Promotional Campaign Duration and Word-of-Mouth in Solar Panel Adoption

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Abstract

Intensive marketing campaigns can be used to increase awareness, consideration, purchase, and word-of-mouth (WOM) of pro-social products. With expanded interest and belief in how social norms and spillovers might be leveraged to combat climate change, it is critical to understand how campaigns designed to leverage such peer effects can be best designed. In this paper, we study the role of campaign duration in 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. We find that the longer campaigns generate more WOM and lead to more adoption post-campaign, despite a comparable number of installations during the campaigns. The shorter campaigns led to 22.6 fewer installations per town in the two years after the campaigns concluded, leading to a cost per acquisition of $2,029 versus $4,367 in the longer campaigns, the former being lower than installers' self-reported acquisition costs, and the latter being substantially higher.

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

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1 Introduction

Motivating risk-averse consumers to adopt new products and technologies has always been a challenge for firms; spurring adoption by mainstream customers after adoption by the early adopters has been referred to as "crossing the chasm" (Rogers, 1995). Resource-intensive promotional campaigns are often utilized in an attempt to achieve this for new product launches, seasonal goods, fundraising, blood drives, etc. Some such campaigns explicitly leverage word-of-mouth (WOM) to spur adoption (Godes and Mayzlin, 2009). Of vital interest to marketers is whether these campaigns are cost-effective. Given the potential long-term effects of WOM in product adoption, measuring campaign effects in both the short and long term is critical.

Selecting the right promotional campaign duration is a practical and important decision for firms, given the implications for ROI. In addition to the cost, there are potential demand-side effects of shortening campaigns on consumers' responses, including effects on consumers' feelings of time pressure and anticipatory regret (Simonson, 1992; Dhar and Nowlis, 1999), perceived value (Inman et al., 1997), and resolution of uncertainty (Surasvadi et al., 2016). The question of optimal campaign duration is additionally complicated if the campaigns are intended to leverage word-of-mouth (WOM).

Marketers have long discussed the benefit of leveraging social spillovers to increase the effectiveness of marketing interventions through the resulting social multiplier (Hartmann et al., 2008), and social interactions are increasingly seen as an important tool to help combat climate change (Frank, 2020; Winterich et al., 2023). We propose that shortening campaigns may lead to reduced levels of WOM, both during and after the campaigns, which will lead to a reduction in the cost-effectiveness of campaigns that are intended to not only lead to product adoption but to

seed WOM. In this paper, we experimentally manipulate the duration of the Solarize Connecticut campaigns for rooftop solar; one key factor of these campaigns is the degree to which they utilize social learning (Gillingham and Bollinger, 2021). The campaigns involve approximately 20 weeks of community outreach, and the primary outreach is performed by volunteer resident "solar ambassadors" who encourage their neighbors and other community members to adopt residential solar photovoltaics (PV).

This research project was funded by the Department of Energy (DOE) as part of the SEEDS[1] program (Solar Energy Evolution and Diffusion Studies) because the agency wanted to evaluate the effectiveness of these campaigns. Of particular interest to DOE was whether expedited versions of the Solarize program could allow such programs to scale more effectively. However, given that offline WOM takes time to operate, we would expect less of a role of WOM in shorter campaigns. And due to the longer carryover effects of WOM than for traditional marketing actions (Trusov et al., 2009), reduced WOM during the campaign may lead to fewer adoptions post-campaign. Because solar is a product that is cued by environmental contexts and is publicly visible, we expect consumers to share WOM both immediately after adoption and in the longer term (Berger and Schwartz, 2011). Thus, we hypothesized that the shorter campaigns would be less effective in leading to long-term solar adoption, greatly reducing the cost effectiveness of the shorter campaigns.

To test this, we ran a large-scale field experiment in which we randomly assigned communities to one of two types of campaigns: "Express" campaigns and "Classic" campaigns. Including a four-week grace period after the official end dates, Express campaigns were 16 weeks long, substantially shorter than the 24-week Classic campaigns. In Express, we increased the market-

[1] https://www.energy.gov/eere/solar/solar-energy-evolution-and-diffusion-studies-seeds

ing intensity accordingly to maintain the same total resources per campaign. Although the DOE (and any firm) would love to see the same number of adoptions with *fewer* resources, if we were to reduce the resources spent on the Express campaigns, long-term carryover effects of other campaign components might also be able to explain lower post-campaign adoption rates in Express. We ruled out this potential confounding factor in assessing the role of WOM by keeping the campaign resources at a similar level.

We found that the Express campaigns generated fewer Solarize leads (people signing up to express interest in solar) at events and workshops and WOM played far less of a role in consumer awareness of the campaigns. Yet the two types of campaigns used exactly the same outreach approaches and led to a similar number of installations during the campaigns themselves. The lack of WOM within the shorter Express campaigns appears to have led to detrimental long-term effects. The shorter campaigns led to 22.6 fewer installations per town in the two years after the campaigns concluded, a decrease that is explained by the lower levels of WOM. As a results, the cost per acquisition was $4,367 for Express vs. $2,029 for Classic, given the Express campaigns were not effectively leveraging social spillovers.

2 Theoretical Background

Social learning has been shown to effectively reduce consumer uncertainty, and thus the risk associated with the adoption of new products (Sorensen, 2006; Conley and Udry, 2010; Lee and Bell, 2013; Zhang and Godes, 2018), and solar specifically (Bollinger and Gillingham, 2012; Gillingham and Bollinger, 2021), leading to geographic clustering of solar installations (Graziano and Gillingham, 2014; Barton-Henry et al., 2021). Marketers use a variety of techniques to leverage

and increase social learning, including referral programs and appeals to descriptive social norms.

Word-of-mouth (WOM), which we consider to be direct communication between individuals, is an important channel through which peer effects can operate through social learning. However, in the absence of interventions facilitating WOM, it may be limited to a much smaller set of peers. Gillingham and Bollinger (2021) show that social learning is an integral part of the Classic Solarize CT campaigns; after the conclusion of the campaigns, they surveyed respondents to ask them about the factors that influenced their decision to install solar. The most important factors were "town information event", "friend or neighbor's recommendation", "recommendation of someone you interact with in your town", and "seeing solar on another home or business". We consider the second and third of these to be measures of WOM. Given this stated importance of WOM, it was unclear whether shortened interventions could effectively leverage social influence during the campaigns, and whether less use of WOM might have long-term implications.

In what follows, we describe the potential effects of shortening the campaigns (installations, WOM, etc.) on 1) the period of the campaigns and 2) the post-campaign period. We will refer to the number of individuals whose contact information we collected at campaign events as "direct leads". Indirect leads are also generated, people hearing and becoming interested in solar via WOM. To get an indicator for the relative number of indirect leads created via WOM, we ask solar adopters how they heard about the Solarize program, with two information sources categorized as WOM, as previously mentioned: friend/neighbor and another solar customer.

2.1 The Effect of Duration on Short-Run Adoption and WOM

The decision of whether to shorten a campaign while increasing the intensity of marketing communications relates to work in the advertising literature on the degree to which advertising

should be front-loaded for new product adoption (Nerlove and Arrow, 1962; Feichtinger et al., 1994). Muller (1983) first presents a dynamic model of new product diffusion that distinguishes between two different advertising objectives in such campaigns: increasing awareness and changing predisposition to buy. Advertising serves either role in that model, affecting awareness or purchase intent, but not both. The two roles are important in our context as well, since the former leads to the creation of more leads and the latter focuses on conversion. In the Solarize campaigns, some elements, such as workshops and town events, are most useful in the generation of leads, which occur at Solarize events. Other communication tools, such as newspaper articles and online and social media, may increase both awareness and increase propensity to install. We do not observe the number of leads generated though these secondary channels, only those at Solarize events.

Depending on the response curve, shortening the campaigns may increase (or decrease) the effectiveness of campaign interventions in leading to conversion. Previous behavioral research indicates that a shorter campaign 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. Time pressure decreasing choice deferral is also consistent with Simonson (1992), if time pressure increases the anticipatory regret of making the wrong choice. 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. Inman et al. (1997) finds that time limits on any corresponding price deals can signal value; this is especially true when overpayment concerns are active, such as in the case of durable goods with uncertain

payoffs (Srivastava and Lurie, 2004; Dutta, 2012). On the other hand, Simonson (1992) also 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 in our research setting, or the role of social influence.

Despite these potential reasons for increased adoption rates in shorter campaigns, longer campaigns allow more time for direct leads to share information via WOM (Punj and Staelin, 1983; Risselada et al., 2014). Many solar adopters are quite passionate about solar, and this emotional response is expected to lead to more offline WOM (Lovett et al., 2013), which can substantially affect adoption (Byers et al., 2012; Godes and Mayzlin, 2009). Figure 1 shows the potential moderating effects of campaign duration (with increased intensity) on 1) the creation of direct leads at events; 2) the creation and conversion of leads through non-event channels (newspaper articles, online information, etc.), and 3) the creation of additional indirect leads via WOM. While we cannot directly measure the indirect leads, we can measure whether solar adopters heard about Solarize via WOM and use this as a proxy measure. We test this model of moderated mediation and do indeed find all three effects.

2.2 The Effect of Duration on Long-Run Adoption

Due to the multiple offsetting effects on expected adoptions during the campaigns, the effect of shortening the duration of the campaign on the number of adoptions during the campaign is a priori unclear. However, even if the levels are similar (which we find), if the Express campaigns lead to fewer non-adopting direct leads at the conclusion of the campaigns and fewer indirect leads via WOM, then the Express communities will have different initial conditions in the post-campaign period.

The relevant question at hand in our setting is: what is the impact of shortening the campaigns on the rate of information diffusion after the campaigns conclude? The longer Solarize campaigns seed additional WOM, which then is likely to increase WOM among non-seeded potential adopters, as shown by Trusov et al. (2009), i.e. the indirect leads. In the case of solar, we might also expect the duration of the WOM to be longer than for a typical product. Berger and Schwartz (2011) shows that some product types only lead to "immediate WOM", which occurs right after a purchase, and others lead to "ongoing WOM", which continues to operate well after the product is bought. They find that products that are cued frequently, such as durable and visible solar panels, will receive more WOM and are more likely to have persistent WOM effects. Using non-experimental data, Bollinger et al. (2022) show that the visibility of peer installations leads to stronger social influence. The high degree to which solar is cued in the media, such as from news stories about the need to transition to renewable energy, in conjunction with its visible nature, plus the temporal lag between the adoption decision and when the installation is installed (approximately 3-6 months), lead us to expect long-lasting WOM effects.

