The Effect of Group Pricing and Deal Duration on Word-of-Mouth and Durable Good Adoption: The Case of Solarize CT

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Abstract

Group pricing, in which a group of consumers receive a lower price if enough people purchase the product, has become very popular. In part this is because group pricing may help spur consumer word-of-mouth (WOM), since consumers are intrinsically motivated to speak with other potential consumers. Limited time price promotions are also extensively used in marketing to spur product demand. However, in product categories that rely on WOM, limiting the duration of the deal may handicap the consumer's ability to share information before the deal expires, reducing both WOM and customer adoption. In this paper we study the effects of both group pricing and deal duration on WOM and solar photovoltaic (PV) adoption in the Solarize Connecticut program. We find that WOM cannot operate as effectively if price promotions are too short, and that group pricing does indeed spur WOM for those affected by the deals; however, in the absence of group pricing, WOM from peers continues to operate and actually increases in effectiveness.

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1 Introduction

With the emergence of platforms such as Groupon, group pricing, or "interpersonal bundling", has seen renewed interest among marketers (Armstrong and Chen 2013). One of the benefits of group pricing is that consumers are intrinsically motivated to share (positive) word-of-mouth (WOM) with others in order for everyone to benefit from the lower resulting price if more people purchase the product. Of course, group pricing requires firms to designate the length of the period over which total purchases are used to determine price. The effect of deal duration has been studied in the marketing literature and we know that restrictions on deals such as purchase limits, purchase preconditions (such as group pricing), and time limits can signal value (Inman, Peter, and Raghubir 1997), especially when overpayment concerns are active (Srivastava and Lurie 2004; Dutta 2012), such as in the case of durable goods with uncertain payoffs.

Despite the emergence of group pricing, there is a dearth of research that assesses the impact of deal duration in the context of durable goods with uncertain payoffs, or the effect of group pricing itself on WOM and resultant sales. There is reason to believe that combining group pricing with short duration deals may be a risky strategy. Theory suggests that with group pricing, sellers benefit from the dissemination of information through peer effects from word-of-mouth (WOM). Previous studies (Kauffman and Wang 2001; Kauffman, Lai, and Ho 2010) have shown that one should expect inertia in group buys, with the greatest uptake at the end of the deal. This intuitively makes sense since consumers have an incentive to wait for more information regarding what the final price would be. However, no study has explored the role of social information on buying behavior with group pricing. Zhou, Xu, and Liao (2013) come the closest to studying this phenomenon; they examine group pricing empirically using an application of the Bass model for two group-buying websites in China. A limitation of the Zhou et al. (2013) approach is that information effects are operationalized simply through cumulative adoptions, whose effect decreases over the duration of the deal.

One of the challenges in assessing the impact of group pricing on social information is the difficulty inherent in measuring offline WOM. We overcome this challenge with a combination of field experiments and extensive survey data from solar adopters.[1] In this paper, we present the results from two randomized field experiments that varied the elements of a limited duration group buy marketing campaign, called Solarize Connecticut (Solarize CT). The goals of these field experiments were to test the importance of sales duration under group pricing, and to examine the effect of group pricing itself on WOM and product adoption. The Solarize CT program leverages WOM with the use of "solar ambassadors"

[1] Lovett and Staelin (2016) use a similar strategy to measure offline WOM.

(volunteer residents who speak to their neighbors about solar) and town events, and offers group pricing through a single contractor so that as more people commit to installing solar during the campaign, the price is lowered for everyone.[2]

In the first field experiment, we tested the effect of program duration on solar adoption by randomly assigning some towns to be in the 20-week "Classic" version of Solarize and other towns to be in the 12-week "Express" version, both of which used group pricing. In the second experiment, we tested the influence of group pricing itself by randomly assigning some towns a flat price (we refer to this treatment as "Solarize Prime"), while the remaining towns used group pricing as before ("Classic" towns). In both rounds, we surveyed all solar adopters to determine the importance of various information sources for how people learned about the Solarize campaign and what affected their adoption decisions.

The first main finding of the paper is that if the deal duration is too short, WOM does not have time to operate effectively. The second main finding is that removing group pricing lessens the effect of WOM from peers who are extrinsically motivated to convince others to install solar due to the group pricing, but strengthens the importance of WOM from other sources. The net result is that group pricing does not increase the effectiveness of the Solarize campaigns. WOM is known to be particularly strong with solar photovoltaics (PV) (Bollinger and Gillingham 2012), and so this finding suggests that group pricing may not be as useful of a strategy in product categories that already experience strong WOM.

Our results have substantive as well as theoretical implications. The Department of Energy and other state and local government agencies have poured millions 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 and the costs of these programs are proportional to the length of the campaign, then then shorter campaigns might be expected to improve the return to taxpayers from the investment. Unfortunately, we find that this is not the case if we shorten the Solarize program deal duration. However, group pricing also involves administrative costs from managing a request-for-proposal process and additional monitoring costs. Our finding that group pricing is not essential for WOM to play a significant role in adoption suggests that this campaign component can be removed, which can reduce costs. Moreover, it may facilitate a more competitive marketplace with more than one installer involved in each campaign, which is logistically impossible with group pricing.

The remainder of the paper is organized as follows. In the next section we discuss the theoretical and behavioral literatures that motivate the study. In Section 2, we describe the

[2]The program typically involves five price tiers with tier 5 providing the lowest price if enough people adopt.

Solarize program and experiment in more detail and describe the data. In Section 5, we discuss our estimation method and results, and we provide a general discussion in Section 7. Section 8 concludes.

2 Experimental Setting

The Solarize CT program is a joint effort between a state agency, the Connecticut Green Bank (CGB), and a non-profit marketing firm, SmartPower. The first critical component to the Solarize program is the use of volunteer promoters or ambassadors to provide information to their community about solar PV. There is growing evidence on the effectiveness of promoters or ambassadors in driving social learning and influencing behavior (Kremer, Miguel, Mullainathan, Null, and Zwane 2011; Vasilaky and Leonard 2011; BenYishay and Mobarak 2014; Ashraf, Bandiera, and Jack 2015).

The second major component to the Solarize program is the focus on community-based recruitment. In Solarize, this consists of mailings signed by the ambassadors, open houses to provide information about panels, tabling at events, banners over key roads, op-eds in the local newspaper, and even individual phone calls by the ambassadors to neighbors who have expressed interest.[3] Figure 1 shows examples of the Solarize presence within a community during a campaign.

The third major component is the group pricing discount offered to the entire community based on the number of contracts signed. This provides an incentive for early adopters to convince others to adopt and to let everyone know how many people in the community have adopted. With the group pricing comes a limited deal duration for the campaign. The limited time frame may provide a motivational reward effect (Duflo and Saez 2003), for the price discount would be expected to be unavailable after the campaign. For more details about the program, see Gillingham, Bollinger, and Staver (2015). In this paper, we experimentally vary the length of the campaigns and whether group pricing is used in order to assess the impact on WOM and total installations.

The standard timeline for a Solarize "Classic" is as follows:

1. CGB and SmartPower inform municipalities about the program and encourage town leaders to submit an application to take part in the program.
2. CGB and SmartPower select municipalities from those that apply by the deadline.

[3] Jacobsen, Kotchen, and Clendenning (2013) use non-experimental data to show that a community-based recruitment campaign can increase the uptake of green electricity using some (but not all) of these approaches.

3. Municipalities issue a request for group discount bids from solar PV installers for each municipality.
4. Municipalities choose a single installer, with guidance from CGB and SmartPower.
5. CGB and SmartPower recruit volunteer "solar ambassadors."
6. A kickoff event begins a ∼20-week campaign featuring workshops, open-houses, local events, etc. coordinated by SmartPower, CGB, the installer, and ambassadors.
7. Consumers that request them receive site visits and if the site is viable, the consumer may choose to install solar PV.
8. After the campaign is over, the installations occur.

With the support of the CGB, The John Merck Fund, The Putnam Foundation, and a grant from the U.S. Department of Energy, six rounds of Solarize CT have been run. Each of the rounds included Solarize Classic campaigns, but in two of the rounds–the third and fifth–we experimented with two additional treatments. In the first field experiment, we examined the impact of campaigns of different length on solar installations and the generation of off-line WOM. Specifically, during round 3 (R3) we randomly assigned towns to either be in the 20-week "Classic" version of Solarize, or the 12-week "Express" version.[4] A total of 11 towns were assigned to the Classic program, while 5 towns received the shorter Express program. The experiment was designed so that the end dates of all campaigns approximately overlap. Table 1 provides a list of all towns and campaign dates.

The second experiment occurred during round 5 (R5). For logistical reasons, there were limits to the number of towns that can be included in any one round. In the second experiment, we randomized across 11 municipalities: seven Classic Solarize towns, which had group pricing, and four "Prime" towns, which did not have group pricing.[5] These are listed with their start and end dates in Table 2. In the main specification, we use the 40 Clean Energy Communities with population between 1,000 and 60,000 (all of our treated municipalities lie within this range) that had not yet participated in a Solarize program or the CT Solar Challenge (an installer-run version of Solarize) as control groups, and we examine the validity of these control groups below. The CT Clean Energy Communities are towns that have expressed particular interest in clean energy and have formed a committee or task force on the topic. Nearly all Solarize towns are drawn from the the CT Clean Energy Communities.

[4]The only exception to the 20-week Classic program is Newtown, where the campaign was extended by two weeks.

[5]Average prices were the same whether there was group pricing or not.

3 Theoretical Background

3.1 Deal Duration

Our first experiment involved greatly shortening the duration of the Solarize campaign. The literature in marketing indicates that a shorter deal duration may in fact lead to greater purchase likelihood (Simonson 1992; Dhar and Nowlis 1999; Suri and Monroe 2003). However, how the duration of the deal affects group buys is unclear a priori due to the combined effects of consumer arrival, information diffusion, and information processing and choice process. When it comes to information processing and choice process, we can turn to the consumer behavior literature to gain insights on what effects we might expect. 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, such as a case when the choice is difficult (as in the case of expensive, durable goods) indicating that the shorter campaign might still be effective in leading to similar adoption levels. Time pressure decreasing choice deferral is also consistent with Simonson (1992) if time pressure increases the anticipatory regret of making the wrong choice, and Suri and Monroe (2003) find that in situations where there is a high motivation to process information, an increase in time pressure will increase the perception of quality and decrease the perception of monetary sacrifice.

These studies demonstrate that shorter duration promotions may lead to a greater purchase likelihood. However, it should be noted that these were all laboratory studies with hypothetical choices between products, and that the no-purchase option was not included. In the case of solar, the most important decision is whether to buy or not and the choice of installer is often secondary (and in the case of Solarize Campaigns, there is only one installer offering the discounted price through the program). 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 as in our setting. These papers also do not consider WOM effects. Previous literature has shown the importance of peer effects in the diffusion of residential solar (Bollinger and Gillingham 2012) and so the duration of the deal must be sufficient to allow WOM time to operate. As noted by Berger and Schwartz (2011), offline WOM is affected by the temporal distance between an experience when WOM would be shared between agents.

If consumer adoption of solar leads to greater WOM, then we would expect the WOM to accelerate in shorter duration campaigns only if adoption is accelerated, since early demand shocks would exhibit positive carryover effects (Moretti 2011). Byers, Mitzenmacher, and Zervas (2012) find this in an online context, showing a positive correlation between purchases

and online WOM using daily deals on Groupon and LivingSocial.[6] However, in online contexts, time is less of a factor in the spread of WOM. In the case of offline WOM, deals that are too short in duration may stifle WOM simply by not allowing enough time for it to operate. This is especially true if adoption is not accelerated, as predicted by Surasvadi, Tang, and Vulcano (2016), who use a model of strategic forward-looking consumers to show that consumers will join the group buy only after a certain time threshold. Given the positive signaling value of the shorter deal duration and the behavior literature's findings regarding choice deferment, in conjunction with the role of WOM in the diffusion of solar PV, we make the following testable predictions:

Hypothesis 1. *For durable goods with strong WOM, shortening the duration of the deal will lead to greater adoption rates but lower overall adoption.*

This hypothesis results from the combination of forces which work in opposite directions, as described above.

Hypothesis 2. *For durable goods with strong WOM, shortening the duration of the deal will lead WOM to have less of a role in product diffusion.*

This hypothesis stems from the simple fact that generating offline WOM takes time.

Hypothesis 3. *For durable goods with strong WOM, each instance of WOM will be similarly effective across campaign durations.*

This third hypothesis is based on the idea that, while the amount of WOM varies with the campaign duration, the effectiveness of the WOM that does occur should be similar because the motivation for the WOM is the same for both the Express and Classic campaigns.

3.2 Group Pricing

The theory literature also tells us that sellers using group pricing may benefit from the dissemination of information through peer effects. Chen and Zhang (forthcoming) explain why interpersonal bundling is a profitable strategy in the presence of demand uncertainty, and how it may further boost profits by stimulating product information dissemination. Jing and Xie (2011) show that group buying dominates traditional individual-selling strategies when the information/knowledge gap between expert and novice consumers is neither too high nor too low (e.g., for products in the midstage of their life cycle) and when interpersonal information sharing is very efficient. The campaign we study in this paper explicitly promotes

[6] It is impossible for the authors to establish causality since unobserved product quality shocks could also lead to the same finding.

education and the diffusion of information, but there are significant information gaps across consumers because it is early in the diffusion process for solar PV.

