using irradiance data and building and roof shapes (Google Project Sunroof). To do so, we take the share of solar suitable houses at the zip-code level and multiply it by the housing stock.[11]

[10] 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.

[11] In case the zip-code was not covered by the Google Project Sunroof data, we used a second satellite-imaging dataset (Geostellar, 2013), that estimated the solar market potential at the county level.

Finally, campaign participants are sent an online survey after the conclusion of the campaign.[12] In addition, we surveyed the non-adopting leads. The e-mail addresses came from Solarize event sign-up sheets and installer contract lists. Approximately six percent of the signed contracts did not have an e-mail address. All others we contacted one month after the end of the round, with a follow-up to non-respondents one month later. We also performed in-person interviews with the group of solar ambassadors, which allowed us to determine the suitability of their homes for solar. We had a response rate of 45 percent for those who adopted solar and about 15 percent for those who expressed interest in solar through the campaign but did not adopt during the campaign.

5 During-Campaign Adoption

In our first assessment of the effectiveness of the different campaign types during the campaigns, we compare the number of adoptions and the number of leads generated by the campaigns per 1000 suitable homes, across the two treatment conditions, shown in Table 1. There are clearly fewer leads generated in the Express campaigns (62 leads per thousand solar-suitable homes in Express versus 246 in Classic), although the number of adoptions are comparable (10.39 versus 7.32 adoptions per thousand solar suitable homes).[13]

In order to get estimates of the treatment effects for the campaigns, in this section we also include a matched control group in the analysis; however, we remain most interested in the Classic versus Express comparison, rather than the total treatment effect. The results of this

[12] This survey was performed through the Qualtrics survey software and was sent to respondents via e-mail, with two iPads raffled off as a reward for responding.

[13] The average number of sales leads are different at 10% significance level in a two-sample t test. On the other hand, we cannot reject equality of mean adoption rates. Controlling for length of the campaign, the adoption rate per month is the same for the two groups, as highlighted in Figure 4.

comparison are the same regardless of control group used and when using no control group at all, comparing only the two treatment conditions. Our preferred control group is the group of environmentally-oriented communities that have a clean energy task force, classified by the state as Connecticut Clean Energy Communities (CEC).[14] Summary statistics for key demographic and socioeconomic variables in these final datasets are also in Appendix A (Table A.2), and indicate a good balance in observables across the treatment and control group.

To further assess the validity of the CEC communities as a control group, it is informative to compare the trends in adoptions between each of the two treatment groups and the control group prior to the campaigns. Figure 2 displays the cumulative PV adoptions for each of the two treatments, beginning two years before the campaigns and ending two years after, overlaid with the trend in the control towns during the same period. The shaded area in the graphs indicates the weeks in which there was an active campaign in at least some of the towns. The pre-treatment trends across the treated and control towns are very similar.

A number of descriptive findings emerge from these simple trends. First, there is a clear overall boost in PV adoptions during each program. Second, this boost appears to take place during the later stage of the campaign, which is to be expected given the durable, expensive nature of the product. In the Classic campaigns, we see an acceleration of the upward trend in adoption at the end of the campaign, consistent with WOM and peer recommendations. In contrast, we see a sharp spike in the Express adoptions with hardly any adoptions leading up to the spike, providing some initial descriptive evidence that WOM may not have had time to effectively operate. The spike in adoptions in the last week of Express results from consumers trying to adopt before the campaign (and price deal) ends. Note that adopters during this last

[14] We remove any Solarize municipalities from the list of CEC communities used as a control group and we also explore matching approaches to develop further control groups as robustness checks.

week of Express do not then have time to spread WOM within the campaign period after their adoption decisions.

5.1 Treatment Effects by Campaign Duration

Let y_{it} denote the number of solar contracts signed in town i during week t per 1000 owner-occupied houses in the town that are suitable for solar.[15] To estimate the treatment effects, we first do a simple comparison of the mean number of installations during the Classic and Express campaigns. The mean number of installations per 1000 suitable homes per week is 1.49 in the Express campaigns and 1.58 in the Classic. The pre-campaign rates are also lower for Express, at 0.92 versus 1.05 for Classic. There is an increase during the campaign of 1.57 for Classic and 1.53 for Express, almost identical lifts for the two durations.

We next use a difference-in-differences regression approach, with and without town fixed effects, which helps address any randomization concerns due to the small number of towns. We define C_i and E_i as dummy variables, indicating the time during a Classic or Express campaign, respectively.

$$y_{it} = \alpha^c C_{it} + \alpha^e E_{it} + \mu_i + \delta_\tau + \epsilon_{it}. \tag{3}$$

In this specification, μ_i are town-specific indicator variables, δ_τ are year-week fixed effects, and ϵ_{it} is an idiosyncratic error term. The municipality fixed effects are useful in our setting to help control for any possible differences across the treated towns and control towns, while the time

[15] Recall that our measure of potential market size in a given town excludes from the total number of owner-occupied homes the fraction of homes which are determined as unprofitable for solar through irradiance data and building and roof shapes. In our robustness checks, we show that our main findings hold even when we do not adjust the total housing stock for solar suitability.

fixed effects are important for flexibly controlling for broader time trends that may influence the solar market.

We estimate specification (3) on all treated municipalities as well as the control municipalities for a measure of the average weekly treatment effect of each program, including two years of pre-campaign observations.[16] We do this twice, without and with town fixed effects. Table 2 presents the results in columns (1) and (2). Looking at column (1), Solarize Express appears to have a substantially larger average weekly treatment effect, although it is less precisely estimated since there are only five Express towns and we cluster standard errors at the town level. This is what we would expect, given the increased resource intensity. To get the total effect of the campaigns, it is necessary to multiply the Classic coefficient by 24 and the Express coefficient by 16. There is no significant difference across the campaigns in total adoptions (3.43 per 1000 suitable homes for Classic and 3.79 for Express, in the specification with town fixed effects).

To get some sense of the dynamics within the campaign, we split the Classic campaigns into three eight-week periods and the Express campaign into two eight-week periods. Results are shown in columns (3) and (4) of Table 2, again first without and then with town fixed effects. In both campaigns, most of the effect is in the last eight weeks. Prior to that, Classic experiences a small positive effect of the campaigns, and Express actually experiences a small negative effect. Potential solar adopters might be reluctant to adopt solar in the early stages of the Express campaigns, especially if they believe that the best price tier may not be reached. This could also serve to reduce WOM in the Express campaigns. Again, over the entire length of the campaign, we find no significant difference across the campaigns in total adoption.

Finally, we interact C_{it} and E_{it} with week-in-campaign dummy variables to flexible compare

[16] See Online Appendix A for results with alternative control groups, which generally show quite similar results.

the effects of the two types of campaigns on solar adoptions over time. This added flexibility is useful for better capturing the campaign dynamics. Using these week-in-campaign coefficient estimates, we derive *weekly* marginal effects over the length of the Classic and Express campaigns. Figure 3 displays these marginal effects. It is clear from these figures that consumers are more willing to adopt solar earlier in the longer Classic campaigns. This would allow WOM via peer recommendations to play more of a role in product adoption. Most of the Express adoptions occur after the campaigns officially end when the installer offer of tier five pricing (the lowest price) ends. Using this more flexible specification, we estimate a total campaign treatment effect of 3.52 installations per 1000 suitable roofs for Classic town, and 3.88 installations per 1000 suitable roofs for Express towns.[17] If anything, the shortened Express campaigns are more effective than the Classic campaigns in leading to more campaign adoptions, although as before there are no significant differences. But this does not mean the campaigns were the same.

5.2 Supply-Side Differences in Campaign Implementation?

Despite the effort to keep the campaigns as similar as possible, given the difference in the adoption patterns across Express and Classic campaigns, we examine evidence of possible differences in effort by any of the primary players in the campaigns: solar ambassadors, installers, CGB, and SmartPower. For this purpose, upon conclusion of the campaign, we conducted in-person interviews with ambassadors in each town. We collected information regarding campaign organization, effectiveness of strategies, and roles of involved parties.

We quantify the survey responses and examine differences in means between Classic and Express campaigns for several responses, including the extent of town vs. volunteer involvement,

[17] Given that Newtown is an outlier due to the two-week campaign extension, we only show the effects for the remaining ten Classic towns in the Figure.

ambassador effort (number of hours per average week of the campaign), any concerns about installer effort (e.g., not following up with potential customers in a timely manner), and any concerns about the effectiveness of CGB and SmartPower's involvement. The only differences in the campaigns that we could find was that there were fewer workshops in the Express towns; there was an average of 4.8 workshops for each Classic campaign, whereas Express had only 3.2. This may not be surprising because the Classic campaigns were longer, and thus there were more weeks to organize workshops. In contrast, the total number of hours spent by the ambassadors was the same across campaign types and the number of tabling events was also the same. The same held for all of the other variables as well–no significant differences between Classic and Express towns.

We further asked the ambassadors to categorize the importance of CGB and SmartPower's input in various campaign aspects. There are 11 aspects, which include selecting and contracting with installers, organizing the kickoff event, facilitating regular ambassador meetings, publicizing the campaign through press releases and social media, etc.[18] As shown in Table B.1 in Appendix B, there were no statistically significant differences in any of the responses across Classic and Express towns.

It is possible that installers invested more effort in additional customer outreach or offered better financial deals in one campaign or the other. To examine this, we interviewed representatives of the installing companies in each town. Responses to the interview's open-ended questions provide additional qualitative insight into the marketing strategies employed by installers. These responses again suggest no difference in effort or overall tactics employed in Classic versus Express towns. Regardless of campaign length, all respondents emphasized the importance

[18]For each of these 11 questions, the response is a categorical variable, which takes values between 1 and 5, with 1 = essential, 2 = very important, 3 = important, 4 = not important, 5 = not needed.

of targeted marketing efforts at the onset of the campaign (e.g., reaching out to residents who are more likely to adopt and to influence others to adopt). Installers that were selected to operate in both Classic and Express towns were asked whether their marketing approaches differed between these towns.[19] They all confirmed that there were no differences in their strategies based on type of campaign.[20]

We finally compare the realized PV system prices across Classic and Express towns (as previously mentioned, all towns were offered tier-five price levels by the end of the campaign). We examine the post-incentive price for each solar contract signed in the 16 municipalities during the Solarize campaign. On average, prices are almost identical, with \$3.93 per Watt (W) of system capacity in Classic towns and \$3.89/W in Express towns, and are not statistically different (p-value = 0.52). Further, we run a specification of equation (3) using price as the dependent variable, with results shown in Table B.2 in Appendix B. There is no significant effect of price across campaign types. Again, this does not mean that the *expected price* was the same throughout the campaign; in the Express towns, tier-five pricing was offered at the end of the campaign, despite not reaching the required threshold in time in all towns. Differences in price expectations in the first part of the campaigns can help explain why adoption rates are so low until the very end of the Express campaigns, which can then explain the differences in WOM.

5.3 Demand-Side Differences in Information Channels?

Although we see no substantial differences in the implementation of the campaigns across treatments, except for the number of workshops held, differences in price expectations and in the timing of adoptions may have led to differences in how potential solar adopters learned about

[19] Three out of the four Express installers were also operating in Classic towns.

[20] Characteristics of the community could affect an installer's choice of a particular marketing approach.

the campaign, and the importance they may have placed on different information sources. We use the data from our large-scale survey to assess this. In particular, two of the key survey questions are: *How did you hear about the Solarize program?* and *How important were the following sources of information in your decision to install solar?* Summary statistics on the responses to these two questions for solar adopters during the experiment can be found in Table 3 for both the Classic and Express campaigns.

One limitation of these data are that they may be subject to a selection effect, given the response rate of approximately 45% among adopters. The average response rate was 49% percent in the Express campaigns and 42% percent in the Classic campaigns; given the similarity in response rates, it is unlikely that any bias in the estimated survey variables resulting from a selection effect would be asymmetric across campaign types.[21] Thus, we interpret differences in the mean responses across treatment groups to be indicative of actual differences in the population-level means, but note the caveat that we cannot entirely rule out some selection.

One notable difference between campaigns is that solar adopters who responded to the survey in the Express campaigns were much less likely to have heard about Solarize through friends and neighbors or other solar customers than in the Classic campaigns, supporting our initial hypothesis that shortened campaigns may limit WOM. For the importance questions, print and social media are listed as relatively more important sources of information in Express towns. Although the decrease in importance of hearing about solar from friends and neighbors is lower in Express, this difference is not significant at the 5% level.

