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Does Information Provision Shrink the Energy Efficiency Gap?

A Cross-City Comparison of Commercial Building Benchmarking and Disclosure Laws

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RESOURCES FOR THE FUTURE

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Abstract

Information failures may help explain the so-called "energy efficiency gap" in commercial buildings, which account for approximately 20 percent of annual US energy consumption and CO_2 emissions. Building owners may not fully comprehend what influences energy use in their buildings and may have difficulty credibly communicating building energy performance to prospective tenants and buyers. Ten US cities and one county have addressed this problem by passing energy benchmarking and disclosure laws. The laws require commercial buildings to report their annual energy use to the government. We evaluate whether the laws have had an effect on utility expenditures in office buildings covered by the laws in four of the early adopting cities—Austin, New York, San Francisco, and Seattle—and find that they have reduced utility expenditures by about 3 percent. Our view is that these estimated effects in the early days of the programs are largely attributable to increased attentiveness to energy use.

Key Words: energy efficiency, information, commercial buildings, differences-in-differences regression

JEL Classification Numbers: L94, L95, Q40, Q48

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Introduction

Missing and asymmetric information may be major factors in explaining the so-called "energy efficiency gap," the observation that many seemingly cost-effective options for reducing energy use are readily available and yet fail to be adopted by firms and individuals (Gillingham et al. 2009; Gillingham and Palmer 2014; Gerarden et al. 2015). This may be particularly true for buildings, which account for approximately 40 percent of annual US energy consumption (EPA 2014). Building owners are unlikely to fully understand the factors that affect energy use and probably have only partial information about how to make cost-effective improvements. They may also find it difficult to credibly communicate building energy performance to prospective renters and buyers, which reduces incentives to invest in improvements.

Ten US cities and one county have addressed this problem by passing energy benchmarking and disclosure laws. The laws require commercial and sometimes multifamily residential buildings to report their annual energy use to a government agency, which usually makes the information available to the public.[1] Building owners are also required to benchmark their energy use to other, similar buildings; this requirement is typically met through use of a US Environmental Protection Agency (EPA) software program called Portfolio Manager. Advocates for these policies believe that having energy information more readily available to market participants, including prospective tenants and buyers, should lead to higher rents and sale prices for more efficient buildings, spur building owners to improve their buildings' energy performance, and ultimately move the market toward greater energy efficiency (Burr et al. 2012; Cox et al. 2013; DDOE 2014). Several studies have found that commercial buildings that are

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[1] In a few cities, the information is not available to the general public but to tenants, buyers, and lenders.

Energy Star or LEED certified have higher rents and sale prices (Eichholtz et al. 2010, 2013), but certification programs provide limited information, noting only whether a building's energy characteristics lead to an indexed "score" that is above or below the threshold determined by the certification program. Moreover, certification may be correlated with other desirable characteristics; this is particularly true for LEED certification, which covers many other "green" characteristics beyond energy use. Walls et al. (2013) find that the capitalization effect of home certification on sale prices appears to outweigh the implicit energy savings in the certification, suggesting buyers are valuing other characteristics or misinterpreting the signal sent by the label. Houde (2014) finds misunderstanding of ENERGY STAR labels on appliances is common—in a study of refrigerator purchases, some consumers appear to overvalue the energy savings while others undervalue. Benchmarking and disclosure ordinances would go further than certification in two ways: first, by providing energy use information and ratings for *all* buildings covered by the law, not just buildings that are voluntarily certified; and second, by providing actual energy use and not just an indicator of being above or below a threshold.

The laws may have an effect on energy use in another way that is more immediate. The simple act of reporting may lead building owners to be more attentive to energy use and costs. The problem of inattention has been proposed as a potentially important reason for the efficiency gap (Sallee 2014; Gillingham and Palmer 2014; Allcott et al. 2014; Palmer and Walls 2015a). Sallee (2014) estimates that between 12 and 19 percent of consumers purchase a vehicle that they would not have purchased had they had full information on fuel costs. Sallee argues that in some settings, however, obtaining full information is too costly; that is, consumers are "rationally inattentive." Jessoe and Rapson (2014) show that providing information about real-time electricity use can dramatically increase the price elasticity of demand among residential consumers. Economists have also analyzed inattention and salience in other contexts, including sales taxes (Chetty et al. 2009), shipping and handling costs (Brown et al. 2010), odometer readings and used car prices (Lacetera et al. 2012), and electronic road tolls (Finkelstein 2009).

Benchmarking of energy use adds another dimension, as it provides building owners with a metric comparing their buildings' energy performance with that of similar buildings. The effects of peer comparisons on energy use in the residential context have been studied by Allcott (2011), Ayres et al. (2013), and Costa and Kahn (2013), all of whom assess the effectiveness of Opower's Home Energy Report programs. These programs send randomly selected homeowners a report comparing their home's energy use with that of a group of similar nearby homes. These studies typically find that receiving a home energy report leads to an average 1–2 percent reduction in short-term energy use among consumers who receive them. To our knowledge, there are no similar studies of the effects of peer comparisons on energy use in commercial buildings.

In this study, we evaluate whether benchmarking and disclosure laws have had an effect on utility expenditures in commercial office buildings. As it is still in the early days for most of these laws, our view is that our results involve searching for an attentiveness effect: Has reporting on energy use and benchmarking relative to others led to reductions in utility bills? To answer this question, we use data from the National Council of Real Estate Investment Fiduciaries (NCREIF), a member-based organization that represents the institutional real estate investment community. The data include quarterly information on property characteristics, such as square footage and age, and operating expenditures, including utility expenditures for a panel of commercial properties across the United States. We estimate a differences-in-differences (DID) model on office buildings in the NCREIF dataset, comparing utility expenditures per square foot before and after the laws go into effect in treated and untreated buildings across the NCREIF sample. Our four “treatment” cities are the early adopters: Austin, New York City, San Francisco, and Seattle. Because we have quarterly observations on individual buildings, we are able to match buildings to themselves before and after treatment through a property fixed effects specification.

We find that disclosure laws have had a negative effect on utility expenditures. In our central specification, which includes a large set of buildings in cities across the country as controls, average utility expenditures per square foot are approximately 3 percent lower in buildings covered by the laws. The finding is fairly robust to alternative specifications and samples of control buildings, though the precision of the estimates varies. When we limit the sample of control buildings to those in cities that either have adopted disclosure laws after the time period of our study or are actively considering such laws, we continue to find a negative effect of the laws, but it is statistically insignificant. Although this sample may represent a closer match to the four treatment cities where laws are in effect, it is quite small. A model estimated using a similar but slightly larger sample that includes buildings in the surrounding metro areas of these cities yields a statistically significant negative effect of the laws that is close to that with the full sample. We find some heterogeneity in the impacts across cities. The percentage reduction in utility expenditures is slightly larger in Seattle, where average utility costs are substantially lower than in the other cities. In a series of falsification tests, we estimate regressions with false treated cities and time periods and find no effect from these “placebo” treatments.