Put together, we expect higher levels of WOM in the Classic campaigns and that the WOM in both campaigns will extend for a substantial period after the campaigns conclude. In terms of the implications for post-campaign solar adoption, agent-based models have shown that in a contagion model with recovering nodes (consumers stop affecting peers), higher contagion leads to more adoption and diffusion slows as nodes recover (Fibich, 2017). Given our expected effects of shortening the campaigns on the creation of indirect leads via WOM, we hypothesize that:

Hypothesis 1. *Shortening the duration of the campaigns will lead to lower long-run adoption rates.*

Hypothesis 2. *The lower long-run adoption rates after the shorter campaigns can be explained by*

the lower levels of WOM during the campaigns.

3 Field Experiment

Program Details The program examined in this study, Solarize CT, was (and is) a joint effort between the Connecticut Green Bank (CGB) and SmartPower. The campaigns studied in this paper were focused on residential rooftop photovoltaic solar systems. Five rounds of the program were run in 58 towns total between 2012 and 2015. During that time period, the number of homes with solar grew from about 800 to over 12,500. Gillingham and Bollinger (2021) demonstrated that the Classic campaigns played a major role in this expansion, although it was not clear what elements of the campaigns were most critical to their success. The campaigns are designed explicitly to leverage peer effects to foster the transfer of information about the benefits of solar technology through tabling at community events, using signage outside of adopters' homes, sharing information about cumulative adoption through the campaign, etc. (see Figure 2 for examples).

Treated municipalities choose a solar installer with whom to collaborate throughout the campaign after a formal request-for-proposals. Each installer submits a bid with a five-tier, group pricing discount offered to the entire community; the final price is based on the number of contracts signed. The intervention begins with a kick-off event and involves roughly 20 weeks of community outreach. The primary outreach is performed by the solar ambassadors. There is growing evidence of the effectiveness of promoters or ambassadors in driving social learning and influencing behavior (Nair et al., 2010; BenYishay and Mobarak, 2014; Ashraf et al., 2015; Kraft-Todd et al., 2018; Vasilaky and Leonard, 2018).

To study the role of promotional campaign duration on durable good adoption, we experimentally manipulated the duration of the campaign by randomly assigning communities to either an Express or Classic campaign, as shown in Figure 3. The Classic and Express campaigns were designed to be identical with the exception of the duration. Express campaigns were officially 12 weeks, versus 20 for Classic. Furthermore, in all campaigns, installers allowed for a grace period of several weeks after the campaign officially ends in which they will continue to offer promotional pricing. When the installers report the signed contracts to the CGB, they classify installations as Solarize and non-Solarize based on whether the contracts were obtained through the campaign. Nearly all of the installations in the four-week period after the official conclusion of the campaigns are classified as Solarize installations. We thus treat the Classic and Express campaign durations as 24 and 16 weeks, respectively, so as to more accurately capture the effects of the campaigns.

Our experiment consisted of eleven Classic towns and five Express towns. 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 high cost of each campaign (approximately $30,000, mostly in SmartPower labor costs) was the dominant factor limiting our experiment to 16 treatment towns. We do still pass our randomization checks, shown in Appendix A.

The timing of the campaigns is shown in Table 2. The start dates for the Express campaigns are later – this was done intentionally so that the campaign end dates are at approximately the same time. This is because we expect most solar contracts to be signed in the last weeks, and thus those are the most important weeks to be aligned; the Express campaigns also end zero-to-four weeks before the Classic campaigns, so the period over which Express campaigns are run is contained entirely within the period the Classic campaigns are run. The workshop events are

evenly spaced throughout the campaign periods, to the best of our abilities and given logistical considerations that must be accounted for with large-scale field interventions (Figure 4).

4 Data

4.1 Data Sources

The primary data source for this study is a database of all residential solar photovoltaic installations that received a rebate from the CGB between 2004 and 2016. Since the state rebate was substantial during the period of study, we are confident that nearly all, if not all, installations in CT are included in the database.[2] 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. Demographic and voter registration data are from the U.S. Census Bureau's 2009-2013 American Community Survey and the CT Secretary of State.

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 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 (homes for which the value of solar would be non-negative) at the zip-code level and multiply it by the number of owner-occupied homes. If the zip-code was

[2]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.

not covered by the Google Project Sunroof data, we used an alternative dataset using satellite-imaging to estimate the solar market potential at the county level (Geostellar, 2013).

Another dataset we use includes lists of all activities facilitated by SmartPower, the nonprofit marketing firm running 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. This also provided a total count of unique individuals who demonstrated an interest in solar at Solarize events. SmartPower also collected the lists of adopting leads from the installers themselves, to which we have access, allowing us to match adopters and leads.

To measure the role of WOM, we ran a survey of all solar adopters and leads generated from the campaigns in the several months after the campaigns concluded, following Punj and Staelin (1983); Newman and Staelin (1972), and Lovett and Staelin (2016). The survey was performed through the Qualtrics survey software and was sent to respondents via email, with an iPad raffled off as a reward for responding. In addition, we surveyed the non-adopting leads. The email addresses were acquired from Solarize event sign-up sheets and installer contract lists.[3] The key survey question is: *How did you hear about the Solarize program?*.

The two key WOM sources that survey respondents could chose were 1) friends and neighbors and 2) other solar customers. These are the two most relevant sets of peers responsible for WOM, according to SmartPower. We also create an aggregate measure of WOM to be used in the post-campaign analyses, in which we average the values of the fraction of solar adopters in the town who report hearing about the campaign from a friend or neighbor and those hearing about the

[3]Approximately six percent of the signed contracts did not have an email address. All others we contacted one month after the end of the round, with a follow-up to non-respondents one month later.

campaign from another solar customer.[4] The full survey instrument is shown in Online Appendix A. Table 1 shows the exact definition of the key variables of interest (leads, WOM, adopters).

4.2 Campaign Differences

Two questions that immediately come to mind are: 1) Did the campaign types differ in their implementation, other than their duration and intensity? And 2) were outcomes across the campaign types different? Despite the effort to keep the campaigns as similar as possible (except for their length), we did explore evidence of possible differences in effort by each of the primary players in the campaigns: solar ambassadors, installers, CGB, and SmartPower. There were no significant differences in prices, installer, or SmartPower activities (see Appendix B for details); the only exception to this that we could find was that there were fewer workshops in the Express towns due to the shorter timeframe, making it logistically impossible to plan as many workshops. The timing of these workshops were fairly evenly spaced for both types of campaigns (Figure 4).

In terms of campaign outcomes, we proposed that the Express campaigns (with the same total resources spent) would potentially lead to more direct leads at events, potentially have a larger direct effect on adoption from among households not included in these leads (recall we only capture leads at Solarize events), and create fewer indirect leads via WOM. As a result, we expect that solar adopters would be less likely to have heard about Solarize via WOM in the Express campaigns. In comparing the campaigns (Table 3), we found that the Express campaigns generated 41 unique leads per thousand solar-suitable homes in Express versus 62 in Classic,

[4] It is possible for a respondent to check both. If a respondent checked both sources, we would want to include both if they were referring to different people, since this represents two sources of WOM. It is possible the respondent checks both when referring to the same person. Only 11% checked both. We also created an alternative variable indicating if the individual heard from a friend or a solar customer. This measure is almost perfectly correlated with our current WOM variable at the campaign level (0.965).

although this difference is not statistically significant ($p = 0.13$). The ratio of the number of adoptions to the number of direct leads is similar, 18% in the Express campaigns and 16% in Classic.[5] This lends support to the first two postulated effects during the campaign of shortening the campaigns.

We also compare the means of the survey responses to assess whether Express led to less WOM. The role of WOM was substantially different. Summary statistics for how solar adopters learned about Solarize can be found in Table 4 for both the Classic and Express campaigns.[6] Solar adopters who responded to the survey who were part of the Express campaigns were much less likely to have heard about the program through friends and neighbors or other solar customers than in the Classic campaigns, supporting the hypothesis that shortened campaigns may limit WOM. We find significant differences in these two WOM variables when including the non-adopting leads as well.[7]

One benefit of asking solar 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. It should be noted that while the survey data provides a good proxy for WOM, it is not an absolute measure of WOM. Other less prominent (anecdotally) sources of WOM are also not directly measured, one potential limitation. As robustness checks, all analyses are conducted using the full set of survey respondents as well to construct the WOM measure,

[5] We do not refer to this as a conversion rate since the denominator only includes direct leads, not indirect leads.

[6] 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. Performing a t-test, we cannot reject the null hypothesis of equal response rates for the two types of campaigns. Thus, we interpret differences in the mean responses across treatment groups to be indicative of actual differences in the population-level means.

[7] The differences are 0.057 and 0.051, respectively, significant at 5%.

rather than limiting the measure to solar adopters; we focus on adopters for the main analyses because we think they are the most relevant population.

To further test whether Express leads to the effects shown in the conceptual model in Figure 1, we turn to methods used in the consumer behavior literature to provide mechanistic support in experimental settings. We use a two-stage moderated mediation process model (Model 85, Hayes (2017)) in which a Solarize campaign leads to direct leads, which lead to the creation of indirect leads via WOM, and both these direct and indirect leads then may adopt solar. We allow the indicator for the Express campaign type (shorter duration, higher intensity) to moderate the generation of direct leads and installations via the non-Solarize event channels. We include a direct effect of event leads on adoption and also allow Express to moderate the conversion of these direct leads to adopters (a non-postulated effect).

The results are shown in Table 5. As expected, Express led to fewer leads than Classic ($p < 0.1$). Leads from Solarize events have a direct effect on the number of Solarize participants who hear about Solarize via WOM, as well as a direct effect on installations. The effect of the number of leads on the number of installations is not significantly different across campaign types, i.e. the campaigns have similar conversion rates. There is a direct positive effect of Express on installations, but there is also a direct effect of WOM on installations, and since Express leads to lower WOM, there is also a negative indirect effect of Express on adoption (via WOM), which offsets the positive direct effect.

5 Adoption after the Campaign

Main results

The main goal of our study is to look at the effect of campaign duration on post-campaign adoption rates and the resulting cost-effectiveness of the campaigns. In Figure 5, we plot the average adoption rates across towns in each treatment for before, during, and after the campaigns conclude, aligning the timing of the campaigns. While we see that the number of adoptions during the Solarize campaigns is very similar across treatments, this plot highlights that in the raw data, the Classic towns show substantially higher post-campaign adoption rates. There are no statistically significant differences pre-campaign.

