ASYMMETRIC INCENTIVES IN SUBSIDIES:
EVIDENCE FROM A LARGE-SCALE ELECTRICITY REBATE PROGRAM

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## ABSTRACT

Many countries use substantial public funds to subsidize reductions in negative externalities. However, such subsidies create asymmetric incentives because increases in externalities remain unpriced. This paper examines implications of such asymmetric subsidy incentives by using a regression discontinuity design in California's electricity rebate program that provided a financial reward for energy conservation. Using household-level panel data from administrative records, I find precisely-estimated zero causal effects in coastal areas. In contrast, the incentive produced a 5% consumption reduction in inland areas. Income and climate conditions significantly drive the heterogeneity. Asymmetric subsidy structures weaken incentives because consumers far from the rebate target show little response. The overall program cost is 17.5 cents per kWh reduction and $390 per ton of carbon dioxide reduction, which is unlikely to be cost-effective for a reasonable range of the social marginal cost of electricity.

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# 1 Introduction

In economic theory, negative externalities can be corrected by Pigouvian taxes that internalize external costs (Pigou, 1924). However, opposition from taxpayers generally makes it difficult to implement such taxes. As an alternative, many countries use substantial public funds to subsidize economic activities that presumably result in reductions in negative externalities. For example, while many countries have failed to introduce a carbon tax on greenhouse gas emissions, they spend substantial public funds to reward energy conservation and pollution abatement.[1] Likewise, regulators often provide subsidies for smoking cessation and alternative transportation rather than introduce high taxes on smoking and congestion.[2]

However, such subsidies create asymmetric incentives because increases in externalities remain unpriced. This asymmetry introduces two inherent problems. First, the subsidies may not be able to correct all externalities if the marginal decisions of many individuals are not affected by the subsidy incentive. This contrasts with Pigouvian taxes, which equalize the private and social marginal costs for all individuals. Second, the subsidies introduce an "additionality" problem (Joskow and Marron, 1992). Some of the observed behavior are not "additional" if they would occur in the absence of the subsidy incentive. Therefore, it is misleading to evaluate the causal effect of such subsidies by simply analyzing those who received a subsidy, although many previous studies take this approach.[3]

In this paper, I investigate these problems by applying a regression discontinuity (RD) design to a large-scale electricity rebate program in California. In the summer of 2005, California residents received a 20% discount on their electricity bills if they could reduce their

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[1] In the US, the American Recovery and Reinvestment Act of 2009 provided $17 billion for energy conservation programs. US electric utilities spent $26 billion dollars on energy efficiency programs in 1994-2011, and the annual spending has been continuously increasing since 2003 (U.S. Department of Energy, 2013)

[2] Many countries provide subsidies for energy-efficient appliances (Davis, Fuchs and Gertler, 2012; Boomhower and Davis, 2013), energy-efficient vehicles (Gallagher and Muehlegger, 2011; Sallee, 2011; Mian and Sufi, 2012; Sallee and Slemrod, 2012), reductions in energy consumption (Reiss and White, 2008; Wolak, 2010; Borenstein, 2013). Carbon offset programs such as the clean development mechanism (CDM) by the United Nations give firms credits if they reduce their pollution relative to a business-as-usual baseline level (Sutter and Parreño, 2007; Schneider, 2007; Duflo et al., Forthcoming). Financial incentives for smoking cessation is becoming a key policy instrument (Volpp et al., 2009). Congestion pricing is still rarely implemented in the US highway system and the federal and state government use substantial public funds to subsidize public transit to address congestion (Anderson, 2013).

[3] Joskow and Marron (1992) argue that many policy evaluations of utility conservation programs fail to take account of the the additionality problem. Boomhower and Davis (2013) studies how this problem affects the cost-effectiveness of an energy-efficient appliance subsidy program in Mexico.

electricity usage by 20% compared to the summer of 2004. The program's eligibility rule provides two advantages for my empirical strategy. First, to be eligible for the program in 2005, households had to open their electricity account before a cutoff date in 2004. Strategic manipulation of account open date was impossible because the program had not been announced until the spring of 2005. This rule created a sharp discontinuity in treatment assignment between households who opened their account before and after the cutoff date. Second, all eligible households automatically participated in the program, which prevents the self-selection problem that is a major challenge in previous studies.[4]

My analysis is based on the administrative data on household-level monthly electricity billing records from the electric utilities that administered the rebate program. Compared to survey data, the full administrative billing records have advantages in its accuracy in the measurement and its comprehensive coverage of consumers. The data also include each customer's nine-digit zip code, with which I match demographic and weather data to investigate potential heterogeneity in response to the subsidy incentive.

Using the discontinuity in treatment assignment, I apply a sharp RD design to estimate the local average treatment effect (LATE). I find that the rebate incentive reduced electricity consumption by 4% to 5% in inland areas in California, where the summer temperature is persistently high and the income level is relatively low. In contrast, I find precisely estimated zero treatment effects in coastal areas in California, where the summer temperature is moderate and the income level is relatively high. To explore what drives the heterogeneity in the treatment effect, I estimate the interaction effects between the treatment variable and climate conditions and those between the treatment variable and income levels. Results from the regressions suggest that the treatment effect increases by a 0.15 percentage point as the average temperature increases by 1 degree Fahrenheit and decreases by a 0.029 percentage point as income levels increase by 1%.

The asymmetric subsidy structure creates the possibility that the response to the subsidy differs between consumers whose consumption is close to the target level of consumption and consumers whose consumption is far from the target. To test whether the asymmetric

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[4]In most utility conservation programs, consumers opt-in to the programs (Joskow and Marron, 1992). This opt-in participation creates self-selection bias because the participants are likely to be different from non-participants of the program.

incentive creates a "giving-up" effect for consumers far from the target level, I estimate the quantile treatment effects on the changes in consumption. I find evidence that most of the treatment effects come from consumers who are closer to the target level of consumption, and that the treatment effect is not significantly different from zero for consumers who are far from the target level. This finding provides evidence that the asymmetry in the subsidy schedule weakens the incentive for conservation compared to a simple Pigouvian tax.

In general, regression discontinuity designs require relatively weak identification assumptions to estimate local average treatment effects (LATE). However, a disadvantage is that it gives LATE instead of average treatment effects (ATE) (Angrist and Rokkanen, 2012). In my research design, the RD estimates come from households that opened their account about a year before the beginning of the treatment period. An important question is whether the treatment effect is different between my RD samples and households that opened their account earlier.

To address this point, I propose a method that combines the RD design with difference-in-difference-in-differences (DDD). This method estimates the ATE with an additional identification assumption. In general, residential electricity consumers have a small positive trend in their electricity consumption after they open their accounts. This trend is translated into a small trend in my running variable for the RD design. To estimate the ATE, I assume that the positive trend in consumption is the same between households that opened their accounts on a certain date and those that opened their account on the same day in the previous year. With this assumption, I can difference out the small positive trend from my RD design and estimate the ATE. I use this method to estimate the ATE for consumers who opened their account 90 days, 180 days, one year, two years, three years, and four years before the eligibility cutoff date. I find that the difference between the ATE and the LATE is small and not statistically different. This finding suggests that the RD estimates are not significantly different from the treatment effects for consumers who opened their accounts earlier than my RD sample.

The findings of this paper provide several important policy implications. First, asymmetric incentives created by subsidy programs are likely to weaken incentives to reduce negative externalities. The evidence of zero treatment effect in coastal areas is consistent with the

theoretical prediction that consumers do not respond to the asymmetric incentive *at all* if the price elasticity is below a cutoff level. Second, the difference between my RD estimates and naive estimates of the treatment effect shows that the additionality problem is a central concern in evaluations of subsidy programs (Joskow and Marron, 1992; Boomhower and Davis, 2013). While my RD estimates show precisely-estimated zero causal effects in coastal areas, naive estimates that ignore the additionality problem indicate that a significant number of consumers responded to the incentive. This result provides evidence that careful empirical analysis is critical for evaluations of energy conservation programs (Allcott and Greenstone, 2012). It is particularly important for recent US energy policy because public spending for energy conservation programs has been growing rapidly. For example, the American Recovery and Reinvestment Act of 2009 provided $17 billion for energy conservation programs. US electric utilities spent $26 billion dollars on energy efficiency programs in 1994-2011, and the annual spending has been continuously increasing since 2003 (U.S. Department of Energy, 2013). Finally, I provide cost-effective analysis. The overall program cost is 17.5 cents per kWh reduction and $390 per ton of carbon dioxide reduction, which is unlikely to be cost-effective for a reasonable range of the social marginal cost of electricity.

# 2 Conceptual Framework

## 2.1 Asymmetric Incentive Structures of Conservation Subsidies

In this section, I use a simple framework to characterize theoretical predictions of consumer behavior in the presence of conservation subsides. Suppose that consumers have quasi-linear utility functions $u(y_i, n_i) = v(y_i) + n_i$ for electricity consumption $y_i$ and a numeraire consumption good $n_i$. Consumers with income $I_i$ and electricity price $p$ maximize $v(y_i) + I_i - py_i$ and consume $y_0$, where $v'(y_0) = p$.

Suppose that regulators consider that electricity price $p$ does not properly reflect the social marginal cost of electricity. For example, $p$ may not reflect the environmental externalities from electricity generation, or $p$ may not reflect the higher marginal cost of supplying electricity when the system faces a supply shortage. The first-best solution is to increase

the price by the cost of the externalities $\tau$. That is, increasing the electricity price by $\tau$ lets consumers choose $y^*$, where $v'(y^*) = p + \tau$.

However, regulators often prefer implementing conservation subsidies instead of introducing a price change. In conservation subsidy programs, regulators first determine "rebate baseline consumption" $b_i$, which is usually a function of consumer $i$'s past consumption level. Then, they offer a subsidy schedule that is based on $b_i$ and $y_i$. For example, figure 1 illustrates the subsidy schedule of the California 20/20 rebate program. Consumers receive a 20% discount on their summer electricity bills if they consume 20% less than their baseline, which is their consumption in the previous year's summer months. This subsidy creates a notch in the budget constraint because it changes both marginal and infra-marginal prices if consumers reach 80% of their baseline. In another type of conservation subsidy program, consumers receive a marginal subsidy for each unit of conservation relative to a certain baseline level. Examples of this type of subsidy schedule include peak-time rebate programs in dynamic pricing (Wolak, 2006, 2011; Faruqui and Sergici, 2010; Borenstein, 2013). In these cases, the subsidy schedule creates a kink at the baseline rather than a notch. In both cases, consumers receive a subsidy for reducing consumption, but do not receive a penalty for increasing consumption. This asymmetry creates important differences between such conservation subsidies and the first-best solution.