Our findings add to the growing literature on the potential of information provision to overcome the energy efficiency gap. To date, this literature has focused overwhelmingly on residential energy use. Our study is one of the first to examine the role of information in a commercial building context and the first to examine the effects of a new policy growing in popularity, energy benchmarking and disclosure. Results suggest that the policies appear to be having an effect on utility expenditures in investor-owned commercial office buildings. The short-run effects that we identify are likely due to increased attentiveness; as the programs mature, it will be important to continue to analyze their longer-run impacts.

We begin with an overview of benchmarking and disclosure ordinances in the early adopter cities, followed by a discussion of the challenges, primarily related to limited data availability, associated with assessing their effects on energy consumption. Next, we describe the building-level characteristics and utility expenditures data that we use in our analysis and the additional data that we merge with them. We then present our regression results and our associated findings on program effects, along with a series of robustness checks and falsification tests. We follow with discussion of limitations and ideas for future research. The final section provides some concluding remarks.

Description of Benchmarking and Disclosure Policies

Local benchmarking and disclosure laws are a recent phenomenon. The first law was passed in Washington, DC, in August 2008 but was not fully implemented until 2013 due to delays in the regulatory process. Austin, Texas, was the next city to adopt, in November 2008, followed by New York City a year later. Seattle and San Francisco followed suit by enacting laws in February 2010 and February 2011, respectively. The first reporting date was October 2011, for buildings in San Francisco. These early adopter cities, with the exception of Washington, are the focus of our analysis.2 Since May 2012, five additional cities—Philadelphia, Minneapolis, Chicago, Boston, and Cambridge, Massachusetts—and one county, Montgomery County, Maryland, adopted policies of their own. A number of other cities are actively considering a benchmarking policy as of March 2015, among them Portland, Oregon; Atlanta, Georgia; and Kansas City, Missouri.

[2] We exclude Washington, DC, from our analysis because the long lag period between passage of the law and mandated reporting complicates the definition of an effective date.

The benchmarking and disclosure laws passed in the four early adopting cities that are the focus of this analysis all bring a building's energy use to the attention of its owners and occupants, as well as to potential tenants or new owners and those who might finance any real estate transactions or property investments. Key parameters of these four laws are summarized in Table 1.[3]

Table 1. Benchmarking and Disclosure Ordinance Provisions in Four Cities

City	*Enactment date*	*Covered buildings: commercial*		*Covered buildings: multifamily*		*Disclosed to*
		Size	***Initial reporting date***	***Size***	***Initial reporting date***	
Austin	11/08	≥75K sf	6/12	≥5 units and ≥10 yrs. old	6/11	Government Buyers Tenants[a]
		≥30K sf	6/13			
		≥10K sf	6/14			
New York	12/09	>50K sf	12/11[b]	>50K sf	12/11	Government Public
San Francisco	02/11	≥50K sf	10/11			Government Buyers Tenants Leasers/lenders Public[c]
		≥25K sf	4/12			
		≥10K sf	4/13			
Seattle	02/10[d]	≥50K sf	4/12	≥50K sf	10/12	Government Buyers Tenants Leasers/lenders
		≥20K sf	4/13	≥20K sf	4/13	

[a] Only multifamily buildings must report to tenants or prospective tenants.
[b] Original date was May 2011 but was pushed back to December 2011.
[c] Only summary statistics in San Francisco publicly disclosed initially; disclosure for individual buildings phased in over time by building size, and as of April 2013, public disclosure for all buildings over 10,000 square feet.
[d] Seattle passed an amendment to the ordinance in September 2012 that raised the size threshold from 10K sf to 20K sf and restructured enforcement.

In all four cities, building owners are required to report building energy use to the relevant government agency. In two cases, New York and San Francisco, public disclosure is also mandated, although for San Francisco, public disclosure was phased in over time. All of the programs cover commercial buildings, although the minimum building size at which the requirement ultimately takes effect varies across the cities, ranging from 10,000 square feet in

[3] For more information about the policies adopted prior to November 21, 2014, including additional provisions of the laws, see Palmer and Walls (2015b).

San Francisco and Austin to 50,000 square feet in New York. In most of the cities, buildings have been, or are being, phased in over time by size, with the largest buildings required to report first. Three of the four ordinances require multifamily residential building owners to report as well, with reporting thresholds based on building size (New York and Seattle) or number of units and age (Austin).

All of the cities have very similar reporting requirements.[4] Building owners or their energy providers are required to submit monthly electric and natural gas bills (as well as other energy purchases and purchases of district steam) and certain building characteristics, including gross square footage, year built, and operating hours, to the administering agency in the city. (New York requires reporting of water usage as well.) For benchmarking energy use to other buildings, most of the ordinances require (and all allow) the use of EPA's Portfolio Manager (PM) software program.[5] The information that is disclosed to the relevant parties varies somewhat across cities but generally includes, at a minimum, energy use intensities (EUIs) and ENERGY STAR (or other) benchmarking scores.[6]

Assessing Causation: Policy Evaluation and Data Shortcomings

The data that building owners are required to report may be useful for understanding building energy use across a set of buildings covered by the law, but they are not sufficient for assessing the causal effects of the benchmarking and disclosure policy. Most cities are collecting a year or two of pre-treatment energy use data for the buildings subject to the laws, but they are not publicly disclosing this information. Moreover, it is now widely recognized that simple before and after comparisons for treatment groups will produce biased estimates of policy impacts (Angrist and Pischke 2010).

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[4] Austin, San Francisco, and New York also have periodic energy audit requirements, and New York and San Francisco require retrocommissioning for buildings that do not meet the minimum level of performance. Retrocommissionsing involves a systematic process for identifying inefficiencies and improving the functioning of equipment, lighting, and control systems in buildings.

[5] PM is an online tool that is used to determine scores for purposes of Energy Star certification for buildings. For a short overview of how PM works, see Palmer and Walls (2015b).

[6] Energy Star scores are based on measures of "source" energy use intensity rather than "site" energy use intensity; the distinction is relevant mainly for electricity, where the source of fuels used to generate electricity is accounted for in the source EUI calculation.

The inclusion of building size thresholds in the design of municipal benchmarking and disclosure laws creates a natural experiment that provides a well-defined control group for assessing program effects. Buildings that fall just short of the minimum size threshold are presumably similar to those just above the threshold. Thus one could use a regression discontinuity approach to compare energy use before and after the policy takes effect between these two groups of buildings, controlling for other factors such as weather (Imbens and Lemieux 2008).

