Housing Market Spillovers: Evidence from the End of Rent Control in Cambridge Massachusetts*

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Abstract

Spillovers in the housing market, such as external effects related to the characteristics of nearby housing or the attributes of nearby neighbors, are a key building block of urban economics. This paper uses the sudden end of rent control in Cambridge Massachusetts in 1995, which affected landlord's investment incentives and the assignment of households to locations, to measure housing market spillovers. Using data on the prices and characteristics of all residential transactions together with exact information on the location of each controlled unit, we find evidence for large price effects on uncontrolled properties from the elimination of rent control. The estimates are robust to specifications including neighborhood trends, different measures of exposure to rent controlled properties, and various geographic controls. Building permit and expenditure data indicate some evidence of investment spillovers for houses in rent control intensive neighborhoods. The elimination of rent control also had a sizable direct effect on the prices of controlled properties. Finally, we use our estimates to measure the change in the overall value of uncontrolled Cambridge housing stock from decontrol due to spillovers.

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1 Introduction

Housing markets are a core ingredient in the evolution of metropolitan communities and the growth of cities. A central building block in the economic analysis of housing markets is the existence of various kinds of externalities across housing and residents. Much of urban economic theory studies the functioning of housing markets in the presence of such externalities (see, e.g., Fujita (1991)), and views on the magnitudes of spillovers play a starring role in discussions of housing market policy, serving as an economic rationale for government intervention (see, e.g., Glaeser and Gyourko (2009)).

While there is considerable theory and policy based on the existence of externalities in housing markets, there has been relatively little empirical evidence on the size and nature of spillovers between nearby residential locations. A large literature has focused on human capital spillovers between workers (see Glaeser (2008) for a recent summary) and agglomeration externalities for firms and industries, such as studies on industrial clustering (Ellison, Glaeser and Kerr (2009)) or on the productivity effects of large manufacturing plant openings (Greenstone, Hornbeck and Moretti (2010)).

In this paper, to measure housing market spillovers, we study the end of rent regulations in Cambridge Massachusetts. Until 1995, residential housing in Cambridge was regulated by a far-reaching rent control plan, with 37.5% of residential units subject to regulation and tight restrictions on decontrol. Rent control was eliminated in a state-wide referendum by a 51% to a 49% vote. Our main focus is whether and how much the sudden elimination of rent control affected the price of nearby uncontrolled housing. Rent control might limit incentives to invest in building quality, so its elimination may lead to an increase in neighborhood investment. Rent control might also affect the allocation of individuals to housing, so its elimination may lead to new neighbors in formerly controlled properties. A central empirical challenge in the economic analysis of housing markets is that many aspects of market equilibrium– who lives where, the quality and quantity of housing, the quality of local public goods, the types of amentities – are determined simultaneously. The variation generated by the sudden policy change in Cambridge provides us with a unique opportunity to identify residential housing market spillovers.

Our data consists of the records of the Cambridge Rent Control Board on the exact location of each controlled unit and data on the prices of transacted houses and condominiums throughout

Cambridge. Our data allow us to directly assess the net price effect of the changes in amentities due to more investment or the re-allocation of neighbors at very small geographies. We also document the direct effect that rent control had on prices and investment in controlled condominiums and houses.

For the typical transacted property in Cambridge, 36% of surrounding housing units within a 0.20 mile radius are rent controlled. Our results imply that when rent control ends, the average uncontrolled housing unit transacts at a 8% higher real price. Across specifications, this estimate varies from between 5% and 14%, and is robust to various measures of exposure to controlled properties, geographic controls, and trends. When the sample is split into houses and condominiums, the results are larger and more precise for uncontrolled houses than uncontrolled condominiums. Our estimate of spillovers for uncontrolled condominiums by themselves is generally positive, but much less precise. The estimates for controlled condominiums, however, are larger than for controlled houses and the evidence suggests that the price effect for this subgroup operates at geographies smaller than 0.20 miles. In addition, we find that adjusted for observed property characteristics, rent controlled properties sold at a discount of about 13-35% relative to uncontrolled properties during the rent control era, but sale prices of these properties increased by approximately 6-11% once rent control was lifted.

We complement this analysis by investigating a dataset of all residential building permits required for significant investments or modifications to housing by the Cambridge Inspectional Services from 1991 through 1998. After the end of rent control, overall permitting activity increased by about 15% and mean expenditure on permits increases by more than double for houses and by more than 5 times for condominiums, with larger increases for controlled locations than uncontrolled ones. However, the results for investment are not as striking as for prices. There is some evidence that a location is more likely to be permitted when rent control is eliminated. Moreover, formerly controlled properties exposed to many other controlled properties invest more– on average about $2,500 more for the typical property, though this estimate displays sensitivities to controls and is driven by large expenditure values. The overall weaker picture for investment suggests that price effects of the end of rent control on uncontrolled properties either require large returns to investment or a component due to mechanisms other than investment spillovers.

We conduct two back-of-the-envelope calculations to understand the economic magnitudes of our spillover estimates. We focus on uncontrolled properties because the change in price for controlled properties involves a transfer from renters (who obtained below-market rents) to landlords, and thus has a more ambiguous welfare interpretation than the change in value of uncontrolled properties. First, using our estimated model for sales prices, where we allow the influence of rent control intensity to vary by year, we compute what the average value of the set of uncontrolled transacted properties would have been without decontrol and assuming the properties transacted each year. In 1997, the difference in the average price of uncontrolled houses if rent control continued is more than $23,000. By 2001, this difference grows to $136,000 and by 2003, the difference is $125,000. Reflecting our smaller estimates for uncontrolled condominiums, the difference is only $3,000 in 1999 and $35,000 by 2003. This calculation has the drawback that it only applies to the sample of the housing stock which transacts. To understand what our estimates imply for the overall value of the Cambridge housing stock, we use property assessment figures from the 2003 Cambridge Assessors database, which assign an assessed value to all housing and condominiums in Cambridge. We estimate the aggregate impact of decontrol on the stock of uncontrolled housing to be $1.0 billion on a total uncontrolled property base of $7.5 billion. As a reference, the total value of the uncontrolled housing stock in the 1995 Assessors database is $2.8 billion, so the assessed value of the uncontrolled housing stock grew by $4.7 billion from 1995 to 2003, of which 22% of the appreciation is due to housing market spillovers, with the uncontrolled housing stock realizing 16% of this gain.

The layout of this paper is as follows. Section 2 provides some background on rent control in Cambridge and describes some related literature. Section 3 develops a simple model of housing market externalities to guide our empirical analysis. Section 4 describes data sources and empirical strategy. Section 5 presents our main results for transaction prices, while Section 6 discusses results for investment. Section 7 describes and interprets economic magnitudes. We conclude with a discussion of some areas for further investigation.

2 Background

The City of Cambridge, which is north of the City of Boston and separated from it by the Charles River, has a population of just over 100,000, with 70% age 20 or older, and 9% age 65 or older according to the 2000 US Census. Whites account for 68% of the population, and Blacks and Asian comprise the two next largest racial groups at 12% of the population each. The city is home to two major research universities which serve as the top two employers, alongside a relatively high concentration of bio-technology companies and hospitals. The median family income in 2000 was $47,900 and 13% of residents were below the poverty line.

In 1970, the Massachusetts state legislature allowed cities and towns with populations over 50,000 to implement rent control to deal with housing shortages.[1] Boston, Brookline, Cambridge, Lynn and Somerville each adopted a rent control plan. Cambridge maintained rent regulations beginning in 1971 and kept them longer than any other city. Lynn repealed its plan in 1974, Somerville in 1979, Boston approved vacancy decontrol in 1974, and Brookline decontrolled most units in 1991. Rent control was seen as an integral part of Cambridge's affordable housing program.

The initial rent control policy in Cambridge applied to all non-owner-occupied rental housing built before 1970. It did not apply to owner-occupied condominiums or structures built after the 1970s or to non-residential structures converted to rental properties after the passage of rent control. The rents in 1971 were set equal to the prevailing rents in 1967, and there were essentially no price increases for the next decade and a half. Starting in the early 1980s, there were some across-the-board price increases (typically about 5%), but landlords who wanted permission to raise prices aside from the schedular increase had to file a petition with the Rent Control Board. Such petitions were rarely granted.

In general, it was very difficult to take units out of circulation. Unlike other rent control regimes, Cambridge did not permit vacancy decontrol. This means that a property that was rent controlled remain controlled even when the protected tenant moved out. The initial reaction to rent control was widespread condominium conversions in older buildings which were then sold to owner-occupants. However, in 1979 the City Council passed the "Removal Permit Ordinance," which limited the removal of controlled units. This ordinance required proof that removal would not aggravate the housing shortage and would "benefit the persons sought to be protected" by the rent control statute (Cantor (1995)).

Throughout this period, the issue of rent control was seen as a central for any candidates to the city council and a defining one in the political debate in Cambridge. Often the battle lines were drawn between owner occupants organized by the Small Property Owners Association (SPOA), which wanted to eliminate rent control, and affordable housing advocates and rent control tenants.

[1] Rent control often takes the form of a rent stabilization plan. For the purposes of this discussion, we make no distinction between rent control and stabilization.

The only attempt to repeal rent control was in 1989 when a local initiative was defeated 66% to 34% (Fain (1995)). Finally, the SPOA brought rent control to a state-wide ballot, spending over 1 million (1994) dollars and collecting more than 100,000 signatures. The outcome of the state referendum was highly uncertain, with residents state-wide voting 51% to 49% to end rent regulation. Residents from Boston, Brookline, and Cambridge overwhelmingly voted to keep rent control in place, with the fraction of votes favoring rent control in Cambridge exceeding 60%.

Following passage of the referendum, the state legislature adopted a bill extending rent control for five years, but Governer William Weld vetoed it. Ultimately, Weld signed a bill that allowed for a two-year phase out. According to a Cambridge lawmaker, rent control was intended to "die with dignity" (Oser (1996)). The majority of properties were decontrolled in January 1995. The phase out plan required tenants without a demonstrated financial concern and full-time students to be decontrolled on January 1, 1995. If the income of all residents of a unit was under 60% of the median for the Boston MSA, or under 80% for the elderly (62+) or disabled, decontrol was deferred until the following January 1, 1996 for condominiums and small buildings (housing three families or less or owner-occupied buildings with up to 12 apartments), and to January 1, 1997 for everything else.

Thus, the elimination of rent control by referendum in Cambridge at the end of 1994 was unlikely to have been wholly anticipated by either landlords or tenants. Even after passage of the referendum, rent control remained in a state of limbo for several months while legislative compromises were struck. Following that, a two year phase-out period meant that rent control was not wholly eliminated until 1997.