To examine how Express, the number of adoptions, the number of leads, and WOM during the campaign all influence post-campaign adoption, we use the following specification using monthly data:

$$y_{it} = \alpha E_i + \alpha^2 E_i 1\{t > 12\} + \boldsymbol{\beta X_{it}} + \gamma B_i + \xi_t + \epsilon_{it}, \tag{1}$$

in which y_{it} is the number of new installations in town i during month t divided by the potential market size, E_i is an indicator for an Express town, $1\{t > 12\}$ is an indicator for being more than one year post-campaign, B_i is the number of adopters during the campaign divided by the potential market size at the end of the campaign, and ξ_t is a monthly fixed effect. The four-week grace period for the campaigns ended in March 2014 for all campaigns, so the post-campaign period begins with April. The two α parameters capture the difference for Express towns relative to Classic in the post-campaign period. Gillingham and Bollinger (2021) provide evidence for no spillovers across town borders, and there is only one case where an Express town borders a Classic town, so spillovers are unlikely to be an important issue in our context.

In Table 6, we show the results from estimating equation (1). In our first specification, shown in column (1) of Table 6, we include a dummy variable for Express in addition to the number of installers (defined as the number of unique installers who performed at least one installation prior to the start of the campaign), and the installed base at the end of the campaign. We find a significant negative effect of the Express campaigns on post-campaign adoptions, with a slightly smaller effect in year 2 (the difference is not significant). The average point estimate for both years is -0.217 fewer installations per 1,000 suitable homes per month. In our second specification shown in column (2), we include the number of workshops in the campaign. 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.

In both the column (1) and (2) results, we find a small, positive effect of the number of installations during a campaign on installations after the campaign, as expected given the past work on diffusion models, such that one new installation per 1,000 suitable homes during the campaign leads to 0.042 installations per 1000 suitable homes per month post-campaign. To assess the total impact of Express on post-campaign adoption, we need to assess the effect on adoption during the campaign as well. We perform this analysis of the effect on adoption during the campaign in Online Appendix B. There is no statistically significant difference across the campaigns in total adoptions (3.43 adoptions per 1,000 suitable homes for Classic and 3.79 adoptions for Express, in the specification with town fixed effects). When estimating the difference in adoptions across campaign types without the control towns, we similarly find the difference to be an insignificant 0.3 adoptions per 1,000 suitable homes. Economically, this effect on post-campaign adoption rates

is minimal, 0.0014 new installations per 1,000 suitable homes[8], 0.008 installations per month for an average sized town.

In our Table 6 column (3) specification, we include the number of direct 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 1,000 potential adopting households. We also include price as an additional control variable that might predict differences across campaigns; its effect is presumably insignificant due to the limited variation in price across campaigns. The effect of leads is positive but not significant when using the clustered standard errors or the wild cluster bootstrap. To assess whether these leads are the primary adopters in the post-campaign period, we combine the data on the number of leads with the CGB database on all solar installations based on personal identifiers. 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.[9]

We find that during the campaign, two-thirds of solar adopters (1,135 out of 1,722 total adoptions for the pooled results) can be directly linked to the campaign; i.e., we can identify the adopters in the list of leads that have been collected during the campaign. In contrast, in the 12-month period following a campaign, this rate decreases to approximately 13% (158 / 1,207). In other words, approximately 87% of adopters in the first year post-campaign period are new customer acquisitions that did not participate in campaign events and thus were not considered 'leads' by the campaigns. Consistent with these findings, our survey results indicate that only

[8] $0.3 \times .048 = 0.0014$

[9] Non-matches are likely due to contract cancellations, cohabiting (different individuals responding to the survey and registering the solar system), and misreporting of installations.

28% of individuals who did not adopt during the campaign answer that there is a 'very good chance' that they will adopt solar in the future, and 27% reported either 'no chance' or 'very little chance' of future adoption.[10] Based on this descriptive evidence, 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 column (4), we include the amount of campaign WOM, as measured by the survey responses. This provides the first test for whether reduced WOM during the campaign explains the lower post-campaign adoption rates. To create our measure of WOM, we average the values of the fraction of solar adopters (who responded to the survey) 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 potentially useful proxy for WOM, it is not an absolute measure of WOM. 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.

The WOM coefficient is 4.670. To interpret the size of this effect, if we increase the fraction of solar adopters who hear about Solarize from a friend or neighbor or another solar customer versus from another source by 10% (increasing the WOM measure from an average of 0.124 across town-month observations to 0.136), the average adoption rate increases from 0.579 adoptions per month per 1,000 suitable households to 0.635, an increase of 9.7% (i.e. the WOM elasticity is 0.97).[11]

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

[11] The point estimate on Express becomes positive and significant when including WOM, but this is simply due to the fact we do not normalize the WOM variable: we would not expect a zero level of WOM, well outside the range

Addressing potential endogeneity of WOM

One potential concern is that WOM during the campaigns might be endogenous to shocks that affect adoption rates post-campaign and WOM during the campaign. While the presence of such shocks is unlikely, given that we observe all installer activities, this potential endogeneity concern motivates an additional instrumental variable analysis. As an instrument for WOM, 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. Suitability of an ambassadors' rooftop satisfies the exclusion restriction since it is pre-determined by the required panel orientation and shading of the housing lot, and we expect it to be relevant given the increased campaign success shown in Kraft-Todd et al. (2018).

In Table 7, we report the results of IV regressions for the set of observations for which we have the ambassador data in addition to the leads and WOM data (221 out of the 276 town-month observations). Column (1) shows the OLS results, which are similar to the column (4) results in Table 6 with this slightly smaller sample, as expected. In the next two columns, we instrument for WOM, using the ambassador's roof suitability and interacting it with a dummy variable for the quarter the campaign began. We include the interaction 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 instruments pass the test of over-identifying restrictions, and are highly relevant, with a large first-stage F-statistic.

As expected, there is a significant, negative effect of Express on WOM in the first stage regression shown in column (2) of Table 7. In column (3), we again show there is a positive and significant effect of campaign WOM on post-campaign adoption rates. The magnitude of the

of the data.

estimate is only slightly lower than that found in Table 6.

Additional robustness checks are in Online Appendix C for other variants of Table 6. In Table OC.2, we include all survey responses (not just those who adopted solar) in constructing the WOM measure. Moreover, in Table OC.3, we include town leaders in addition to friends or neighbors and solar customers in the construction of the WOM variable. In OC.7, we include all homes as the set of potential adopters, not just those deemed by Google Sunroof to have sufficient sunlight. All results are robust. We also run the same analyses without dividing by the market size for the dependent variable, number of campaign installations, and number of campaign leads. The WOM result is robust in all specifications.

6 Managerial Implications

Installers we have spoken with approximate that customer acquisition costs outside of Solarize to be in the range of $2,000 to $3,000. To evaluate the cost-effectiveness of acquisition via Solarize, it is essential to understand how the duration of such campaigns affects adoption decisions both during and after the campaigns. One benefit of intensive marketing campaigns can be continued WOM after the campaigns conclude, especially for categories in which WOM has been shown to affect adoption and for which we might expect ongoing WOM well after adoption, such as solar. 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 0.173 monthly installations per 1,000 solar suitable homes due to the shortening of the campaigns (using the column (1) results from Table 6). This translates to an average of 22.6 fewer installations for

Express than Classic.[12]

This has important ramifications for the calculated cost-effectiveness of the campaigns. The total variable costs to SmartPower per campaign are approximately $33,333 for Classic and $33,500 for Express. See Online Appendix D for an explanation for how we calculate these costs. We do not have installer time costs but these are small relative to the cost of the labor supplied by SmartPower. We proceed to convert these cost figures into a dollar-per-contract measure of the cost-effectiveness of each campaign. To estimate the total effect of each treatment, we match the treated towns with a set of control towns. The details are discussed in the Appendix.

The estimates in columns (2) in Table OB.2 in Online Appendix B suggest that the average campaign leads to approximately 18.7 additional installations for Classic and 20.1 additional installations for Express during the campaigns. This small difference and the cost figures in Online Appendix D suggest an average cost of approximately $1,783 per additional contract for Classic and $1,670 per contract for Express based only on adoptions during a campaign for an average market of 5,453 households and not accounting for the price discounts during the campaigns. Using the estimated price decline of $0.64 per watt due to Solarize campaigns we found in Gillingham and Bollinger (2021), with an average system size of 4.23 kW, the cost of the price discount is $2,700 per installation (assuming no additional cost declines due to Solarize), which is in the range of the customer acquisition costs installers pay on their own (implying pass through of the savings in customer acquisition). Hence the total cost of acquiring a customer through Solarize is over $4,000, and is similar for Classic and Express, *if we ignore post-campaign effects.*

But we cannot ignore the long-term. The Classic campaigns lead to an average of 22.6 more installations per town than Express campaigns in the 24 months after the campaigns concluded

[12] With an average solar market size of 5,453 households for the Express and Classic towns, we have that $(24 * 0.217 - 12 * 0.089) * 5,453/1,000 = 22.6$.

due to the increased WOM, a net difference of 21.2 total installations across campaign types in total, including both during and after the campaigns. When we also account for the post-campaign differences as well, the Classic campaigns are clearly much more cost-effective. Working with the assumption that the Express campaigns return to similar adoption rates as we would see in a control group, we calculate the cost per installation in Express to be $4,367, versus $2,029 in Classic.[13] The assumption that adoption rates return to the same level as in the control towns in the past-campaign period may overstate the post-campaign treatment effects, since Figure 5 shows the control towns' cumulative installations increasing at a slightly faster rate than the treated towns, indicating some of the post-campaign installations may have been from inter-temporal shifting. If this is the case, the costs per installation are underestimated since some installations would have occurred anyway in the two years after the campaigns concluded. The qualitative conclusions that acquisition costs for Classic are substantially less than what installers typically pay and that acquisition costs in Express are substantially more than what installers typically pay are not affected. In sum, while the Classic campaigns are very cost effective in acquiring new customers, the Express version of the campaign is not cost effective, relative to installers acquiring new customers themselves.