In Table 4, we display a similar comparison between Classic and Express, this time focusing on non-adopters (who expressed interest in solar and thus were included in our e-mail list), reveal-

[21] Performing a t-test, we cannot reject the null hypothesis of equal response rates for the two types of campaigns.

ing different response patterns. For instance, information about Solarize is more likely to reach non-adopters in Express towns through newspaper inserts and Solarize events, whereas online media appear to work better in Classic towns. However, for both adopters and non-adopters, in Express campaigns, more people indicate non-WOM channels as the way they heard about Solarize. Because of the randomization, these difference in the information channels through which consumers heard about Solarize should be due to the treatment, rather than than any systematic differences between the treatment groups.

5.4 Robustness Checks

Small Sample Inference

The treatment group of Classic and Express towns in our sample features a relatively small number of clusters (16). Given that inference regarding our baseline treatment effect estimates relies on asymptotic arguments, the town-level clustered standard errors may still understate the uncertainty in the estimates. Previous studies have addressed such small-sample issues using a number of techniques. For instance, Cohen and Dupas (2010) and Bloom et al. (2013) have implemented randomization inference, which does not require asymptotic arguments or distributional assumptions (Fisher, 1935; Rosenbaum et al., 2002). An alternative approach is the Young (2016) degrees of freedom correction method (see, for example, Bandiera et al. (2017)). Lastly, the Cameron et al. (2008) wild bootstrap method has found wide application in recent empirical studies (Giné and Yang, 2009; Ben-David et al., 2013; Bloom et al., 2013).

We employ some of the above small sample inference techniques to test the robustness of the statistical inference for our first main finding: that Classic and Express campaigns are equally effective in increasing solar adoption during the campaigns. First, we drop all pre-campaign ob-

servations and trim our data to a sub-sample consisting only of Classic and Express towns. We then run a simple regression of weekly installations on an Express dummy variable to capture any difference in effectiveness between the two campaigns.[22] We drop town fixed effects because the Express dummy is time-invariant within this sample. As shown in Table OA.1, the estimated Express coefficient is not statistically significant, regardless of whether we include year-week fixed effects in the regression. To address potential small sample concerns, we present p-values derived from randomization inference and wild cluster bootstrap techniques in the same table. Again, these methods suggest that there are no statistical differences between the treatments. Figure OA.1 visually illustrates how the Express coefficient estimates produced through randomization inference compare to the estimate in the main regression. It is consistent with our finding that the difference between the two campaign effects is statistically indistinguishable from zero.

As another robustness check for our baseline results, we redo the estimation in Table 2 using the wild bootstrap approach to derive the p-values and 95% confidence intervals. The regression output is shown in Online Appendix Table OA.2. Our findings are robust to the use of this alternate approach. In particular, even with bootstrapped standard errors, it is still the case that the implied total Classic and Express adoptions are not statistically different from each other, regardless of the model specification used.[23]

Choice of Control Group

Although our main object of interest in the treatment effect estimates was the relative performance of the Express and Classic campaigns in the post-period, we included the control towns to get a sense of the absolute treatment effects for each. We now perform a number of robust-

[22] We scale down the Classic and Express dummy variables by the number of weeks in the respective campaign, so that the estimated campaign coefficient represents total (instead of average weekly) campaign-related installations.

[23] The p-values from the joint significance tests exceed 0.90 in all specifications.

ness checks regarding the definition of the control group. Detailed results from these robustness checks are presented in Online Appendix A.

The first robustness check redefines the control group using propensity score matching, in order to match each treated town with its two most similar towns from the control group.[24] As displayed in Figure OA.3, this specification yields similar weekly treatment effects to our baseline results in Figure 3. In addition to the similar weekly effects over time, the overall implied treatment effects with this alternative control group are also comparable to our main results. In particular, as shown in Table OA.4, the campaign treatment effects amount to 3.33 installations per 1000 suitable roofs in an average Classic town versus 4.03 in an average Express town. These effects are consistent with our findings in Section 5.1.

We also estimate a specification in which the Solarize towns in a later round of campaigns (round six) are used as the set of control towns (using these towns' pre-Solarize data). Figure OA.4 shows the estimated weekly treatment effects using these alternative controls. Once again, the overall trend is very close to our baseline results. Similarly, the implied total treatment effects (per 1000 suitable homes) with this alternative control group, shown in the last column of Table OA.4, equal 3.07 and 3.97 for Classic and Express, respectively, and are quite close to the effects we found in Section 5.1.

Finally, we omit the control group entirely and use direct comparison of Express and Classic, excluding the control towns. Under this specification, the Express and Classic treatment effects are identified because of the staggered timing of the campaigns, with Express being shorter. Given

[24]For the matching, we use cumulative installations in the pre-treatment period, as well as the following demographic variables: population density, median household income, ethnicity, education level, unemployment, voting registration, and housing units. Table OA.3 shows the balance of covariates between each of the two campaigns and their respective matched control group. We also show the cumulative pre-trends for the treatment and matched control groups in Figure OA.2.

that in this setting, the time fixed effects absorb some of the treatment effects, we also re-run the analysis without year-week fixed effects to alleviate this issue. The detailed regression output is presented in Tables OA.5 and OA.6. As expected, dropping the time fixed effects yields total treatment effect estimates that are closer to the baseline results in Table 2. Importantly, regardless of the exact regression specification used, we find that the equality of the implied total treatment effects across the two campaign types cannot be rejected,[25] which is consistent with our main results.[26]

Outcome Variable

As a further robustness check, we employ an alternative measure of market size. More specifically, we use the total number of owner-occupied homes instead of only the subset of homes that are suitable for solar. Thus, the outcome variable in our regression becomes the number of new solar contracts per 1000 owner-occupied houses in the respective town. We then redo the analysis from Section 5.1, using CEC communities as our control group. Table OA.8 shows our regression estimates with the alternative outcome variable. As expected, the treatment effect magnitudes are different, but our main finding regarding the relative effectiveness of Classic and Express campaigns still holds. In particular, the implied total campaign effects in an average Classic and Express town are not statistically different (p-values of joint equality tests range between 0.68 and 0.78 across regression specifications). Lastly, we also derive the weekly marginal effects over the length of the Classic and Express campaigns using this alternative dependent variable.

[25] Tests of joint equality of the total number of Classic and Express installations yield p-values that range between 0.79 and 0.94 depending on the exact specification.

[26] Given the small number of clusters in this specification (16), we also implement the wild cluster bootstrap method from Cameron et al. (2008) to derive the 95% confidence intervals and test our hypotheses. The regression output is presented in Table OA.7. Again the results are the same: we see that the equality in the number of installations during the campaign across the two types of treatment cannot be rejected, with p-value from the joint equality test equal to 0.91 without town fixed effects and 0.94 with town fixed effects.

As shown in Figure OA.5, the weekly effects follow a pattern that is very similar to the baseline results in Figure 3. Hence, our findings regarding the overall effectiveness and dynamics of the two campaigns are robust to the use of a different market size measure.

6 Post-Campaign Adoption

We found that, during the campaigns, in Express there were fewer workshops, there were fewer non-adopting leads, and solar adopters were less likely to report that they had heard about Solarize via WOM and rated social information sources as less important in their adoption decision. This section explores how post-campaign adoption differs across the two campaign types as a result of these differences.

We begin by plotting the adoption rates relative to campaign timing for the Express and Classic campaigns in Figure 4. While the growth during Solarize is very similar between the two groups, this plot further underscores that Classic towns show substantially higher post-campaign adoption rates.

For a more formal analysis, we use the 24-month period post-Solarize campaign to identify the post-campaign effects on several measures. This sample consists of the 16 Classic and Express municipalities run contemporaneously. Given that our main interest is in the differences in solar adoptions across the two types of campaigns, we focus our inference on cross-campaign variation and exclude the matched control towns (for whom some of the measures do not exist).

We first explore how the number of adoptions during the campaign influences post-campaign

adoption using the following specification:

$$y_{it} = \alpha + \boldsymbol{\beta X_{it}} + \gamma B_{i,t=S} + \xi_t + \epsilon_{it}, \tag{4}$$

where y_{it} is the number of new installations in month t divided by the potential market size, $B_{i,t=S}$ is the number of adopters during the Solarize campaign divided by the potential market size at the end of the campaign (hence $t = S$), X_{it} includes a variety of control variables, and ξ_t is a time fixed effect. The control variables include the number of active installers and the number of Solarize workshops. Despite the fact we find no significant differences in prices across treatments in the post-campaign period (Table OB.8), we also include the average price of solar installations (within the unit of observation) as an additional control variable. The potential market size is the share of suitable houses minus installations that have already occurred.

In our first specification, the X_{it} includes a simple dummy variable for Express in addition to the number of active installers. The results are shown in column (1) of Table 5. We find a positive effect of the campaign installations, $B_{i,t=S}$, on post-campaign adoptions, as expected from the diffusion literature. We also find a negative effect of the Express campaigns on adoption. In our second specification (the second column), we include the number of workshops in the campaign, since the number of workshops is the only difference we found in campaign implementation across the treatment groups other than the duration. The small, insignificant coefficient on the number of workshops implies that the difference in workshops across treatments cannot explain the differences in post-campaign adoption rates, after controlling for the number of campaign installations and the Express dummy. Our third specification, in column (3), includes price as an additional control variable that might predict differences across campaigns; its effect is presum-

ably insignificant due to the limited variation in price across campaigns. Time series variation in price is captured by the monthly fixed effects.

In our last specification, in column (4), we include the number of non-adopting leads that resulted from the campaigns, which we expect to be affected by the campaign duration both directly and through the number of workshops. Like the dependent variable and the campaign installations, the leads are measured in total leads per 1000 potential adopting households. The effect of leads is positive and significant, but we find that the effect of campaign installations has almost 30 times the effect of the non-adopting leads on post-campaign adoption rates. This is not surprising if the effect of the leads is to share WOM – we would expect solar adopters to be more likely to share WOM than non-adopters, despite the role of both.

To examine whether the effect of more campaign leads is due to WOM or is simply because those leads then adopt in the post-campaign period, we combine the data on sales leads with the CGB database on all solar installations based on personal identifiers.[27] We find that during the Solarize campaign, most adopters (1,135 out of 1,722 total adoptions for the pooled results) can be directly linked to the Solarize campaign; i.e., we can identify the adopters in the list of sales leads that have been collected during the campaign. This corresponds to roughly two-thirds of all adopters during the campaign time window. Focusing on the 12-month period following a Solarize campaign, this rate decreases to approximately 13% (158 / 1207). In other words, approximately 87% of adopters in the first year post-campaign period are new adopters that did not participate in Solarize events and thus were not considered 'leads' by the campaigns. In line with these findings, our survey results indicate that only 28% of individuals that did not adopt

[27] We are able to successfully match 85% of all (self-declared) survey adopters to the CGB database based on individual names, addresses, municipalities, and email addresses. Non-matches are likely due to contract cancellations, cohabiting (different individuals responding to the survey and registering the solar plant), and misreporting of installations.

during Solarize answer that there is a 'very good chance' that they will adopt solar in the future. A similar percentage (27%) see either 'no chance' or 'very little chance' of future adoption.[28] This descriptive evidence leads us to believe that it is quite unlikely that the effect of leads on post-campaign adoption is that these leads wait to adopt themselves. A far more likely interpretation, consistent with our survey evidence, is that the effect of leads on post-campaign adoption is due to the WOM generated by these leads.

In our last specification, in column (5), we replace the number of leads with the amount of campaign WOM, as measured by the survey responses. To create our measure of WOM, we average the values of the fraction of survey respondents in the town who report hearing about the campaign from a friend or neighbor and those hearing about the campaign from another solar customer. It should be noted that while the survey data provides a good proxy for WOM, it is not an absolute measure of WOM. One benefit of asking adopters how they heard about the campaign is that we are less concerned with invalid reporting than if we had asked about WOM that they shared, an issue noted by Godes and Mayzlin (2009) in their design. However, since adopters' exposure to WOM will not necessarily be the same as potential adopters, the quantification of the role of WOM will be subject to this caveat.

We find the same pattern of results as with leads, but with stronger statistical significance. Campaign WOM has a positive and significant effect on post-campaign adoption rates. If we increase the number of respondents who hear about Solarize from a friend or neighbor or another solar customer by 10% (increasing the WOM measure from an average of 0.124 across town-month observations to 0.136), the adoption rate increases from an average of 0.608 adoptions per month per 1000 suitable households to 0.657, an increase of 8.1%. In other words, the WOM elasticity is

[28] Pooled survey answers from Solarize Classic and Express of individuals that did not adopt during Solarize, with N = 1,023.

0.81.