Hu, Shi, and Wu (2013) study the impact of simultaneous and sequential provision of information in group buys. They find that group buys with sequential information (i.e., information is provided prior to the consumer purchase decision) lead to higher deal success rates and consumer surplus than when information is provided simultaneously with the purchase decision. WOM may be useful for providing such information prior to the consumer purchase. We are not aware of any empirical work that studies the role of group pricing on offline WOM, although it has been documented that more than 75% of WOM occurs through face-to-face interactions (Keller and Libai 2009). Given the many psychological drivers of WOM (see Berger (2014) for a review), we would expect the effectiveness of WOM to be altered with the inclusion of group pricing, given that group pricing includes extrinsic motivation for WOM.

Berger and Schwartz (2011) is one paper of which we are aware that studies incentives as a driver of WOM. The authors use revealed preference and experimental (field and lab) data and find that free product giveaways can indeed spur WOM. Jin and Huang (2014) study the tradeoff between extrinsic motivation in the form of referral rewards and finds that monetary compensation from referring a brand can be an inferior incentive (relative to in-kind rewards such as product giveaways) in terms of generating WOM and getting others to adopt, although if both parties are compensated that such incentives are more effective. In our context of group pricing, WOM involves benefits for both the generator of WOM and the recipient since the final price depends on future adoptions by others, leading to an extrinsic motivation that is absent in product giveaways.

In our context, consumers are made aware through the Solarize program (by both the installer and the town's Solarize website) of the current pricing "tier" that the program has reached before their purchase, consistent with sequential provision. Shi, Sui, and Yang (2014) explicitly introduce social networks of different structure in their model of group buying to show that group buys are more profitable in less disperse social networks. Liang, Ma, Xie, and Yan (2014) model information and demand dynamics focusing on the informational aspect of the group-buying mechanism and show that an improvement in information quality has a positive effect on customer surplus and the group-buying success rate.

These findings lead to the following testable predictions:

Hypothesis 4. *Group pricing leads to extrinsically-motivated WOM, increasing the effectiveness of deals.*

This hypothesis follows directly from the expected expanded role of WOM in Solarize campaigns.

Hypothesis 5. *Group pricing campaigns in which people are more likely to hear about the campaigns from extrinsically-motivated peers will be less effective without group pricing.*

This hypothesis follows from the presumption that when peers in the community are no longer incentivized to encourage others to adopt, the nature of WOM from people in the community becomes less persuasive in nature.

Hypothesis 6. *Group pricing campaigns for which WOM from extrinsically-motivated peers is rated as important will be less effective without group pricing.*

This hypothesis predicts that campaigns in towns in which community members' WOM is more important are less effective without group pricing, for the same reason–by removing the extrinsic motivation for WOM, the qualitative content is changed.

4 Data

We obtained data on contracts signed to install residential solar PV systems in each of the control and treatment towns from CGB. Using the approval dates, we matched each contract signing to a respective week, which allowed us to track the pattern of solar adoptions over time not only during, but also before and after the official Solarize campaign. This pattern is displayed in Figure 2 for the first experiment, and shows the number of weekly PV contracts signed across all Classic and Express towns over a 134-week time period. The shaded area in the graphs indicates the weeks in which there was an active campaign in at least some of the towns. A number of descriptive findings emerge from these simple trends. First, there is a clear overall boost in PV adoptions during each program. Second, this boost appears to take place during the later stage of the campaign, which is a common pattern for group buy programs and is also consistent with the notion that WOM and peer recommendations may require time to create an effect on potential adopters. Lastly, the Express campaign takes fewer weeks to accomplish the boost in adoptions. Overall, the raw data appear to be in accord with the findings of the existing literature discussed in Section 3.

In Figure 3, we plot the weekly contracts signed for the second experiment. Again we see the sharp spike at the end of the Classic campaign, but in the Prime campaign there is more of a steady increase. This implies that WOM may be playing less of a role in adoptions in the Prime campaign (consistent with Hypothesis 2), in which all consumers receive the same price regardless of the number who adopt. We further examine this hypothesis with detailed survey data on the reasons consumers adopt solar PV.

Following the completion of each round of Solarize, we sent online surveys to solar adopters in all of the towns. Across the five rounds of Solarize Classic, Express, and Prime,

we had a response rate of 41 percent. Survey responses help us explore the mechanisms through which information about the Solarize program reaches potential customers, as well as the importance that adopters place on different information sources. In particular, two of the key survey questions are: *How did you hear about the Solarize program?* and *How important were the following sources of information in your decision to install solar?* Summary statistics from individual responses to these two questions are presented in Table 3 for the first experiment (R3) and Table 4 for the second experiment (R5), respectively.

The response data provide insight into the extent to which differences in campaign duration affect the channels through which information reaches solar customers. In general, WOM takes time to operate. So, not surprisingly, the responses displayed in the top panel of Table 3 suggest that some of the primary Solarize channels, such as the recommendation of friend or neighbor and the recommendation of other customers, are used less in the Express towns than the Classic towns. A t-test for differences in means confirms that that this finding is statistically significant.

With less WOM, one might expect installers in Express towns to rely on other methods of disseminating information, such as inserts in papers and social media. The summary statistics in the lower panel of Table 3 are consistent with this story. The importance of recommendations from friends/neighbors is greater in Classic towns, whereas different print and social media are listed as relatively more important sources of information in Express towns. Surprisingly, work colleagues are listed as more important in Express, perhaps because neighbor feedback is important and the town campaign is too short to leverage these connections as much, so work connections become more important. Table 3 also indicates that a longer campaign makes seeing a completed solar installation more likely to be a factor in influencing adopters' decisions.

Table 4 shows the survey responses for the Classic/Prime comparison. Friends/neighbors and other solar customers are listed as an information source more often in the Classic campaigns than in the Prime campaigns, and the installer is listed as a more likely information source in the Prime campaigns. A notable finding is that respondents in Prime towns report that ambassadors are less important, while the people you interact with in the town and people you work with are more important on average. Seeing solar is also listed as more important. In order to assess the role of these different information sources and consumers' stated importance of these sources, we include these survey responses in some of our analyses; when we do so, we include all five Solarize Classic rounds. A summary of the responses for all five Solarize Classic rounds can be found in Table 5.

To estimate the treatment effects for the different campaigns, we convert our solar contract and survey data to a panel dataset, where the unit of observation is a town-week. This

allows us to examine the effect of campaign duration on the timing of solar adoptions, while controlling for possible unobserved heterogeneity through the use of town and week fixed effects in our regressions. We include 134 weeks of observations per treated town, with the initial week for each town chosen so that it precedes the town's official campaign start date by 2 years. We also ensure that, for each weekly observation from a treated town, there are observations from all towns in the control group. The fact that the Classic and Express campaigns in the first round of our experiment have different lengths and starting dates results in a 142-week period of observations for each control town. In contrast, in the second round of the experiment, the time window for the control group is 134 weeks, since both the Classic and Prime campaigns overlap and are of the same length. This leads to a panel of 7,824 observations in 56 towns for the Classic vs. Express analysis and 6,834 observations in 51 towns for the Classic vs. Prime analysis. Summary statistics for key demographic and socioeconomic variables in these final datasets are presented in Table 6 and 7, which generally indicate a good balance in observables across treatment and control groups.

In order to allow for some time for firms to compile all of the contracts signed through the Solarize campaigns, customers were offered Solarize prices and financial incentives as long as they signed within 10 days after the official end date of each campaign. We therefore extend the campaign by two weeks when defining the campaign dummies in our analysis. This results in a 22-week Classic campaign (24-week in Newtown due to the official two-week extension) in both rounds, a 22-week Prime campaign, and a 12-week Express campaign. Furthermore, due to the time lag between submission and approval of each PV contract, some installations with the contract signing occurring during a Solarize campaign have approval dates after the end of the program. In order to properly capture the effect of the program, we add these installations to the last week of the respective town's campaign.

To assess the validity of the control groups for each round, it is informative to compare the trends in adoptions between each of the two treatment groups and the control group. Figure 4 displays the cumulative PV adoptions during the 40-week time window for each of the two treatments in the first round of the experiment, overlaid with the trend in the control towns during the same period. It is reassuring to see that both pre- and post-treatment trends across the treated and control towns are similar, although the rate of adoption is higher in the Express towns pre-treatment. This is driven by the fact that the Express towns are larger, with greater population and more rooftops. The bottom panel in Figure 4 shows that when we examine the adoptions per rooftop, the pre-trends match much more closely.

Figure 5 presents the same figures for the second round. These figures again clearly demonstrate the steady adoption rate in Prime in contrast to the sharp acceleration of adoption that occurs in the Classic and Express campaigns at the end of the campaign. One

explanation for this difference–consistent with the theory in the literature discussed above–is that price uncertainty that comes with the group pricing mechanism may deter consumers from adopting in the short run.

Towns in the control group were not randomly assigned, so we use a host of robustness checks to validate the main findings. As robustness checks, we use propensity score matching to develop an alternative control group to estimate the treatment effects for each type of campaign, and we also estimate a specification in which the Solarize towns in round six are used as the set of control towns (using only their pre-Solarize data). In both cases, we find similar results, suggesting that the results are quite robust to the exact choice of the set of control towns.

Furthermore, the main objective of the paper is to assess the difference in effectiveness and role of WOM across the types of campaigns, which is why the emphasis was placed on random assignment of towns to different types of campaigns. To help address any further unobservables time invariant differences across towns, our approach is a difference-in-differences analysis with town fixed effects.

5 Method and Results

5.1 Classic vs. Express

Let y_{it} denote the number of solar contracts signed in town i during week t, where the subscript t refers to the number of weeks since the beginning of the campaign for the respective town (as opposed to the current calendar week). Furthermore, let C_{it} and E_{it} be dummy variables indicating the presence of an active Classic or Express campaign, respectively. Finally, in our second specification we use w_t to denote week-in-campaign dummies. These allow for a different effect of the campaign depending on how far the campaign had progressed, consistent with both the theory discussed above and the descriptive results showing a clear spike in adoptions towards the end of each campaign. We use a difference-in-differences estimation approach, with the following two empirical specifications:

$$y_{it} = \alpha^c C_{it} + \alpha^e E_{it} + \mu_i + \delta_\tau + \epsilon_{it}, \tag{1}$$

$$y_{it} = \alpha_t^c w_t C_{it} + \alpha_t^e w_t E_{it} + \mu_i + \delta_\tau + \epsilon_{it}, \tag{2}$$

where μ_i are town-specific indicator variables, δ_τ are calendar week fixed effects, and ϵ_{it} is an idiosyncratic error term.

Specification (1) estimates an average weekly treatment effect for each program. Specification (2) allows us to compare the effects of the two programs on solar adoptions over time, and is our preferred specification because we expect the effects of the Solarize program to vary over the length of the campaigns. We can aggregate the weekly treatment effects in specification (2) in order to get the aggregate effect of the program, which then allows us to directly compare the overall effectiveness of Classic and Express. We further use these estimates to conduct a simple cost-effectiveness analysis.

First, we estimate specification (1), which gives us a measure of the average weekly effect of each program. Table 8 lists the estimated coefficients from this specification. Solarize Express appears to have a substantially larger average weekly treatment effect (t-stat = 1.084), although the total effect of the Classic campaign per town is larger since the program is 22 rather than 12 weeks (including the extra two weeks we allocate to the campaigns for the analysis). A joint test of differences in the means fails to reject the null that the total effects for the two programs are equal (t-stat = 0.778), although this may not be surprising given that we cluster our standard errors at the town level. A one-sided t-test rejects the null hypothesis that Express was less effective per week with the p-val = 0.218 and another one-sided t-test rejects the null hypothesis that Express was less effective in aggregate with the p-val = 0.139. These findings support Hypotheses 1 in that shortening the deal duration increases the adoption rate, but lowers overall adoption. While the differences between the two types of campaigns are not statistically significant at traditional levels due to the small sample size and clustered standard errors, the differences are economically significant.

We then estimate specification (2). The week-in-campaign α_t^c and α_t^e coefficients are identified based on the variation in the differences in trends between the treatment and control towns over time. Using these week-in-campaign coefficient estimates, we derive *weekly* marginal effects of the Classic and Express campaigns. Figure 6 displays these marginal effects, and the total effect is shown in Table 8 for easy comparison to specification (1) Using the first (second) approach, we estimate a total campaign treatment effect of 28 (27) installations per Classic town, and 22 (24) installations per Express town. Given that Newtown is an outlier due to the two-week campaign extension, we only show the effects for the remaining 10 Classic towns. There is a significant spike at the end of each program, while the earlier weeks feature mostly negligible campaign effects. This is consistent with the installation patterns observed in Figure 2. Turning to the data, the average number of Solarize installations is 34 in a Classic town and 31 in an Express town. Using the results from our second specification, this means that only six households (18%) in a Classic campaign and six households (19%) in an Express campaign who would have installed solar PV anyway even if there had not been a campaign. These are lifts of around 450%.