We tested Express with the same total resources in order to remove a potential confound of lowering resources spent. We would expect the post-campaign (and campaign) to be even lower with fewer resources. However, it should be noted that because money is more important than time pressure, the government could consider spending less on an Express campaign. This could increase the cost effectiveness of the Express campaigns to some degree, if a non-optimal level

[13] Express: ((20.1 * 2,700) + 35,000)/(20.1 + 0) = 4,367. Classic: ((18.7 * 2,700) + 33,300)/(18.7 + 22.6) = 2,029. Figure 5 shows post-adoption levels for Express that generally follow the pre-campaign trend); the 36.7 more installations in the Classic campaigns in the following two years are a lift in installations relative to baseline.

(too many) resources are being spent in the current version. However, the campaigns would suffer still due to the lack of WOM post-campaign; any increased efficiency would be due to lowering costs to acquire the installations that occur during the campaign, and there should be no expectation of long-term benefits from WOM. Given the length of these campaigns was not optimized, ideally, a firm would experiment with the campaign duration. The firm could account for any tradeoff in the effectiveness of resources spent during the campaign on the number of installations during the campaign and the amount of generated WOM, which will be predictive of post-campaign adoptions.

7 Concluding Remarks

This field experiment was run as part of a four-year study funded by the SEEDS program was intended to provide a substantive contribution. The Department of Energy and other state and local government agencies have spent 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 lower total resource usage, then acquisition costs could be reduced. And if shorter campaigns have similar or greater efficacy with the same total marketing resources per campaign, adoption of solar panels (which result in positive externalities through learning-by-doing and environmental benefits) could still be expedited.

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 a large-scale grassroots program promoting residential solar adoption by leveraging

WOM. Specifically, we compare the effectiveness and cost-effectiveness of a 24-week Classic program versus a 16-week Express program by randomly assigning towns to each of the programs. Past theory has predicted positive effects of shortening the campaign duration due to the signal value of the limited time availability of the deal and potential convex response to promotion, whereas other theory points to potential negative effects, since shortening the campaigns does not allow for as much resolution of price uncertainty before the campaigns conclude and limits the degrees to which WOM can be shared.

Our survey results verified that there was less WOM created in the Express campaigns than in the Classic campaigns. As expected, we also found that more WOM led to more solar installations during the campaign, leading to a negative indirect effect of Express on adoption. However, this was offset by a positive direct effect leading to statistically similar levels of solar adoption during the campaigns. The main difference across campaign types at their conclusion was the degree to which WOM played a role in solar adoption, i.e. the degree to which WOM was seeded.

The findings of Berger and Schwartz (2011) led us to believe that solar is a category in which we might expect ongoing WOM after adoption, and thus this difference would lead to fewer adoptions after the Express campaigns than after the Classic campaigns (H1). We found evidence in support of this. Furthermore, in both the OLS and IV regressions, we found that more WOM during the campaigns led to more adoptions post-campaign (H2). In other categories in which WOM is expected to play a prominent role over a long time horizon in product diffusion, our results suggest that longer campaigns should be used. Furthermore, the results suggest that post campaign surveys, despite the fact they capture a single snapshot, can be effectively utilized to assess the degree to which WOM is contributing to information sharing during the campaign, which is indicative of the degree to which there is seeded WOM that will contribute to future

product adoption. One limitation of our paper is that the WOM is measured at a single point in time. which limits assessment of post-campaign dynamics. If managers can capture WOM over time through repeated surveys, the duration of the long-run benefits can be better assessed.

The main limitation of this paper is clearly 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 communities must be randomly assigned as a unit, rather than randomization occurring at the household level. Such randomization is necessary here given the desired object of study, namely peer influence among those in the community who are not necessarily locally proximate. However, despite the limited number of treated units, we were able to conclude with enough certainty that Express campaigns lead to detrimental long-term effects to inform the decisions made by SmartPower and DOE regarding the cost-effectiveness of expedited campaigns. The Department of Energy instead focused on expanding geographic reach and facilitating low and moderate income adoption as part of a follow-up SEEDS 2 program, using the Classic campaign format (Bollinger et al., 2020b). All of the Solarize campaigns run by SmartPower across the United States since this study was run (in CT, PA, MA, AZ, RI, NH, etc.) use the longer campaign duration, as a direct result of our findings. The most recent Solarize campaigns run by SmartPower in India are also 20 weeks long (again allowing for a grace period afterwards).[14] Finally, a Solarize Guidebook summarizing these findings (Gillingham and Bollinger, 2017) has led other organizations running Solarize campaigns to also use longer-duration campaigns, including in NC, AK, and other geographies.

Although we believe that the most likely explanation for differences in post-campaign adoption rates is that solar adopters who hear about the program via WOM are then more likely to

[14] https://www.smartpower.org/lets-solarize-new-delhi/

share WOM, we cannot fully rule out other alternative explanations. For example, it could be that differences in the price expectations during the campaign, due to the group pricing, 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 types 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 can still be influenced by the differences in WOM, and thus the conclusions regarding the lack of cost-effectiveness of the Express campaigns and the role of WOM are unchanged.

We also recognize that there may be equally important channels in the diffusion of other durable goods. Visibility of adoption has been shown to pay a role for in-flight drink purchases on airplanes (Gardete, 2015), dry landscape conversions (Bollinger et al., 2020a), and in the diffusion of solar (Bollinger et al., 2022). The fact we still see evidence that WOM affects future adoption implies that WOM might play an even larger role in other durable product categories, in which product adoption is less visible.

One interesting avenue of future study is an examination of the conditions under which our results no longer hold; a limitation of this paper is our inability to test these directly. Instead, we lean upon the findings of Berger and Schwartz (2011) in developing our hypotheses. Specifically, solar is cued frequently, with many news stories about the need for investment in renewable energy, and solar adoption is often visible. Both factors make it more likely that solar would exhibit ongoing WOM post-adoption. In categories in which this does not apply, we might expect to see no long-term detrimental effect of shortening campaigns (Berger and Schwartz, 2011). Products

with short-term relevance may be good candidates for expedited promotional campaigns, even when these campaigns do try to leverage WOM; for frequently cued durables (e.g. automobiles, appliances), we would expect our results to generalize. The results are especially relevant for green technologies, many of which are durable, are frequently cued, and may have less certain product benefits due to consumer inexperience with them.

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Tables

Table 1: Key Variable Definitions

VARIABLE	Definition	How collected
Leads	The list of contacts (name and contact information) recorded at campaign events	At the events
Campaign installations	The number of installations that occur in the campaign period (including the four-week grace period)	Continuously from CT Green Bank
Active installers	The number of unique installers active in each town in 24-month window prior to the start of Solarize	Continuously from CT Green Bank
Post campaign installations	The number of installations that occur after the campaign grace period ends	Continuously from CT Green Bank
WOM	The average of the fraction of solar adopter survey respondents in town who report hearing about Solarize from friends/neighbors and from another solar customer	Survey conducted 1-3 months post-campaign
Solar-suitable homes	The share of houses for which the value of solar would be non-negative at the zip-code level multiplied by the number of owner occupied homes	From 2010 Census data

Table 2: 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 3: Solarize Installations & Leads

	Classic		Express		
VARIABLE	Mean	Sd	Mean	Sd	T-test
Leads / solar suitable homes [×1000]	62.02	25.40	40.93	20.65	0.13
Campaign installs / solar suitable homes [×1000]	10.51	7.80	7.37	5.92	0.44
Campaign installs / leads	0.159	0.064	0.178	0.08	0.62
# 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]. 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 4: 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

Note: Each survey 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. 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: Mediation Analysis

VARIABLE	Leads	Word-of-mouth	Installations
Express	-0.021	0.028	0.343
	(0.012)*	(0.069)	(0.126)***
Leads		2.236	9.710
		(0.716)***	(1.708)***
Express X Leads		-2.968	2.124
		(0.978)***	(3.416)
Word-of-Mouth			2.827
			(0.672)***

Note: Conducted with structural equation modeling. Standard error clustered at town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 6: Installation Growth Post-Solarize

VARIABLE	**(1)**	**(2)**	**(3)**	**(4)**
Express	-0.217	-0.222	-0.061	0.420
	(0.098)**	(0.155)	(0.258)	(0.110)***
	[-0.462, 0.029]*	[-0.63, 0.2]	[-0.903, 0.885]	[0.13, 0.732]**
Express × year 2	0.089	0.089	-0.061	-0.037
	(0.141)	(0.139)	(0.115)	(0.104)
	[-0.242, 0.411]	[-0.243, 0.413]	[-0.288, 0.16]	[-0.288, 0.214]
Campaign installs	0.042	0.042	0.073	0.045
	(0.021)*	(0.020)*	(0.023)***	(0.019)**
	[-0.019, 0.131]	[-0.018, 0.126]	[0, 0.174]*	[-0.017, 0.119]
# Active installer	0.045	0.045	0.02	0.017
	(0.018)**	(0.018)**	(0.021)	(0.021)
	[0.001, 0.103]**	[0.001, 0.1]**	[-0.039, 0.083]	[-0.068, 0.082]
Workshop		-0.002	-0.035	-0.033
		(0.026)	(0.053)	(0.037)
		[-0.086, 0.087]	[-0.262, 0.122]	[-0.156, 0.088]
Price per W			-0.085	-0.057
			(0.089)	(0.080)
			[-0.364, 0.126]	[-0.342, 0.102]
Leads			0.008	0.007
			(0.007)	(0.007)
			[-0.013, 0.029]	[-0.017, 0.037]
Word-of-Mouth				4.670
				(1.588)**
				[-0.158, 10.311]*
Year-Month FE	yes	yes	yes	yes
Constant	yes	yes	yes	yes
R-squared	0.193	0.193	0.497	0.549
N	384	384	276	276

Note: Dependent variable is the monthly number of solar installations normalized by the 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 leads (normalized by potential solar market) collected during Solarize, and word-of-mouth, defined as the average share of adopters that heard about Solarize from friends/neighbors or from another solar customer. Unit of observation is town-month. Main sample: Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Two-way clustered standard errors by town (16 clusters) and month (24 clusters) in parentheses. Year-Month FE is absorbed. Wild cluster bootstrap (Cameron et al., 2008) 95% confidence intervals, reported in square brackets (1000 draws). Results of our hypothesis tests are identical using both methods. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 7: WOM Effects

