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By

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Abstract

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This dissertation explores two aspects of environmental economics and the evaluation of energy policies in the buildings sector. The first chapter focuses on energy standards, and the second chapter focuses on green labels.

The first chapter assesses whether commercial real estate market participants are willing to pay a premium for an energy efficient building that has not received a green label. I utilize a unique dataset of detailed building-level observations and a spatial semiparametric matching framework that exploits quasi-experimental state-by-year variation in the implementation of mandatory building energy codes, to estimate selling price and rent premiums for a more stringent code. I find that buildings constructed under a more stringent energy code are associated with rent and selling price premiums of approximately 2.7% and 10%, respectively, compared to buildings constructed just before the code came into effect. When tenants pay directly for utilities, buildings constructed under an energy code are associated with 5.7% higher rents. While building energy codes have been promoted to address landlord-tenant informational asymmetries that would not be addressed by a carbon-pricing strategy, these estimated premiums are consistent with complete capitalization of estimated building-level savings, and as such they cast doubt on the existence of an energy efficiency gap resulting from adverse selection between landlords and tenants.

In the second chapter, I assess whether nonrandom selection affects the frequently-touted benefits of green-labeling policies in the commercial building stock. While green-labeled buildings have been found to sell at a premium compared to nearby controls with similar observable characteristics, the voluntary nature of the labeling decision implies green-labeled buildings may have different unmeasured characteristics that may account for at least a portion of the premium. Therefore, it is unclear whether green-labeled building premiums are a causal effect of the labels. I use data on repeat sales transactions and detailed hedonic characteristics to test whether green-labeled office buildings were selling at a premium before they were labeled, and combine these results with post-labeling price premium estimates to identify realized cost-benefit ratios for green-labeling policies. The data suggest the causal

net benefits of green labels range from \$11.50-\$19.95 per square foot. The estimated net benefits are smaller than previous estimates that have focused solely on the benefits and ignored the potential biases from nonrandom selection.

To Danielle, Andy, Hashmat, and Ariane

Contents

1. Energy Codes and the Landlord-Tenant Problem

Commercial buildings consume close to 40% of the electricity and 20% of all energy in the U.S. economy, and mandatory building energy codes affecting the energy efficiency of most new construction in the U.S. have been credited with delivering significant, net beneficial energy savings.[1] If these codes deliver valuable energy savings, both prospective owners and tenants should be willing to pay a premium to purchase or locate in buildings constructed under a more stringent energy code, as the energy savings are internalized in these market transactions. However, market premiums from energy efficiency investments may be mitigated due to asymmetric information about a building's energy use characteristics, which has been a long-standing subject of debate among economists and policymakers (Gillingham et al. (2009)).

A frequently cited informational market failure is the landlord-tenant problem in residential and commercial buildings: when a building's energy efficiency is costly to observe, prospective tenants or buyers may not be willing to pay a premium for higher efficiency levels because they are unaware, or unconvinced, of a building's efficiency attributes, thereby weakening the owner's incentive to invest in energy efficiency, even in cases when it is economically efficient to do so. Such foregone net beneficial investments may contribute to an energy efficiency 'gap' between realized levels of energy conservation investment versus the larger set of economically efficient ones (Jaffe and Stavins (1994)). The landlord-tenant problem has been widely cited as a likely source of investment inefficiency that may merit policy intervention (Brown (2001), EPA (2003), Murtishaw and Sathaye (2006), Allcott and Greenstone (2012)), yet remarkably few empirical studies have evaluated the impact of informational market failures on the economic efficiency of energy use in commercial real estate, and no work thus far has empirically assessed the prevalence of landlord-tenant informational asymmetries in the commercial building stock. This leaves a considerable gap in our knowledge given that conservative estimates indicate close to 50% of office and retail buildings are multi-tenanted or non-owner-occupied.

[1] Department of Energy (1993), Cort et al. (2002), CEC (2007), EPA (2009).

Market premiums have been demonstrated in office buildings that have received a green label in recognition of their superior energy efficiency characteristics. Nevertheless, the open question of whether similar premiums accrue to the stock of unlabeled, energy efficient buildings remains a first order concern, for several reasons that extend beyond the potential for information market failures in unlabeled energy efficient buildings. The value of purchasing or locating in green-labeled buildings is at least partly related to the intangible effects of the label (Eichholtz et al. (2010)), and the voluntary nature of the labeling decision suggests unobservable building characteristics may account for at least a portion of the premium. In addition, the commercial building stock is increasingly composed of small, low-rise buildings located at the urban fringe, whereas green-labeled buildings are disproportionately made up of large structures located in central cities and account for less than three percent of the office building stock (EIA (2003a), Glaeser and Kahn (2004), Kok et al. (2011)), such that it remains to be determined whether commercial real estate markets are internalizing the benefits of energy efficiency in the average newly constructed commercial building.[2]

In this study I assess whether prospective owners and tenants are willing to pay a premium to purchase or locate in unlabeled office buildings constructed with more stringent energy efficiency characteristics. My identification strategy makes use of a unique dataset of geocoded building-level observations that includes information on rental rates, transaction prices, and whether tenants or landlords pay for utilities, combined with a spatially explicit semiparametric matching framework that assigns each building to a particular efficiency level by exploiting quasi-experimental year of adoption variation in the implementation of state-level mandatory energy codes. To obtain a credible control sample I match buildings constructed within three years of each other, just before and just after an energy code came into effect, located an average of half a mile apart. By constructing a dataset that identifies energy code adoptions in thirty six states over the past twenty years, I have been able to

[2]This is particularly salient given that many unlabeled buildings constructed under mandatory energy codes are associated with similar estimated energy savings as green-labeled buildings. Compared to the average office building in the Energy Information Administration's Commercial Building Energy Consumption Survey, both Energy Star and LEED office buildings use approximately 35% less energy (Turner and Frankel (2008), EPA (2006)), whereas buildings constructed in compliance with ASHRAE standards 90.1-1999 or 90.1-2004 are estimated to use 45-50% less energy (Federal Register (2002), Federal Register (2008)).

match a large proportion of recently erected U.S. office buildings with their energy code status, and thereby estimate the value premium associated with energy codes in the commercial building sector.

The results indicate that unlabeled buildings constructed under an energy code are associated with significant rent and selling price premiums of approximately 2.7% and 10%, respectively. In buildings where tenants pay directly for utilities, buildings constructed under an energy code are associated with approximately 5.7% higher rents relative to structures built just before a code came into effect. These rent and selling price premiums suggest owners obtain returns to energy conservation investments even in buildings where it is more difficult to precisely observe energy efficiency characteristics. Further calculations suggest these premiums are consistent with complete capitalization of estimated building-level savings. The results are invariant to a number of robustness checks. These premiums cast doubt on the existence of an energy efficiency gap resulting from adverse selection between office building landlords and tenants, particularly when building occupants pay for their own utility bills.

The remainder of the chapter is organized as follows. Section 1.1 presents background information on building energy codes in the U.S., explains how energy codes differ from green labels, and how these differences can lead to adverse selection in buildings constructed under an energy code. Section 1.2 outlines the identification strategy and empirical model. Section 1.3 provides a detailed overview of the data and provides an estimate of building-level energy and operating cost savings. Section 1.4 presents the empirical results, and Section 1.5 briefly concludes.

1.1 Background

The primary goal of this paper is to estimate the average premium from an increase in the stringency of the building energy codes that govern the efficiency characteristics of the building stock. I exploit variation in state-year energy code adoptions that allows me to identify whether real estate market participants are willing to pay for more stringent estimated levels of energy efficiency. Some details of these energy codes are summarized below.

1.1.1 Energy Codes

Building energy codes or energy standards are designed to provide minimum criteria for designing energy-efficient buildings through guidelines that affect insulation levels, heat loss and heat gain from doors and windows, the size and energy use of heating, ventilation and air-conditioning equipment, as well as the number, location and type of lighting installations (ASHRAE (2007)).[3]

The first commercial building energy standard in the U.S., Standard 90-75, emerged in the 1970s as the result of a collaboration between the American Society of Heating, Refrigerating and Air-Conditioning Engineers (ASHRAE), the Illuminating Engineering Society of North America (IES) and the American National Standards Institute (ANSI). Federal involvement in energy standard development formally began with passage of the Energy Conservation Standards for New Buildings Act of 1976 and the establishment of the Building Energy Standards Program (BESP), which brought together the Department of Energy (DOE), ASHRAE, ANSI, IES, state governments, and other stakeholders with the aim to improve the energy savings and enforceability of Standard 90-75 (Hattrup (1995)).[4] As a result of the BESP collaboration, Standard 90-75 was updated to Standard 90A-1980 (hereafter ASHRAE 1980). DOE continued to work with ASHRAE and other stakeholders throughout the 1980s, bringing together experts in the design, construction, and measurement/estimating fields to develop cost-effective energy efficiency improvements to ASHRAE-1980. This resulted in Standard 90.1-1989 (hereafter ASHRAE 1989), and a similar collaborative process has led to the development of Standards 90.1-1999, 90.1-2004 and 90.1-2007 (heareafter ASHRAE 1999, ASHRAE 2004 and ASHRAE 2007). In addition, some states have adopted the commercial requirements of the International Energy Conservation Code 2000 edition (hereafter IECC 2000), which is equivalent to ASHRAE 1989 except for the lighting requirements; the IECC

[3]Following common practice, the terms energy standard and energy code are used interchangeably.

[4]The Energy Conservation Standards for New Buildings Act mandated the DOE to develop energy standards for buildings "to achieve the maximum practicable improvements in energy effciency and use of non-depletable resources for all new buildings."

2000 lighting requirements are equivalent to ASHRAE 1999 (Winiarski et al. (2003)).[5]

In response to low rates of voluntary state-level energy standard adoption and compliance efforts, the Energy Policy Act (EPAct) of 1992 included a requirement that states adopt the most recent ASHRAE standard. EPAct also mandated the Department of Energy to promote adoption and compliance activities at the state-level by providing resources for training building officials and other industry stakeholders, developing compliance software, and producing both state-level and national benefit-cost studies to demonstrate the benefits of energy standards (Department of Energy (1999), ASHRAE (2010)). Before formally adopting a new version of the ASHRAE Standard, several individual states commissioned the DOE to conduct simulation studies specific to their state to ensure that positive net benefits could be attained from a new standard level.[6] At present, 44 states have formally adopted an energy standard that is at least as stringent as ASHRAE 1989. Given that most states' decisions to adopt or update their building standard in the post-1992 timeframe has been based on a combination of the EPAct mandate and the demonstration of net benefits for the average building, the statewide implementation date provides a source of quasi-experimental variation in energy efficiency that I will exploit in my identification strategy.

1.1.2 How energy standards differ from green labels

The process of constructing a building in accordance with an energy standard differs from that of obtaining a green label in many respects. Green labels can be obtained for either new construction or existing buildings. The labeling procedure for new construction requires third-party verification and monitoring of building performance through all stages of construction. This begins at the building design stage and continues through to building commissioning and at least the first year of building operation, which may culminate in official recognition and certification (USGBC (2009a)). A building is certified as 'green' only

[5] The International Energy Conservation Code (IECC) is published by the International Code Council, which publishes national model building codes for the construction industry. Starting with IECC 2001 and ASRHAE 1999, both IECC and ASHRAE have coordinated the design of their energy codes so that they would bring about equivalent energy savings.

[6] States that have followed this approach include Illinois, Iowa, Louisiana, Massachusetts, Michigan, New Mexico and New York.

after adequately demonstrating criteria for energy and environmental performance above a predetermined threshold. For existing buildings, third-party building energy performance monitoring is typically required for periods up to 1 year (USGBC (2009b)). A labeled building must be re-certified every one to five years in order to retain its labeled status (USGBC (2012), EPA (2012)).

To construct a building in compliance with an energy standard, building architects must design the structure to satisfy code requirements before they can obtain a building permit by the local jurisdiction. Once a building permit is obtained, the local building department may perform a random spot-check during construction, though only a subset of buildings undergo a site inspection (Department of Energy (2010), Department of Energy (2010b)). In contrast to green-labeled buildings, constructing a building in accordance with an energy standard does not require monitoring of the building's energy characteristics after building completion, and market participants do not observe an explicit signal of energy performance.[7] In this respect, communicating the efficiency characteristics of a building constructed under an energy standard faces similar challenges as a building in which the owner or developer has independently made the decision to incorporate energy efficient features. These characteristics of energy standards contribute to the perception that informational market failures lead to the under-pricing of energy efficiency in real estate markets (EPA (2003)).[8]

1.1.3 Sources of asymmetric information

The potential effect of asymmetric information between landlords and tenants on energy use decisions in buildings was first noted by Blumstein et al. (1980), whose study included four interviews of commercial real estate market participants in California's Bay Area. Several interviewees noted the difficulty of recouping efficiency investments as mitigating their interest in energy conservation, while one property developer specifically mentioned that tenants were not willing to pay higher rents for more efficient buildings.[9] Recent evi-

[7] For example, it's not possible to observe, from past building permits in jurisdictional records, which standard a specific building has been constructed under.

[8] See p.9.

[9] On page 362: "His experience has been that if a building is fairly energy-conservative but the rent is marginally higher due to the increased construction costs, the building is much harder to rent."

dence suggests this belief persists among real estate equity investors, lenders and developers (Galuppo and Tu (2010)).[10]

The impact of imperfect information between landlords and tenants on energy investments is frequently conceptualized by defining two parties, one party that is informed regarding the energy use characteristics of a building (typically the owner or operator), and an uninformed party that does not have the same information set as the informed party (the prospective buyer or tenant). As a result, the onus is on the informed party to convince the uninformed party of the building's energy use characteristics. In buildings where tenants pay directly for utilities, this means owners will only obtain a positive return from energy efficiency investments if tenants are willing to pay a rent premium to locate in a more energy efficient building. This may contribute to the problem of net beneficial energy conservation investments being foregone if building owners cannot successfully convince tenants of their buildings' efficiency characteristics (IEA (2007), Cortese et al. (2010)); one recent study suggests between 40-90% of commercial space may be subject to landlord-tenant market failures (Prindle et al. (2006)).

Part of the difficulty of credibly signaling a building's efficiency characteristics to prospective tenants entails the resources needed to correctly evaluate a building's energy performance. Assessing a building's energy performance requires some combination of observing insulation levels, lighting power densities and HVAC equipment efficiencies, and/or tracking its energy use over time and comparing it to other similar buildings that have undergone the same process (Matson and Piette (2005)). If this information is lacking at the time a tenant [buyer] is making a location [purchasing] decision, it may be difficult to determine whether energy use differences between similar buildings are due to efficiency characteristics or occupant behavior. This has recently prompted several federal, state and local policies to improve the ability of commercial real estate participants to compare buildings' energy performance (Federal Register (2011), CEC (2012), NYC (2011)).

On the other hand, the rising popularity of 'green leases', in which tenancy contracts

[10]The study also found that mortgage lenders and real estate participants with no experience in energy efficiency projects held the most pessimistic views regarding the likelihood of obtaining value premiums greater than the incremental cost of energy efficiency investments.

explicitly set out how to allocate energy cost savings between owners and tenants (Oberle and Sloboda (2010)), suggests there is a channel through which building owners may benefit from lower utility costs in energy efficient buildings. Reed et al. (2004) conclude that agency issues between landlords and tenants are unlikely to be a major problem affecting energy use. This conclusion was made on the basis of information regarding the prevalence of owner-occupied commercial buildings and the general structure of leasing contracts, though no formal empirical analysis was presented.[11]

The Department of Energy estimates close to 50% of office and retail buildings are multi-tenanted (EIA (2003a)), and this statistic is likely to underrepresent the true value since the survey on which it is based counts space which is only partially owner-occupied as being completely owner-occupied.[12] Given that landlord-tenant informational asymmetries have the potential to affect a large proportion of office buildings, the preceding discussion suggests a notable gap in our understanding of the empirical importance of the landlord-tenant problem thus far is the dearth of reliable empirical studies on the commercial sector.

1.2 Empirical Strategy

The outcome of interest is the sample average treatment effect on the treated (SATT), the average impact of energy codes on rents and selling values in buildings constructed under an energy code regime.

Buildings are assigned to one of two states: unlabeled energy efficient buildings constructed under a newly implemented energy code, or unlabeled buildings with lower energy efficiency attributes (i.e. higher estimated energy use), identified by having been constructed before the energy code was implemented. Using the potential outcomes framework, let D_i=1 denote a treated observation if building i has been constructed under a new code, and D_i=0

[11]In addition, Reed et al. (2004) note that their data sources are disproportionately from the largest firms: as has been pointed out by Kok et al. (2011), the largest firms and buildings are precisely those that are most likely to adapt to new information. Therefore, it is not clear to what extent the study is representative of the building sector.