These findings demonstrate that Express and Classic are both effective in increasing the adoption of solar PV, and we find evidence that Express leads to a higher adoption rate, but that this increased rate does not fully compensate for the shorter duration of the campaign. In section 6, we will show that the mechanism behind the success differs across the campaign types by augmenting these analyses with the survey data.

5.2 Classic vs. Prime

We use the same specifications to estimate the effectiveness of the Prime campaign, which removes the group buy aspect of Solarize. Results for specification (1) are shown in Table 9. In sharp contrast to Hypothesis 4, Solarize Prime actually has a larger average treatment effect (although a joint test fails to reject the equality of the two program coefficients at p-value = 0.544). Using specification (1), we find that Classic led to approximately 30 additional installations and Prime led to an additional 40 installations. These differences are very economically meaningful, and are possibly due to the reduction in price uncertainty. This is a surprising finding given that we would expect WOM to decline with the absence of group pricing.

Using specification (2) for the Classic/Prime comparison, we find that the aggregate effectiveness is the same: 30 installations for Classic and 40 for Prime. We plot the marginal weekly effects over time in Figure 7. In contrast to the Classic results, we see significant effects on adoption even in the very first weeks of the Prime campaigns. This is exactly what we saw in the raw data as well. Participants in the Prime campaigns are not waiting until the end of the campaign to commit to installing solar. Hypothesis 5 predicts that group pricing campaigns where WOM travels from extrinsically motivated peers, such as the Classic campaigns, will be less effective without the group pricing (exactly what the Prime campaigns are). We test whether the predicted number of adoptions is higher in the first half of the Prime campaign (15.41, s.e. = 6.45) relative to the Express Campaigns (6.96, s.e. = 1.98) and find that it is significantly higher (at 10%) using a one-side t-test.

To further test the assumption that the Prime campaign leads to a different pattern of adoption over time, we examine another specification in which we replace the non-parametric treatment effect function in (2) with a quadratic function of the week-in-campaign. We find that Classic exhibits a positive intercept (p = 0.027), a negative week-in campaign linear time trend (p = 0.054) with a positive quadratic term (p=0.001) whereas Prime exhibits a large, positive but imprecisely estimated linear term (p=0.364) and no quadratic term (p=0.967). We predict that the linear term will be higher in Prime and the quadratic term will be higher in Classic, and find this is the case for both using a one-sided t-test (at 10%).

This supplemental analysis again demonstrates how the diffusion process varies considerably by campaign type.[7] We provide evidence suggesting that this is due to the different role of WOM in Section 6.

The observed average number of installations for a R5 Classic and Prime campaign are 46.7 and 59.0. Using the estimates from specification 2, this implies that 35.6% and 31.7% of installations would have occurred without the campaign. These estimates of free-ridership are higher than in R3 but are not surprising since the baseline adoption rates have increased over time. This still means that the Solarize campaigns led to a lift of about 200% and no significant difference in the number of installations due to the Solarize Classic program between R3 and R5.

5.3 Robustness Checks

We perform several robustness checks. To begin with, we also use the wild cluster bootstrap from Cameron, Gelbach, and Miller (2008) in order to identify confidence intervals and we find that the results of our hypothesis tests are identical. We also try alternative specifications for the control group since one might be concerned that the control groups used to estimate the treatment effects are systematically different than the treatment groups. Although we showed above that several observable demographic variables are similar, we can further explore the implications of different control groups to confirm that our results are robust.

One approach would be to use propensity score matching in order to match each treated town with its two most similar towns from the control group. For the matching, we use cumulative installations in the pre-treatment period, as well as the following demographic variables: population density, median household income, ethnicity, education level, unemployment, voting registration, and housing units. We show the cumulative pre-trends for the treatment and matched control groups, as well as our estimation results using propensity score matching in Appendix A. As expected, the pre-trends for the treatment and control group are quite similar under the propensity score matching approach. Moreover, both the qualitative and quantitative results of the propensity score matching estimation are very similar to our primary results above. We find no significant difference in the weekly effectiveness of Express in comparison to Classic, and again find suggestive evidence that Prime may be the most effective.

To further examine robustness to our choice of control group, we perform another estimation using a set of towns that ran a Solarize program after R5 as a possible control, since

[7] These results are available upon request.

these towns applied for a Solarize campaign just like the towns in R3 and R5. This new set of towns can be called the Round 6 (R6) towns. We compared the treatment towns during the treatment and pre-period to the R6 towns during the same period (before the R6 towns were treated). The sample size is much smaller in this robustness check, but one could argue that the towns that later had a Solarize campaign are as similar as possible to those we are examining in this study. Pre-trends and results are shown in Appendix B. Again, we see no significant difference in the estimation results.

As a final robustness check, we use negative binomial regression to estimate the treatment effects in order to confirm robustness to our preferred choice of a linear model. These results are shown in Appendix C. Again, the marginal effects of each treatment are similar to those found in our main specification. Ultimately, the main focus of the paper is in the differences between the campaign types, and these are largely invariant to the choice of the control group.

6 Drivers of Campaign Effectiveness

In order to assess the drivers of campaign success, we estimate two different versions of (1) and (2) in which we interact dummy variables for different types of Solarize campaigns with the mean of the survey response variables and key demographic variables. Demographics are included to ensure that the effects of the survey variables on installations are not simply driven by differences in demographics. We do this once to explore how consumers found out about Solarize, and a second time to examine the importance of the different information channels for adoption. For these regressions, we include every Solarize Classic town over all five rounds in order to make use of all of the survey data available.

These specifications with interactions are as follows:

$$y_{it} = \alpha_t^c C_{it} + \alpha_t^e E_{it} + \alpha_t^p P_{it} + \sum_{k \in K} \beta_k x_{ik} S_{it} + \sum_{k \in K^{WOM}} \beta_k^e x_{ik} E_{it} + \sum_{k \in K^{WOM}} \beta_k^p x_{ik} P_{it} + \mu_i + \delta_\tau + \epsilon_{it}, \tag{3}$$

$$y_{it} = \alpha_t^c w_t C_{it} + \alpha_t^e w_t E_{it} + \alpha_t^p w_t P_{it} + \sum_{k \in K} \beta_k x_{ik} S_{it} + \sum_{k \in K^{WOM}} \beta_k^e x_{ik} E_{it} + \sum_{k \in K^{WOM}} \beta_k^p x_{ik} P_{it} + \mu_i + \delta_\tau + \epsilon_{it}. \tag{4}$$

In the above specifications, E_{it} is a dummy variable indicating the presence of an active Express campaign, P_{it} is a dummy variable indicating the presence of an active Prime campaign, and S_{it} is an indicator variable for whether any Solarize campaign is in progress.

Finally, $x_{ik} \in \mathbf{X}_i$ is an element of the K-dimensional vector of demographic and survey variables. These specifications allow us to obtain a better understanding of the mechanisms through which direct and offline WOM contributes to the increase in solar adoptions from a campaign.

One potential concern about these analyses could be endogeneity of survey responses. The surveys are performed post-campaign in order to study how the campaign fared, but since the surveys are conditional on the consumer installing solar, towns that are more successful are likely to mechanically have more respondents. This could create an issue if there is selection bias that differs across towns due to the differences in town performance. However, we have no reason to believe that the distribution of people who do install should vary much. But more importantly, even if there was a selection issue due to some campaigns being more successful than others, these time-invariant differences across towns would be captured by the town fixed effects. Thus, our specification explicitly addresses this possible concern.

6.1 The Use of Different Information Channels

In the specification examining how consumers heard about Solarize, we can test our hypothesis about the role of WOM by interacting the campaign-type dummy with variables for whether you heard about Solarize from an event, town leader, or friend/neighbor. Similarly, we allow for different effects for the importance of friends/neighbors, people in the town you interact with, and people you work with to have different effects for the "importance of information sources" regressions. Note that because we utilize survey responses from households who signed a contract to install solar PV rather than a larger pool of potential solar customers, by design all respondents have heard about the program.

Results for the regressions with the survey responses regarding how people heard about Solarize are shown in Table 10. In the first column we do not interact the campaign type dummy with week-in-campaign, while in the second column we do. The results indicate that Solarize campaigns are more effective in towns with younger populations. In addition, towns in which consumers were more likely to hear about Solarize through other solar customers or newspapers were more effective, underscoring the importance of WOM and earned media.

The interactions between the Express or Prime dummies and the WOM variables shown in Table 10 indicate no difference in the effectiveness of any one information channel for Express towns relative to Classic towns, with the exception of the town leader. This means that Express towns in which people heard about the campaign from a town leader were more effective than those in which they heard about Express from other information sources,

relative to the negligible effect for Classic towns. This indicates that Express towns with more active town leaders were more effective in spurring solar adoption, perhaps because it is these leaders who seed the WOM. The increased effect of town leaders on installation decisions in Express towns may be due to the increased time pressure–in Classic programs, even towns with less engaged leaders have more time for WOM to operate. In towns in which WOM (excepting the town leaders) was the way people heard about the program, we see no disparity in its effectiveness between Classic and Express campaigns. However, as was seen in Table 3, the number of people listing the WOM channels as the way they heard about Solarize is lower in Express towns.

In contrast, our Hypothesis 6 suggested that the information transferred via WOM channels would differ in the Prime campaign because the extrinsic motivation for people to talk to others about solar is lessened for the peers for whom group pricing is relevant, namely others in the town. As expected, we see that Prime campaigns in which people are more likely to hear about Solarize through friends/neighbors are less effective relative to the Classic campaigns; adopters do not have the same pecuniary incentive to convince others in the town to adopt without group pricing. Our findings also suggest that town leaders can actually help compensate for this lower effectiveness, perhaps because the effect of hearing about the campaign through a town leader is even higher for the Prime towns.

6.2 The Importance of Different Information Channels

In the previous subsection, we studied how the channel through which people heard about Solarize campaigns affected adoptions. In this subsection, we examine how peoples' assessment of the importance of different information channels affects adoptions. Table 11 includes the results from our regressions assessing the influence of these importance ratings on the effectiveness of Solarize campaigns. Specifically, we interact the variables from the bottom panels of Tables 3 and 4 with the Solarize campaign. The second column includes the week-in-campaign dummy and campaign interactions. The WOM variables of interest are the importance of information from a friend/neighbor, someone in the town, and someone you work with.

Again, we see that Solarize is more effective in towns with younger populations and that towns that rated the ambassador as more important were also more successful, further underscoring the importance of the ambassadors in increasing solar adoption. This helps explain why Express campaigns still were effective in inducing solar adoptions–despite respondents saying that friends/neighbors were less important information sources in these shortened campaigns, they rated the ambassadors as being even more important than in the Classic

campaigns, suggesting that the ambassadors helped to take on some of the role that peers had taken before.

We see no difference in the effectiveness of the three main WOM variables for Express campaigns. This is as expected–there is no reason that WOM would be less effective in Express campaigns because the motivation behind WOM is exactly the same. In contrast, when we look at the interactions for the Prime campaign in Table 11, we see that Prime towns in which people placed higher importance on recommendations from people in the town had fewer adoptions from the campaigns. This is exactly as predicted because without group pricing, individuals would not have the same pecuniary incentive to try to convince others to adopt solar (Hypothesis 6). What we did not predict is the effect from the other WOM variables. Towns that placed more importance on friends/neighbors and people you work (the latter is only significant at 10% in the first regression) were even more effective in Prime campaigns than Classic campaigns. It appears that the weakening of one WOM channel through the elimination of the group pricing led to a greater effectiveness of WOM through other channels.

7 Discussion

7.1 Campaign Differences

In testing which campaigns are more effective, we find no statistically significant differences across campaign types due to the relatively large standard errors that result from clustering at the town level with the relatively small number of towns in the Express and Prime treatments. This is a natural consequence of how costly a single campaign is to run and the need to randomize at the community level in order to study how campaigns leverage WOM.

That said, the results are very suggestive that Express is more effective per week, but less effective overall relative to the Classic program, in accordance with Hypothesis 1. The differences in the effectiveness are economically meaningful. Furthermore, we find that the WOM channels in Express are significantly less likely to be the channels through which customers heard about Solarize (Table 3), indicating that the mechanisms underlying the total estimated treatment effects are different for the two campaign types (Hypothesis 2). WOM did not have as much time to operate in the Express campaigns. The summary statistics in the lower panel of Table 3 are also consistent with this story. The importance of recommendations from friends/neighbors is significantly greater in Classic towns, whereas different print and social media are listed as relatively more important sources of information in Express towns. Surprisingly, ambassadors are rated as less important in the Prime towns

and the people you interact with in the town and at work are rated as more important.

The differences in the survey responses provide strong support for Hypothesis 2, that WOM has less of a role to play in product diffusion in shorter campaigns. The regression results in section 6 provide evidence for Hypothesis 3–although fewer people report that they heard about Solarize through WOM channels and it was reported to be less important, for those who did hear about it in this way or for whom WOM is particularly important, we find no significant differences across Express and Classic campaigns of the effect these variables on adoption. We view this as intuitive because there in no difference in the motivation for WOM in the two treatments.