Related studies and questions

Regulatory intervention in housing markets is widespread and rent control is among the most important interventions historically (Glaeser and Gyourko (2009)). The modern era of U.S. rent controls began as a part of World War II-era price controls and as a reaction to housing shortages following demographic changes immediately after the war. Even though the prevalence of rent control as a housing market policy has decreased considerably from this period, rent control or stabilization plans are still in place in many U.S. and European cities (e.g., Alston, Kearl and Vaughan (1992), Arnott (1995)). New York City's system of rent regulation affects at least one million apartments, while cities such as San Francisco, Los Angeles, Washington DC, and many towns in California and New Jersey have various forms of rent regulation. Rent control continues to be an active topic of debate among affordable housing advocates.

One of the first economic analyses of rent control is by Friedman and Stigler (1946), who articulate a series of arguments about World War II-era controls:

> *Rent ceilings, therefore, cause haphazard and arbitrary allocation of space, inefficient use of space, retardation of new construction and indefinite continuance of rent ceilings, or subsidization of new construction and a future depression in residential building. Formal rationing by public authority would probably make matters still worse.*

The early empirical literature on rent control focused on some of the issues highlighted in the above quote, including the effects on the supply of rental properties (Olsen (1972)) and the incentives of landlords to invest in building quality (Frankena (1975), Gyourko and Linneman (1989), Sims (2007)). A more recent literature continues this line of investigation by examining how below-market rents may encourage individuals to spend effort on obtaining cheap housing and how this may lead to a mis-allocation of housing (Glaeser and Luttmer (2003), Sims (2009)).

More generally, outside of housing markets, price controls are widespread. There is a large literature examining the effects of price controls in the labor market (e.g., Holzer, Katz and Krueger (1991), Card and Krueger (1995), Lee and Saez (2010)), gasoline markets (e.g., Frech and Lee (1987), Davis and Killian (2008)), and other regulated markets. Relative to this literature, our main interest is on a relatively under-explored consequence of price controls: the external effects on the uncontrolled sector.

The questions examined in this paper are also related to work studying neighborhood revitalization, which may involve the indirect benefits of the improvement of blighted areas (see, e.g., Schwartz, Ellen, Voicu and Schill (2005), Rossi-Hansberg, Sarte and Owens (2009)) and gentrification (see, e.g., Hurst, Guerrieri and Hartley (2009)). Linden and Rockoff (2008)'s study of the housing market impacts of the arrival of registered sex offenders into a neighborhood is a related study on allocative externalities in residential housing. Finally, the measurement of external effects is also of recent interest given the current wave of home foreclosures and the concern for their impact on immediate neighbors and neighborhoods (see, e.g., Campbell, Giglio and Pathak (2009)).

3 The Price Effects of Rent Control

3.1 Model

This section develops a stylized equilibrium model of the housing market in the presence of rent control to guide our empirical approach. The model is in the tradition of the spatial equilibrium models of Alonso-Mills-Muth.[2]

Geographies. A city consists of $g = 1, ..., G$ geographies, or neighborhoods. Within each neighborhood is a continuum of locations indexed by $j \in [0, 1]$, so that the pair (j, g) refers to a particular location in a neighborhood.

Landlords. Each location within a neighborhood is owned by a landlord who decides how much maintenance $m \in \mathbb{R}^+$ to undertake. Housing services are produced according to the following technology,

$$H = f(m),$$

where f is increasing. Even though the model is static, our interpretation is that housing services are a flow (per unit of time). The price of housing services, p, is a per-period price, which we interpret as a rental price.

The cost of maintenance is an increasing convex function $c(m)$. The problem of the landlord is to choose a level of maintenance m to maximize profits:

$$\max_m pH - c(m).$$

Substituting the production function, we have the first-order condition for an interior solution:

$$pf'(m) = c'(m).$$

This condition implies that the amount of housing maintenance the landlord's housing maintenance is an increasing function of the price of housing services. Let us write this function as follows:

$$m^* = m(p), \quad m'(p) > 0.$$

[2]See Glaeser (2008) for a survey of these models.

Households. A household of type α has preferences given by:

$$U(C, H) = C + \phi(\alpha, H) + A_g,$$

where C is consumption, ϕ is a concave function of housing H and α, a taste parameter, and A_g is the total amount of "amenities" in neighborhood g. Aside from their relative taste for housing, individuals are identical and have endowment of wealth W. The price of housing at location j in neighborhood g is denoted $p_g(j)$, so a household who lives at (j, g) faces the budget constraint

$$C + p_g(j)H_g(j) = W. \tag{1}$$

Utility maximization yields downward sloping demand for housing services at location j in neighborhood g:

$$H_g(j) = D(p_g(j)).$$

The amenities in a neighborhood may in general depend on various features of the neighborhood. To capture the most relevant dimensions for our study, we assume that amenities depend on the level of maintenance and the types of residents in the neighborhood:

$$A_g = \int_0^1 \mathcal{A}_g(m_g(j), \alpha_g(j))dj,$$

where $m_g(j)$ is the level of maintenance at location j in neighborhood g and $\alpha_g(j)$ is the α type in neighborhood g at location j, and $\mathcal{A}_g(\cdot, \cdot)$ is an increasing function in both arguments.

Equilibrium. The solution concept is based on spatial equilibrium. We assume that there is free-entry and perfect mobility of households. The outside option for a household of type α is simply $\bar{U}_\alpha$.

Definition 1 *An equilibrium is a triple $(\alpha_g(j), p_g(j), H_g(j))$ for each neighborhood g and location j where $\alpha_g(j)$ is the household's type, $p_g(j)$ is the price of housing services, and $H_g(j)$ is the level of housing services such that*

i) each household obtains at least their outside option,

ii) no household wishes to move to another neighborhood or location, and *iii) landlords maximize profits.*

Benchmark model. We impose particular functional forms to generate an econometric model. For the supply side, assume that housing is produced by a CRS technology, $H = f(m) = m$, and there are quadratic costs of maintenance, $c(m) = \frac{1}{2}m^2$. These assumptions imply that the optimal level of maintenance at each location is exactly equal to the price of housing:

$$m^* = p.$$

For the demand side, assume that $\phi(\alpha, H) = \alpha \ln(H)$. For an interior solution (W large enough), housing demand independent of wealth for a household of type α

$$H = \alpha/p. \tag{2}$$

Let the amentity function be additively separable and the same for each neighborhood,

$$\mathcal{A}_g(m_g(j), \alpha_g(j)) = \log(m_g(j)) + \beta\alpha_g(j),$$

where $\beta > 0$ is the weight placed on the neighbor type in determining amenities. Furthermore, for simplicity, suppose there are G household types α, where $\alpha_1 > \ldots > \alpha_G > 1$, and we denote consumer α_g's outside option as $\bar{U}_g$.

3.2 Equilibrium in the Uncontrolled Economy

We consider an equilibrium where all α_g types live in neighborhood g, $\alpha_g(j) = \alpha_g$. Free-entry and mobility of households implies that all locations j and neighborhood g the utility is equal to $\bar{U}_g$.

Plugging this into utility for location j together with the budget constraint (1) and expression for demand (2) and we obtain:

$$\bar{U}_g = \underbrace{W - \alpha_g}_{=C} + \alpha_g \ln \alpha_g - \alpha_g \ln(p_g(j)) + A_g.$$

Hence, the log price of housing is given by:

$$\log(p_g(j)) = \log(\alpha_g) + \frac{1}{\alpha_g}\left(W - \bar{U}_g - \alpha_g + A_g\right). \tag{3}$$

Given our parameterization for amentities, we obtain

$$A_g = \int_0^1 \log(m_g(j))dj + \beta \int_0^1 \alpha_g(j)dj = \int_0^1 \log(m_g(j))dj + \beta\alpha_g.$$

Since the maintenance levels are equal to the price of housing, we have:

$$\log(p_g(j)) = \log(\alpha_g) + \frac{1}{\alpha_g}\left(W - \bar{U}_g - \alpha_g + \int_0^1 \log(m_g(j))dj + \beta\alpha_g\right) = \log(m_g(j)).$$

Symmetry among landlords implies that $m_g(j) = m_g$, or

$$\log(\alpha_g) + \frac{1}{\alpha_g}(W - \bar{U}_g - (1-\beta)\alpha_g + \log(m_g)) = \log(m_g).$$

This relationship captures the feedback between overall maintenance in the neighborhood and location specific maintenance choices. It is where the assumption that $\alpha_g > 1$ is important. Rearranging,

$$\log(m_g) = \frac{\alpha_g}{\alpha_g - 1}\left(\log(\alpha_g) + \frac{1}{\alpha_g}(W - \bar{U}_g - (1-\beta)\alpha_g)\right) \equiv \log(m_g^u).$$

Since maintenance levels are the same for all locations within neighborhood g, the price is the same for properties at all locations j within g. Using the expression for the level of maintenance, the log price of housing at neighborhood g in the uncontrolled economy is:

$$\log(p_g) = \frac{\alpha_g}{\alpha_g - 1}\left(\log(\alpha_g) + \frac{1}{\alpha_g}(W - \bar{U}_g - (1-\beta)\alpha_g)\right) \equiv \log(p_g^u).$$

Because of spatial arbitrage, each household in neighborhood g obtains utility $\bar{U}_g$. The spatial equilibrium concept implies that there is no consumer surplus as each household is indifferent between living in the neighborhood and the outside option. The constant returns to scale production technology implies that landlords obtain zero profits.

3.3 Equilibrium in the Controlled Economy

Within neighborhood g, suppose that a fraction λ_g of structures are rent-controlled and $1-\lambda_g$ are uncontrolled. We first examine the pricing and maintenance decisions for controlled properties.

Rent controlled properties. The price unit per housing service is set by the Rent Control Board. For each controlled location j, let $\bar{p}_g(j)$ be the rent controlled price and we assume that for each controlled property, the controlled price is less than the corresponding price in the uncontrolled economy, $\bar{p}_g(j) < p^u_g$.