Conducting such an analysis requires energy consumption data beyond that collected under the policy, however. A time series of energy use data prior to implementation would be needed for buildings below and above the minimum size threshold. No data are currently being collected for those buildings below the threshold. Electric and natural gas utilities will have these data but are typically reluctant to share with academic researchers, both because of the costs it imposes on them and because of privacy concerns. Moreover, challenges would remain even with utility-level data. Electricity and natural gas billing data are meter-specific and not necessarily building-specific. Identifying individual buildings from meter data and then matching with other data on building characteristics from, say, a local government property tax assessment office will often be quite challenging.

Similar hurdles exist for another approach, a differences-in-differences (DID) regression that would compare treatment buildings pre- and post-treatment with a set of similar control buildings pre- and post-treatment from other municipalities (Meyer 1995). As the control buildings would be from a city (or cities) without a benchmarking law, they would not be required to disclose their energy use, and one would need to turn to utilities or other independent sources.

Such independent sources are not widely available for a large enough set of buildings to perform statistical analysis. The CoStar commercial property data, which have been used in a number of academic studies (Eichholtz et al. 2010, 2013), do not include information on energy use. The US Department of Energy's Commercial Building Energy Consumption Survey (CBECS) is a snapshot of commercial building energy use at a point in time; furthermore, the current dataset is from the survey conducted in 2003 and thus is quite out of date.[7] Private data analytics companies are starting to collect energy use data at a fine temporal resolution (sometimes 15-minute interval data) for properties for which they are providing energy management services, but so far these are small datasets and not generally available to the research community. EPA now reportedly has a dataset of 400,000 commercial and multifamily properties that have used its Portfolio Manager benchmarking software, either voluntarily or because of benchmarking laws. This dataset includes information on building type, size, occupancy, and monthly energy consumption by type, among other variables. The US Department of Energy has incorporated EPA's data into a larger crowd-sourced dataset on building energy use for more than 790,000 buildings, the Building Performance Database, which it makes available in an anonymized form for graphing and construction of aggregate tables through a public access website. However, to our knowledge, neither of these datasets has been made widely available to researchers for hands-on analysis.[8] Moreover, they may not serve as useful controls in an analysis of benchmarking ordinance effects, because by definition these buildings have already reported data on energy use, and many of them have undergone a benchmarking exercise.

Of course, data challenges for rigorous program evaluation are nothing new. However, we feel it is important to point them out here as benchmarking and other policies targeting the commercial building sector proliferate in US cities. Having the right data to assess the energy savings from these programs is critical. In the next section, we describe the data we have available to analyze the impacts of benchmarking laws, data that have some strengths and weaknesses, followed by a discussion of the DID regressions we perform.

Description of Data

Our empirical analysis combines data on individual buildings with zip code–level data and some county-level data on several other variables related to building energy use.

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[7] Data from a 2012 CBECS are currently being process by the Energy Information Administration (EIA), with a release expected in the spring of 2015. While the number of buildings surveyed in this latest CBECS is 28 percent higher than in the prior survey, the dataset still contains a small number of buildings for any particular city and does not track them over time.

[8] For more information about the Building Performance Database, see http://energy.gov/eere/buildings/building-performance-database.

Building Characteristics and Utility Expenditures

The National Council of Real Estate Investment Fiduciaries (NCREIF) is a member-based nonprofit association that represents the institutional real estate investment community. Since 1979, NCREIF has maintained a property dataset from its members that includes quarterly information on income and cash flow, property valuation, capital improvement expenditures, operating expenditures, and other information. The size of the dataset has grown over time, with more than 30,000 properties included as of mid-2013. Since 2000, the NCREIF dataset has included quarterly utility expenditures for individual buildings and building characteristics such as size, number of floors, and age. The dataset also includes information on building location, including zip code, core-based statistical area (CBSA), and county and city information, which can be used to match with data from other datasets. The NCREIF data have been used in a number of academic papers, but those have been mainly focused on the widely cited NCREIF Property Index and issues related to real estate market trends (see Geltner and Goetzmann 2000 and Li et al. 2009, for example). Use of the individual property-level data is less common and to our knowledge, this is the first use of the NCREIF data in a study related to energy use and policy.[9]

The NCREIF data have some strengths and weaknesses for our analysis. On the one hand, the dataset provides an independent source of information on commercial properties and their utility expenditures. Buildings in cities with and without benchmarking laws, before and after laws were adopted, are in the dataset. Moreover, other characteristics of the properties are available as well, so there is no need to find and match with additional sources of data on the properties. The NCREIF data are limited to investor-owned properties, however, and thus sole proprietorships, limited liability companies (LLCs), and other owner types are not included. More important, only institutional owners who are members of NCREIF are included. And membership fluctuates over time, so we do not have a balanced panel of buildings over the time period of the study. Finally, while the dataset is large, it is still not large enough to investigate heterogeneity in the laws' effects by, say, building size, utility expenditures, and other factors that could be important.

We focus on office buildings because they are the most homogeneous types of commercial buildings and represent the largest share of buildings in the NCREIF data. Our

[9] Cannon and Cole (2011) use the individual property data to assess whether commercial real estate appraisals accurately reflect sale prices.