VARIABLE	OLS Solar inst. **(1)**	1st stage WOM **(2)**	IV Solar inst. **(3)**
Express	0.466	-0.064	0.390
	(0.152)***	(0.015)***	(0.107)***
	[-0.023, 0.98]*		[-0.102, 0.906]*
Campaign installs	0.046	0.001	0.051
	(0.022)*	(0.002)	(0.021)**
	[-0.048, 0.157]		[-0.028, 0.138]
# Active installer	0.019	-0.005	0.020
	(0.020)	(0.003)*	(0.018)
	[-0.095, 0.078]		[-0.043, 0.081]
Price per W	-0.056	-0.001	-0.061
	(0.092)	(0.002)	(0.090)
	[-0.452, 0.134]		[-0.373, 0.193]
Leads	0.007	0.001	0.007
	(0.007)	(0.001)	(0.006)
	[-0.022, 0.032]		[-0.015, 0.03]
Word-of-Mouth	4.686		3.957
	(1.642)**		(1.208)***
	[0.388, 10.073]**		[0.279, 8.468]**
Solar amb. home suitable		0.116	
		(0.024)***	
Quarter Solarize=4		0.134	
		(0.022)***	
Solar amb. home × Qtr. Solarize=4		-0.234	
		(0.022)***	
Year-Month FE	yes	yes	yes
Constant	yes	yes	yes
R-squared	0.568	0.947	0.522
N	221	221	221
First stage F statistic			44.35
Hansen J-statistic (p-value)			0.20

Dependent variable is the monthly number of solar installations normalized by the 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 Solarize from friends/neighbors or from another solar customer. Unit of observation is town-month. Main sample: Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Column (2) presents the first stage results for the IV results presented in Column (3). The excluded instruments for WOM are "solar ambassador home suitable for solar" × quarter of campaign start. Year-Month FE is absorbed (partialled-out in the IV regresssions). Two-way clustered standard errors by town (16 clusters) and month (24 clusters) in parentheses. Wild cluster bootstrap (Cameron et al., 2008) 95% confidence intervals, reported in square brackets (1000 draws). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Figures

Figure 1: Proposed Effects of WOM

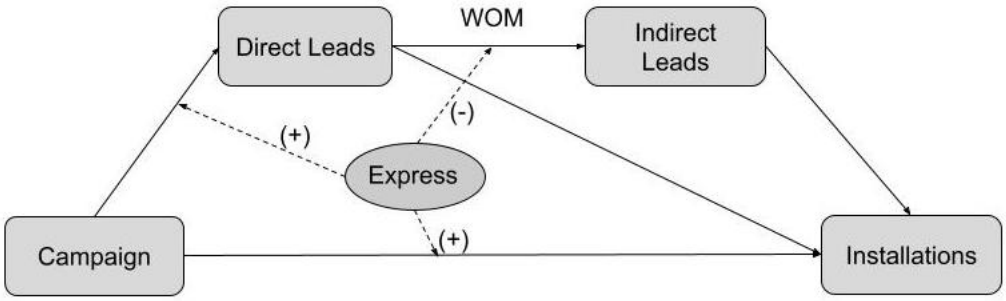


Figure 2: Examples of Solarize Presence in the Community

Figure 3: Location of Field Experiment

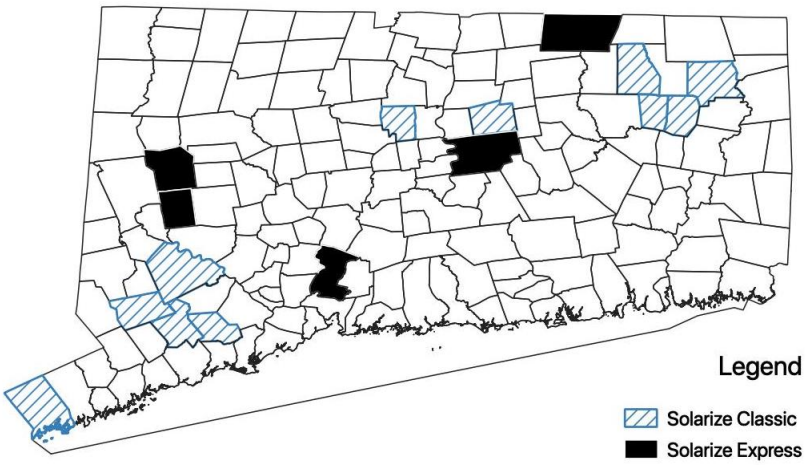
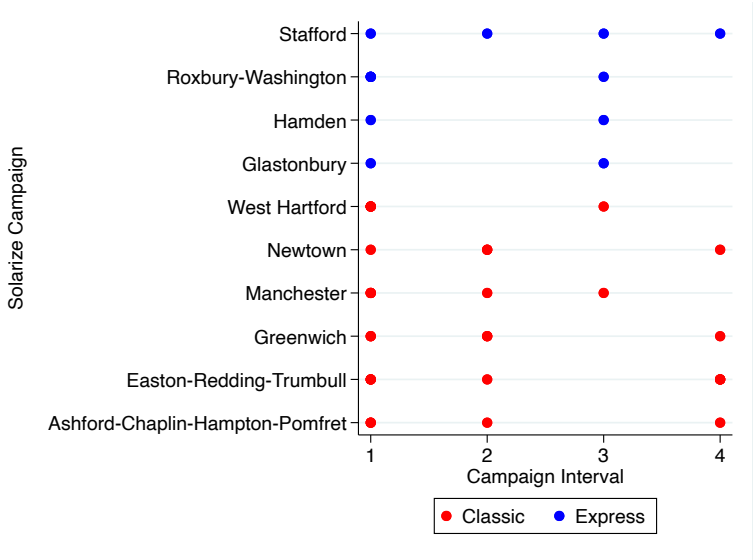
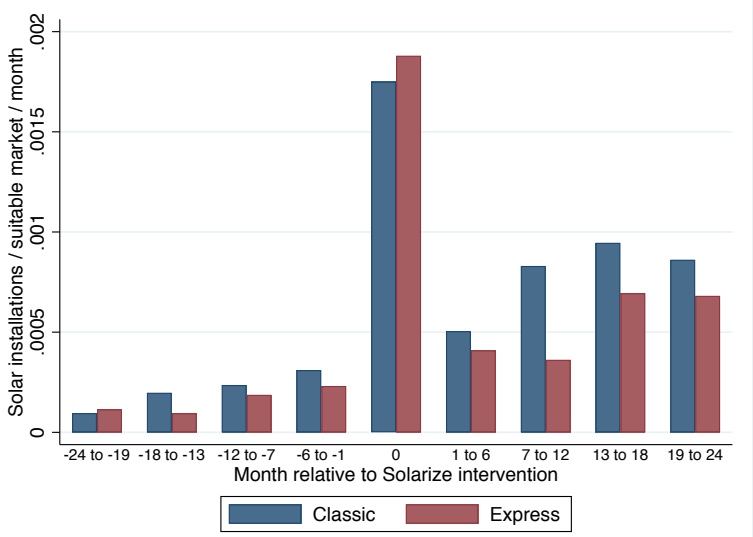


Figure 4: Workshops per Campaign Quartile



Note: Figure shows the spacing of events in the different campaigns.

Figure 5: Mean Solar Growth, Express vs. Classic



Note: Figure indicates mean solar growth, defined as number of solar installations divided by suitable solar market, measured in units of 1000 owner-occupied homes which would get positive value from installing solar. 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: Randomization Checks

Table A.1: Balance of Covariates

	Classic		Express		p-val
VARIABLE	Mean	St. Dev.	Mean	St. Dev	
Population density	820.7	955.5	578.3	746.5	0.63
Median income	97714	27868	81568	17717	0.26
% White	0.905	0.083	0.887	0.113	0.73
% College degree	0.480	0.041	0.493	0.050	0.57
% Unemployed	0.083	0.022	0.070	0.022	0.30
% Democrat voters	0.309	0.070	0.330	0.090	0.61
#Occupied units	9394	9866	8622	9375	0.89
% Solar suitable homes	57.60	4.92	66.88	17.64	0.12
# Solar suitable homes	5524	5848	4720	4534	0.79
Number of towns	11		5		

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. p-values in last column are from a t-test comparing the mean of each variable across Classic and Express towns.

Appendix B: Examining Campaign Differences

To test differences in campaign implementation across treatments, we conducted in-person interviews with ambassadors in each town upon conclusion of the campaign. We collected information regarding campaign organization, effectiveness of strategies, the extent of town vs. volunteer involvement, 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 difference 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 workshops. This may not be surprising because the Classic campaigns were longer, and thus there were more weeks to organize workshops. Importantly, 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 campaigns. 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.[15] As shown in Table B.1, there were no statistically significant differences across treatments.

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 of these representatives emphasized the importance 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. Three out of the four Express installers were also operating in Classic towns. They all confirmed that there were no differences in their strategies based on type of campaign.

We finally compared the realized solar system prices across Classic and Express towns (as discussed, all towns were offered the lowest tier price level by the end of the campaign). We examine the post-incentive price for each solar contract signed in the 16 municipalities during

[15]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.

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 ran a specification of equation (1) using price as the dependent variable, showing no significant difference in the effect of price across campaign types (Table B.2).

Even if there had been any differences in these campaign characteristics, it is not clear how they would have led to a different impact across campaign types after the campaigns concluded. Differences in post-campaign prices that might have resulted from the difference in campaigns is one potential reason for different adoption rates, although it is unlikely given prices during the campaigns were the same. To test this, we estimate equation (1) with the monthly post-campaign data, replacing the dependent variable with price (and omitting it as a regressor). The results in Table OC.9 in the Online Appendix show no differences in post-campaign prices.

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: Data are collected from survey of 30 solar ambassadors. 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**
	(0.116)	(0.247)
Express	-0.441**	-0.446**
	(0.220)	(0.218)
F-test	0.42	0.05
	(0.52)	(0.82)
Town FE	no	yes
Year-Week FE	yes	yes
R-squared	0.262	0.419
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. Two-way clustered standard errors by town (62 clusters) and week (120 clusters) in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Online Appendix A: Survey Instrument

Included as an attached document.

Online Appendix B: During Campaign Treatment Effects

Estimating During-Campaign Treatment Effects Using a Control Group

In order to get estimates of the treatment effects for the campaigns, in this subsection we also include a control group in the analysis. 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).[16] Summary statistics for key demographic and socioeconomic variables in these final datasets are also in Table OB.1 and indicate a good balance in observables across the treatment and control group.