Surprisingly, we find no support for Hypothesis 4, that extrinsic WOM leads to more effective campaigns. Indeed, the results strongly suggest the opposite, that the Prime campaigns actually were more effective. This counterintuitive result can be explained be the regression results in Table 10 and Table 11. We did indeed find that WOM channels for those affected by groups pricing (other people in the town) were more effective in Classic relative to Prime, showing support for Hypotheses 5 and 6. However, other WOM channels (such as the ambassadors and friends) became more effective, possibly due to the simplification of the message and possibly due to the fact that potential adopters seek out information from peers regardless of whether group pricing increases the amount of a specific type of WOM. In support of this contention, we find that in the towns where people placed more importance on information from other people in the town, the Prime campaigns were less effective than Classic campaigns. Prime campaigns in which people heard about the campaign from town leaders were more effective relative to Classic, as shown in the information source regression results in Table 10. This could be because the town leaders were not extrinsically motivated in any of the campaigns without the group pricing. Another reason could be that the tiered pricing complicates the message, a hypotheses supported by interviews with town leaders and ambassadors.

7.2 Cost Effectiveness

In this section, we examine what our results imply for the cost-effectiveness of reducing the length of the Solarize programs. We first break down the total costs of the program into a fixed component that 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 kick-off 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.

After deducting the fixed costs, the total variable costs per campaign are approximately $33,333 for Classic, $33,500 for Express, and $35,000 for Prime.[8] We proceed to convert these cost figures into a dollar-per-contract measure of the cost-effectiveness of each campaign. For the Classic/Express comparison, our estimates in Table 8 suggest that the Solarize program led to a total of 27 additional installations for the Classic towns and 24 additional installations for the Express towns. This implies an average cost of approximately $1,225 per additional contract of the Classic program and $1,385 per contract for Express. Hence, while total campaign costs for the average participant are roughly the same for Classic and Express, the lower campaign effectiveness in terms of total induced installations leads to lower cost-effectiveness for Express. A likely factor that may have contributed to the lower effectiveness of Express is the fact that the holiday season occurred halfway through the campaign, which may have impacted the 12-week campaign more than the longer Classic campaign.

We conduct a similar cost-effectiveness analysis for the Classic/Prime comparison. Based on our treatment effect estimates, Solarize Prime induced a larger number of new installations per participating town. On the other hand, as discussed above, average campaign costs per town are similar for Prime and Classic (although Prime campaigns are logistically easier to administer since they do not involve real-time calculation of the pricing tier for each town). Hence, Solarize Prime appears significantly more cost-effective than Classic–the implied average cost per contract ($870) during Prime is less than 80 percent of the cost of Solarize Classic ($1,110).

It should be noted that the installers we have worked with throughout the Solarize program place customer acquisition costs in the range of $2,000 to $3,000, so all versions of the Solarize campaigns are more efficient relative to installers marketing without a campaign. Of course, solar ambassadors are not compensated for their time and one of the benefits of

[8]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 did a separate campaign.

a campaign such as Solarize is its use of volunteer third parties to spread information.

7.3 Managerial Implications

Beyond the substantive findings in the domain, we believe that these results have more generalizable managerial implications. Group pricing has become an increasingly prevalent way to spur consumer demand and it aims to leverage the extrinsic motivation it creates to increase WOM. We find that the duration of the period over which the group pricing deal has effect can be critical if WOM is to be successfully leveraged. Furthermore, the findings from our second experiment show that in some product categories, this additional motivation behind WOM can undermine the effectiveness of other operating WOM channels. We found that when we "turned off" group pricing, and therefore the extrinsic motivation to talk about solar PV, the effectiveness of WOM through friends and coworkers increased.

These results imply that marketers need to be aware of the current level and type of WOM that is already prevalent in the product category before making the decision whether to use group pricing as an additional driver of WOM in order to increase sales. Solar PV is a category in which researchers have demonstrated a significant role of WOM. Including group pricing may complicate the message and information that consumers are already sharing with each other, and the change in motivation may even undermine the message. In such situations, campaigns such as Solarize Prime may be very successful because they help increase the level of WOM through other marketing tools rather than group pricing. If group pricing is used to help spur WOM and sales, marketers need to be aware of the effect of the deal duration on WOM, for the appropriate duration of the deal will depend on the speed with which WOM can diffuse in that context.

8 Concluding Remarks

In light of the evidence in the literature indicating that shorter deal durations could increase the likelihood of purchase, this paper employs a field experiment to test the effect of reducing the length of the "Solarize CT" program on residential PV adoption. Specifically, we compare the effectiveness and cost-effectiveness of a 20-week Solarize Classic program vs. a 12-week Solarize Express program by randomly assigning towns to each of the programs. We compare these two randomly-assigned treatments to a control group of CT towns and employ a difference-in-differences approach to estimate and compare the treatment effects. Our results suggest that both programs led to very significant lifts in adoption, although the Express campaign was just as costly to run despite its shorter duration due to the extra effort required

to run the campaign successfully. We explore the interplay between campaign duration and the leading mechanisms through which information reaches solar adopters and find that WOM is a larger driver of success in Solarize Classic than in the shorter Express campaigns.

We also study the role of group pricing in driving the amount of WOM and the effectiveness of WOM using a field experiment in which we "turn off" group pricing for a randomly selected set of towns. We find that the Prime campaigns without group pricing are just as effective or more effective than the Classic campaigns. In the Prime campaigns, we find that WOM from people in the town is less effective, likely because they have less of an incentive to promote solar without group pricing. In contrast, WOM from friends and coworkers is actually more effective. This implies that WOM does not operate through a single channel, and that WOM though one channel can be substituted for by WOM through an alternative channel. We also find that the solar ambassadors are more effective in the Prime campaigns, likely due to their ability to simplify the message in the absence of group pricing.

The main limitation of this paper is the small number of towns we are able to experimentally assign due to logistical and cost considerations.[9] This is not uncommon in the development economics literature, in which entire communities must be randomly assigned as a unit, rather than randomization occurring at the household unit. Such randomization is necessary given the desired object of study, namely peer influence among those in the community. That said, our findings are very robust to alternative specifications, and the very large lift that results from the campaigns leads us to estimate statistically significant results, even with the smaller sample size and after clustering standard errors at the town level. The combination of the large-scale field experiment with the extensive survey data allows us to examine the WOM mechanisms behind durable good adoption. This is the first paper we are aware of that alters the components of such marketing campaigns in order to examine the effect of deal duration and group pricing on not just the output measure of solar adoptions, but also of one of the key underlying mechanisms, namely WOM.

In summary, deal duration and group pricing both have implications for the usefulness of WOM in the diffusion of new products. We find that deals need to be of long-enough duration for WOM to operate if this mechanism is going to be leveraged to increase adoptions, and that while group pricing may spur WOM, it can also complicates the message and may not be necessary if WOM can operate through other channels.

[9] The cost of the 27 towns in R3 and R5 alone exceeded $800,000.

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Figure 1: Examples of Solarize Presence in the Community