[12]See questions C3 and C5 in EIA (2003b). My data, which only categorize a building as owner-occupied if it is solely occupied by the owner, suggest that out of approximately 91,000 observations with information on whether a building is owner-occupied or multi-tenanted, about 70% of the buildings are multi-tenanted.

denote a control observation if building i has been constructed under a less stringent code version.[13] Potential outcome $Y_i(1)$ denotes building values (rents or prices) in building i contingent on having been constructed under a more stringent energy code regime, and potential outcome $Y_i(0)$ denotes building values in building i, contingent on having been constructed under a less stringent code. The SATT can be expressed as

$$\alpha_{TT} = E\left[Y_i(1) - Y_i(0) | D_i = 1\right]. \tag{1}$$

Rents or prices in buildings constructed after the implementation of a new energy code can be used to identify $E\left[Y_i(1) | D_i = 1\right]$. However, the counterfactual, average building values in buildings constructed under a newly implemented code *had they been constructed under a less stringent code*, $E\left[Y_i(0) | D_i = 1\right]$, is unobserved. My identification strategy generates a credible estimate of counterfactual building values by exploiting the data's geographic precision to find control observations located within two miles of a treated observation, thereby holding unobservable small-scale locational characteristics constant.[14]

Many states in my sample have adopted multiple, increasingly stringent versions of an energy code (see Figure 1). Table 1 presents the six treated categories and matched untreated controls that I observe in the data, along with the percent share of the rent and sales samples in each category. Therefore, the estimated average treatment effect pools across building matches with different estimated energy savings. As explained in Section 1.3.2, while the estimated savings from a given treated-control match vary between 5-11%, the weighted average estimated energy saving from each match in my sample is about 10% in both the rental and sales datasets.

1.2.1 Testable hypotheses

The literature summarized in Section 1.1.3 provides arguments for and against the null hypothesis that building energy standards will have no effect on value premiums in

[13] Where a 'less stringent code' includes no energy code.

[14] The reasons for selecting a 2-mile radius are discussed in Section 1.2.2. Results obtained from steadily decreasing the radius are presented in the Appendix.

commercial real estate markets. Three testable implications follow from this more general hypothesis.

Hypothesis 1: *Energy efficient buildings are not associated with rent premiums.* Assessing a building's efficiency level is costly and typically requires some combination of observing insulation levels, lighting power densities and HVAC equipment efficiencies, and/or requesting and analyzing utility bills. While prospective tenants or their representatives may be willing to pay a premium to locate in an energy efficient building, either to benefit from lower utility bills or insure against the impact of energy price increases, they may find it difficult to evaluate a building's efficiency level or may not be convinced by owner/operator claims about a building's energy conservation characteristics.

Hypothesis 2: *Rental premiums are the same regardless of who pays for utilities.* Rental contracts differ in terms of which party is responsible for utility bill payments. In buildings constructed under an energy code where utilities are paid directly by tenants (which is the case in almost half of my sample), tenants will face lower utility payments and may therefore be willing to pay a premium to locate in these buildings. However, this will not be observed if tenants or their representatives are unconvinced of a building's efficiency characteristics.

Hypothesis 3: *Energy efficient buildings are not associated with sale price premiums.* Owners of buildings constructed under an energy code may benefit from higher net incomes since they may obtain either higher rents, if tenants pay for utilities, or benefit from lower utilities directly, if owners pay for utilities. However, if current owners are unable to convince prospective buyers of the building's energy conservation characteristics, or if prospective buyers believe they will not be able to convince prospective tenants to pay a rent premium, energy efficient buildings will not sell at a premium.

1.2.2 Spatial semi-parametric matching

I implement a spatial matching estimator combined with regression-based bias adjustment (Abadie and Imbens (2006) and Abadie and Imbens (2011)). The average treatment effect on the treated is estimated by:

$$\tau_{sm} = \frac{1}{N_1} \sum_{j \in I_1} \left[Y_j - \sum_{k \in I_0} \frac{1}{m_{jk}} \left(Y_k + \hat{\mu}_0(\mathbf{X}_j) - \hat{\mu}_0(\mathbf{X}_k) \right) \right], \tag{2}$$

where N_1 is the number of treated buildings, I_1 is the set of treated buildings, I_0 is the set of control buildings, and j and k index treated and control buildings, respectively. Y_j and Y_k denote building values (log rent or log selling price) in the treated and control buildings; $\mathbf{X}_j$ and $\mathbf{X}_k$ denote covariate vectors for the treated and control units. The term $(\hat{\mu}_0(\mathbf{X}_j) - \hat{\mu}_0(\mathbf{X}_k))$ implements a bias adjustment that modifies the control outcome Y_k for the difference in covariate values between the treated and control units, $\mathbf{X}_j$ and $\mathbf{X}_k$. Since the outcome of interest is the SATT, the estimate for $\hat{\mu}(\cdot)$ is obtained by regressing the control outcomes on their covariates.

Each treated building j is matched with all k control buildings located within a 2-mile radius, constructed no more than three years earlier, such that m_{jk} is the number of matches for observation j.[15] A 2-mile radius was chosen to balance two competing factors: the desirability of minimizing the distance between treated and control observations versus the impact on the sample size. The importance of controlling for unobservable locational characteristics at a fine geographic scale is well-established in the real estate literature (Bollinger et al. (1998)), and from an econometric standpoint avoiding 'geographic mismatch' is important in order to achieve balance among the unobservables in the treated and control samples (Heckman et al. (1997), Duranton and Overman (2005)).[16] However, the pattern of increasing decentralization and decreasing density in new office space construction implies that the average distance between buildings is greater in newer buildings (Brueckner (2000), Lang (2000)), which constrains how small a radius between buildings can be used in order

[15] Matched buildings must also be located in the same city. This rules out buildings located within two miles of each other located on either side of a city boundary.

[16] An alternative to matching buildings on the basis of geographic distance is Mahalanobis matching. The Mahalanobis metric defines two buildings as near each other if they have similar covariates. Interestingly, matching buildings in my sample using Mahalanobis distance and all observable covariates results in very similar overlap in the covariate distributions between treatment statuses compared to matching solely on the basis of distance. However, since Mahalanobis matching results in considerably larger distances between treated and control buildings, geographic matching is likely preferable in this case so as to avoid locational mismatch.

to maintain a reasonable sample size.[17]

Bias-adjustment covariates included in the regression include building size, number of stories, building age, an indicator for class A buildings and an indicator for building-level amenities.[18] Buildings with active rental listings at the time I obtained the data also include information on whether utilities are included in rent or if tenants pay directly for their own utilities. Rental contracts are separated into three categories: Gross contracts, net contracts, and plus utilities contracts. Gross contracts quote rental rates inclusive of all services for the first year of the contract. Subsequent years are subject to 'escalation' clauses based on increases in expenses (including the impact of energy prices). Plus utilities contracts do not include any utilities in rent, in which case tenants pay for rent plus a separate utilities bill. In net contracts, all services are paid separately, including utilities and other operating costs such as cleaning, insurance and security. Hereafter I will refer to a 'utilities' contract as a contract where tenants pay for utilities - either a plus utilities contract or a net contract.[19]

The sales sample also includes the year of sale, and the change in employment in the building's metropolitan statistical area the year prior to sale (to control for changes in the regional demand for office space) among the bias-adjustment covariates. Since energy prices affect the cost of utilities, and therefore building-level operating costs, prevailing energy prices at the time a building sold may introduce heterogeneity in the willingness to pay for energy efficiency. To control for this effect I also include the price of the average 1-12 month regional wholesale forward electricity and natural gas contract six months before the building sold. The electricity price data were constructed using auction data from the major electricity trading hubs in the U.S., and the natural gas price data are from Henry Hub

[17]The sales results are robust to trimming the sample to treated and control buildings no more than approximately 1.0 miles apart, and the rent results are robust to trimming the sample to treated and control buildings no more than approximately 0.8 miles apart. See Appendix Section A.2 for these results.

[18]Amenities include: property manager on site, concierge, corner lot, courtyard or atrium, waterfront location, or the availability of nearby public transit, restaurants, day care, retail shops, or a fitness center.

[19]Given that advertised rental listings are observed at the same time for all buildings, I assume that the other operating costs applying to net contracts are the same across buildings in the treatment and control samples. If the selection on observables and overlap identifying assumptions described in Section 1.2.4 hold, as suggested by Tables 2 and 3 and the falsification test presented in Section 1.4.3, this is a plausible assumption.

auction data.[20]

1.2.3 Regression on the matched sample

In equation (2) above, the primary interest has been on estimating the average effect of energy codes on office building values. However, absent adverse selection between landlords and tenants, the premiums accruing to energy efficient buildings are predicted to vary depending on whether tenants or owners pay directly for utilities. In this Section I turn to a regression framework on the matched sample to test for evidence of this heterogeneity in the returns to energy efficiency. The estimating equation is:

$$Y_i = \alpha D_i + \beta' X_i + \theta Util_i D_i + \delta_j + \mu_i, \tag{3}$$

where D_i is a treatment indicator, X_i denotes the covariate vector, and interaction term $\theta Util_i D_i$ assesses whether the premium to buildings constructed under a code is heterogeneous in buildings where tenants pay directly for utilities.[21] δ_j denotes a locational fixed effect for group j, i.e. building j and the control buildings located within 2 miles of j; the error term is denoted by μ_i and is assumed independent of X_i and D_i.

1.2.4 Identifying assumptions

The crucial identifying assumption is unconfoundedness: controlling for observable covariates, the distribution of control outcomes must be the same in buildings with and without energy codes. Second, there must be a sufficiently dense overlap between the covariate distributions of treated and control observations, such that outcomes are observed for each treatment status at all values of the joint covariate distribution. Finally, identification also relies on the assumption of no general equilibrium effects, also referred to as stable unit treatment value (SUTVA): each buildings' potential outcomes are not affected by the treatment status of other buildings. These three assumptions define sufficient conditions to interpret

[20] Wholesale prices were used as they reflect the true resource cost of energy, and are incorporated into retail prices charged by utility companies over time (see Jaffee et al. (2012)). The results do not change significantly if I use retail energy prices instead of wholesale prices in the analysis.

[21] $Util$ is a dummy variable equal to one in buildings where tenants pay for utilities.

the estimated difference in outcomes as the causal effect of energy codes (Barnow et al. (1980), Rosenbaum and Rubin (1983), Rubin (1986)).

As detailed in Section 1.1.1, the decision to adopt a more stringent energy standard typically stems from a finding of positive net benefits from the standard, based on simulated energy standard impacts on the aggregate commercial building stock in a given state. A positive net benefit finding is not dependent on individual building characteristics, and code adoptions apply to all buildings constructed after the implementation date. This feature of the adoption decision bolsters the identification strategy. However, since buildings constructed under an energy code are one to three years newer than their matched control observation, and newer buildings may rent and sell at a premium, the regression-adjustment might not fully account for this effect in regions where there is poor overlap in the year built distribution. In addition, an implication of the unconfoundedness assumption is that the covariates are predetermined, or unaffected by treatment status. A covariate which may conceivably not be predetermined is the type of rental contract, particularly as it defines who is responsible for paying utilities. In Section 1.4.3 I present robustness checks to evaluate whether these factors might affect the results. While SUTVA is not testable in principle, in Appendix Section A.4 I provide an indirect test of whether it is a plausible maintained assumption.[22]

1.3 Data

This Section presents an overview of the steps involved in creating the dataset, followed by an estimate of the average building-level energy and operating cost savings.

1.3.1 Dataset creation

Cross-sectional data on office building hedonic characteristics, advertised rental rates (in $/sq.ft.) and last sale price were obtained from the CoStar Group, which maintains a building-level database and multiple listing service that has been tracking the commercial

[22] An alternative identification strategy that may seem advantageous is a regression discontinuity design. However, the highly discrete nature of the running variable, year of construction, is not suitable for the application of local linear regression methods.

real estate industry since the early 1980s.[23] Each building-level observation is also geocoded with a precise latitude and longitude coordinate.

CoStar's transaction notes were used to discard sales observations that were either made under "distressed" conditions, deferred tax transactions (1031 exchanges), bulk or portfolio transactions (which results in a sale price per square foot representing an average over several disparate properties), or non-arm's-length transactions. Data on the change in employment the year prior to building sale are from the Bureau of Labor Statistics, and wholesale electricity and natural gas forward contract prices are from Platts, the energy data vendor.[24] Observations from the rent sample that listed the rental rate as 'negotiable' were discarded.

Panel I in Figure 2 presents a map of the full population of CoStar office buildings (424,183 observations).[25] Observations that include either sales or rent data (133,068 observations) are shown in Panel II. Panels III and IV illustrate the sales (80,919 observations) and rent (76,285 observations) data, respectively. As can be seen in panels III and IV, though the sales and rent observations are a subset of the CoStar building population, the geographic distribution of both subsets retain a high degree of overlap with the full population.

Buildings were associated with a particular efficiency level by exploiting state-level and year-of-adoption variation in the implementation of the ASHRAE building energy standard, which has periodically prescribed more stringent energy efficiency characteristics in new commercial buildings since the 1980s (Figure 1). Data sources and further details of the dataset construction can be found in Section A.1 of the appendix. Identifying these state-level adoption dates has enabled me to associate buildings in my dataset as being constructed under a specific energy code regime. The code implementation dates identified in Figure 1 are the dates on which building permit applications were required to satisfy the new, more

[23]CoStar defines an office building as a structure in which the primary use is "to house employees of companies that produce a product or service primarily for support services such as administration, accounting, marketing, information processing and dissemination, consulting, human resources management, financial and insurance services, educational and medical services, and other professional services." CoStar also keeps information on retail buildings and 'flex' buildings that combine features of office and retail structures, but they are not included in the analysis.

[24]I thank Nancy Wallace for providing me with the energy price data. See Jaffee et al. (2010) for a detailed description of how the energy price dataset was created.

[25]This represents approximately half of the estimated population of office buildings in the U.S. (EIA (2003a)).

stringent energy code criteria in order to be approved by the local building department. Since I don't observe the date on which a building obtained its building permit, and since a lag occurs between the time a building permit is obtained and the building's construction, the following decision rule was used. In a given state, if the effective date of a new energy code implementation is in February or earlier, buildings constructed the year following the code's effective year are categorized as having been subject to the new energy code; buildings constructed before the new energy code (inclusive of the effective year) are categorized as controls. If the effective date of a new energy code implementation is in March or later, buildings constructed two years following the code's effective year are categorized as having been subject to the new energy code; buildings constructed before the new energy code are categorized as controls. This may lead to some degree of measurement error if a building is mis-categorized, and therefore to attenuation of the estimated coefficients.[26]

Having identified 'treated' buildings as having been constructed under a specific energy code regime, the geocode associated with each building was used to create the control sample, composed of all buildings located within a 2-mile radius of a treated building that were constructed before a given standard came into effect. The complete dataset is therefore composed of all buildings that could be identified as being constructed under one of four energy code regimes and all control buildings located within a 2-mile radius of a treated building. A list of treated building categories (ASHRAE 1989, IECC 2000, ASHRAE 1999 or ASHRAE 2004) and their control matches (pre-ASHRAE 1989, ASHRAE 1989, IECC 2000, or ASHRAE 1999) is presented in Table 1.[27]

Figure 3 depicts an example of two matched buildings in Scottsdale, Arizona. They exemplify the low-rise, tilt-up concrete construction practices used in new commercial building

[26]Estimates obtained by varying this decision rule by one month do not significantly change the results reported below. I have also obtained estimates by discarding buildings constructed during the effective year, and the results are qualitatively unchanged from those reported in the paper (the point estimates are slightly larger and remain statistically significant).

[27]The pooled nature of the sample resulted in a small number of buildings appearing simultaneously as both a treated and control observation; while including these buildings twice in the analysis does not change the results, in the tables reported below I have dropped the repeat matches that appeared under both treatments. For each observation appearing as both treated and control, it is only counted in the treatment regime with the smallest distance to its treated/control match.

structures that tend to be located outside central cities Lang (2003). Since each observation is geocoded, it is possible to analyze the extent of spatial dependence of the year built distribution. A large literature has documented the increasing decentralization of new residential and commercial construction that has been occurring since at least mid-century (Anas et al. (1998), Glaeser and Kahn (2004), Irwin and Bockstael (2007)), and the spatial dimension of the data is consistent with this finding. As illustrated in Figure 4, where office building observations are color-coded by the quantiles of the year built distribution in Maryland and Southern California, newer commercial construction (represented by blue dots) has a tendency to occur at the urban fringe. This phenomenon is prevalent in all the major urban areas in the dataset.

