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Can Social Norms Motivate Employee Conservation Efforts?

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Abstract
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A randomized experiment is used to test whether employees will take actions to lower short- and long-run electricity use when their actions are unobservable and only the firm can benefit. Results suggest that social norms act as a coordinating device supporting worker conservation efforts. Electricity use fell 5.2% on average in buildings that were provided information on their own energy use compared to that in a paired building. The energy reductions have persisted over three years. Feedback on own past usage and provision of promotional information induced smaller and statistically insignificant reductions in electricity use.

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Can Social Norms Motivate Employee Conservation Efforts?

Following the surge in oil price after the 1973 OPEC oil embargo, public surveys began to document widespread and consistent concern about energy supplies. By 1979, 85% of the U.S. population described the energy situation as very or fairly serious, a proportion that remained reasonably stable for the following 27 years (Bolsen and Cook, 2008). More recently, the concerns have spread to the accumulation of greenhouse gasses in the atmosphere, peaking at 41% of the population claiming to worry a great deal about global climate change around the time of the 2007 publication of the Intergovernmental Panel on Climate Change and the release of the movie, *An Inconvenient Truth* (Brulle *et al,* 2012). As early as 1974, 65% of the population said consumers bore at least some of the blame for the energy crisis, a proportion that rose to 79% by 2006. About two-thirds of surveyed adults said they were likely to drive less or turn down thermostats in order to conserve energy (Bolsen and Cook, 2008).

As public concern about energy supply rose in the 1970s, psychologists began examining the incentives for individuals to participate in energy conservation efforts. As reviewed by Stern (1992) and Abrahamse *et al* (2005), individual energy conservation efforts were only partially based on pecuniary returns. One problem was that consumers often lacked information on cost-effective energy reduction strategies even when they were motivated to conserve. In addition, information on actual energy use came at a considerable lag through billing statements, and so consumers had difficulty matching conservation efforts with feedback on whether those efforts were successful. An added complication is that when landlords pay energy bills rather than tenants, renters will have no monetary incentive to conserve (Gillingham, Harding, and Rapson , 2012; Myers, 2014). Consistent with these observations, psychological studies found that providing information alone did not affect conservation efforts, but monetary incentives and improved feedback did reduce energy use (Hayes and Cone, 1977; Abrahamse *et al*, 2005).

Building on evidence that social norms influence individual voluntarily donations to charities,[1] researchers have also investigated whether individuals reduce energy consumption if they believe that their neighbors are also conserving. A company called OPOWER contracted with energy companies around the country to provide a Home Energy Report letter that contained feedback on a customer's energy use relative to similar households. The customer report also included information on how to lower energy use. As part of the experimental design, a randomly selected subset of customers were placed in a control group that did not receive the report. Customers receiving the information on their neighbors energy usage lowered their electricity consumption by about 2% compared to comparable controls (Allcott, 2011; Ayres *et al*, 2009). The effect persisted even after the social norming messages were discontinued, diminishing only 10-20% per year (Allcott and Rogers, 2014).

In most of these studies, it is not clear whether the reduction is due to the information or the provision of social norms. However, a similar experiment on water conservation randomized whether the household received only the information on conservation strategies, the information plus an appeal to conserve, or both of the previous materials plus a comparison of own versus other consumption. Those who received the social norm information lowered their water use by 4.8% compared to only a 0.6% decrease for those who received only the information and a 2.3% decrease for those who received the information and the appeal (Ferraro and Price, 2013).[2]

[1] Frey and Meier (2004), Croson and Shang (2008), Martin and Randall (2008), Goldstein et al (2008).
[2] List and Price (2013) review this literature, concluding that the most promising mechanisms encouraging private conservation are social norms and feedback.

Nevertheless, while these individual energy or water conservation efforts may be influenced by a sense of social obligation, consumers still garner private benefits from reduced utility bills.[3]

Firms also could act to conserve on social norming grounds, although evidence suggests that firm conservation efforts are motivated by expected financial rewards. The review by Khanna (2001) suggested that voluntary firm adoption of environmental self-regulation occurs when the firm is responding to pressure from stockholders or when the firm acts strategically to lower the likelihood of more costly government regulations (Khanna,2001). Participation in the EPA's 33/50 program in which firms voluntarily reduce emissions appears to have been profitable, even though there is only mixed evidence of actual emission reductions (Khanna and Damon, 1999; Vidovic and Khanna, 2007). Voluntary firm adoption of environmental enhancements is positively correlated when there is a prospect for positive publicity (Videras and Alberini, 2000) or enhanced brand reputation (Potoski and Prakash (2005). Eichholtz, Kok and Quigley (2010) found that green buildings can command 7% higher rental returns and a 16% higher sales price compared to comparable buildings in the same market.

This study examines whether employees would exercise effort to conserve electricity when the only beneficiary is their employer. There are many ways for individual workers to conserve energy including turning off lights, shutting down equipment, or adjusting thermostats. The firm would benefit from reduced input costs, but there is no obvious mechanism by which workers can be rewarded for their actions. Energy saving is likely to be only a small fraction of firm net revenue, and so profit sharing through share ownership, gain sharing, or other group compensation mechanisms are unlikely to provide much incentive. Individual employee actions

[3] There is only limited information on whether these efforts dissipate over time. However, Gneezy and List (2006) and Ferraro and Price (2013) found that the effect of social norms diminished over time, perhaps because the focus on social objectives shift to other concerns over time. For electricity use, Ayers et al (2009) did not find evidence of diminishing effort over the months that customers received the OPOWER Home Energy Reports.

are not observable, and so pay or punishment cannot be tied to individual employee actions. Even if an individual employee is diligent in conserving energy, his or her actions will only result in firm energy reductions if other employees also conserve. Consequently, there is a strong incentive to free ride on fellow employee conservation efforts. In such environments, will workers take the initiative to reduce energy consumption when they cannot possibly expect any pecuniary benefits?