An inherent feature of such a subsidy schedule is that it creates asymmetry in the incentive to change consumption. In the case with the first-best solution, consumers have a simple price increase of $\tau$, which gives all consumers the same change in marginal incentive irrespective of where they fall in the budget constraint in Figure 1. In contrast, the introduction of conservation subsidies creates different incentives for consumers, depending on 1) where they fall in the budget constraint, 2) how price-elastic they are, and 3) how much uncertainty in consumption they have.[5]

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[5]Borenstein (2013) provides a detailed description of similar problems for peak-time rebate programs in dynamic electricity pricing.

## 2.2 Theoretical Predictions of Consumer Behavior

I begin with a simple case with two assumptions that may not be realistic in practice: (a) consumers have no uncertainty in consumption, and (b) their baseline $b_i$ is set reasonably close to $y_0$, which is their optimal consumption in the absence of the subsidy incentive.

First, consumers do not respond to the subsidy incentive *at all* if the price elasticity in absolute value is smaller than a certain cutoff level. To illustrate this point, suppose that consumers have a quasi-linear and iso-elastic utility function, $u(y_i, n_i) = \alpha_i \cdot \frac{y_i^{1+1/e}}{1+1/e} + n_i$, where $\alpha_i$ is a heterogeneous taste parameter and $e \leq 0$ is a constant price elasticity. In Figure 1, I illustrate two indifference curves, $A$ and $B$, with $|e_B| > |e_A|$. With inelastic price elasticity $e_A$, consumers do not change their consumption at all because the indifference curve does not reach the notch point. Price elasticity $e_B$ is the least elasticity required for consumers to change their consumption. This prediction implies that the subsidy incentive induces no change in consumption when the price elasticity is smaller than $|e_B|$ in absolute value. This result contrasts with the result in the first-best solution. When consumers have a simple price increase of $\tau$, the new budget constraint would have a steeper slope in the figure. Accordingly, all consumers reduce consumption based on the new slope.

Second, given assumptions (a) and (b), there should be bunching of consumers if the price elasticity is larger than $|e_B|$ in absolute value. In Figure 1, all indifference curves that have larger price elasticities than $|e_B|$ would have the optimal consumption at the notch point in the presence of the subsidy incentive.

However, in reality, assumptions (a) and (b) are unlikely to hold in residential electricity demand. Let me begin with assumption (a). Electricity consumers have significant uncertainty for their monthly electricity consumption. When consumers face this uncertainty, rational consumers do not respond to the exact nonlinear budget constraint (Saez, 1999; Borenstein, 2009; Ito, Forthcoming). Instead, they incorporate the uncertainty and respond to the expected price schedule, which is presented as the smoothed dotted line in Figure 1. The response to the smoothed schedule changes the first and second predictions above. First, the cutoff level of the price elasticity has to be even larger than the standard case with no uncertainty. In the previous example, consumers who have price elasticity $e_B$ no longer

respond to the subsidy incentive. Second, because the smoothed schedule no longer has a notch, there can be no bunching of consumers even if the price elasticity is nonzero.

Finally, the subsidy's incentive can be further weakened if rebate baseline $b_i$ is set far below $y_i^0$, which is consumer $i$'s optimal consumption in the absence of subsidy incentive. Conservation subsidy programs usually do not adjust $b_i$ for changes in weather or idiosyncratic shocks to each consumer. As a result, if the base year's weather is more moderate than the target year's weather, consumers are more likely to have harder baselines to reach. Furthermore, when consumers experience other negative shocks in consumption in the base year, their baselines become harder to reach. This endogenous baseline (Wolak, 2010; Borenstein, 2013) can introduce a "giving-up effect" because consumers far above the baseline consider the subsidy to be out of reach. Conversely, when consumers have the opposite direction of shocks in consumption, $b_i$ can be closer to $y_i^0$. In this case, consumers who would not respond to the subsidy reduce consumption less than 20% because now they are closer to the cutoff point. That is, the endogenous baseline makes the marginal incentive depend on where the baseline falls in the budget constraint. These predictions contrast with the prediction for the first-best solution, which produces the same marginal incentive for all consumers.

In the following sections, I empirically test these theoretical predictions by applying a regression discontinuity design to the California 20/20 rebate program. In the next section, I describe the research design and data for my empirical analysis.

# 3 Research Design and Data

This section provides the institutional background and research design of this study. First, I provide a brief history of the California 20/20 electricity rebate program. Second, I discuss the evidence and challenges in the existing studies. Finally, I describe how I address these challenges by the regression discontinuity design that exploits a sharp discontinuity in treatment assignment.

## 3.1 California 20/20 Electricity Rebate Programs

The California 20/20 electricity rebate program originates from the initial rebate program initiated by California Governor Gray Davis in 2001 during the California electricity crisis.[6] The California Public Utility Commission (CPUC) expected the continuous electricity shortage to cause rolling blackouts. To prevent them in the summer of 2001, the CPUC ordered the three largest California investor-owned electric utilities, Pacific Gas and Electric (PG&E), Southern California Edison (SCE) and San Diego Gas & Electric (SDG&E), to give their customers financial incentives to reduce electricity consumption. In the summers of 2001 and 2002, customers of the three California investor-owned electric utilities received a 20% discount for their June, July, August, and September bills if their monthly consumption was at least 20% lower than the same billing month in 2000. The CPUC ordered the same program in 2005 with a slight change in the scheme. In 2005, the original month-based rule was replaced by a summer-based rule, where customers received a 20% discount for their bills over the entire four-month period if they reduced their entire summer consumption by at least 20% relative to 2004.

This conservation rebate program was the largest in scale that paid households to reduce their energy consumption. Table 1 shows the scale of the 2005 rebate program for PG&E, SCE, and SDG&E. In 2005, 8% to 9% of customers received a rebate, and the total rebates amounted to $25 million. More customers received at least one rebate in 2001 and 2002, when the program was based on monthly consumption. Reiss and White (2003) report that about 39% of monthly residential bills in SDG&E qualified for a rebate in June, July, August, and September 2001. For the same 2001 rebate program, Goldman, Barbose and Eto (2002) find that about 33% of consumers received a rebate.

Although the CPUC aimed for a substantial reduction in electricity consumption[7], the effectiveness of the program was highly controversial. Proponents of the program have argued

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[6]By August of 2000, wholesale energy prices had more than tripled from the end of 1999, which caused price spikes in retail electricity rates, and financial losses to electric utilities in California. Many cost factors and demand shocks contributed to this rise, but several studies have also found the market power of suppliers to be significant throughout this period (Joskow, 2001; Borenstein, Bushnell and Wolak, 2002; Borenstein, 2002).

[7]For example, in the executive order, CPUC (2001) estimated that the program would help reduce energy consumption by up to 3,500 gigawatt hours in total and up to 2,200 megawatt hours during critical summer peak consumption periods.

that the simplicity of the program makes it straightforward for customers to understand the incentive and will likely encourage energy conservation. The rebate program also has been more politically appealing than alternative pricing policies such as an increase in electricity price or the introduction of dynamic pricing. In contrast to these alternative policies, the rebate program does not make customers feel that they bear a large economic burden even though the program is paid for by ratepayers as an increase in electricity price.

Opponents of the program have questioned its fairness and effectiveness. For example, Faruqui and George (2006) note that while the program is politically popular, it is unlikely to be effective for energy conservation. One concern is that the program does not incorporate weather differences between the base year and the target year. Therefore, if the target year is cooler than the base year, many households may receive a rebate simply because of the weather difference. The second concern is that even if there is no significant weather difference between the two years, many customers will receive a rebate because of random fluctuations in their electricity consumption. For example, customers that had a friend visit in the base year or customers that traveled in the target year can reduce their target year's consumption by 20% over their base year without conservation efforts.

Table 2 shows some evidence for the two concerns. I use household-level electricity consumption data to calculate what fraction of households reduced their summer electricity consumption by more than 20% in years when there was no rebate program. I calculate each household's change in consumption from 2003 to 2004 and from 1999 to 2000 for Southern California Edison. Note that the rebate programs was not in effect in any of the four years. From 2003 to 2004, the median household reduced consumption by 1.7% because the summer of 2004 was cooler than 2003. More importantly, 14.3% of households reduced their consumption by more than 20%. This statistic suggests that 14.3% of households would have received a rebate without a conservation effort if a rebate program had been in effect in 2004. In contrast, the summer of 2000 was warmer than 1999. As a result, the median household increased consumption by 7.7%. However, even in this case, 6.8% of households reduced consumption by 20% or more. Thus, random fluctuations in household electricity consumption create necessary costs for this rebate program. This issue sometimes leads to a concern about fairness because the program could induce a simple income transfer from one

household to others unrelated to their conservation efforts. Moreover, if the rebate expense for these random fluctuations exceeds the program's actual benefit, the cost-effectiveness of the program can be lower than previous estimates.

## 3.2   Challenges in Estimating the Treatment Effect

To examine the cost-effectiveness of the program, we need a reliable estimate of the treatment effect that is produced solely by the program incentive. The estimation of this treatment effect is, however, generally challenging with non-experimental data. Obviously, it is misleading to draw a conclusion simply by looking at the total consumption reduction achieved by the customers that received a rebate. Some of the rebated customers received a rebate not because of their conservation effort. On the other hand, some un-rebated customers may have responded to the program incentive but failed to reach the 20% reduction cutoff to receive a rebate. Therefore, comparing rebated and un-rebated customers does not provide much information about the program's treatment effect. The second challenge is how to control for potential differences between the base and target years that are unrelated to the program but affected electricity consumption. For instance, differences in weather and economic conditions likely affect electricity consumption in the two years. Therefore, changes in electricity consumption between the two years include the program's treatment effect and other confounding factors that are unrelated to the program, and these two effects must be disentangled by researchers to find the treatment effect.

Previous studies acknowledge that it is difficult to estimate the causal effect of the program. Goldman, Barbose and Eto (2002) is the first study to examine the impact of the original California 20/20 rebate program in 2001 by using survey data and aggregated load data. Based on a survey of 400 residential customers, they find that 70% of surveyed customers took some active steps to save electricity in 2001, 40% of surveyed customers knew about the program, and 57% of those who took active steps knew about the program. They concludes that the cost of purchasing savings through the 20/20 program was about 9 cents per kWh given the assumption that their estimated load reductions are solely attributable to the 20/20 program.

For the same 2001 rebate program, Reiss and White (2008) estimate the treatment effect

by using household-level billing data for 70,000 households in SDG&E. The study explores how household-level electricity consumption changes from the years before the California electricity crisis in 2001 to the years after the crisis. Based on the average within-household consumption changes relative to the same month during pre-crisis years, they conclude that the rebate program lowers consumption by approximately 4% to 6%. However, they also note that it is difficult to conclude that this estimate solely reflects the program's treatment effect because there were other conservation programs and public appeals in this period.