This price will determine the level of maintenance according to the producer's first-order condition, which yields

$$\bar{m}_g(j) = \bar{p}_g(j),$$

and in turn the amount of housing services at location j is given by:

$$H_g(j) = f(\bar{m}_g(j)) = \bar{p}_g(j).$$

Uncontrolled properties. Spatial arbitrage determines the prices of the uncontrolled sector. Hence, the same arbitrage relation determines prices as in equation (3):

$$\log(p_g(j)) = \log(\alpha_g) + \frac{1}{\alpha_g}\left(W - \bar{U}_g - \alpha_g + A_g\right).$$

Since $\bar{m}_g(j) = \bar{p}_g(j)$, the average level of the amenity is given by:

$$A_g = \int_0^1 1\{j = RC\}\log(\bar{p}_g(j))dj + \int_0^1 1\{j \neq RC\}\log(m_g(j))dj + \beta\int_0^1 \alpha_g(j)dj.$$

Since the landlords of uncontrolled properties are symmetric, we have that $m_g(j) = m_g$. This expression allows us to simplify the expression for A_g further:

$$A_g = \int_0^1 1\{j = RC\}\log(\bar{p}_g(j))dj + (1-\lambda_g)\log(m_g) + \beta\int_0^1 \alpha_g(j)dj.$$

We focus on equilibrium where $\alpha_g(j) = \alpha_g$ for all uncontrolled locations j, but we leave the allocation of household types to controlled properties unmodelled. This yields:

$$A_g = \int_0^1 1\{j = RC\} \left[\log(\bar{p}_g(j)) + \beta\alpha_g(j)\right] dj + (1 - \lambda_g)(\log(m_g) + \beta\alpha_g).$$

In general, the price of all controlled properties in neighborhood g differs at each controlled location, so we cannot further simplify the first term. Furthermore, the second term depends on the way that households are assigned to controlled housing. Let

$$\lambda_g K_1 \equiv \int_0^1 1\{j = RC\} \log(\bar{p}_g(j))dj \quad \text{and} \quad \lambda_g K_2 \equiv \int_0^1 1\{j = RC\}\alpha_g(j)dj.$$

Since $\bar{p}_g(j) < p_g^u$, it is clear that

$$K_1 < \log(p_g^u) = \log(m_g^u). \tag{4}$$

However, if higher α types manage to obtain controlled housing it is possible that K_2 is either larger or smaller than the corresponding term for the uncontrolled economy α_g. For example, if only α_1 types obtain controlled housing, amenities in the controlled neighborhood will improve from the presence of good neighbors. On the other hand, if only α_G types obtain controlled housing, amenities are lower in the neighborhood when it is controlled.

Our additive structure on amenities yields

$$A_g = \lambda_g(K_1 + \beta K_2) + (1 - \lambda_g)(\log(m_g) + \beta\alpha_g).$$

To compute the level of maintenance in uncontrolled properties, we follow the steps above to find:

$$\log(m_g^c) \equiv \frac{\alpha_g}{\alpha_g + \lambda_g - 1}\left[\log(\alpha_g) + \frac{1}{\alpha_g}(W - \bar{U}_g - \alpha_g + \lambda_g(K_1 + \beta K_2) + (1 - \lambda_g)\beta\alpha_g)\right].$$

We can write this in terms of the level of maintenance per location in the uncontrolled economy:

$$\log(m_g^c) = \frac{\alpha_g - 1}{\alpha_g + \lambda_g - 1}\log(m_g^u) + \frac{\lambda_g}{\alpha_g + \lambda_g - 1}(K_1 + \beta(K_2 - \alpha_g)).$$

When there is no rent control ($\lambda_g = 0$), the two levels of maintenance are the same.

Is it always the case that there is more maintenance in neighborhood g in the uncontrolled economy, $m_g^u > m_g^c$? The required condition is

$$\underbrace{\log(m_g^u) - K_1}_{\text{maintenance effect}} > \underbrace{\beta(K_2 - \alpha_g)}_{-\text{allocative effect}} . \tag{5}$$

Since $K_1 < \log(m_g^u)$ by relation (4), whether uncontrolled prices in neighborhood g are lower in the controlled economy depends on the relative importance allocative channel for determining amentities. If neighbors are not important for determining amentities (β is small), then maintenance (and hence prices) will be higher in the uncontrolled economy. However, if the allocative distortion is such that K_2 is much larger than α_g (e.g., so many good types live in controlled properties in the neighborhood) then it is possible that amenities are greater in the controlled economy, so that uncontrolled prices are greater in the controlled economy.

The change in price for an uncontrolled property at location j is:

$$\begin{aligned}\Delta \log(p_g(j)) = \log(p_g^u(j)) - \log(p_g^c(j)) = \frac{1}{\alpha_g}\Delta A_g = \underbrace{\frac{1}{\alpha_g}[A_g^u - A_g^c]}_{\text{amentity effect}} \\ = \frac{\lambda_g}{\alpha_g + \lambda_g - 1} \underbrace{[\log(m_g^u) - K_1]}_{\text{maintenance effect>0}} + \frac{\beta\lambda_g}{\alpha_g + \lambda_g - 1} \underbrace{[\alpha_g - K_2]}_{\text{allocative effect}} .\end{aligned} \tag{6}$$

When $\beta = 0$, the price change for uncontrolled properties only captures the maintenance effect. If relation (5) holds, when λ_g increases, the change in prices for controlled properties increases. Larger values of K_1, for instance when the prices of controlled properties are lower than their market value, generate larger changes in uncontrolled prices from the elimination of rent control. When β is large, and $\alpha_g - K_2$ is positive (so on average there are more lower α-types relative to the average α type who would be in the neighborhood in the uncontrolled economy), then the price of uncontrolled properties increases due to the re-allocation.

The change in price for a controlled property at location j is:

$$\log(p_g^u(j)) - \log(\bar{p}_g^c(j)) = \left(\underbrace{\log(p_g^u(j)) - \log(p_g^c(j))}_{\text{amenity effect}}\right) + \left(\underbrace{\log(p_g^c(j)) - \log(\bar{p}_g^c(j))}_{\text{decontrol effect}}\right), \tag{7}$$

where the first term, the *amenity effect*, is the price change for uncontrolled properties going from a controlled to an uncontrolled economy which is in turn due to maintenance and allocative effects as in equation (6), and the second term, the *decontrol effect*, is the price change in a controlled economy going from a rent controlled property to an uncontrolled property. For a formerly controlled property, the direct effect of decontrol is larger when the controlled price of the property is lower.

We can summarize the relevant considerations from this model in the following proposition:

Proposition 2 *Suppose condition (5) holds. When rent control ends, the price change for uncontrolled properties is greater for geographies*

i) with more rent control intensity ($\lambda \uparrow$),

ii) where the price of controlled properties is further depressed from their market price ($K_1 \downarrow$),

iii) where there is greater mis-allocation of household types relative to the types in the uncontrolled economy ($\alpha_g - K_2 \uparrow$).

Furthermore, when rent control ends, controlled properties experience an additional price change due to the direct effect of decontrol.

This model guides our analysis of the data, but it is important to point out some of its main limitations for our setting. First, the price of housing services is an abstraction which allows for no distinction between house prices and rents. In particular the model is static, so it does not allow for realistic dynamics to capture expectations of neighborhood appreciation and the option value of ownership. Second, housing services within a neighborhood are homogenous, so households have no desire to substitute between locations within a geography. If housing services are differentiated products, there may be a role for substitution between different locations within a geography. If, for instance, there is new construction due to the end of rent control, there may be a price response on nearby uncontrolled housing due to increased housing supply. Another limitation of the model is within a neighborhood, all occupants of uncontrolled housing obtain the same utility by the spatial arbitrage assumption. If the outside utility for occupants of a neighborhood is the same in the controlled and uncontrolled economy, then even though amentities may improve in the uncontrolled economy and prices increase, these households do not obtain any additional utility. The model also only allows for general equilibrium effects of the end of rent control through changes in household's outside options.

4 Empirical Strategy and Data

4.1 Econometric Implementation and Identification

The comparison of prices for uncontrolled properties between the controlled and uncontrolled economy is our main interest. Ideally, we observe the same uncontrolled property transact in both the controlled and uncontrolled economy. Holding fixed the attributes of the house, any difference in price can be attributed to changes in amenities due to the end of rent control. Focusing on properties that transact twice or more, however, has two limitations. First, 35% of transactions in our dataset transact only once so focusing exclusively on them would throw out a large number of observations. Moreover, the set of transactions that transact multiple times are different from properties that transact only once. In particular, properties that transact more than once are 16% more likely to be condominiums, and as a result, these properties have fewer total rooms and bedrooms on average.

Instead, within a geography, we compare different uncontrolled properties in the two economies. The econometric model for the log sales price of uncontrolled property i in neighborhood g in year t is:

$$\log(p^u_{igt}) = \gamma_g + \delta_t + \beta' X_i + \rho_1 \text{RCI}_i + \rho_2 \text{RCI}_i \cdot \text{POST}_t + \epsilon_{igt}, \tag{8}$$

where γ_g are controls for geography, δ_t are controls for year, and X_i are house characteristics, included because we do not observe the same house transact. POST_t is an indicator equal to 1 if the date of the transaction is 1995 or after, RCI_i is a measure of property i's rent control exposure and ϵ_{igt} corresponds to random fluctuations in house prices. The coefficient of interest is ρ_2, which measures the price change due to the amenity effect and is the sum of maintenance and allocative effects. It is identified from the pre-post variation in amenities within the neighborhood due to the elimination of rent control.

When we consider both controlled and uncontrolled properties, we have the following hedonic model of log sales prices:

$$\log(p_{igt}) = \gamma_g + \delta_t + \beta' X_i + \lambda_1 \text{RC}_i + \lambda_2 \text{RC}_i \cdot \text{POST}_t + \rho_1 \text{RCI}_i + \rho_2 \text{RCI}_i \cdot \text{POST}_t + \epsilon_{igt}. \tag{9}$$

This equation includes additional terms to capture the de-control effect, which is measured by λ_2.

This specification imposes that the effect of rent control exposure is the same for both controlled and uncontrolled properties. The following regression equation allows the effects to differ by control status and is our workhorse specification:

$$\begin{aligned}\log(p_{igt}) = \gamma_g + \delta_t + \beta' X_i + \lambda_1 \mathrm{RC}_i + \lambda_2 \mathrm{RC}_i \cdot \mathrm{POST}_t \\ + \rho_1 \cdot \text{non-RC}_i \cdot \mathrm{RCI}_i + \rho_2 \cdot \text{non-RC}_i \cdot \mathrm{RCI}_i \cdot \mathrm{POST}_t \\ + \rho_3 \cdot \mathrm{RC}_i \cdot \mathrm{RCI}_i + \rho_4 \cdot \mathrm{RC}_i \cdot \mathrm{RCI}_i \cdot \mathrm{POST}_t + \epsilon_{igt}. \end{aligned} \tag{10}$$

In this specification, non-RC_i is an indicator equal to 1 if the property is uncontrolled, while RC_i is an indicator equal to 1 if the property is controlled. The effect of rent control exposure for these two groups is captured by ρ_2 and ρ_4, respectively.

We estimate models of transaction prices for condominiums, single and multi-family properties. Our pooled specifications include dummies for each property type as well as interactions with property type and POST_t. In all of our specifications, we report heteroskedasticity-robust standard errors, clustered at 1990 census block groups.