Figure 2: Weekly Residential PV Contracts Signed in Solarize R3 Towns

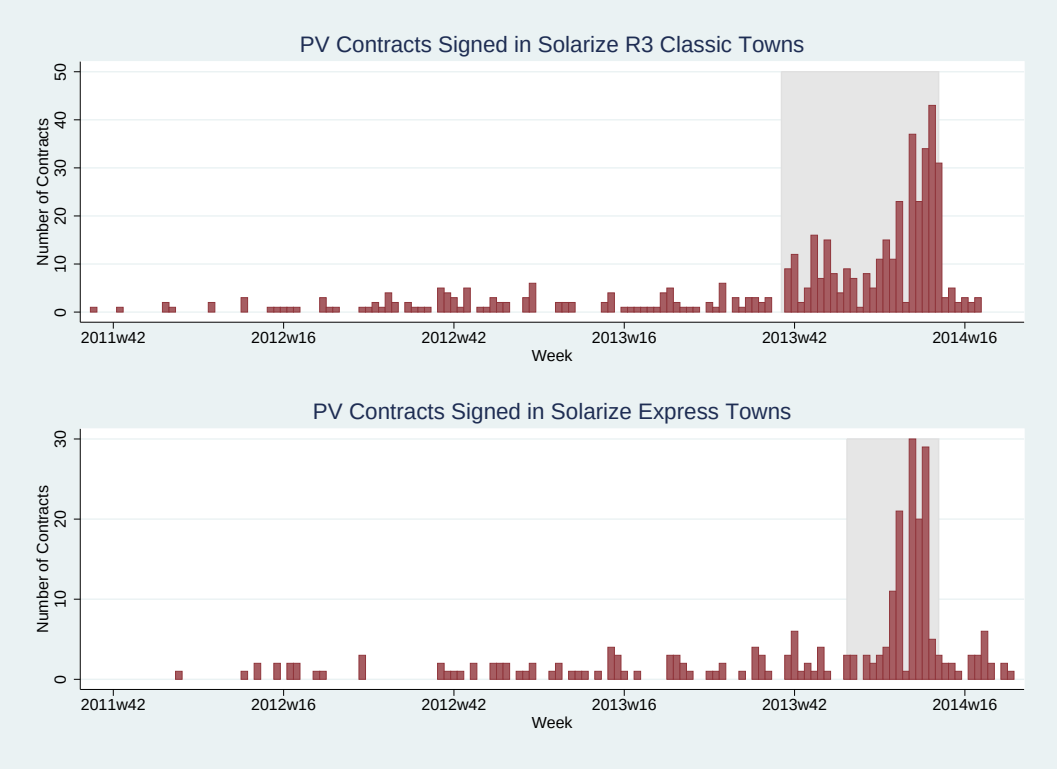

Figure 3: Weekly Residential PV Contracts Signed in Solarize R5 Towns

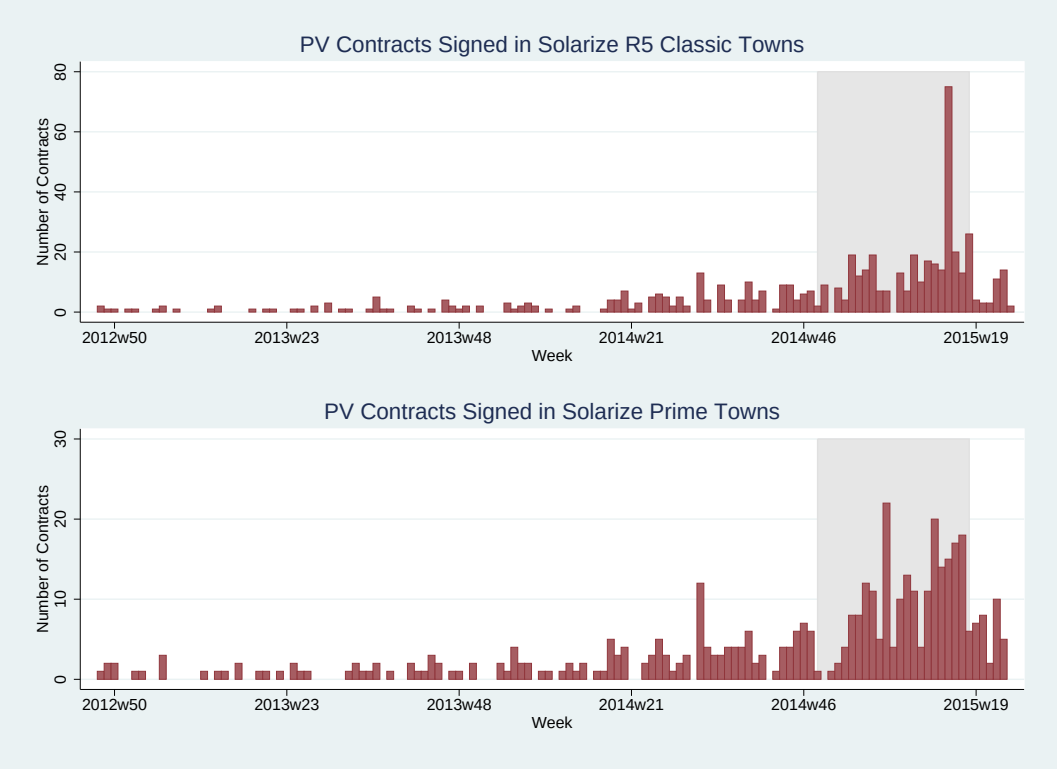

Figure 4: Cumulative Number of Contracts in R3

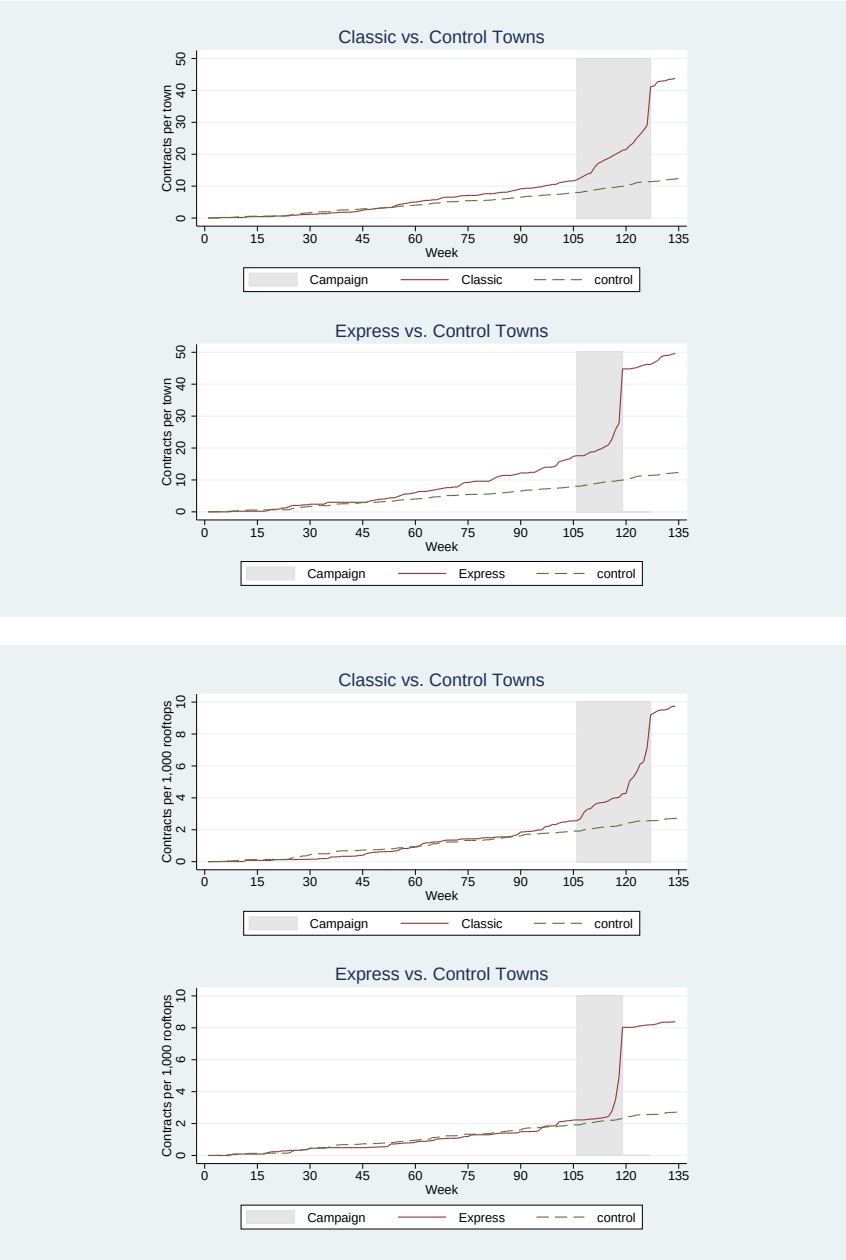

Figure 5: Cumulative Number of Contracts in R5

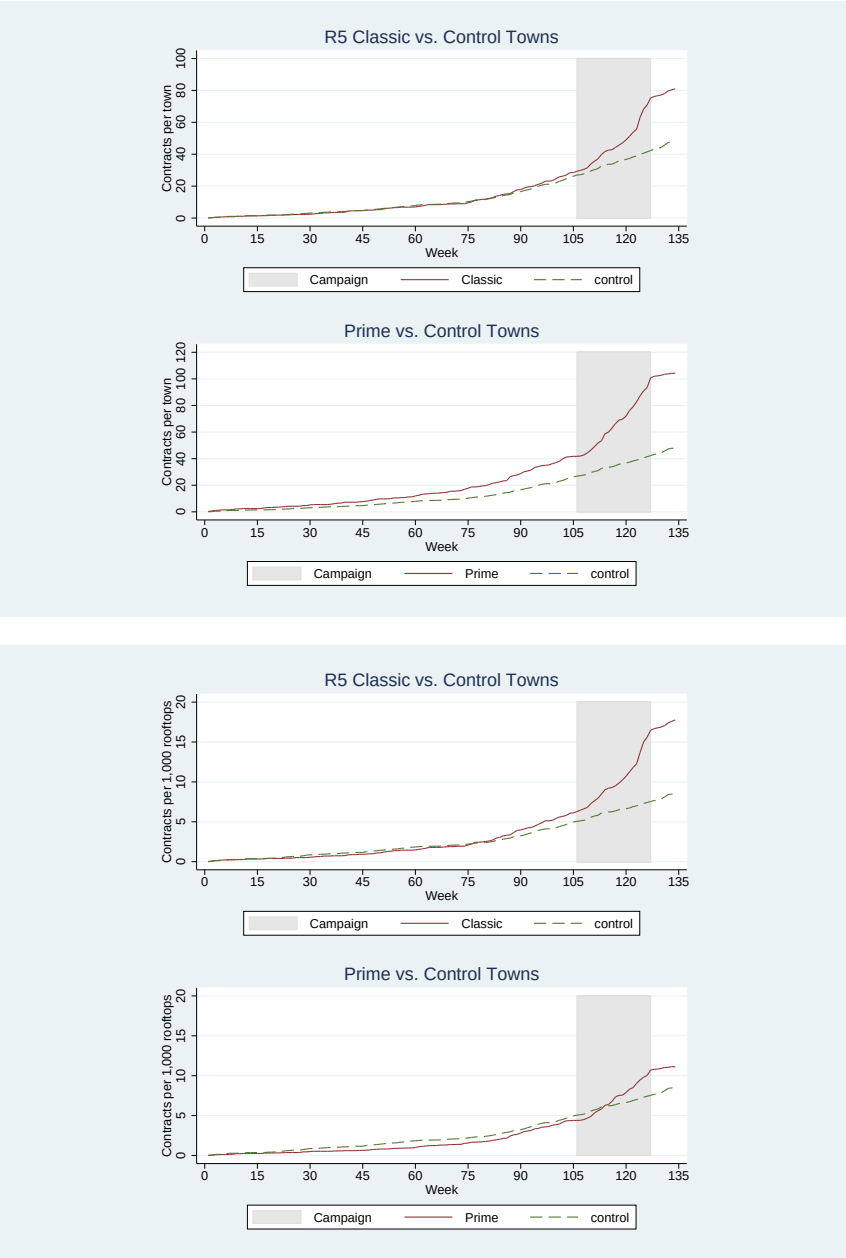

Figure 6: Weekly Treatment Effects of Solarize R3 Classic and Express over Time

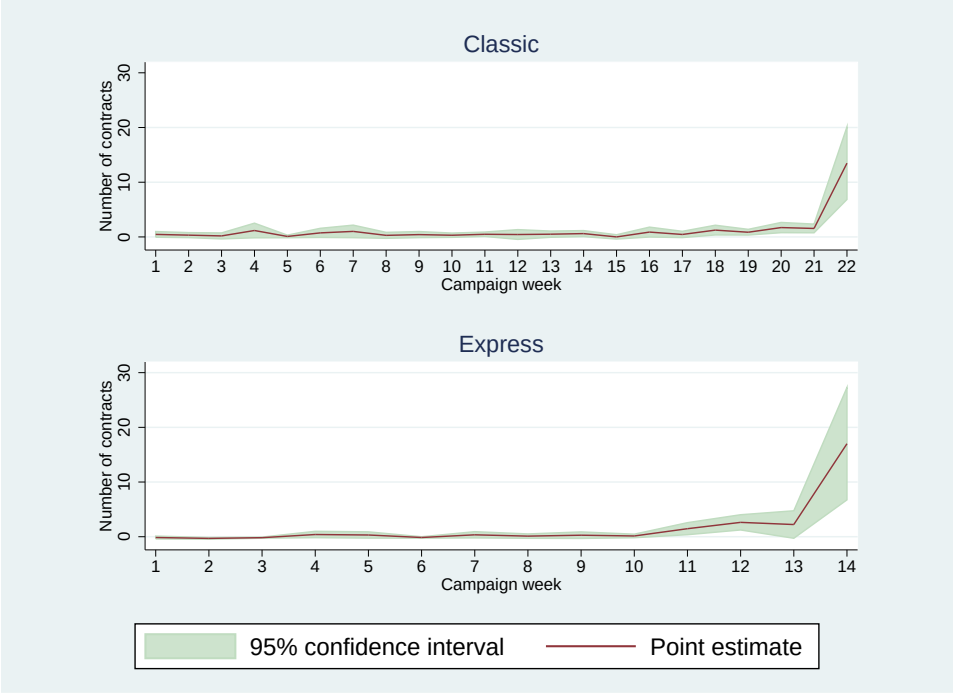

Figure 7: Weekly Treatment Effects of Solarize R5 Classic and Prime over Time

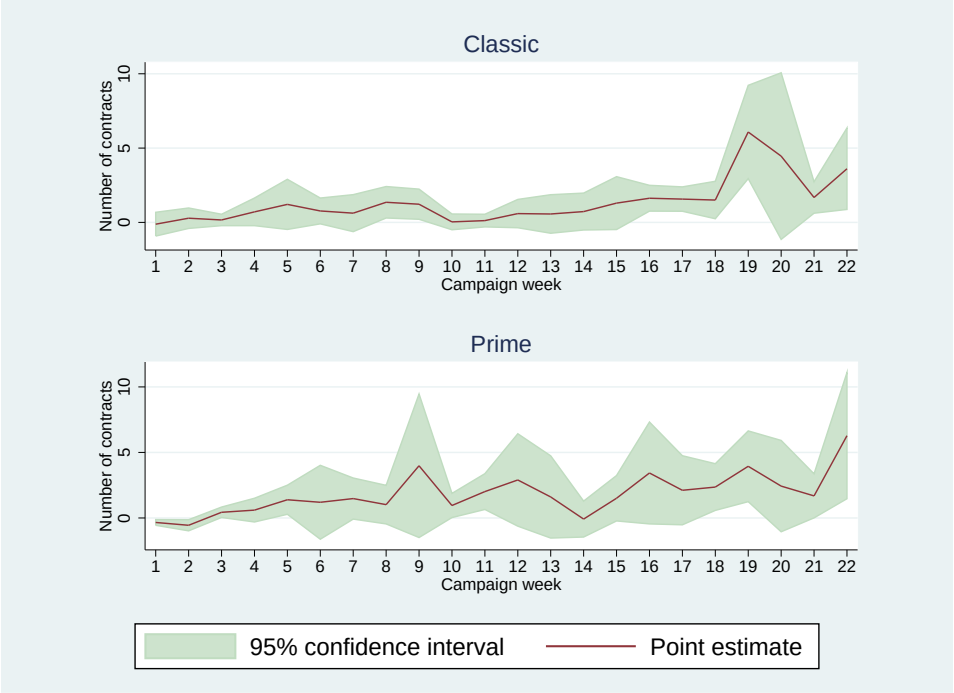

Table 1: Solarize R3 Campaign Dates

Classic towns			Express towns		
TOWN	Start date	End date	TOWN	Start date	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 20 weeks long, while the campaigns in all Express towns last approximately 12 weeks.

Table 2: Solarize R5 Campaign Dates

Classic towns			Prime towns		
TOWN	Start date	End date	TOWN	Start date	End date
Burlington	11/19/2014	4/9/2015	Avon	11/20/2014	4/10/2015
East Granby	12/2/2014	4/22/2015	Griswold	12/8/2014	4/28/2015
New Canaan	12/2/2014	4/22/2015	Milford	12/3/2014	4/23/2015
New Hartford	11/17/2014	4/7/2015	Southbury	11/19/2014	4/9/2015
Suffield	12/2/2014	4/22/2015			
Windsor	12/2/2014	4/22/2015			
Windsor Locks	12/2/2014	4/22/2015			

Table 3: Summary Statistics of Survey Responses: R3 Classic vs. Express

WOM Channels						
	R3 Classic towns			R3 Express towns		
VARIABLE	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Friend/neighbor	460	0.135**	0.342	181	0.077**	0.268
Town leader	460	0.167	0.374	181	0.166	0.373
Solar customer	460	0.089**	0.285	181	0.039**	0.193
Newspaper	460	0.130**	0.337	181	0.199**	0.400
Social Media	460	0.039	0.194	181	0.061	0.240
Online media	460	0.126**	0.332	181	0.066**	0.249
Solarize event	460	0.215**	0.411	181	0.309**	0.464
Installer	460	0.052	0.223	181	0.050	0.218
Importance of Information Sources						
	R3 Classic towns			R3 Express towns		
VARIABLE	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Friend/neighbor	137	3.431	2.165	67	3.224	1.976
Social media	134	1.903**	1.381	66	2.394*	1.568
Installer website	134	3.649	1.901	66	3.576	1.789
Solarize ambassador	135	3.896	2.155	70	4.157	1.893
Someone in town	135	3.444	2.188	67	3.463	1.894
Someone you work with	134	2.769	1.911	66	3.015	1.893
Print media	136	3.301**	1.906	69	3.855**	1.825
Seeing solar	138	4.188	1.862	67	3.866	1.841
Num. of responses per coalition	6	76.667	33.685	4	45.25	22.396