Table OB.1: Balance of Covariates

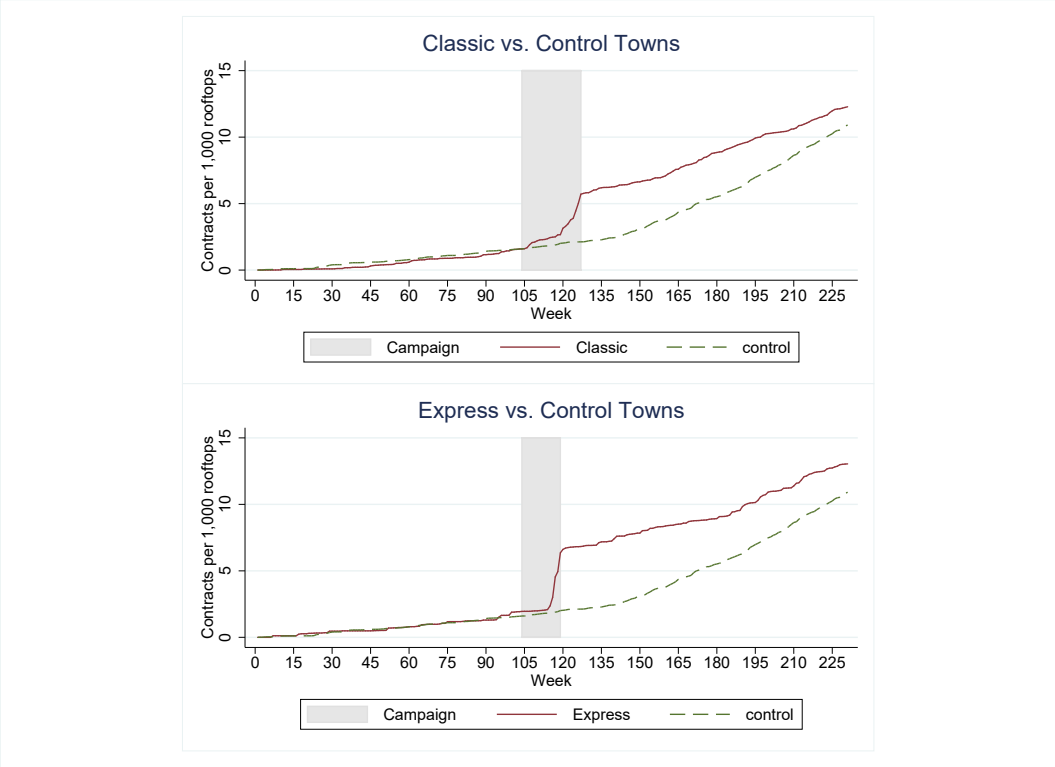
	Classic		Express		Control	
VARIABLE	Mean	St. Dev.	Mean	St. Dev	Mean	St. Dev
Population density	820.7	955.5	578.3	746.5	781.4	878.0
Median income	97714**	27868	81568	17717	80245	24076
% White	0.905	0.083	0.887	0.113	0.908	0.096
% College degree	0.480	0.041	0.493	0.050	0.483	0.043
% Unemployed	0.083	0.022	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. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

[16] 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.

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 OB.1 displays the cumulative solar 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.

Figure OB.1: Cumulative Number of Contracts in 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.

A number of descriptive findings emerge from these simple trends. First, there is a clear overall increase in solar adoptions during the campaigns for both Classic and Express. The sharp increase of adoptions during the Classic campaigns is consistent with the findings in Gillingham

and Bollinger (2021), but it is the comparison between the two types of camapigns we are focused on here. Second, this increase in adoptions 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 low levels of adoption for most of the campaign, with an acceleration of the upward trend in adoption at the end of the campaign, consistent with WOM and peer recommendations. For the Express campaigns, there are virtually no installations for the first eleven weeks, just like with Classic, likely due to the time it takes WOM to operate. This also might be partly due to potential adopters in Express having a greater preference for waiting until the end of the campaign to see the final price.

Treatment Effects by Campaign Duration To estimate the treatment effects, we 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. Let y_{it} denote the number of solar contracts signed in town i during week t per 1,000 owner-occupied houses in the town that are suitable for solar.[17] We define C_{it} and E_{it} as dummy variables, indicating the time during a Classic or Express campaign, respectively.

$$y_{it} = \alpha^c C_{it} + \alpha^e E_{it} + \mu_i + \delta_t + \epsilon_{it}. \tag{OB.1}$$

In this specification, μ_i are town-specific indicator variables, δ_t are 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

[17]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 number of owner-occupied homes for solar suitability.

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

Table OB.2: Average Treatment Effects of Classic and Express

VARIABLE	**(1)**	**(2)**	**(3)**	**(4)**
Classic	0.143	0.143		
	(0.056)**	(0.054)**		
	[0.016, 0.276]**	[0.016, 0.276]**		
Classic first 8 weeks			0.054	0.054
			(0.030)*	(0.028)*
			[-0.014, 0.130]	[-0.014, 0.130]
Classic middle 8 weeks			0.029	0.029
			(0.021)	(0.020)
			[-0.014, 0.079]	[-0.014, 0.079]
Classic last 8 weeks			0.357	0.357
			(0.127)***	(0.126)***
			[0.046, 0.739]**	[0.046, 0.739]**
Express	0.237	0.230		
	(0.154)	(0.154)		
	[-0.042, 0.614]	[-0.042, 0.614]**		
Express first 8 weeks			-0.016	-0.023
			(0.008)**	(0.008)***
			[-0.030, -0.001]**	[-0.030, -0.001]**
Express last 8 weeks			0.528	0.521
			(0.302)*	(0.302)*
			[0.019, 1.548]**	[0.019, 1.548]**
Town FE	no	yes	no	yes
Year-Week FE	yes	yes	yes	yes
R-squared	0.093	0.113	0.140	0.161
N	8,716	8,716	8,716	8,716

Note: Dependent variable is the weekly number of signed solar contracts per 1,000 owner-occupied solar-suitable homes. Unit of observation is town-week. Total effect in an average Classic and Express town is calculated as the sum of weekly effects over the duration of the respective campaign. Total Classic and Express effects are not statistically different in all specifications. Two-way clustered standard errors by town (62 clusters) and week (144 clusters) in parentheses. Wild cluster bootstrap (Cameron et al., 2008) 95% confidence intervals, reported in square brackets (1,000 draws). $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

We estimate specification (OB.1) 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.[18] We do this twice, without and with town fixed effects. Table OB.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 esti-

[18]See Table OB.5 for results with alternative control groups, which generally show quite similar results.

mated since 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 statistically significant difference across the campaigns in total adoptions (3.43 per 1,000 suitable homes for Classic and 3.79 for Express, in the specification with town fixed effects).

Given that there are a small number of clusters in the treatment groups of Classic and Express (16), 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. In particular, 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). The results of our hypotheses tests are nearly the same using this approach. We report the 95th percent confidence intervals in Table OB.2 in brackets.

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 OB.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.[19] This could also serve to reduce WOM in the Express campaigns. Again, we find no significant difference across the campaigns in total adoptions over the entire length of the campaigns in these split specifications.

[19] Surasvadi et al. (2016) use a model of strategic forward-looking consumers to show that consumers will join the group buy only after a certain time threshold.

During Campaign Analysis without Control Towns

To further examine whether the Classic and Express campaigns led to a different number of total adoptions during the campaigns, we ran a

Table OB.3: Net Effect of Shortening the Campaign on Installations During the Campaign

VARIABLE	(1)	(2)	(3)	(4)
Express	0.105	0.105		
	(0.152)	(0.162)		
Classic middle 8 weeks			-0.036	-0.024
			(0.034)	(0.032)
Classic last 8 weeks			0.295**	-0.098
			(0.134)	(0.313)
Express first 8 weeks			-0.075***	-0.057
			(0.033)	(0.037)
Express last 8 weeks			0.457	0.164
			(0.311)	(0.155)
Constant	0.172***		0.085**	
	(0.057)		(0.032)	
Δ Total Effect	0.300	0.300	0.300	0.300
p-value				
Baseline regression	0.90	0.91	0.91	0.91
Wild cluster bootstrap	0.94	0.96	0.94	0.95
Year-Week FE	no	yes	no	yes
R-squared	0.006	0.224	0.104	0.234
N	344	344	344	344

Note: Dependent variable is the weekly number of signed solar contracts per 1,000 owner-occupied solar-suitable homes. Unit of observation is town-week. Sample includes only campaign observations from Classic and Express towns. Total effect in an average Classic and Express town is calculated as the sum of weekly effects over the duration of the respective campaign. Δ Total Effect equals the difference between the predicted total effect in an average Express town relative to an average Classic town. Two-way clustered standard errors by town (16 clusters) and week (27 clusters) in parentheses. p-values reported from testing the null hypothesis that Δ Total Effect = 0. "Wild cluster bootstrap" reports the same p-values using a wild cluster bootstrap (1,000 draws) technique. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

simple regression of weekly installations on an Express dummy variable to capture any difference in effectiveness between the two campaigns.

$$y_{it} = \alpha E_i + \mu_i + \delta_t + \epsilon_{it}. \tag{OB.2}$$

in which E_i is a dummy variable indicating an Express campaign and y_{it} is the number of solar adoptions per 1,000 suitable non-adopting homes at time t. For the during-campaign analysis, we use weekly data during the campaign; using weekly data allows us to control for the slight stag-

gering of the campaign end dates using weekly dummy variables, δ_t. The α coefficient captures the differential weekly effect of the Express treatment relative to the Classic treatment. We use the estimates to calculate the difference between the total predicted installations in an average Express town relative to an average Classic town. In the post-campaign analysis, we use monthly data due to the lower adoption rates, for a 24-month post-campaign period.

The results of estimating equation (OB.2) with the during campaign data without week fixed effects are shown in Table OB.3 in column (1). The intercept of 0.172 in column (1) can be interpreted as the weekly number of installations per 1,000 homes for a Classic campaign. The Express coefficient is the additional number of weekly installations per 1,000 homes for an Express campaign. The point estimate for Express is 61% the size of the intercept, indicating the weekly adoptions are 61% higher in Express, which is to be expected, since the campaigns are only two-thirds as long and the resource intensity is higher. The main object of interest is the total difference in adoptions across the campaigns. We find that Express campaigns are not significantly different than Classic campaigns in terms of total adoption over the campaigns, with a point estimate for the difference of just 0.3 installations per 1,000 suitable homes (1.6 installations for an average size town).