One potential critique of these results is that the number of leads and WOM during the campaigns might be endogenous to shocks that persist during and after the campaigns which affect adoption rates post-campaign and WOM during the campaign. It is hard to think of such unobservables that might remain after the inclusion of the number of installations during the campaign, but it is possible. The same critique might be made regarding the number of campaign adoptions. This potential endogeneity concern motivates one additional set of analyses. Given the fact that the Express dummy variable becomes insignificant, with a point estimate near zero, when we include the variables for WOM and the number of leads, and the fact that we find the number of workshops in and of themselves do not increase post-campaign adoption (controlling for adoptions during the campaign), we can instrument for the number of leads or WOM with the Express dummy. The exclusion assumption is that the effect of the experimental assignment to Express and/or the number of workshops operates indirectly through leads or WOM.

As a second instrument (which allows us to perform a test of over identifying restrictions), we turn to findings by Kraft-Todd et al. (2018), who found that a solar ambassador's adoption decision causally impacts campaign success because of the credibility it lends to the campaign via second order beliefs (the potential adopters beliefs about what the ambassadors believe about the value of solar and the Solarize program). Kraft-Todd et al. (2018) used the suitability of the ambassadors' rooftops as an instrument for the ambassadors' adoption decisions, since the suitability of the roof can be considered exogenous as it is pre-determined by the required panel orientation and shading of the housing lot. Similarly, we can use the suitability of the ambassadors' roof for solar as an instrument for the WOM during the campaign.

In practice, when we try to instrument for the number of leads with the experimental assign-

ment to the Express condition, the first stage F-statistic is too low, indicating that Express is not a strong enough manipulator of leads to avoid a weak instruments issue. The same is not true for WOM. In Table 6, we report the results of IV regressions in which we instrument for WOM. Column 1 shows the OLS results, which now exclude the Express dummy variable. By omitting the Express dummy and the number of workshops (which create the leads), the coefficient on the non-adopting leads doubles relative to the Table 5 results. In column (2), we instrument for WOM, using the Express dummy and the ambassador's roof suitability, also interacting both with the start date of the campaign since we expect the effects on WOM to vary based on the start dates (later campaigns will run longer into the colder, winter months). The first stage regression results are shown in Online Appendix C.

We find that there is a positive and significant effect of campaign WOM on post-campaign adoption rates. The magnitude is only slightly lower than that found in Table 5; one explanation for the slight difference is that this is a local average treatment effect of WOM, measured via the number of non-adopting leads and the survey responses, rather than an average treatment effect. The exclusion restriction for Express is supported by the linear regression shown in Table 5, where we see no direct effects of Express, the number of workshops, or campaign installations when we account for leads or the WOM survey responses.

To allow for different effects of WOM over time, we also interact the leads variable with a time dummy that indicates the distance (in years) from the campaign conclusion. Analogous tables to Tables 5 and 6 allowing for time-varying effects are shown in Online Appendix B. All of the findings hold, although the effects are smaller and lose significance in the second year post-campaign.

6.1 Robustness Checks

Similar to the campaign analyses, one critique of the post-campaign analyses is the small sample size. Again, we perform additional robustness checks, with results reported in Online Appendix B. First, we use randomization inference when estimating the main effect of Express on post-campaign adoptions for small-sample robust inference. We again find statistical significance in Express leading to lower post-campaign adoption rates. Similarly, employing wild cluster bootstrapped standard errors, we find that Express remains significant with p-values around 10% in the main regressions.

Another solution to the small sample issue is to use more data. Thus, we repeat the analyses for the main results, pulling in data from other rounds of the Solarize program. We have data for three other rounds of the Classic Solarize program, one before this experiment and two after. We do not include the very first round of Solarize Classic campaigns due to missing data on the leads and workshops. The timeline for the four rounds of campaigns we use, rounds two to five, are shown in Online Appendix C in Table OC.2 – the Express experiment was run in round 3. We run the same analysis as before using this full set of 35 towns across the four rounds, employing month fixed effects and indicator variables for each round of Solarize implementation. The results are completely robust when including these other campaigns.[29]

As a final robustness check, we use an alternative measure of market size, namely the entire set of owner-occupied homes instead of just the number of homes deemed suitable for solar due to available sunlight. Again, all of the results are robust.

To test whether a reduced (stated) importance of WOM leads to fewer adoptions in the post-campaign period for Express, we also turn to methods used in the consumer behavior literature

[29] As we include further rounds of Solarize, it is key to control for price differences across campaigns.

to provide mechanistic support in experimental settings. We use a two-stage mediation process model (Model 6, Hayes, 2013) to test whether the Express campaign led to fewer people hearing about Solarize via WOM, which *then* led to fewer people rating WOM as important in their adoption decision, which *then* led to fewer post-campaign adoptions. This does allow for a direct causal effect of Express on post-campaign adoptions as well. For the first mediator, we use the WOM measure as before by averaging the fraction of respondents who heard about the campaigns from solar customers and from friends or neighbors. We use the survey response for the importance of WOM as our second mediator. The model (with results) is shown in Appendix D in Figure 8.[30] As with our regression results, we find that the effect of Express on adoptions is mediated by WOM: the Express campaign leads to fewer people hearing about Solarize via WOM, which leads to lower stated importance of WOM, which leads to lower campaign adoptions.

7 Implications for Marketers and Policymakers

Intensive marketing campaigns are used for new product launches, seasonal goods, fundraising, blood drives, etc. To maximize efficacy, it is essential to understand how the duration of such campaigns affect adoption decisions both during and after the campaigns. In our application, we found that campaign adoptions were comparable in the experimentally shortened campaigns, but that in the 24 months post-campaign, adoption is reduced by 0.287 monthly installations per 1000 solar suitable homes (using the column (1) results from Table 5). This translates to an average of 37.5 fewer installations for the Express campaigns than in the Classic campaigns.[31]

[30] We use structural equation modeling to implement the mediation analysis. Our hypothesis is that the only significant links will be from Express to how people heard about Solarize (a_1), how they heard to the importance of WOM (d_{21}), and the importance of WOM to the number of post-campaign installations (b_2).

[31] Average total adoption is 72.2 during the 24 months post campaign period. With an average solar market size of 5453 households for the Express and Classic towns, we have that $24 * 0.287 * 5453/1000 = 37.5$.

At least some of the effect of intensive marketing campaigns after they conclude is through continued WOM, especially for categories in which WOM has been shown to affect adoption, such as for durable and pro-social goods. We do indeed find that the main differences in the shortened campaigns are the reduction in the number of workshops, the number of leads, and the amount and importance of WOM. We find that the reduction in the leads and WOM significantly reduces adoption in the post-campaign period. Since few of the leads themselves are adopting post-campaign, we interpret both results as indicative of the importance of campaign WOM for post-campaign adoption rates.

This has huge ramifications for the calculated cost-effectiveness of the campaigns. After deducting the fixed costs, the total variable costs per campaign are approximately $33,333 for Classic and $33,500 for Express.[32] We proceed to convert these cost figures into a dollar-per-contract measure of the cost-effectiveness of each campaign. Our estimates in columns (2) in Table 2 suggest that the average Solarize program leads to a total of 22 additional installations for Classic towns and 24 additional installations for Express towns. This implies an average cost of approximately $1,515 per additional contract for the Classic program and $1,289 per contract for Express.

These estimates ignore the costs associated with the price discounts during the campaign.

[32]We first break down the total costs of the program into a fixed component, which is independent of campaign duration and number of participating towns, and a variable component which varies along either one or both of these dimensions. CGB and SmartPower staff time is allocated to drafting the initial request for proposals for solar installers and towns, as well as reaching out to communities to inform them about the program and encourage them to apply. The cost of this staff time is fixed, regardless of campaign length or number of participating municipalities. On the other hand, a number of program-related costs, associated with creating a campaign website for each town, supplying marketing materials to the communities, initial meetings with town leaders and volunteers, and organizing the kickoff event in each municipality, are independent of the campaign duration, but vary by the number of towns participating in the program. Lastly, travel expenses for SmartPower staff, related to organizing and participating in events throughout the program, are both participation- and duration-dependent. These duration-dependent costs are lower for shorter campaigns. Some of the towns participated in the campaign as a coalition. In particular, there were two coalitions among the Classic towns: Ashford-Chaplin-Hampton-Pomfret and Easton-Redding-Trumbull, and one coalition among the Express towns: Roxbury-Washington. We calculate costs on a per-campaign basis since costs are shared across towns in the coalitions, and then calculate the costs per installation as if each town had carried out a separate campaign.

Using the estimated price effects for Solarize campaigns found in (Gillingham and Bollinger, 2017), who found a decline in price of $0.64 per Watt with an average system size of 4.23kW, the cost of the price discount is $2,700 per installation. This ignores any additional cost declines that might result from participating in Solarize. Hence the total cost of acquiring a customer through Solarize is over $4,000, and are similar for Classic and Express, if we ignore post-campaign effects.

When we also account for the fact that Classic campaigns lead to an average of 37.5 *more* installations per town than Express campaigns in the 24 months after the campaigns concluded due to the increased WOM, the Classic campaigns are clearly much more cost-effective. Working with the assumption that the Express campaigns return to similar post-campaign adoption rates, i.e. that the 37.5 more installations in the Classic campaigns in the following two years are a lift in installations relative to baseline, we calculate the cost per installation in Express to be approximately $4300, versus $1500 in Classic.[33] Installers we have spoken with place customer acquisition costs in the range of $2,000 to $3,000, so while the Classic campaigns are very cost effective in acquiring new customers, the Express version of the campaign is less efficient, relative to installers acquiring new customers themselves.

8 Concluding Remarks

In light of the evidence in the literature indicating that shorter deal durations could increase the likelihood of purchase, this paper employs a field experiment to test the effect of reducing the length of the "Solarize CT" program on residential PV adoption. Specifically, we compare the effectiveness and cost-effectiveness of a 24-week Solarize Classic program versus a 12-week Solarize Express program by randomly assigning towns to each of the programs. We compare

[33] Express: ((21.7 * 2700) + 35000)/(21.7 + 0) = 4313. Classic: ((19.2 * 2700) + 33300)/(19.2 + 37.5) = 1502.

these two randomly assigned treatments to a control group of CT towns and employ a difference-in-differences approach to estimate and compare the treatment effects. We then focus on the long-term adoption rates across the campaign types. We found that although the short-term effects are comparable, long-term adoption rates after the Express campaigns are suppressed, highlighting the risk of shortening marketing campaigns for high-involvement products that rely on WOM.

The main limitation of this paper is the small number of towns we are able to experimentally assign due to logistical and cost considerations. This is 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. Such randomization is necessary given the desired object of study, namely peer influence among those in the community. 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. The combination of the randomized field experiment with the extensive survey data allows us to examine the mechanisms behind the long-term effects on durable good adoption.

In terms of mechanism, we demonstrate that the role of WOM during the campaigns can explain the differences in post-campaign adoption rates. The early adopters in Express were likely to be reached via information channels other than WOM, since SmartPower increased the resource intensity and it takes time for WOM to spread. We think the most likely explanation for differences in post-campaign adoption rates is that solar adopters who hear about Solarize via WOM are then more likely to share WOM, although we cannot rule out other alternative explanations. For example, it could be that differences in the number of workshops or price expectations during the campaign lead different types of consumers to adopt in Express compared

to those who adopt in Classic (realized prices would not lead to a selection effect since price were shown to be the same across campaign types). If the type of consumers who adopt in Classic are different than those who adopt in Express, and if they are also more likely to be sharers of WOM, then this could also explain why the levels of WOM are different across treatments, and why post-campaign adoption rates are different. It should be noted that under this alternative explanation, the differences in post-campaign adoption rates are still explained by the differences in WOM, and thus the conclusions regarding the cost-effective of the Express campaigns and the importance of WOM are unchanged.

We also recognize that there may be other equally important mechanisms in the diffusion of other products. Other potential mechanisms behind peer effects include visibility of adoption as shown in the context of dry landscape conversions (Bollinger et al., 2018), and the establishment of social norms by observable aggregate statistics specific to the area or to peers (Allcott, 2011). However, solar PV is a category in which we would expect these other mechanisms to play a role, and yet we still see strong evidence that WOM affects future adoption. Thus, we might expect WOM to play an even larger role in other durable product categories, which might be less visible and less subject to social norms.

Our results are informative for product and marketing managers. In new product launches, managers need to manage a number of control variables. Little is known about the optimal duration of these campaigns, and how the duration interacts with word-of-mouth. Even less is known about the post-campaign implications. We contribute to this very nascent literature on introduction campaign dynamics and deal duration, as well as to the more established literature on peer effects in durable good adoption.

