DRAFT: Please do not cite or quote.
November 2016

Can Providing Information Shrink the Energy Efficiency Gap? An Evaluation of City Benchmarking and Disclosure Laws for Commercial Buildings *

Karen Palmer[a] and Margaret Walls[a,**]

[a]*Resources for the Future, 1616 P St, N.W., Washington, DC 20036*

Abstract

Missing, incomplete and asymmetric information may help explain the so-called "energy efficiency gap" in commercial buildings, the observation that building owners fail to invest in seemingly cost-effective options for improving energy efficiency. Fifteen local jurisdictions in the United States have addressed this problem by passing energy benchmarking and disclosure laws, which require commercial building owners to report annual energy use to the government. We evaluate whether the laws have had an effect on utility expenditures in office buildings in four of the early adopting cities using a property fixed effects, difference-in-differences regression approach. We find that the laws have reduced utility expenditures by about 3 percent, which we view as largely attributable to increased attentiveness to energy use.

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

JEL Classification Numbers: L94, L95, Q40, Q48

* This research was supported by a grant from the Alfred P. Sloan Foundation. We are grateful to the National Council of Real Estate Investment Fiduciaries (NCREIF) for providing us with access to the individual property-level NCREIF data through an academic research confidentiality agreement. We appreciate the excellent research assistance of Jessica Chu and Matt Leisten and comments on an earlier version of this paper from Nils Kok, Lucija Muehlenbachs, and Richard Sweeney.

** Corresponding author. *Email address:* walls@rff.org

Can Providing Information Shrink the Energy Efficiency Gap? An Evaluation of City Benchmarking and Disclosure Laws for Commercial Buildings

I. 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 many factors that affect energy use and probably have only partial information about how to make cost-effective investments in energy-using equipment and other upgrades (Bardhan et al. 2014). They may also find it difficult to credibly communicate building energy performance to prospective renters and buyers, which reduces incentives to invest in improvements.

In cities, buildings account for an even larger share of total energy use—up to 80 percent by some estimates. Sixteen 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. The thinking is 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 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.[2] 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. (2016) find that the capitalization of broader green certifications in sale prices of single-family homes is larger than that of energy certifications and too large to reflect only energy savings. Houde (2014) finds that 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 2015).

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 a building owner with a metric comparing his building's 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 our data cover the early days of these laws, our view is that our results involve searching mostly for an attentiveness effect: has reporting on energy use and benchmarking it relative to others led to short-run 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 difference-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, 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 are associated with lower 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 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, the empirical literature has focused overwhelmingly on residential energy use (see, for example, Davis and Metcalf 2015; Palmer and Walls 2015; Jessoe and Rapson 2014; Newell and Siikamaki 2014; Allcott 2011). 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.

II. 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 in any city was October 2011, for buildings in San Francisco. These early adopter cities, with the exception of Washington, are the focus of our analysis.[3] As of November 2016, a total of 17 local jurisdictions (16 cities and one county) have adopted benchmarking and disclosure laws and a number of additional cities are actively considering them.[4]

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 as they apply to commercial buildings are summarized in Table 1.[5]

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 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). We disregard the multifamily provisions here as our empirical focus is on commercial properties.

Table 1. Commercial Building Benchmarking and Disclosure Ordinance Provisions in Four Cities

City	*Enactment date*	*Building Size*	*Initial reporting date*	*Disclosed to*
Austin	11/08	≥75K sf	6/12	Government Buyers
		≥30K sf	6/13	
		≥10K sf	6/14	
New York	12/09	>50K sf	12/11[a]	Government Public
San Francisco	02/11	≥50K sf	10/11	Government Buyers Tenants Leasers/lenders Public[b]
		≥25K sf	4/12	
		≥10K sf	4/13	
Seattle	02/10[c]	≥50K sf	4/12	Government Buyers Tenants Leasers/lenders
		≥20K sf	4/13	

[a] Original date was May 2011 but was pushed back to December 2011.
[b] 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.
[c] Seattle passed an amendment to the ordinance in September 2012 that raised the size threshold from 10K sf to 20K sf and restructured enforcement.
Note: Austin, New York, and Seattle also require multifamily residential buildings to benchmark and disclose.