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

Table 4: Summary Statistics of Survey Responses: R5 Classic vs. Prime

How did you hear about Solarize?						
	R5 Classic towns			R5 Prime towns		
VARIABLE	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Friend/neighbor	189	0.127	0.334	149	0.121	0.327
Town leader	189	0.280	0.450	149	0.208	0.407
Solar customer	189	0.069	0.254	149	0.047	0.212
Newspaper	189	0.148	0.356	149	0.128	0.335
Social Media	189	0.053	0.224	149	0.054	0.226
Online media	189	0.069	0.254	149	0.087	0.283
Solarize event	189	0.323	0.469	149	0.315	0.466
Installer	189	0.032***	0.176	149	0.101***	0.302
Importance of Information Sources						
	R5 Classic towns			R5 Prime towns		
VARIABLE	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Friend/neighbor	97	3.546	2.180	68	3.456	2.062
Social media	96	1.917	1.374	67	2.090	1.535
Installer website	98	3.459	2.062	70	3.543	2.062
Solarize ambassador	97	3.887	1.978	70	3.500	2.185
Someone in town	97	3.258	2.103	70	3.657	2.132
Someone you work with	99	2.667*	1.954	66	3.197*	1.971
Print media	95	3.021	1.851	66	3.121	1.714
Seeing solar	99	4.020	1.932	68	4.279	1.811
Num. of responses per coalition	4	47.27	29.341	4	37.25	22.765

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

Table 5: Summary Statistics of Survey Responses: All Classic Towns

WOM Channels			
VARIABLE	Obs.	Mean	Std. Dev.
Friend/neighbor	1375	0.139	0.346
Town leader	1375	0.204	0.403
Solar customer	1375	0.092	0.290
Newspaper	1375	0.143	0.350
Social Media	1375	0.045	0.208
Online media	1375	0.102	0.303
Solarize event	1375	0.253	0.435
Installer	1375	0.051	0.220
Importance of Information Sources			
VARIABLE	Obs.	Mean	Std. Dev.
Friend/neighbor	524	3.586	2.173
Social media	518	2.037	1.476
Installer website	496	3.484	1.938
Solarize ambassador	524	4.025	2.042
Someone in town	519	3.663	2.142
Someone you work with	515	2.790	1.921
Print media	519	3.422	1.936
Seeing solar	528	4.131	1.947
Num. of responses per coalition	24	57.292	28.196

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

Table 6: Solarize R3: 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
Number of towns	11		5		40	

Note: Voting data are collected from the Office of the Secretary of State. All other data come from the 2009-2013 wave of the American Community Survey. 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. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 7: Solarize R5: Balance of Covariates

	Classic		Prime		control	
VARIABLE	Mean	St. Dev.	Mean	St. Dev	Mean	St. Dev
Population density	597.1	399.1	919.7	800.8	781.4	878.0
Median income	94817	32321	82190	23913	80245	24076
% White	0.856	0.154	0.912	0.022	0.908	0.096
% college degree	0.522**	0.038	0.471	0.067	0.483	0.043
% unemployed	0.066	0.017	0.079	0.020	0.082	0.025
% Democrat voters	0.295	0.087	0.279	0.049	0.316	0.073
% occupied units	5118	2933	10030	6903	6073	5667
Number of towns	7		4		40	

Note: Voting data are collected from the Office of the Secretary of State. All other data come from the 2009-2013 wave of the American Community Survey. 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. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 8: Average Treatment Effects of Classic and Express, R3

VARIABLE		
Classic	1.286***	
	(0.273)	
Express	1.842***	
	(0.434)	
Total of Classic	28.292	27.249
Total of Express	22.104	24.222
Town FE	yes	yes
Calendar week FE	yes	yes
Single TE dummy	yes	no
Week-in-campaign TE dummies	no	yes
Observations	7,824	7,824

Note: Dependent variable is the weekly number of signed solar contracts. Unit of observation is town-week. Robust standard errors clustered at the town level in parentheses (56 clusters). We also use the wild cluster bootstrap from Cameron et al. (2008) and we find that the results of our hypothesis tests are identical. $p < 0.001$ (***).

Table 9: Average Treatment Effects of Classic and Prime, R5

VARIABLE	**(1)**	**(2)**
Classic	1.377***	
	(0.307)	
Prime	1.835**	
	(0.706)	
Total Effect of Classic	30.294	30.078
Total Effect of Prime	40.370	40.345
Town FE	yes	yes
Calendar week FE	yes	yes
Single TE dummy	yes	no
Week-in-campaign TE dummies	no	yes
Observations	6,834	6,834

Note: Dependent variable is the weekly number of signed solar contracts. Unit of observation is town-week. Robust standard errors clustered at the town level in parentheses (51 clusters). We also use the wild cluster bootstrap from Cameron et al. (2008) and we find that the results of our hypothesis tests are similar, although Prime becomes statistically insignificant. $p < 0.05$ (**), $p < 0.01$ (***).

Table 10: Regression Output: How heard about Solarize

	(1)	(2)
solarize x age	-0.163***	-0.109***
	(0.039)	(0.037)
solarize x income (1000s)	0.002	0.001
	(0.005)	(0.005)
solarize x pop. density (1000/sq. mile)	-0.007	0.073
	(0.129)	(0.149)
solarize x perc. white	-0.001	0.010
	(0.009)	(0.010)
solarize x friend/neighbor	-3.977	-2.372
	(3.006)	(3.121)
solarize x town leader	-6.242*	-5.114
	(3.505)	(3.663)
solarize x solar customer	14.037***	10.307**
	(4.691)	(4.740)
solarize x newspaper	7.421*	10.776***
	(3.538)	(3.551)
solarize x installer	10.536	13.591
	(8.127)	(8.717)
solarize x social media	11.398*	2.699
	(6.081)	(6.237)
solarize x online media	-1.301	2.386
	(3.902)	(4.396)
solarize x solarize event	-2.083	-2.504
	(2.534)	(2.526)
express x friend/neighbor	-7.326*	7.699
	(3.916)	(8.517)
express x town leader	12.995***	14.320***
	(4.368)	(4.960)
express x solarize event	-4.946	1.749
	(4.117)	(4.235)
prime x friend/neighbor	-275.930***	-269.798***
	(47.889)	(48.886)
prime x town leader	166.227***	154.963***
	(30.758)	(30.704)
prime x solarize event	12.504	13.606
	(11.102)	(10.904)
R-squared	0.230	0.624
Observations	18,012	18,012
Town FE	yes	yes
Calendar week FE	yes	yes
Campaign dummy	yes	no
Week-in-campaign FE x campaign dummy	no	yes

Note: Dependent variable is the weekly number of signed solar contracts. Unit of observation is town-week. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Table 11: Regression Output: Importance of Information Source

	(1)	(2)
solarize x age	-0.170***	-0.139***
	(0.050)	(0.047)
solarize x income (1000s)	0.009	0.007
	(0.009)	(0.009)
solarize x pop. density (1000/sq. mile)	-0.144	-0.149
	(0.265)	(0.270)
solarize x perc. white	0.007	0.008
	(0.018)	(0.019)
solarize x friend/neighbor	-0.244	-0.391
	(0.294)	(0.267)
solarize x social media	0.203	0.429
	(0.878)	(0.820)
solarize x solar ambassador	1.260**	1.284*
	(0.503)	(0.521)
solarize x someone in town	0.153	0.113
	(0.561)	(0.581)
solarize x someone work with	-0.796	-0.774
	(0.918)	(0.947)
solarize x print media	-0.010	-0.296
	(0.622)	(0.443)
solarize x installer website	-0.415	-0.444
	(0.640)	(0.656)
solarize x seeing solar	-0.104	0.247
	(0.590)	(0.413)
express x friend/neighbor	1.826	-0.844
	(2.774)	(1.190)
express x someone in town	-0.481	1.470
	(2.747)	(1.944)
express x someone work with	-3.585	0.985
	(4.193)	(1.907)
prime x friend/neighbor	3.303***	3.444***
	(0.810)	(0.775)
prime x someone in town	-4.871***	-4.887***
	(0.820)	(0.829)
prime x someone work with	2.199*	1.984
	(1.288)	(1.364)
R-squared	0.221	0.616
Observations	18,012	18,012
Town FE	yes	yes
Calendar week FE	yes	yes
Campaign dummy	yes	no
Week-in-campaign FE x campaign dummy	no	yes

Note: Dependent variable is the weekly number of signed solar contracts. Unit of observation is town-week. Robust standard errors are clustered at the town level. $p < 0.1$ (*), $p < 0.05$ (**), $p < 0.01$ (***).

Appendix

Appendix A Results with Propensity Score Matching

As a robustness check, we use an alternative set of control towns in the analysis. Instead of using Clean Energy Communities as the control, 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 2-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://www.ct.gov/sots).

Our approach is straightforward. First, for each Solarize program (Express, Prime, and two rounds of Classic), 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, described above. We then match, with replacement, each of the treated towns to the two control towns with closest propensity scores. As shown in Tables A1 and A2, this nearest two-neighbor (2N) matching procedure results in a balanced set of covariates across treatment and control groups. Furthermore, Figures A1 and A2 display the pre-treatment trends, which are very similar between each group of control and treated towns. Using the new sample, we re-run our analysis from Section 5. As seen in Figures A3 and A4 and Table A3, our results are very close to the ones obtained in the baseline analysis and are hardly affected by the use of a different set of control towns.