Our setting also provides a substantive contribution. The Department of Energy and other

state and local government agencies have poured billions of dollars into initiatives to increase solar adoption and reduce the costs associated with an installation, a large part of which is the customer acquisition cost. If shorter campaigns have similar or even greater efficacy than longer campaigns with the same total marketing effort, adoption of solar panels (which result in positive externalities through learning-by-doing and environmental benefits) can be expedited. In this application, the Express campaigns were just as costly to run despite a shorter duration both due to the extra effort required to run the campaign successfully and to a desire to keep the total resources allocated to each campaign the same. Given this finding, and the lower post-campaign adoption rates in the shortened campaigns, SmartPower and the U.S. Department of Energy have moved away from running compressed versions of the Classic campaigns, and have shifted its focus to expanding geographic reach and facilitating low and moderate income adoption using the Classic campaign format.

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Figures & Tables

Table 1: Solarize Installations & Sales Leads

	Classic			Express			
VARIABLE	Mean	Sd	Median	Mean	Sd	Median	T-test
Sales leads / solar suitable homes [×1000]	246.36	223.03	219.88	61.81	44.40	65.30	0.09
Campaign installs / solar suitable homes [×1000]	10.39	7.80	6.93	7.32	5.97	4.01	0.45
# Municipalities		11			5		

Note: Classic: subset of Solarize towns that participated in Classic campaign contemporaneous to Solarize Express. Campaign installs is defined as total number of installations during Solarize / solar suitable homes [x1000]. Sales leads is total number of leads collected during Solarize / solar suitable homes [x1000]. Two-sample t test for differences in mean. Unit of observation: town.

Table 2: Average Treatment Effects of Classic and Express

VARIABLE	(1)	(2)	(3)	(4)
Classic	0.143***	0.143***		
	(0.044)	(0.041)		
Classic first 8 weeks			0.054***	0.054***
			(0.020)	(0.017)
Classic middle 8 weeks			0.029	0.029
			(0.019)	(0.018)
Classic last 8 weeks			0.357***	0.357***
			(0.115)	(0.112)
Express	0.237*	0.230*		
	(0.133)	(0.131)		
Express first 8 weeks			-0.016**	-0.023***
			(0.007)	(0.008)
Express last 8 weeks			0.528*	0.521*
			(0.282)	(0.281)
Total Classic Adoptions (per 1000 HH)	3.43	3.43	3.52	3.52
Total Express Adoptions (per 1000 HH)	3.79	3.68	4.10	3.98
Town FE	no	yes	no	yes
Year-Week FE	yes	yes	yes	yes
R-squared	0.093	0.087	0.140	0.137
N	8716	8716	8716	8716

Note: Dependent variable is the weekly number of signed solar contracts per 1000 solar suitable owner-occupied homes. Unit of observation is town-week. TE refers to treatment effect. Robust standard errors clustered at the town level in parentheses (62 clusters). We also use the wild cluster bootstrap from Cameron et al. (2008) and we find that the results of our hypothesis tests are identical. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 3: Summary Statistics of Adopter Survey Responses: Classic vs. Express

WOM Channels							
	Classic towns			Express towns			Difference
VARIABLE	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Mean
Friend/neighbor	149	0.154	0.363	75	0.040	0.197	0.114**
Town leader	149	0.148	0.356	75	0.133	0.342	0.015
Solar customer	149	0.094	0.293	75	0.013	0.115	0.081**
Newspaper	149	0.107	0.311	75	0.160	0.369	-0.053
Social Media	149	0.034	0.181	75	0.053	0.226	-0.019
Online media	149	0.128	0.335	75	0.080	0.273	0.048
Solarize event	149	0.329	0.471	75	0.307	0.464	0.022
Installer	149	0.034	0.181	75	0.040	0.197	-0.006
Importance of Information Sources							
	Classic towns			Express towns			Difference
VARIABLE	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Mean
Friend/neighbor	137	3.431	2.165	67	3.224	1.976	0.207
Social media	134	1.903	1.381	66	2.394	1.568	-0.491**
Installer website	134	3.649	1.901	66	3.576	1.789	0.073
Solarize ambassador	135	3.896	2.155	70	4.157	1.893	-0.261
Someone in town	135	3.444	2.188	67	3.463	1.894	-0.019
Someone you work with	134	2.769	1.911	66	3.015	1.893	-0.246
Print media	136	3.301	1.906	69	3.855	1.825	-0.554**
Seeing a solar installation	138	4.188	1.862	67	3.866	1.841	0.322
Num. of responses per coalition	6	24.833	9.806	4	18.75	8.18	

Note: Each response variable for "WOM Channels" is a binary variable, which equals 1 if the respondent learned about the Solarize program through the respective information channel and 0 otherwise. Each response variable for "Importance of Information Sources" is a categorical variable, which takes values between 1 and 7 depending on the perceived importance of the information source. The outcome values are defined as follows: 1 = not at all important, 2 = very unimportant, 3 = somewhat unimportant, 4 = neither important nor unimportant, 5 = somewhat important, 6 = very important, 7 = extremely important. Asterisks next to variable means denote significance levels from a t-test between the two treatment groups. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 4: Summary Statistics of Non-adopter Survey Responses: Classic vs. Express

	WOM Channels					
	Classic towns			Express towns		
VARIABLE	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Friend/neighbor	311	0.125	0.332	106	0.104	0.306
Town leader	311	0.177	0.382	106	0.189	0.393
Solar customer	311	0.087	0.282	106	0.057	0.232
Newspaper	311	0.141**	0.349	106	0.226**	0.42
Social Media	311	0.042	0.2	106	0.066	0.25
Online media	311	0.125**	0.332	106	0.057**	0.232
Solarize event	311	0.161***	0.368	106	0.311***	0.465
Installer	311	0.061	0.24	106	0.057	0.232

Note: Each response variable is a binary variable, which equals 1 if the respondent learned about the Solarize program through the respective information channel and 0 otherwise. Asterisks next to variable means denote significance levels from a t-test between the two treatment groups. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 5: Installation Growth Post-Solarize

	(1)	(2)	(3)	(4)	(5)
Campaign installs	0.048*	0.048*	0.112***	0.069***	0.084***
	(0.026)	(0.026)	(0.029)	(0.016)	(0.026)
Express	−0.287*	−0.397*	−0.362	−0.392	0.119
	(0.152)	(0.205)	(0.378)	(0.250)	(0.260)
# Active installers	0.256**	0.258**	0.272*	0.235**	0.207
	(0.117)	(0.116)	(0.135)	(0.095)	(0.120)
Workshop		−0.033	−0.025	−0.105*	−0.021
		(0.038)	(0.046)	(0.051)	(0.034)
Price per Watt			−0.070	−0.067	−0.051
			(0.075)	(0.070)	(0.067)
Sales leads				0.002**	
				(0.001)	
Word-of-Mouth					3.915***
					(1.308)
Constant	−0.084	0.130	−0.083	0.388	−0.438
	(0.407)	(0.381)	(0.410)	(0.406)	(0.262)
Observations	384	384	276	276	276
R^2	0.211	0.214	0.538	0.598	0.573
Month FE	Y	Y	Y	Y	Y

Note: Dependent variable: solar installations normalized by potential market size (solar suitable households - cumulative installations) [x1000]. Solarize related variables: campaign installs, defined as total number of installations during Solarize normalized by potential solar market [x1000], Express: categorial variable for shorter version of Solarize, number of Workshop and Sales leads (normalized by potential solar market) collected during Solarize, Word-of-Mouth, defined as the average share of adopters that heard about solar from a friend or neighbor. Unit of observation is town-month. Main sample: 16 Solarize Classic and Express campaigns observed for 24 month after the conclusion of Solarize. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

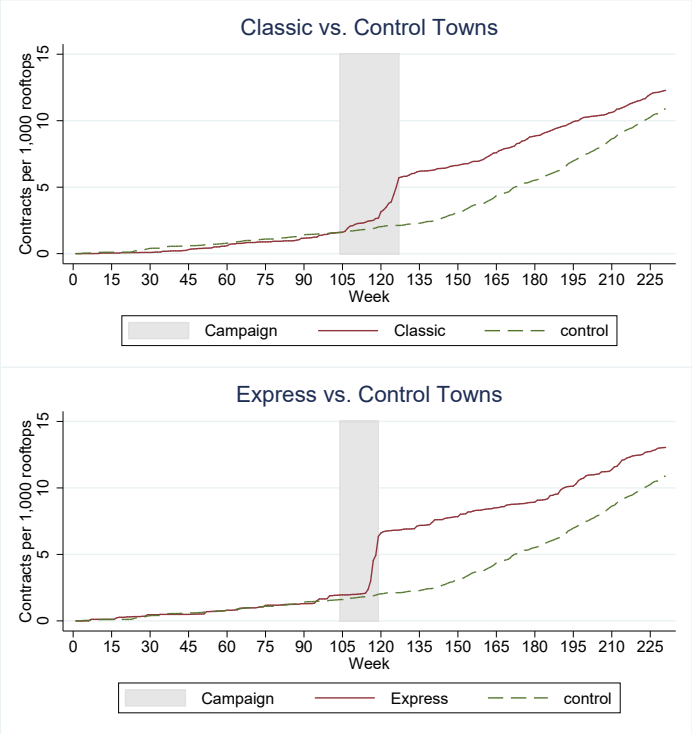
Table 6: Word-of-Mouth effects

	OLS (1)	IV (2)
Campaign installs	0.086***	0.089***
	(0.021)	(0.019)
# Active installer	0.317**	0.306***
	(0.110)	(0.103)
Price per W	−0.029	−0.035
	(0.071)	(0.064)
Word-of-Mouth	3.732**	3.264**
	(1.462)	(1.431)
Constant	−0.729**	−0.164
	(0.278)	(0.369)
Observations	221	221
R^2	0.605	0.604
# Excluded Instruments	-	4
First stage F statistic	-	93.84
Month FE	Y	Y

Note: Dependent variable: solar installations normalized by potential market size (solar suitable households - cumulative installations) [x1000]. Campaign installs, defined as total number of installations during Solarize normalized by potential solar market [x1000], Word-of-Mouth (WOM), defined as the average share of adopters that heard about solar from a friend or neighbor. Unit of observation is town-month. Main sample: 13 Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Column 2 instruments for WOM with "solar ambassador home suitable for solar" × quarter of campaign start and the experimental variation from Express. First stage F-statistic obtained after partialling-out the month fixed-effects. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

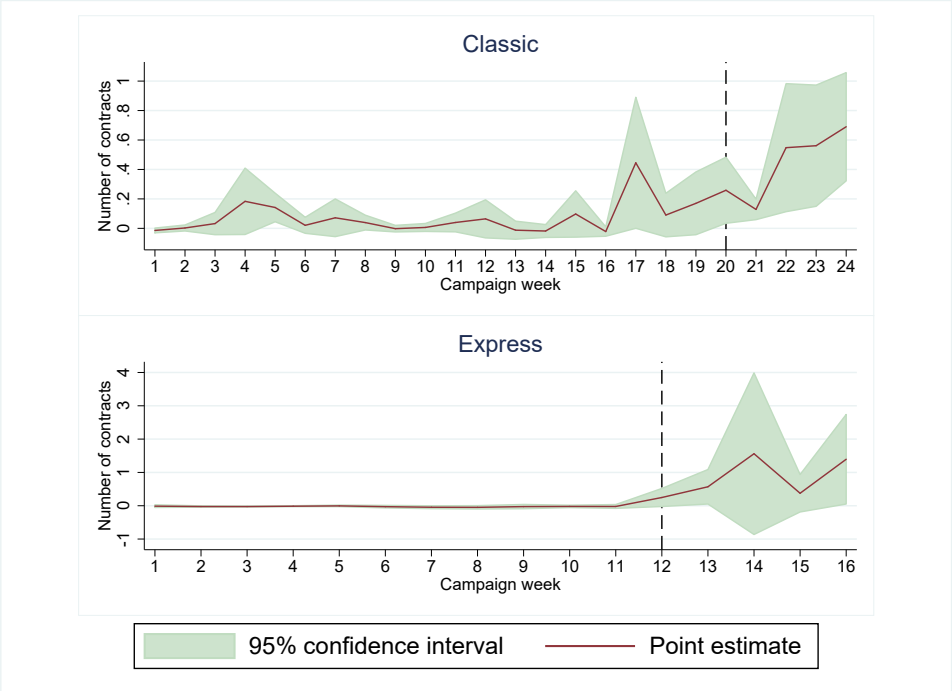
Figure 1: Examples of Solarize Presence in the Community

Figure 2: Cumulative Number of Contracts in Express Field Experiment



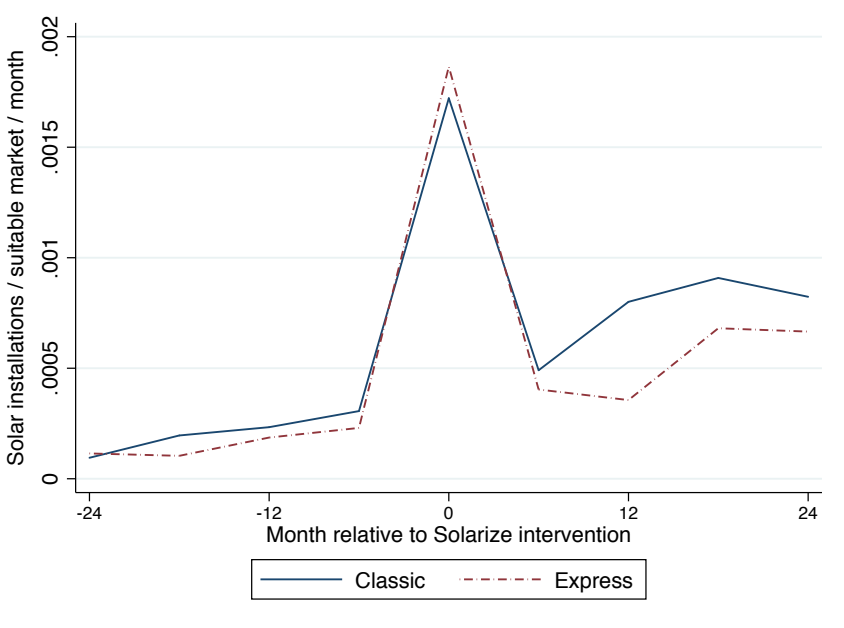
Note: Adoptions, including two years prior to the campaign start dates, 24-week period for Classic campaigns, 16 week period for Express campaigns, and two years post the campaign end dates.