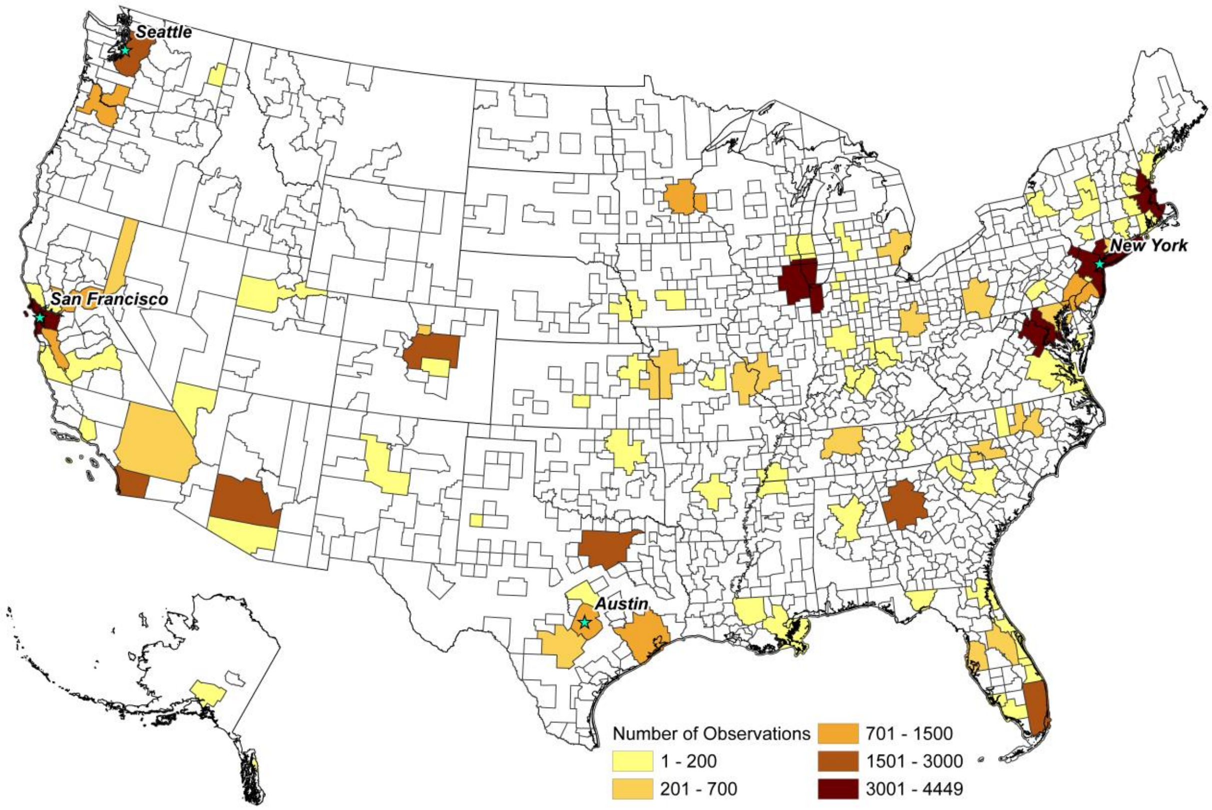
variable of interest is quarterly utility expenditures per square foot of floor space. The utility expenditures variable combines expenditures on natural gas, electricity, and water and sewer, so it is more comprehensive than just energy alone. However, water and sewer costs are typically on the order of 10 percent of overall utility costs (Romani et al. 2009). We take a number of steps to assess and ensure the consistency of the building-level data over time and to purge the dataset of outliers, including very small and very large buildings and extreme values of utility expenditures, and observations with missing values for key variables. The data cleaning and trimming steps are described in the appendix. We also convert utility expenditures to inflation-adjusted 2000$ using the all-items consumer price index (CPI) published by the Bureau of Labor Statistics. Finally, as explained above, we drop buildings in Washington, DC, because of the ambiguity associated with assigning them to the treatment or control categories.

Buildings can come in and out of the dataset (based in part on fluctuating NCREIF memberships and in part on our data cleaning), and thus our sample is not a balanced panel of buildings. Over the period from first quarter of 2003 through third quarter of 2013, we have a total of 56,277 observations from 3,937 individual office buildings.[10] Figure 1 shows the geographic location of buildings in our full sample by US Census defined core-based statistical areas (CBSAs), with the four treatment cities denoted with a star. As the map makes clear, the sample is fairly dispersed across the country, with a greater number of observations in the larger CBSAs and in our treatment cities.

We match the NCREIF property data with data on electricity prices; temperature data, which we use to create heating degree-days (HDD) and cooling degree-days (CDD); and local unemployment data, as a measure of local economic activity.

[10] We use data that start in 2003 because prior to that date, the number of observations for buldings that are eventually treated and for which we have data on utility expenditures is quite small.

Figure 1. Location of Office Buildings in Our Data Sample, by CBSA



Electricity Prices

The electricity prices are from the US Energy Information Administration (EIA) and are quarterly retail prices per megawatt-hour ($/MWh) for commercial end users constructed from the monthly data on revenues ($) and electricity sales (MWh) collected on EIA form 826 for most utilities in each state. These monthly data were converted to quarterly average prices.[11] Electricity prices were then mapped from the utility service territory level to the zip code level based on a correspondence of zip code–level maps and utility service territory maps. If a zip

[11] For those utilities that are not reported separately in the 826 database, annual utility-specific data from EIA form 861 was used and then adjusted for differences across seasons within the year using the data for "rest of state" from EIA form 826. This adjustment was possible only through the end of 2012, as the 2013 annual data are not yet available from EIA. For buildings in those utility service territories without monthly electricity price data, our data stop at the end of 2012.

code was split between two or more utilities, the price was based on the average weighted by area within zip code covered by each utility.[12] We use the CPI to convert all prices to 2000$.

Unemployment

Quarterly unemployment data at the county level from the US Bureau of Labor Statistics' Local Area Unemployment Statistics database are included to capture general economic conditions.[13] This dataset is constructed based on information from the Current Population Survey, the Current Employment Statistics program, and state unemployment insurance systems.

Weather

Quarterly heating and cooling degree-day counts by zip code are created using temperature information from the NASA Land Process Distributed Active Archive Center. These eight-day averaged land surface temperature data are available at a 1-square-kilometer spatial resolution; we use zip code overlay maps to create zip code averages.[14] HDD and CDD are calculated as the number of degree-days in the quarter that the average daily temperature is below or above 65 degrees Fahrenheit, respectively.

Table 2 shows summary statistics for selected property characteristics, electricity prices, heating and cooling degree-days, and unemployment rates. The table includes means and standard deviations for observations from buildings that are never treated (i.e., never subject to the benchmarking and disclosure laws) and buildings that are eventually treated (after the disclosure laws are in place), for both pre-treatment periods and the treatment period. The laws were adopted at slightly different times in the four treatment cities. For the eventually treated buildings, we use a treatment date of the first quarter after building owners were required to

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[12] We also collected and matched data on natural gas prices—annual average prices of natural gas delivered to commercial customers by local distribution companies and marketers—with the NCREIF building data. The data are from EIA form 176 and mapped to the zip code level using overlapping maps. However, the natural gas price data are not as up-to-date as the electricity price data, and thus we are not able to use them in our regressions without losing some observations, so we focus on electricity prices.

[13] See http://www.bls.gov/lau/.

[14] For more information on the temperature data, see https://lpdaac.usgs.gov/products/modis_products_table.

report;[15] for the never treated buildings, we choose to show summary statistics for pre- and post-first quarter 2012 time periods, as this is the first of the treatment dates.

