2012 Impact Evaluation of Southern California Edison's 10 For 10 Rebate Program

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ABSTRACT

In January 2012, Southern California Edison (SCE) closed the San Onofre Nuclear Generating Station. SCE defaulted non-residential customers in Orange County onto the 10/10 Rebate Program in summer 2012 to mitigate the potential for local power outages as a result of shutting down this large (2,200 MW) generation resource. Under this program, if an eligible non-residential customer in Orange County reduced summer usage by 10 percent or more relative to usage in summer 2011, the customer received a 10 percent summer bill credit. This paper summarizes the evaluation of the 10/10 Program.

The evaluation uses a matched control group from customers outside of Orange County, which comprises roughly one-sixth of SCE's non-residential population. The evaluation finds no statistically significant energy savings resulted from this program although almost $2.5 million was spent on rebates to approximately 20,000 customers whose usage in the 2012 summer was 10 percent lower than in 2011. The analysis suggests that these incentives were paid for fluctuations in energy use that were unrelated to the 10/10 Program.

SCE research in August 2012 found that 26 percent of 10/10 participants were aware of the program, which suggests that lack of awareness was not a key reason for the lack of energy savings. Even though a non-trivial portion of participants were aware of the program, there were not any measureable impacts. As such, this paper concludes that the program as designed is ineffective and does not encourage business customers to save energy.

Introduction

This report presents energy impact estimates for the 10/10 Program, which SCE offered to customers in summer 2012[1] in an effort to mitigate the potential for local power outages as a result of the closure of the San Onofre Nuclear Generating Station (SONGS). Under this program, if an eligible non-residential customer in Orange County reduced summer usage by 10 percent or more relative to usage in summer 2011, the customer received a 10 percent bill credit for that time period. The primary evaluation objective is to estimate the energy savings attributable to the 10/10 Program. Considering that many customers may have responded to 10/10 without fully achieving a usage reduction of 10 percent or more, the energy impact of the 10/10 Program is not limited to customers who received a rebate. Therefore, this evaluation estimates the impact of 10/10 on all eligible non-residential customers in Orange County. This approach required the development of a counterfactual—what usage would have been without the 10/10 Program—for the approximately 107,500 service accounts that were eligible for 10/10 rebates. Freeman, Sullivan & Co. (FSC) employed a quasi-experimental approach to develop this counterfactual, using selected SCE customers outside of Orange County as a matched control group. To develop the energy impact estimates, FSC compared changes in control group summer usage to changes in summer usage for 10/10 customers.

[1] In this paper, "summer" refers to July 1 through September 30.

Program Eligibility and Marketing

Eligibility for the 10/10 bill credit was based on daily usage and the credit was applied to the total bill.[2] The program was offered to bundled general service, agricultural and pumping customers who were not enrolled in any of SCE's demand response programs and had at least 12 months of service with SCE prior to July 1, 2012. Since the 10/10 Program rebate was only offered to business customers in Orange County, SCE conducted various targeted marketing efforts, including email to assigned customers;[3] telephone calls and in-person meetings with assigned customers; and direct mail to all eligible customers. SCE supplemented these targeted marketing efforts with general awareness campaigns in Orange County through: Partnerships with community and industry organizations; fact sheets and flyers in five languages for use at customer events and street canvassing; and a 10/10 Program Web page on sce.com. Overall, SCE research in August found that 26 percent of 10/10 participants were aware of the program. The 10/10 Program design and marketing offers a unique opportunity to estimate the impact of a program with relatively large incentives that was intensively marketed to specific customers, but likely required substantial behavior change to qualify for the rebate.

Model Development

To accurately identify customer response to the 10/10 Program, FSC used a difference-in-differences (DID) regression model with a control group. To ensure that the control group was as similar as possible to 10/10 customers, FSC applied the same criteria for program participation to control group customers as was required for 10/10 customers, with an obvious exception that the control group customers could be drawn from outside Orange County (considering that SCE's 10/10 Program marketing was concentrated in Orange County, it is unlikely that these control group customers were aware of the program). FSC identified customers from the pool of SCE's commercial and industrial (C&I) population that met 10/10 Program requirements in terms of applicable rates and at least a year of billing data prior to the 2012 summer. FSC excluded customers enrolled on demand response programs such as Critical Peak Pricing or the Capacity Bidding Program from the pool of eligible control group customers since these customers were not eligible for 10/10.