This study proposes that if workers are motivated to lower electricity use because of environmental concerns, they may do so even without any pecuniary benefit. But they will only do so if they believe their coworkers will also be conserving so that their own actions are amplified. Applying results from high performance management strategies, [4] we explore whether informal teams can induce the information sharing and social pressure in the workplace needed to lower electricity use, even when all benefits go to the employer.

About 85% of U.S. establishments use at least some of these high performance management strategies, although only about 1% are heavy adopters (Blasi and Kruse, 2006). A common strategy is to organize workers in teams. In theory, teams are useful for sharing and disseminating information on production strategies, new technologies and other knowledge critical to firm profitability (Dyer and Nobeoka, 2000). However, teams also can lower the incidence of free riding because peers will monitor the work of their colleagues to insure effort meets the expected norm (Gollan, Poutsma, and Veersma, 2006). Hamilton et al (2003), Falk and Ichino (2006) and Mas and Moretti (2006) have found evidence that production in teams can

[4] See Bloom and Van Reenen (2011) for a recent review of this literature.

dominate individual production. And in the last two cases, apparent social pressure to produce at the level of the best performers is what motivated the added effort.[5]

Our context is a large Midwestern university campus. Individual worker conservation efforts are insignificant compared to aggregate energy use at either the building or university level, information on aggregate energy use by building is not easily available, and building occupants are not necessarily grouped naturally into homogeneous groups that would have preexisting norms or rules regarding energy use. Into that environment, we introduce three strategies that build on the past theoretical and empirical work on social norming and group conservation efforts. The first strategy is to install motivational signs that encourage conservation efforts and might provide some expectation that other workers will be expending effort as well. The second provides more frequent feedback on aggregate energy use and relates it to historical use, providing frequent updates on whether building energy use is falling, confirming that a majority of employees in the building are expending conservation efforts. The third provides information on a building's energy use compared to contemporaneous energy use at a comparable building in the form of a head to head competition. This provides information on whether employees in other buildings are conserving energy.

The experiment was implemented for one academic year and then energy use was monitored for three more years thereafter. Buildings that were assigned the competition intervention reduced their electricity use by 5.2%. Combinations of competition plus feedback lowered electricity use by as much as 7.5%. Signage alone did not significantly alter electricity use, nor did it significantly improve the performance of the other interventions.

Description of Interventions

[5] Hamilton et al (2003) find the largest productivity effects in heterogeneous teams which would be consistent with social pressure imposed by the most productive workers, but it could also be that there is a greater range of information that can be shared in heterogeneous firms.

Iowa State University presents a typical example of the challenge employers face in encouraging energy conservation by their employees. The University has about 100 buildings excluding dormitories, athletic facilities and performance venues. Energy utilization is monitored by building and not by outlet or office, and so the building represents the smallest unit of workers whose aggregate consumption can be monitored. Even that level of monitoring is imprecise. While the newest buildings have electronic reporting which can be monitored daily, the older buildings still have meters that require visual inspection which is performed monthly. Most of these meters are in locked mechanical rooms and so even if the workers decided collectively to take conservation measures, they would not be able to monitor their own usage.

Most buildings are a mixture of offices, classrooms, and laboratories. Some buildings encompass a single department, but most either house only part of a department or else have several departments. Because the natural coordinating mechanism would be by department and not by building, workers in a building will find it difficult to forge informal agreements to aggregate individual actions to lower energy consumption for the building as a whole. Even formal agreements would be complicated by the lack of common meetings at the building level rather than the department. Moreover, not only are the costs of a coordinated agreement high, the benefits from the effort are negligible. Electricity expenses are borne centrally and not at the department or building level. Consequently, any pecuniary gain from individual conservation efforts will go to the university and not the individual or the department. The only plausible incentive for an individual to voluntarily reduce electricity use is from a desire to add to the public good.

The Iowa State energy experiment represents an attempt to induce coordinated individual efforts to reduce electricity usage in this environment where workers are unlikely to reach the coordinated effort on their own. Three different interventions were randomly assigned to subsets of the 28 buildings. All three interventions could plausibly induce a change in electricity conservation behavior by addressing one or more of the reasons individuals might not reduce their electricity consumption on their own.

The first intervention, labeled *Signage*, involved placing attractive signs on publicly accessible light switches, walls and stairwells reminding individuals to use energy wisely. Other than reminding individuals to turn off the lights, the signs provided no additional specifics on how to reduce energy consumption. However, the signs could serve as a coordination mechanism that would encourage workers to presume that their individual efforts were being matched by others in the building including students and other short-term building occupants as well as permanent office residents in other departments or administrative units

The second intervention, called *Feedback*, provided each resident of the building weekly updates on the building's energy use compared to its average over the past three years. The updates were provided by Email from the University's Director of Sustainability Programs. The update included a graphical representation of the building's past week's electricity usage compared to usage for the same period averaged over the previous three years. The Email was accompanied by a brief description that described how to interpret the graph. Because information on energy use was not commonly available to building residents, and what was available only came at a considerable lag, this Emailed information would be the first information building residents would have gotten that might link their own conservation efforts to timely information on building energy. Of course, the Email would also serve as a coordination mechanism across the building's office residents but not the short–term occupiers such as students in classrooms.