Finally, to my knowledge, Wirtshafter Associates (2006) is only the previous study that explores the effect of the 2005 California 20/20 program. The study uses some billing data from the electric utilities and also conduct a survey of 1,177 customers. The study uses the survey results to make two adjustments for estimation: subtract the reduction achieved by the rebated customers that was not due to their conservation efforts; and add the consumption reduction achieved by the non-rebated customers who tried but failed to reach the 20% cutoff. The study concludes that the cost per kWh savings range from 29 cents to $1 per kWh because a substantial level of load reductions may or may not be attributable to the program in their estimation.

A fundamental challenge in these previous studies is that there is no control group. In the absence of control group, it is difficult to disentangle the causal effect of the program from confounding factors. For example, researchers have to control for changes in weather, electricity price, and macroeconomic shocks between the base and target years. Moreover, Reiss and White (2008) and Goldman, Barbose and Eto (2002) note that it is even more challenging to control for the effect of other conservation programs that are in effect during the study period. In the next section, I describe how I address these challenges by using a regression discontinuity design for the 2005 rebate program.

## 3.3   A Sharp RD Design for the 2005 Rebate Program

To overcome the challenges described in the previous section, I exploit a discontinuous eligibility rule in the 2005 California 20/20 rebate program. To be eligible for the 2005 rebate program, customers had to open their electricity account by the program's eligibility cutoff date in 2004. Figure 2 illustrates how this eligibility rule was applied. In SCE, the cutoff

date was June 5, 2004. Therefore, customers that started their electricity service on or before June 5, 2004 received a notice letter in the spring of 2005 about the 2005 rebate program. Customers that started their service after the cutoff date (e.g. June 6, 2004) were not eligible for the program in 2005.

The rule includes two additional key components. First, it was impossible for customers to anticipate the 2005 rebate program when they started their electricity service in 2004, because the program was announced in the spring of 2005. This means that it was not possible for customers to strategically choose their start date in favor for the rebate program. Second, all eligible customers automatically participated in the program, which prevents the self-selection problem that is a major challenge in previous studies. Finally, the electric utilities that administered the program strictly enforced these rules without exception.

This quasi-experimental environment provides the following advantages. The discontinuous eligibility rule generated essentially random assignment of the program among households who started their account near the cutoff date. For example, customers that started their electricity service right before the cutoff date and right after the cutoff date are likely to have similar underlying properties for their electricity consumption, but they were assigned into different groups in terms of the treatment assignment of the rebate program. Even if there is a concern that the underlying properties might be correlated with their service start date, a regression discontinuity design (RDD) can eliminate the bias as long as the correlation between unobservable factors of electricity consumption and service start dates is continuous around the cutoff date for the rebate program.

## 3.4 Data

The primary data for this study consist of a panel data set of household-level monthly electricity billing records from 2004 to 2005 for the three largest investor-owned electric utilities in California. Under a confidentiality agreement, Pacific Gas & Electric (PG&E), Southern California Edison (SCE) and San Diego Gas & Electric (SDG&E) provided the complete billing history for essentially all residential customers in their service areas.[8] For

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[8]A very small number of customers are not individually metered in this area. The billing data sets include only individually-metered customers.

the main analysis of this paper, I focus on the data for SCE. I find similar results for PG&E and SDG&E and show them in the appendix. Each monthly record includes a customer's account ID, premise ID, billing start date and end date, monthly consumption, monthly bill, tariff type, climate zone, and nine-digit zip code. Customers' names and exact addresses are excluded in the records available for this study. The billing records also include customers' tariff information.

Figure 3 shows the service areas of California's electric utilities. PG&E provides gas and electric service for northern California, SCE serves electric customers in most southern California areas, and SDG&E provides gas and electric service around the greater San Diego metropolitan area. The 2005 rebate program was applied to customers served by all three electric utilities. The key variable for the regression discontinuity design of this study is each customer's account open date. The billing records include the exact open and close dates for each customer. Each day in California, about 10,000 customers open an electric account. For my main estimation, I use customers that open their electricity account within 90 days before and 90 days after the cutoff date.

The billing data do not include customers' exact address or demographic information. To obtain demographic information, I match each customer's nine-digit zip code to a census block group in the 2000 US Census data. In addition, I collect daily weather data from the Cooperative Station Dataset published by the National Oceanic and Atmospheric Administration's (NOAA's) National Climate Data Center (NCDC).[9] The data set includes daily minimum and maximum temperature for 370 weather stations in California. I match households' zip codes with the nearest weather station by following the matching mechanism in (Aroonruengsawat and Auffhammer, 2011; Chong, 2012).

# 4    Empirical Analysis and Results

In this section, I first use a regression discontinuity design to estimate the program's local average treatment effect (LATE). Second, I examine heterogeneity in the treatment effects by investing how income and weather affect the treatment. Third, I estimate whether the

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[9]I thank Anin Aroonruengsawat, Maximilian Auffhammer, and Howard Chong for sharing the data.

nonlinearity in the subsidy schedule induces a "giving-up" effect for consumers who are far from the 20% target level. Fourth, an important question raised from the RD design is whether the treatment effects are different from my RD samples and overall population. In the final part of this section, I propose a way to estimate the average treatment effect (ATE) by using the RD design with the difference-in-difference-in-difference (DDD) method.

## 4.1   A Regression Discontinuity Design to Estimate LATE

Consider household $i$'s electricity consumption $y_{it}$ for billing month $t$ before and during the California 20/20 program period. Let $D_i = 1\{i \in \text{treatment group}\}$, $D_t = 1\{t \in \text{treatment period}\}$, and $D_{it} = D_i \cdot D_t$. If the treatment status $D_i$ is randomly assigned, one can obtain a consistent estimate of the program's average treatment effect (ATE) by estimating the fixed effect estimation,

$$\ln y_{it} = \alpha \cdot D_{it} + \theta_i + \lambda_t + \eta_{it}, \tag{1}$$

by the ordinary least squares (OLS), where $\theta_i$ is household fixed effects, $\lambda_t$ is time fixed effects, and $\eta_i$ is an error term. However, the treatment assignment in the California 20/20 program was not random. Instead, it was assigned by $D_i = 1\{d_i \leq c\}$, where $d_i$ is household $i$'s account open date and $c$ is the eligibility cutoff date. I define the account open date relative to the cutoff date by $x_i = d_i - c$. Therefore, $D_i = 1\{x_i \leq 0\}$. Then, I apply a sharp regression discontinuity design for the discontinuity in the treatment assignment and estimate the following equation by OLS:

$$\ln y_{it} = \alpha \cdot D_{it} + f(x_i) \cdot D_t + \theta_i + \lambda_t + \eta_{it}. \tag{2}$$

This sharp RD design produces a consistent estimate of the program's local average treatment effect (LATE) if $E[\eta_{it} \cdot D_{it}] = 0$. That is, the identification assumption is that the error term has to be uncorrelated with the treatment conditional on a flexible continuous control function $f(x_i) \cdot D_t$ and other covariates.

Household fixed effects absorb time-invariant effects of $x_i$ on $\ln y_{it}$. Therefore, potential confounding factors that I need to control for are the time-*varying* effects of $x_i$ on $\ln y_{it}$. Con-

sider the demeaned consumption $\widetilde{\ln y_{it}} = \ln y_{it} - \bar{\theta}_i$. In residential electricity consumption data in general, the plot of $\widetilde{\ln y_{it}}$ against $x_i$ produces a nearly flat yet very slightly upward-sloping relationship. This upward-sloping relation comes from the fact that households tend to increase their consumption gradually after they open their electricity account. Ignoring this relation creates a bias for the treatment effect. However, because this relation is empirically smooth in $x_i$, I can control for it by including $f(x_i) \cdot D_t$.

Imbens and Lemieux (2008) describe two approaches to specifying a smooth control function $f_t(x_i)$. The first approach is to include a flexible parametric function. The second approach is to use a local linear regression with a triangular kernel to put more weights on data close to the cutoff point. I use the first approach for my main result and show that the second approach does not change my results. To avoid the mis-specification of $f_t(x_i)$ as much as possible, I focus on the data close to the cutoff date. For my main result, I use households that opened their accounts within 90 days before or after the cutoff date. I also use 60-day and 120-day bandwidths to show that my results are robust to these different bandwidths.

## 4.2 Testing the Validity of the Regression Discontinuity Design

A threat to the validity of RD designs is that the identification assumption is violated if there is self-selection at the cutoff, although this is unlikely to be the case for my research design. In the summer of 2004, no consumers knew that the California 20/20 program would be implemented in the following summer. Therefore, there was no way for households to self-select by strategically choosing their account open date.

Still, it can be a concern if there is a discontinuous difference between households around the cutoff date. To assess the validity of the regression discontinuity design, I first plot the number of new accounts opened per day in Figure 4. The horizontal axis is the account open date relative to the eligibility cutoff date, which is July 5, 2004. Each dot shows the mean number of new accounts per day over the 15-day bandwidth. Everyday, about 1,500 customers opened an account with SCE. The solid line shows the local linear fit and the dashed lines are its 95% confidence intervals. Over the 90-day bandwidth, there is a slight upward trend in the number of new accounts, although the slope is not statistically significant

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from zero. The figure also shows that there is no discontinuous jump at the cutoff date.

Figure 4 also shows the plot of household characteristics against the account open date relative to the cutoff date. I match the nine-digit zip codes in the billing data with the census block group to obtain demographic and housing characteristics. The figures include the mean over the 15-day bandwidth, the local linear fit, and its 95% confidence intervals.[10] None of the three variables show a statistically significant discrete jump at the cutoff date.

## 4.3 RD Estimates of the Effect of the Conservation Subsidy

In RD estimation, graphical analyses are an important part of quantifying the magnitudes of treatment effects as well as testing the validity of the identification strategy. The nature of regression discontinuity design suggests that the effect of the treatment of interest can be measured by the value of the discontinuity in the expected value of the outcome at a particular point (Imbens and Lemieux, 2008). Therefore, inspecting the estimated version of this conditional expectation is a simple yet powerful way to visualize the identification strategy.

Figure 5 presents a graphical analysis of the RD estimation for September billing, which is the last month of the treatment period. Using the data before and during the treatment period, I first estimate $\widetilde{\ln y_{it}} = \ln y_{it} - \bar{\theta}_i$. Then, I calculate the local mean of $\widetilde{\ln y_{it}}$ for each fifteen-day bandwidth over the running variable $x_i$. The local means are presented as as dots in the figure. Finally, I fit a local linear regression and a quadratic function to estimate $f(x_i) \cdot D_t$ for each side of the cutoff date. The dashed line is the local linear fit and the solid line is the quadratic fit. On the horizontal axis, the treatment group is on the left-hand side of the cutoff date because households that opened accounts before the cutoff date participated in the rebate program. Therefore, if the rebate incentive had an effect, there should be a discontinuous jump in the outcome variable at the cutoff point.