Table 2. Descriptive Statistics for Eventually Treated and Never Treated Office Buildings, Pre- and Post-treatment Time Periods: Mean [Standard Deviation] (observations)

	Never treated		Eventually treated	
	Pre-2012 Q1	**Post-2012 Q1**	**Pre-treatment**	**Post-treatment**
Utility expend per sq ft ($)	0.40 [0.25] (45,576)	0.36 [0.20] (7,940)	0.53 [0.31] (2,048)	0.47 [0.26] (713)
Floor space (sq ft)	225,044 [178,298] (45,576)	222,100 [178,149] (7,940)	324,118 [210,027] (2,048)	333,106 [234,631] (713)
Year built	1987 [14] (44,444)	1990 [14] (7,832)	1965 [33] (2,031)	1962 [35] (679)
Number of floors	7.13 [8.22] (45,576)	6.72 [7.48] (7,940)	15.90 [14.12] (2,048)	16.80 [13.12] (713)
HDD	476.82 [792.04] (45,576)	367.81 [679.17] (7,940)	518.01 [682.60] (2,048)	424.44 [615.07] (713)
CDD	1,329.63 [1,141.02] (45,576)	1,519.19 [1,187.46] (7,940)	893.01 [899.35] (2,048)	933.85 [827.10] (713)
Unemployment rate	6.41 [2.40] (45,576)	7.77 [1.50] (7,940)	6.98 [2.12] (2,048)	7.38 [1.12] (713)
Percent leased	85 [16] (45,576)	83 [17] (7,940)	89 [14] (2,048)	89 [14] (713)
Electricity price ($/MWh)	88.72 [27.97] (45,576)	84.37 [25.09] (7,940)	101.93 [42.12] (2,048)	106.40 [39.27] (713)

15 In alternative specifications, we assume that treatment occurred with passage of the law. This allows us to include a wider set of cities in our sample, as several cities have adopted benchmarking and disclosure laws that had not taken effect by the end date of our sample, which is the third quarter of 2013. However, we feel that the date at which building owners must comply with the law is a more appropriate measure of treatment, especially given that there can be a fairly significant delay between the date of law passage and the date of first reporting.

Table 2 reveals that, in general, buildings in the treatment group tend to be larger, taller, and older and have higher utility bills per square foot than those in the control group. Our property fixed effects specification, which we describe below, will control for these differences. In the next section, we describe our test for common trends in pre-treatment utility expenditures per square foot in treatment and control buildings.

Empirical Model and Results

Our basic estimation approach is a DID regression model. In our preferred specification, we use property fixed effects to purge the effects of time-invariant building unobservables. As a consequence, we do not include building characteristics, as they are subsumed in the fixed effects, though in alternative specifications in the next section, we replace the property fixed effects with some property characteristics and zip code–level fixed effects. We are able to include one property characteristic that varies over time: the percentage of the building that is leased. We also include a set of explanatory variables that vary by location and over time and may affect utility expenditures; as described in the data section above, these variables are related to weather, energy prices, and local economic conditions. Finally, to control for temporal trends, we include quarter-by-year fixed effects. Our primary specification is shown in equation 1.

$$\ln y_{ict} = \beta_0 + \boldsymbol{X}_{ct}\beta_1 + \beta_2 \%leased_{ict} + \beta_3 T_{ict} + \alpha_{ic} + \gamma_t + \varepsilon_{ict} \qquad (1)$$

In the equation, y_{ict} is real utility expenditures per square foot in building i in city c at time t (t is year-quarter); the vector $\boldsymbol{X}_{ct}$ includes heating degree-days, cooling degree-days, and the inflation-adjusted electricity price, all available at the zip code level, and the unemployment rate for the county in which the city is located; T_{ict} is the treatment group indicator that equals 1 for buildings above the size threshold in cities with the laws during the periods when the law is in effect; α_{ic} is individual property fixed effects and γ_t is quarter-year fixed effects; and ε_{ict} is an idiosyncratic error term. In all regressions, standard errors are clustered at the CBSA level to allow for arbitrary correlation across time and buildings within a CBSA.

The coefficient β_3 is our primary coefficient of interest. In order for this coefficient to be an unbiased estimate of the effects of the disclosure policies on utility expenditures, we assume the change over time in utility expenditures per square foot in buildings used as controls is an unbiased estimate of what would have happened in the treated buildings in the absence of treatment. One way to test the reasonableness of this assumption is to see if the control buildings and the eventually treated buildings follow similar trends in the pre-treatment time period. Following Galiani et al. (2005), we test for common trends by estimating a modified version of equation (1) using only data from the pre-treatment period for treated buildings and data from the entire time period for the control buildings. In the modified regression, we include all the covariates and the building-level fixed effects, but we exclude the treatment variable. We also include year dummies as well as year dummies interacted with an indicator variable that is equal to one if a building is ever treated. We then test for the significance of the coefficients on these interaction terms. Table A1 in the appendix shows the results of this regression. The coefficients on the interaction terms are insignificant in each year, and thus we cannot reject the hypothesis that utility expenditures in these two sets of buildings in our sample (never treated and eventually treated) had identical time trends during the pre-treatment periods.

Table 3 reports the results of our DID regressions. The dependent variable is *ln*(real utility expenditures per square foot + 1). We add 1 before taking the log in order to reduce the skewness in the distribution of the logged variable that would result from observations with very small values of utility expenditures per square foot, as some buildings report spending substantially less than $1 per square foot on utilities in some quarters.

The results indicate that treatment has a significant negative effect on the transformed utility expenditures variable. Transforming the estimated coefficient to obtain a percentage change requires multiplying the coefficient estimate by the ratio [*(average value of real utility expenditure per square foot + 1)/average utility expenditures per square foot*] for treated buildings. This leads to an average –2.9 percent average treatment effect on the treated (ATT) estimate—that is, all else equal, on average, buildings covered by disclosure laws have approximately 3 percent lower utility expenditures per square foot after the laws are passed.

Most of the other variables in the model have the expected effects. HDD, CDD, the price of electricity, and the percentage of the property that is leased all have a positive and significant effect on utility expenditures per square foot, as expected. The local unemployment rate also has a positive effect on utility expenditures, though it is significant only at the 10 percent level.

Table 3. Regression Results

Variables	*ln*(real utilities per sq ft +1)
T_{ict}	-0.0105***
	(0.00229)
HDD	2.44e-05***
	(2.82e-06)
CDD	8.61e-06***
	(2.63e-06)
Unemployment	0.00234*
	(0.00137)
Percent leased	0.0853***
	(0.00955)
Real electricity price	0.00174***
	(0.000107)
Constant	0.0417***
	(0.0148)
Year*Quarter FE	Yes
Property FE	Yes
Observations	56,277
R-squared	0.189
Number of properties	3,937

* $p < .10$; ** $p < .05$; *** $p < .01$. Standard errors in parentheses.

Additional Specifications

Alternative Samples

This initial analysis uses a sample that includes buildings across the United States. While the NCREIF sample is a relatively homogeneous group—all institutional real estate investors that are members of a national association, and most located in urban areas, as Figure 1 shows—and we have limited the sample to office buildings, the properties are located in a diverse set of cities. Some of these cities may differ from the treatment cities in their propensity to adopt disclosure laws or other energy efficiency policies. If this propensity to adopt an ordinance is correlated with some unobservable variable that also has an effect on building-level energy use, then the coefficient on the treatment variable may be biased, and it is difficult to know if that potential bias is positive or negative.