The third intervention, labeled *Competition*, compared the weekly electricity usage of two buildings that were paired based on building age, types of use, and past energy use. Building residents received a weekly Email from the University's Director of Sustainability Programs that reported whether the building's weekly energy use relative to its past use was greater or less than the energy use of its paired building. The Email included a chart that tracked how the building's use compared to its rival. A building scored a win for the week if its percentage reduction in energy compared to its past usage was larger than the percentage reduction at the paired building over the same time period. A building could still win for the week if it increased energy usage provided that its paired building increased its usage by a larger percentage. The weekly update also reported how many weekly wins the building had accumulated compared to its rival. The information only included the relative energy use of the two buildings and so the residents were not told if their weekly use was higher or lower than their past use. In contrast with *Feedback*, this intervention examines whether individual behavior is motivated more by a reference to one's peers rather than a reference to an absolute standard based on past use. *Competition* should offer the same coordination mechanism as *Feedback* through the weekly Emails.[6]

[6] We did not find many examples of behavioral experiments aimed at reducing energy use at universities or firms. Exceptions include a one semester analysis of how four families responded to feedback and monetary incentives to reduce energy at the University of West Virginia (Hayes and Cone, 1977) and a one semester experiment that examined how electricity use varied between 2 dormitories given frequent feedback versus 18 given weekly feedback but subject to competitions on energy reduction. (Petersen *et al*, 2007). These studies resulted in implausibly large reductions. Neither study controlled for seasonal variation in use. Other studies at the University of Michigan (Marans and Edelstein, 2010) and at a large Midwestern University (Scherbaum *et al*, 2008) examined employee factors that contributed to self-reported energy conservation efforts in nonexperimental settings.

Experimental Design

The experiment was conducted on 28 buildings. These buildings represented 37% of the gross square feet of space and 39% of the electricity use on the Iowa State campus. They housed around 3000 employees and thousands more students and staff who spent some time in the buildings for classes or meetings. These buildings represented the most challenging environments for energy conservation efforts to occur without coordination. The buildings were constructed between 1893 and 1998. All buildings predated the installation of electronic energy monitoring and so electricity use is monitored by monthly meter reading. While these monthly readings as far back as 2000 are kept by Facilities Management, they are not routinely shared with the buildings' residents and so there is no easy way for residents to assess their own energy use. While some buildings have had renovations, none had been subjected to recent installations of new windows, doors, or other energy saving improvements that would alter the electricity usage. Absent those expensive capital improvements, the most plausible way to lower electricity use is if individual workers change how they use electricity in the building.

Six of the 28 buildings were randomly selected to serve as controls. No interventions were applied in these buildings. The remaining 22 buildings were divided into 11 pairs based on similarities in building age; building size, the research, teaching and administrative activities conducted in the building; and the building's past electricity use per square foot. For example, buildings with labs were paired with other buildings with labs because laboratories are big users of electricity. Similarly, classroom buildings were paired with other classroom buildings. The building pairs were randomly assigned to various combinations of the three interventions. Because the buildings were paired on similar features, the ones assigned to the competition intervention were expected to face similar opportunities to reduce their electricity use.

While the experiment covers thousands of employees and even more students who are occasional building users, the true unit of observation is the 28 buildings. That limits how many observations we will have for each intervention. To increase the sample size for each intervention, we randomly assigned interventions to building pairs while allowing multiple interventions. That means that a building pair could be assigned into zero, one, two or all three intervention groups. In total, there were 8 possible intervention combinations summarized in table 1. For example, a building that was randomized into both *Signage* and *Feedback* would get both the signs and weekly updates on the building's energy usage compared to its past. A building that was placed in both *Feedback* and *Competition* would get weekly updates on its energy usage compared to both its past and compared to its paired building. We opted for 2 building pairs to receive only the competition and so we have 16 total buildings that experienced the *Competition* intervention against 12 that did not. Twelve buildings received *Feedback* alone or in combination against 16 that did not. Similarly, 12 buildings received *Signage* alone or in combination against 16 that did not. We can recapture the marginal effect of each intervention from a regression of the form

$$ln\left(\frac{E_{it}}{\overline{E_{i0}}}\right) = \alpha_0 + \alpha_B \ln(\overline{E_{i0}}) + \alpha_t t + Z'_{it}\gamma_Z + [\alpha_F + T_{it}(\beta_F + \beta_{Ft}t)]F_i + [\alpha_S + T_{it}(\beta_S + \beta_{St}t)]S_i + [\alpha_C + T_{it}(\beta_C + \beta_{Ct}t)]C_i + \varepsilon_{it} \quad (1)$$

The dependent variable is the weekly electricity use for building *i* in year *t* relative to the average electricity use in that building for the same month over the 2004-2009 period, the six years preceding the experiment. Because past electricity use is available only on a monthly basis, we scale up the weekly energy use to its month equivalent. It is possible that the log change in electricity use will have a persistent component, and so we add controls for base energy use, E_{i0}, and trend *t*. A negative coefficient estimate of α_B will imply that electricity use is subject to mean reversion, while a positive value will imply that heavy electricity users will have faster growth in future electricity use. The sign of α_t will tell us if electricity use across all the buildings is rising or falling over time. The Z'_{it} is a vector of controls for subsequent building projects that are exogenous to the interventions but that could potentially raise or lower energy use in some of these buildings.

With only 28 buildings total and only 6 control buildings, it is possible that our random assignment will fail to produce similar results in the pretreatment period. We test this possibility for each of our three interventions by testing for differences between the treatment and control buildings during the six years period 2003-2009. We report these in the boxed results presented in Figures 1-3. As we discuss below, we cannot reject the null hypothesis that growth in electricity use was never significantly different between the treated and control buildings in the pretreatment period. The mean annual change in electrical use was -0.005 in the control buildings and -0.008 in the treatment buildings. The declining trend in electricity use in the control buildings will make it harder to show an impact from the interventions because they have to create an even faster decline in electricity use in the treated buildings.

Our primary interest is in the rest of the parameters. Dummy variables indicate if the building was assigned to *Feedback* (F_i); *Signage* (S_i); or *Competition* (C_i). Coefficients on these dummy variables, α_i; $i = F, S, C$; will control for any systematic tendencies in electricity use by these buildings. The treatment dummy, T_{it}, indicates whether building *i* had been subject to at least one of the interventions and equals 1 for those treated buildings from 2010 on. Treatment effects are allowed to alter both the level ($\beta_j; j = F, S, C$) and the trend growth ($\beta_{jt}; j = F, S, C$) in electricity use. Note that the estimates of β_j and β_{jt} are net of the effects of past average electricity use by building and trend growth in electricity use.