The dataset obtained from the steps outlined above results in a high degree overlap between the covariate distributions in the treated and control samples, with the notable exception of the building age distribution: buildings constructed under a code are almost 30 years newer, on average, than buildings in the control sample. This is to be expected, since most of the treated buildings are constructed under energy standards that began being implemented by states in the latter half of the 1990s. To improve this discrepancy in the year built distribution, I discard control observations constructed more than three years before the treated observation it is matched with. Panels (a) and (c) of Figure 5 show histograms of the year built distribution by treatment status before trimming out the older control buildings, while panels (b) and (d) show the trimmed histograms. The overlap in the year built distribution is much improved after trimming the sample.

Table 2 presents summary statistics for the treated and control buildings in the rent data set after trimming the sample to improve overlap in the year built distribution. Table 3 presents summary statistics for the trimmed sales sample. The tables indicate that the average building in the sample has two stories and measures about 30 thousand square feet, a profile that closely resembles the average office building in the U.S. (EIA (2003a)).[28]

The normalized difference for each covariate presented in the last column of Tables 2 and 3 is a measure of overlap among the covariates in the treated and control samples. A

[28] In contrast, the average green-labeled building is 15 stories high and measures over 300,000 sq. ft.

normalized difference less than 0.3 or so is typically considered good overlap (Imbens and Wooldridge (2009)).[29] Both tables indicate good overlap for most of the covariates. In both trimmed samples the mean disparity in the year built distribution by treatment status is 2 years (and, as noted above, is restricted to be no more than 3 years for a given treated building and its match). In the following Section, I present the results of a falsification test to assess whether the modest lack of overlap in the year built distribution might affect the results.

1.3.2 Estimated building-level energy and operating cost savings

Several engineering simulation studies have been conducted by the DOE to estimate ex-ante average energy savings attributable to ASHRAE Standards 1989, 1999 and 2004 and the IECC 2000 (Hadley and Halverson (1993), Department of Energy (2002), Department of Energy (2008)). These studies estimate the average reduction in site energy use intensity (EUI) per square foot attributable to upgrading to a more stringent ASHRAE code, assuming that actual construction practice is conducted in accordance with code requirements.[30]

The estimated site EUI energy savings arising from the matches in Table 1, based on the studies cited above, range from 5-11%. The simulated energy savings in these studies are obtained from a weighted average of the savings from buildings located in 11 climate regions in the U.S., with weights corresponding to the estimated share of new building construction in each region.[31] To obtain an estimate of the average EUI savings arising from the matches in my sample, I calculate a weighted average of the Department of Energy's simulated EUI savings for each treated-control match I observe in the data (Table 1), in each of the 11 climate regions, with weights corresponding to: (1) the share of the in-sample buildings in each climate region; and (2) the share of building matches (treated and controls) constructed

[29] The normalized difference reports the difference in average covariate values by treatment status, scaled by the square root of the sum of a given covariate's variance.

[30] Site EUI is defined as the annual BTU value of energy at the point it enters the building, normalized by building area; its unit of measurement is thousands of BTUs per square foot per year (kBTU/sf/yr).

[31] The climate regions roughly correspond to U.S. census divisions, except that northern and southern California each have their own climate region. It should be noted that the average savings range of 5-11% masks a considerable amount of variation in the savings across climate zones in the U.S. For example, in the southern Atlantic region the savings are typically higher by about 2-3%.

under each standard version. Performing this weighted average for the rent and sales samples separately results in similar estimated EUI savings of approximately 10% in each sample.

Assuming that reductions in site EUI lead to proportional reductions in utility costs, and given that office building utilities in the 11 DOE climate regions averaged approximately \$3.36/sq.ft./yr. in 2009 (BOMA (2010)), a 10% reduction in annual building energy costs will reduce utility costs by close to \$0.34/sq.ft./yr., or approximately 1.7% of sample average rent in my sample (which totals \$19.40).[32] Obtaining an estimate of the impact of a 10% utility operating cost saving on selling prices is a bit more difficult as it requires an estimate of net operating income (NOI), which I do not observe in the data.[33] An estimate from the Building Owners and Managers Association (BOMA (2010)) suggests average net operating income in the U.S. was \$18.90 per square foot in 2009; in that case, a saving of \$0.34/sq.ft. implies a 1.8% increase in NOI.[34] Both of these percent savings estimates represent one year of energy savings, and would therefore accrue to tenants over the length of a tenancy contract and to owners over the length of ownership.

1.4 Results

1.4.1 Spatial semi-parametric matching

Table 4 shows the results of estimating equation (2) for the trimmed rental and sales samples. Columns (1) and (2) present simple, or non-bias-adjusted, matching estimates, and columns (3)-(6) present the bias-adjusted estimates, where all of the observable covariates from Tables 2 and 3 are used in the bias-adjustment. These include the number of stories, building size, building class, whether or not tenants pay directly for utilities, the occupancy rate, and the availability of local or building amenities. For the sales sample the bias-

[32]The weighted average energy operating cost figure was obtained by using average energy operating costs in each of the 11 climate regions, weighted by the share of the in-sample buildings located in each climate region. The data are from the Building Owners and Managers Association's Experience Exchange Report. See BOMA (2010).

[33]The market price of commercial property can be expressed as $P_0 = \sum_{t=1}^{L} \frac{NOI_t}{(1+\delta_t)^t}$, where P_0 is the price at the purchase date, L is the expected length of ownership, NOI_t is net operating income (operating income - operating costs) in period t, and δ_t is the discount rate at t. Therefore, changes in operating income affect the selling price.

[34]Similar percent saving estimates result if NOI figures from 2007, 2008 or 2010 are used.

adjustment also includes the year the building was sold. The effect of energy prices on the sales price are incorporated in columns (5) and (6).

The first row of Table 4 presents the estimated impact of energy codes on the logarithm of rents (log rent). The bias-adjusted estimates in columns (3) and (4) suggest buildings constructed under an energy code are associated with approximately 2.7% higher rents. The second row presents the sale price results, which also indicate a significant impact of energy codes on selling prices: the bias-adjusted results in columns (3) and (4) imply that buildings constructed under an energy standard are associated with a 10.3% selling price premium. Columns (5) and (6) include the average regional wholesale forward electricity and natural gas prices six months before the building sold, in the bias-adjustment regression. The estimate is about 2% higher. The appendix presents results that indicate the robustness of these results to decreasing the maximum allowable distance between buildings.[35]

Taken together, these results provide evidence to reject empirical hypotheses 1 and 3, since more energy efficient buildings are associated with statistically significant rent and selling price premiums. The falsification test results in Section 1.4.3 below also indicate that this finding is not driven by the modest lack of overlap in the year built distribution.

1.4.2 Heterogeneity in the returns to energy efficiency

In Table 5 I assess whether the rent value premium differs on the basis of which party is contractually responsible for utility payments. In columns (5) and (6), the 'Utilities*Code' interaction term identifies whether tenants are responsible for utility bill payments. The estimate indicates that buildings constructed under a more stringent energy code in which tenants pay for utilities are associated with a 5.7% rent premium compared to buildings constructed just before a code came into effect. This estimate most clearly represents tenants' valuation of their expected energy cost savings over the length of their tenancy contract: since tenants benefit directly from lower utility bills in these buildings, they should also be

[35]For the sales samples I also estimated a specification that includes a variable capturing the amount of time between the year of construction and the year of sale. For the rent sample I estimated a specification that includes the amount of time between the year of construction and the year I observed the rent listings (2010). Including these variables does not change the point estimates substantively.

willing to pay higher rents to locate there. In this specification the estimated coefficient for the 'Code' variable is interpreted as the rent premium in buildings constructed under a code where owners pay for utilities for the first year of the contract. The coefficient value of -1.7%, though statistically insignificant, suggests some owners may be willing to charge lower rents relative to less energy efficient buildings, consistent with the possibility that some owners benefit from lower utility bill payments and may therefore be willing to reduce rental prices to attract tenants. In addition, the point estimate is economically significant in that it is in line with my back of the envelope estimate that on average, more stringent versions of the ASHRAE codes in my sample are associated with a 1.9% annual rent saving.

Since, as explained in Section 1.2.2, contracts in which building owners are responsible for utility bill payments also typically include escalation clauses for any increases in operating expenses, the estimated coefficient for such 'gross' contracts would be expected to reflect owners' valuations of annual energy savings exclusive of the expected effect of future energy price increases.[36]

The observed heterogeneity between buildings where tenants pay directly for utilities versus buildings where utilities are paid by the landlord provides statistically significant evidence that hypothesis 2 can be rejected. In buildings where tenants pay directly for utilities, rent premiums are higher in more energy efficient buildings.

1.4.3 Assessing unconfoundedness

To assess whether the rental contract is predetermined with respect to treatment status, I test whether the treatment and control observations in the matched sample have a statistically significant difference in the rate of rental contracts that stipulate tenants must pay directly for their utility bill ('utilities' contracts).[37] The third row of Table 4 presents these results, which are not suggestive of a systematic relation between the type of rental contract and treatment status, given the statistically insignificant difference in the prevalence

[36] Electricity prices were expected to increase across all geographic hubs as evidenced by positive differences between near-term (1-12 months) and long-term (25-36 month) forward electricity contracts at the time the rental rates were advertised (March 2010).

[37] Effectively, I estimate equation (2) but replace Y with an indicator variable for a utilities contract.

of utilities contracts between the treated and control buildings.

Treated buildings in Tables 2 and 3 are two years newer than their matched control observations, on average, and the normalized difference in the means of the year built distribution by treatment status suggests a modest imbalance in overlap. To assess whether this may cause a residual positive bias, I create a new sample that assigns a false energy code status to buildings, but retains the feature that newer buildings are matched to nearby buildings constructed no more than three years earlier. The sample is constructed by subtracting four years from the original coding file that assigns a treatment status to each building. While the resulting data assignment exhibits a similar discrepancy in the year built distribution as the original file (the false 'treated' buildings are two years newer on average), treatment status is uncorrelated in the original and falsified samples.[38] For example, fewer than 30% of the same buildings appear in the original versus falsified samples; buildings that appear in both samples have close to a 50/50 chance of having a different treatment status in the falsified sample.[39]

Table 6 presents the results of this falsification test. For the log rent results in the top row of Panel I, the simple matching estimates in columns (1) and (2) indicate a small positive difference between the false energy code buildings compared to the false controls, but it is statistically insignificant. The bias-adjusted log rent diffrentials in column (3) are negative and statistically insignificant, whereas the sales results, without incorporating energy prices, are centered at zero. Incorporating energy prices increases the point estimate but it remains highly insignificant.[40] Panel II of Table 6 also indicates that there is no statistical difference in rents between buildings in which either tenants or owners pay the utility bills.

Additional robustness checks are presented in the Appendix, including results that reduce

[38] The average 'treated' building in the falsified sample was constructed in 2003, whereas the average 'control' building was constructed in 2001. The normalized difference for the year built distribution is approximately 0.57. While the summary statistics in the falsified samples are not presented here, they are available from the author upon request.

[39] In the rent sample, 21% of buildings in the falsified sample appear in the original sample. In the sales sample, 27% of buildings in the falsified sample appear in the original sample.

[40] Clustering the standard error in the rent sample, in column (4) of Table 6, causes close to a 50% drop in the standard error estimate, which suggests significant negative intracluster correlation.While this may be indicative of misspecification of the chosen cluster size, clustering at smaller and larger levels of the sample does not alleviate the problem. Therefore, the results without clustering may be the most reliable.

the maximum allowable distance between buildings (Sections A.2 and A.3), test for the plausibility of the stable unit value treatment assumption (Section A.4), and assess whether there is evidence that building developers tried to 'game' energy code implementations (Section A.5). The evidence suggests the results are unaffected by these checks.

1.5 Conclusion

The question of whether energy efficient yet unlabeled office buildings are associated with premiums that reflect the value of energy savings has, thus far, remained undetermined. This is an important question not only because it reflects how broadly real estate markets internalize the returns to energy efficiency across heterogeneous segments of the building stock, but because it can shed light on whether adverse selection between landlords and tenants mitigates the returns to energy conservation investments in commercial buildings. In turn, assessing the prevalence of adverse selection about the energy use characteristics of commercial buildings is an important step in the determination of the optimal mix of policies to address the climate change externality.

In this study I use quasi-experimental variation in state-level energy code adoptions over the past twenty years, to find that on average, non-green-labeled buildings constructed under a more stringent energy code are associated with statistically significant rent and selling price premiums of approximately 2.7% and 10%, respectively. When tenants pay directly for utilities, buildings constructed under an energy standard are associated with statistically significant rent premiums of 5.7% compared to buildings constructed just before a code came into effect. These premiums suggest building owners obtain returns to energy conservation investments even in buildings where it is more difficult to observe energy efficiency characteristics, compared to buildings that have received a green label.

The results are also consistent with complete capitalization of the estimated energy savings in rents and prices. As noted previously, the estimated average energy operating cost savings from constructing a building under a more stringent energy standard is approximately 10%. This represents 1.9% of average sample rent and 1.8% of average building-level net operating income, and both of these percent savings estimates represent one year of energy savings, which would therefore accrue to tenants (that pay directly for utilities) over the length of a tenancy contract, and to owners over the length of ownership.

As detailed further in Appendix Section A.6, given plausible assumptions for the growth of utility costs, expected ownership or rental contract length, and the discount rate, rent and selling price premiums of approximately 5% and 10% (respectively) correspond to complete capitalization of the estimated energy savings. In other words, though the contract length, real estate market participants' subjective discount rate, and the assumed growth in utility costs are unobserved, a 5% rent premium when tenants pay for utilities and a 10% selling price premium represent complete capitalization for plausible values of these unobserved variables.

These results cast doubt on the frequently cited suggestion that adverse selection between landlords and tenants, pertaining to building energy use characteristics, merits policy intervention by contributing to an energy efficiency gap. An advantage of focusing on estimated premiums in unlabeled, energy efficient buildings is that they can generate a definitive answer as to whether commercial real estate markets fully reflect the value of energy efficiency. In this case, the evidence suggests that the answer is yes.

2. Pre-Labeling Market Valuations and the Causal Effect of Green Labels

Environmentally sustainable building practices, as sanctioned by green-labeling programs developed by the Environmental Protection Agency and the United States Green Buildings Council, have been growing at near-exponential levels in recent years (Kok et al. (2011)), yet an unresolved question is whether value premiums accruing to green-labeled buildings are a causal effect of receiving a label. Green-labeled buildings differ significantly from the average office building on the basis of observable characteristics, and since participation in these programs is voluntary, nonrandom selection into the stock of green buildings may result in both observed and unobserved heterogeneity that may account for at least a portion of the premium. For example, buildings whose owners seek to undergo the third-party monitoring and verification required in the labeling process tend to be landmark structures with unique architectural characteristics, which reinforces the likelihood that unobservable characteristics differ among labeled and unlabeled buildings.

The Energy Star and LEED labeling programs have been credited with delivering both significant energy savings and value premiums in green-labeled buildings (Turner and Frankel (2008), EPA (2006)), Eichholtz et al. (2010), Eichholtz et al. (2013)). However, determining to what extent green building premiums arise from selection bias affects the realized net benefits of green labeling policies, and has broader implications for climate policy. Green labels can be an economically efficient response to informational market failures that dampen the returns to energy conservation investments (Jaffe and Stavins (1994)). They may improve market outcomes in cases when adverse selection makes property managers unable to persuasively communicate building characteristics to potential buyers and tenants (Milgrom (2008)). If green labels cause energy efficient buildings to obtain market premiums that they otherwise would not have received due to adverse selection, they can play a part in the optimal mix of policy responses to the climate change externality, to the extent that the benefits of green labels outweigh their costs. However, this latter point remains open to question (Fuerst and McAllister (2011a), Newsham et al. (2009), Navarro (2009)).

In this paper, I use repeat sales observations and detailed building hedonic characteristics to estimate pre-labeling price premiums in buildings that subsequently received a green label, compared to similar buildings that never received a label. Since the green building sample is restricted to 206 buildings with data on sales transactions both before and after they received a label, I proceed to estimate post-labeling value premiums in green buildings and take the difference in the pre- and post-labeling price premiums, to obtain an estimate of the gain to labeling. Finally, I combine the gain to labeling estimate with data on the costs of obtaining a label, and calculate realized cost-benefit ratios for green labels.

The identification strategy uses the repeat sales data to difference out the effect of unobserved characteristics on building value. By differencing out potential sources of bias that remain constant before and after a building obtained a label, and incorporating the costs of green labels to obtain an estimate of the net benefits of a label, the approach improves upon previous work that has found large positive value premiums from green labels, such as Eichholtz et al. (2010).