We thus reestimate equation (1) on two more restrictive samples of buildings.[16] In the first, we use as control observations only buildings in cities that either have passed disclosure laws that are not yet in effect or are considering such laws through their participation in the City Energy Project.[17] This sample is one-fifth the size of our sample underlying the results in Table 3, and thus our second alternative sample expands this group somewhat to incorporate properties located in the surrounding metro areas as well as the cities themselves. In the first alternative sample, we thus use buildings located in Atlanta, Boston, Cambridge, Chicago, Denver, Houston, Kansas City, Los Angeles, Minneapolis, Orlando, Philadelphia, and Portland, Oregon, as well as Montgomery County, Maryland. In the second sample, we use buildings located in the CBSAs that contain these cities. We continue to include property-level and year-quarter fixed effects. Results are reported in Table 4.

In both regressions, we retain a negative coefficient on the treatment variable. In the CBSA sample, the estimated effect is statistically significant at the 1 percent level and the magnitude of the effect is approximately the same as with the full sample: transforming the coefficient yields an ATT of approximately –2.6 percent. In the more restricted city sample, the magnitude of the effect drops to –0.5 percent and is no longer statistically significant at the 10 percent level. The much smaller sample size using only the limited number of cities contributes to imprecision in the estimates, but it is possible that the results for this more targeted sample better reflect the effects of the laws to date. However, the similarities between the full sample results and the results for the more restricted CBSA sample give some assurance that these ordinances are having an effect and that selection issues associated with the local jurisdiction's propensity to adopt an ordinance may not be as confounding as one might expect.[18]

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[16] We test for common pre-treatment trends in these two samples, using the same approach described above and shown in Table A1 in the appendix. We reject that the trends differ in these two samples as well.

[17] The City Energy Project is an intiative run by the Natural Resources Defense Council and the Institute for Market Transformation that helps cities adopt and implement a variety of building energy efficiency policies, including benchmarking and disclosure laws. The initiative currently operates in 10 cities. See http://www.cityenergyproject.org/cities/.

[18] Arimura et al. (2012) analyze the effects of energy efficiency spending on electricity demand at the utility level, and they find no difference in results between models that allow for endogeneity of propensity to have energy efficiency programs in a utility service territory and those that assume policies are exogenous.

Table 4. Regression Results for Two Alternative Samples

	Restricted city sample	Restricted CBSA sample
Variables	*ln*(real utilities per sq ft +1)	*ln*(real utilities per sq ft +1)
T_{ict}	–0.00163	–0.00891***
	(0.00715)	(0.00281)
HDD	3.02e-05***	2.64e-05***
	(4.49e-06)	(3.95e-06)
CDD	–1.38e-06	6.56e-06*
	(4.58e-06)	(3.50e-06)
Unemployment	0.00690*	0.00221
	(0.00345)	(0.00183)
Percent leased	0.112***	0.0981***
	(0.0223)	(0.0130)
Real electricity price	0.00187***	0.00179***
	(0.000158)	(0.000123)
Constant	0.0209	0.0398**
	(0.0267)	(0.0176)
Year*Quarter FE	Yes	Yes
Property FE	Yes	Yes
Observations	56,277	36,070
R-squared	0.189	0.195
Number of properties	3,937	2,508

* $p < .10$; ** $p < .05$; *** $p < .01$. Standard errors in parentheses.

Alternative Fixed Effects

All of the regressions thus far have included individual property fixed effects. Thus all buildings are essentially matched to themselves, indicating that the estimated coefficients measure how changes in an independent variable affect utility expenditures per square foot within an individual building over time. We also estimated an alternative specification with zip code–level fixed effects; this specification allowed us to include some additional property characteristics as explanatory variables—age, number of floors, and square feet of floor space, as well as vintage dummy variables to capture building codes and other factors that change with the year built. The results are reported in Table 5 and show a negative coefficient with roughly the same magnitude as that in the property fixed effects model (Table 3), although not statistically significant.[19]

Table 5. Regression Results for Specification with Zip Code Fixed Effects

Variables	ln (real utilities per square foot +1)
T_{ict}	–0.00969 (0.00625)
Square feet	–1.32e-08 (1.95e-08)
Age	0.000243 (0.000675)
Number of floors	0.000851** (0.000399)
HDD	2.43e-05*** (2.92e-06)
CDD	8.19e-06*** (3.06e-06)
Unemployment	0.00113 (0.00186)
Percent leased	0.0788*** (0.0110)
Real electricity price	0.00179*** (0.000133)
Constant	0.0280 (0.0630)
Year*Quarter FE	Yes
Property FE	Yes
Building vintage dummy variables	Yes
Observations	54,986
R-squared	0.104
Number of properties	1,053

* $p < .10$; ** $p < .05$; *** $p < .01$. Standard errors in parentheses.

[19] The sample size is slightly smaller than in Table 3, as some observations are missing age.

This translates to a –2.8 percent effect on normalized utility expenditures in office buildings affected by the program. Neither building size, as measured by square footage, nor age is statistically significant in explaining utility expenditures per square foot, but the number of floors has a positive and significant effect. Some other studies of commercial buildings have found building age to be either unassociated with energy use per square foot or even negatively associated—that is, older buildings consume less energy (Kontokosta 2012).

Heterogeneity in Treatment Effects

Benchmarking and disclosure laws are quite similar across cities, but as we pointed out above, there are some ways in which they differ. One important difference is the nature of disclosure. As shown in Table 1, Austin and Seattle require disclosure to the government and to others engaged in real estate transactions, such as potential buyers or tenants, whereas New York and San Francisco require public disclosure. In addition, other factors such as weather and electricity prices can vary substantially by geographic region. New York City has an average electricity price for our sample period of $155/MWh, while Seattle’s is only $49/MWh. The average CDD is 1059 in Austin over our sample period but only 455 in San Francisco. Table 6 thus shows the results of our original model, estimated on the full sample, but with separate treatment effects for each of the four cities.

The estimated coefficients on the treatment variable for New York, San Francisco, and Seattle are all statistically significant and of the same order of magnitude. We cannot reject that they are equal to each other and equal to the single coefficient reported in Table 3 for the cities as a whole. The treatment coefficient for Austin is not statistically different from zero. While this seems to indicate that the disclosure law has not yet had a noticeable effect on utility expenditures per square foot in buildings in that city, we are concerned that we do not have enough treatment period observations to identify an effect. As shown in Table 1, Austin has the latest reporting date of the four cities in our sample. As a result, we have far fewer observations for the treatment period than in the other three cities.

Table 6. Regression Results with City-Level Treatment Effects

Variables	*ln*(real utilities per sq ft +1)
T_{itNYC}	–0.0140***
	(0.00236)
T_{itSF}	–0.0102***
	(0.00233)
T_{itAus}	0.00256
	(0.00429)
T_{itSea}	–0.0111***
	(0.00317)
HDD	2.44e-05***
	(2.82e-06)
CDD	8.61e-06***
	(2.63e-06)
Unemployment	0.00237*
	(0.00139)
Percent leased	0.0853***
	(0.00956)
Real electricity price	0.00174***
	(0.000108)
Constant	0.0416***
	(0.0148)
Year*Quarter FE	Yes
Property FE	Yes
Observations	56,277
R-squared	0.189
Number of properties	3,937

* $p < .10$; ** $p < .05$; *** $p < .01$. Standard errors in parentheses.