Figure 3: Weekly Treatment Effects of Solarize Classic and Express over Time



Note: Total estimated Installs per 1000 HH is 3.52 for Classic campaigns, 3.88 for Express.

Figure 4: Mean Solar Growth, Express vs. Classic



Note: Figure indicates mean solar growth, defined as number of solar installations divided by suitable solar market. 16 Solarize Classic and Express campaigns. Unit of observation is town-half year. Period of observation: 24 months prior to Solarize, up to 24 months post-Solarize.

Appendix A: Solarize Timelines and Covariates

Table A.1: Campaign Dates

Classic towns			Express towns		
TOWN	Start date	Official end date	TOWN	Start date	Official end date
Ashford	9/24/2013	2/11/2014	Hamden	11/18/2013	2/11/2014
Chaplin	9/24/2013	2/11/2014	Glastonbury	11/18/2013	2/11/2014
Easton	9/22/2013	2/9/2014	Roxbury	11/10/2013	2/4/2014
Greenwich	10/2/2013	2/18/2014	Stafford	11/14/2013	2/11/2014
Hampton	9/24/2013	2/11/2014	Washington	11/10/2013	2/4/2014
Manchester	10/3/2013	2/20/2014			
Newtown	9/24/2013	2/28/2014			
Pomfret	9/24/2013	2/11/2014			
Redding	9/22/2013	2/9/2014			
Trumbull	9/22/2013	2/9/2014			
West Hartford	9/30/2013	2/18/2014			

Note: With the exception of Newtown, where the Solarize campaign was extended by two weeks, the campaigns in all remaining Classic towns are officially 20 weeks long, while the campaigns in all Express towns last 12 weeks. In both variants, the majority of installations in the following four-week period are classified by the installers as Solarize installations.

Table A.2: Balance of Covariates

	Classic		Express		Control	
VARIABLE	Mean	St. Dev.	Mean	St. Dev	Mean	St. Dev
Population density	820.7	955.5	578.3	746.5	781.4	878.0
Median income	97714**	27868	81568	17717	80245	24076
% White	0.905	0.083	0.887	0.113	0.908	0.096
% College degree	0.480	0.041	0.493	0.050	0.483	0.043
% Unemployed	0.083	0.022	0.070	0.022	0.082	0.025
% Democrat voters	0.309	0.070	0.330	0.090	0.316	0.073
#Occupied units	9394	9866	8622	9375	6073	5667
% Solar suitable homes	57.60	4.92	66.88	17.64	71.75	12.45
# Solar suitable homes	5524	5848	4720	4534	6829	8077
Number of towns	11		5		40	

Note: Voting data are collected from the Office of the Secretary of State. Solar suitability from Google Project Sunroof and GeoStellar. All other data come from the 2009-2013 wave of the American Community Survey. Solar-suitable homes: # homes (2010) x percentage of homes suitable for solar. Asterisks next to mean values denote the significance levels from a t-test comparing the mean of each variable across the control group and the respective treatment group. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Appendix B: Examining Campaign Differences

Table B.1: Importance of CGB and SmartPower

VARIABLE	Classic	Express	p-val
Selecting installer	2.08	2.71	0.19
Contracting with installer	1.64	2.50	0.16
Weekly report of installer data	2.81	2.33	0.49
Biweekly call with installer and volunteers	2.42	2.63	0.72
Marketing materials	2.13	2.88	0.21
Town website	3.54	3.29	0.73
Social media	3.36	3.67	0.61
Press releases	2.69	3.14	0.39
Kickoff event	2.47	2.44	0.96
Solar ambassador meetings	2.87	2.88	0.99
Troubleshooting issues	3.19	3.14	0.94

Note: For each category, ambassadors are asked to rate the importance of CGB and SmartPower's input. Each response is a categorical variable: 1 = essential, 2 = very important, 3 = important, 4 = not important, 5 = not needed. "Classic" and "Express" columns display means of responses within the respective campaign. p-values in last column are from a t-test comparing the mean of each variable across Classic and Express towns.

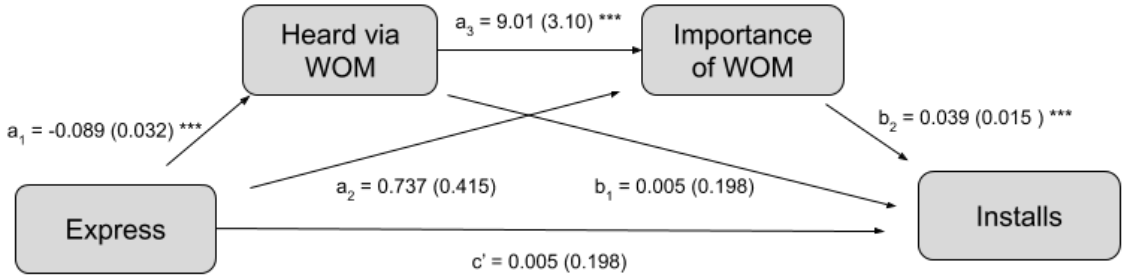
Table B.2: Average System Price Effects

VARIABLE	**(1)**	**(2)**
Classic	-0.310***	-0.510**
	(0.110)	(0.224)
Express	-0.441**	-0.446**
	(0.210)	(0.175)
F-test	0.52	0.07
	(0.47)	(0.79)
Town FE	no	yes
Year-Week FE	yes	yes
R-squared	0.262	0.226
N	631	631

Note: Dependent variable is the average weekly post-incentive solar system price. Unit of observation is town-week. Only observations with positive sales (and observed prices) used in the analysis. TE refers to treatment effect. "F-test" displays F-statistic (p-value in parentheses) from a test of equality of the two campaign effects. Robust standard errors clustered at the town level in parentheses (62 clusters). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Appendix C: Post-Campaign Mediation Analysis

Figure C.1: Mediation Analysis



We use a two-stage mediation process model (Model 6, Hayes 2013). How people heard about Solarize is mediator M_1 and the importance of WOM is mediator M_2. We confirm our hypothesis is that the only significant links will be from Express to how people heard about Solarize (a_1), how they heard to the importance of WOM (d_{21}), and the importance of WOM to the number of installations (b_2). The direct effect of Express on post-campaign installations, c', is insignificant. For this analysis, we only include the Express municipalities and the contemporaneous Classic municipalities, relying exclusively on variation from the experimental manipulation. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Online Appendix A: Treatment Effects Robustness Checks

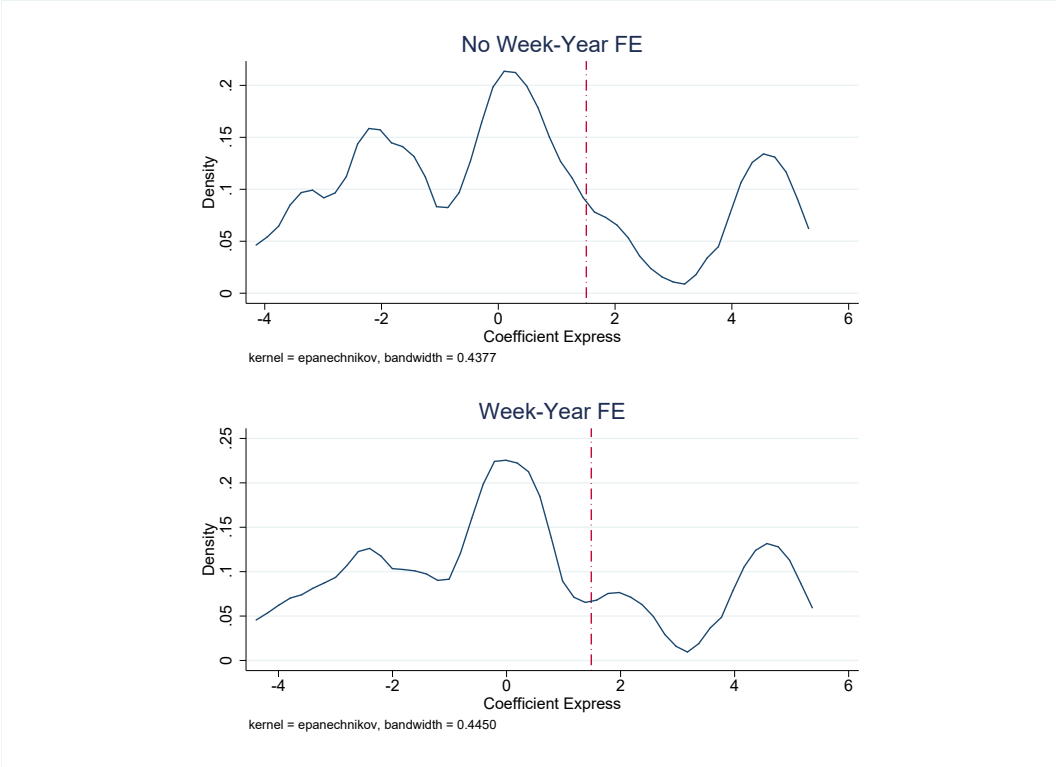
During Campaign Analysis using Small Sample Inference

Table OA.1: Small Sample Robustness Checks

VARIABLE	**(1)**	**(2)**
express	1.508	1.486
	(2.275)	(2.427)
p-*value*		
Baseline regression	0.52	0.55
Randomization inference	0.69	0.57
Wild cluster bootstrap	0.74	0.61
Town FE	no	no
Year-Week FE	no	yes
R-squared	0.005	0.225
N	360	360

Note: Dependent variable is the weekly number of signed solar contracts per 1000 owner-occupied homes. Unit of observation is town-week. Sample includes only campaign observations from Classic and Express towns. Express coefficient measures total (instead of average weekly) effect relative to Classic. Robust standard errors clustered at the town level (16 clusters). p-values reported from testing the null hypothesis that the treatment has no effect. "Randomization inference" and "Wild cluster bootstrap" report the same p-value using randomization inference (1000 draws) and wild cluster bootstrap (1000 draws) techniques, respectively. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Figure OA.1: During Campaign Analysis: Randomization Inference



Note: Randomization inference for the effect of Solarize Express in Table OA.1. The figure plots the kernel density for estimates that are obtained by randomly assigning the Express treatment (5 towns) within the total set of 16 towns. We perform all possible combinations to calculate the exact p-values for one-sided tests. The vertical line indicates the true treatment effect for Express. In both specifications, we find that approximately 70% of all possible combinations produce an estimate that is smaller than the true effect and 30% produce a larger estimate, indicating p-values greater than 0.1 for both one-sided tests.