The figures provide a couple of important insights. First, there is a slight upward trend in $\widetilde{\ln y_{it}}$ over $x_i$, the account open date relative to the cutoff date. This upward trend comes from a general tendency in residential electricity consumption data; households tend to gradually

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[10]Because the variables are from the 2000 U.S. Census at the census block group level, I cluster the standard errors at the census block group level.

increase their consumption after they open their electricity account. Because ignoring this relation creates a bias for the treatment effect, it is important to control for the trend. The fitted lines of the local linear regression and quadratic regression over $x_i$ indicate that the RD estimates are likely to be robust between the local linear regression and the quadratic regression.

Second, Panel B shows evidence that the rebate incentive had a significant effect on electricity consumption in the inland climate zones. There is a clear discontinuous change in consumption between the treatment and control groups at the cutoff point. Visually, the treatment group's consumption is about 5% less than the control group's consumption. In contrast, Panel A suggests that the rebate incentive did not significantly change consumption in the coastal climate zones. There is no discontinuous change in consumption between the treatment and control groups at the cutoff point.

Table 3 presents the RD estimates of the effect of subsidy incentives on energy conservation. In columns 1 and 3, I estimate the program's overall treatment effect during the entire treatment period. In columns 2 and 4, I allow the treatment effects to differ for each billing month of the treatment period. I report RD estimates with 90-day bandwidths and quadratic controls for $f_t(x_i)$. Using different bandwidths and the local linear regression does not change my results, as I present in the next table. To adjust serial correlation in electricity consumption data, I cluster standard errors at the household level.[11]

In coastal climate zones, the treatment effects are essentially zero with tight standard errors. Because of the tightly estimated point estimates, the 95% confidence intervals do not include 1% treatment effects, suggesting that the program did not have a significant effect on households in coastal climate zones. In contrast, the subsidy incentive had a significant effect on electricity consumption in the inland climate zones. The overall treatment effect is about 4% and the treatment effect of each month ranges from 4% to 5%.

Because I have data for several months before the treatment period, a useful robustness check is to produce the RD estimates for billing months before the treatment period, which I examine to determine whether there is a discrete difference in the outcome variable between the treatment and control groups. Figure 6 presents the RD estimates of the difference in

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[11]In fact, ignoring the serial correlation produces very small standard errors.

log consumption between the treatment and control groups for the January 2005 through October 2005 billing months. In the coastal climate zones, the RD estimates are essentially zero both before and during the treatment period. In inland climate zones, the RD estimates are not statistically significant from zero before the treatment period. In contrast, the estimates during the treatment period suggest that the households in the treatment group reduced their consumption by about 5%. This figure provides evidence that the reduction in consumption is unlikely to come from factors unrelated to the program.

Another important robustness check is to examine how the choice of bandwidths and the method to control for $f_t(x_i)$ affects the estimates. In Table 4, I present RD estimates with 60-day and 120-day bandwidths and RD estimates with the local linear regression. Consistent with the suggestive evidence from Figure 5, the estimates are not sensitive to the choice of bandwidths and the method to control for $f_t(x_i)$. While the standard errors change slightly between different bandwidth choices, the RD estimates are fairly stable between different bandwidth choices. In addition, using the local linear regression instead of quadratic controls does not significantly change the estimates.

## 4.4 Heterogeneity in the Treatment Effect

### 4.4.1 Income and Climate Conditions

In the previous section, I find that the treatment effects are larger in inland areas than in coastal areas. This section explores what drives the heterogeneous treatment effect of the 2005 rebate program. In particular, I examine whether climate conditions or income differences can explain the heterogeneous treatment effects.

One of the significant differences between inland and coastal California is the summer climate conditions. For example, Figure A.1 illustrates the cooling degree days (CDD) in California in August 2005 by five-digit zip code areas. Generally, the summer temperature is persistently high in the inland areas, but quite moderate in the coastal areas. As a result, inland households typically use air conditioners throughout the summer while coastal households either use air conditioners very little or do not own them at all. For typical residential consumers who do not use air conditioners, a 20% reduction in summer electricity

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consumption is challenging. In contrast, for households that constantly use an air conditioner during the summer season, a 20% consumption reduction can be achieved by slightly changing the temperature settings or the length of usage.

Another significant difference between inland and coastal California is their demographic characteristics. For instance, income levels tend to be higher in coastal areas than inland areas. In previous studies on residential electricity demand, many studies find slightly larger price elasticity estimates for low income households (Reiss and White, 2005). Because the 20/20 rebate program is essentially a price-discount rebate program, lower-income households may be more likely to respond to the incentive if their price elasticity is larger than that of higher-income households.

To examine how climate conditions and income levels affect the program's treatment effects, I pool data from all climate zones and estimate the interaction effects. First, I calculate the average temperature at the nine-digit zip code level by calculating the mean of the daily mean temperature of the summer days in 2004 and 2005. Second, I obtain median per-capita income at the census block group level from the 2000 Census. Column 1 of Table 5 shows the RD estimate of the interaction term between the treatment variable and the average temperature in Fahrenheit. The estimate implies that the treatment effect increases by a 0.15 percentage point with an increase in the average temperature by one degree Fahrenheit. The estimate in Column 2 implies that the the treatment effect *decreases* by a 0.029 percentage point with a one-percent increase in income. These two interaction effects remain the same when both terms are included in the regression in Column 3. The results indicate that both climate conditions and income levels have a statistically significant effect on the magnitude of the program's treatment effect.

### 4.4.2 Nonlinearity in the Subsidy Schedule

A theoretical prediction in Section 2 implies that the nonlinearity in the subsidy schedule may induce a "giving-up" effect. Even if a consumer has a large price elasticity, the consumer may not respond to the incentive at all if she is far from the cutoff point of the 20% reduction in consumption. This implies that the treatment effect may not come from all consumers equally.

Consider $\Delta \ln y_{it}$, the change in log consumption from 2004 to 2005. If there is the giving-up effect, I expect that the different parts of the distribution of $\Delta \ln y_{it}$ to have different treatment effects. In particular, I would expects no change in consumption for higher percentiles in the distribution, because the treatment intervention is likely to have no effect on these percentiles if there is the giving-up effect.

To test the prediction, I estimate the quantile treatment effects in my RD design (Frandsen, Frölich and Melly, 2012):

$$\Delta \ln y_{it} = \alpha \cdot D_i + f(x_i) + \lambda_t + \epsilon_{it}, \tag{3}$$

where $D_i = 1\{x_i \leq c\}$. Note that this is a quantile regression on the *changes* in consumption. It estimates how the treatment intervention changes the distribution of the changes in consumption. I estimate the equation for the changes in consumption for August and September for households in inland climate zones.

Table 6 presents the quantile treatment effects for the 5th, 10th, 25th, 50th, 75th, 90th, and 95th percentiles. The treatment effect is larger for the lower tails of the distribution of the change in consumption. On the other hand, the treatment effect is not statistically significant from zero in the median and the higher tails of the distribution. This suggests evidence that the treatment effect mostly comes from the lower tail of the distribution. It also suggests that households that happen to be relatively far from the 20% reduction target are likely to give up and do not respond to the incentive. In theory, the rebate incentive can increase consumption in the left tail of the distribution, because if consumers are sure to receive a rebate, the rebate program is a decrease in price for these consumers. I find a positive point estimate for the fifth percentile, but the estimate is too noisy in the tail of the distribution to be statistically different from zero.

## 4.5   An RD-DDD Design to Estimate ATE

In general, regression discontinuity designs require relatively weak identification assumptions to estimate local average treatment effects (LATE). I can obtain consistent estimates as long as the conditional expectation of the error term is smooth at the cutoff of the running variable

$x_i$ and the control function $f_t(x_i)$ properly controls the smooth relationship.

A disadvantage of RD designs is that it gives the LATE instead of average treatment effects (ATE). In my research design, my RD estimates come from households that opened their account about a year before the beginning of the treatment period. An important question is whether the treatment effect is different between my RD samples and households that opened their account earlier. In RD designs, it is generally challenging to estimate the treatment effect away from the cutoff point, although recent papers provide several potential approaches to address this important question (Angrist and Rokkanen, 2012).

In my research design, there is a way to estimate the ATE by making one additional identification assumption. I propose a method that combines an RD design with a difference-in-difference-in-differences (DDD) method. The idea behind this method is similar to the approach in Jackson (2010). Recall that there is a slight upward trend of the outcome variable over the running variable in my RD estimation. This trend comes from a general tendency in residential electricity consumption data; households tend to gradually increase their consumption after they open their electricity account. Consider that household $i$'s consumption can be modeled as $\ln y_{it} = \theta_i + \lambda_t + g(t - d_i) + \epsilon_{it}$, where $d_i$ is its account open date. That is, the consumption depends on household fixed effects, time fixed effects, and the growth of consumption $g(t - d_i)$, which is a function of how long household $i$ has been an electricity customer at the same address. The actual functional form of $g(t - d_i)$ is unknown. Consider households $A$ that opened their accounts on $d_i$ in a certain year, and households $B$ that opened their accounts on the *exact same date* in the year before. The identification assumption that I make in this section is that $g(t - d_i)$ is common to households $A$ and $B$. That is, they have a common growth pattern in consumption after they open their accounts.

I make two data sets. My first data set is the electricity consumption data for the summer of 2005. This is exactly the same data set in the previous section. I define $x_i = d_i - c$, where $d_i$ is the account open date and $c$ is June 5, 2004. That is, $x_i$ is the account open date relative to the cutoff date $c$. I also define the treatment group by $D_i = 1\{x_i \leq 0\}$ and the treatment period by $D_t = 1\{t \in \text{treatment period of the rebate program}\}$.

My second data set is the electricity consumption data for the summer of 2004. Imagine that I make a data set for a placebo test by using data for exactly one year before my

main data set. I define every variable to be analogous to the first data set. I define $x_i = d_i - c_{2003}$, where $d_i$ is the account open date and $c_{2003}$ is June 5, 2003. Then, I define a placebo treatment group by $D_i = 1\{x_i \leq 0\}$ and a placebo treatment period by $D_t = 1\{t \in \text{treatment period of the rebate program - 365}\}$.