The results indicate that the stock of green-labeled buildings that sold before they received a label did not sell at a premium compared to observationally similar control buildings. The estimated post-labeling premium is approximately 12%, which corresponds to a premium of \$20 per square foot. Combining these results with cost estimates of obtaining a green label, which range from \$0.05-\$8.50 per square foot, suggests the net benefits of green labels vary from \$11.50-\$19.95 per square foot. The estimated net benefits suggest building owners obtain sizable returns from green labels, but they are smaller than previous estimates that have ignored the costs of green labeling strategies, which have found premiums of 13%-20%, corresponding to benefits in the range of \$22-\$42 per square foot (Eichholtz et al. (2010), Fuerst and McAllister (2011b), Eichholtz et al. (2013)). The statistically insignificant pre-labeling premiums suggest nonrandom selection is not a source of bias affecting the estimated benefits of labels.

The paper is organized as follows. Section 2.1 presents background information on green labels and reviews the existing evidence on their effectiveness. Section 2.2 describes the data set creation, Section 2.3 presents the empirical strategy, and Section 2.4 discusses the results.

Section 2.5 briefly concludes.

2.1 Background

Green labels are awarded to buildings that demonstrate superior energy and environmental performance. In the U.S. buildings sector, two organizations are responsible for assigning the lion's share of these labels, the U.S. Green Buildings Council (USGBC) and the Environmental Protection Agency (EPA). The USGBC's Leadership in Energy and Environmental and Design (LEED) designation was introduced in 1993 to aid stakeholders involved in the building construction and operation trades to improve the environmental sustainability of the building stock (USGBC (2009a)). The EPA's Energy Star label was established in 1992 as a voluntary labeling program to promote energy efficient products. The Energy Star program was expanded to office buildings starting in 1999, and is awarded to buildings in the top quartile of energy performance (EPA (2012), EPA (2013)).

While the growth of certified commercial building space was slow to take off in the early years of these programs, the past five years have seen close to exponential growth in the fraction of certified space, with close to 20,000 certified commercial buildings in the U.S. as of the end of 2010 (Kok et al. (2011)). Several studies have been conducted on the market premiums resulting from green-labeled buildings, which have found benefits in the range of $27-$42 per square foot (Eichholtz et al. (2010), Fuerst and McAllister (2011b), Eichholtz et al. (2013)).

The Energy Star and LEED labels are widely touted by policymakers as bringing about improvements in the energy conservation characteristics of the building stock and increasing building values (EPA (2011), USGBC (2013), McGraw Hill Construction (2010)). However, though some studies have found that green-labeled buildings are associated with lower levels of energy use compared to an average building (Turner and Frankel (2008), EPA (2006)), others have found that ex-post evaluations of the energy performance of many labeled buildings is poorer than expected (ACEEE (2008), Newsham et al. (2009)).

Another consideration in the evaluation of green-labeled building performance is that participation in labeling programs is voluntary. The labeling procedure begins when a building owner or operator registers with either LEED or Energy Star for the purpose of obtaining a label. This is followed by third-party building energy performance monitoring, typically for an 8-12 month period (USGBC (2009b)), and a building is certified as 'green' only after adequately demonstrating criteria for energy and environmental performance above a predetermined threshold. It is the voluntary participation decision at the outset of the process that creates a potential for selection bias in the estimation of the benefits of a label. Nonrandom selection into the pool of certified buildings is evidenced by the observable characteristics of green buildings in comparison to the average office building: the typical green building is 15 stories high and measures over 300,000 square feet, in contrast with the average office building, which is about two stories high and measures about 20,000 square feet (EIA (2003a)).

2.2 Data

Both of the major green labeling programs for the building sector, Energy Star and LEED, publish the addresses of labeled buildings on their website. I matched the addresses of labeled buildings to the CoStar Group's repeat sales database, a building-level archive of commercial building sales transactions with detailed hedonic characteristics on 2.4 million commercial properties. Each building-level observation is geocoded with a precise latitude and longitude coordinate. CoStar's transaction notes were used to discard sales observations that were made either under "distressed" conditions, deferred tax transactions (or 1031 exchanges), bulk or portfolio transactions (which results in a sale price per square foot representing an average over several disparate properties), or that were not arm's-length transactions. I also discarded building observations that underwent a renovation between the pre- and post-labeling sale transactions, in order to rule out price effects that arise from a change in building features that are not controlled for in both the pre- and post-labeling transactions.[41] This matching process culminated in 206 labeled buildings with recorded sale transactions both before and after a building was labeled.

[41] Therefore, the labeled sample includes office buildings with pre- and post-labeling sale prices that were either renovated before both transactions occurred or with no recorded renovations.

The hedonic building characteristics included in the analysis are building size, number of stories, building age, year of sale, latitude, longitude, an indicator for high quality class A buildings and an indicator for building-level amenities.[42]

Figure 6 presents a map of the labeled building geographic distribution. The sample of green buildings spans eighteen states. At the state-level, California, Texas, Florida and Colorado have the largest concentration of green buildings in the sample, a pattern consistent with the population of green buildings in the U.S. (EPA (2011)).

A comparison group for the green buildings was created by matching each labeled building with two unlabeled buildings located in the same "market" as defined by CoStar, which approximately corresponds to the U.S. Census definition of a metropolitan statistical area. The labeled buildings were matched to their comparison buildings using the Mahalanobis metric, which selects matches by finding the smallest covariance-weighted Euclidean distance between the vectors of hedonic characteristics for a given labeled building and the unlabeled buildings in the same market. Since year of sale is one of the variables in the vector of hedonic characteristics, the matching process resulted in two separate comparison samples, one for the pre-labeling sales transactions and one for the post-labeling transactions. Figure 7 illustrates sets of pre- and post- labeling matches for green buildings in Boston, Massachussetts and Denver, Colorado.

Table 7 presents summary statistics for the pre-labeling sample, and Table 8 presents summary statistics for the post-labeling sample. The normalized difference for each covariate presented in the last column of each Table is a measure of overlap among the covariates in the green buildings and their control samples. A normalized difference less than 0.3 or so is typically considered good overlap (Imbens and Wooldridge (2009)).[43] Though the green buildings are slightly larger and taller than their controls on average, there is sufficient variability in these characteristics to maintain good overlap for all of the observable covariates.

[42] Amenities include: property manager on site, concierge, corner lot, courtyard or atrium, waterfront location, or the availability of nearby public transit, restaurants, day care, retail shops, or a fitness center.

[43] The normalized difference reports the difference in average covariate values by treatment status, scaled by the square root of the sum of a given covariate's variance.

2.3 Empirical Strategy

The outcome of interest is the sample average treatment effect on the treated (SATT), the average impact of green labels on selling values in labeled buildings. In contrast to previous work that has focused on estimating the SATT on building values exclusively in buildings that have already received a label, I estimate the SATT in two samples: selling prices in buildings that have received a green label and selling prices in the same set of buildings *before* they received a label.

Buildings are assigned to one of two states: labeled and unlabeled buildings. Using the potential outcomes framework, let D_i=1 if building i is green-labeled, and D_i=0 if building i has never received a label. Potential outcome $Y_i(1)$ denotes building values in building i contingent on having received a label (at the time of data collection), and potential outcome $Y_i(0)$ denotes building values in building i, contingent on never having received a label. The SATT can be expressed as

$$\alpha_{TT} = E\left[Y_i(1)|D_i = 1\right] - E\left[Y_i(0)|D_i = 1\right] = E\left[Y_i(1) - Y_i(0)|D_i = 1\right]. \tag{4}$$

Observed prices in green-labeled buildings can be used to identify $E\left[Y_i(1)|D_i = 1\right]$, average building values in labeled buildings. However, the counterfactual $E\left[Y_i(0)|D_i = 1\right]$, average building values in labeled buildings *had they never received a label*, is unobserved. If the set of green-labeled buildings had been randomly selected to receive a label, it would be the case that, on average, values in labeled buildings had they not received a label would be the same as values in buildings that never obtained a label:

$$E\left[Y_i(0)|D_i = 1\right] = E\left[Y_i(0)|D_i = 0\right], \tag{5}$$

and the set of buildings that have never received a label could be used as a control group to estimate the unobserved counterfactual. However, the voluntary nature of the green-labeling decision creates nonrandom selection into treatment, such that

$$E\left[Y_i(0)|D_i = 1\right] = E\left[Y_i(0)|D_i = 0\right] + \eta, \tag{6}$$

where η represents a systematic variation in the value of the set labeled of buildings, before they receive a label, from the set of buildings that have never been labeled, which may result from nonrandom selection. My identification strategy generates a credible estimand of the causal effect of the label (denoted α^*_{TT}) by pointing out that if the unobservable characteristics in green buildings that generate η remain constant before and after a building receives a label, the following two SATT estimands can be used to identify α^*_{TT}:

$$\alpha_{prl} = E\left[Y_{i,prl}(1)|D_i = 1\right] - E\left[Y_{i,prl}(0)|D_i = 0\right] + \eta, \tag{7}$$

where α_{prl} measures the average difference in green-labeled buildings and nearby control buildings before they received a label (*prl* refers to this pre-labeled status), and

$$\alpha_{pol} = E\left[Y_{i,pol}(1)|D_i = 1\right] - E\left[Y_{i,pol}(0)|D_i = 0\right] + \eta, \tag{8}$$

where α_{pol} measures the average difference in green-labeled buildings and nearby control buildings after they received a label (*pol* refers to this post-labeled status).

α^*_{TT} is generated by taking the difference between (5) and (4):

$$\begin{aligned}
\alpha^*_{TT} &= \alpha_{pol} - \alpha_{prl} \\
&= E\left[Y_{i,pol}(1)|D_i = 1\right] - E\left[Y_{i,pol}(0)|D_i = 0\right] + \eta \\
&- E\left[Y_{i,prl}(1)|D_i = 1\right] - E\left[Y_{i,prl}(0)|D_i = 0\right] + \eta \\
&= E\left[Y_{i,pol}(1)|D_i = 1\right] - E\left[Y_{i,pol}(0)|D_i = 0\right] \\
&- E\left[Y_{i,prl}(1)|D_i = 1\right] - E\left[Y_{i,prl}(0)|D_i = 0\right].
\end{aligned} \tag{9}$$

Using repeat sales data on pre- and post-labeling green building valuations can be used to difference out the η in the last two lines of equation (6). This generates the causal effect of green labels on values under the assumption that the unobservable characteristics determining selection into treatment remain constant before and after a building receives a label.

2.3.1 Spatial semi-parametric matching

To estimate the α^*_{TT} estimand defined above, I implement a spatial matching estimator combined with regression-based bias adjustment (Abadie and Imbens (2006) and Abadie and Imbens (2011)). The average treatment effect on the treated in the pre-labeling is estimated by:

$$\tau_{prl} = \frac{1}{N_1} \sum_{j \in I_1} \left[Y_j - \sum_{k \in I_0} \frac{1}{m_{jk}} \left(Y_k + \hat{\mu}_0(\mathbf{X}_j) - \hat{\mu}_0(\mathbf{X}_k) \right) \right], \tag{10}$$

where N_1 is the number of green-labeled buildings (hereafter referred to as green buildings), I_1 is the set of green buildings, I_0 is the set of control buildings, and j and k index green and control buildings, respectively. Y_j and Y_k denote building values (log selling price) in the pre-labeled green buildings and the control buildings; $\mathbf{X}_j$ and $\mathbf{X}_k$ denote covariate vectors for the green and control units. The term $(\hat{\mu}_0(\mathbf{X}_j) - \hat{\mu}_0(\mathbf{X}_k))$ implements a bias adjustment that modifies the control outcome Y_k for the difference in covariate values between the green and control units, $\mathbf{X}_j$ and $\mathbf{X}_k$. Since the outcome of interest is the SATT, the estimate for $\hat{\mu}(\cdot)$ is obtained by regressing the control outcomes on their covariates (see Abadie and Imbens (2011) for further details).

Each green building j is matched with the two 'nearest' control buildings located in the same real estate market, where nearness is defined using the Mahalanobis distance, as described in the Section 2.2. The control observations are indexed by k, and m_{jk} is the number of matches for observation j. In this case, m_{jk}=2. The Mahalonobis metric used for matching incorporates the following covariates: building size, number of stories, building age, year of sale, latitude, longitude, an indicator for class A buildings and an indicator for building-level amenities.[44] The bias-adjustment covariates included in the regression to obtain $\hat{\mu}$ includes the same covariates. The average distance between the buildings in this approach is about 4 miles. The importance of controlling for locational characteristics at a fine geographic scale is well-established in the real estate literature (Bollinger et al. (1998)), and from an econometric standpoint avoiding 'geographic mismatch' is important in order to achieve balance among the unobservables in the treated and control samples (Heckman et al. (1997), Duranton and Overman (2005)). However, since green building tend to be 'trophy' or landmark buildings with unique characteristics (for example, they are taller and larger than nearby buildings), few buildings with similar observable characteristics appear in the immediate vicinity of a green building. For this reason, the matching region was set to buildings in the same metropolitan area.

[44]Amenities include: property manager on site, concierge, corner lot, courtyard or atrium, waterfront location, or the availability of nearby public transit, restaurants, day care, retail shops, or a fitness center.

The matching estimator from equation (7) is also implemented to estimate post-labeling valuations:

$$\tau_{pol} = \frac{1}{N_1} \sum_{j \in I_1} \left[Y_j - \sum_{l \in I_0} \frac{1}{m_{jl}} \left(Y_l + \hat{\mu}_0(\mathbf{X}_j) - \hat{\mu}_0(\mathbf{X}_l) \right) \right], \tag{11}$$

where the same set of green buildings Y_j is used, but since the year of sale differs from the pre-labeling sample, the set of control buildings l is also different.[45]

2.3.2 Realized benefit-cost ratios

Having obtained estimates for both $\hat{\tau}_{prl}$ and $\hat{\tau}_{pol}$, which are both asymptotically normally distributed (Abadie and Imbens (2006)), the following test is applied to assess whether the two estimates are statistically different:

$$\text{DIFF} = \frac{\hat{\tau}_{pol} - \hat{\tau}_{prl}}{\sqrt{se(\hat{\tau}_{pol})^2 + se(\hat{\tau}_{prl})^2}}. \tag{12}$$

If DIFF is greater than the critical value for a two-tailed Z-test at the 5% significance level (1.96), I will take this as evidence that we cannot reject the hypothesis that the pre- and post-labeling premiums differ from each other. Either way, the term $\hat{\alpha}^* = \hat{\tau}_{pol} - \hat{\tau}_{prl}$ is an estimate of the causal effect of a green label, and represents the average benefits a building owner can expect from obtaining a label. It is also measures the market valuation of the expected stream of benefits accruing from a green label.

From a policy evaluation perspective, a more relevant calculation is the benefit cost ratio of a green-labeling policy, which requires considering the present value of the net benefits (i.e. the benefits net of the costs) of a label. To calculate the average net benefit, I will combine $\hat{\alpha}^*$ with information on the costs of obtaining a green label, discussed below in Section 2.4.2.

[45] However, close to 50% of the control buildings appear in both samples.

2.4 Results

2.4.1 Matching

The first row of Table 9 presents the results of estimating equation 10, and the second row presents results for equation 11. For purposes of comparison, columns (1) and (2) show results when only geographic distance is used as a criterion to match green buildings with the two control nearest buildings. Columns (3) and (4) show results from using the Mahalanobis metric and all observable covariates to match green buildings with the nearest two control buildings located in the same real estate market. Columns (1) and (3) present results of applying a simple matching estimator without applying the bias adjustment function $\hat{\mu}_0$. Columns (2) and (4) show the results of implementing the bias-adjustment.

Both the geographic matching and Mahalanobis matching estimates indicate a statistically insignificant pre-labeling premium, as shown in the first row, columns (2) and (4). In contrast, the post-labeling premium is statistically significantly positive using both geographic and Mahalanobis matching, shown in columns (2) and (4) of the second row. The premium is approximately 9% using the bias-adjusted geographic matching estimator, in column (2), and approximately 12% using the bias-adjusted Mahalanobis matching estimator, in column (4). Plugging the bias-adjusted estimates from column (4) into equation 12 results in a test statistic value of 2.45, which provides evidence to reject the null hypothesis that the pre- and post-labeling premiums are equal, at the 1% level.

To address concerns regarding whether premiums in the pool of buildings with sales observations both before and after a building sold may differ from those in which only one post-labeling transaction is observed, the third row of Table 9 presents results of applying equation 11 to estimate post-labeling premiums in the set of buildings that sold after being labeled. As shown, 206 buildings were observed to be sold both before and after they were labeled, whereas 966 building sale transactions were observed in the full post-labeled sample. The bias-adjusted Mahalanobis matching premium in the full post-labeled sample is approximately 10%, which is not statistically different from the estimate of 12% in the restricted sample. These results suggest that, on the basis of post-labeling premiums in the two samples, there is no evidence of selection into the pool of buildings that sold twice, before and after they were labeled, compared to buildings that are only observed to have sold after they were labeled.