There are two identifying assumptions for measuring the external effect of exposure to rent control on transaction prices (ρ_2 is equation (8, 9), ρ_2 and ρ_4 is equation (10)). First, we assume that the change in rent control status is not fully anticipated. This assumption appears plausible as rent control was narrowly eliminated by a state-wide referendum that an overwhelming majority of Cambridge residents voted against. Second, we assume that conditional on detailed geographic and time effects, the interaction between initial rent control intensity (RCI) and a time dummy for the post rent-control period is uncorrelated with other unmeasured factors that affect local house prices and change simultaneously but are not caused by the elimination of rent control. While this assumption is harder to verify, we subsequently present event-study graphs that strongly suggest that the effect of rent control intensity on house prices is not present prior to the elimination of rent control and strongly evident thereafter. The identification of the de-control effect (λ_2) rests on a stronger assumption that housing characteristics of a controlled property remain unchanged before and after rent control.

4.2 Data sources

There are approximately 15,000 taxable parcels of land in the City of Cambridge organized into unique geographic units known as "map lots." The foundation for our dataset is a snapshot of the entire universe of residential and commercial structures from an electronic version of the 2001 Cambridge Property Assessor's database. This database includes the map lot, address, owner's name and address, usage, and property tax assessments for the entire stock of housing as of January 2000, for fiscal year 2001. Cambridge does not maintain an electronic version of the 1995 Property Assessor's database, so our closest approximation to the entire housing stock at the time of rent control's elimination is from January 2000. The usage categories are broken down into various commercial and residential designations. We treat any usage code where individuals are likely to live as a residential structure. The main residential usage codes include condominiums, single, two-family, and three-family residential, multi-unit apartment complexes, and mixed residential-commercial structures.

We identify rent controlled properties from the historical records of the Cambridge Rent Control Board obtained via a Freedom of Information Act request. Each record includes the map lot, address, and owner's address for all rent controlled units in 1994. Using the map lot, we merge this file to the housing structures file. For rent controlled records that we cannot match, we attempt to manually merge the record to the corresponding street address. We are able to match all of the rent controlled properties to a residential structure, indicating that 22% of all residential structures and 37.5% of residential units were subject to rent control in Cambridge as of 1994.[3] Even though there may be multiple units of housing at a given map lot, we consider a map lot as rent controlled if any unit at that map lot is controlled. In the case where not all units are rent controlled, this has the potential to over-assign rent control status to a map lot code. This approach is conservative since it eliminates the possibility that we would undercount rent-controlled units and thereby potentially overestimate spillovers.[4] Figure 1 shows a map of all map lots in Cambridge by their rent control status. The maps indicates how pervasive rent control was throughout the city. Neighborhoods

[3]Some popular accounts of rent control in Cambridge estimate the number of controlled dwelling units at between 40-50%. There are a total of 14,123 rent controlled dwelling units in 1995, and 37,639 total units in the Assessing file in 2001, which is 37.5% of units. Rent controlled structures typically had multiple units, which is why a larger fraction of units than structures was controlled.

[4]When measuring the rent control intensity of a given geographic area, however, we calculate the fraction of residential units (rather than structures) that are rent-controlled. This is also conservative in that our calculations err in direction of overassigning units to rent-controlled status while not overstating the extent of exposure of a unit to other rent control properties.

east of Harvard Square with high concentrations of renters have more rent controlled structures than those in the largely owner-occupied parts of western Cambridge.

The resulting data file is matched to a dataset on changes in ownership of residential properties, provided to us by the Warren Group. The Warren Group data cover the period 1988 to 2005 and record basic characteristics of houses involved in each transaction obtained from records of deeds. The data include the sales price of each property, its address, map lot, and various property characteristics such as the number of bedrooms, number of bathrooms, lot size, year built and the property type. We have carefully cleaned the data to remove transactions that appear to be intra-family transfers of ownership rather than arms-length transactions, and duplicate transactions that reflect intermediation or corrections of public records. When the dataset reports property characteristics that are impossible (for example, zero total rooms), we treat these as missing. In total we have 14,654 transactions.[5]

Table 1 presents descriptive statistics of the transactions database by time period, structure type, and rent control status. Slightly more than two-thirds of our transactions are condominiums, while the rest are either single or multi-family houses. Moreover, rent controlled properties account for about one third of all of our transactions, with the large majority of our rent controlled transactions being condos. Sales prices in the early period, 1988-1994, are lower than those in the later period, 1995-2005, and houses typically transact at higher prices than condominiums. In both the early period, 1988-1994, and the later period, 1995-2005, rent controlled houses and condominiums sold at lower prices on average. The characteristics of transacted houses and condominiums do not differ greatly in the early and later period, for both rent controlled and non-rent controlled properties.

The last dataset consists of building permits obtained from the Cambridge Inspectional Services Department. According to Section 110.0 of the Massachusetts State Building Code, anyone "seeking to construct, alter, repair or demolish a structure" must obtain a building permit before the start of work, post this permit at the job site, and commence within 6 months of obtaining the permit. Ordinary repairs such as painting, wallpapering, adding shingles to roofs, defined in the Building Code as "any Maintenance which does not affect the structure, egress, fire protection

[5] We also eliminate a small number of condominiums that were sold hastily at very low prices in 1989 via a legal loophole that was discovered and briefly exploited by a number of landlords, leading the City to quickly close the loophole.

systems, fire ratings, energy conservation provisions, plumbing, sanitary, gas, electrical or other utilities" do not require a permit. The data consists of the address and proposed expenditure for each building permit from 1991-1998, but do not include map lot codes. Using the addresses, we match the building permit data to the structures file. The remaining addresses in the permits data which we were unable to match to a map lot code were matched against the 2005 and 2008 Cambridge Assessor's database, and finally looked up by hand in the 2009 Assessor's database. For the remaining permits, we geocoded the addresses using StreetMaps USA to obtain latitude and longitude, and we matched based on latitude and longitude to the nearest map lot.

4.3 Rent control intensity (RCI)

Our main approach to measuring the exposure of a property to rent control calculates a circle of a given radius around each property, computes the number of rent controlled units inside the circle other than the property that are rent controlled and divides this by the total number of units in the circle. For each structure in our dataset, we compute this measure for 0.10, 0.20, 0.30, and 0.40 miles where distance is measured in terms of a straight line. The relative size of these groups can be seen in Figure 1, where we plot concentric rings with radii 0.10, 0.20 and 0.30.

It is useful to contrast the radial measures of rent control exposure with geographies from the decennial census, which we use for geographic controls. The three main geographies relevant from the U.S. Census are census blocks, block groups and tracts, where census blocks are the smallest geographic unit tabulated by the U.S. Census. As can be seen in the first table of the appendix, Table A.1, tracts have three times the size of block groups, which are five times larger than census blocks. In Cambridge, there are 626 census blocks, which according to the 1990 Census have on average 131 people, just under 60 residential units or 17 residential structures, and an area of 0.01 square miles. Several census blocks make up a census block group, and are 92 census block groups in Cambridge. Census block groups have an area of 0.05 square miles, have 892 people, and 407 residential units or 119 residential structures. There are 30 census tracts, which each consists of several block groups. Additional details on the distribution of mileage across the census geographies is contained in Table A.1.

We focus most of our attention on a definition of rent control intensity for a radius of 0.20 miles, which corresponds to about 0.13 square miles, an area larger than a census block group but smaller than a census tract. The second appendix table, Table A.2, displays summary statistics on the various measures of rent control intensity. The first panel reports the intensity measures for transacted properties in 1988-1994, while the second panel reports the years 1995-2005. For example, for a typical transacted property in the early period, 36% of neighboring properties within a circle of 0.20 miles are rent controlled. The mean level of rent control intensity for transactions which take place in the early or later period is roughly comparable for radii between 0.10 and 0.40 miles, with larger standard deviations at smaller geographies. Overall, there is considerable variation in rent control intensity at 0.20 miles, with a standard deviation of 0.17 and areas where there are 81% of nearby units are rent controlled.[6]

4.4 Threats to validity

4.4.1 Selection into Transaction

One concern for measuring external effects of rent control on the housing stock is that the sales price is only observed for properties that transact. An alternative might be to estimate models of the assessed values of properties as our measure of outcomes, but this has the drawback that assessed values are not market prices and are instead imputed values based on observed market prices. Since we observe a transaction only when a seller is able to find a buyer, there is potential for composition changes to result from selection into transactions. If the characteristics of houses that transact after the end of rent control are different from those that transact during rent control, our estimates may be driven by changes in composition and not directly by the end of rent control.

Figure 2 provides a sense of how much of the overall housing stock in Cambridge is transacted each year. The figure plots the percentage of annual sales that are attributed to rent controlled properties over time. The percentage of sales bottoms out in 1992 at about 18% during the New England housing recession, and climbs to about 27% in 1997 as owners of rent controlled properties sold their properties after rent deregulation. The second plot indicates the fraction of the housing stock that is turned over per year. Both before and after rent control's end, uncontrolled units turn over more frequently than rent controlled units.

[6] Another measure of rent control exposure is to simply count all controlled units within a census geography. The reason we do not focus on measures like this is that with census geographic units it is possible that two properties on opposite sides of the same street are part of different units. Table A.2 in the appendix shows how this measure differs from the radial measures. For both census block and block group, the mean is 0.20 and provides some indication that the census measures of rent control intensity might be measuring different types of exposure than the radial measures.

To explore the importance of compositional changes, we estimate Seemingly Unrelated Regression (SUR) models of how our covariates vary with rent control intensity, and report the estimates in Table 2. The specification is the same as our main specification but the dependent variables are the house characteristics (and of course, equations for all covariates are estimated simultaneously). Table 2 reports regressions of total rooms, bathrooms, bedrooms, square footage, log(lot size) and log(age) on rent control intensity, year of sale dummies, census block group fixed effects for transacted properties in our sample. For each property characteristic, we report coefficients on rent control intensity measured by the radial rent control intensity measure at 0.20 miles. Differences in composition are broken out by uncontrolled houses, controlled houses, uncontrolled condominiums, and controlled condominiums. For example, for uncontrolled houses, the number of total rooms in transacted properties are on average larger in neighborhoods with high rent control intensity, and a one standard deviation increase in rent control intensity is associated with about 0.08 more total rooms. While there are more favorable characteristics of uncontrolled houses that transact after the post period, these differences are insignificant with the exception of square footage and mostly small. The last column of the table reports the test statistic from a Wald test for joint significance across all equations. We cannot reject the hypothesis that the composition of transacted uncontrolled houses have the same observable characteristics of those that transact prior to 1995.

The next three rows of Table 2 parallel the first row. In contrast to the large number of transactions involving non-rent controlled houses, we only have 605 houses that were rent controlled and transact. The joint tests suggests there are not significant changes in observable property characteristics for this group or for uncontrolled condominiums. The one marginally significant test which suggests differences in property characteristics is for controlled condominiums. However, here we see that not all characteristics point in the same direction. While there are more total rooms, there are fewer bathrooms, for instance.