Let $y_{its}$ be electricity consumption for household $i$, billing month $t$, and data set $s = \{1, 2\}$. Define the treatment group in the first data set by $D_{its} = D_{it} \cdot 1\{s = 1\}$. Pooling the first and second data sets, I estimate:

$$\ln y_{its} = \alpha \cdot D_{its} + \beta \cdot D_{it} + g(x_i) \cdot D_t + \theta_{is} + \lambda_{ts} + \eta_{its}. \tag{4}$$

$\alpha$ is the difference-in-difference-in-differences (DDD) estimate of the program's average treatment effect (ATE). The basic idea behind this approach is that I can estimate $g(x_i) \cdot D_t$ by using the assumption that the growth pattern of consumption is common to households that opened an account on a certain date and those that opened their account on the exactly same date in the year before.

I begin with estimation of the ATE for households with $-180 \leq x_i \leq 90$. This estimation includes households whose account open dates fall between 180 days before and 90 days after the cutoff date. For the first data set, this means households whose account open dates fall between January 5, 2004 and September 5, 2004. For the second data set, this means households whose account open dates fall between January 5, 2003 and September 5, 2003. Given my identification assumptions, this estimation provides the rebate program's ATE for households whose account open dates fall between January 5, 2004 and September 5, 2004.[12] Similarly, I estimate the ATE for households with $x_i \geq -360$ (one year before the cutoff date), $x_i \geq -720$ (two years), $x_i \geq -1080$ (three years), and $x_i \geq -1440$ (four years) to examine whether the treatment effect differs for households that opened their accounts

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[12]To understand the intuition behind the estimation, it is useful to think about four groups included in the estimation: 1) the treatment group, who opened an account between January 5, 2004 and June 5, 2004, 2) the control group, who opened an account between June 6, 2004 and September 5, 2004, 3) the placebo treatment group, who opened an account between January 5, 2003 and June 5, 2003, 4) the placebo control group, who opened an account between June 6, 2003 and September 3, 2004. I control for the slope of my running variable (the growth of consumption after households open their accounts) by using the data of the placebo groups. I control for shocks specific to households in the main data set by using the data of the control group. As a result, the estimation provides the ATE for the treatment group given my identification assumptions

earlier than my RD sample.

Table 7 presents the LATE and ATE for inland climate zones. Column 1 shows the LATE from my RD estimation in the previous section. Column 2 shows the ATE for households whose account open dates fall between January 5, 2004 and September 5, 2004. The point estimates of the ATE are very close to those of the LATE, while the standard errors are tighter with the ATE. This is because the ATE is estimated from the broader range of samples, while the LATE is estimated essentially from the samples close to the eligibility cutoff date.

In columns 2 through 6, I include increasingly many treatment groups in the regression. For example, column 6 includes households that have been at the same address for four years or less. Overall, I find that the point estimates of the ATE are slightly larger in absolute value by about 0.001 to 0.01 percentage points. However, the differences between the ATE and LATE are not statistically significant. This means that the program's treatment effect is a 4% to 6% reduction in consumption, both for the RD samples and the samples that have been at the same address longer than the RD samples. [13]

# 5  Policy Implications

## 5.1  Cost-Effectiveness of the Program

In the literature, evaluation of energy conservation programs usually report two cost-effective measures: 1) program cost per unit of energy saved and and 2) program cost per ton of emissions abated (Joskow and Marron, 1992; Boomhower and Davis, 2013). Although these measures are not direct measures of welfare, they provide a valuable starting point and are widely used in policy discussions. I first provide these measures and discuss welfare implications in the next section.

In Table 8, I provide the program's cost-effectiveness based on my RD estimates of the effect of rebate incentives on energy conservation.[14]  First, I calculate the direct program

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[13]I find the same results for households in coastal areas. The ATE does not significantly differ from the LATE.

[14]I use the LATE from my RD estimation. Using the ATE from the previous section does not significantly change the results because my ATE and LATE are not statistically different.

cost for rebate payment, which is the total amount of rebate paid to consumers. Note that this direct cost does not include indirect costs such as expenses for administration and advertisement. Second, I calculate the reduction in electricity usage based on my RD estimates. Third, I translate this reduction into the reduction in carbon emissions by using the average carbon intensity of electricity consumed in California, which is 0.9 lb. per kWh in California Air Resources Board (2011). Finally, I obtain the program cost per unit of electricity saved and the cost per ton of carbon emissions abated.

The results provide important policy implications. First, the cost-effectiveness differs substantially between the coastal and inland areas. In the coastal areas, the program is a very expensive way to reduce electricity usage. This is because the program did not induce significant reductions in usage, although it paid substantial rebates for consumers who reduced their consumption for reasons unrelated to the program's incentive. The program cost, 94.5 cents per kWh is large relative to any reasonable range of the marginal cost of electricity. This is likely to be true even if I take account of the externality from carbon emissions. The program cost per ton of carbon emissions abated is $2099. This is quite large relative to $21, the social cost of carbon dioxide emissions per ton estimated by Greenstone, Kopits and Wolverton (2011). In contrast, the program cost per kWh reduction is much smaller in the inland areas. The program spent 2.5 cents to obtain a kWh reduction in consumption and $55 to obtain a reduction in a ton of carbon emissions.

Second, overall, the program is unlikely to be cost-effective within a reasonable range of assumptions on the private and social costs of electricity. The overall program cost is 17.5 cents per kWh reduction. The average cost of electricity supplied in SCE is 13.37 cents per kWh in 2005. To justify the program's cost-effectiveness only by the externality from carbon emissions, the social cost of carbon has to be larger than $92 per ton of carbon dioxide, which is larger than most estimates in the literature. In addition, this calculation does not include indirect costs such as expenses for administration and advertisement. According to the interview by Wirtshafter Associates (2006), SCE spent about $4 million for the administration and advertisement of the program. The overall program cost is 24.1 cents per kWh if I include these indirect costs into my calculation.

One important caveat is that the California 20/20 rebate program provided a rebate for

consumers based on their monthly electricity consumption. The marginal cost of electricity is generally higher in peak hours and lower in off-peak hours. If the reductions mostly come from peak hour usage, the benefit comes not only from reductions in emissions but also from savings of relatively high marginal cost of electricity. In this case, the cost-effectiveness would be better than my calculation. On the other hand, if the reduction mostly comes from off-peak usage, the cost-effectiveness would be worse than my calculation. From monthly billing data, one cannot quantify how many reductions come from peak and off-peak hours.

## 5.2   Implications for Welfare

The high cost-effectiveness ratio found in the previous section does not necessarily mean that the program is not welfare-improving, because the rebate expense can be thought as a transfer between consumers. The regulated electric utilities that administered the rebate program passed through the cost to consumers by increasing electricity price afterwords.[15] That is, the rebate expense is a transfer from rebated consumers to all consumers after the program's treatment period.

Consider two simple cases. Suppose that consumers pay a linear electricity price that is lower than the social marginal cost of electricity. In this case, the rebate program can be welfare-improving even if there is zero treatment effect. The rebate expense slightly increases electricity price afterwords and it can improve welfare if the new price is closer to the social marginal cost of electricity. Conversely, if consumers pay a linear electricity price that is higher than the social marginal cost of electricity, the rebate program is likely to lower welfare because there is a more price distortion after the rebate expense increases electricity price.

What complicates the welfare analysis of the California 20/20 program is the increasing block pricing (Borenstein, 2012; Ito, Forthcoming), where the marginal price of electricity is an increasing step function of monthly consumption. That is, consumers pay higher marginal price when they consume more during their billing month. In 2005 and 2006, the marginal

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[15]Most utility conservation programs in the US recover the cost by increasing electricity price. This is a notable difference from the energy-efficient appliance replacement program in Mexico (Boomhower and Davis, 2013), where the cost was paid by the tax revenue

prices for the first to fourth tiers were 12, 14, 17, and 20 cents per kWh. The key question is, what is the social marginal cost of electricity. I have to make two assumptions to provide an estimate of the social marginal cost. First, suppose that the long-run private marginal cost of electricity is equal to the average cost of electricity under the existing tariff schedule. Then, the private marginal cost is 13.37 cents per kWh for SCE in 2005. Second, suppose that the externality from carbon emissions is 0.95 cents per kWh. This estimate comes from the assumptions that 1) the social marginal cost of ton of carbon emissions equals \$21 according to Greenstone, Kopits and Wolverton (2011) and 2) the average carbon intensity of electricity consumed is 0.9 lb. per kWh according to California Air Resources Board (2011). Then, the social marginal cost of electricity is 14.32 cents per kWh for SCE in 2005.

In the billing data, about a half of consumers are in the first and second tiers and the other half of consumers are in the third and fourth tiers. That is, a half of consumer pay slightly lower marginal prices relative to the social marginal cost, while the other half pay significantly higher marginal prices relative to the social marginal cost. In theory, there is the possibility that the rebate program can improve welfare if the cost is recovered from the two lower tiers. However, in practice, California electric utilities have taken the opposite approach when they recovered the cost, because of a regulatory constraint. After the California electricity crisis, regulators and state legislators were concerned about the impact of price increases on lower income households. As a result, the first two tiers were virtually frozen. In fact, SCE increased only the third and fourth tier rates in 2006 (from 17 to 23 and from 20 to 32 cents), while it did not change the first and second tier rates. Therefore, it is hard to argue that it is welfare-improving unless the externality of electricity is much larger than the one that is used in my analysis.

# 6    Conclusion

Many countries use substantial public funds to subsidize reductions in negative externalities. However, such subsidies create asymmetric incentives because increases in externalities remain unpriced. In this paper, I study implications of such asymmetric subsidy incentives by applying a regression discontinuity design for the California 20/20 electricity rebate program.

Using household-level panel data from administrative records, I find precisely-estimated zero causal effects in coastal areas. In contrast, the incentive produced a 5% consumption reduction in inland areas. Income and climate conditions significantly drive the heterogeneity. Asymmetric subsidy structures weaken incentives because consumers far from the rebate target show little response.

My findings provide several important policy implications. First, the asymmetric incentive structure of subsidy programs is likely to weaken incentives to reduce negative externalities. Second, the difference between my RD estimates and naive estimates of the treatment effect shows that the additionality problem is important in evaluation of similar subsidy programs. Finally, the overall program cost is 17.5 cents per kWh reduction and $390 per ton of carbon dioxide reduction, which is unlikely to be cost-effective for a reasonable range of the social marginal cost of electricity.