2.4.2 Net benefits

Given that the pre-labeling premium is statistically zero, the estimated premium for a green building, as discussed in the previous section, is approximately 12%. Since the average building selling price prior to receiving a label is $171, the average premium is approximately $20 per square foot. This figure reflects the market's valuation of the net present value of the benefits of owning and operating a green building.

Costs incurred in the green-labeling process include capital costs of building upgrades, process modifications, labeling fees, as well as consulting and contractor fees. The number of studies that have assessed the financial costs of green labels is smaller than the work that focus solely on their benefits by an order of magnitude, and the former tend to be based on small sample sizes. Studies that do assess the financial costs of green buildings suggest the additional outlays, for buildings of approximately the same size and height as those in the sample, range from about $0.35-$8.50 per square foot (Kats (2003a), Kats (2003b) and Yudelson (2007)). The labeling fees alone come to about $0.05-$0.07 per square foot.

These benefit and cost values lead to a range of net benefit estimates. On the high end, a building owner that purchases an unlabeled building that is already energy efficient, without any need for capital upgrades or process changes, and does not pay a premium for the energy efficiency characteristics (a likely outcome based on the pre-labeled building results in Table 9) can expect to pay only about $0.05 per square foot to obtain a label. This reduces the benefit estimate of $20 per square foot by a negligible amount, to $19.95 per square foot. On the lower end, a building owner who must first invest in building upgrades and all the other associated costs before receiving a label can expect a net benefit between $11.50-$19.50 per square foot.

2.5 Conclusion

This paper has proposed a simple approach to identify the causal net benefits of green labels. Most of the popular discussion on the benefits of green labels has both ignored the potential bias that may arise from nonrandom selection and neglected to consider the costs incurred in the labeling process. I have implemented a matching estimator that makes use of green building sales transactions before they received a label to identify the causal value premium of a green label, of approximately 12%, or $20 per square foot. This estimate represents the real estate markets's assessment of the net present value of the benefits of owning and operating a green building. Combining these results with estimates of the costs associated with obtaining a green label suggests the causal net benefits of obtaining a green label range from $11.50-$19.95 per square foot.

These estimated net benefits suggest building owners obtain returns from green labels that are smaller than previous estimates that have focused solely on the benefits, which have found premiums of 13%-20%, corresponding to benefits in the range of $22-$42 per square foot (Eichholtz et al. (2010), Fuerst and McAllister (2011b), Eichholtz et al. (2013)). This implies that while the lower bound of previous estimates of the benefits of green labels are quite similar to the estimated premiums in this study, incorporating the costs of green labels can reduce the estimated net benefits by up to 50%.

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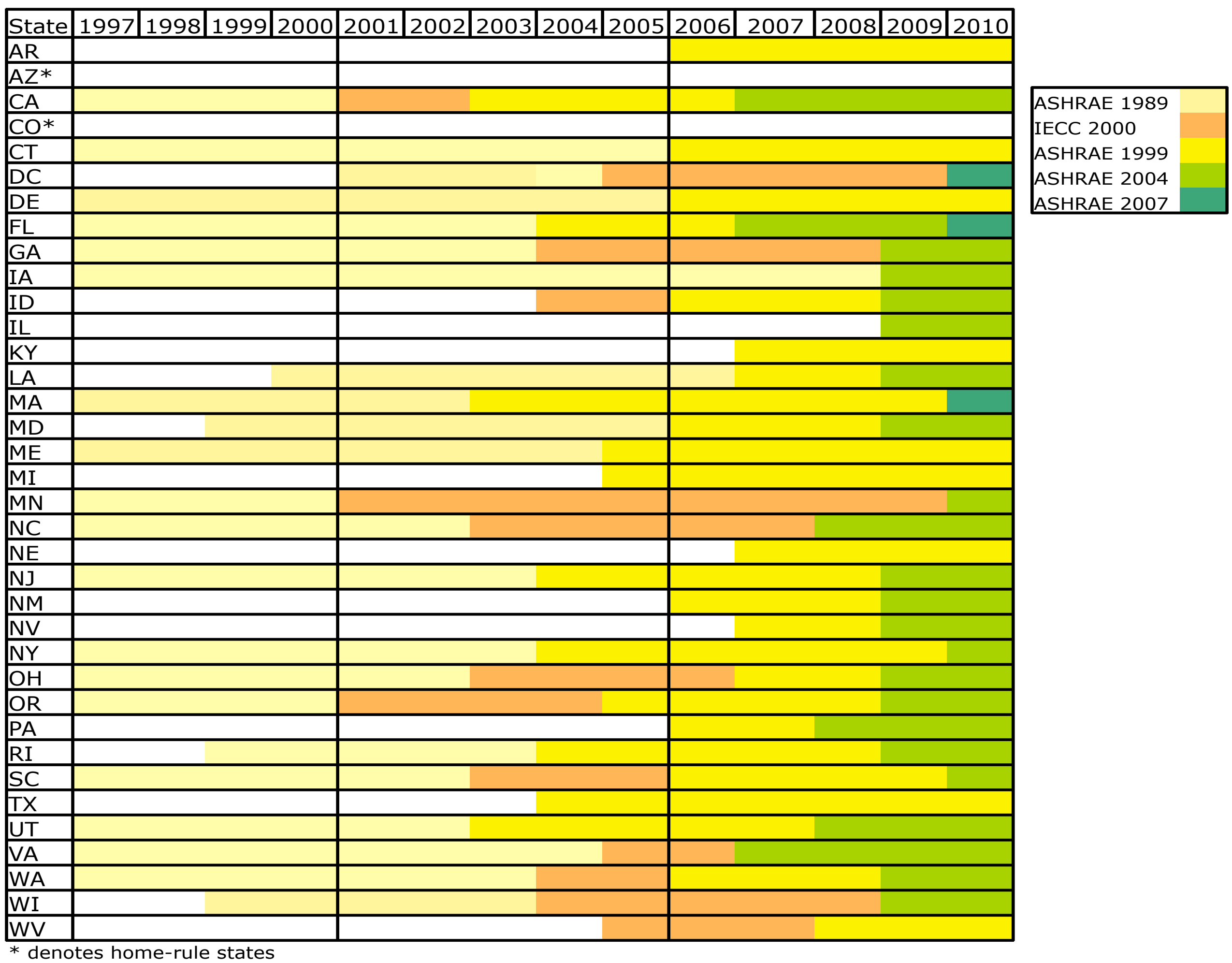
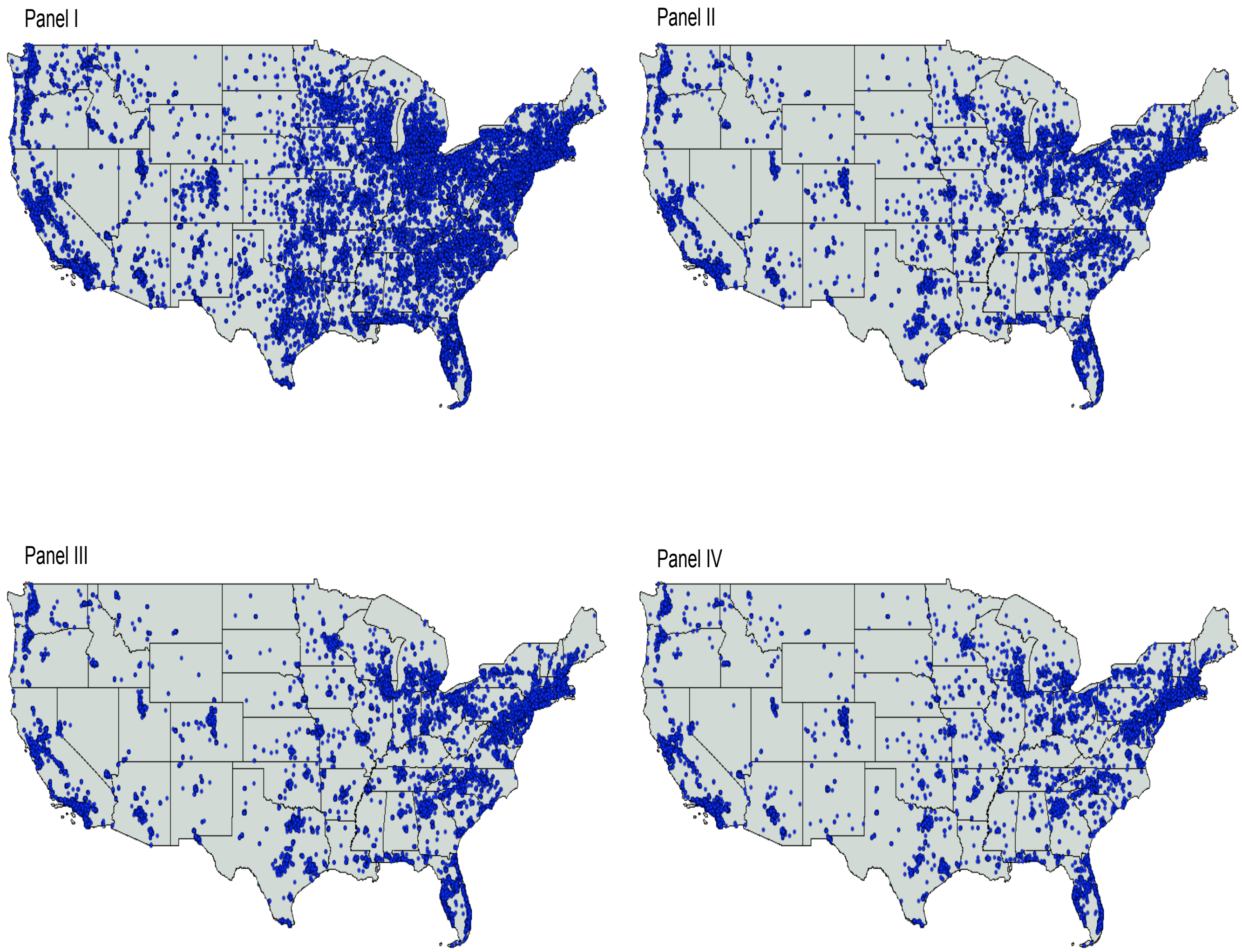
Figures and Tables

Figure 1: State Adoptions



Notes: The figure identifies state-level implementation dates for increasingly stringent versions of a mandatory energy efficiency standard. Details of the dataset creation are described in Section 1.3 and Appendix Section A.1.

Figure 2: Buildings Data

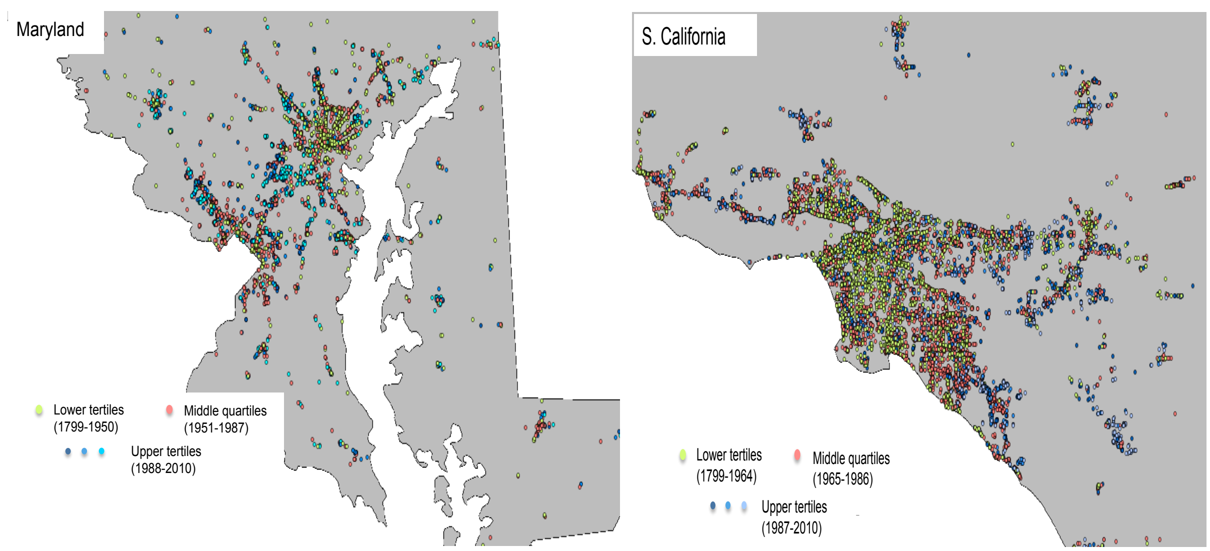


Notes: Buildings are represented by blue dots. Panel I depicts the full population of CoStar office buildings. Panel II presents observations that include either rent or sales data. Panels III and IV depict the sales and rent data, respectively.

Figure 3: Building Match Example

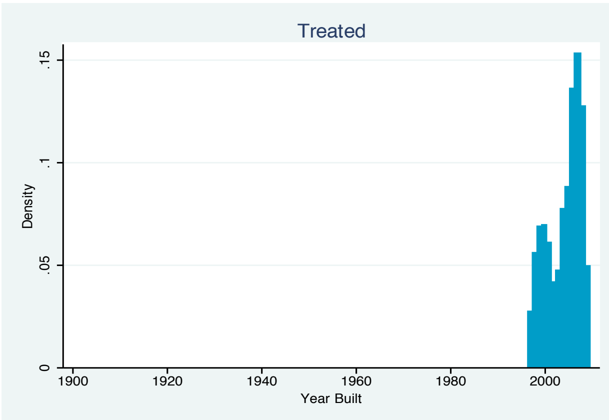
Notes: Treated and control matches located in Scottsdale, AZ. The building on the left was constructed in 2006. The building on the right was constructed in 2003. ASHRAE 1999 came into effect in September 2003.

Figure 4: Quantiles of the Year Built Distribution

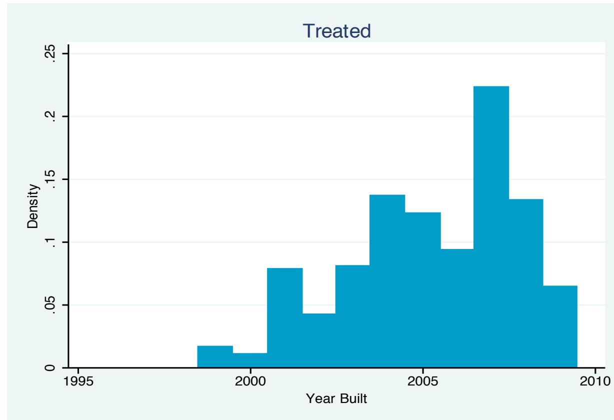


Notes: Each dot represents a building. The lowest tertiles of the year built distribution (the oldest buildings) are represented by yellow dots,t he middle quartiles are represented by red dots, and the upper tertiles (newest buildings) are represented by blue dots.