Of course, we cannot rule out the possibility that there are different unobserved characteristics for transacted properties before and after rent control. For observable characteristics, we do not detect statistically significant changes in controlled and uncontrolled houses and uncontrolled condominiums, but we do detect changes in the composition of transacted rent controlled condominiums. This gives us greater confidence with results involving houses over condominiums. As a step towards addressing this concern, we report models where we do not include property characteristics and those that do. It is reassuring, as we will see, that these control variables do not have much impact on the magnitudes of our estimates.

4.4.2 Measuring Exposure to Rent Control

We have already discussed that there are many options for measuring rent control intensity and our approach is to report various measures. An additional concern is that our measure of rent control intensity is proxying for unobserved attributes of a neighborhood's quality. Since we include various geographic fixed effects, the unobserved variation must be within the geographic unit, which is quite small. Despite this, it is less obvious that the effect of rent control intensity on formerly *rent controlled* properties is in fact an external effect due to the proximity of rent controlled properties. It may be unmeasured dilapidation at the location, for instance due to a common infrastructure investment as might be seen in a condominium complex. This issue also might apply to the study of uncontrolled units, but under our identification assumptions, this dilapidation occurs due to proximity to rent control.

Another concern is related to our classification of structures as rent controlled if any dwelling unit at the structure is controlled. Data limitations prevent us from identifying if a sold unit itself was rent controlled instead of the particular structure. If there are many dwelling units at a particular structure, we might be overestimating the fraction of housing that is controlled at that location. This error will lead to an attenuation of the direct effect of rent control in the case where only a fraction of dwelling units are controlled, and a dilution of the spillover effect because the units that we misclassify as rent controlled may actually have the largest spillovers because of their proximity to actual rent control units. Hence, we may be underestimating spillovers, a problem that is likely more severe for condominiums than free standing houses.

4.4.3 Housing Market Trends

The end of rent control in 1995 coincides with a period of nationwide house price appreciation. According to the Federal Housing Finance Agency's OFHEO house price index (HPI) of single-family houses for the Boston Metropolitan Statistical Area, there was a 270% increase in the HPI from the first quarter of 1995 to the first quarter of 2005. This period was also associated with easier credit, which might have lead to greater price changes in lower-income regions. For instance, Mian and Sufi (2009) argue that the rise in mortgage defaults was larger in zip codes with a disproportionately large share of subprime borrowers. If rent control intensity is a proxy for low-income regions, our empirical approach might be capturing differential trends in house appreciation by income due to credit market expansions.

This motivates the inclusion of geographic controls at a fine geographic level such as census block groups, since credit market policies may be more likely to operate at larger geographies as in the zip code level evidence in Mian and Sufi (2009). We also consider the addition of trends in our specifications and show sensitivity of our results to these trends. Let $\tau(i)$ denote the census tract of property i. The specifications with trends include the same model as in (10) together with quadratic trends of the form:

$$\alpha_{0,\tau(i)} + \alpha_{1,\tau(i)}t + \alpha_{2,\tau(i)}t^2, \quad (11)$$

where each tract has a main effect, a linear and quadratic time trends. Since there are 30 census tracts in Cambridge, this involves the addition of up to 90 additional controls. This allows for the flexible evolution of house prices across time and census tracts, which may capture changes in the credit market for low income populations unrelated to rent control exposure.

5 Results

5.1 Effects of Rent Control and Rent Control Intensity

Table 3 reports estimates of equations (9) and (10). Across the columns, we report results varying geographic fixed effects and other control variables. The first two columns report estimates from models that do not include measures of rent control intensity and hence capture the direct effect of decontrol absent any other channels. Columns (2), (4), (8) and (9) include property characteristics interacted with property type (condominium, single family, two family, multi-family) as well as census block group fixed effects. Column (9) also includes trends by adding terms as in equation (11).

Across all specifications, the direct effect of decontrol is large and significant. Rent controlled properties sold before the end of rent control experienced a discount ranging from -13 to -35 log points, or between 12% and 30% price reduction. Rent controlled properties which transact after the end of rent control experienced a price increase of between one and two-thirds of this price discount across specifications.

The estimates for the effect of rent control intensity measured using the radial measure at 0.20 miles as in equation (9) in columns (3) and (4) are also large and significant. In all specifications, we demean the RCI measure so that the rent-control main effect continues to estimate the price differential for the average rent-controlled structure.

We find that the rent control intensity is significantly correlated with the overall price of properties in the absence of geographic fixed effects. The precision of this correlation drops when census block group fixed effects are included in the model. That is, the census block group captures some of the unobserved neighborhood characteristics measured by existing rent control intensity. Of greater interest is the coefficient of RCI $\times$ Post which allows us to measure the effect of rent control intensity on transactions prices after 1995. For the specifications in columns (3)-(4), we find that the coefficient has a smaller magnitude than the coefficient on RCI. This suggests that the net effect of rent control intensity is negative. That is, if the same house in the same neighborhood was sold prior to the end of rent control versus after rent control, the house would sell at a greater discount before than after the end of rent control.

The next three columns report estimate of equation (10) with non-RC and RC interactions with rent control intensity. The estimates allow us to contrast the effect of rent control intensity on controlled versus non-controlled properties. The table reveals an interesting pattern: Prior to the end of rent control, the RCI main effect is generally negative, but with various degrees of precision. But it is large and robustly negative for rent-controlled properties. Following rent control's end, rent control properties rise in value (see the rent control x post variable), while property values rise by relatively more in rent control intensive areas. The RCI effect for non-RC properties is of similar magnitude for controlled versus non-controlled properties, which is a first piece of evidence suggesting positive spillovers from the elimination of rent control to uncontrolled properties. The estimates from the model with trends do not differ significantly from the model without trends in column (7).

5.2 Property types

To understand the underlying source of the price effects, we report our main specification for subsamples of houses and condominiums, separately in Table 4. We find that the direct negative effect of rent control is smaller for houses than the overall sample, and the price appreciation after rent deregulation is closer to the price discount than for the overall sample. In contrast, the price discount for condominiums is larger than for houses, and the price appreciation for rent control is smaller than for houses.

Across the specifications in for houses in the table, the coefficient on RCI × Post is significantly positive. The magnitude implied by column (3) is that a one standard deviation higher RCI (about 0.16) leads to a 0.321*0.16 = 0.05 or about a 5% increase in the price of uncontrolled houses after 1994. The size of the estimates is similar across the various specifications including geographic fixed effects and quadratic tract trends. The estimates for formerly controlled houses are imprecise and sensitive to controls, which indicate that the spillovers detected for houses accrue primarily or exclusively to non-controlled houses. The total number of rent controlled houses in our sample is only 605, so this may be indicative of a lack of power.

Condominiums show a different pattern, with larger estimates for controlled properties. Without any geographic controls or trends, the estimate for the effect of RCI on formerly controlled properties is large and significant and is also significant for uncontrolled condominiums, but it loses precision with the addition of controls. These estimates are not sufficiently precise overall to warrant firm conclusions. Indeed, it is possible that the end of rent control lead to an increase in the supply of properties which are substitutes for condominiums and this confound prevents us from detecting any significant pattern.

To understand the time series of these effects, in Figure 3, we report "event study" plots of estimates of equations (9) and (10) without trends, where we allow λ_2 and ρ_2 to different each year, by interacting it time. In the figure, we normalize the base year to 1994, so that the estimated coefficient is zero that year. Since the models allow for the effects to differ each year, they are estimated with greater noise, so we plot both the point estimate for each year and a 95% confidence interval.

The first panel shows that effect of rent control is relatively flat before 1994, but after the end of rent control there is an upward trend. The pattern is the same in panel (b) for rent control intensity, where the effect is relatively flat prior to 1994, but is on an upward trend thereafter, with the sharpest increase immediately after decontrol. By focusing on uncontrolled properties, we see an even clearer picture with a significant RCI trend after 1994, and a relatively flat trend before. The sharpest picture emerges for houses in panel (d). Taken together, the last three panels strongly indicate that the effect of rent control intensity on house prices is not present prior to the elimination of rent control and apparent thereafter. Moreoever, the upward trajectory of the effect of rent control intensity is broadly consistent with a causal effect.

5.3 Alternative Measures of Rent Control Intensity

As mentioned above, we have no a priori reason to measure rent control exposure with a radius of 0.20 miles or census block groups. To explore the sensitivity of our main estimates to the measure of rent control intensity and geographic effects, in Table 5 we report estimates from a battery of other specifications. The models estimated in the top panel are the same as those in column (8) of Table 3, while the models in the bottom panel are the same as those in column (9) of Table 3. The table only reports estimates from interactions with Post.

For all of the models we estimate, the direct effect of decontrol ranges between 0.06 to 0.10 log points. The effect is slightly larger for houses than for condominiums, and the set of estimates is relatively insensitive to the RCI definition and geographic fixed effects. This is, perhaps, unsurprising because the direct effect of decontrol does not depend on geographies and instead only on the particular location.

For radii between 0.20, 0.30 and 0.40 miles, the estimates for rent control intensity for both controlled and uncontrolled property are of similar magnitudes, though with varying degrees of precision across specifications. In particular, in the sample of all transactions, uncontrolled properties at a radius of 0.20 miles or greater have estimates of between 0.2 and 0.3. The mean level of RCI at each of the radii – 0.20, 0.30 and 0.40 – are roughly the same at 0.37 (see Table A.2). Hence, the implied effect across the estimates is between 8% and 11% using estimates without trends, and it is similar in the models with trends, though in the estimate of the model with trends using block group fixed effects, the point estimates are no longer significant.

With a smaller radius for RCI of 0.10 and 626 census block controls, the effect of RCI on uncontrolled properties is no longer significant, though this may be due to a large positive result for uncontrolled houses for the same specification (as in column (5)) and no result for condominiums (as in column (7)). At this small geography, exposure to rent control appears to influence the price of formerly controlled properties, and the point estimate becomes more significant with the inclusion of trends. Looking at the break out by houses and condominiums, the pattern is mirrored by the condominium results. This provides further indication that the spillovers may operate differently between structure types, with results being larger, more precise, and robust for condominiums at smaller geographies.

It is important to note that since there is overlap in RCI defined, say at 0.20 and 0.30 miles, we cannot directly use these estimates to compare the relative impact of spillovers. In unreported specifications, we investigate models where we allow for two different measures of rent control intensity in the same regression equation. The "inner" measure corresponds to rent control intensity inside the given radius (as before), while the "outer" measure consists of rent control intensity between the boundary of the inner measure and the next distance.