The aforementioned control group was developed using a statistical technique known as propensity score matching. In this procedure, a probit model is used to estimate a score for each customer based on a set of observable variables that are assumed to capture summer usage patterns. A probit model is designed to estimate probabilities—in this case, the probability that a customer in the eligible pool of control group customers is similar to a 10/10 customer in terms of a number of observable variables meant to capture summer usage patterns. Each customer in the 10/10 population is matched with a customer from the pool of eligible control group customers. The pool of eligible control group customers was restricted to customers in coastal climate zones similar to Orange County with maximum usage less than or equal to the maximum usage observed for customers in the 10/10 population. The propensity score matching regressions were run for non-TOU and TOU customers separately. For the non-TOU customers, separate regressions were run within deciles of average kW, which resulted in better matches. The characteristics used to model the propensity score for the matches were average kW during each quarter leading up to the 2012 summer.

The control group can be used on its own to estimate 10/10 impacts through subtraction; however, a more appropriate method is to use DID regression to account for exogenous factors. A DID regression uses information from both the control and treatment groups to estimate the effect of an

[2] Customers on TOU rates received bill credits based on peak usage; however, there were only a handful of these customers.
[3] Assigned customers are larger electricity users who have a specifically assigned account representative.

intervention (in this case the 10/10 Program). In this procedure, the difference between control group usage before and after the intervention of 10/10 is subtracted from the difference between treatment group usage between the two periods, hence *difference-in-differences*. This method ensures that exogenous factors such as Flex Alerts are not confounded with estimated impacts from the 10/10 intervention. The DID regression model also accounted for time effects and time-consistent unobservables at the customer level.

Accuracy Assessment

When choosing between competing methods and/or model specifications, it is important to use out-of-sample testing for accuracy assessment whenever possible. This type of accuracy assessment ensures that the model is not over-fit to the available data and provides a realistic expectation of the level of statistical precision that can be expected from the results. Without out-of-sample testing, an evaluator cannot objectively choose between competing methods and/or model specifications and is more likely to produce impact estimates that are an artifact of the model chosen as opposed to actual efforts by customers to change their usage in response to the program.

The persistent nature of the 10/10 Program demands a modified approach to out-of-sample testing relative to what is commonly done in DR load impact evaluations. For event-based DR programs, impacts are only expected on certain days of the summer when events are called, so evaluators can take advantage of various event-like days within the same time period to use for out-of-sample testing. For the 10/10 Program, impacts are persistent throughout the summer, so days within the same time period cannot be used in the accuracy assessment. Therefore, this evaluation took advantage of data from the previous summer when 10/10 was not in effect.

During summer 2011, the 10/10 Program was not active, so using this time period for testing ensures that the model accurately estimates summer usage when 10/10 rebates are not available. This is important to demonstrate because an estimate of summer usage without 10/10 rebates is the basis for the summer 2012 impact estimates. Considering that we do not include summer 2011 usage data in the model estimation, this approach constitutes an out-of-sample test that simulates what the model is used for in the summer 2012 impact evaluation. If the model predicts summer 2011 usage accurately, then we can be confident that the same model (applied one year later) will produce accurate summer 2012 impact estimates for 10/10.

Figure 1 shows the average hourly loads of 10/10 participants and the matched control group from July 2010 through September 2011. This is the time period over which 10/10 participants and control group customers will be analyzed for the 10/10 evaluation, but shifted one year earlier. The 2011 summer period of July through September was not included in the match in this accuracy assessment. FSC compared the treatment and control group loads during the summer 2011 period as a preliminary out-of-sample test. Since there was no 10/10 intervention during this time, the loads should match closely between treatment and control customers. During this time period, the percent difference in overall usage between the two groups is never greater than 1 percent, which suggests that the control group works quite well at the program level.