All of the cities have very similar reporting requirements.[6] 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.[7] 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.[8]

III. Data and Empirical Approach

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. Although most cities are collecting a year or two of pre-treatment energy use data for the buildings subject to the laws, .simple before and after comparisons for treatment groups will produce biased estimates of policy impacts (Angrist and Pischke 2010). Moreover, cities are not publicly reporting the pre-treatment data, thus obtaining access for research may be difficult.

We see two possible approaches to assessing the causal effect on energy use, both of which require data from buildings not covered by the laws. The first is a regression discontinuity approach using the building size thresholds in the laws. Buildings that fall just short of the minimum size threshold would be compared to those just above the threshold before and after the policy takes effect using a regression discontinuity approach (Imbens and Lemieux 2008). Unfortunately, most cities do not have a large number of commercial buildings, thus limiting the data only to buildings on either side of the threshold will probably not generate a large enough sample size for analysis.

A more promising approach is a difference-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).

This still requires data on buildings not covered by the laws. Although electric and natural gas utilities have in limited cases made data from individual meter readings available to academic researchers (see, for example, Kotchen and Grant 2011; Jessoe and Rapson 2014), [9] such instances are rare and somewhat idiosyncratic. We do not have access to customer-level billing data for the four cities in this study. Instead, we make use of an independent data source.

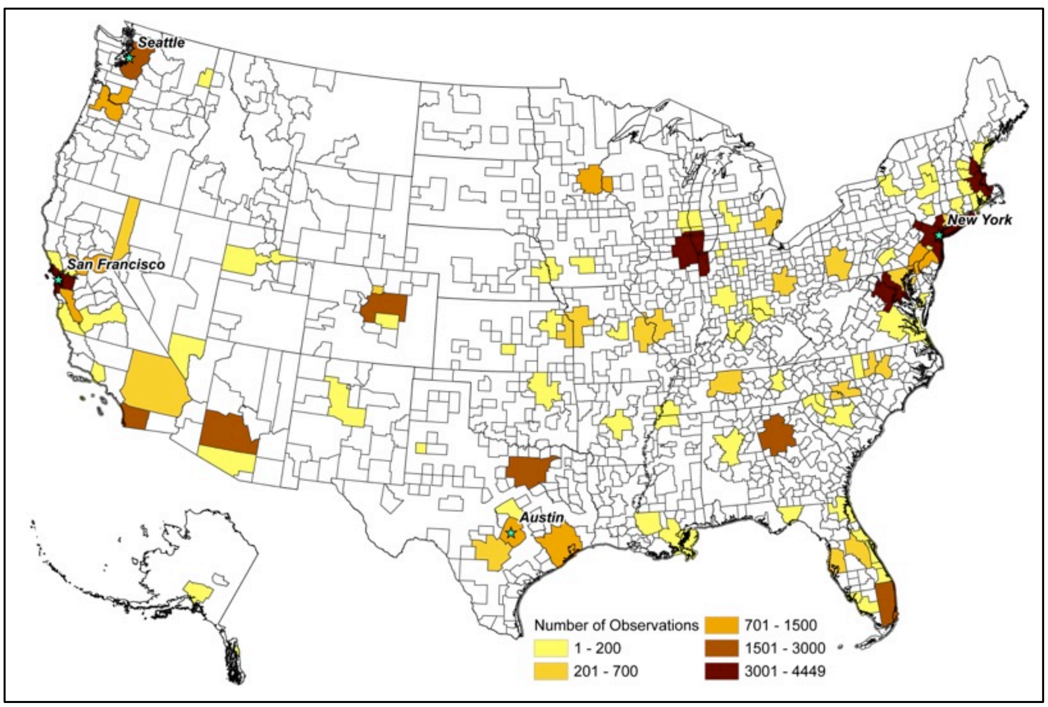
III.1. Data. Our empirical analysis uses data on individual commercial buildings from a unique source for energy-related research, the National Council of Real Estate Investment Fiduciaries (NCREIF). NCREIF is a member-based nonprofit association that represents the institutional real estate investment 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.[10]

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 available. Moreover, other characteristics of the properties are available as well, such as building age, size and location. However, the data cover only institutional real estate owners who are members of NCREIF and membership fluctuates over time, so we do not have a balanced panel of buildings during the time period of the study.[11] 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 type of commercial buildings and represent the largest share of buildings in the NCREIF data. Our 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 and are less variable over time than energy 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). 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.[12] 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.