Data

Our initial data included monthly reports of electricity use by building on the Iowa State University campus over the 2004-2009 period. That provided us an aggregate electricity use for each building as well as a six-year average electricity use for each building. That background allowed us to pair buildings based on similar electricity use per square foot of space.

The experiments were conducted during the academic year. We used October to test the Email reporting systems and so we only used the data from November through the end of March. Because the university only read the meters once per month, we conducted weekly rounds to read and record each meter. These readings were used to prepare the weekly Email updates and charts used for the *Feedback* and *Competition* interventions. These weekly data were also collected for the control buildings and the buildings that received the *Signage* intervention only, but residents of those buildings did not receive any information on their building's electricity use. Because of holidays and spring break, we conducted a total of 19 readings. During these vacations, we just extended the collection period until the end of the vacation and then prorated the results to their month equivalents.

Our measures of Z_{it}' included information provided by the Facilities Management office on subsequent physical changes to buildings that could affect electricity use. One building was fitted with new lights, another began housing a supercomputer that generated sufficient heat that the air conditioner ran all winter, a third undertook new construction that closed parts of the building and a fourth had a greenhouse demolished. We controlled for these changes using dummy variables for each building in the affected time frames. Our results are virtually identical when we include or exclude these variables, but we report the results including them to eliminate any possible spurious outcomes.

A Preliminary Graphical Assessment

Before proceeding to the regression analysis, it is useful to take a bird's eye view of how the residents of these 28 buildings responded to these innovations during and after the experiment compared to their previous electricity use. Figures 1-4 present averages of $ln\left(\frac{E_{it}}{E_{\iota 0}}\right)$ for the various treated groups compared to the control buildings. E_{it} is measured in kilowatt-hours per month, so weekly values are scaled up to their implied monthly equivalent. The vertical axis is interpretable as the average percentage deviation in electricity use for the building group compared to its baseline average for the same month over the 2004-2009 period. The treatments occurred during the 2010-11 academic year as indicated by dashed lines. No treatments occurred in either the preceding or subsequent academic years.

The comparison with the control buildings holds constant common factors that might affect electricity use such as weather, breaks in the academic year, or time in the semester. As is apparent, electricity use commonly varies by about 10 percent above or below baseline depending on the year, even in the absence of any interventions. In addition, electricity use was on a modest downward trend in the years before the experiment was conducted. Both the large variance and downward trend in electricity use will make it more difficult for these interventions to alter electricity use sufficiently to pass standard tests of statistical significance.

Figure 1 displays the time paths of electricity use for all of the control buildings and of the treated buildings regardless of treatment. From 2004 through 2009, the two series differ by less than one percentage point and the differences are not significant. In 2010, the treated series falls to 2 percentage points below the controls, but the difference is not statistically significant. The period includes a sharp reduction in electricity use that is common across both treated and control buildings due to a furlough that shut down many operations on campus in the middle of the 2010 academic year. Even though energy use fell more in the treated buildings than in the control buildings over that period, the surge in variance lowered the t-statistic and so we could not reject the null hypothesis of no difference between the two series. However, the lower electricity use by the treated buildings persisted even after the experiment ended. Including the post experiment period, electricity use in the treated buildings averaged 1.3 percentage points below the controls and passes the standard test of significance.

Of course we would not expect the three interventions to have the same effect. In Figure 2, we show the average energy use in the buildings subject to *Feedback* against the control buildings. Again there is no significant difference in electricity use between the two building groups before the experiment begins. During the experiment, the *Feedback* buildings averaged 1.8 percentage points less energy use compared to the building's baseline than did the control buildings, but the difference is not statistically significant. Carrying the period through to the end of the 2013 academic year, the energy reduction difference increases to 3 percentage points and is statistically significant. As Figure 2 shows, electricity use in the *Feedback* buildings stayed low even after the experiment ended and consistently fell below the control building levels even though the controls continued the general trend toward lower electricity use that began in 2009.

Figure 3 shows the comparison between the buildings treated to *Signage* against the control buildings. Unlike the previous two figures, *Signage* offers no benefits relative to the controls. Electricity use in the buildings with signs promoting energy conservation did not decrease as much as it did in the control buildings, although the difference never differed significantly from electricity use in the control buildings.

The results for *Competition* in Figure 4 are nearly identical to those for *Feedback.* The buildings receiving weekly information on energy use compared to their paired competitor decreased electricity use by 2.7 percentage points more than did the control buildings, but again the difference fails a test of significance. However, the effect persists over time so that *Competition* buildings average 2.3 percentage points less energy than did the control buildings from 2010 on, a difference that does pass the significance test.

The graphs show that there is a *prima facie* case suggesting that *Feedback* and *Competition* may have lowered energy use modestly during the implementation and that the reduced electricity use persisted over time. However, these simple averages do not hold other confounding factors constant and we have the added problem that the treated buildings may have experienced more than one intervention. To derive comparative static estimates of the treatment effects, we turn to the regression analysis.

Regression Results:

Table 2 reports the results of our estimation of equation (1). We correct the standard errors for clustering at the building. We report three specifications. The first imposes the restriction that the treatment effects are equal across the three interventions. The estimates suggest that the interventions lowered electricity usage by 6.8% compared the building's previous usage, even when we allow for the fact that electricity was trending downward modestly at 0.4% per year.

While this estimate is promising, it is doubtful that all the interventions had the same effect. The second column allows the interventions to have an impact on the level of usage but holds trend growth at its historical level. All three interventions had at least a marginal negative effect on electricity use, but only *Competition* lowered electricity consumption significantly. Other things constant, buildings involved in a competition with a paired building lowered their electricity use by 4.9%. Having weekly *Feedback* on electricity usage lowered usage by an additional 2.6%, but the effect is not precisely estimated. The literal interpretation is that *Competition* is marginally more effective when residents are given information on their current usage relative their past use and not just their usage compared to another building. *Signage* lowered electricity use by only 0.045%, and the effect is not significantly different from zero.