In column (2), we add week fixed effects, with the same results. In columns (3) and (4), we allow the campaigns to have different effects at different times of the campaigns. The first eight weeks of the Classic campaigns are before the Express campaigns begin. The middle eight weeks align with the first eight weeks of the Express campaigns. The last eight weeks for all campaigns occur approximately at the same time. We find the exact same difference in the expected total number of adoptions across campaign types, with similar lack of significance. Robustness checks are in Online Appendix C.

During Campaign Analysis with Different Control Towns

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 OB.4. 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. As shown in Table OB.5, this yields total treatment effects that are quite similar to our earlier results. Gillingham and Bollinger (2021) provide evidence for no spillovers across town borders, which would lead us to slightly underestimate the effects.

Table OB.4: 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 OB.5: Total Treatment Effect by Specification

VARIABLE	Matching	Current
Total Classic Adoptions (per 1,000 suitable homes)	3.33	3.07
Total Express Adoptions (per 1,000 suitable homes)	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.

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. Under this specification, the Express and Classic treatment effects are identified because of the staggered timing of the campaigns, with Express being shorter. Table OB.5 displays the total treatment effect in an average Classic and Express town, which are consistent with our earlier findings.

Online Appendix C: Robustness Checks

Post-campaign Analysis excluding Campaign Leads

To test to see if the post-campaign effect is due exclusively to the campaign leads adopting after the campaign concluded, we use the same analysis for as in Table 6, excluding those households who were leads generated during the campaigns. The effects of WOM remain positive and significant.

Table OC.1: Installation Growth Post-Solarize

VARIABLE	(1)	(2)	(3)	(4)
Express	-0.183	-0.177	0.029	0.461
	(0.094)*	(0.141)	(0.229)	(0.120)***
	[-0.423, 0.057]	[-0.542, 0.208]	[-0.742, 0.793]	[0.155, 0.804]**
Express × year 2	0.057	0.057	-0.18	-0.132
	(0.141)	(0.141)	(0.133)	(0.130)
	[-0.298, 0.407]	[-0.303, 0.407]	[-0.498, 0.172]	[-0.44, 0.211]
Campaign installs	0.037	0.038	0.065	0.039
	(0.018)*	(0.018)*	(0.020)***	(0.017)**
	[-0.011, 0.105]*	[-0.012, 0.102]*	[0.014, 0.151]**	[-0.015, 0.118]*
# Active installer	0.044	0.045	0.017	0.015
	(0.017)**	(0.017)**	(0.018)	(0.017)
	[0.002, 0.105]**	[-0.001, 0.107]*	[-0.027, 0.064]	[-0.046, 0.062]
Workshop		0.002	-0.031	-0.026
		(0.023)	(0.048)	(0.032)
		[-0.083, 0.082]	[-0.262, 0.134]	[-0.175, 0.097]
Price per W			-0.114	-0.068
			(0.113)	(0.096)
			[-0.877, 0.186]	[-0.566, 0.113]
Leads			0.007	0.006
			(0.006)	(0.006)
			[-0.01, 0.025]	[-0.013, 0.027]
Word-of-Mouth				4.362
				(1.412)***
				[0.469, 8.591]**
Year-Month FE	yes	yes	yes	yes
Constant	yes	yes	yes	yes
R-squared	0.202	0.202	0.498	0.551
N	384	384	266	266

Note: Dependent variable is the monthly number of solar installations, excluding leads that adopted during the post-Solarize period, normalized by the 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 leads (normalized by potential solar market) collected during Solarize, and word-of-mouth, defined as the average share of adopters that heard about Solarize from friends/neighbors or from another solar customer. Unit of observation is town-month. Main sample: Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Two-way clustered standard errors by town (16 clusters) and month (24 clusters) in parentheses. Year-Month FE is absorbed. Wild cluster bootstrap (Cameron et al., 2008) 95% confidence intervals, reported in square brackets (1000 draws). Results of our hypothesis tests are identical using both methods. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Post-campaign Analysis with alternative Definitions for WOM

Table OC.2 shows the results of the main analysis using all survey responses, including non-adopting leads as well as solar adopters in the construction of the WOM variable.

Table OC.3 shows the results of the main analysis using an alternative WOM measure that includes the information received by town leaders in addition to friends or neighbors and other

solar customers in the construction of the WOM variable.

Table OC.2: Installation Growth Post-Solarize

VARIABLE	**(1)**	**(2)**	**(3)**	**(4)**
Express	-0.216	-0.222	-0.061	0.325
	(0.098)**	(0.155)	(0.258)	(0.138)**
	[-0.462, 0.027]*	[-0.632, 0.201]	[-0.903, 0.883]	[-0.064, 0.754]*
Express × year 2	0.089	0.089	-0.061	-0.048
	(0.141)	(0.139)	(0.115)	(0.102)
	[-0.242, 0.412]	[-0.243, 0.413]	[-0.287, 0.159]	[-0.302, 0.201]
Campaign installs	0.042	0.042	0.073	0.052
	(0.021)*	(0.020)*	(0.023)***	(0.019)**
	[-0.019, 0.131]	[-0.018, 0.126]	[0, 0.174]*	[-0.013, 0.13]*
# Active installer	0.045	0.045	0.02	0.005
	(0.018)**	(0.018)**	(0.021)	(0.018)
	[0.001, 0.103]**	[0.001, 0.1]**	[-0.039, 0.084]	[-0.065, 0.055]
Workshop		-0.002	-0.035	0.034
		(0.026)	(0.053)	(0.030)
		[-0.086, 0.087]	[-0.256, 0.122]	[-0.081, 0.185]
Price per W			-0.085	-0.074
			(0.089)	(0.079)
			[-0.362, 0.126]	[-0.31, 0.124]
Leads			0.008	0.001
			(0.007)	(0.005)
			[-0.013, 0.029]	[-0.017, 0.019]
Word-of-Mouth				4.557
				(1.700)**
				[-1.451, 11.454]*
Year-Month FE	yes	yes	yes	yes
Constant	yes	yes	yes	yes
R-squared	0.193	0.193	0.497	0.540
N	384	384	276	276

Note: Dependent variable is the monthly number of solar installations normalized by the 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 leads (normalized by potential solar market) collected during Solarize, and word-of-mouth, defined as the average share of survey respondents who adopted solar that heard about Solarize from friends/neighbors or from another solar customer. Unit of observation is town-month. Main sample: Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Two-way clustered standard errors by town (16 clusters) and month (24 clusters) in parentheses. Year-Month FE is absorbed. Wild cluster bootstrap (Cameron et al., 2008) 95% confidence intervals, reported in square brackets (1000 draws). Results of our hypothesis tests are identical using both methods. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table OC.3: Installation Growth Post-Solarize

VARIABLE	(1)	(2)	(3)	(4)
Express	-0.216	-0.222	-0.061	0.513
	(0.098)**	(0.155)	(0.258)	(0.119)***
	[-0.462, 0.029]*	[-0.63, 0.201]	[-0.903, 0.884]	[0.205, 0.827]**
Express × year 2	0.089	0.089	-0.061	-0.062
	(0.141)	(0.139)	(0.115)	(0.108)
	[-0.242, 0.412]	[-0.243, 0.413]	[-0.288, 0.16]	[-0.34, 0.216]
Campaign installs	0.042	0.042	0.073	0.058
	(0.021)*	(0.020)*	(0.023)***	(0.016)***
	[-0.019, 0.131]	[-0.018, 0.126]	[0, 0.173]*	[0.004, 0.122]**
# Active installer	0.045	0.045	0.02	0.056
	(0.018)**	(0.018)**	(0.021)	(0.031)*
	[0.001, 0.103]**	[0.001, 0.101]**	[-0.039, 0.083]	[-0.045, 0.174]
Workshop		-0.002	-0.035	0.034
		(0.026)	(0.053)	(0.024)
		[-0.086, 0.087]	[-0.262, 0.122]	[-0.065, 0.116]
Price per W			-0.085	-0.049
			(0.089)	(0.077)
			[-0.363, 0.127]	[-0.311, 0.111]
Leads			0.008	-0.001
			(0.007)	(0.005)
			[-0.013, 0.029]	[-0.018, 0.015]
Word-of-Mouth				5.842
				(1.967)***
				[-0.775, 12.062]*
Year-Month FE	yes	yes	yes	yes
Constant	yes	yes	yes	yes
R-squared	0.193	0.193	0.497	0.550
N	384	384	276	276

Note: Dependent variable is the monthly number of solar installations normalized by the 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 leads (normalized by potential solar market) collected during Solarize, and word-of-mouth, defined as the average share of survey respondents who adopted solar that heard about Solarize from friends/neighbors, from another solar customer, or from a town leader. Unit of observation is town-month. Main sample: Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Two-way clustered standard errors by town (16 clusters) and month (24 clusters) in parentheses. Year-Month FE is absorbed. Wild cluster bootstrap (Cameron et al., 2008) 95% confidence intervals, reported in square brackets (1000 draws). Results of our hypothesis tests are identical using both methods. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

No Scaling by Market Size

Table OC.4 shows the results of the main analysis using log counts for the number of installing home as the DV and using the number of past installations and leads as the regressors, rather than scaling these by market size.