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



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. 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.[13] 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 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. We use the CPI to convert all prices to 2000$. 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.[14] This dataset is constructed based on information from the Current Population Survey, the Current Employment Statistics program, and state unemployment insurance systems. 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.[15] 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

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

	Never treated		Eventually treated	
	Pre-2012 Q1	Post-2012 Q1	Pre-treatment	Post-treatment
Quarterly utility expenditures per sq ft ($)	0.40 [0.25]	0.36 [0.20]	0.53 [0.31]	0.47 [0.26]
Floor space (sq ft)	225,044 [178,298]	222,100 [178,149]	324,118 [210,027]	333,106 [234,631]
Year built	1987 [14]	1990 [14]	1965 [33]	1962 [35]
Number of floors	7.13 [8.22]	6.72 [7.48]	15.90 [14.12]	16.80 [13.12]
HDD	476.82 [792.04]	367.81 [679.17]	518.01 [682.60]	424.44 [615.07]
CDD	1,329.63 [1,141.02]	1,519.19 [1,187.46]	893.01 [899.35]	933.85 [827.10]
Unemployment rate	6.41 [2.40]	7.77 [1.50]	6.98 [2.12]	7.38 [1.12]
Percent leased	85 [16]	83 [17]	89 [14]	89 [14]
Electricity price ($/MWh)	88.72 [27.97]	84.37 [25.09]	101.93 [42.12]	106.40 [39.27]
No. of observations	45,538	7,978	2,048	592
No. of individual buildings	3,521	1,497	123	121

Note: Numbers of never treated observations for year built are slightly lower than other variables at 44,431 and 7,933, respectively, for pre- and post-2012 Q1.

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 report;[16] 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 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. We control for these differences in our model by using a property fixed effects specification, which we describe below. In the next section, we describe our test for common trends in pre-treatment utility expenditures per square foot in treatment and control buildings.

III.2. Empirical Model. 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 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 statistically insignificant in each year, and thus we cannot reject the hypothesis that utility expenditures in these two sets of buildings (never treated and eventually treated) had identical time trends during the pre-treatment periods.

III.3. Results. 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.

Table 3. Baseline 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

Robust standard errors, clustered at the CBSA level, in parentheses.
$* p < .10$; $** p < .05$; $*** p < .01$.

The results indicate that treatment has a significant negative effect on the transformed utility expenditures variable. The estimated coefficient on the treatment dummy is -0.015; 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 a –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. For an average office building in our sample, this is approximately $20,000 in annual savings. Utility expenditures are, on average, approximately 13 percent of total building expenditures, which include maintenance costs, management fees, administrative and marketing costs, insurance, taxes, and other miscellaneous expenses. Thus a 3 percent savings in utility expenses lowers overall building expenses by less than one-half of one percent for an average building in our sample.

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 positive and significant effects on utility expenditures per square foot, as expected. The local unemployment rate also has a positive effect on utility expenditures, which seems counter-intuitive as we would expect expenses to be higher when economic activity is greater, which would typically be when the unemployment rate is lower. However, the regression controls for occupancy rates, which will also pick up economic activity effects, as well as electricity and gas prices, and all of these coefficients have the expected signs. Moreover, the coefficient on the unemployment rate is significant only at the 10 percent level.

III.4. Results for Alternative Samples. This baseline 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 further limited the sample to only 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.[17] In the first, we use as control observations only buildings in cities that either have passed disclosure laws not yet in effect or are considering such laws through their participation in the City Energy Project, an independent initiative promoting building energy efficiency policies in a set of US cities.[18] 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. The first alternative sample includes buildings in Atlanta, Boston, Cambridge, Chicago, Denver, Houston, Kansas City, Los Angeles, Minneapolis, Orlando, Philadelphia, Portland, Oregon, and 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.

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	12,359	36,070
R-squared	0.173	0.195
Number of properties	876	2,508

Robust standard errors, clustered at the CBSA level, in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$.

In both regressions, we retain a negative estimated 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 (about one-third of that in the CBSA sample) 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. 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, but more research as the programs advance in other cities should shed light on this issue.[19]

III.5. Results with 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.[20] This translates to a –2.8 percent effect on utility expenditures per square foot 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, or negatively associated, with energy use (Kontokosta 2012).

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
Zip Code FE	Yes
Building vintage FE	Yes
Observations	54,986
R-squared	0.104
Number of properties	1,053

* $p < .10$; ** $p < .05$; *** $p < .01$. Robust standard errors, clustered at the CBSA level, in parentheses.