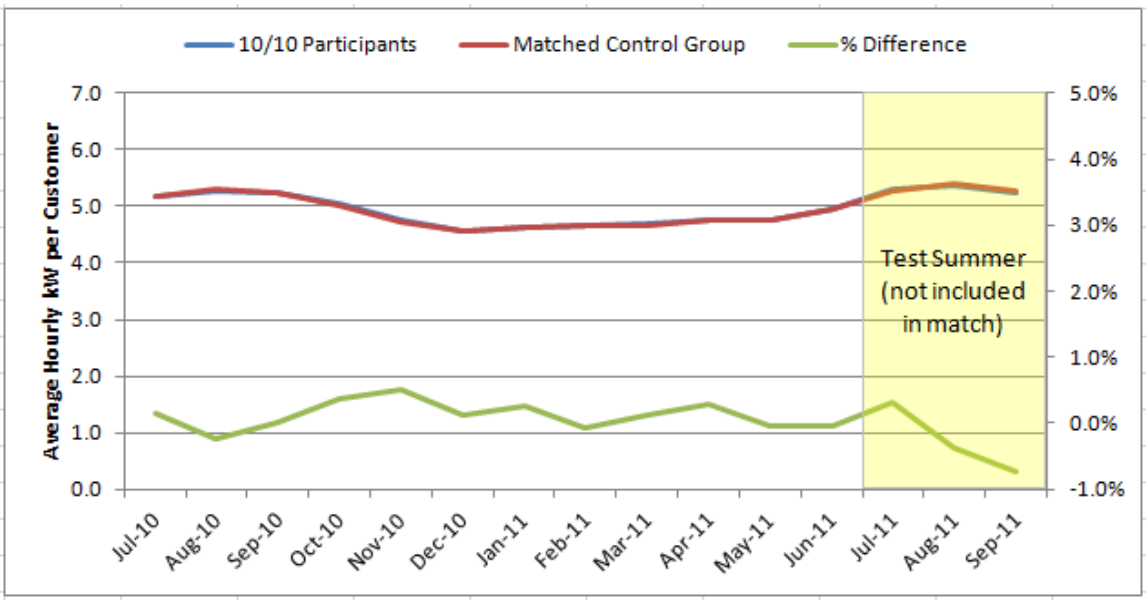

Figure 1: Test Summer Match - All Customers

DID regression was used to control for factors not associated with 10/10 that could be confounded with 10/10 impacts. An important part of the out-of-sample validation is to verify that the DID regression on the simulated intervention (2011 summer) produces substantively insignificant impacts. In this process, the 2011 summer is identified as the intervention time period in the DID regression model. The variable indicating the intervention is interacted with a variable identifying the treatment group (10/10 participants). The coefficient on this variable can be interpreted as the marginal effect of the simulated intervention on the treatment group. In this case, the simulated intervention is associated with a 0.5 percent increase in average kW for treatment group customers. Though statistically significant, this result is substantively insignificant. With large enough samples, almost everything is statistically significant and a 0.5 percent effect is smaller than the error associated with the matching process. This validation of the DID regression also shows that the control group works quite well at the program level.

Energy Impact Estimates

Estimation of energy impacts from the 10/10 Program follow from the validation analysis and methodology described in the previous section. A control group was developed using propensity score matching based on a year of pre-intervention usage data and the final estimates were produced using a DID regression to account for exogenous factors. Though this section only shows energy impact estimates on an aggregate basis, results were also calculated for usage quintiles and by industry segment.

Figure 2 is similar to the comparison of 10/10 participant and matched control group loads from the previous section; however, in this figure the analysis is shifted one year later and the summer time period represents the 10/10 intervention. Average hourly usage matches well in the months leading up to the intervention, but the lines diverge slightly during the intervention time period as the 10/10 participants use slightly more energy.