The evidence of the heterogeneity in the treatment effect suggests that one way to improve the cost-effectiveness is to target lower-income households and households in areas with high summer temperature, because these households are more likely to respond to the subsidy incentive. Another point that can be improved is that the program should target only peak-hour consumption if the primary goal is to reduce high marginal costs of electricity in peak-hours. In 2005, regulators had no choice because California residents had conventional meters that recorded only monthly consumption. Recently, a growing number of consumers in many countries gain access to smart meters that record real-time consumption. This technology makes it possible to target consumption in particular time periods (Wolak, 2010; Ito, Ida and Tanaka, 2013; Jessoe and Rapson, Forthcoming). However, the fundamental problems that I find in this paper remain the same if regulators continue to provide asymmetric subsidy incentives that subsidize reductions in usage but do not tax increases in usage.[16]

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[16]Despite this problem, such rebate schemes are still politically favored in the ongoing discussions of dynamic electricity pricing. There is strong political opposition against critical peak pricing, which increases prices in peak-hours. In contrast, there is relatively less opposition against peak-time rebate programs, which is in principle very similar to the California 20/20 rebate program (Borenstein, 2013).

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**Wolak, Frank A.** 2010. "An Experimental Comparison of Critical Peak and Hourly Pricing: The PowerCentsDC Program." *Working Paper*.

**Wolak, Frank A.** 2011. "Do Residential Customers Respond to Hourly Prices? Evidence from a Dynamic Pricing Experiment." *The American Economic Review*, 101(3): 83–87.

Figure 1: Theoretical Predictions
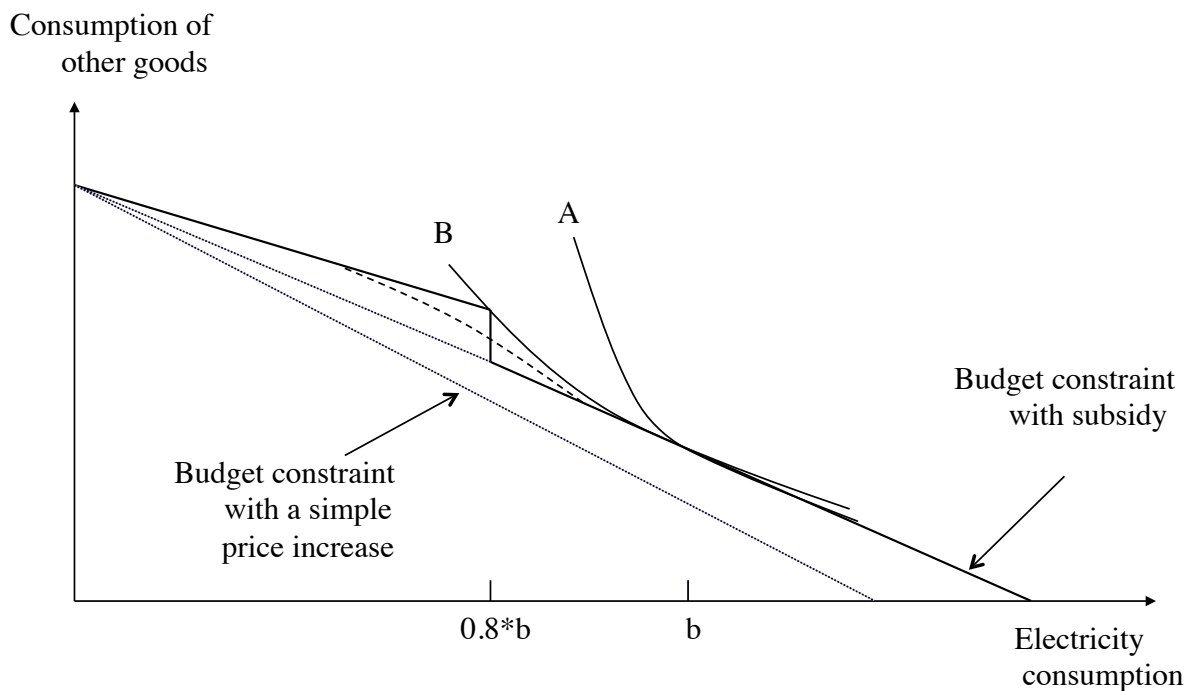


Note: This figure illustrates the theoretical predictions of consumer behavior when they are faced with the conservation subsidy schedule of the California 20/20 rebate program. The subsidy makes a notch in the budget constraint. If consumers respond to the expected price because of the uncertainty in consumption, the budget constraint based on the expected price becomes the dotted smooth line.

Figure 2: Eligibility Rule of the California 20/20 Rebate Program in 2005
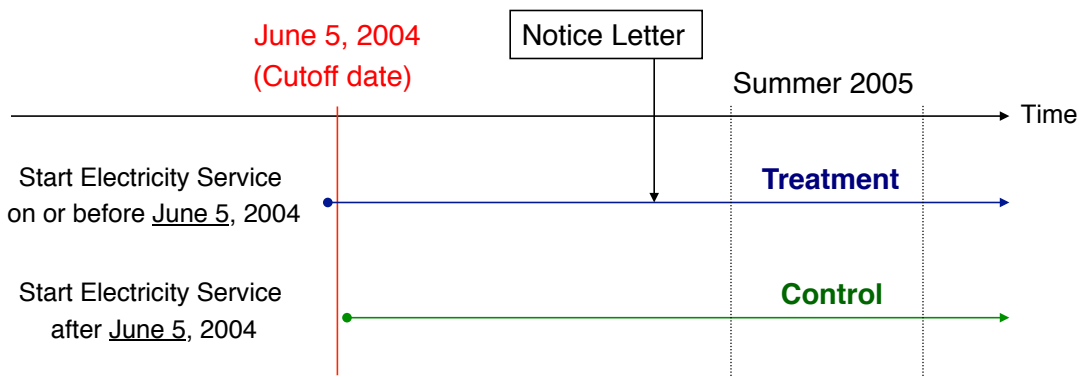


Note: Consumers who opened their electricity account on or before the cutoff date received a notice letter around April 2005. The letter informed them that they were going to receive a 20% discount on their entire summer electricity bill if they could reduce electricity consumption by 20% relative to their consumption in the summer of 2004. Those who opened their account after the cutoff date were excluded from the program. This eligibility rule created a sharp discontinuity in program enrollment because it was strictly enforced by the power companies.

Figure 3: California Electric Utility Service Areas



**California Electric Utility Service Areas**

Note: This figure shows the service area map of California electric utilities. Source: California Energy Commission.

Figure 4: Testing the Validity of the Regression Discontinuity Design



1) Number of new accounts opened per day

2) Median year structure built

3) Median household income ($)

4) Median rent ($)
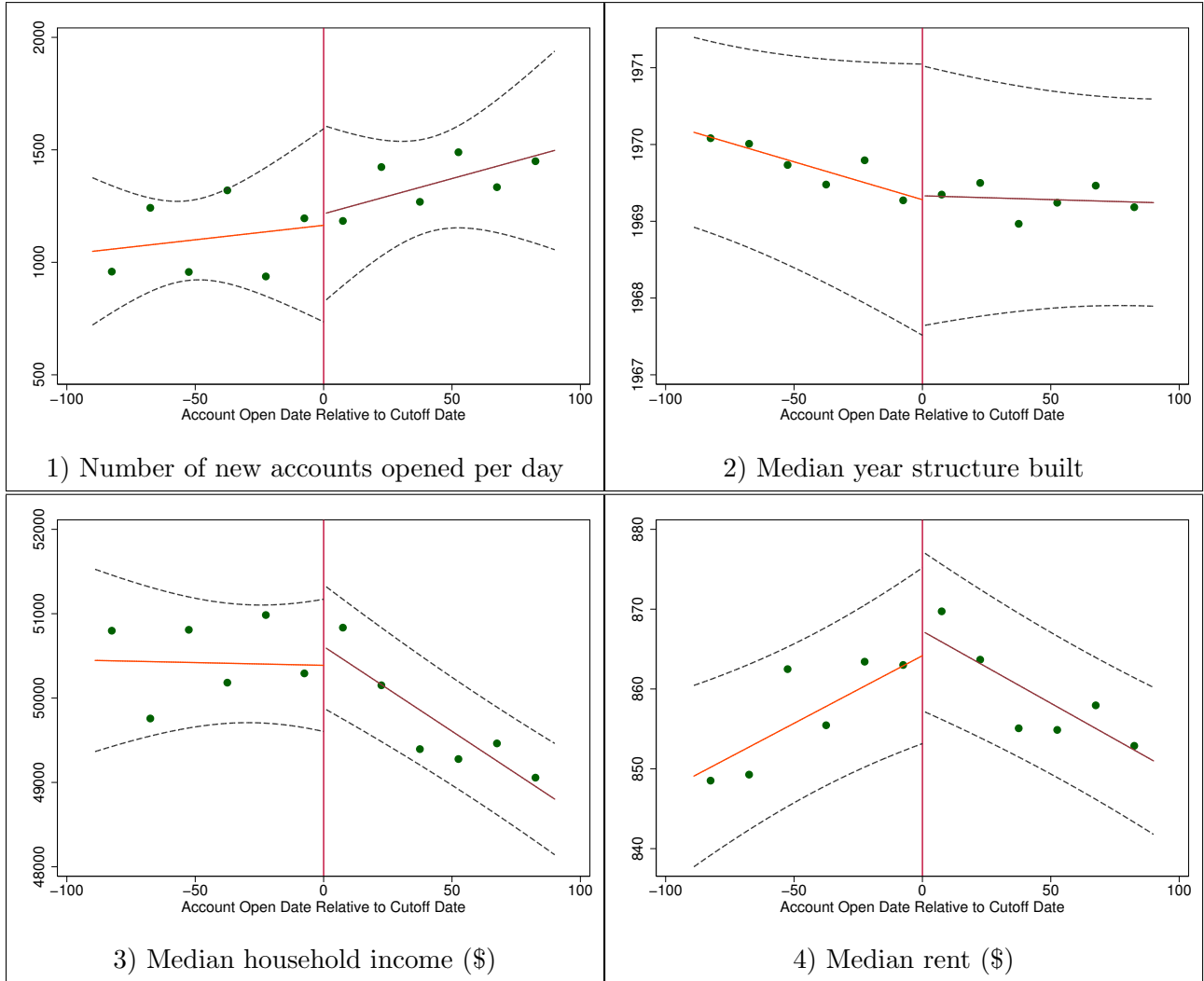
Note: The horizontal axis shows the account open date relative to the cutoff date of the program eligibility, which was June 5, 2004. Each dot shows the local mean with a 15-day bandwidth. The solid line shows the local linear fit and the dashed lines present the 95% confidence intervals. The confidence intervals for the fitted lines for variables from Census data are adjusted for clustering at the census block group level.