III.6. 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, New York and San Francisco require public disclosure, but Austin and Seattle do not. Seattle requires disclosure to the government and to others engaged in real estate transactions, such as potential buyers, tenants, and lenders. Austin, on the other hand, only requires disclosure to the government and to buyers. These differences could lead the laws to have differential effects by city. 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, suggesting that the disclosure law has not had a noticeable effect on utility expenditures per square foot in buildings in that city. One possible reason for the difference between Austin and the other cities may be related to the nature of disclosure. Austin's more limited disclosure—only to the government and building buyers—may mean the law has less of an effect there.

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

Robust standard errors, clustered at the CBSA level, in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$.

The Austin coefficient estimate is noisy, however, which is likely a result of the comparatively small number of treatment period observations. As shown in Table 1, Austin has the latest reporting date of the four cities in our sample. It thus has fewer observations for the treatment period than the other three cities. We note that the lower bound of the 95 percent confidence interval for the estimated treatment effect in Austin is -0.01, which is roughly the mean estimate for the other cities.

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.

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

	Estimated coefficient (from Table 6)	Average utility expenditures/sq ft[a]	Estimated % 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

[a]Average utility expenditures per square foot calculated from the NCREIF sample of buildings used in our econometric analysis.

III.7. 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.

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)

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 building-level utility expenditures per square foot.

IV. 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.[21] The reductions estimated in these studies are on the order of 1 to 2 percent, slightly below our estimates. 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.[22] 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 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 due to time-of-use electricity rates. 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. In another study with time-of-use rates, however, Harding and Lamarche (2016) find that in-home displays have little effect unless paired with a programmable thermostat, which allows automation of response to price changes. They find that households with programmable thermostats and in-home displays have 55-60 percent lower peak period electricity consumption, on average, than those with in-home displays alone.[23] 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.[24] 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. Given the contribution of commercial buildings to total energy use and CO_2 emissions and the recent emphasis on policies targeted at this sector, particularly at the municipal level, more research on these issues is needed.

V. 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. The first law to take effect was New York City's in 2011, but since then, policy adoption has snowballed: a total of 15 cities and one county now have such laws, with seven of these adopted in 2014 and 2015 alone.

We use a dataset of 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 may be 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. However, we note that Austin's law only requires disclosure to the government and to property buyers and not the public at large as in New York and San Francisco or even to tenants and lenders as in Seattle.

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

A.1. 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 lie 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.

A.2. 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

* $p < .10$; ** $p < .05$; *** $p < .01$. Robust standard errors, clustered at the CBSA level, in parentheses.

References

Allcott, Hunt. 2011. Social Norms and Energy Conservation. *Journal of Public Economics* 95: 1082–1095.

Allcott, Hunt, S. Mullainathan, and D. Taubinsky. 2014. Energy Policy with Externalities and Internalities. *Journal of Public Economics* 112: 72–88.

Allcott, Hunt, and Todd Rogers. 2014. The Short-Run and Long-Run Effects of Behavioral Interventions: Experimental Evidence from Energy Conservation, *American Economic Review* 104(10): 3003–3037.

Allcott, Hunt, and Richard Sweeney. 2015. Can Retailers Inform Consumers about Energy Costs? Evidence from a Field Experiment. New York University Department of Economics working paper (January).

Allcott, Hunt, and Dmitry Taubinsky. Forthcoming. Evaluating Behaviorally-Motivated Policy: Experimental Evidence from the Lightbulb Market. *American Economic Review.*

Angrist, Joshua D., and Jörn-Steffen Pischke. 2010. The Credibility Revolution in Empirical Economics: How Better Research Design Is Taking the Con out of Econometrics. *Journal of Economic Perspectives* 24(2): 3–30.

Arimura, Toshi, Shanjun Li, Richard Newell, and Karen Palmer. 2012. Cost Effectiveness of Electricity Energy Efficiency Programs. *Energy Journal* 33(2): 63–99.

Ayres, Ian, Sophie Raseman, and Alice Shih. 2013. Evidence from Two Large Field Experiments That Peer Comparison Feedback Can Reduce Residential Energy Usage. *Journal of Law, Economics and Organization* 29(5): 992–1022.

Bardhan, Ashok, Dwight Jaffee, Cynthia Kroll, and Nancy Wallace. 2014. Energy Efficiency Retrofits for U.S. Housing: Removing the Bottlenecks. *Regional Science and Urban Economics* 47: 45-60.