Figure A1: Cumulative Number of Contracts in R3

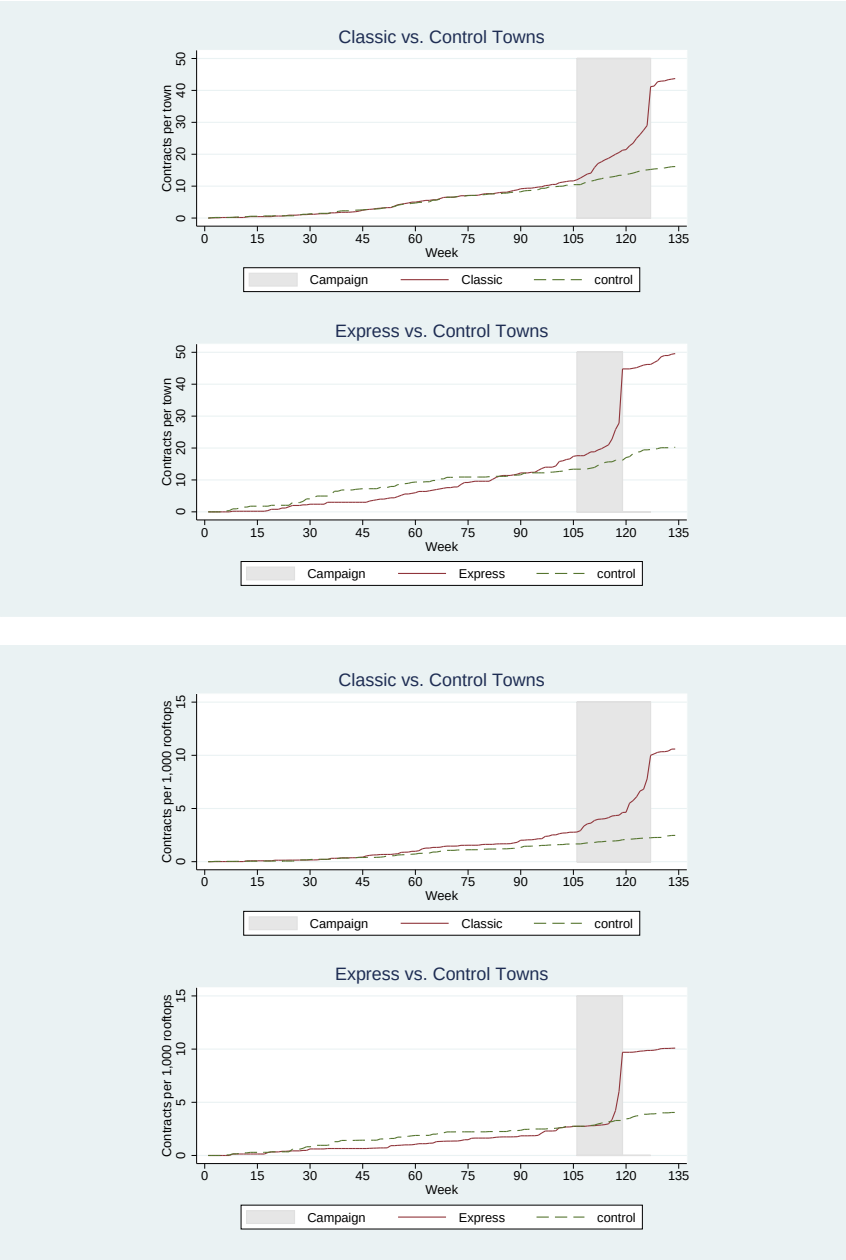

Figure A2: Cumulative Number of Contracts in R5

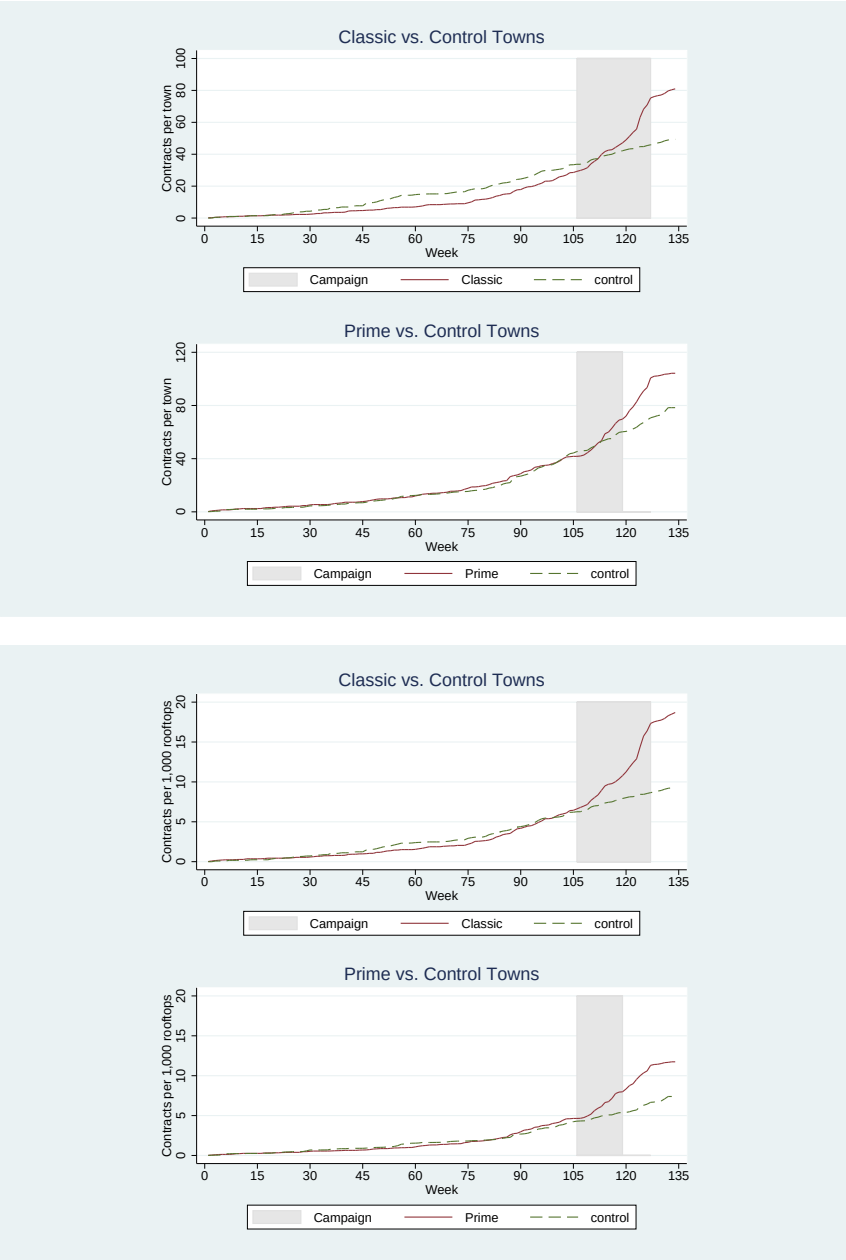

Figure A3: Weekly Treatment Effects in Solarize R3 with 2N Matching

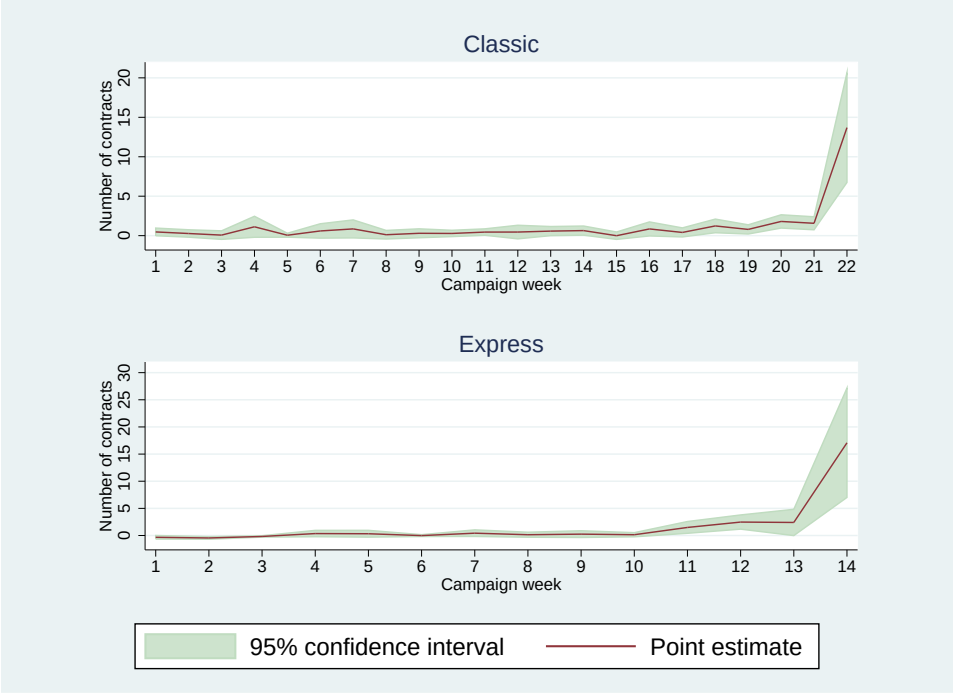

Figure A4: Weekly Treatment Effects in Solarize R5 with 2N Matching

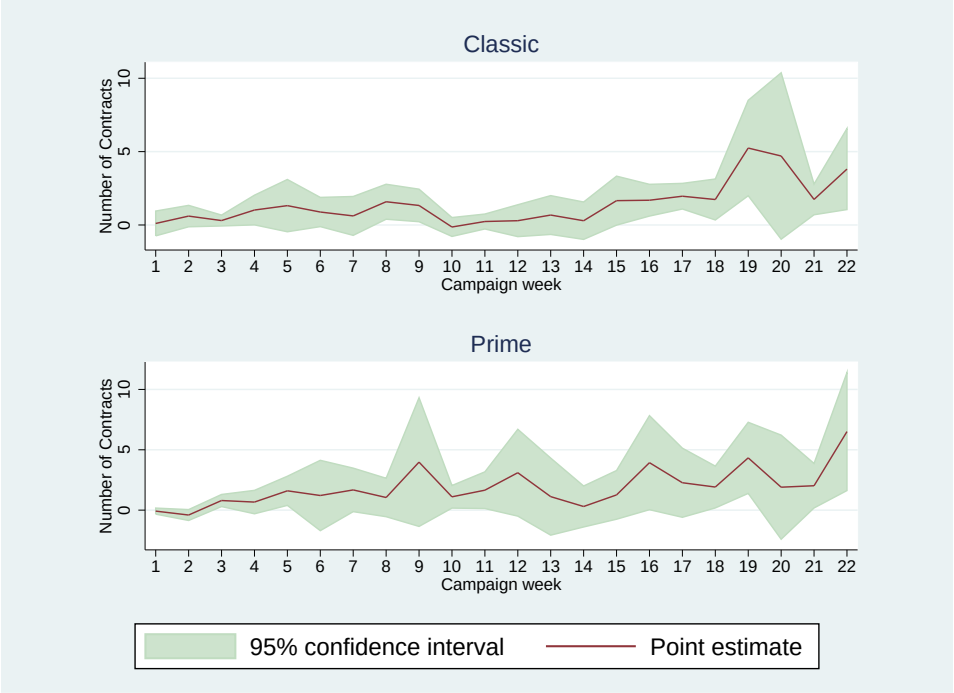

Table A1: Solarize R3: Balance of Covariates with 2N Matching

	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	977.5	986.0	578.3	746.5	349.8	317.9
Median income	97714	27868	90723	46343	81568	17717	91760	10913
% White	0.905	0.083	0.898	0.095	0.887	0.113	0.942	0.059
% college degree	0.480	0.041	0.472	0.070	0.493	0.050	0.523	0.029
% unemployed	0.083	0.022	0.085	0.026	0.070	0.022	0.072	0.018
% Democrat voters	0.309	0.070	0.331	0.080	0.330	0.090	0.299	0.012
% occupied units	9394	9866	11633	11099	8622	9375	5582	4344
Number of towns	11		20		5		6	

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 A2: Solarize R5: Balance of Covariates with 2N Matching

	Classic		Control for Classic		Prime		Control for Prime	
VARIABLE	Mean	St. Dev.	Mean	St. Dev	Mean	St. Dev	Mean	St. Dev
Population density	597.1	399.1	671.8	835.2	919.7	800.8	1007.3	789.9
Median income	94817	32321	94452	21971	82190	23913	89481	48744
% White	0.856	0.154	0.937	0.048	0.912	0.022	0.887	0.117
% college degree	0.522	0.038	0.521	0.054	0.471	0.067	0.467	0.045
% unemployed	0.066	0.017	0.053	0.022	0.079	0.020	0.083	0.014
% Democrat voters	0.295	0.087	0.281	0.042	0.279	0.049	0.308	0.092
% occupied units	5118	2933	5469	1775	10030	6903	12516	10616
Number of towns	7		7		4		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 A3: Treatment Effect per Town with 2N Matching

Treatment	Total effect
R3 Classic	26.6
Express	24.2
R5 Classic	31.7
Prime	41.9

Note: Total treatment effect is calculated as a sum of the weekly marginal effects in each program.

Appendix B Results with Current Solarize Towns as Controls

As another robustness check, 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 7 towns in Connecticut. These are the towns of 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 6 towns.