Table OA.2: Average Treatment Effects of Classic and Express (Wild Bootstrap)

VARIABLE	(1)	(2)	(3)	(4)
Classic	0.143***	0.143***		
	[0.044, 0.250]	[0.051, 0.243]		
Classic first 8 weeks			0.054***	0.054***
			[0.010, 0.104]	[0.017, 0.098]
Classic middle 8 weeks			0.029	0.029
			[-0.007, 0.069]	[-0.006, 0.068]
Classic last 8 weeks			0.357***	0.357***
			[0.100, 0.628]	[0.109, 0.620]
Express	0.237**	0.230**		
	[0.021, 0.825]	[0.022, 0.811]		
Express first 8 weeks			-0.016**	-0.023***
			[-0.036, -0.001]	[-0.044, -0.004]
Express last 8 weeks			0.528**	0.521**
			[0.062, 1.769]	[0.063, 1.754]
Town FE	no	yes	no	yes
Year-Week FE	yes	yes	yes	yes
R-squared	0.093	0.087	0.140	0.137
N	8716	8716	8716	8716

Note: Dependent variable is the weekly number of signed solar contracts per 1000 owner-occupied homes. Unit of observation is town-week. TE refers to treatment effect. Robust standard errors clustered at the town level (62 clusters). 95% confidence intervals, reported in square brackets, obtained with wild cluster bootstrap (1000 draws). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

During Campaign Analysis with Different Control Towns

Methodology

Propensity score matching: We employ a propensity score matching procedure to select towns closest to our treated communities in terms of cumulative pre-Solarize contracts (i.e., total number of PV contracts signed during the two-year pre-treatment period) and a set of demographic and socioeconomic characteristics. We obtain town-level data on population density, median household income, ethnic groups, education level, unemployment, and housing units from the 2009-2013 wave of the American Community Survey (ACS). We also draw town-level voting registration data for 2013 from the Office of CT's Secretary of the State (http://portal.ct.gov/sots). Our approach is straightforward. First, for each Solarize program (Classic and Express), we utilize a Probit model to estimate a propensity score, representing the probability of selecting into the program, as a function of the vector of covariates, listed in Table OA.3. We then match, with replacement, each of the treated towns to the two control towns with closest propensity scores. Using the new sample, we re-run our analysis from Section 5.4.

Current towns: We use an alternative set of control towns which are currently part of the Solarize program. In the Spring of 2016, Solarize campaigns were run in seven towns in Connecticut: Barkhamsted, Fairfield, Harwinton, Hebron, New Haven, North Haven, and Wilton. Because Fairfield was already part of an earlier Solarize campaign from September 2012 until January 2013, we exclude it from the list of control towns, leaving us with six towns.

Table OA.3: Balance of Covariates with 2N Matching

	Classic		Control for Classic		Express		Control for Express	
VARIABLE	Mean	St. Dev.	Mean	St. Dev	Mean	St. Dev	Mean	St. Dev
Population density	820.7	955.5	1085.9	1184.8	578.3	746.5	1366.5	1506.3
Median income	97714	27868	89128	23851	81568	17717	92739	26555
% White	0.905	0.083	0.860	0.129	0.887	0.113	0.870	0.140
% college degree	0.480	0.041	0.492	0.060	0.493	0.050	0.513	0.062
% unemployed	0.083	0.022	0.084	0.018	0.070	0.022	0.088	0.028
% Democrat voters	0.309	0.070	0.304	0.048	0.330	0.090	0.306	0.087
# Occupied units	9394	9866	13899	15428	8622	9375	15985	17279
% Solar suitable homes	57.60	4.92	57.94	9.56	66.88	17.64	58.89	8.16
# Solar suitable homes	5524	5848	7932	8837	4720	4534	9971	11110
Number of towns	11		11		5		8	

Note: Matching weights are used to calculate the weighted mean and standard deviation of the control groups. A t-test comparing the weighted mean of each variable in the control group to the mean in the respective treatment group finds no statistically significant differences.

Table OA.4: Total Treatment Effect by Specification

VARIABLE	Matching	Current
Total Classic Adoptions (per 1000 HH)	3.33	3.07
Total Express Adoptions (per 1000 HH)	4.03	3.97

Note: Total treatment effect is calculated as a sum of the weekly marginal effects in each program. The column "Matching" refers to the specification using propensity score matching to select the control group. The column "Current" refers to the specification using current Solarize towns as a control group.

Table OA.5: Average Treatment Effects of Classic and Express without Control Towns

VARIABLE	(1)	(2)	(3)	(4)
classic	0.105**	0.103**		
	(0.045)	(0.045)		
classic first 8 weeks			0.069**	0.068**
			(0.030)	(0.031)
classic middle 8 weeks			0.154	0.152
			(0.095)	(0.097)
classic last 8 weeks			0.228	0.226
			(0.145)	(0.145)
express	0.199	0.199		
	(0.143)	(0.143)		
express first 8 weeks			0.086	0.085
			(0.078)	(0.079)
express last 8 weeks			0.462*	0.461*
			(0.262)	(0.262)
Total Classic Adoptions (per 1000 HH)	2.56	2.48	3.60	3.57
Total Express Adoptions (per 1000 HH)	3.18	3.18	4.38	4.37
Town FE	no	yes	no	yes
Year-Week FE	yes	yes	yes	yes
R-squared	0.254	0.259	0.266	0.271
N	2092	2092	2092	2092

Note: Dependent variable is the weekly number of signed solar contracts per 1000 solar suitable owner-occupied homes. Unit of observation is town-week. TE refers to treatment effect. Robust standard errors clustered at the town level (16 clusters). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table OA.6: Average Treatment Effects of Classic and Express without Control Towns (No Time Fixed Effects)

VARIABLE	**(1)**	**(2)**	**(3)**	**(4)**
classic	0.148***	0.151***		
	(0.043)	(0.042)		
classic first 8 weeks			0.062***	0.064***
			(0.018)	(0.017)
classic middle 8 weeks			0.031*	0.034*
			(0.016)	(0.016)
classic last 8 weeks			0.363***	0.366***
			(0.115)	(0.114)
express	0.243*	0.238*		
	(0.134)	(0.133)		
express first 8 weeks			-0.008	-0.013
			(0.007)	(0.008)
express last 8 weeks			0.525*	0.520*
			(0.290)	(0.289)
Total Classic Adoptions (per 1000 HH)	3.56	3.62	3.65	3.71
Total Express Adoptions (per 1000 HH)	3.88	3.80	4.13	4.05
Town FE	no	yes	no	yes
Year-Week FE	no	no	no	no
R-squared	0.067	0.068	0.153	0.157
N	2092	2092	2092	2092

Note: Dependent variable is the weekly number of signed solar contracts per 1000 solar suitable owner-occupied homes. Unit of observation is town-week. TE refers to treatment effect. Robust standard errors clustered at the town level (16 clusters). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table OA.7: Average Treatment Effects of Classic and Express without Control Towns (Wild Bootstrap)

VARIABLE	(1)	(2)	(3)	(4)
classic	0.105**	0.103**		
	[0.014, 0.205]	[0.015, 0.203]		
classic first 8 weeks			0.069**	0.068**
			[0.012, 0.133]	[0.009, 0.133]
classic middle 8 weeks			0.154	0.152
			[-0.026, 0.377]	[-0.034, 0.381]
classic last 8 weeks			0.228	0.226
			[-0.075, 0.554]	[-0.077, 0.559]
express	0.199	0.199		
	[-0.050, 0.761]	[-0.050, 0.756]		
express first 8 weeks			0.086	0.085
			[-0.091, 0.266]	[-0.094, 0.263]
express last 8 weeks			0.462*	0.461*
			[-0.047, 1.369]	[-0.044, 1.364]
Town FE	no	yes	no	yes
Year-Week FE	yes	yes	yes	yes
R-squared	0.254	0.259	0.266	0.271
N	2092	2092	2092	2092

Note: Dependent variable is the weekly number of signed solar contracts per 1000 solar suitable owner-occupied homes. Unit of observation is town-week. TE refers to treatment effect. Robust standard errors clustered at the town level (16 clusters). 95% confidence intervals, reported in square brackets, obtained with wild cluster bootstrap (1000 draws). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Figure OA.2: Cumulative Number of Contracts with 2N Matching

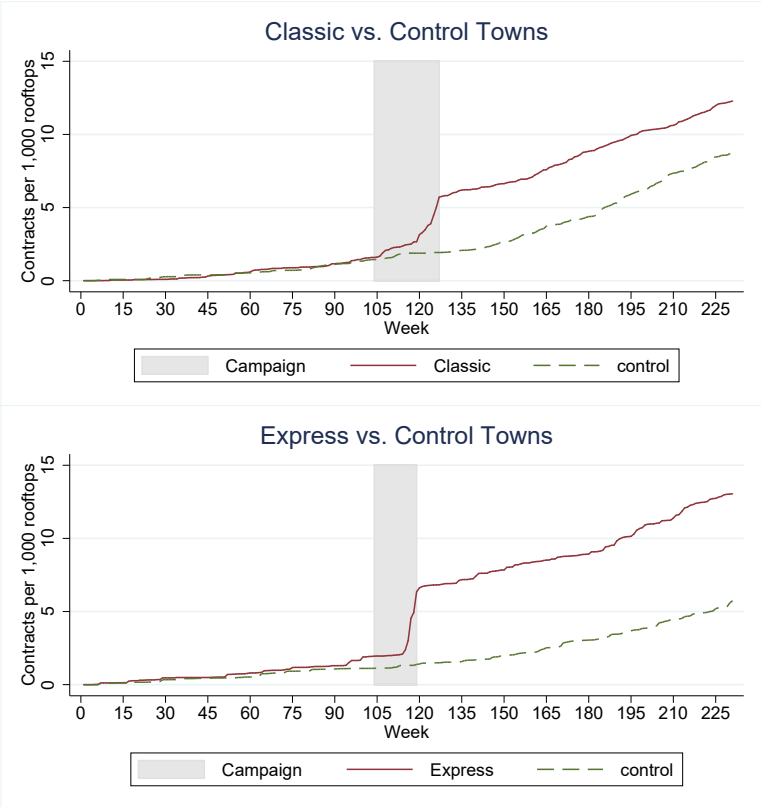


Figure OA.3: Weekly Treatment Effects with 2N Matching

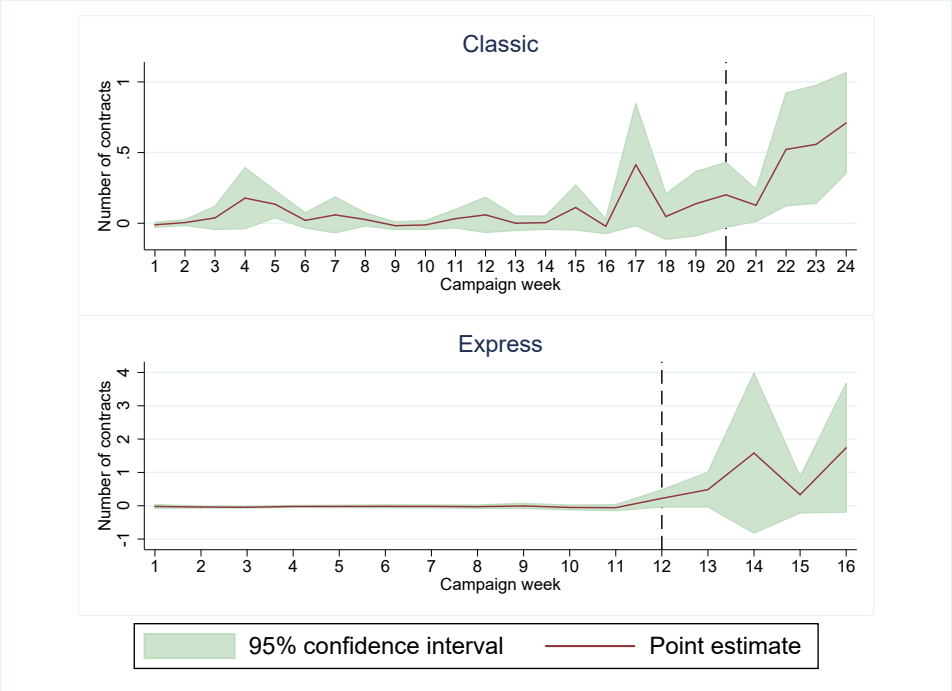
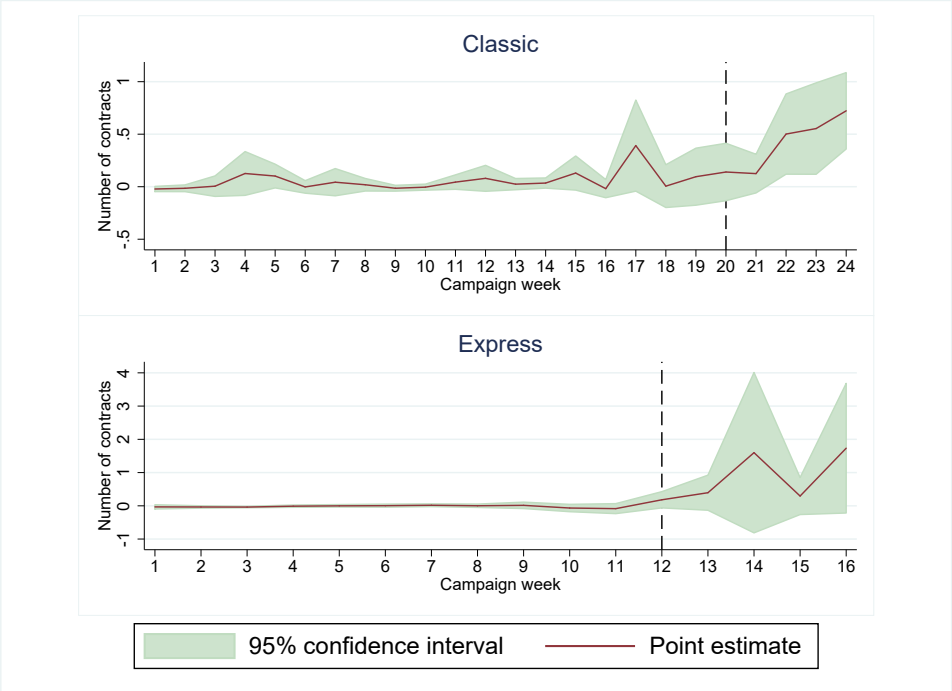