Figure 5: RD Estimates of the Effect of Subsidy Incentives on Energy Conservation: September
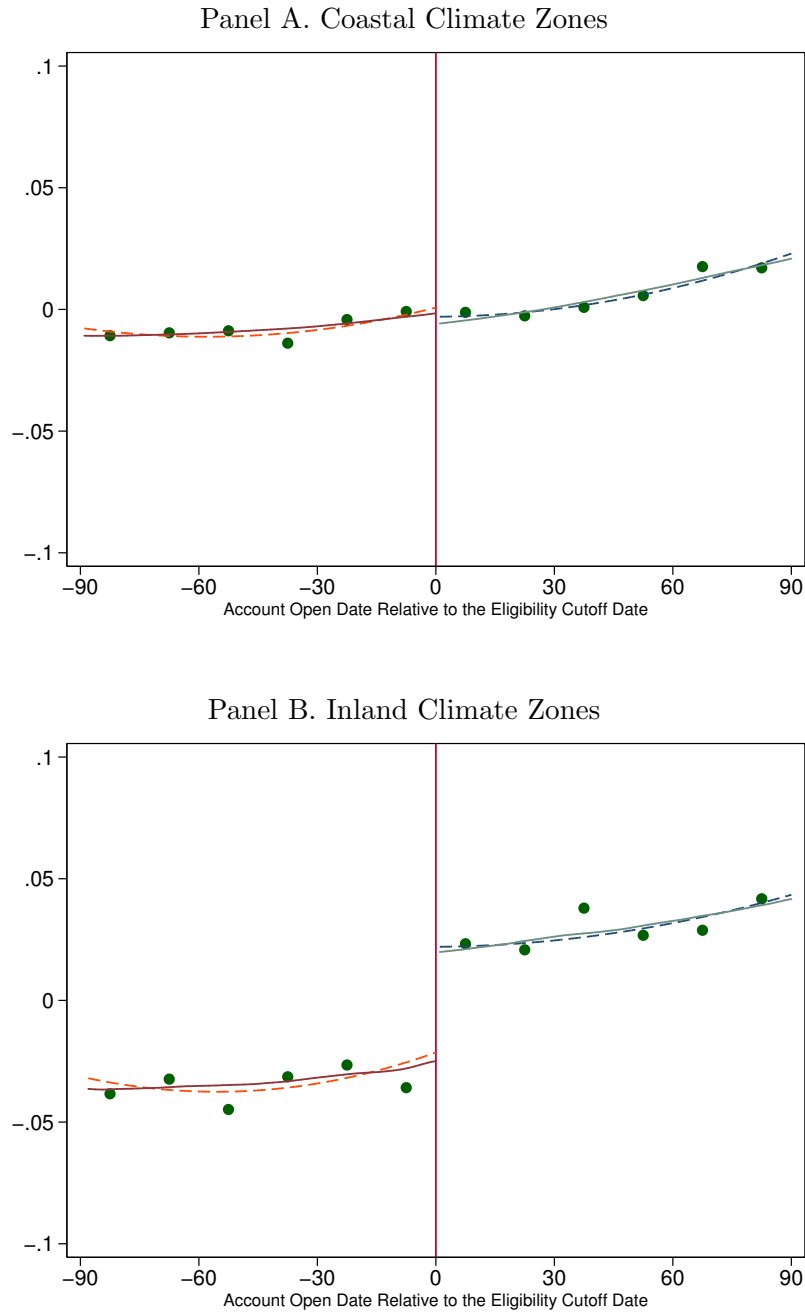
Panel A. Coastal Climate Zones



Account Open Date Relative to the Eligibility Cutoff Date

Panel B. Inland Climate Zones



Account Open Date Relative to the Eligibility Cutoff Date

Note: This figure presents the RD estimates of the effect of subsidy incentives on energy conservation for billing for September 2005. The horizontal axis shows households' account open date relative to the cutoff date for program eligibility, which was June 5, 2005. The vertical axis presents the log electricity consumption in September 2005, in which I subtract household fixed effects by using the consumption data in billing months before the treatment period. Each dot shows the local mean with a15-day bandwidth. The solid line shows the local linear fit and the dashed line shows quadratic fit.

Figure 6: RD Estimates of the Difference in Log Consumption between Treatment and Control
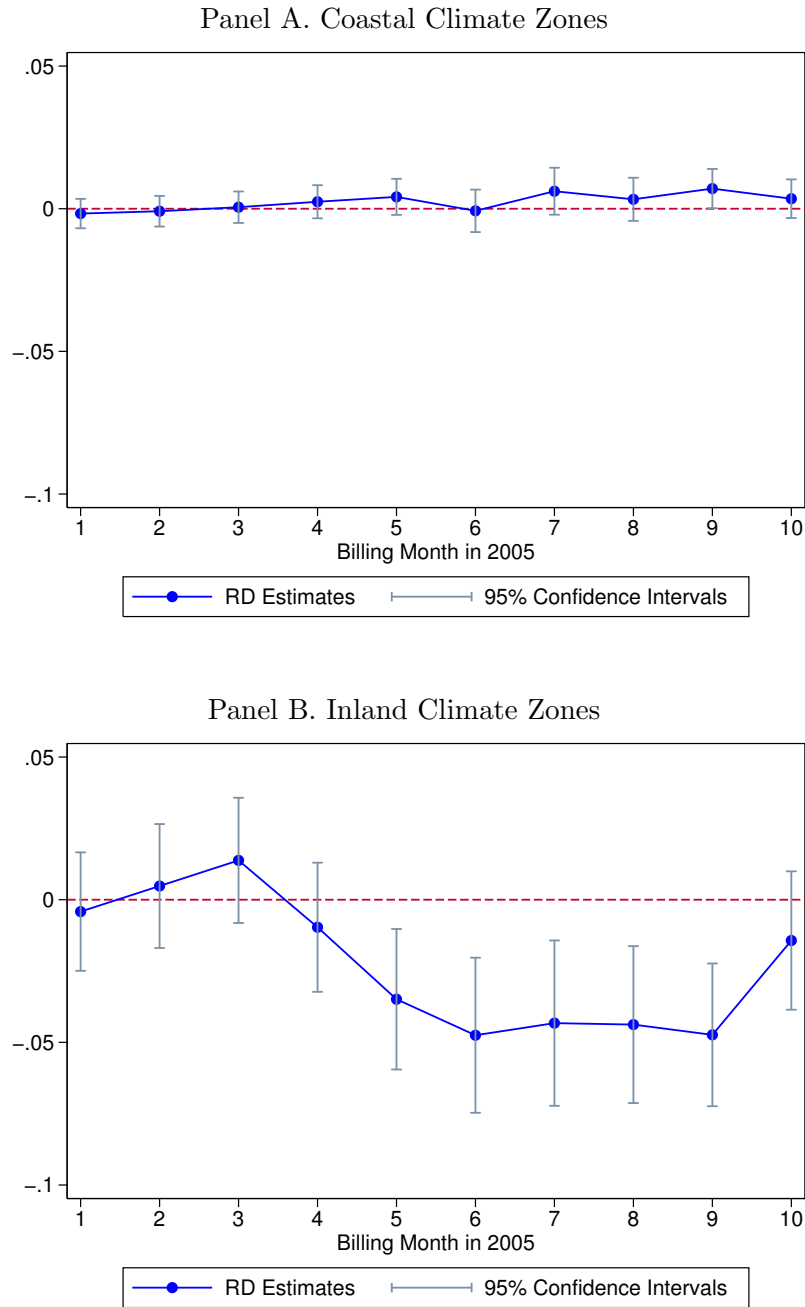
Panel A. Coastal Climate Zones



Panel B. Inland Climate Zones



Note: This figure presents the RD estimates of the difference in log consumption between the treatment and control groups. Household fixed effects are subtracted by using consumption data before January 2005. I use a 90-day bandwidth and quadratic controls for the trend of the running variable, which is the same specification as my main estimation result in Table 3.

Table 1: Aggregate Consumption and Rebates in the Summer Billing Months in 2005

| Utility | Consumption (kWh) | Revenue ($) | Households Receiving Rebates | Rebate ($) |
|---|---|---|---|---|
| PG&E | 10,065,216,512 | 1,320,995,584 | 8.24% | 10,786,594 |
| SCE | 9,401,883,648 | 1,257,056,768 | 7.91% | 10,609,540 |
| SDG&E | 2,284,046,848 | 363,180,320 | 9.07% | 4,325,000 |

Note: This table reports the statistics based on the residential billing data for June, July, August, and September 2005 billing months. I include customers who maintained their account in both the summers of 2004 and 2005. The rebate expenditure does not include the administrative and advertising costs of the program. All expenditures are in nominal 2005 dollars.

Table 2: Changes in Summer Electricity Consumption in SCE

| Year | Changes in Summer Weather | Median of % Changes in Consumption | % Households with 20% or More Reduction |
|---|---|---|---|
| From 2003 to 2004 | Cooler in 2004 | -1.7% | 14.3% |
| From 1999 to 2000 | Hotter in 2000 | 7.7% | 6.8% |

This table reports the statistics for within-household changes in summer electricity consumption for Southern California Edison (SCE). I first calculate the change in consumption for each household between the two years. I then calculate the median value of the change and the percentage of households that reduced their consumption more than 20%. Note that SCE customers did not experience a price spike during the California electricity crisis in 2000 because their retail rates are capped (Ito, Forthcoming).