When we estimate models where the inner measure is rent control intensity within 0.20 miles and the outer measure is between 0.20 miles and 0.30 miles, our pattern is consistent with a spillover effect that depends on geographic proximity. The estimate of RCI is virtually unchanged from our model with only one intensity measure, and the point estimate for the outer measure is 0 (with standard error 0.15). However, when we repeat the same exercise when the inner geography is RCI within 0.10 miles, and the outer geography is between 0.10-0.20 miles, we find instead that 0.10-0.20 miles has greater weight. This suggests that that RCI within 0.10 miles is not very informative for measuring localized spillovers for uncontrolled housing.

One last robustness check we examined but do not report is to examine a shorter time period. Between 1990-2000, 64% of our transactions take place between 1990-2000. Focusing just on this period, the point estimates of ρ_2 for uncontrolled houses parallel the pattern described for the longer time period, though as might be expected with fewer observations they are no longer precisely estimated.

6 Building Permits and Investment

In contrast to the data on transactions prices, the data on building permits available from the City of Cambridge span only the period of 1991-1998. Unfortunately, Cambridge Inspectional Services does not maintain any records of permitting activity after 1998, so this severely limits the time period of our analysis. Table 6 presents some descriptive statistics on investment for properties by their structure type. After matching each permit to a map-lot, we identify the usage of the structure using the usage code in the Assessing file. This allows us to identify houses and condominiums.[7] It is important to note that investment data are self-reported data and so it is possible that the incentives to declare or file for investment permits changed with the rent control regime. In principle, under control, a landlord might have been more likely to declare investment activity to justify a price increase from the rent control board, a possibility we cannot exclude.

Since permit activity can either be filled for a structure (as in a condominium complex with multiple condominiums) or for each unit within a structure, we report the number of permits and the number of permitted units as the first two rows of Table 6. If a structure has a permit, to compute the permitted units, we say assume that each unit at the structure obtains a permit. Hence, the number of permitted units is greater than the number of structures with a permit, and this difference is more pronounced for condominiums (which have more units) than for houses.

The number of houses with a permit is small, with about 5% of uncontrolled houses ever with a permit and 7% of controlled houses. Interestingly, this number is virtually unchanged before and after decontrol. For condominiums, overall permitting activity is much greater. Permitting activity for uncontrolled condominiums, however, show the opposite pattern: about 19% percent of uncontrolled condominiums units ever have a permit before decontrol, and surprisingly, the number after decontrol is lower at 17%.

Each building permit includes an estimated expense and we convert this to 2008 dollars using Consumer Price Index housing price index for all cities (series CUUR0000SAH) as the numeraire. Cambridge experienced an overall investment boom after the end of rent control. Aggregate investment at houses and condominiums went from 27 million to 67 million (2008 dollars) from 1991-1994 to 1995-1998. Most of this change comes from a difference of 30 million dollars in investment at uncontrolled houses. At a per unit level, uncontrolled houses more than doubled their mean investment to $1,800 and the level for uncontrolled condominiums is similar. Mean investment expenditure at controlled houses and condominiums is lower than the corresponding amount for uncontrolled structures, but the percentage increase in the post period is greater.

Housing investment expenditure is in general lumpy, a fact seen in other studies of investment (Caballero and Engel (1999)). Most units do not receive investments in a given year, and so the median expenditure is zero. However, conditional on receiving investment, there are large

[7] Houses and condominiums constitute the largest structure types in the Assessing file. This file also includes investment data for apartments, which we do not focus on because we have no corresponding prices for apartments.

increases during decontrol for all structure types, whether controlled or uncontrolled. The standard deviation and maximum value of investment is large, which motivates examining the sensitivity of our estimates to outliers in Table 10.

We begin our investigation of the effects of rent control on investment by measuring the direct effect of rent control on permitted investments. Since we match the permit data to the structures file, we are able to observe permitting activity at every structure and hence our data is a balanced panel of structures by year. This gives us many more observations than the transactions price analysis, though the majority of observations are zeros. In the regression, we include year of sale dummies, structure type and post interactions, and number of units and its square as controls. We regress investment expenditure on a rent control indicator and the rent control indicator interacted with various post measures.

The estimates in Table 7, columns (1) and (2) are the direct effect of decontrol on investment per unit. Although there is strong evidence for an aggregate change in investment, there is no differential effect where formerly controlled properties experience more investment than controlled properties. While column (8) suggests that controlled units are 3% more likely to undertake any investment, this estimate is impreciise and is mostly driven by condominiums as can be seen in Table 8. Overall, Table 7 shows a discouraging picture for both the direct effect of greater investment or any spillover effect of investment.

In Table 8, we split the sample by houses and condominiums. The table reveals some weak evidence that investment increased more for controlled houses in rent control intensity neighborhoods. Note that although only 626 controlled houses transact, there are a little less than 1,000 controlled houses in the housing stock. Investment at condominiums does not seem to be differentially affected by decontrol.

Paralleling Table 5 for prices, in Table 9 we show various specifications for investment. The table indicates that precision increases in our models with trends. Although we add more control variables, since our investment dataset is at the unit level, there are many more observations. Hence, it is not totally unsurprising that there are precision gains for specifications that include trends.

Finally, in Table 10 we consider the effect of extreme values of investment on our estimates. This table reports the estimates where we winsorize at the 98th percentile and 96th percentile.

This means that in the sample, for each year, we compute the distribution of investment per unit when it is positive. For houses, this is about 0.06 of units, while for condominiums it is about 0.20. Then, we set any value at the 98th percentile or greater equal to the value at the 98th percentile and add a dummy to our specification. This change effects an average of 0.02*0.06 = 1% of housing observations and 0.4% of condominium observations at the 98th percentile, and double these numbers at the 96th percentile. The estimates show that the results are sensitive to these extreme values and become much smaller. This suggests that our results for investment spillovers for controlled houses are not definitive.

Given the aggregate investment boom in Cambridge after decontrol, it is surprising that we find little evidence for more building expenditure at controlled properties. It is possible that an even larger investment boom takes place more in controlled properties after 1998, the last year of our data. Another interpretation is that the end of control was a Cambridge coordination device which led to more investment independent of rent control and exposure to other controlled properties. Of course, given our data, it is impossible to distinguish this effect from an overall increasing trend in investment over time. A second factor is a lack of power, since the vast majority of investment activity is zero. Despite this, our estimates can rule out large investment effects. Investment may be occurring in ways that we cannot measure, for instance with activity not requiring permits. Still, even with an overall increase in investment of 40 million dollars, and if each unit experienced a $1,000 increase in investment which would imply an additional approximately 10 million dollars of investment, it seems unlikely that the investment effects we see, even at the aggregate, would be sufficient to explain the large overall price changes for uncontrolled properties.

7 Interpreting the Magnitude of Spillovers

The estimates so far reveal statistically significant spillovers from the elimination of rent control onto neighboring residential houses. But how large are these spillovers? The answer is not entirely obvious from the tables above; because average house prices were initially lower in high RCI areas, the average dollar value of spillovers is less than would be implied by applying the point estimates obtained above to the (exponentiated) mean house price in the sample. To generate an estimate that takes account of the covariance between initial house prices and the differential price rises in high RCI areas, we perform a regression-based simulation using a variant of the main estimating equation above:

$$\log(p_{igt}) = \alpha_t + \gamma_g + \beta' X_i + \rho_{1,0} RCI_i + \sum_{j=1995}^{2005} 1\left[t = j\right] \cdot \rho_{1,j} \cdot RCI_i + \alpha_{0,\tau(i)} + \alpha_{1,\tau(i)} t + \alpha_{2,\tau(i)} t^2 + \epsilon_{igt}.$$

This model is identical to our main estimating equation except that it is estimated exclusively for uncontrolled properties, it includes a full set of RCI $\times$ year interactions for the post rent control era, and it includes quadratic tract trends.

To calculate counterfactuals, we first fit this equation to the price data and calculate an expected real log price for each unit sold in each year. With 92 block group dummies, this model fits the price series quite well (R^2 of 0.66). We next by zero out the coefficients $\rho_{1,1995}, \rho_{1,1996}, ..., \rho_{1,2005}$ to calculate a counterfactual predicted price for each unit sold in each year. Implicitly, this counterfactual assumes that were rent control not eliminated, the relationship between rent control intensity and house prices estimated for 1988 through 1994 ($\hat{\rho}_{1,0}$) would have continued through 2005. We plot the mean predicted price in Figures 4a and 4b. The gap between these two series provides an estimate of how much the elimination of rent control affected the price of uncontrolled houses sold in each year on average.

Uncontrolled house prices commence a steep real price increase in 1996, as shown in the first panel of Figure 4a. This growth is of course not attributable entirely to the end of rent control, as there were many other macroeconomic factors at play in this period. Notably, the (predicted) actual and counterfactual sales price series in the first panel track closely until 1998, at which point the series diverge, indicating that spillovers became economically significant. The cumulated mean estimated spillover rises from \$24,339 in 1996 to \$136,213 in 2001, before falling to \$118,000 in 2004. Figure 4b repeats the main estimates from the first panel of Figure 4a, here for uncontrolled condominiums. Consistent with our smaller estimates for condominiums, the two lines in the plot are much closer together than for houses.

A final noteworthy feature of these figures is that the counterfactual series begin to reconverge with the (predicted) actual series after 2001, and dramatically so after 2003. Though we do not have a definitive explanation for this pattern, one conjecture is that rent control intensive neighborhoods may have received significant liquidity injections during the subprime mortgage boom of the 2000s, which in turn may have lead to a further run-up and then subsequent drop in house prices. This consideration suggests that some caution should be implied when interpreting the point estimates for the later years of our sample as reflecting only rent control spillovers.

The figures reveal the price path for transacted houses, but do not reveal the effect of the elimination on the overall uncontrolled stock of Cambridge housing. As we saw in Figure 2, less than 5% of the overall housing stock turns over each year of the sample. We have 4,255 transactions of uncontrolled housing, and only 2,612 or 61% occur between 1995-2005. In contrast, we have 7,937 uncontrolled houses. Uncontrolled condominiums turn over more frequently than houses: there are 4,853 uncontrolled condos in the housing stock, and 5,678 transactions of uncontrolled condominiums, though not all of condo stock transacts.

To understand the magnitude of the end of control on the housing stock, it is necessary to estimate the value of properties that did not transact. To this end, we use estimates from the Cambridge Property Assessing database as our measure of the value of the housing stock. This has the drawback that these are not necessarily market prices, but the assessor imputes a valuation for each property using transaction prices and historical prices as inputs. A second caveat is that we only have access to the Assessment records with assessed values from 2003, so we are unable to track the evolution of the effect of decontrol for the intermediate years.