Figure B1: Cumulative Number of Contracts in R3

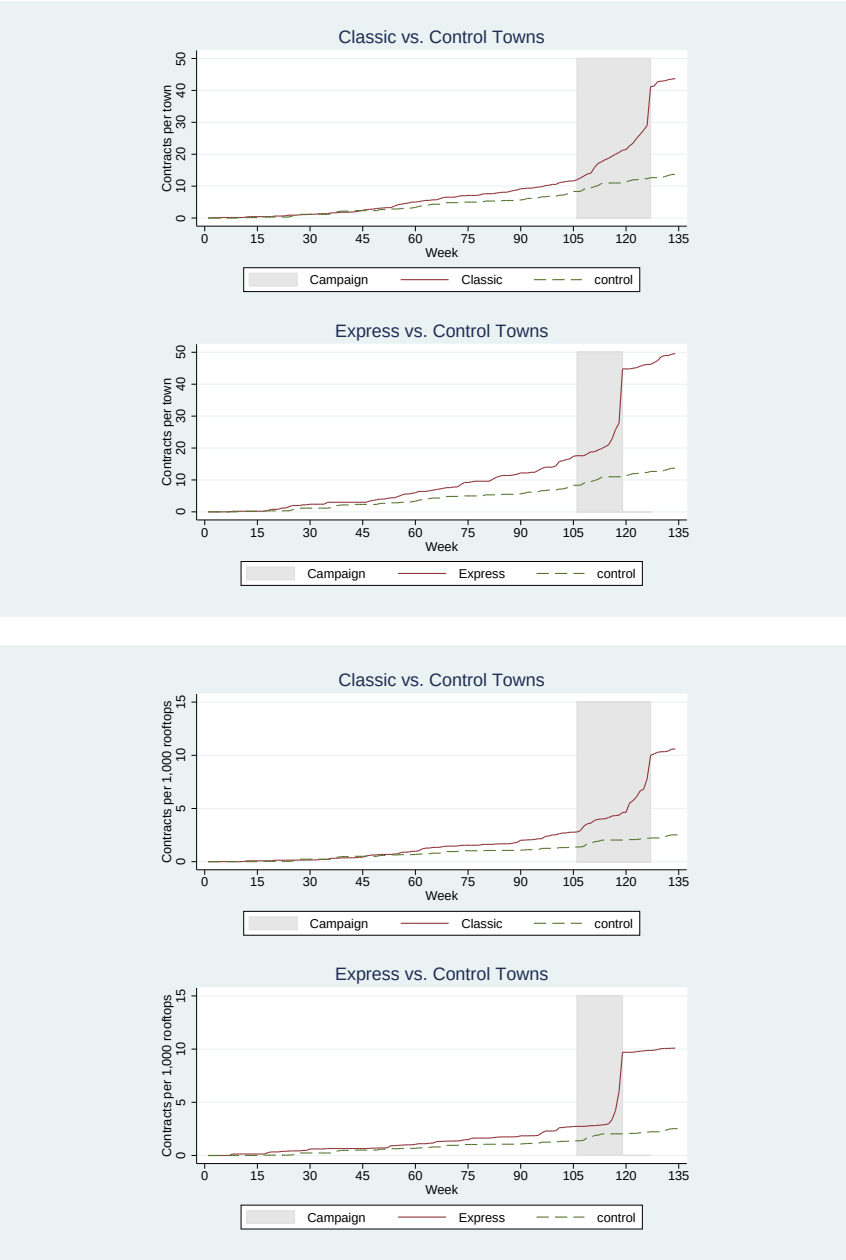

Figure B2: Cumulative Number of Contracts in R5

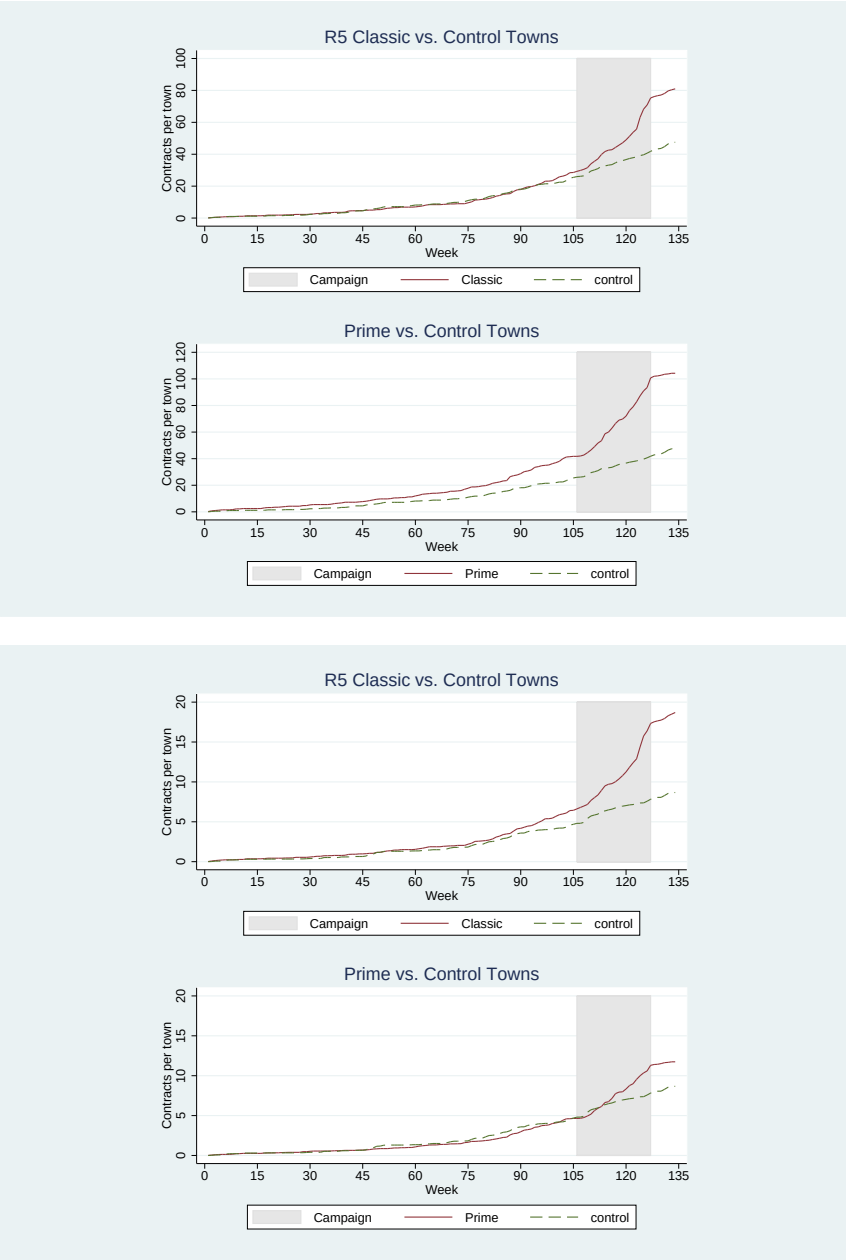

Figure B3: Weekly Treatment Effects in Solarize R3 with Current Solarize Control Towns

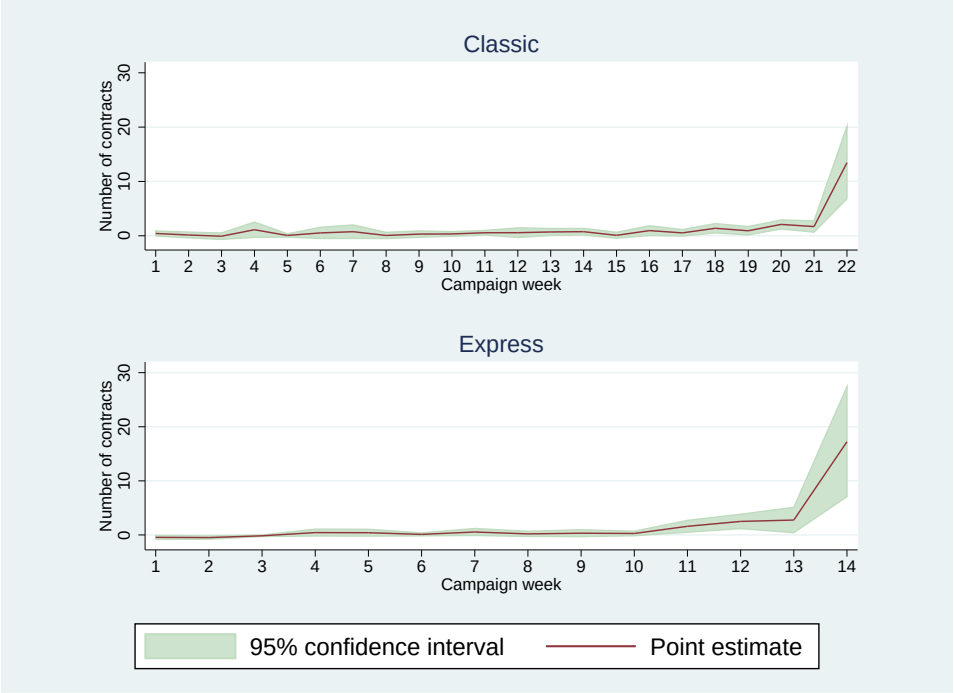

Figure B4: Weekly Treatment Effects in Solarize R5 with Current Solarize Control Towns

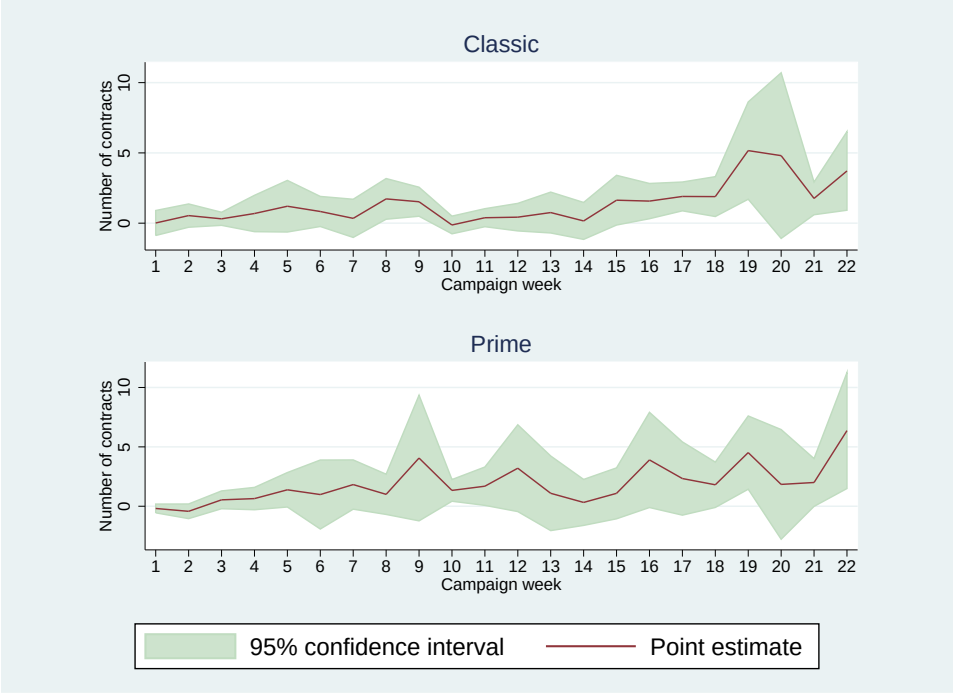

Table B1: Treatment Effect per Town with Current Solarize Control Towns

Treatment	Total effect
R3 Classic	27.3
Express	25.3
R5 Classic	31.2
Prime	41.3

Note: Total treatment effect is calculated as a sum of the weekly marginal effects in each program.

Appendix C Results using Negative Binomial

As a final robustness check, we use the original set of control towns (Clean Energy Communities, which have not yet participated in a Solarize program or in the CT Solar Challenge) and estimate a negative binomial specification. Figures C1 and C2 display the weekly marginal effects under this specification. The total estimated treatment effects are shown in Table C1.

Figure C1: Weekly Marginal Effects in Solarize R3 using Negative Binomial

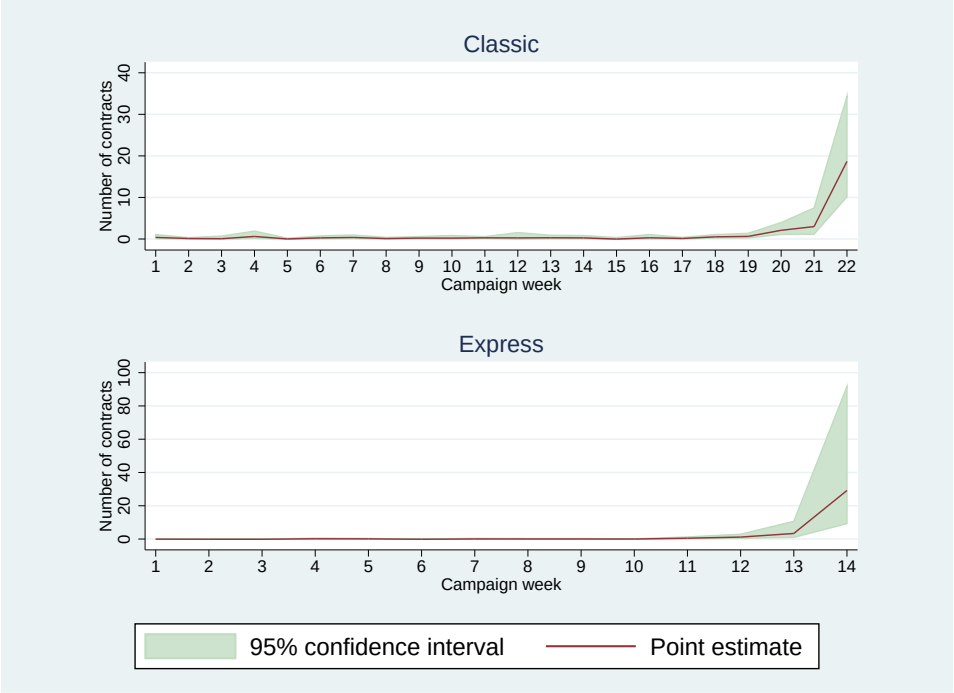

Figure C2: Weekly Marginal Effects in Solarize R5 using Negative Binomial

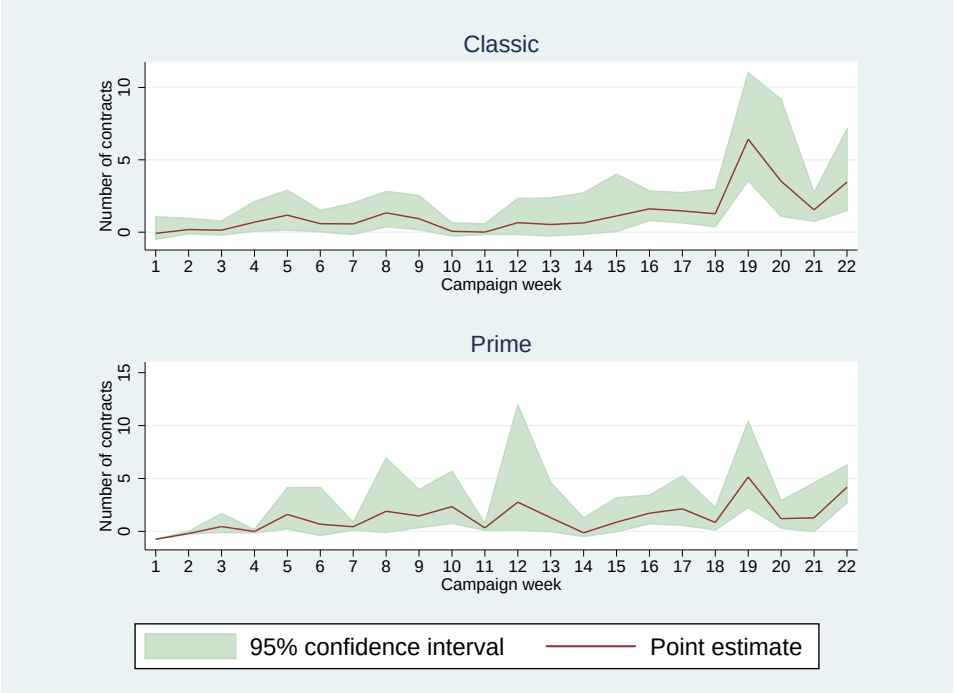

Table C1: Treatment Effect per Town using Negative Binomial

Treatment	Total effect
R3 Classic	28.9
Express	34.3
R5 Classic	27.9
Prime	29.6

Note: Total treatment effect is calculated as a sum of the weekly marginal effects in each program.