The third column allows the interventions to affect both the level and the trend change in electricity use. None of the estimates of β_j or β_{jt} is statistically significant. However, we estimated the joint effect of the interventions through the level and trend terms and report those at the bottom of column 3. The estimates are quite consistent with those in column 2. *Competition* lowered electricity by 5.2% evaluated at sample means. Neither of the other two interventions had a significant effect on electricity use.

Both columns 2 and 3 allow us to test the significance of combinations of interventions. All combinations that include *Competition* lowered electricity use significantly. For example, combining *Competition* with *Feedback* lowers electricity use by 7.5%. As the cost of the combined intervention is virtually identical to the price of implementing either one alone, it would appear that the joint intervention is the most promising. Incorporating *Signage* adds additional cost with only a negligible marginal reduction in electricity use. The impact of combining *Signage* and *Feedback* without *Competition* is a 3% reduction in electricity use, but the estimate is not significantly different from zero. Apparently information alone does not alter conservation behavior – and so references to concurrent electricity use by other buildings helps to reinforce electricity conservation behavior of building residents.

When we test for the joint significance of the intervention effects interacted with trend, we cannot reject the null hypothesis that the interventions did not affect trend electricity use.

That implies that the data are most parsimoniously represented by the specification in column 2, and that the interventions caused a once and for all reduction in energy use. However, it is only the use of *Competition* that lowered electricity use during the treatment period and the effect persisted over time. Importantly, these behavioral changes come at almost no cost beyond collection of the energy use and dissemination to the residents.

The electricity reductions we found using our *Competition* intervention are larger than the 1-2% reduction in private electricity use obtained with the OPOWER experiments (Allcott, 2011; Ayers *et al*, 2009; Costa and Kahn, 2013) or the 4.8% reduction from the use of social norming in private water use (Ferraro and Price, 2013).[7] There are many reasons behavioral innovations may have greater scope for energy reductions in firms than private dwellings. First, engineering estimates suggested that electricity use at Iowa State could decrease by 15% or more just from simple acts such as turning off lights and computers at night. These estimates are comparable to those reported by the European Environment Agency (2013). As a result, there was greater scope for energy reduction from simple behavioral changes at Iowa State than would be typical of a private residence.

Second, it is plausible that, compared to private consumers, office workers were neither as informed regarding how to reduce energy use or as empowered to take action. Office workers would not know the energy consumption of office equipment they did not purchase. They may also be uncomfortable turning off lights or shutting down equipment that are not part of their assigned tasks. Furthermore, private electricity use has no coordination requirement with other households. That means that firms would be expected to have more waste in their baseline

[7] Our energy reductions are much smaller than the very large reductions in energy use reported using samples of college students (Hayes and Cone, 1977; Petersen *et al*, 2007; Delmas and Lessem, 2012), but none of these studies controlled for seasonal energy use or used broadly drawn populations that might reflect more diverse interests in conservation.

electricity use and therefore greater capacity for reducing energy usage. Past research has shown that high performance work practices that empower employees to monitor one another can raise productivity, even when there is no added compensation from the higher productivity (Falk and Ichino, 2006; Devaro, 2008; Mas and Moretti, 2009). This experiment shows that when workers are motivated to conserve, providing information on conservation efforts exerted by peers can enable workers to coordinate their conservation efforts and solve the free rider problem, even if they are not financially rewarded for their actions.

Conclusions

Belzer (2009) surveyed the available evidence on cost-effective strategies to reduce electricity consumption. Some studies are based on technical potential, meaning that they estimate the savings from instantaneous adoption based on available technologies. Other studies focus on economic potential, assuming that only cost-effective technologies are adopted and are typically installed as older technologies require replacement. His report examines the evidence of a broad array of options including upgraded ventilation, windows, building design, lighting, refrigeration, heating, and office equipment. He concluded that new lighting technologies had the largest and most cost-effective energy saving potential in existing commercial buildings. If installed where economically efficient, these technologies could lower electricity use by 3-12%. A subsequent study by Navigant Consulting (2012) stated that universal installation of new lighting technologies had the technical potential of lowering electricity use by 3.4% by 2020 and by 8.3% by 2030.

These studies do not consider the technical or economic potential of behavioral changes for energy conservation. As shown in this study, low cost behavioral interventions can lower electricity use by 5-7.5%, a savings that falls in the range of the most cost effective capital investments to reduce electricity use.

A drawback to our study is the small sample size. Despite including thousands of employees and students, the natural unit of observation is a building, and we only had 28 buildings available for the experiment. On the other hand, few employers would have that many buildings at a single site. Moreover, finding statistically significant effects with such a small experiment is even more impressive than statistical significance built on the weight of large samples. A more cautious assessment is that our findings warrant replication in other settings, the logical extension being a more powerful test that pools many such experiments across a large number of employers. Given the modest cost of these experiments compared with alternative conservation strategies requiring large capital investments, the financial risk of experiments aimed at altering energy conserving behavior must be lower than the risk associated with the much more expensive capital investment options reviewed by Belzer and Navigant Consulting.

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Table 1: Number of buildings assigned to each intervention type