Figure OA.4: Weekly Treatment Effects with Current Solarize Control Towns



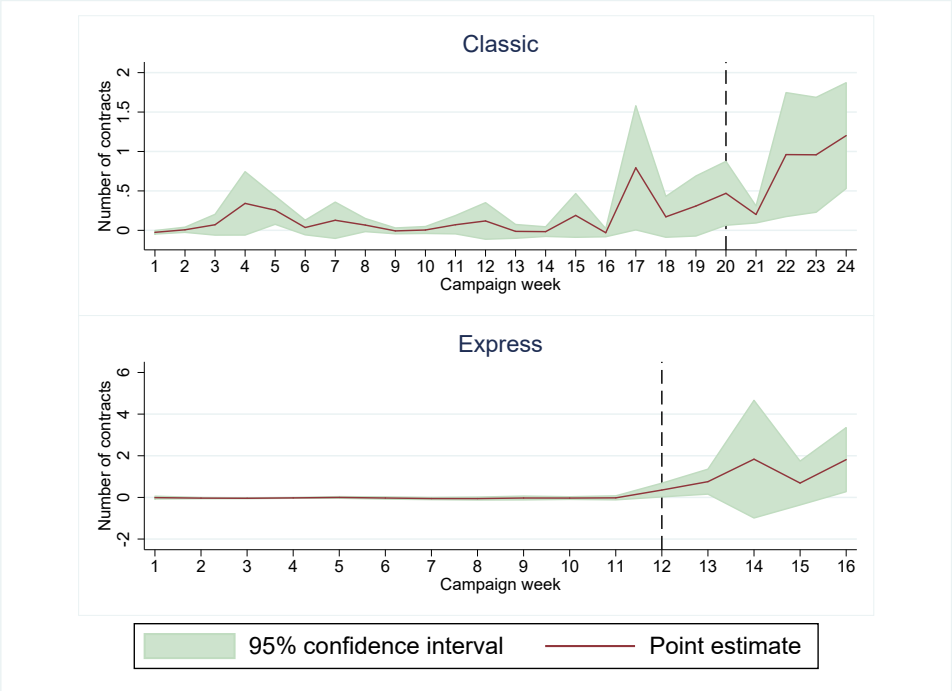
During Campaign Analysis using Different Outcome Variable

Table OA.8: Average Treatment Effects of Classic and Express (Market: All Owner-Occupied Homes)

VARIABLE	(1)	(2)	(3)	(4)
classic	0.261***	0.254***		
	(0.078)	(0.074)		
classic first 8 weeks			0.106***	0.100***
			(0.035)	(0.030)
classic middle 8 weeks			0.060*	0.052*
			(0.031)	(0.030)
classic last 8 weeks			0.635***	0.628***
			(0.205)	(0.201)
express	0.317**	0.304*		
	(0.153)	(0.152)		
express first 8 weeks			-0.015	-0.028**
			(0.011)	(0.012)
express last 8 weeks			0.702**	0.689**
			(0.327)	(0.326)
Total Classic Adoptions (per 1000 HH)	6.26	6.10	6.40	6.25
Total Express Adoptions (per 1000 HH)	5.08	4.86	5.49	5.29
Town FE	no	yes	no	yes
Year-Week FE	yes	yes	yes	yes
R-squared	0.114	0.106	0.171	0.165
N	8716	8716	8716	8716

Note: Dependent variable is the weekly number of signed solar contracts per 1000 owner-occupied homes. Unit of observation is town-week. TE refers to treatment effect. Robust standard errors clustered at the town level (62 clusters). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Figure OA.5: Weekly Treatment Effects. Market: All Owner-Occupied Homes



Online Appendix B: Post-Campaign Robustness Checks

Post-Campaign Analysis allowing for Different Year Two Effect

Table OB.1: Installation Growth Post-Solarize

	(1)	(2)	(3)	(4)	(5)
Campaign installs	0.048*	0.048*	0.112***	0.070***	0.084***
	(0.026)	(0.026)	(0.029)	(0.016)	(0.026)
Express	−0.329**	−0.439**	−0.353	−0.417	0.000
	(0.126)	(0.153)	(0.340)	(0.246)	(0.278)
Post=2	0.249	0.249	0.438**	0.344	0.139
	(0.270)	(0.270)	(0.179)	(0.274)	(0.334)
Express × post=2	0.084	0.084	−0.016	0.057	0.256
	(0.153)	(0.153)	(0.090)	(0.100)	(0.171)
# Active installer	0.256**	0.258**	0.272*	0.233**	0.205
	(0.118)	(0.116)	(0.134)	(0.092)	(0.118)
Workshop		−0.033	−0.025	−0.105*	−0.019
		(0.038)	(0.046)	(0.050)	(0.033)
Price per W			−0.070	−0.076	−0.063
			(0.075)	(0.071)	(0.068)
Sales leads				0.002***	
				(0.001)	
Sales leads × post=2				0.001	
				(0.001)	
Word-of-Mouth					2.821**
					(1.141)
Word-of-Mouth × post=2					2.195
					(1.546)
Constant	−0.071	0.143	−0.086	0.483	−0.239
	(0.427)	(0.394)	(0.404)	(0.426)	(0.258)
Observations	384	384	276	276	276
R^2	0.211	0.214	0.538	0.601	0.578
Month FE	Y	Y	Y	Y	Y

Note: Dependent variable: solar installations normalized by potential market size (solar suitable households - cumulative installations) [x1000]. Solarize related variables: campaign installs, defined as total number of installations during Solarize normalized by potential solar market [x1000], Express: categorial variable for shorter version of Solarize, Post: categorial variable indicating the period 13 to 24 months after campagin conclusion, number of Workshop and Sales leads (normalized by potential solar market) collected during Solarize, Word-of-Mouth, defined as the average share of adopters that heard about solar from a friend or neighbor. Unit of observation is town-month. Main sample: 16 Solarize Classic and Express campaigns observed for 24 month after the conclusion of Solarize. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table OB.2: WOM effects

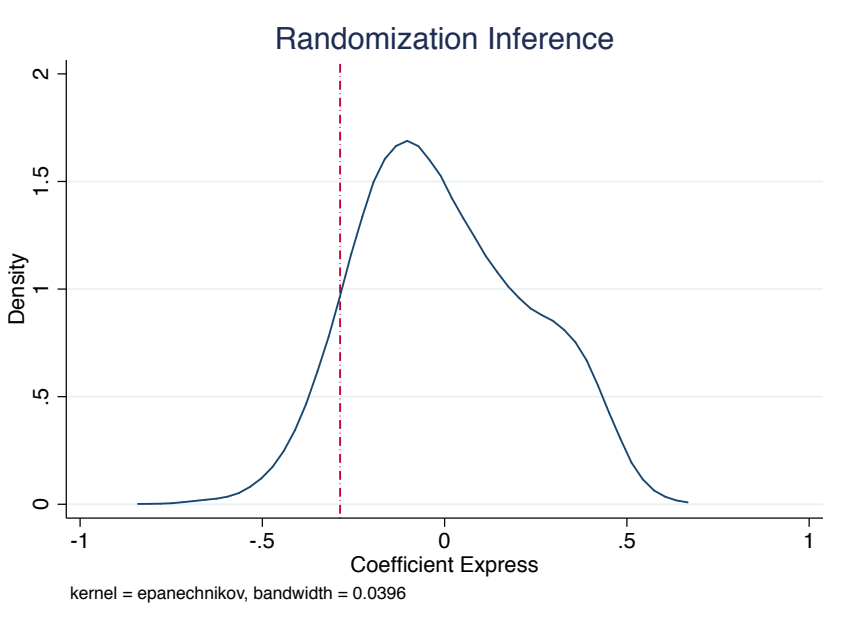
	OLS (1)	IV (2)
Campaign installs	0.087***	0.089***
	(0.021)	(0.019)
# Active installer	0.314**	0.305***
	(0.106)	(0.101)
Price per W	−0.034	−0.037
	(0.073)	(0.067)
Post=2	0.370	0.535**
	(0.331)	(0.216)
Word-of-Mouth	3.056**	3.055***
	(1.078)	(1.101)
Word-of-Mouth × post=2	1.244	0.373
	(1.140)	(0.880)
Constant	−0.632**	−0.711***
	(0.227)	(0.235)
Observations	221	221
R^2	0.608	0.606
# Excluded Instruments	-	5
F statistic (WOM)	-	83.33
F statistic (WOM× post=2)	-	49.36
Month FE	Y	Y

Note: Dependent variable: solar installations normalized by potential market size (solar suitable households - cumulative installations) [x1000]. Campaign installs, defined as total number of installations during Solarize normalized by potential solar market [x1000], Word-of-Mouth (WOM), defined as the average share of adopters that heard about solar from a friend or neighbor. Unit of observation is town-month. Main sample: 13 Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Column 2 instruments for WOM with "solar ambassador home suitable for solar" × quarter of campaign start and the experimental variation from Express interacted with the post dummy. First stage F-statistic obtained after partialling-out the month fixed-effects. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Post-Campaign Analysis: Small Sample

Randomization Inference Analysis

Figure OB.1: Express vs. Classic



Note: Randomization inference (RI) for the main effect of Solarize Express. The figure plots the kernel density for estimates that are obtained by randomly assigning the Express treatment (5 towns) within the total set of 16 towns. We perform all possible combinations to calculate the exact p-values for a one-sided test. The vertical line indicates the true treatment effect for Express as reported in column 1 of Table 5. We find that 448 out of 4,368 possible combinations produce an estimate that is smaller than -0.287. The implied p-value for a two-tailed test is 0.10.

Wild Cluster Bootstrap Inference

Table OB.3: Installation Growth Post-Solarize.