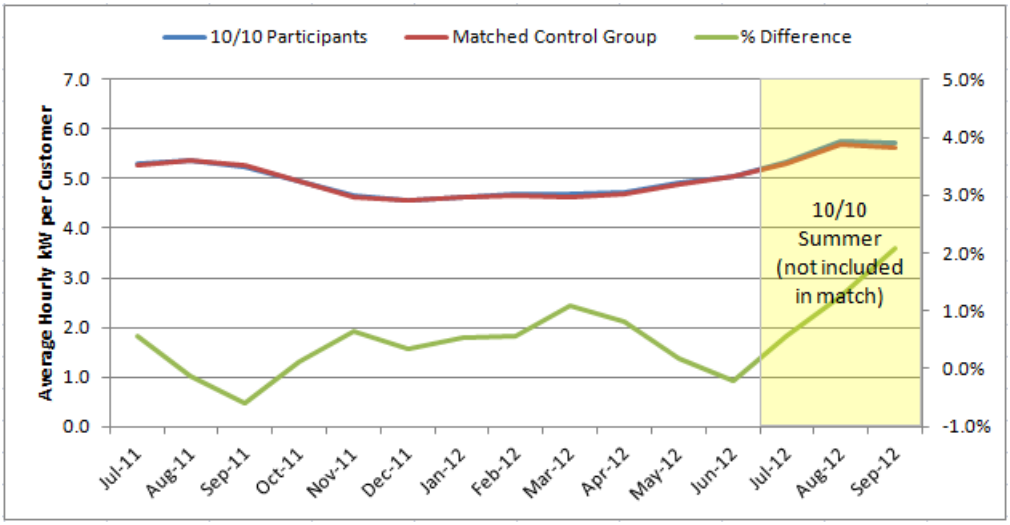

Figure 2: 10/10 Summer Match - All Customers

The DID regression takes the information from the figure above, but controls for additional factors including customer-level fixed effects and time effects. In this particular model, the marginal effect of the 10/10 intervention on program participants is a 0.56 percent increase in average kW. This effect is so small as to be substantively insignificant. The validation analysis found a 0.5 percent increase in average kW when there was no 10/10 intervention and the regression result with the 10/10 intervention is almost identical. The results of the analyses by usage quintile and industry segment were also substantively insignificant. Even though a non-trivial portion of participants were aware of the program (26 percent in August[4]), there were not any measureable impacts. As such, the program as designed is ineffective and does not encourage business customers to save energy.

This result is consistent with the evaluation of a similar program in 2005. The 20/20 Program urged residential and non-residential end users in California to reduce summer usage by 20 percent compared to usage in the previous summer in order to receive a 20 percent credit on their electric bill. Wirtshafter Associates' *Evaluation of the California Statewide 20/20 Demand Reduction Programs* (2006) also found that the program did not lead to a usage reduction among program participants.

Why Are Non-rebated Customers Included in the Analysis?

As discussed in the previous section, on an aggregate basis the 10/10 Program does not produce substantive load impacts. There is particular interest in looking at the breakdown of 10/10 impacts across customer categories such as bins of usage and industry, but even within these categories, the program does not yield impacts. A seemingly plausible alternative that would allow us to see impacts from 10/10 could be to look only at customers who received rebates. However, this is not a valid analysis. The raw data suggests that nearly all customers who received rebates under 10/10 received them due to factors not associated with the program. This is particularly clear in Figures 3 and 4, which represent the 19,759 10/10 participants who show a 10 percent usage reduction in 2011(when the program was not in effect) relative to 2010 and the 18,545 participants who show a 10 percent usage reduction in 2012 relative to 2011, respectively. The figures also show the load for the matched control group customers across the time periods associated with the validation analysis and 10/10 impact

[4] Summer Readiness Survey, Southern California Edison, November 27, 2012.

analysis.[5] Even though there is no 10/10 intervention in 2011, the impacts for this subset of customers are quite similar to the impacts observed when the 10/10 intervention is in place. This suggests that usage reductions compared to the previous summer occur due to factors that are not associated with the 10/10 Program. Basically, the matched control group is not accurate when we constrain the analysis to only focus on customers who reduced usage by 10 percent or more. Therefore, the impact estimates specifically for customers who received rebates are biased and cannot be attributed to the 10/10 Program. DID regression was also used to assess the impact of simulated and actual 10/10 intervention on customers with results that follow from the raw matched control group data.