In 2003, for the 7,937 uncontrolled houses and 4,853 uncontrolled condominiums in the Cambridge housing stock, the total assessed value is $7.5 billion, with houses accounting for 72% of the value. To gauge how the elimination of rent control influenced these valuations, for each location, we compute rent control intensity at that location, and subtract the implied price change due to the elimination of rent control exposure. For instance, the aggregate value of the stock of uncontrolled houses is $5.4 billion in 2003 and had rent control continued, our estimates from the model with Census block group fixed effects and trends imply that the aggregate value would have been $4.5 billion. For condominiums, the aggregate value of the stock of uncontrolled condominiums is $2.1 billion and had rent control continued the value would be $1.9 billion. Summing the difference for these two uncontrolled property types generate our estimate that the elimination of rent control lead to about a $1 billion change in value of the uncontrolled housing stock.

8 Conclusion

This paper has focused on the measurement of spillovers from the elimination of rent control onto transactions prices and investments of both formerly controlled and uncontrolled residential properties. Our main finding is a positive price spillover onto uncontrolled properties driven mostly by houses. Our back-of-the envelope calculation suggests that 22% of increase in the value of the uncontrolled housing stock in Cambridge by 2003 is due to decontrol. We investigate one channel for this effect by examining changes in investment. This analysis suggests that either aggregate changes in investment expenditure generate large price effects or other channels play a significant role in generating price effects.

Externalities that result from price regulations such as rent control can operate via a number of channels that merit additional exploration. Glaeser and Luttmer (2003) provide evidence that the allocative inefficiency of rent control is large and may swamp the direct effects of rent control on quantity undersupply and underinvestment. Together with data on the identities of tenants, the unique episode of rent deregulation in Cambridge will permit us to further analyze the allocative effects of this price regulation by exploring the characteristics of individuals who lived in rent controlled apartments prior to 1995, and what demographic changes took place in these units and surrounding dwellings thereafter. Research on these questions will bring us closer to a comprehensive understanding of the welfare consequences, and accompanying neighborhood dynamics, resulting from public policies that shape the quantity, quality and pricing of residential housing.

Table 1. Descriptive Statistics for Transactions Database

	Houses				Condominiums			
	Uncontrolled		Controlled		Uncontrolled		Controlled	
	1988-1994	1995-2005	1988-1994	1995-2005	1988-1994	1995-2005	1988-1994	1995-2005
log price (real dollars)	12.38	13.01	12.09	12.76	12.08	12.56	11.70	12.32
	(0.74)	(0.82)	(0.80)	(0.76)	(0.54)	(0.60)	(0.65)	(0.70)
total rooms	9.18	9.41	10.25	10.28	4.77	5.01	4.39	4.40
	(3.35)	(3.45)	(3.57)	(3.66)	(1.52)	(1.93)	(1.61)	(1.56)
bedrooms	4.06	4.10	4.56	4.63	2.00	2.11	1.68	1.74
	(1.69)	(1.72)	(1.79)	(1.85)	(0.78)	(0.96)	(0.71)	(0.82)
bathrooms	2.77	2.80	2.93	2.91	1.57	1.60	1.15	1.21
	(0.95)	(0.95)	(0.87)	(0.85)	(0.66)	(0.73)	(0.43)	(0.51)
interior sq ft (100)	236.58	238.10	240.01	240.76	120.21	125.39	92.33	93.85
	(113.18)	(106.67)	(91.23)	(90.18)	(83.03)	(82.36)	(43.70)	(45.10)
has lot	0.99	0.99	0.99	0.99	0.02	0.04	0.02	0.02
	(0.11)	(0.09)	(0.09)	(0.09)	(0.16)	(0.19)	(0.13)	(0.15)
lot size	42.04	42.31	33.20	34.56	1.19	1.62	0.73	0.88
	(34.20)	(34.37)	(19.71)	(20.23)	(15.86)	(11.60)	(5.85)	(7.07)
year built	1903.32	1902.87	1890.87	1892.20	1946.77	1935.78	1916.75	1916.90
	(36.92)	(37.23)	(24.69)	(24.35)	(42.02)	(40.12)	(26.62)	(26.26)
# of units	1.58	1.62	1.95	1.99	43.30	28.14	28.85	25.33
	(1.00)	(1.15)	(0.81)	(0.92)	(53.30)	(43.39)	(27.83)	(27.41)
N=	1,643	2,612	263	342	2,173	3,505	1,424	2,692

Notes: Transactions data from the Warren Groups change of ownership file for 1988-2005. Lot size is conditional on having a lot. Number of units for a condo is the number of units in the building the invididual condo was transacted.

Table 2. Tests for Changes in Characteristics for Transacted Properties

	total rooms	bathrooms	bedrooms	square feet[γ]	lot size[η]	log(age)	Wald test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				I. Uncontrolled Houses			
RCI x Post	0.49	0.10	0.23	30.52*	4.83	-0.03	4.18
	(0.46)	(0.15)	(0.28)	(16.26)	(5.67)	(0.13)	p=0.65
N=				4255			
				II. Controlled Houses			
RCI x Post	-2.42	-0.63	-0.97	-34.06	-23.00**	0.06	9.36
	(1.50)	(0.41)	(0.94)	(41.57)	(9.26)	(0.19)	p=0.15
N=				605			
				III. Uncontrolled Condominiums			
RCI x Post	-0.30	-0.09	-0.14	-4.97	0.01	-0.25*	4.57
	(0.26)	(0.11)	(0.14)	(12.50)	(0.03)	(0.14)	p=0.60
N=				5678			
				IV. Controlled Condominiums			
RCI x Post	0.32	-0.19*	0.26	4.10	-0.02	-0.05	13.20
	(0.34)	(0.11)	(0.17)	(9.98)	(0.03)	(0.08)	p=0.04
N=				4116			

Notes: Rent Control Intensity (RCI) measure based on radius of 0.20 miles

All specifications include RCI, year of sale dummies, block group fixed effects, a dummy variable for imputed year built and interactions between structure type and post.

[γ] Square feet is thousand square feet of interior space

[η] Lot size is hundred square feet of lot size in house specifications and a dummy for non-zero lotsize in condo specifications

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3. Price Effects of Rent Control and Rent Control Intensity with Interactions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RC	-0.350***	-0.224***	-0.309***	-0.210***	-0.294***	-0.131***	-0.192***	-0.188***
	(0.050)	(0.033)	(0.051)	(0.034)	(0.049)	(0.041)	(0.034)	(0.033)
RC x Post	0.111***	0.104***	0.076*	0.086**	0.070*	0.060*	0.087***	0.079**
	(0.041)	(0.034)	(0.042)	(0.034)	(0.040)	(0.035)	(0.033)	(0.032)
RCI			-0.436**	-0.443				
			(0.212)	(0.303)				
RCI x Post			0.360***	0.212***				
			(0.092)	(0.072)				
non-RC x RCI					-0.352	-0.123	-0.257	-0.287
					(0.250)	(0.206)	(0.277)	(0.281)
non-RC x RCI x Post					0.337***	0.282***	0.221***	0.236**
					(0.105)	(0.098)	(0.078)	(0.106)
RC x RCI					-0.676**	-0.477**	-0.780**	-0.842**
					(0.299)	(0.237)	(0.354)	(0.357)
RC x RCI x Post					0.431**	0.407**	0.155	0.255
					(0.204)	(0.181)	(0.192)	(0.212)
Other X's		x		x		x	x	x
Block Group fixed effects		x		x			x	x
Trends								x
Observations	14654	14654	14654	14654	14654	14654	14654	14654
R-squared	0.361	0.644	0.364	0.645	0.364	0.576	0.647	0.647

Notes: Heteroskedasticity-robust standard errors clustered by 1990 Census Block Group level

RC = rent control indicator, Rent Control Intensity (RCI) measure based on radius of 0.20 miles, Post = 1(year >= 1995)

All specifications include year of sale dummies and structure type x post interactions

Other X's include number of total rooms, bathrooms, bedrooms, interior square footage, lot size and age of structure interacted with structure type

Block group fixed effects consist of 92 blocks groups from 1990 Census

Trends are quadratic functions of sale year where function is allowed to vary for each census tract

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4. Price Effects of Rent Control and Rent Control Intensity with Interactions by Structure Type

	Houses			Condominiums		
	(1)	(2)	(3)	(4)	(5)	(6)
RC x Post	0.059	0.104*	0.101*	0.058	0.074*	0.064*
	(0.054)	(0.054)	(0.053)	(0.045)	(0.039)	(0.037)
non-RC x RCI x Post	0.297***	0.381***	0.321**	0.264*	0.117	0.073
	(0.104)	(0.098)	(0.150)	(0.135)	(0.102)	(0.146)
RC x RCI x Post	0.159	0.037	-0.065	0.449**	0.237	0.328
	(0.300)	(0.280)	(0.312)	(0.195)	(0.198)	(0.226)
Block Group fixed effects		x	x		x	x
Trends			x			x
Observations	4860	4860	4860	9794	9794	9794
R-squared	0.564	0.664	0.672	0.525	0.611	0.624

Notes: Heteroskedasticity-robust standard errors clustered by 1990 Census Block Group level

RC = rent control indicator, Rent Control Intensity (RCI) measure based on radius of 0.20 miles, Post = 1(year >= 1995)

All specifications include main effects for RC, non-RC x RCI, RC x RCI, year of sale dummies and structure type x post interactions

Specifications also include number of total rooms, bathrooms, bedrooms, interior square footage, lot size and age of structure interacted with structure type

Trends are quadratic functions of sale year where function is allowed to vary for each census tract

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5. Price Effects for Various Neighborhood Definitions

Sample	All transactions				Houses		Condos	
Radius for RCI	0.10	0.20	0.30	0.40	0.10	0.30	0.10	0.30
Fixed Effects	Block	Block Group	Block Group	Tract	Block	Block Group	Block	Block Group
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					I. No Trends			
RC x Post	0.076***	0.087***	0.088***	0.083**	0.101*	0.105*	0.061*	0.078*
	(0.027)	(0.033)	(0.034)	(0.034)	(0.054)	(0.054)	(0.032)	(0.040)
non-RC x RCI x Post	0.052	0.221***	0.211**	0.308***	0.269***	0.370***	-0.050	0.079
	(0.068)	(0.078)	(0.093)	(0.096)	(0.090)	(0.105)	(0.083)	(0.142)
RC x RCI x Post	0.257*	0.155	0.177	0.239	0.229	0.009	0.321**	0.262
	(0.137)	(0.192)	(0.195)	(0.189)	(0.235)	(0.306)	(0.128)	(0.211)
					II. Quadratic Census Tract Trends			
RC x Post	0.077***	0.079**	0.081**	0.078**	0.096*	0.102*	0.055*	0.069*
	(0.027)	(0.032)	(0.032)	(0.033)	(0.051)	(0.052)	(0.030)	(0.037)
non-RC x RCI x Post	0.034	0.236**	0.201	0.302**	0.127	0.282*	-0.090	0.030
	(0.068)	(0.106)	(0.130)	(0.139)	(0.124)	(0.168)	(0.086)	(0.187)
RC x RCI x Post	0.289**	0.255	0.263	0.342	0.063	-0.127	0.373***	0.321
	(0.145)	(0.212)	(0.248)	(0.254)	(0.255)	(0.341)	(0.139)	(0.283)