Brown, Jennifer, Tanjim Hossain, and Morgan John. 2010. Shrouded Attributes and Information Suppression: Evidence from the Field. *Quarterly Journal of Economics* 125(2): 859–876.

Burr, Andrew, Cliff Majersik, and Sarah Stellberg. 2012. *Analysis of Job Creation and Energy Cost Savings from Building Energy Rating and Disclosure Policy*. Washington, DC: Institute for Market Transformation (March).

Cannon, Susanne Ethridge and Rebel Cole. 2011. How Accurate Are Commercial Real Estate Appraisals? Evidence from 25 Years of NCREIF Sales Data. *Journal of Portfolio Management* 37(5): 68-88.

Chetty, Raj, Adam Looney, and Kory Kroft. 2009. Salience and Taxation: Theory and Evidence *American Economic Review* 99(4): 1145–1177 (September).

Costa, Dora, and Matt Kahn. 2013. Energy Conservation "Nudges" and Environmentalist Ideology: Evidence from a Randomized Residential Electricity Field Experiment. *Journal of the European Economic Association* 11(3): 680–702.

Cox, Matt, Marilyn Brown, and Xiaojing Sun. 2013. Energy Benchmarking of Commercial Buildings: A Low-Cost Pathway toward Urban Sustainability. *Environmental Research Letters* 8.

Davis, Lucas, and Gilbert Metcalf. 2015. Does Better Information Lead to Better Choices? Evidence from Energy Efficiency Labels. E2e Working Paper 015.

DDOE (District Department of the Environment). 2014. *Green Building Report 2012*. Washington, DC: DDOE.

Eichholtz, Piet, Nils Kok, and John M. Quigley. 2010. Doing Well by Doing Good? Green Office Buildings. *American Economic Review* 100(5): 2492–2509.

———. 2013. The Economics of Green Building. *Review of Economics and Statistics* 95(1): 50–63.

EPA (US Environmental Protection Agency). (2014) Draft Inventory of US Greenhouse Gas Emissions and Sinks, 1990–2013. http://www.epa.gov/climatechange/ghgemissions/usinventoryreport.html.

Finkelstein, Amy. 2009. E-ZTax: Tax Salience and Tax Rates. *Quarterly Journal of Economics* 124(3): 969–1010.

Galiani, Sebastian, Paul Gertler, and Ernesto Schardgrodsky. 2005. Water for Life: The Impact of the Privatization of Water Services on Child Mortality. *Journal of Political Economy* 113(1): 83–120.

Geltner, David and William Goetzmann. 2000. Two Decades of Commercial Property Returns: A Repeated-Measures Regression-Based Version of the NCREIF Index. *Journal of Real Estate Finance and Economics* 21(1): 5-21.

Gerarden, Todd, Richard Newell, and Robert Stavins. 2015. Deconstructing the Energy-Efficiency Gap: Conceptual Frameworks and Evidence. *American Economic Review Papers and Proceedings* 105(5).

Gilbert, Ben, and Joshua Graff Zivin. 2014. Dynamic Salience with Intermittent Billing: Evidence from Smart Electricity Meters. *Journal of Economic Behavior and Organization* 107: 176–190.

Gillingham, K., R. G. Newell, and K. Palmer. 2006. Energy Efficiency Policies: A Retrospective Examination, *Annual Review of Environment and Resources* 31: 161–192.

———. 2009. Energy Efficiency Economics and Policy. *Annual Review of Resource Economics* 1: 597–620.

Gillingham, K., and K. Palmer. 2014. Bridging the Energy Efficiency Gap: Policy Insights from Economic Theory and Empirical Evidence. *Review of Environmental Economics and Policy* 8(1): 18–38.

Harding, Matthew and Carlos Lamarche. 2016. Empowering Consumers through Smart Technology: Experimental Evidence on the Consequences of Time-of-Use Electricity Pricing. *Journal of Policy Analysis and Management* 35(4): 906–931.

Houde, Sébastien. 2014. *How Consumers Respond to Environmental Certification and the Value of Energy Information*. Working Paper 20019. National Bureau of Economic Research.

Imbens, Guido and Thomas Lemeiux. 2008. Regression Discontinuity Designs: A Guide to Practice. *Journal of Econometrics*. 142(2): 615-635.