	(1)	(2)	(3)	(4)	(5)
Campaign installs	0.048***	0.048***	0.112**	0.069**	0.084**
	[0.005, 0.13]	[0.004, 0.13]	[0.026, 0.193]	[0.016, 0.119]	[0.024, 0.187]
Express	−0.287	−0.397*	−0.362	−0.392	0.119
	[-0.723, 0.055]	[-1.081, 0.039]	[-1.616, 0.505]	[-1.184, 0.41]	[-1.098, 0.994]
# Active installer	0.256*	0.258*	0.272	0.235	0.207
	[-0.062, 0.628]	[-0.064, 0.649]	[-0.109, 0.709]	[-0.09, 0.515]	[-0.077, 0.626]
Workshop		−0.033	−0.025	−0.105	−0.021
		[-0.164, 0.084]	[-0.21, 0.127]	[-0.319, 0.108]	[-0.168, 0.119]
Price per W			−0.070	−0.067	−0.051
			[-0.296, 0.084]	[-0.273, 0.093]	[-0.262, 0.089]
Sales leads				0.002	
				[-0.001, 0.005]	
Word-of-Mouth					3.915**
					[0.958, 8.896]
Observations	384	384	276	276	276
R^2	0.211	0.214	0.538	0.598	0.573
Constant	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y

Note: Dependent variable: solar installations normalized by potential market size (solar suitable households - cumulative installations) [x1000]. Solarize related variables: campaign installs, defined as total number of installations during Solarize normalized by potential solar market [x1000], Express: categorial variable for shorter version of Solarize, number of workshop and sales leads (normalized by potential solar market) collected during Solarize, and word-of-mouth, defined as the average share of adopters that heard about solar from a friend or neighbor. Unit of observation is town-month. Main sample: 16 Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Robust standard errors clustered at the town level. 95% confidence interval, reported in square brackets, obtained with wild cluster bootstrap (999 draws). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table OB.4: WOM effects

	OLS	IV
	(1)	(2)
Campaign installs	0.086***	0.089***
	[0.044, 0.19]	[0.046, 0.175]
# Active installer	0.317	0.306
	[-0.964, 1.176]	[-0.36, 1.058]
Price per W	−0.029	−0.035
	[-0.276, 0.117]	[-0.289, 0.101]
Word-of-Mouth	3.732*	3.264*
	[-0.124, 9.477]	[-0.248, 8.122]
Constant	Y	Y
Observations	221	221
R^2	0.605	0.604
# Excluded Instruments	-	4
First stage F statistic	-	93.84
Month FE	Y	Y

Note: Dependent variable: solar installations normalized by potential market size (solar suitable households - cumulative installations) [x1000]. Campaign installs, defined as total number of installations during Solarize normalized by potential solar market [x1000], Word-of-Mouth (WOM), defined as the average share of adopters that heard about solar from a friend or neighbor. Unit of observation is town-month. Main sample: 13 Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Column 2 instruments for WOM with "solar ambassador home suitable for solar" × quarter of campaign start and the experimental variation from Express. First stage F-statistic obtained after partialling-out the month fixed-effects. Robust standard errors clustered at the town level. 95% confidence interval, reported in square brackets, obtained with wild cluster bootstrap (999 draws). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Post Campaign Analysis with All Rounds of Classic Campaigns

Table OB.5: Installation Growth Post-Solarize. All Classic Campaigns.

	(1)	(2)	(3)	(4)	(5)
Campaign installs	0.019*	0.019*	0.038**	0.010	0.029**
	(0.010)	(0.010)	(0.015)	(0.009)	(0.011)
Express	−0.241	−0.340	−0.098	−0.154	0.264
	(0.168)	(0.237)	(0.337)	(0.239)	(0.221)
# Active installer	0.052	0.060	−0.074	−0.003	−0.033
	(0.087)	(0.084)	(0.098)	(0.104)	(0.074)
Workshop		−0.029	0.002	−0.101	−0.022
		(0.031)	(0.042)	(0.062)	(0.035)
Price per W			−0.125**	−0.133**	−0.083*
			(0.061)	(0.050)	(0.042)
Sales leads				0.003**	
				(0.001)	
Word-of-Mouth					4.204**
					(1.546)
Constant	0.191	0.330*	0.563	1.014**	0.020
	(0.145)	(0.182)	(0.375)	(0.423)	(0.278)
Observations	777	777	598	598	598
R^2	0.150	0.152	0.279	0.390	0.354
Month FE	Y	Y	Y	Y	Y
Solarize Round FE	Y	Y	Y	Y	Y

Note: Dependent variable: solar installations normalized by potential market size (solar suitable households - cumulative installations) [x1000]. Solarize related variables: campaign installs, defined as total number of installations during Solarize normalized by potential solar market [x1000], Express: categorial variable for shorter version of Solarize, number of workshop and sales leads (normalized by potential solar market) collected during Solarize, and word-of-mouth, defined as the average share of adopters that heard about solar from a friend or neighbor. Unit of observation is town-month. Main sample: 35 Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Post Campaign Analysis using All Owner-Occupied Homes

Table OB.6: Installation Growth Post-Solarize. All Owner-Occupied Homes

	(1)	(2)	(3)	(4)	(5)
Campaign installs	0.043*	0.043	0.104***	0.065***	0.074**
	(0.024)	(0.025)	(0.029)	(0.017)	(0.026)
Express	−0.164	−0.226	−0.177	−0.200	0.128
	(0.111)	(0.154)	(0.239)	(0.165)	(0.191)
# Active installer	0.148*	0.151*	0.149	0.137**	0.103
	(0.076)	(0.076)	(0.087)	(0.063)	(0.076)
Workshop		−0.019	−0.013	−0.058*	−0.007
		(0.025)	(0.027)	(0.031)	(0.020)
Price per W			−0.033	−0.030	−0.024
			(0.041)	(0.039)	(0.038)
Sales leads				0.002**	
				(0.001)	
Word-of-Mouth					2.234***
					(0.743)
Constant	−0.035	0.079	−0.047	0.194	−0.253
	(0.226)	(0.211)	(0.228)	(0.229)	(0.169)
Observations	384	384	276	276	276
R^2	0.183	0.186	0.495	0.561	0.533
Month FE	Y	Y	Y	Y	Y

Note: Dependent variable: solar installations normalized by number of households (2010) [x1000]. Solarize related variables: campaign installs, defined as total number of installations during Solarize normalized by number of households [x1000], Express: categorial variable for shorter version of Solarize, number of workshop and sales leads (normalized by number of households) collected during Solarize, and word-of-mouth, defined as the average share of adopters that heard about solar from a friend or neighbor. Unit of observation is town-month. Main sample: 16 Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table OB.7: WOM effects. All Owner-Occupied Homes

	OLS	IV
	(1)	(2)
Campaign installs	0.081***	0.084***
	(0.018)	(0.016)
# Active installer	0.178**	0.171***
	(0.061)	(0.059)
Price per W	−0.007	−0.011
	(0.041)	(0.035)
Word-of-Mouth	1.933**	1.670*
	(0.817)	(0.865)
Constant	−0.404**	−0.078
	(0.164)	(0.209)
Observations	221	221
R^2	0.567	0.566
# Excluded Instruments	-	4
First stage F statistic	-	209.7
Month FE	Y	Y

Note: Dependent variable: solar installations normalized by number of households (2010) [x1000]. Campaign installs, defined as total number of installations during Solarize normalized by number of households [x1000], Word-of-Mouth (WOM), defined as the average share of adopters that heard about solar from a friend or neighbor. Unit of observation is town-month. Main sample: 13 Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Column 2 instruments for WOM with "solar ambassador home suitable for solar" × quarter of campaign start and the experimental variation from Express. First stage F-statistic obtained after partialling-out the month fixed-effects. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Price as dependent variable

Table OB.8: Installation Prices Post-Solarize.

	(1)	(2)	(3)	(4)
Campaign installs	−0.008	−0.009	−0.008	−0.001
	(0.014)	(0.015)	(0.012)	(0.015)
Express	0.085	0.206*	0.207*	0.076
	(0.099)	(0.113)	(0.115)	(0.153)
# Active installer	−0.060	−0.068	−0.067	−0.050
	(0.069)	(0.070)	(0.065)	(0.068)
Workshop		0.033	0.035	0.031
		(0.022)	(0.028)	(0.022)
Sales leads			−0.000	
			(0.001)	
Word-of-Mouth				−1.049
				(0.895)
Constant	4.395***	4.206***	4.195***	4.280***
	(0.189)	(0.239)	(0.214)	(0.223)
Observations	276	276	276	276
R^2	0.184	0.190	0.190	0.194
Month FE	Y	Y	Y	Y

Note: Dependent variable: Price per Watt. Solarize related variables: campaign installs, defined as total number of installations during Solarize normalized by number of households [x1000], Express: categorial variable for shorter version of Solarize, number of workshop and sales leads (normalized by number of households) collected during Solarize, and WOM, defined as the average share of adopters that heard about solar from a friend or neighbor. Unit of observation is town-month. Main sample: 16 Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Online Appendix C: Additional Tables

Table OC.1: First stage for IV results

	(1)
# Active installer	0.008
	(0.012)
Price per W	−0.002
	(0.004)
Solar ambassador home suitable	0.077*
	(0.036)
Quarter Solarize=4	0.109***
	(0.012)
Solar ambassador home suitable × Quarter Solarize=4	−0.173***
	(0.036)
Express	−0.094***
	(0.022)
Campaign installs	0.006**
	(0.003)
Constant	0.026
	(0.033)
Observations	221
R^2	0.901
Month FE	Y

Note: First stage regressions for IV results presented in Table 6. Dependent variable: WOM. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Information on other Solarize Rounds

Table OC.2: Detailed Timeline of All Classic Campaigns

		Start Date	End Date
Round 2	Bridgeport	Mar 26, 2013	July 31, 2013
	Coventry	Mar 30, 2013	July 31, 2013
	Canton	Mar 19, 2013	July 31, 2013
	Mansfield	Mar 11, 2013	July 31, 2013
	Windham	Mar 11, 2013	July 31, 2013
Round 3	Easton	Sept 22, 2013	Feb 9, 2014
	Redding	Sept 22, 2013	Feb 9, 2014
	Trumbull	Sept 22, 2013	Feb 9, 2014
	Ashford	Sept 24, 2013	Feb 11, 2014
	Chaplin	Sept 24, 2013	Feb 11, 2014
	Hampton	Sept 24, 2013	Feb 11, 2014
	Pomfret	Sept 24, 2013	Feb 11, 2014
	Greenwich	Oct 2, 2013	Feb 18, 2014
	Newtown	Sept 24, 2013	Feb 28, 2014
	Manchester	Oct 3, 2013	Feb 28, 2014
	West Hartford	Sept 30, 2013	Feb 18, 2014
Round 4	Tolland	Apr 23, 2014	Sept 16, 2014
	Torrington	Apr 24, 2014	Sept 16, 2014
	Simsbury	Apr 29, 2014	Sept 23, 2014
	Bloomfield	May 6, 2014	Sept 30, 2014
	Farmington	May 14, 2014	Oct 7, 2014
	Haddam	May 15, 2014	Oct 7, 2014
	Killingworth	May 15, 2014	Oct 7, 2014
Round 5	Burlington	Nov 19, 2014	Apr 9, 2015
	East Granby	Dec 2, 2014	Apr 22, 2015
	Suffield	Dec 2, 2014	Apr 22, 2015
	Windsor	Dec 2, 2014	Apr 22, 2015
	Windsor Locks	Dec 2, 2014	Apr 22, 2015
	New Canaan	Dec 2, 2014	Apr 22, 2015
	New Hartford	Nov 17, 2014	Apr 7, 2015

Table OC.3: Solarize Installations & Sales Leads for All Rounds of Classic

VARIABLE	Mean	Sd	Median
Sales leads / households [$\times$1000]	83.397	86.176	52.262
Campaign installs / households [$\times$1000]	8.109	8.476	5.285
# Municipalities		39	

Note: Classic: All Solarize Classic towns participating in Solarize CT, pooled rounds 2012-2015. Campaign installs is defined as total number of installations during Solarize / households [x1000]. Sales leads is total number of leads collected during Solarize / households [x1000]. Unit of observation: town.

Table OC.4: Summary Statistics of Adopter Survey Responses: All Rounds of Classic

WOM Channels			
VARIABLE	Obs.	Mean	Std. Dev.
Friend/neighbor	640	0.145	0.353
Town leader	640	0.183	0.387
Solar customer	640	0.094	0.292
Newspaper	640	0.123	0.329
Social Media	640	0.034	0.182
Online media	640	0.091	0.287
Solarize event	640	0.322	0.468
Installer	640	0.042	0.201
Importance of Information Sources			
VARIABLE	Obs.	Mean	Std. Dev.
Friend/neighbor	597	3.568	2.165
Social media	590	2.047	1.477
Installer website	568	3.445	1.927
Solarize ambassador	596	4.025	2.03
Someone in town	592	3.623	2.139
Someone you work with	588	2.816	1.929
Print media	593	3.427	1.931
Seeing a solar installation	603	4.111	1.938

Note: Each response variable for "WOM Channels" is a binary variable, which equals 1 if the respondent learned about the Solarize program through the respective information channel and 0 otherwise. Each response variable for "Importance of Information Sources" is a categorical variable, which takes values between 1 and 7 depending on the perceived importance of the information source. The outcome values are defined as follows: 1 = not at all important, 2 = very unimportant, 3 = somewhat unimportant, 4 = neither important nor unimportant, 5 = somewhat important, 6 = very important, 7 = extremely important.

Table OC.5: Summary Statistics of Non-adopter Survey Responses: All Rounds of Classic

WOM Channels			
VARIABLE	Obs.	Mean	Std. Dev.
Friend/neighbor	949	0.129	0.335
Town leader	949	0.221	0.415
Solar customer	949	0.083	0.276
Newspaper	949	0.161	0.368
Social Media	949	0.056	0.23
Online media	949	0.102	0.303
Solarize event	949	0.218	0.413
Installer	949	0.061	0.24

Note: Each response variable is a binary variable, which equals 1 if the respondent learned about the Solarize program through the respective information channel and 0 otherwise.