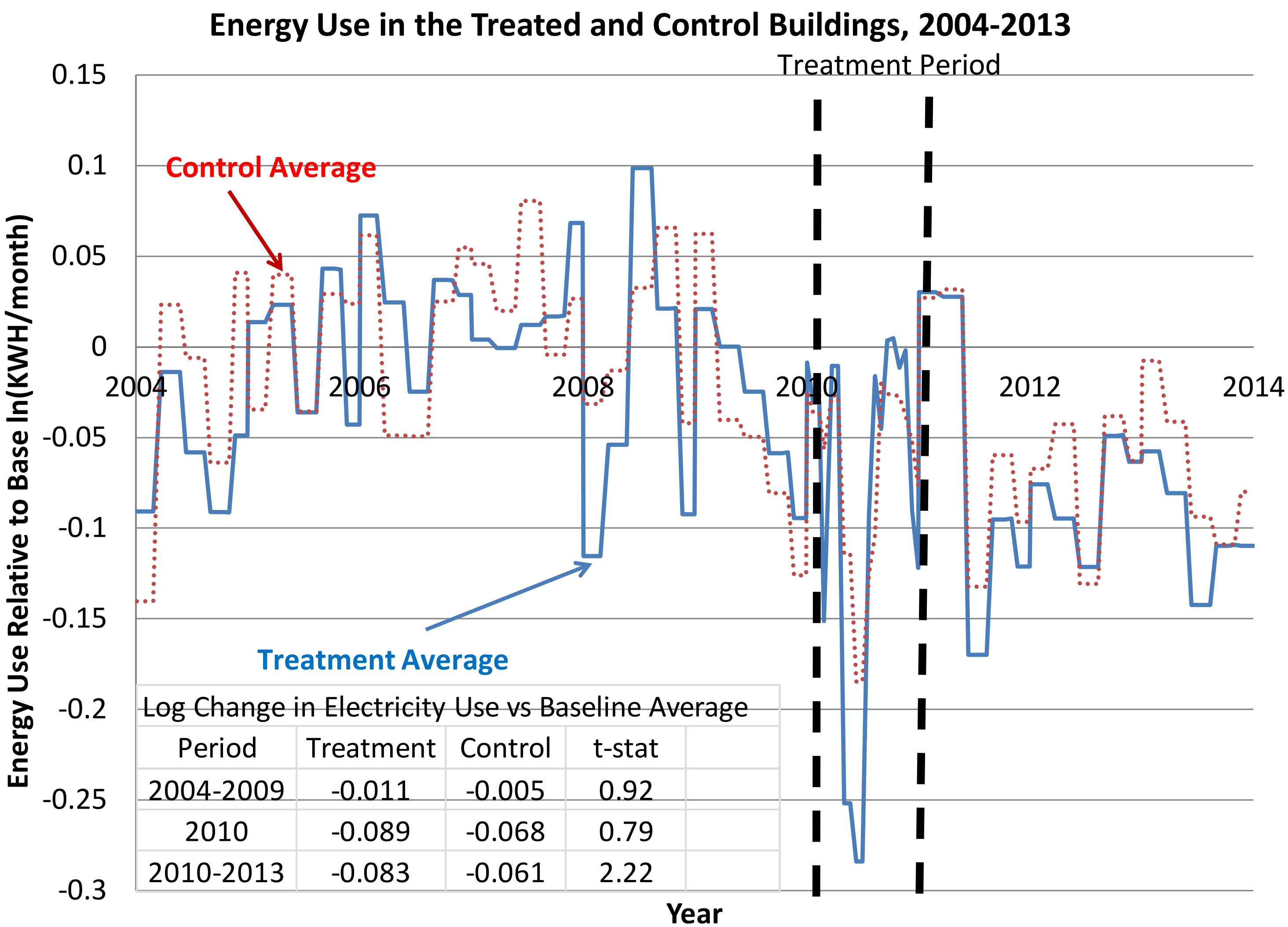
Number	Intervention
6	None
2	Signage
2	Feedback
4	Competition
2	Signage and Feedback
4	Signage and Competition
4	Feedback and Competition
4	Signage, Feedback and Competition
28	Total

Table 2: Difference in differences estimates of the impact of *Feedback*, *Signage*, and *Competition* on electricity use

Feedback	4[ab]	5[b]	6[b]
α_F	0.006	0.008	0.009
	(0.67)	(1.08)	(1.15)
β_F	-0.068**	-0.026	0.023
	(3.85)	(1.04)	(0.20)
β_{Ft}			-0.006
			(0.45)
Signage			
α_S	$= \alpha_F$	0.015**	0.0155**
		(2.11)	(2.19)
β_S	$= \beta_F$	-0.005	0.119
		(0.19)	(0.96)
β_{St}			-0.0149
			(0.98)
Competition			
α_C	$= \alpha_F$	-0.006	-0.006
		(0.56)	(0.48)
β_C	$= \beta_F$	-0.049**	-0.087
		(2.03)	(0.80)
β_{Ct}			0.004
			(0.32)
Controls			
α_B	-0.0013	-0.004	-0.004*
	(0.27)	(0.49)	(0.51)
α_t	-0.005	-0.007**	-0.006*
	(1.57)	(2.36)	(1.67)
α_0	0.015**	0.048	0.044
	(0.25)	(0.51)	(0.47)
R^2	0.14	0.14	0.14
Feedback:$\beta_F + \beta_{Ft}t$			-0.028 (1.08)
Signage:$\beta_S + \beta_{St}t$			-0.007 (0.30)
Competition: $\beta_C + \beta_{Ct}t$			-0.052** (2.14)

[a] Coefficients on the three interventions are constrained to equal one another
[b] Standard errors corrected for clustering at the building level
t-statistics in parentheses. **significance at the .05 level. * significance at the .10 level.