Jessoe, Katrina, and David Rapson. 2014. Knowledge Is (Less) Power: Experimental Evidence from Residential Energy Use. *American Economic Review* 104(4): 1417–1438.

Kahn, M. E., & Kok, N. (2014). The Capitalization of Green Labels in the California Housing Market. *Regional Science and Urban Economics*, 47: 25-34.

Kontokosta, Constantine. 2012. Local Law 84 Energy Benchmarking Data: Report to the New York City Mayor's Office of Long-Term Planning and Sustainability. http://www.nyc.gov/html/gbee/downloads/pdf/ll84_kontoska_report.pdf.

Kotchen, Matthew and Laura Grant. 2011. Does Daylight Savings Time Save Energy? Evidence from a Natural Experiment in Indiana. *Review of Economics and Statistics* 93(4): 1172-1185.

Lacetera, Nicola, Devin Pope, and Justin Sydnor.2012. Heuristic Thinking and Limited Attention in the Car Market. *American Economic Review* 102 (5): 2206–2236.

Li, Jinliang, Robert Mooradian, and Shiawee Yang. 2009. The Information Content of the NCREIF Index. *Journal of Real Estate Research* 31(1): 93-116.

Meyer, Bruce. 1995. Natural and Quasi-Experiments in Economics. *Journal of Business and Economics Statistics* 13(2): 151–161.

Newell, Richard G., and Juha V. Siikamaki. 2014. Nudging Energy Efficiency Behavior: The Role of Information Labels. *Journal of the Association of Environmental and Natural Resource Economists* 1(4): 555–598.

Palmer, Karen, and Margaret Walls. 2015. Limited Attention and the Residential Energy Efficiency Gap. *American Economic Review Papers and Proceedings* 105(5): 192-195.

———. 2016. Using Information to Close the Energy Efficiency Gap: A Review of Benchmarking and Disclosure, *Energy Efficiency* (published online Sept. 15).

Romani, Luca, Michael Towers, Douglas Abate, and Richard Dotz. 2009. *The Whitestone Facility Operations Cost Reference: Third Annual Edition*. Washington, DC: Whitestone Research Corporation.

Sallee, James. 2014. Rational Inattention and Energy Efficiency. *Journal of Law and Economics* 57(3): 781–820.

Taubinsky, Dmitry. 2014. From Intentions to Actions: A Model and Experimental Evidence of Inattentive Choice. Harvard Department of Economics working paper (February).

Walls, Margaret, Todd Gerarden, Karen Palmer, and Xian Bak. 2016. Is Energy Efficiency Capitalized in Home Prices? Evidence from Three U.S. Cities. Forthcoming *Journal of Environmental Economics and Management.*

Endnotes

---

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

[2] The Energy Star program is operated by the U.S. EPA; Portfolio Manager is the software tool used to generate the scores used for certification. The U.S. Green Building Council designs and implements LEED, which stands for Leadership in Energy and Environmental

[2] The Energy Star program is operated by the U.S. EPA; Portfolio Manager is the software tool used to generate the scores used for certification. The U.S. Green Building Council

designs and implements LEED, which stands for Leadership in Energy and Environmental Design. Some studies have also looked at whether these certifications are capitalized in residential building prices (Kahn and Kok 2014; Walls et al. 2015).

[3] 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.

[4] In addition to the five early adopters, the jurisdictions include Philadelphia, Minneapolis, Chicago, Boston, Cambridge, Berkeley, Montgomery County, Maryland, Portland, Oregon, Atlanta, Kansas City, Missouri, Boulder, and Pittsburgh.

[5] For more information about the policies adopted prior to November 21, 2014, including additional provisions of the laws, see Palmer and Walls (2016).

[6] 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.

[7] 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 (2016).

[8] 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.

[9] Researchers who have worked with OPower (for example, (Allcott 2011; Costa and Kahn 2013) have had access to utility data from utilities that are OPower clients.

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

[11] Having only institutional investors means that we do not have properties in our dataset that are sole proprietorships, limited liability companies (LLCs), and other types of owners. These categories are less prevalent in major cities than institutional investors.

[12] 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.

[13] 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.

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

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

[16] 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.

[17] 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 cannot reject that the trends are the same for the treatment and control buildings in these two samples as well.

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

[19] 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.

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

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

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

[23] The effect on total electricity consumption was much smaller as this decrease during peak periods was partially offset by a 19-23 percent increase in consumption during off-peak periods.

[24] 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.