Table 7 shows the calculated percentage change in utility expenditures per square foot, along with the estimated coefficients from Table 6, and the average utility expenditures per square foot for all four cities. While the coefficients for New York, San Francisco and Seattle are close to one another in size, the estimated percentage effects differ, with the most pronounced difference being between Seattle and the other two cities. We calculate a 3.3 and 3.5 percent drop in utility expenditures per square foot from disclosure laws in New York and San Francisco, respectively, and a 5.2 percent drop in Seattle. This result is attributable to the relatively low utility expenditures per square foot in Seattle, which in turn is attributable in large part to low electricity prices. During the treatment period, the average electricity price in Seattle was \$50.75/MWh, in San Francisco \$114.84/MWh, and in New York \$148.51/MWh. This highlights a potentially interesting result—that the laws may have a larger percentage impact on utility bills in cities with relatively low average electricity prices and thus low average bills. It also highlights an important aspect of our analysis—that we are focused on the effects of disclosure laws on utility expenditures and not energy use. Ultimately, we are interested in the effects of the policy on energy use and CO_2 emissions, but data on energy use are not available from our data source. The translation from energy savings to CO_2 emissions reductions will also vary by region, but an analysis of these effects is beyond the scope of this paper.

Table 7. Estimated Coefficients, Average Utility Expenditures in Treated Buildings, and Percentage Change in Utility Expenditures from Treatment, by City

	Estimated coefficient	Average utility expend/sq ft	Estimated % change effect of treatment on utility expend/sq ft
New York	–0.014	\$0.673	–3.5
San Francisco	–0.010	\$0.441	–3.3
Austin	0	\$0.384	0
Seattle	–0.011	\$0.270	–5.2

We cannot discern a difference attributable to public versus private disclosure. New York and San Francisco disclose energy use data to the public at large, typically via a government website, whereas Seattle and Austin make the information available on request to tenants, buyers, and other market participants. As we believe that these short-run effects we are estimating are primarily an indication of building owners’ increased attentiveness to energy use, finding no difference that appears to be related to the nature of disclosure seems plausible. In the long run, we expect that some differences may arise.

Placebo Regressions

Finally, we explore whether our results may be due to spurious correlation by conducting three placebo tests with "false" treatment groups. In the first two placebo tests, we assume that the buildings in the cities that have adopted benchmarking and disclosure rules are untreated and alternative cities are treated. In the first of these "false cities" specifications, we select buildings above 50,000 square feet in size and located in seven cities that are geographically close to our treatment cities but have not adopted disclosure laws. These seven cities are Dallas, Hartford, Sacramento, San Diego, Nashville, Baltimore, and Memphis. In our second placebo test, we define the treatment group to include a randomly selected set of office buildings above 50,000 square feet from the entire set of nontreated jurisdictions in our sample. In both of these cases, we ensure that the number of "treated" properties is approximately the same as in our actual treated sample. The third placebo test is based on treatment date. We assume that the treated buildings are located in the actual treatment cities but move the treatment date back 20 quarters prior to the first actual treatment date, to the first quarter of 2007. We select this alternative timing for treatment to avoid having the test results influenced by the timing of the 2008 financial crisis. In all three tests, we use the full sample of office building observations between the first quarter of 2003 and the third quarter of 2013.

The coefficient estimates on the false treatment variables in each of the placebo regressions are reported in Table 8. In every case, the placebo treatment has a statistically insignificant effect on normalized building-level utility expenditures.

Table 8. Placebo Regression Results

Placebo regression	Coefficient on treatment variable (std error)
Buildings above 50K sq ft in cities without policies selected based on geography	–0.0153 (0.0106)
Randomly selected buildings above 50K sq ft in cities without policies	–0.00716 (0.00455)
Results with alternative treatment date	–0.0107 (0.00997)

Discussion

The results from the DID models suggest a nontrivial impact from the disclosure laws in the four early adopter cities: buildings covered by the laws have seen their utility expenditures per square foot drop by 2.5 to 3 percent, on average. Because a relatively short amount of time has passed since the policies have taken effect, we view the effect as resulting from increased attentiveness to energy use and costs. Building owners and managers have had to look carefully at their utility bills and fill out forms to submit to the local government. This may have led to operational changes such as adjusting temperature and lighting controls or fine-tuning air-handling systems, which are relatively easy and could have been spurred by the policies in the short run. More long-run responses to the policies, such as upgrading of equipment or improvements in building shells, as well as tenant turnover, will take time and thus are unlikely to be a factor in the short-run response that we estimate.

The magnitude of our estimated effects is in the neighborhood of the short-run impacts of Home Energy Reports on residential electricity consumption in Allcott (2011), Ayres et al. (2013), and Costa and Kahn (2013). A Home Energy Report provides information on an individual home's energy use and compares it with that of neighboring homes. Thus it is similar in spirit to disclosure and benchmarking laws, albeit in a residential context.[20] The reductions estimated in these studies are on the order of 1 to 2 percent, slightly below our estimates. A recent paper by Allcott and Rogers (2014) explores the persistence of the effects of home energy reports and suggests that this consumption reduction effect can be substantially maintained by continuing to distribute the reports to households over several months.[21] They find that reductions in home energy use immediately after the report arrives gradually erode until the next report arrives, but this pattern of erosion in savings in response to the report tends to dissipate over time. Benchmarking and disclosure laws require annual reporting and thus will involve regular updating by owners of measures of absolute and relative energy performance, so it is possible that the laws will have ongoing impacts.

Information provision has also been shown to affect residential energy use and appliance choice in other contexts. Gilbert and Graff Zivin (2014) find that household electricity bills provide information that can affect consumption patterns. In a study using household-level

[20] We are unaware of any similar reports for commercial buildings that are used as widely as Home Energy Reports.