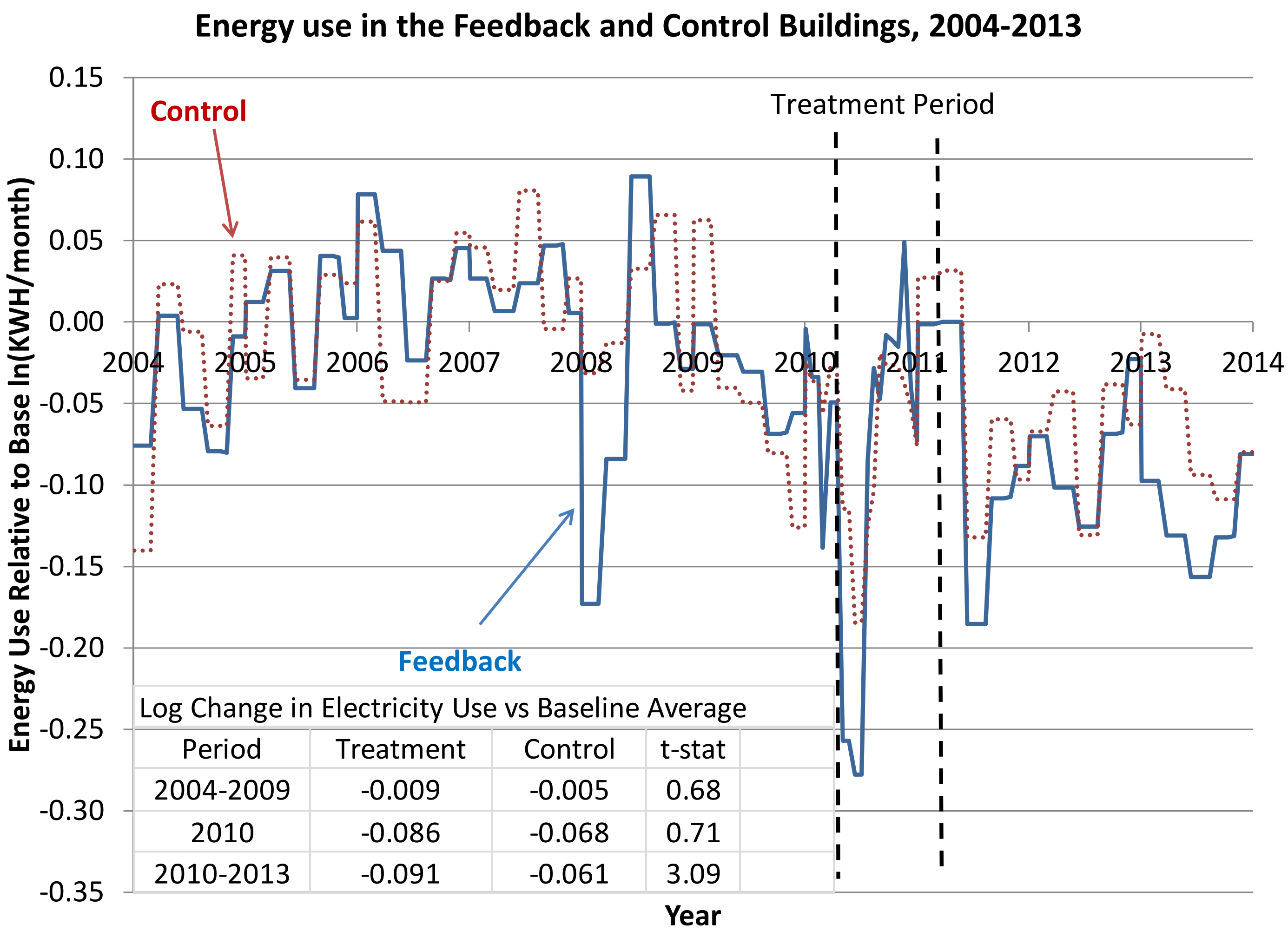
Figure 1: Treated and Control Buildings



Log Change in Electricity Use vs Baseline Average

Period	Treatment	Control	t-stat
2004-2009	-0.011	-0.005	0.92
2010	-0.089	-0.068	0.79
2010-2013	-0.083	-0.061	2.22

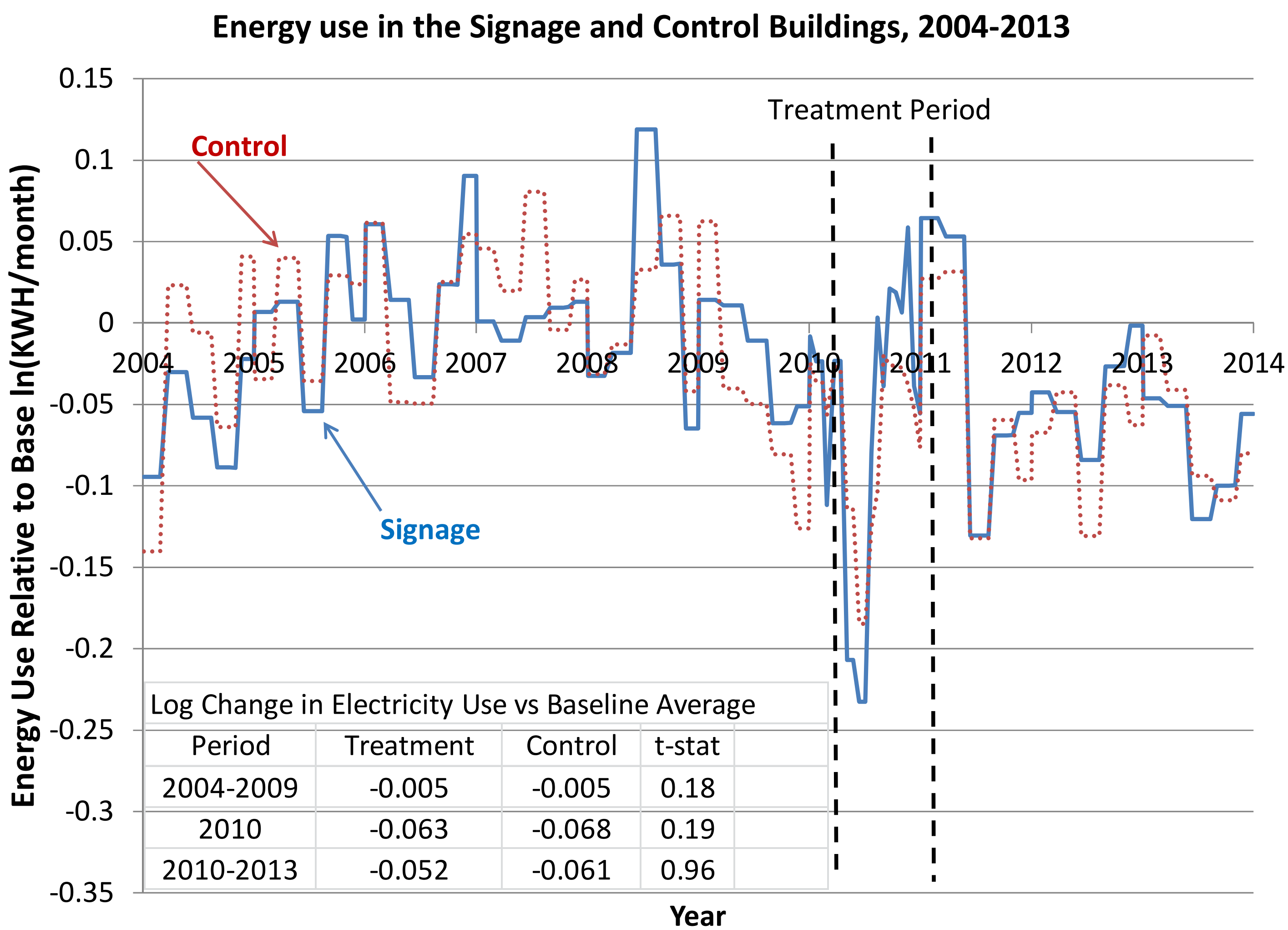
Figure 2: Feedback and Control Buildings