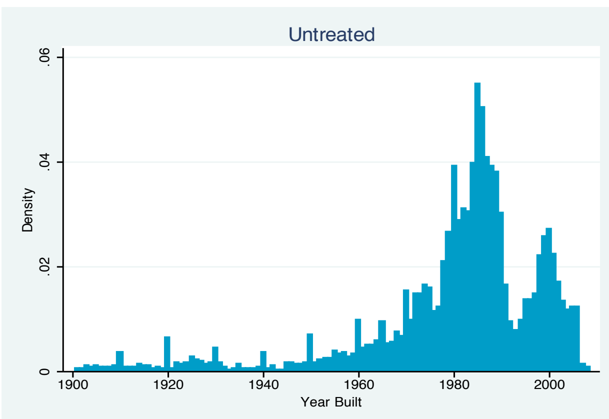
Figure 5: Year Built Distribution (Combined Rent and Sales Samples)



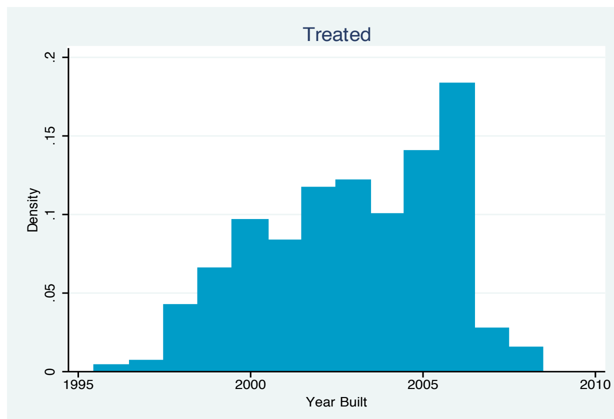
(a) Full sample (mean=2003)



(b) Trimmed sample (mean=2005)



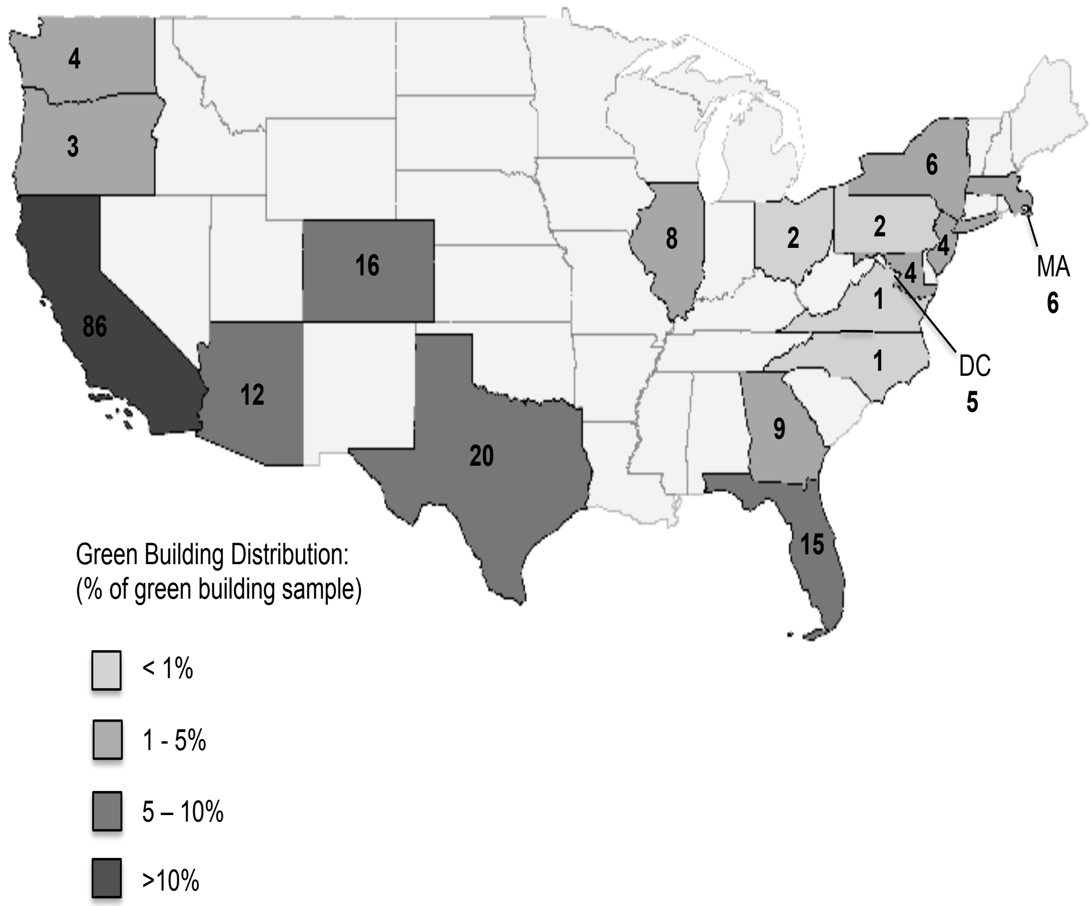
(c) Full sample (mean=1982)



(d) Trimmed sample (mean=2003)

Notes: Panels (a) and (c) show histograms of the year built distribution by treatment status before trimming out the older control buildings, while panels (b) and (d) show the trimmed histograms.

Figure 6: Sample Green Building Distribution



Note: The number of green office building observations in each state is also listed above.

Figure 7: Building Match Examples

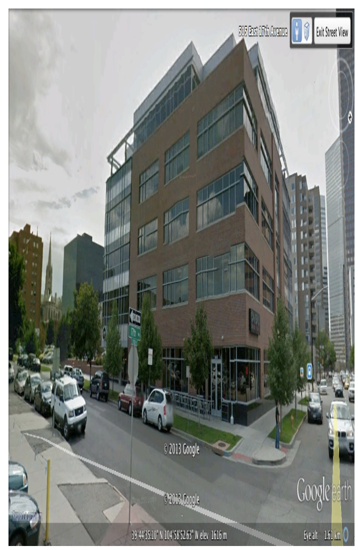
(a) Green building, MA

(b) Control, pre-label, MA

(c) Control, post-label, MA

(d) Green building, CO

(e) Control, pre-label, CO



(f) Control, post-label, CO

Notes: Each row shows a green building and its associated pre- and post-labeling matches. The top row buildings are located in Boston, Massachusetts. The bottom row buildings are located in Denver, Colorado.

Table 1: Treated and Control Categories

Treated	Control
ASHRAE 1989	pre-ASHRAE 1989
IECC 2000	ASHRAE 1989
ASHRAE 1999	ASHRAE 1989
ASHRAE 1999	IECC 2000
ASHRAE 2004	IECC 2000
ASHRAE 2004	ASHRAE 1999

Notes: The data pool together building matches from multiple treated and control categories, listed above.

Table 2: Energy Code Summary Statistics, Rentals

	CODE				NO CODE				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	2.24	2.22	1	57	2.18	2.97	1	70	0.02
Size (000s)	35.23	38.91	2.00	315.60	36.21	66.64	1.20	1504	-0.02
Built	2005	2.52	1999	2009	2003	2.63	1996	2008	0.55
Class A (%)	19.78	39.85	0	1	18.36	38.73	0	1	0.02
Utilities (%)	51.93	49.98	0	1	47.75	49.97	0	1	0.06
Occupancy (%)	70.80	19.62	30	100	75.55	18.79	30	100	-0.17
Amenities (%)	28.84	45.32	0	1	34.59	47.59	0	1	-0.09
Observations:	861				1,289				
Avg. Distance:	0.56	miles							

Notes: The table presents summary statistics for the sample of green buildings and nearby controls located within the same real estate market and matched using the Mahalanobis metric. The normalized difference presented in the last column measures the degree of overlap for each covariate across the treated and control samples. It is defined as $(\bar{X}_1 - \bar{X}_0)/(\sqrt{S_1^2 + S_0^2})$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table 3: Energy Code Summary Statistics, Sales

	CODE				NO CODE				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	1.72	1.28	1	16	1.49	0.91	1	13	0.15
Size (000s)	24.09	40.15	0.89	350	19.65	34.35	1.28	402	0.08
Year Sold	2007	1.58	2002	2009	2006	1.79	2002	2009	0.42
Built	2006	2.15	1999	2009	2004	2.37	1996	2008	0.61
Class A (%)	10.69	30.91	0	100	6.37	24.74	0	100	0.11
Amenities (%)	20.11	40.10	0	100	35.42	47.85	0	100	-0.25
High Vacancy	0.005	0.067	0	1	0.005	0.074	0	1	-0.01
Employment Δ	2.10	2.05	-7.70	6.35	2.25	2.23	-7.70	6.35	-0.12
Elec. Price (\$/MWh)	69.90	14.79	29.09	130.6	67.02	17.33	25.97	123.5	0.13
Nat. Gas Price (\$/MMBtu)	8.02	1.69	2.52	12.72	7.66	2.10	2.52	12.72	0.13
Observations:	393				1,104				
Avg. Distance:	0.50	miles							

Notes: The table presents summary statistics for the sample of green buildings and nearby controls located within the same real estate market and matched using the Mahalanobis metric. The normalized difference presented in the last column measures the degree of overlap for each covariate across the treated and control samples. It is defined as $(\bar{X}_1 - \bar{X}_0)/(\sqrt{S_1^2 + S_0^2})$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table 4: Matching and Bias-Adjusted Matching Results

	(1)	(2)	(3)	(4)	(5)	(6)	Treated	Control
Dependent Variable:								
Log(rent)	0.047***	0.047***	0.027**	0.027**			861	1,289
	(0.014)	(0.012)	(0.014)	(0.012)				
Log(price)	0.041	0.041	0.103***	0.103**	0.122***	0.122***	393	1,104
	(0.028)	(0.031)	(0.028)	(0.030)	(0.028)	(0.030)		
Utilities	0.037*	0.037*	0.010	0.010			861	1,289
	(0.021)	(0.019)	(0.021)	(0.020)				
Bias-Adjusted	NO	NO	YES	YES	YES	YES		
Energy Prices	NO	NO	NO	NO	YES	YES		
Robust s.e.	YES	NO	YES	NO	YES	NO		
Clustered s.e.	NO	YES	NO	YES	NO	YES		
Mean distance, rent:	0.6 miles							
Mean distance, sales:	0.5 miles							

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Table 5: Heterogeneity in the Returns to Energy Efficiency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Code	0.047***	0.047***	0.051***	0.051***	0.026*	-0.017	-0.017
	(0.010)	(0.014)	(0.010)	(0.013)	(0.015)	(0.032)	(0.043)
Utilities∗Code					0.055**	0.057**	0.057*
					(0.024)	(0.024)	(0.031)
Utilities					-0.118***	-0.120***	-0.120***
					(0.021)	(0.021)	(0.026)
Size (000s)			0.037*	0.037	0.036*	0.032	0.032
			(0.021)	(0.023)	(0.021)	(0.021)	(0.021)
Stories			0.010**	0.010**	0.009**	0.008**	0.008*
			(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Class A			0.098***	0.098***	0.088***	0.087***	0.087**
			(0.021)	(0.027)	(0.021)	(0.021)	(0.025)
Occupancy			0.092**	0.092	0.087	0.101*	0.101
			(0.038)	(0.047)	(0.038)	(0.039)	(0.049)
Amenities			0.017	0.017	0.013	0.016	0.016
			(0.018)	(0.021)	(0.017)	(0.018)	(0.022)
Constant	2.96***	2.96***	2.39***	2.39***	2.55***	2.73***	2.73***
	(0.184)	(0.007)	(0.328)	(0.336)	(0.323)	(0.329)	(0.331)
R^2	0.70	0.70	0.72	0.72	0.73	0.73	0.73
adj. R^2	0.50	0.50	0.53	0.53	0.54	0.54	0.54
Fixed effects	YES	YES	YES	YES	YES	YES	YES
Robust s.e.	YES	NO	YES	NO	YES	YES	NO
Clustered s.e.	NO	YES	NO	YES	NO	NO	YES
Year built dummies	NO	NO	NO	NO	NO	YES	YES
Observations:	2,150	2,150	2,150	2,150	2,150	2,150	2,150
Mean distance:	0.6 miles						
Maximum distance:	2.0 miles						

Notes: Standard errors are in parentheses. * denotes significance at 10% level, ** denotes significance at 5% level, and *** denotes significance at 1% level. Clustered s.e. denotes clustering at the market level. Each regression includes 850 clusters made up of a treated building and its nearby controls.

Table 6: Falsification Test

	(1)	(2)	(3)	(4)	(5)	(6)	Treated	Control
Panel I: Matching								
Log(rent)	0.018	0.018*	-0.028*	-0.028***			870	1,346
	(0.014)	(0.010)	(0.014)	(0.010)				
Log(price)	-0.177***	-0.177*	-0.007	-0.007	0.076	0.076	413	887
	(0.053)	(0.105)	(0.053)	(0.104)	(0.053)	(0.104)		
Utilities	0.037*	0.037*	0.024	0.024			870	1,346
	(0.021)	(0.019)	(0.021)	(0.019)				
Bias-Adjusted	NO	NO	YES	YES	YES	YES		
Energy Prices	NO	NO	NO	NO	YES	YES		
Robust s.e.	YES	NO	YES	NO	YES	NO		
Clustered s.e.	NO	YES	NO	YES	NO	YES		
Mean distance, rent:	0.5 miles							
Mean distance, sales:	0.5 miles							

	(1)	(2)	(3)	(4)	(5)	(6)
Panel II: Heterogeneity by Utility Contract						
Code	0.020*	0.022*	0.022	0.009	0.007	0.007
	(0.012)	(0.011)	(0.013)	(0.015)	(0.031)	(0.035)
Utilities∗Code				0.035	0.031	0.031
				(0.025)	(0.025)	(0.028)
Utilities				-0.151***	-0.151***	-0.151***
				(0.023)	(0.022)	(0.027)
R^2	0.64	0.67	0.67	0.69	0.69	0.69
adj. R^2	0.41	0.45	0.45	0.48	0.48	0.48
Fixed effects	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	YES	YES	YES	YES
Year built dummies	NO	NO	NO	NO	YES	YES
Robust s.e.	YES	YES	NO	YES	YES	NO
Clustered s.e.	NO	NO	YES	NO	NO	YES
Observations:	2,216	2,216	2,216	2,216	2,216	2,216

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Table 7: Summary Statistics, Pre-Label Sample

	GREEN				CONTROL				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	14.7	11.5	2	62	11.1	9.4	1	52	0.24
Size (000s)	322.2	270.1	11.4	2,002	226.2	262.3	1.03	2,550	0.25
Year Sold	2001	3.5	1991	2013	2002	4.1	1991	2009	-0.19
Built	1982	16.9	1912	2004	1979	19.6	1900	2006	0.12
Class A (%)	82.0	38.4	0	100	74.0	43.9	0	100	0.14
Amenities (%)	97.1	16.8	0	100	97.3	16.1	0	100	-0.01
Observations:	206				412				

Notes: The table presents summary statistics for the sample of green buildings and nearby controls located within the same real estate market and matched using the Mahalanobis metric. The normalized difference presented in the last column measures the degree of overlap for each covariate across the treated and control samples. It is defined as $(\bar{X}_1 - \bar{X}_0)/(\sqrt{S_1^2 + S_0^2})$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table 8: Summary Statistics, Post-Label Sample

	GREEN				CONTROL				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	14.7	11.5	2	62	11.1	9.1	1	49	0.25
Size (000s)	322.2	270.1	11.4	2,002	229.5	241.2	1.0	2,438	0.26
Year Sold	2007	2.2	2000	2013	2006	2.2	1997	2009	0.32
Built	1982	16.9	1912	2004	1980	20.2	1900	2009	0.08
Class A (%)	82.0	38.4	0	100	74.0	43.9	0	100	0.14
Amenities (%)	97.1	16.8	0	100	95.9	19.9	0	100	0.05
Observations:	206				412				

Notes: The table presents summary statistics for the sample of green buildings and nearby controls located within the same real estate market and matched using the Mahalanobis metric. The normalized difference presented in the last column measures the degree of overlap for each covariate across the treated and control samples. It is defined as $(\bar{X}_1 - \bar{X}_0)/(\sqrt{S_1^2 + S_0^2})$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table 9: Matching and Bias-Adjusted Matching Results

Nearest two control neighbors located near a green building

	(1)	(2)	(3)	(4)	Treated	Control
Dependent Variable:						
Log(price)	-0.006	0.040	-0.104**	-0.051	206	412
pre-label	(0.040)	(0.040)	(0.047)	(0.047)		
Log(price)	0.507***	0.091**	0.141***	0.119**	206	412
post-label	(0.039)	(0.039)	(0.050)	(0.050)		
Geographic Distance	YES	YES	NO	NO		
Mahalanobis Distance	NO	NO	YES	YES		
Bias-Adjusted	NO	YES	NO	YES		
Mean distance, geo. match:	0.4 mi					
Mean distance, maha. match:	4.2 mi					

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Appendix

A.1 Further details of the dataset creation

In order to obtain code implementation information going back far enough in time to track adoption dates for ASHRAE-1989, data from a variety of sources were utilized, including an online database maintained by the Building Codes Assistance Project (hereafter BCAP) (BCAP (2010)); archives of BCAP's bi-monthly newsletters going back to 1997, obtained by e-mail from BCAP staff; the Department of Energy's online energy codes database (Department of Energy (2010)); and one report from the Department of Housing and Urban Development (HUD (1997)).[46]

Renovated buildings were dropped from the analysis: although certain types of building renovations are subject to an energy code, and CoStar identifies buildings that have been renovated, it is not possible to identify whether the renovation undertaken in a particular building triggered energy code requirements.[47]

Arizona and Colorado are unique states with respect to energy code adoptions. Because these are 'Home-Rule' states, state-level energy standard legislation cannot be legally enforced in individual municipalities and/or counties. However, many jurisdictions in these states have independently adopted energy codes; I have tracked jurisdictional-level adoptions in these states by going through municipal registers (many of which are available online at www.municode.com) and emailing jurisdictional building officials.[48]

Some states have adopted their own codes, though in several of these cases the state-

[46]ASHRAE 1989 standard adoptions were more difficult to pinpoint, as in some cases different sources cited inconsistent dates. More accurate record-keeping for states' adoptions began improving in the mid-1990s. As a result, I only include ASHRAE 1989 adoptions if all the data sources had matching implementation dates. I was not able to find adoption dates for standards prior to ASHRAE 1989. However, as noted in Section 1.1.1, many states only began adopting increasingly stringent energy standards in the mid-1990s as a result of the 1992 EPAct.

[47]For example, the most recently adopted ASHRAE standard is applicable to renovations if more than 50% of the lighting fixtures are replaced, but not if the roof and floor are altered where no new cavities are created, if storm windows are installed, or if existing windows are replaced over an area less than 25% of the total fenestration area.