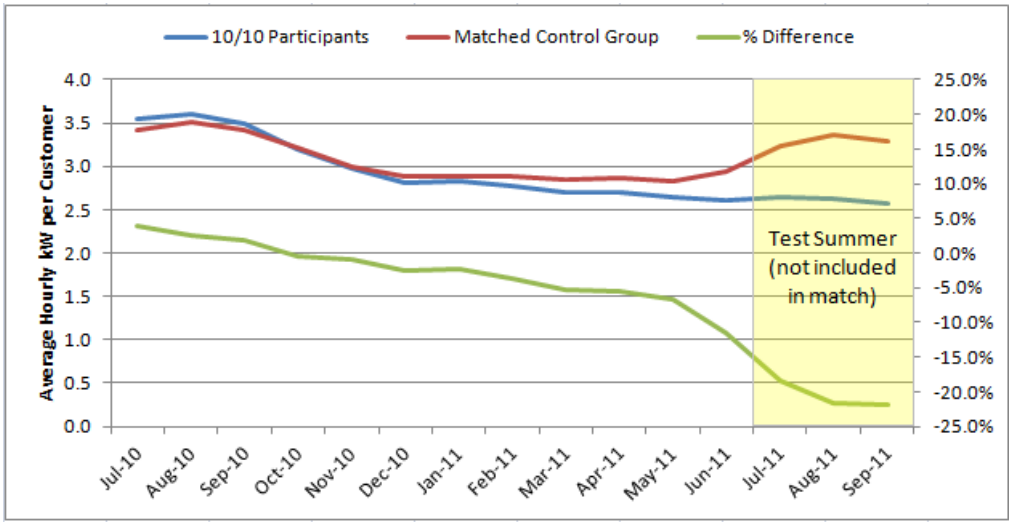

Figure 3: Test Summer Match - All Customers Who Show a 10 Percent or Greater Usage Reduction between 2010 and 2011 (n=19,759)

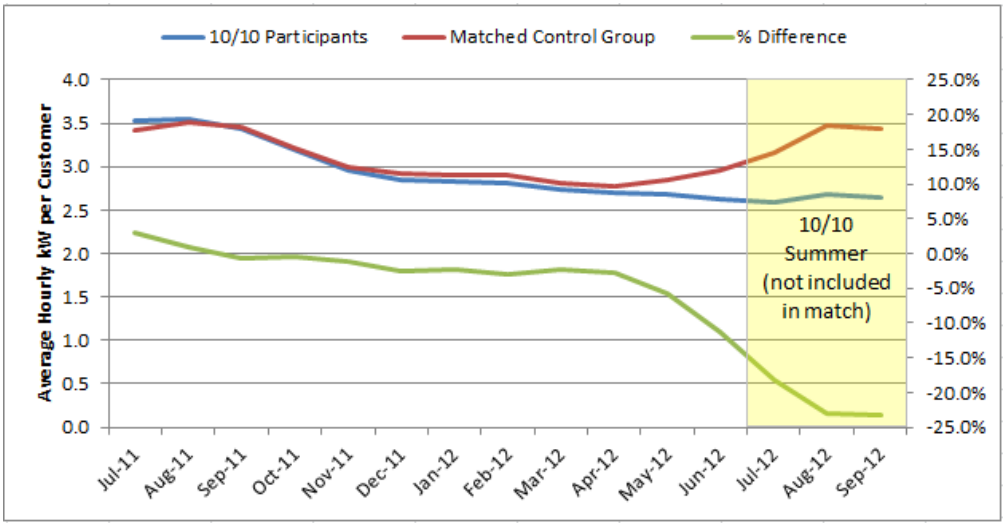

Figure 4: 10/10 Summer Match - All Customers Who Show a 10 Percent or Greater Usage Reduction between 2011 and 2012 (n=18,545)

[5] The two control groups include different sets of customers, but they were drawn using the same methodology, so this validation properly gauges the expected bias in the 10/10 control group for customers who received rebates.

Conclusions

This report presents energy impact estimates for the 10/10 Program, which SCE offered to customers in summer 2012. In total, rebates totaling $2.47 million were paid to approximately 20,000 customers whose usage in 2012 was 10 percent or more below their 2011 summer usage. However, quantitative analysis shows that these usage reductions were likely due to factors not associated with the 10/10 Program, which is consistent with the findings of the 20/20 Program evaluation in 2006. SCE research in August found that 26 percent of 10/10 participants were aware of the program, which suggests that lack of awareness was not a key reason for the lack of energy savings. Even though a non-trivial portion of participants were aware of the program, there were not any measureable impacts. As such, this paper concludes that the program as designed is ineffective and does not encourage business customers to save energy.