Log Change in Electricity Use vs Baseline Average

Period	Treatment	Control	t-stat
2004-2009	-0.009	-0.005	0.68
2010	-0.086	-0.068	0.71
2010-2013	-0.091	-0.061	3.09

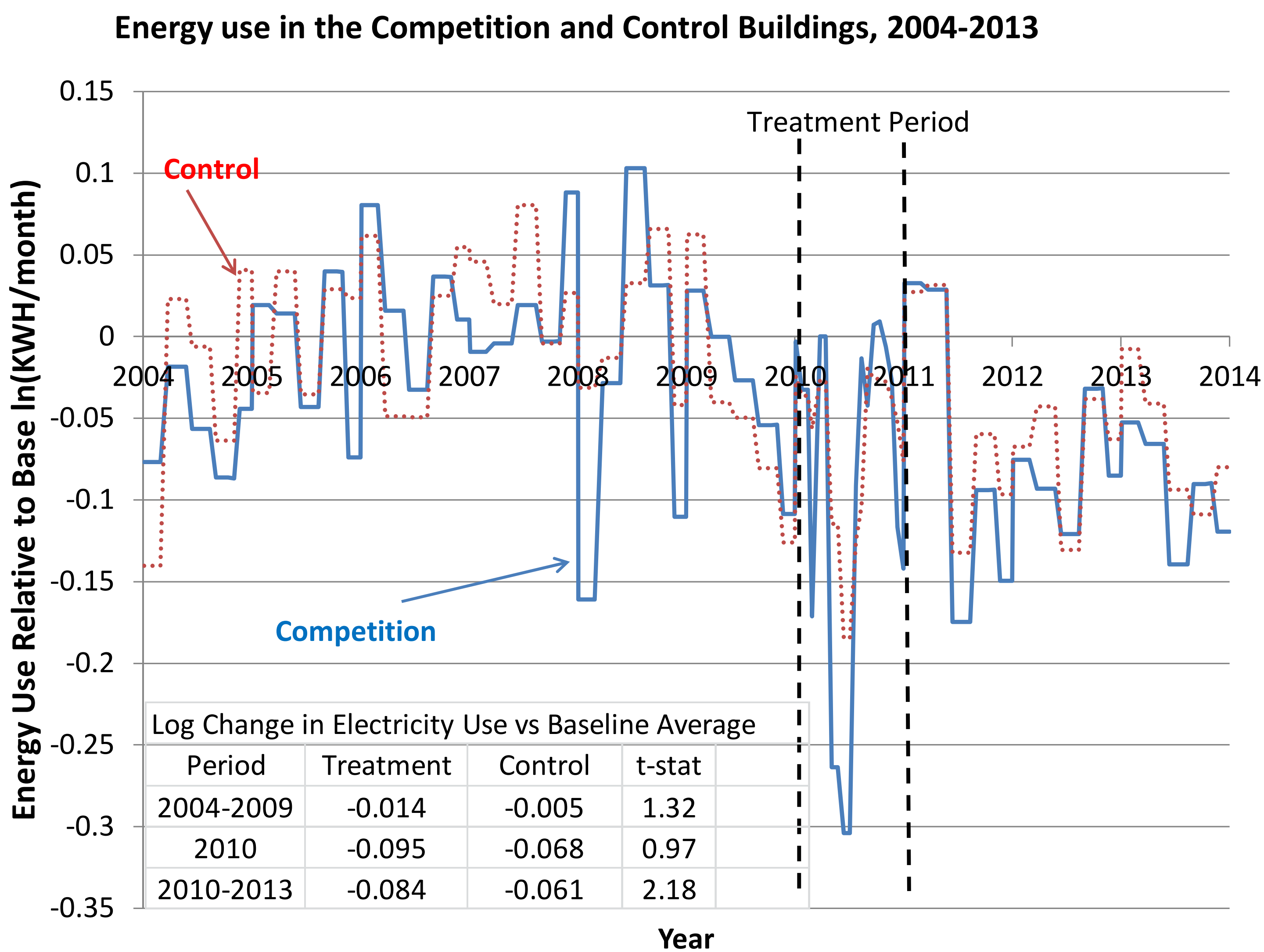
Figure 3: Signage and Control Buildings



Log Change in Electricity Use vs Baseline Average

Period	Treatment	Control	t-stat
2004-2009	-0.005	-0.005	0.18
2010	-0.063	-0.068	0.19
2010-2013	-0.052	-0.061	0.96

Figure 4: Competition and Control Buildings



Log Change in Electricity Use vs Baseline Average

Period	Treatment	Control	t-stat
2004-2009	-0.014	-0.005	1.32
2010	-0.095	-0.068	0.97
2010-2013	-0.084	-0.061	2.18