[48]One issue that may arise with respect to home rule states is the possibility that treatment status may be correlated with changes in unobserved local regulations, which may bias the estimates. To address any concerns from this possibility, I have also conducted estimation without buildings from home-rule states, with no substantive change in the results.

developed code has adopted one of the ASHRAE standards by reference and made only minor modifications to the original standard. In these states I have relied on estimates of the energy use intensity (in kBTU/s.f./yr.) of each code update and matched them to the ASHRAE or IECC standard version with similar energy saving estimates.

A.2 Varying the Distance Between Buildings

Tables A1-A12 present summary statistics matching results where the maximum allowable distance between buildings steadily decreases in 0.25 mile increments, starting with 1.75 miles and ending with 1.0 miles. Summary statistics tables for both the rent and sales samples are also included that indicate the covariate balance is almost identical to the 2.0 mile samples. The results closely resemble those in the main paper, where the maximum distance is 2 miles, though some attenuation in the estimates can be observed as the sample size decreases.

A.3 Varying the Distance Between Buildings: Falsification Test

Tables A13-A16 show results for the falsification test where the maximum allowable distance between buildings steadily decreases in 0.25 mile increments, starting with 1.75 miles and ending with 1.0 miles. Covariate balance in the falsified samples is closely resemble the original sample balance. The log rent results for each maximum distance-based sample closely resemble the results in the main paper. The clustered standard errors, in column (4), fall by almost 50% in the samples with a maximum distance of less than 1.75 miles. This suggests that as the sample size falls there is significant negative intracluster correlation.[49] The bias-adjusted sales results accounting for the impact of energy prices are similar to the main results, though the point estimates vary from being statistically insignificant and negative, to statistically insignificant and positive, depending on the sample.

[49]While this may be indicative of misspecification of the chosen cluster size, clustering at smaller and larger levels of the sample does not alleviate the problem. This suggests the results without clustering may be the most reliable.

A.4 Assessing the Plausibility of SUTVA

My identification strategy assumes that constructing a building under an energy code does not affect potential outcomes in other buildings (also known as the stable unit treatment value, or SUTVA, assumption). One channel through which SUTVA violations could occur is if building managers in control buildings undertake energy-saving behavioral responses as a reaction to the construction of more energy efficient buildings.

While I cannot directly observe such behavioral responses in the control buildings in my sample, in the rental data I observe the company responsible for building-level real estate management services. In recent years, several of the largest (as measured by market capitalization) integrated property management and leasing companies have begun to incorporate energy use management as a core area of expertise.[50] If control building owners in my sample have undertaken behavioral responses to the presence of energy efficient buildings constructed under an energy code, it would be plausible to expect they may hire one of these firms. Table A17 presents the results of applying the bias-adjusted matching estimator where the dependent variable is whether or not a major real estate services firm is responsible for property management. The results are highly insignificant, which is consistent with the identifying assumptions.

A.5 Manipulating Year of Construction

One concern is that since adoption and effective dates for energy codes are publicly known, building developers may try to "game" their building's construction date by rushing to obtain their building permits before the new energy code comes into effect, which would result in a discontinuity in the year built distribution whereby fewer buildings may end up being constructed in the year or two following a code effective date.

Figure A1 depicts the distribution of building construction dates in the full sample of sales and rent observations, two years before and two years after a code came into effect. Close to 25% of buildings were constructed in each of the four years, and there is less than a

[50] See, for example, http://www.cbre.us/services/sustainability/Pages/home.aspx.

one percent difference between the share of buildings constructed just before and just after a code came into effect, which is not suggestive of strategic energy code avoidance behavior.

A.6 Present Value of the Energy Savings

As noted in Section **??**, a 10% reduction in estimated building-level energy savings suggests annual energy cost savings of approximately \$0.34/sq.ft./yr in 2009. This represents about 1.9% of sample average rent and 1.8% of average building-level net operating income in the U.S. These estimates of the annual percent savings accruing to the occupants and owners of a building constructed under a more stringent energy code can be compared to the estimated rent and price premiums (5.7% and 10.3%, respectively) to assess whether the energy savings are fully reflected in observed office market pricing decisions. Since the savings accrue over the length of a tenancy contract or over the expected length of ownership, a simple present value calculation, given plausible assumptions for the growth of utility costs, the discount rate, and expected ownership or rental contract length, suggests that rent and selling price premiums of approximately 5% and 10% correspond to complete capitalization of the estimated energy savings.

Consider the following present value specification:

$$PV = S\left[\left(\frac{1+g}{1+\delta}\right) + \left(\frac{1+g}{1+\delta}\right)^2 + ... + \left(\frac{1+g}{1+\delta}\right)^L\right], \tag{1}$$

where S represents the value of energy cost savings (in this case, approximately \$0.34/sq.ft./yr), g represents the annualized growth of utility costs, δ is the discount rate (assumed constant), and L is the contract length or length of ownership. The specification assumes the savings to a more energy efficient building grow in line with the average rate of increase in the price of energy.

In the rental market, the discount rate for energy savings can be approximated by the capitalization rate for commercial buildings, since the volatility of rental income is highly correlated with the volatility of energy prices.[51] Capitalization rates in the U.S. were rising

[51] The capitalization rate is the ratio between net operating income and the market value of a building.

throughout the 2008-2009 recession compared to previous years, to an average of approximately 7.5% by the end 2009 (Chervachidze and Wheaton (2010)). With a discount rate of 7.5%, and assuming a three year tenancy contract and a 2% expected annual growth in energy prices, the present value of the utility cost savings total approximately 5% of average sample rent.[52]

The market for purchasing commercial property is increasingly composed of real-estate investment trusts and mutual funds, which are known to have high annual portfolio turnover rates (for example, Carhart (1997) finds annual turnover rates of 60-90%). Therefore, while buildings are long-lived assets the ownership length of commercial building assets is likely to be considerably lower, particularly for buildings sold in the time period under consideration, 2002-2009. The estimated sales price premium is consistent with complete capitalization of the energy savings for both a relatively short expected ownership length of 7 years and a discount rate corresponding to 7.5%, or a longer ownership length of 10 years with a discount rate of 12%, corresponding to the average 10-year annualized return of the S&P 500 in 2006, both calculated assuming a 2% increase in energy costs. Of course, a number of other plausible combinations of these variables can also produce savings estimates consistent with complete capitalization.[53]

[52]Tenancy contracts typically span over multiple years. Historical commercial sector electricity retail prices over the past 10 years (as compiled by the Energy Information Administration, www.eia.gov) have risen at a rate of about 2% per year; 5-year forward market wholesale electricity prices at the time the rent listings were observed suggested expected increases over the short-term of about 1-2% per year.

[53]The former calculation implies the present value of the savings represent just over 10.8% of average net operating income; the latter calculation implies the present value of the savings represents approximately 10.5% of average net operating income.

Appendix References

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Figure A1: Buildings constructed pre- and post- code

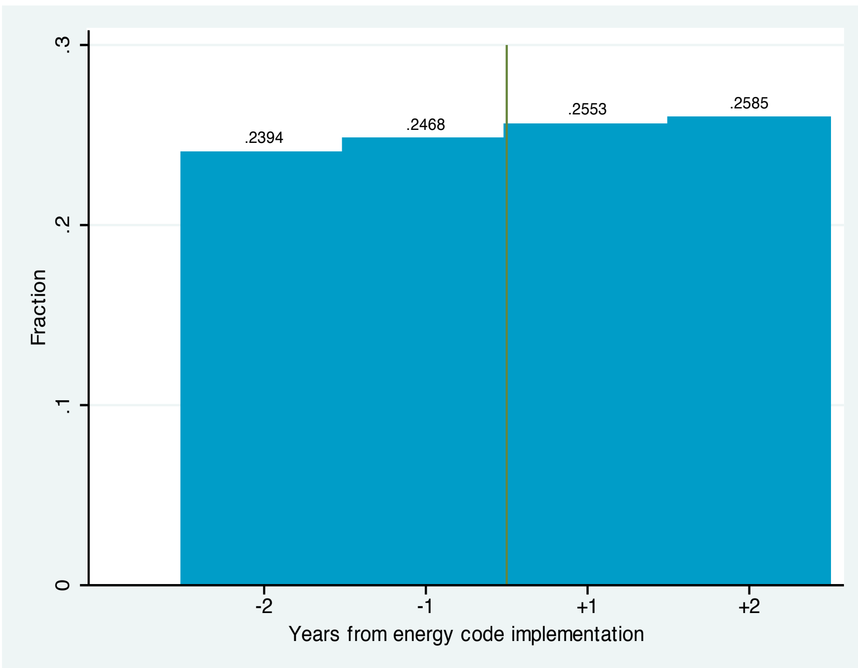

Table A1: Energy Code Summary Statistics, Rentals, 1.75 miles

	CODE				NO CODE				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	2.24	2.23	1	57	2.17	2.99	1	70	0.02
Size (000s)	35.28	38.93	2.00	315.60	36.39	67.49	1.20	1504	-0.05
Built	2005	2.53	1999	2009	2003	2.64	1996	2008	0.55
Class A (%)	19.81	39.87	0	100	18.37	38.74	0	100	0.03
Utilities (%)	51.04	50.01	0	100	48.24	49.99	0	100	0.04
Occupancy (%)	70.88	19.79	30	100	75.71	18.83	30	100	-0.18
Amenities (%)	29.39	45.57	0	100	35.06	47.74	0	100	-0.09
Observations:	830				1,252				
Avg. Distance:	0.53	miles							

Notes: The normalized difference measures the degree of overlap for each covariate across the treated and control samples. It is defined as $\frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_1^2 + S_0^2}}$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table A2: Energy Code Summary Statistics, Sales, 1.75 miles

	CODE				NO CODE				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	1.73	1.30	1	16	1.49	0.89	1	13	0.15
Size (000s)	24.35	40.78	0.89	350	19.27	33.75	1.28	402	0.10
Year Sold	2007	1.59	2002	2009	2006	1.78	2002	2009	0.42
Built	2006	2.17	1999	2009	2004	2.39	1996	2008	0.60
Class A (%)	10.24	30.34	0	100	5.73	23.26	0	100	0.12
Amenities (%)	19.92	39.96	0	100	34.49	47.56	0	100	-0.23
High Vacancy	0.005	0.068	0	1	0.006	0.075	0	1	-0.01
Employment Δ	2.08	2.06	-7.70	6.35	2.45	2.22	-7.70	6.35	-0.12
Elec. Price ($/MWh)	69.90	14.95	29.09	130.6	66.92	17.25	25.97	123.5	0.13
Nat. Gas Price ($/MMBtu)	8.02	1.71	2.52	12.72	7.65	2.10	2.52	12.72	0.14
Observations:	373				1,064				
Avg. Distance:	0.47	miles							

Notes: The normalized difference measures the degree of overlap for each covariate across the treated and control samples. It is defined as $\frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_1^2 + S_0^2}}$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table A3: Matching and Heterogeneity by Utility Contract, 1.75 miles

	(1)	(2)	(3)	(4)	(5)	(6)	Treated	Control
Panel I: Matching								
Log(rent)	0.048***	0.048***	0.040***	0.040***			830	1,252
	(0.014)	(0.012)	(0.014)	(0.012)				
Log(price)	0.040	0.040	0.106***	0.106**	0.123***	0.123***	373	1,064
	(0.028)	(0.031)	(0.028)	(0.031)	(0.025)	(0.031)		
Utilities	0.035	0.035*	0.006	0.006			830	1,252
	(0.021)	(0.020)	(0.021)	(0.020)				
Bias-Adjusted	NO	NO	YES	YES	YES	YES		
Energy Prices	NO	NO	NO	NO	YES	YES		
Robust s.e.	YES	NO	YES	NO	YES	NO		
Clustered s.e.	NO	YES	NO	YES	NO	YES		
Mean distance, rent:	0.5 miles							
Mean distance, sales:	0.5 miles							

	(1)	(2)	(3)	(4)	(5)	(6)
Panel II: Heterogeneity by Utility Contract						
Code	0.048***	0.052***	0.052***	0.026*	-0.020	-0.020
	(0.011)	(0.010)	(0.014)	(0.015)	(0.033)	(0.043)
Utilities∗Code				0.056**	0.060**	0.060*
				(0.024)	(0.025)	(0.032)
Utilities				-0.113***	-0.116***	-0.116***
				(0.021)	(0.022)	(0.026)
R^2	0.70	0.72	0.72	0.73	0.73	0.73
adj. R^2	0.50	0.53	0.53	0.54	0.54	0.54
Fixed effects	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	YES	YES	YES	YES
Year built dummies	NO	NO	NO	NO	YES	YES
Robust s.e.	YES	YES	NO	YES	YES	NO
Clustered s.e.	NO	NO	YES	NO	NO	YES
Observations:	2,082	2,082	2,082	2,082	2,082	2,082

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Table A4: Energy Code Summary Statistics, Rentals, 1.5 miles

	CODE				NO CODE				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	2.25	2.25	1	57	2.17	3.05	1	70	0.02
Size (000s)	35.67	38.76	2.29	315.60	36.64	68.66	1.20	1504	-0.01
Built	2005	2.52	1999	2009	2003	2.63	1996	2008	0.55
Class A (%)	20.15	40.13	0	100	18.40	38.77	0	100	0.03
Utilities (%)	50.87	50.01	0	100	47.96	49.98	0	100	0.04
Occupancy (%)	70.96	19.65	30	100	75.68	18.88	30	100	-0.17
Amenities (%)	28.98	45.38	0	100	35.80	47.96	0	100	-0.10
Observations:	792				1,201				
Avg. Distance:	0.48	miles							

Notes: The normalized difference measures the degree of overlap for each covariate across the treated and control samples. It is defined as $\frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_1^2 + S_0^2}}$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table A5: Energy Code Summary Statistics, Sales, 1.5 miles

	CODE				NO CODE				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	1.73	1.30	1	16	1.48	0.90	1	13	0.16
Size (000s)	24.13	40.57	0.89	350	18.98	32.54	1.28	402	0.10
Year Sold	2007	1.59	2002	2009	2006	1.78	2002	2009	0.42
Built	2006	2.16	1999	2009	2004	2.37	1996	2008	0.61
Class A (%)	10.02	30.04	0	100	5.73	23.24	0	100	0.11
Amenities (%)	19.85	39.90	0	100	34.64	47.60	0	100	-0.24
High Vacancy	0.005	0.069	0	1	0.006	0.075	0	1	-0.01
Employment Δ	2.10	2.06	-7.70	6.35	2.47	2.21	-7.70	6.35	-0.12
Elec. Price (\$/MWh)	69.97	14.89	29.09	130.6	66.98	17.23	25.97	123.5	0.13
Nat. Gas Price (\$/MMBtu)	8.03	1.71	2.52	12.72	7.66	2.10	2.52	12.72	0.14
Observations:	363				1,048				
Avg. Distance:	0.46	miles							

Notes: The normalized difference measures the degree of overlap for each covariate across the treated and control samples. It is defined as $\frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_1^2 + S_0^2}}$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table A6: Matching and Heterogeneity by Utility Contract, 1.5 miles

	(1)	(2)	(3)	(4)	(5)	(6)	Treated	Control
Panel I: Matching								
Log(rent)	0.041***	0.041***	0.030**	0.030**			792	1,201
	(0.014)	(0.012)	(0.014)	(0.012)				
Log(price)	0.031	0.031	0.109**	0.109**	0.130***	0.130***	363	1,048
	(0.028)	(0.033)	(0.028)	(0.033)	(0.028)	(0.033)		
Utilities	0.040*	0.040*	-0.016	-0.016			792	1,201
	(0.022)	(0.021)	(0.022)	(0.021)				
Bias-Adjusted	NO	NO	YES	YES	YES	YES		
Energy Prices	NO	NO	NO	NO	YES	YES		
Robust s.e.	YES	NO	YES	NO	YES	NO		
Clustered s.e.	NO	YES	NO	YES	NO	YES		
Mean distance, rent:	0.5 miles							
Mean distance, sales:	0.5 miles							

	(1)	(2)	(3)	(4)	(5)	(6)
Panel II: Heterogeneity by Utility Contract						
Code	0.041***	0.046***	0.046***	0.027*	-0.003	-0.003
	(0.011)	(0.010)	(0.014)	(0.015)	(0.033)	(0.044)
Utilities$*$Code				0.042*	0.045*	0.045
				(0.024)	(0.024)	(0.031)
Utilities				-0.113***	-0.116***	-0.116***
				(0.021)	(0.022)	(0.026)
R^2	0.70	0.72	0.72	0.73	0.73	0.73
adj. R^2	0.50	0.53	0.53	0.54	0.54	0.54
Fixed effects	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	YES	YES	YES	YES
Year built dummies	NO	NO	NO	NO	YES	YES
Robust s.e.	YES	YES	NO	YES	YES	NO
Clustered s.e.	NO	NO	YES	NO	NO	YES
Observations:	1,993	1,993	1,993	1,993	1,993	1,993