[21] See Taubinsky (2014) for a general theoretical model with inattention and optimal cues or reminders.

interval billing data, they find that households reduce average daily electricity consumption by 0.6 to 1 percent in the first week after receiving a bill, an effect that evaporates as salience fades and consumers tend to revert to higher consumption patterns. Jessoe and Rapson (2014) find that an in-home energy display that provides information on real-time electricity consumption, electricity price, estimated monthly usage, and bills increases responsiveness to short-run price fluctuations. Over time, households that have these in-home energy displays tend to exhibit conservation behaviors beyond periods of high prices. Jessoe and Rapson suggest this additional effect contributes a 1 to 2 percent reduction in CO_2 emissions. Davis and Metcalf (2015) use a stated choice experiment to analyze the effects of tailoring information provision in Energy Guide labels to local weather and electricity price conditions on participants' choice of room air conditioners. They find that providing more location-specific information results in more efficient appliance choices. Although they do not have actual energy consumption data, the authors are able to construct typical energy consumption profiles for room air conditioner options offered in their experiment, and they find that the implied annual energy savings from a more informative label average about $2.14 per treated respondent. In another stated choice experiment focused on hot water heaters, Newell and Siikamaki (2014) find that augmenting Energy Guide information with ENERGY STAR labels or energy efficiency ratings increases the uptake of more energy-efficient hot water heaters.[22] Studies of commercial building energy use are limited, and no study, to our knowledge, has examined the role of information on energy use in commercial buildings.

Conclusion

Energy efficiency characteristics of buildings and their operating cost implications can be difficult to observe, and this lack of transparency undercuts the incentives that building owners have for making costly investments in equipment or building shells or changes in building operations to improve efficiency and reduce energy costs. Local energy benchmarking and disclosure laws are being adopted by a growing number of US cities as one component of local efforts to reduce greenhouse gas emissions. These laws are intended to transform commercial real estate markets to explicitly account for building energy performance. We use a dataset of

[22] Not all studies have found information provision to affect product choices. Allcott and Taubinsky (forthcoming) and Allcott and Sweeney (2015) find no statistically significant effect from provision of information about energy costs to purchases of compact fluorescent lightbulbs and hot water heaters, respectively.

property-level data from the National Council of Real Estate Investment Fiduciaries to study the effects of these programs on utility expenditures in office buildings in four cities.

Our findings suggest that enactment of benchmarking laws has led to about a 3 percent reduction in quarterly utility bills in buildings covered by the laws in the four early-adopter cities that we study. When we look for heterogeneity in the effects across cities, we find a similarly sized percentage impact in New York and San Francisco, a much larger impact in Seattle, and no statistically significant effect in Austin. Seattle's larger impact is due to low average utility expenditures in buildings there, which in turn appears to be due, at least in part, to relatively low electricity prices. Austin's negligible impact is probably due to the small treatment sample size there, as the city's law was the most recent to go into effect among the four cities.

We view our results as short-run effects related to improved attentiveness: as building owners and managers compile the necessary information to fill out required forms and comply with the laws, the energy performance of their buildings becomes more salient to them. Whether a longer-run effect through changes in owner and tenant behavior will result from these laws remains to be seen and is an important topic for future research.

Appendix

Data Cleaning and Dealing with Outliers

The NCREIF dataset includes 76,614 building-level quarterly data points for commercial office buildings from the first quarter of 2003 through the third quarter of 2013. To purge the dataset of very small and very large commercial buildings that could have disproportionate effects on our results, we drop all buildings smaller than 10,000 square feet as well as those larger than 1 million square feet. We also drop observations that have missing or zero values for utility expenditures per square foot, which is our dependent variable.

At the other end of the spectrum, some observations include extremely high values of utility expenditures per square foot. To assess the reasonableness of these observations, we use data from EIA's Commercial Buildings Energy Consumption Survey (CBECS) dataset, which contains data on commercial building energy use in 2003, to calculate total annual energy expenditures associated with use of electricity, natural gas, and heating oil per square foot for all of the commercial buildings included in CBECS. We then identify the full range of these values, adjusted for inflation (deflated to real 2000$), and find that across the buildings included in the dataset, utility costs per square foot range from $0.03 to $12.51 per year. According to Whitestone Research (Romani et al. 2009), water and sewer costs per square foot range from $0.032 to $0.552 per year (in 2000$).

Collapsing these distributions under the assumption that the building with the lowest (highest) energy cost also has the lowest (highest) water cost, and putting everything on a quarterly basis, suggests that real quarterly utility costs per square foot should fall below $3.26, and thus we eliminate all observations with values that fall above that value from the analysis.

We also check the sample for the reasonableness of the data on building expenditures more generally. We drop observations with negative values for total building expenditures and for which utility expenditures exceed total expenditures. We also drop the observations with outlier values of utility expenditures as a share of total building expenditures, trimming at the 1st and 99th percentiles.

We make some further adjustments to the data to fix some small inconsistencies. These include fixing a handful of miscoded zip codes to make them consistent across all observations for each building and adjusting the occupancy rate so it is always no higher than 1.0.

Finally, as noted in the text, we delete all the observations from Washington, DC, which represented about 2,738 observations in the original NCREIF dataset. In the end, we have a sample of 53,047 observations and 3,753 buildings. Of all the data-trimming steps we take, dropping the negative and zero observations for utility expenditures per square foot trims the greatest number of observations.

Testing for Common Trends in Pre-treatment Utility Expenditures

In Table A1, we show the results of our regression testing for common pre-treatment trends. The regression is similar to equation (1) but without the treatment dummy variable and using only pre-treatment observations on the buildings that are eventually treated, along with all observations on buildings that are never treated. The regression includes year dummy variables and interactions of those year dummies with an indicator variable for whether the observation is from a building that is eventually treated. As all of the coefficients on those dummy interaction terms are statistically insignificant, we conclude that our assumption of common trends holds.

Table A1. Regression Results: Test for Common Pre-treatment Trends

Variables	*ln*(real utilities per sq ft +1)
HDD	2.53e-05*** (2.41e-06)
CDD	1.66e-05*** (1.67e-06)
Unemployment	0.00324*** (0.000977)
Percent leased	0.0856*** (0.00976)
Real electricity price	0.00194*** (0.000112)
2003 dummy	–0.0162*** (0.00525)
2004 dummy	–0.0163*** (0.00460)
2005 dummy	–0.0330*** (0.00528)
2006 dummy	0.0429*** (0.00539)
2007 dummy	0.0389*** (0.00519)
2008 dummy	0.0331*** (0.00431)
2009 dummy	0.0151*** (0.00450)
2010 dummy	0.00636 (0.00394)
2011 dummy	0.00254 (0.00249)
2012 dummy	–0.00482** (0.00198)
2004 dummy*treated	–0.0112 (0.0197)
2005 dummy*treated	–0.0291

	(0.0238)
2006 dummy*treated	–0.0172
	(0.0270)
2007 dummy*treated	–0.00356
	(0.0337)
2008 dummy*treated	–0.00826
	(0.0341)
2009 dummy*treated	–0.0265
	(0.0280)
2010 dummy*treated	–0.0196
	(0.0236)
2011 dummy*treated	–0.0266
	(0.0282)
2012 dummy*treated	–0.0112
	(0.0256)
Constant	0.00281
	(0.0172)
Property FE	Yes
Observations	55,564
R-squared	0.156
Number of properties	3,895

* *p < .10*; ** *p < .05; **** *p < .01*. Standard errors in parentheses.

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