Table OC.4: Installation Growth Post-Solarize, Log Counts

VARIABLE	**(1)**	**(2)**	**(3)**	**(4)**
Express	-0.049	-0.165	0.124	0.506
	(0.158)	(0.276)	(0.304)	(0.350)
Express × year 2	0.083	0.083	-0.065	-0.058
	(0.102)	(0.109)	(0.099)	(0.101)
Campaign installs	0.018**	0.016**	0.021	0.021
	(0.007)	(0.007)	(0.014)	(0.012)
# Active installer	0.083**	0.085***	0.042	0.031
	(0.030)	(0.028)	(0.032)	(0.029)
Workshop		-0.034	-0.010	-0.010
		(0.053)	(0.063)	(0.056)
Price per W			-0.033	-0.019
			(0.041)	(0.038)
Leads			-0.001	0.000
			(0.001)	(0.001)
Word-of-Mouth				2.431**
				(1.131)
Year-Month FE	yes	yes	yes	yes
Constant	yes	yes	yes	yes
R-squared	0.538	0.542	0.552	0.576
N	384	384	276	276

Note: Dependent variable is the log monthly number of solar installations (plus one). Solarize related variables: campaign installs, defined as total number of installations during Solarize. Express: categorial variable for shorter version of Solarize, number of workshop and leads collected during Solarize, and word-of-mouth, defined as the average share of survey respondents who adopted solar that heard about Solarize from friends/neighbors or another solar customer. Unit of observation is town-month. Main sample: Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Two-way clustered standard errors by town (16 clusters) and month (24 clusters) in parentheses. Year-Month FE is absorbed. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Post-Campaign Analysis Allowing for Different Year Two Effect of WOM

Table OC.5: Installation Growth Post-Solarize

VARIABLE	(1)	(2)	(3)	(4)
Express	-0.217**	-0.222	-0.084	0.300**
	(0.098)	(0.155)	(0.266)	(0.113)
Express × year 2	0.089	0.089	-0.02	0.218*
	(0.141)	(0.139)	(0.096)	(0.114)
Campaign installs	0.042*	0.042*	0.074***	0.046**
	(0.021)	(0.02)	(0.024)	(0.019)
# Active installer	0.045**	0.045**	0.02	0.016
	(0.018)	(0.018)	(0.02)	(0.020)
Workshop		-0.002	-0.035	-0.031
		(0.026)	(0.053)	(0.036)
Price per W			-0.089	-0.067
			(0.095)	(0.080)
Leads			0.007	0.008
			(0.006)	(0.006)
Leads × year 2			0.002	-0.001
			(0.004)	(0.003)
Word-of-Mouth				3.512***
				(1.137)
Word-of-Mouth × year 2				2.303*
				(1.197)
Year-Month FE	yes	yes	yes	yes
Constant	yes	yes	yes	yes
R-squared	0.193	0.193	0.498	0.554
N	384	384	276	276

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 campaign conclusion, number of workshop and leads (normalized by potential solar market) collected during Solarize, Word-of-Mouth, defined as the average share of adopters that heard about Solarize from a friend or neighbor. Unit of observation is town-month. Main sample: Solarize Classic and Express campaigns observed for 24 month after the conclusion of Solarize. Year-Month FE is absorbed. Two-way clustered standard errors by town (16 clusters) and month (24 clusters) in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table OC.6: WOM Effects

VARIABLE	OLS **(1)**	IV **(2)**
Express	0.473***	0.404***
	(0.154)	(0.120)
Campaign installs	0.046*	0.051**
	(0.022)	(0.021)
# Active installer	0.018	0.018
	(0.020)	(0.018)
Price per W	-0.061	-0.066
	(0.093)	(0.082)
Leads	0.007	0.007
	(0.006)	(0.005)
Leads × year 2	-0.001	-0.001
	(0.003)	(0.003)
Word-of-Mouth	3.893**	3.065***
	(1.322)	(0.989)
Word-of-Mouth× year 2	1.541	1.836*
	(0.947)	(0.945)
Year-Month FE	yes	yes
Constant	yes	yes
R-squared	0.571	0.526
N	221	221
# Excluded instruments		6
F statistic (WOM)		73.79
F statistic (WOM x year 2)		20.69
Hansen J statistic		0.21

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 Solarize from friends/neighbors or another solar customer. Unit of observation is town-month. Main sample: 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 interacted with the post dummy. Year-Month FE is absorbed (partialled-out in the IV regresssion). Two-way clustered standard errors by town (13 clusters) and month (24 clusters) in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

To allow for different effects of WOM over time, we estimate a specification in which we interact the WOM variable with a time dummy that indicates the distance (in years) from the campaign conclusion. We interact our previous set of instruments with a dummy variable indicating the second year post-campaign in order to create the additional instruments. Analogous tables to Tables 6 and 7 allowing for time-varying effects are shown in Tables OC.5 and OC.6. All of the findings hold, although the effects are smaller and lose significance in the second year

post-campaign.

Post-campaign Analysis using All Owner-Occupied Homes

As another 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 (Table OC.7); all of the results are robust.

Table OC.7: Installation Growth Post-Solarize. All Owner-Occupied Homes

VARIABLE	**(1)**	**(2)**	**(3)**	**(4)**
Express	-0.134**	-0.134	-0.055	0.154*
	(0.061)	(0.096)	(0.106)	(0.087)
Express × year 2	0.082	0.082	-0.005	0.006
	(0.079)	(0.078)	(0.061)	(0.062)
Campaign installs	0.035*	0.035*	0.036**	0.034**
	(0.019)	(0.018)	(0.015)	(0.014)
# Active installer	0.024**	0.024**	0.019	0.012
	(0.011)	(0.011)	(0.014)	(0.013)
Workshop		0.000	-0.061	-0.039
		(0.015)	(0.036)	(0.032)
Price per W			-0.031	-0.027
			(0.046)	(0.045)
Leads			0.010**	0.007
			(0.005)	(0.004)
Word-of-Mouth				1.750***
				(0.525)
Year-Month FE	yes	yes	yes	yes
Constant	yes	yes	yes	yes
R-squared	0.163	0.163	0.515	0.533
N	384	384	276	276

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 leads (normalized by number of households) collected during Solarize, and word-of-mouth, defined as the average share of adopters that heard about Solarize from friends/neighbors or another solar customer. Unit of observation is town-month. Main sample: Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Year-Month FE is absorbed. Two-way clustered standard errors by town (16 clusters) and month (24 clusters) in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table OC.8: WOM effects. All Owner-Occupied Homes

VARIABLE	OLS **(1)**	IV **(2)**
Express	0.252	0.227
	(0.074)***	(0.062)***
Campaign installs	0.029	0.029
	(0.017)	(0.016)*
# Active installer	0.010	0.011
	(0.011)	(0.011)
Price per W	-0.021	-0.022
	(0.051)	(0.049)
Leads	0.006	0.007
	(0.003)	(0.004)*
Word-of-Mouth	1.907	1.642
	(0.572)***	(0.482)***
Year-Month FE	yes	yes
Constant	yes	yes
R-squared	0.556	0.451
N	221	221
First stage F statistic		33.9
Hansen J-statistic (p-value)		0.45

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 friends/neighbors or another solar customer. Unit of observation is town-month. Main sample: 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" $\times$ quarter of campaign start and the experimental variation from Express in Column (2) Column (3) omits "Express" from the list of excluded instruments. Year-Month FE is absorbed (partialled-out in the IV regresssion). Two-way clustered standard errors by town (13 clusters) and month (24 clusters) in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Installation Prices Post-Solarize

We estimate equation (1) to see if there are any post-campaign differences in price across campaign types (Table OC.9). We see no significant differences.

Table OC.9: Installation Prices Post-Solarize as DV

VARIABLE	(1)	(2)	(3)	(4)
Express	0.024	0.100	0.103	-0.023
	(0.137)	(0.173)	(0.183)	(0.215)
Express × year 2	0.063	0.064	0.067	0.061
	(0.205)	(0.203)	(0.203)	(0.202)
Campaign installs	-0.008	-0.008	0.007	0.014
	(0.012)	(0.012)	(0.021)	(0.021)
# Active installer	-0.018	-0.016	-0.024	-0.023
	(0.010)	(0.009)	(0.017)	(0.017)
Workshop		0.022	0.041	0.040
		(0.016)	(0.026)	(0.025)
Leads			-0.005	-0.005
			(0.006)	(0.006)
Word-of-Mouth				-1.218
				(1.006)
Year-Month FE	yes	yes	yes	yes
Constant	yes	yes	yes	yes
R-squared	0.188	0.190	0.194	0.20
N	276	276	276	276

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 workshops and leads (normalized by number of households) collected during Solarize, and WOM, defined as the average share of adopters that heard about Solarize from friends/neighbors or another solar customer. Unit of observation is town-month. Main sample: Solarize Classic and Express campaigns observed for 24 month after the conclusion of the Solarize intervention. Year-Month FE is absorbed. Two-way clustered standard errors by town (16 clusters) and month (24 clusters) in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Effects Using all Rounds of Solarize Classic

To address the small sample size, we repeat the analyses for the WOM results, augmenting our main dataset with data from other rounds of the Solarize program. We have data for three other rounds of the Classic campaigns, one before this experiment and two after. We do not include the very first round of 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 D in Table OC.10 – 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 robust when including these other campaigns (controlling for price differences across campaigns).

Table OC.10: 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.11: Solarize Installations & Number of leads for All Rounds of Classic

VARIABLE	Mean	Sd	Median	T-test
Leads / solar suitable homes [$\times$1000]	55.52	27.37	53.37	0.27
Campaign installs / solar suitable homes [$\times$1000]	11.62	8.34	10.22	0.29
# Municipalities		30		

Note: All Solarize Classic towns participating in Solarize CT, pooled rounds 2013-2015. Campaign installs is defined as total number of installations during Solarize / solar suitable homes [x1000]. Leads is total number of leads collected during Solarize / solar suitable homes [x1000]. Two-sample t-test for differences in mean to Express (Table 3). Unit of observation: town.

Table OC.12: 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

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.

Table OC.13: Installation Growth Post-Solarize. All Classic Campaigns.

VARIABLE	**(1)**	**(2)**	**(3)**	**(4)**
Express	-0.285**	-0.342*	-0.167	0.195
	(0.106)	(0.177)	(0.250)	(0.193)
Express ×year 2	0.110	0.110	0.024	0.053
	(0.103)	(0.103)	(0.108)	(0.104)
Campaign installs	0.018**	0.019**	0.033**	0.023*
	(0.009)	(0.009)	(0.015)	(0.012)
# Active installer	0.026**	0.025*	-0.024	-0.003
	(0.012)	(0.013)	(0.017)	(0.016)
Workshop		-0.018	-0.018	-0.034
		(0.029)	(0.047)	(0.034)
Price per W			-0.130*	-0.088*
			(0.067)	(0.050)
Leads			0.003	0.003
			(0.004)	(0.004)
Word-of-Mouth				4.103***
				(1.335)
Year 2	0.102	0.103	0.202**	0.217**
	(0.117)	(0.118)	(0.095)	(0.096)
Year-Month FE	yes	yes	yes	yes
Solarizeround FE	yes	yes	yes	yes
Constant	yes	yes	yes	yes
R-squared	0.159	0.159	0.294	0.361
N	777	777	598	598

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 leads (normalized by potential solar market) collected during Solarize, and word-of-mouth, defined as the average share of adopters that heard about Solarize from friends/neighbors or another solar customer. 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. Year-Month FE is absorbed. Two-way clustered standard errors by town (35 clusters) and month (37 clusters, given staggered implementation) in parentheses. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Online Appendix D: Campaign Costs

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.