Table 3: RD Estimates of the Effect of Rebate Incentives on Energy Conservation

|  | (1) Coastal | (2) Coastal | (3) Inland | (4) Inland |
|---|---|---|---|---|
| Treatment Effect | -0.001 (0.002) | | -0.042*** (0.013) | |
| Treatment Effect in May | | 0.003 (0.003) | | -0.034** (0.015) |
| Treatment Effect in June | | -0.001 (0.003) | | -0.055*** (0.017) |
| Treatment Effect in July | | 0.004 (0.004) | | -0.041** (0.019) |
| Treatment Effect in Aug. | | -0.003 (0.004) | | -0.037** (0.018) |
| Treatment Effect in Sep. | | -0.004 (0.003) | | -0.056*** (0.016) |
| Observations | 2,540,472 | 2,540,472 | 208,537 | 208,537 |

Note: This table presents the RD estimates of the effect of rebate incentives on energy conservation. The dependent variable is the log of electricity consumption. I estimate (2) with a 90-day bandwidth and quadratic controls for the trend in the running variable. Standard errors are clustered at the household level to adjust for serial correlation. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Table 4: Robustness Checks: Alternative Bandwidths and Specifications

|  | (1) Coastal | (2) Coastal | (3) Coastal | (4) Inland | (5) Inland | (6) Inland |
|---|---|---|---|---|---|---|
| Treatment Effect in May | 0.004 | 0.003 | 0.005 | -0.034** | -0.039*** | -0.029* |
|  | (0.004) | (0.003) | (0.004) | (0.015) | (0.014) | (0.017) |
| Treatment Effect in June | -0.002 | -0.001 | -0.003 | -0.055*** | -0.059*** | -0.050** |
|  | (0.004) | (0.004) | (0.004) | (0.017) | (0.016) | (0.019) |
| Treatment Effect in July | 0.004 | 0.005 | 0.005 | -0.041** | -0.039** | -0.042** |
|  | (0.004) | (0.004) | (0.005) | (0.019) | (0.017) | (0.022) |
| Treatment Effect in Aug. | -0.004 | -0.005 | -0.003 | -0.036** | -0.034** | -0.035* |
|  | (0.004) | (0.004) | (0.004) | (0.018) | (0.016) | (0.020) |
| Treatment Effect in Sep. | -0.005 | -0.003 | -0.004 | -0.056*** | -0.053*** | -0.052*** |
|  | (0.003) | (0.004) | (0.004) | (0.016) | (0.015) | (0.018) |
| Controls for f(x) | local linear | quadratic | quradratic | local linear | quadratic | quradratic |
| Bandwidth | 90 days | 120 days | 60 days | 90 days | 120 days | 60 days |
| Observations | 2,540,472 | 3,325,388 | 1,707,589 | 208,537 | 237,264 | 162,067 |

This table presents RD estimates with different bandwidth choices and alternative controls for the running variable. The dependent variable is the log of electricity consumption. Standard errors are clustered at the household level to adjust for serial correlation. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Table 5: RD Estimates Interacted with Income and Climate

|  | (1) | (2) | (3) |
|---|---|---|---|
| Treatment | 0.095** | -0.297*** | -0.199*** |
|  | (0.051) | (0.055) | (0.077) |
| Treatment*Ave.Temp.(F) | -0.0015** |  | -0.0016** |
|  | (0.0007) |  | (0.0008) |
| Treatment*ln(Income) |  | 0.029*** | 0.029*** |
|  |  | (0.005) | (0.005) |
| Observations | 2,749,009 | 2,749,009 | 2,749,009 |

This table presents the RD estimates of the effect of rebate incentives on energy conservation interacted with income and climate conditions. The dependent variable is the log of electricity consumption. I use a 90-day bandwidth and quadratic controls for the trend in the running variable. Standard errors are clustered at the household level to adjust for serial correlation. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels..

Table 6: Quantile Regressions on the Change in Log Consumption for Inland Climate Zones

|  | (1) p5 | (2) p10 | (3) p25 | (4) p50 | (5) p75 | (6) p90 | (7) p95 |
|---|---|---|---|---|---|---|---|
| Treatment | 0.034 | -0.099*** | -0.078*** | -0.007 | -0.020 | -0.019 | -0.025 |
|  | (0.056) | (0.035) | (0.018) | (0.018) | (0.019) | (0.033) | (0.063) |
| Observations | 37,914 | 37,914 | 37,914 | 37,914 | 37,914 | 37,914 | 37,914 |

This table presents the quantile RD estimates of the effect of rebate incentives on energy conservation interacted with income and climate conditions. The dependent variable is the change in the log of electricity consumption from 2004 to 2005. I use a 90-day bandwidth and quadratic controls for the trend in the running variable. Standard errors are clustered at the household level to adjust for serial correlation. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Table 7: Average Treatment Effects (ATE) for Inland Climate Zones

| Estimates:<br>Bandwidth: | (1)<br>LATE<br>(90 days) | (2)<br>ATE<br>(180 days) | (3)<br>ATE<br>(1 year) | (4)<br>ATE<br>(2 years) | (5)<br>ATE<br>(3 years) | (6)<br>ATE<br>(4 years) |
|---|---|---|---|---|---|---|
| Treatment Effect in May | -0.034** | -0.039*** | -0.040*** | -0.039*** | -0.043*** | -0.040*** |
| | (0.015) | (0.009) | (0.008) | (0.006) | (0.006) | (0.005) |
| Treatment Effect in June | -0.055*** | -0.054*** | -0.062*** | -0.057*** | -0.063*** | -0.057*** |
| | (0.017) | (0.011) | (0.009) | (0.006) | (0.006) | (0.005) |
| Treatment Effect in July | -0.041** | -0.042*** | -0.045*** | -0.043*** | -0.046*** | -0.042*** |
| | (0.019) | (0.012) | (0.010) | (0.007) | (0.007) | (0.006) |
| Treatment Effect in Aug. | -0.037** | -0.040*** | -0.042*** | -0.043*** | -0.041*** | -0.040*** |
| | (0.018) | (0.011) | (0.009) | (0.007) | (0.007) | (0.006) |
| Treatment Effect in Sep. | -0.056*** | -0.054*** | -0.061*** | -0.057*** | -0.063*** | -0.057*** |
| | (0.016) | (0.010) | (0.008) | (0.006) | (0.006) | (0.005) |
| Observations | 208,537 | 258,454 | 406,924 | 638,580 | 830,332 | 1,001,691 |

This table presents the RD-DDD estimates of the effect of rebate incentives on energy conservation for inland climate zones. Given the identification assumptions described in the main text, this estimation produces the average treatment effect (ATE) for the samples included in each estimation. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Table 8: Program Cost Per Estimated Reductions in Consumption and Carbon Dioxide

|  | Coastal | Inland | Total |
|---|---|---|---|
| Number of Customers | 3,190,027 | 299,178 | 3,489,205 |
| Consumption in Summer 2005 (kWh) | 8,247,457,920 | 1,154,292,248 | 9,401,750,168 |
| Direct Program Cost for Rebate ($) | 9,358,919 | 1,250,621 | 10,609,540 |
| Estimated Reduction (kWh) | 9,908,840 | 50,605,714 | 60,514,555 |
| Estimated Reduction in Carbon Dioxide (ton) | 4,459 | 22,773 | 27,232 |
| Program Cost Per kWh ($/kWh) | 0.945 | 0.025 | 0.175 |
| Program Cost Per Carbon Dioxide ($/ton) | 2,099 | 55 | 390 |

Note: This table reports the cost-benefit analysis of the 20/20 program for SCE's coastal areas, inland areas, and all service areas. Row 1 shows the number of residential customers who maintained their accounts in the summer of 2004 and 2005. Row 2 presents the aggregate consumption in the summer months. Raw 3 reports the aggregate amount of rebate paid to customers. Row 4 shows the estimated kWh reduction from the treatment effect of the program. Row 5 translates this reduction into the reduction in carbon emissions by using the average carbon intensity of electricity consumed in California, which is 0.9 lb. per kWh in California Air Resources Board (2011).

# Online Appendix (Not For Publication)

Figure A.1: Cooling Degree Days in August 2005 in California
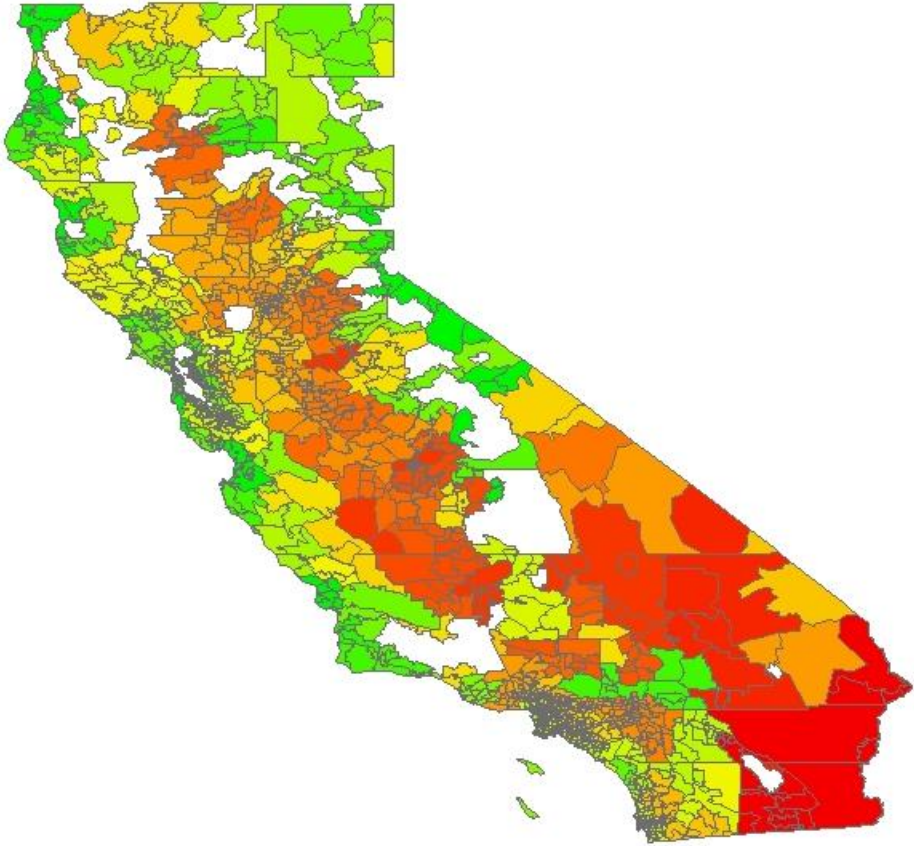


Note: This figure shows the cooling degree days (CDD) in August 2005 in California by zip code.

Table A.1: RD Estimates of the Effect of Rebate Incentives on Energy Conservation in PG&E and SDG&E

|  | (1) PG&E Coastal | (2) PG&E Inland | (3) SDG&E Coastal |
|---|---|---|---|
| Treatment Effect in May | 0.001 (0.003) | -0.031*** (0.010) | 0.001 (0.007) |
| Treatment Effect in June | -0.002 (0.002) | -0.041*** (0.011) | -0.002 (0.007) |
| Treatment Effect in July | -0.001 (0.003) | -0.043*** (0.010) | -0.001 (0.008) |
| Treatment Effect in Aug. | 0.002 (0.003) | -0.042*** (0.011) | 0.002 (0.008) |
| Treatment Effect in Sep. | 0.003 (0.003) | -0.035*** (0.009) | 0.003 (0.007) |
| Observations | 2,764,960 | 867,884 | 1,234,346 |

Note: This table presents the RD estimates of the effect of rebate incentives on energy conservation for samples in PG&E and SDG&E. Note that the program eligibility cutoff date is June 1, 2004 for PG&E and June 30, 2004 for SDG&E. In SDG&E, the vast majority of consumers live in coastal areas. The dependent variable is log of electricity consumption. I use the 90-day bandwidth and quadratic controls for the trend in the running variable. Standard errors are clustered at the household level to adjust for serial correlation. ***, **, and * show 1%, 5%, and 10% statistical significance respectively.