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Table A7: Energy Code Summary Statistics, Rentals, 1.25 miles

	CODE				NO CODE				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	2.26	2.30	1	57	2.17	3.10	1	70	0.02
Size (000s)	36.21	39.80	2.29	315.60	36.89	69.62	1.20	1504	-0.01
Built	2005	2.53	1999	2009	2003	2.64	1996	2008	0.55
Class A (%)	20.44	40.34	0	100	18.60	38.93	0	100	0.03
Utilities (%)	50.92	50.01	0	100	47.86	49.98	0	100	0.04
Occupancy (%)	71.02	19.62	30	100	75.51	18.97	30	100	-0.17
Amenities (%)	28.78	45.75	0	100	35.98	48.02	0	100	-0.11
Observations:	749				1,145				
Avg. Distance:	0.43	miles							

Notes: The normalized difference measures the degree of overlap for each covariate across the treated and control samples. It is defined as $\frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_1^2 + S_0^2}}$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table A8: Energy Code Summary Statistics, Sales, 1.25 miles

	CODE				NO CODE				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	1.74	1.32	1	16	1.49	0.90	1	13	0.16
Size (000s)	24.58	41.09	0.89	350	19.09	32.71	1.28	402	0.10
Year Sold	2007	1.60	2002	2009	2006	1.79	2002	2009	0.42
Built	2006	2.16	1999	2009	2004	2.37	1996	2008	0.60
Class A (%)	10.09	30.13	0	100	5.64	23.08	0	100	0.12
Amenities (%)	20.47	40.37	0	100	35.41	47.85	0	100	-0.24
High Vacancy	0.005	0.070	0	1	0.006	0.077	0	1	-0.01
Employment Δ	2.11	2.08	-7.70	6.35	2.49	2.21	-7.70	6.35	-0.13
Elec. Price ($/MWh)	69.87	14.94	29.09	130.6	66.82	17.39	25.97	123.5	0.13
Nat. Gas Price ($/MMBtu)	8.02	1.72	2.52	12.72	7.64	2.12	2.52	12.72	0.14
Observations:	344				1,011				
Avg. Distance:	0.44	miles							

Notes: The normalized difference measures the degree of overlap for each covariate across the treated and control samples. It is defined as $\frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_1^2 + S_0^2}}$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table A9: Matching and Heterogeneity by Utility Contract, 1.25 miles

	(1)	(2)	(3)	(4)	(5)	(6)	Treated	Control
Panel I: Matching								
Log(rent)	0.038***	0.038***	0.026*	0.026**			749	1,145
	(0.014)	(0.012)	(0.014)	(0.012)				
Log(price)	0.027	0.027	0.041	0.041	0.067**	0.067**	344	1,011
	(0.027)	(0.032)	(0.027)	(0.032)	(0.027)	(0.032)		
Utilities	0.044**	0.044**	0.001	0.001			749	1,145
	(0.022)	(0.019)	(0.022)	(0.019)				
Bias-Adjusted	NO	NO	YES	YES	YES	YES		
Energy Prices	NO	NO	NO	NO	YES	YES		
Robust s.e.	YES	NO	YES	NO	YES	NO		
Clustered s.e.	NO	YES	NO	YES	NO	YES		
Mean distance, rent:	0.5 miles							
Mean distance, sales:	0.5 miles							

	(1)	(2)	(3)	(4)	(5)	(6)
Panel II: Heterogeneity by Utility Contract						
Code	0.039***	0.043***	0.043***	0.024*	-0.019	-0.019
	(0.011)	(0.011)	(0.014)	(0.016)	(0.033)	(0.044)
Utilities∗Code				0.042*	0.043*	0.043
				(0.024)	(0.024)	(0.032)
Utilities				-0.089***	-0.091***	-0.091***
				(0.022)	(0.023)	(0.027)
R^2	0.71	0.73	0.73	0.74	0.74	0.74
adj. R^2	0.53	0.55	0.55	0.56	0.56	0.56
Fixed effects	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	YES	YES	YES	YES
Year built dummies	NO	NO	NO	NO	YES	YES
Robust s.e.	YES	YES	NO	YES	YES	NO
Clustered s.e.	NO	NO	YES	NO	NO	YES
Observations:	1,894	1,894	1,894	1,894	1,894	1,894

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Table A10: Energy Code Summary Statistics, Rentals, 1.0 miles

	CODE				NO CODE				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	2.28	2.37	1	57	2.20	3.22	1	70	0.02
Size (000s)	36.58	39.99	2.29	315.60	37.73	72.08	1.20	1504	-0.01
Built	2005	2.54	1999	2009	2003	2.65	1996	2008	0.55
Class A (%)	21.00	40.75	0	100	19.21	39.41	0	100	0.03
Utilities (%)	50.23	50.02	0	100	47.11	49.94	0	100	0.04
Occupancy (%)	71.08	19.61	30	100	75.48	18.93	30	100	-0.16
Amenities (%)	29.80	45.76	0	100	35.57	47.89	0	100	-0.09
Observations:	687				1,057				
Avg. Distance:	0.37	miles							

Notes: The normalized difference measures the degree of overlap for each covariate across the treated and control samples. It is defined as $\frac{\bar{X}_1-\bar{X}_0}{\sqrt{S_1^2+S_0^2}}$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table A11: Energy Code Summary Statistics, Sales, 1.0 miles

	CODE				NO CODE				NORM. DIFF
	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	
Stories	1.75	1.33	1	16	1.48	0.90	1	13	0.17
Size (000s)	24.67	41.46	0.89	350	18.87	32.72	1.28	402	0.11
Year Sold	2007	1.61	2002	2009	2006	1.79	2002	2009	0.42
Built	2006	2.15	1999	2009	2004	2.35	1996	2008	0.61
Class A (%)	10.13	30.19	0	100	5.27	22.35	0	100	0.13
Amenities (%)	20.47	40.37	0	100	34.95	47.71	0	100	-0.23
High Vacancy	0.005	0.071	0	1	0.004	0.064	0	1	-0.01
Employment Δ	2.11	2.07	-7.70	6.35	2.49	2.21	-7.70	6.35	-0.13
Elec. Price ($/MWh)	69.96	14.96	29.09	130.6	66.94	17.39	25.97	123.5	0.13
Nat. Gas Price ($/MMBtu)	8.02	1.72	2.52	12.72	7.64	2.12	2.52	12.72	0.14
Observations:	329				987				
Avg. Distance:	0.42	miles							

Notes: The normalized difference measures the degree of overlap for each covariate across the treated and control samples. It is defined as $\frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_1^2 + S_0^2}}$, where $\bar{X}_i$ denotes the mean of a given covariate for each treatment status $i = 0, 1$, and S_i^2 denotes the sample variance of X_i. A normalized difference of less than 0.3 is typically considered good overlap.

Table A12: Matching and Heterogeneity by Utility Contract, 1.0 miles

	(1)	(2)	(3)	(4)	(5)	(6)	Treated	Control
Panel I: Matching								
Log(rent)	0.031***	0.031***	0.028**	0.028***			687	1,057
	(0.014)	(0.010)	(0.014)	(0.010)				
Log(price)	0.020	0.020	0.033	0.033	0.061**	0.061*	329	987
	(0.030)	(0.036)	(0.030)	(0.036)	(0.030)	(0.036)		
Utilities	0.043*	0.043**	-0.009	-0.009			687	1,057
	(0.023)	(0.020)	(0.023)	(0.019)				
Bias-Adjusted	NO	NO	YES	YES	YES	YES		
Energy Prices	NO	NO	NO	NO	YES	YES		
Robust s.e.	YES	NO	YES	NO	YES	NO		
Clustered s.e.	NO	YES	NO	YES	NO	YES		
Mean distance, rent:	0.5 miles							
Mean distance, sales:	0.5 miles							

	(1)	(2)	(3)	(4)	(5)	(6)
Panel II: Heterogeneity by Utility Contract						
Code	0.033***	0.036***	0.036***	0.017	-0.057	-0.057
	(0.010)	(0.011)	(0.014)	(0.015)	(0.031)	(0.040)
Utilities∗Code				0.044*	0.046*	0.046
				(0.024)	(0.025)	(0.032)
Utilities				-0.093***	-0.099***	-0.099***
				(0.023)	(0.023)	(0.027)
R^2	0.73	0.73	0.75	0.75	0.76	0.76
adj. R^2	0.55	0.58	0.58	0.59	0.59	0.59
Fixed effects	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	YES	YES	YES	YES
Year built dummies	NO	NO	NO	NO	YES	YES
Robust s.e.	YES	YES	NO	YES	YES	NO
Clustered s.e.	NO	NO	YES	NO	NO	YES
Observations:	1,744	1,744	1,744	1,744	1,744	1,744

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Table A13: Falsification Test, 1.75 miles

	(1)	(2)	(3)	(4)	(5)	(6)	Treated	Control
Panel I: Matching								
Log(rent)	0.020	0.020**	-0.029**	-0.029***			833	1,292
	(0.014)	(0.009)	(0.014)	(0.009)				
Log(price)	-0.164***	-0.164	-0.306***	-0.306***	-0.051	-0.051	401	873
	(0.054)	(0.101)	(0.054)	(0.097)	(0.054)	(0.010)		
Utilities	0.035*	0.037*	0.013	0.013			833	1,292
	(0.021)	(0.020)	(0.021)	(0.020)				
Bias-Adjusted	NO	NO	YES	YES	YES	YES		
Energy Prices	NO	NO	NO	NO	YES	YES		
Robust s.e.	YES	NO	YES	NO	YES	NO		
Clustered s.e.	NO	YES	NO	YES	NO	YES		
Mean distance, rent:	0.5 miles							
Mean distance, sales:	0.5 miles							

	(1)	(2)	(3)	(4)	(5)	(6)
Panel II: Heterogeneity by Utility Contract						
Code	0.023*	0.024**	0.024*	0.011	0.009	0.009
	(0.012)	(0.012)	(0.014)	(0.016)	(0.032)	(0.036)
Utilities∗Code				0.034	0.029	0.029
				(0.026)	(0.026)	(0.029)
Utilities				-0.149***	-0.148***	-0.148***
				(0.023)	(0.023)	(0.028)
R^2	0.64	0.67	0.67	0.68	0.68	0.68
adj. R^2	0.41	0.45	0.45	0.47	0.47	0.47
Fixed effects	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	YES	YES	YES	YES
Year built dummies	NO	NO	NO	NO	YES	YES
Robust s.e.	YES	YES	NO	YES	YES	NO
Clustered s.e.	NO	NO	YES	NO	NO	YES
Observations:	2,125	2,125	2,125	2,125	2,125	2,125

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Table A14: Falsification Test, 1.5 miles

	(1)	(2)	(3)	(4)	(5)	(6)	Treated	Control
Panel I: Matching								
Log(rent)	0.018	0.018***	-0.033**	-0.033***			794	1,244
	(0.015)	(0.007)	(0.015)	(0.007)				
Log(price)	-0.175***	-0.175***	0.001	0.001	0.090*	0.090	401	873
	(0.054)	(0.075)	(0.054)	(0.075)	(0.054)	(0.072)		
Utilities	0.040*	0.040*	0.024	0.024			794	1,244
	(0.022)	(0.020)	(0.022)	(0.020)				
Bias-Adjusted	NO	NO	YES	YES	YES	YES		
Energy Prices	NO	NO	NO	NO	YES	YES		
Robust s.e.	YES	NO	YES	NO	YES	NO		
Clustered s.e.	NO	YES	NO	YES	NO	YES		
Mean distance, rent:	0.5 miles							
Mean distance, sales:	0.5 miles							

	(1)	(2)	(3)	(4)	(5)	(6)
Panel II: Heterogeneity by Utility Contract						
Code	0.021*	0.023*	0.023	0.011	0.018	0.018
	(0.012)	(0.012)	(0.014)	(0.016)	(0.032)	(0.036)
Utilities*Code				0.034	0.029	0.029
				(0.027)	(0.026)	(0.029)
Utilities				-0.147***	-0.146***	-0.146***
				(0.024)	(0.023)	(0.029)
R^2	0.64	0.67	0.67	0.68	0.69	0.69
adj. R^2	0.42	0.46	0.46	0.48	0.48	0.48
Fixed effects	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	YES	YES	YES	YES
Year built dummies	NO	NO	NO	NO	YES	YES
Robust s.e.	YES	YES	NO	YES	YES	NO
Clustered s.e.	NO	NO	YES	NO	NO	YES
Observations:	2,038	2,038	2,038	2,038	2,038	2,038

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Table A15: Falsification Test, 1.25 miles

	(1)	(2)	(3)	(4)	(5)	(6)	Treated	Control
Panel I: Matching								
Log(rent)	0.019	0.019***	-0.029*	-0.029***			731	1,148
	(0.016)	(0.005)	(0.016)	(0.005)				
Log(price)	-0.096*	-0.096	-0.253***	-0.253***	0.026	0.026	335	733
	(0.054)	(0.083)	(0.054)	(0.080)	(0.054)	(0.079)		
Utilities	0.047**	0.047*	0.029	0.029			731	1,148
	(0.023)	(0.022)	(0.023)	(0.022)				
Bias-Adjusted	NO	NO	YES	YES	YES	YES		
Energy Prices	NO	NO	NO	NO	YES	YES		
Robust s.e.	YES	NO	YES	NO	YES	NO		
Clustered s.e.	NO	YES	NO	YES	NO	YES		
Mean distance, rent:	0.4 miles							
Mean distance, sales:	0.5 miles							

	(1)	(2)	(3)	(4)	(5)	(6)
Panel II: Heterogeneity by Utility Contract						
Code	0.022*	0.022*	0.022	0.012	0.026	0.026
	(0.013)	(0.013)	(0.014)	(0.016)	(0.034)	(0.037)
Utilities*Code				0.033	0.028	0.028
				(0.028)	(0.028)	(0.031)
Utilities				-0.145***	-0.143***	-0.143***
				(0.025)	(0.025)	(0.030)
R^2	0.64	0.67	0.67	0.68	0.68	0.68
adj. R^2	0.41	0.45	0.45	0.47	0.47	0.47
Fixed effects	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	YES	YES	YES	YES
Year built dummies	NO	NO	NO	NO	YES	YES
Robust s.e.	YES	YES	NO	YES	YES	NO
Clustered s.e.	NO	NO	YES	NO	NO	YES
Observations:	1,879	1,879	1,879	1,879	1,879	1,879

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Table A16: Falsification Test, 1.0 miles

	(1)	(2)	(3)	(4)	(5)	(6)	Treated	Control
Panel I: Matching								
Log(rent)	0.011	0.011	-0.050***	-0.050***			667	1,053
	(0.016)	(0.008)	(0.016)	(0.008)				
Log(price)	-0.090	-0.090	-0.191***	-0.191***	0.089	0.089	298	656
	(0.058)	(0.080)	(0.058)	(0.080)	(0.058)	(0.086)		
Utilities	0.037	0.037	-0.015	-0.015			667	1,053
	(0.024)	(0.023)	(0.024)	(0.023)				
Bias-Adjusted	NO	NO	YES	YES	YES	YES		
Energy Prices	NO	NO	NO	NO	YES	YES		
Robust s.e.	YES	NO	YES	NO	YES	NO		
Clustered s.e.	NO	YES	NO	YES	NO	YES		
Mean distance, rent:	0.3 miles							
Mean distance, sales:	0.5 miles							

	(1)	(2)	(3)	(4)	(5)	(6)
Panel II: Heterogeneity by Utility Contract						
Code	0.014*	0.016*	0.016	0.004	0.004	0.004
	(0.013)	(0.013)	(0.015)	(0.017)	(0.039)	(0.042)
Utilities∗Code				0.033	0.029	0.029
				(0.029)	(0.029)	(0.032)
Utilities				-0.150***	-0.148***	-0.143***
				(0.026)	(0.026)	(0.031)
R^2	0.64	0.66	0.66	0.68	0.68	0.68
adj. R^2	0.42	0.45	0.45	0.47	0.47	0.47
Fixed effects	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	YES	YES	YES	YES
Year built dummies	NO	NO	NO	NO	YES	YES
Robust s.e.	YES	YES	NO	YES	YES	NO
Clustered s.e.	NO	NO	YES	NO	NO	YES
Observations:	1,720	1,720	1,720	1,720	1,720	1,720

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.

Table A17: An Indirect SUTVA Test

	(1)	(2)	Treated	Control
Management Company	0.004	0.004	850	1,269
	(0.019)	(0.022)		
Bias-Adjusted	YES	YES		
Clustered s.e.	NO	YES		
Robust s.e.	YES	NO		

Notes: Standard errors are in parentheses. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Clustering is at the market level.