Notes: Heteroskedasticity-robust standard errors clustered by 1990 Census Block Group level
All specifications include main effects for RC, non-RC x RCI, RC x RCI, year of sale dummies and structure type x post interactions.
Specifications also include number of total rooms, bathrooms, bedrooms, interior square footage, lot size and age of structure interacted with structure type
Trends are quadratic functions of sale year where function is allowed to vary for each census tract
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6. Descriptive Statistics for Investment

	Houses				Condominiums			
	Uncontrolled		Controlled		Uncontrolled		Controlled	
	1991-1994	1995-1998	1991-1994	1995-1998	1991-1994	1995-1998	1991-1994	1995-1998
# Permits*	1443	1440	262	270	227	341	177	254
# Permitted Units	2512	2462	608	613	2847	2950	2696	3918
Fraction of units permitted**	0.05	0.05	0.07	0.07	0.19	0.17	0.17	0.24
Mean units in permitted structures	1.74	1.71	2.32	2.27	12.54	8.65	15.23	15.43
Expenditure per Unit								
Mean ($1,000s of 2008 $)	0.78	1.80	0.63	1.59	0.36	1.89	0.21	1.20
Standard Deviation	26.31	42.75	5.56	40.89	3.20	47.00	1.78	21.77
Expenditure per Permitted Unit								
Mean ($1,000s of 2008 $)	15.98	37.68	9.29	23.16	2.31	11.71	1.32	5.11
Standard Deviation	118.12	192.08	19.33	154.57	7.81	116.39	4.26	44.74
Median	3.95	5.27	2.88	3.49	0.40	0.89	0.38	0.37
Min	0.07	0.03	0.14	0.07	0.02	0.02	0.03	0.03
Max	5675.5	5227.8	163.2	2613.9	98.7	3267.4	56.8	1151.7
N=	29,678		3,947		3,002		1,666	

Notes: Summary statistics are frequency weighted by number of units in structure

*Number of Permits is the number of structures with a building permit during the year

**Average number of units in permitted structures

Table 7. Investment Effects of Rent Control and Rent Control Intensity with Interactions

	Investment per Unit							Any Investment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RC	-0.04	-0.03	-0.03	-0.04	0.00	0.10	0.06	0.01
	(0.08)	(0.11)	(0.09)	(0.12)	(0.09)	(0.13)	(0.10)	(0.01)
RC x Post	-0.19	-0.19	-0.17	-0.17	-0.65*	-0.65*	-0.57*	0.03*
	(0.58)	(0.58)	(0.51)	(0.51)	(0.38)	(0.38)	(0.32)	(0.02)
RCI			-0.16	1.97				
			(0.61)	(1.52)				
RCI x Post			-0.21	-0.22				
			(1.62)	(1.63)				
non-RC x RCI					-0.06	2.30	0.46	-0.28*
					(0.80)	(1.60)	(2.10)	(0.16)
non-RC x RCI x Post					-1.72	-1.75	1.94	0.02
					(1.89)	(1.90)	(2.35)	(0.06)
RC x RCI					-0.46	0.46	-0.67	-0.23*
					(0.28)	(1.38)	(1.57)	(0.13)
RC x RCI x Post					4.66	4.66	6.92**	0.04
					(3.99)	(3.99)	(3.31)	(0.21)
Block Group fixed effects		x		x		x	x	x
Trends							x	
Observations	190677	190677	190677	190677	190677	190677	190677	190864
R-squared	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.11

Notes: Heteroskedasticity-robust standard errors clustered by 1990 Census Block Group level

Investment per Unit is frequency weighted by the number of units in a structure

RC = rent control indicator, Rent Control Intensity (RCI) measure based on radius of 0.20 miles, Post = 1(year >= 1995)

All specifications include main effects for RC, non-RC x RCI and RC x RCI, and year dummies

Trends are quadratic functions of sale year where function is allowed to vary for each census tract

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8. Investment Effects of Rent Control and Rent Control Intensity with Interactions by Structure Type

	Houses				Condominiums			
	Investment per Unit			Any Investment	Investment per Unit			Any Investment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RC x Post	-0.29	-0.29	-0.17	0.00	-1.01	-1.01	-0.95	0.10**
	(0.39)	(0.39)	(0.39)	(0.01)	(0.63)	(0.63)	(0.60)	(0.04)
non-RC x RCI x Post	-1.61	-1.61	4.07	-0.00	-1.99	-2.04	-0.83	0.39
	(2.16)	(2.16)	(2.83)	(0.02)	(2.84)	(2.88)	(2.92)	(0.31)
RC x RCI x Post	10.63*	10.63*	14.46**	0.03	3.22	3.22	7.30*	0.12
	(5.84)	(5.84)	(5.80)	(0.05)	(3.91)	(3.91)	(3.83)	(0.31)
Block Group fixed effects		x	x	x		x	x	x
Trends			x	x			x	x
Observations	120842	120842	120842	120896	69835	69835	69835	69968
R-squared	0.00	0.00	0.01	0.01	0.00	0.01	0.01	0.15

Notes: Heteroskedasticity-robust standard errors clustered by 1990 Census Block Group level
Dependent variable is investment per unit, frequency weighted by the number of units
RC = rent control indicator, Rent Control Intensity (RCI) measure based on radius of 0.20 miles, Post = 1(year >= 1995)
All specifications include main effects for RC, non-RC x RCI and RC x RCI, and year dummies
Trends are quadratic functions of year where function is allowed to vary for each census tract
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9. Effects on Investment per Unit for Various Neighborhood Definitions

Sample	All transactions				Houses		Condos	
Radius for RCI	0.10	0.20	0.30	0.40	0.10	0.30	0.10	0.30
Fixed Effects	Block	Block Group	Block Group	Tract	Block	Block Group	Block	Block Group
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					I. No Trends			
RC x Post	-0.64*	-0.65*	-0.46	-0.46	-0.25	-0.12	-1.08*	-0.86
	(0.39)	(0.38)	(0.44)	(0.43)	(0.40)	(0.42)	(0.65)	(0.75)
non-RC x RCI x Post	-2.09	-1.75	-2.94	-2.49	-1.16	-1.74	-3.60	-6.37
	(1.73)	(1.90)	(2.09)	(2.03)	(2.04)	(2.00)	(2.67)	(4.15)
RC x RCI x Post	3.86	4.66	2.73	3.13	8.16	8.16*	3.08	1.13
	(3.42)	(3.99)	(2.82)	(3.31)	(4.96)	(4.35)	(3.41)	(2.74)
					II. Quadratic Census Tract Trends			
RC x Post	-0.55*	-0.57*	-0.37	-0.42	-0.16	-0.01	-0.98*	-0.75
	(0.32)	(0.32)	(0.39)	(0.39)	(0.40)	(0.43)	(0.53)	(0.65)
non-RC x RCI x Post	-0.07	1.94	1.60	2.73	2.34	4.69	-2.98*	-4.57
	(1.75)	(2.35)	(3.73)	(4.00)	(2.13)	(4.47)	(1.67)	(3.61)
RC x RCI x Post	4.64*	6.92**	5.18	7.18	10.50**	12.53**	5.20*	4.01
	(2.69)	(3.31)	(3.39)	(4.34)	(4.63)	(5.54)	(2.83)	(3.98)

Notes: Heteroskedasticity-robust standard errors clustered by 1990 Census Block Group level
Dependent variable is investment per unit, frequency weighted by the number of units
All specifications include main effects for RC, non-RC x RCI, RC x RCI, geographic fixed effects and year dummies
Trends are quadratic functions of year where function is allowed to vary for each census tract
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 10. Effect on Investment for Various Neighborhood Definitions, Sensitivity to Outliers

	Houses			Condominiums		
Winsorization	None	at 98th percentile	at 96th percentile	None	at 98th percentile	at 96th percentile
	(1)	(2)	(3)	(4)	(5)	(6)
				I. No Trends		
RC x Post	-0.29	-0.33	-0.19	-1.01	-0.57	-0.06
	(0.39)	(0.27)	(0.22)	(0.63)	(0.41)	(0.26)
non-RC x RCI x Post	-1.61	-0.25	-0.02	-2.04	-1.39	-1.32
	(2.16)	(1.31)	(0.84)	(2.88)	(2.00)	(1.39)
RC x RCI x Post	10.63*	6.62*	4.96	3.22	3.19	0.58
	(5.84)	(3.67)	(3.16)	(3.91)	(3.91)	(1.78)
				II. Quadratic Census Tract Trends		
RC x Post	-0.17	-0.27	-0.18	-0.95	-0.60	-0.18
	(0.39)	(0.27)	(0.24)	(0.60)	(0.37)	(0.22)
non-RC x RCI x Post	4.07	3.31**	2.20*	-0.83	-1.73	-1.28
	(2.83)	(1.55)	(1.15)	(2.92)	(2.48)	(1.23)
RC x RCI x Post	14.46**	9.17**	6.57*	7.30*	5.27*	1.78
	(5.80)	(3.89)	(3.35)	(3.83)	(2.78)	(1.50)

Notes: Heteroskedasticity-robust standard errors clustered by 1990 Census Block Group level
Dependent variable is investment per unit, frequency weighted by the number of units
All specifications include main effects for RC, non-RC x RCI, RC x RCI, block group fixed effects, and year dummies
Winsorization points are computed for each sample, and involve setting any value greater than the winsorization point equal to the winsorization point and including a dummy for these cases
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 11. Estimated Average Price Path with and without the End of Rent Control

Year	Uncontrolled Houses				Uncontrolled Condominiums			
	No FE, no trends		FE and trends		No FE, no trends		FE and trends	
	without Rent Control	with Rent Control	without Rent Control	with Rent Control	without Rent Control	with Rent Control	without Rent Control	with Rent Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1995	$352,851	$345,490	$352,911	$356,821	$249,556	$263,711	$252,097	$269,912
1997	$381,998	$364,048	$390,749	$367,390	$262,357	$237,813	$271,335	$267,418
1999	$523,499	$423,276	$529,506	$405,275	$330,063	$308,751	$342,813	$339,789
2001	$723,901	$621,829	$726,061	$589,848	$435,026	$392,303	$431,594	$410,036
2003	$766,554	$640,126	$772,463	$647,271	$442,007	$392,629	$443,956	$408,546

Notes: Estimates from specification of Table 4 columns (1) and (3) for houses and columns (4) and (6) for condominiums

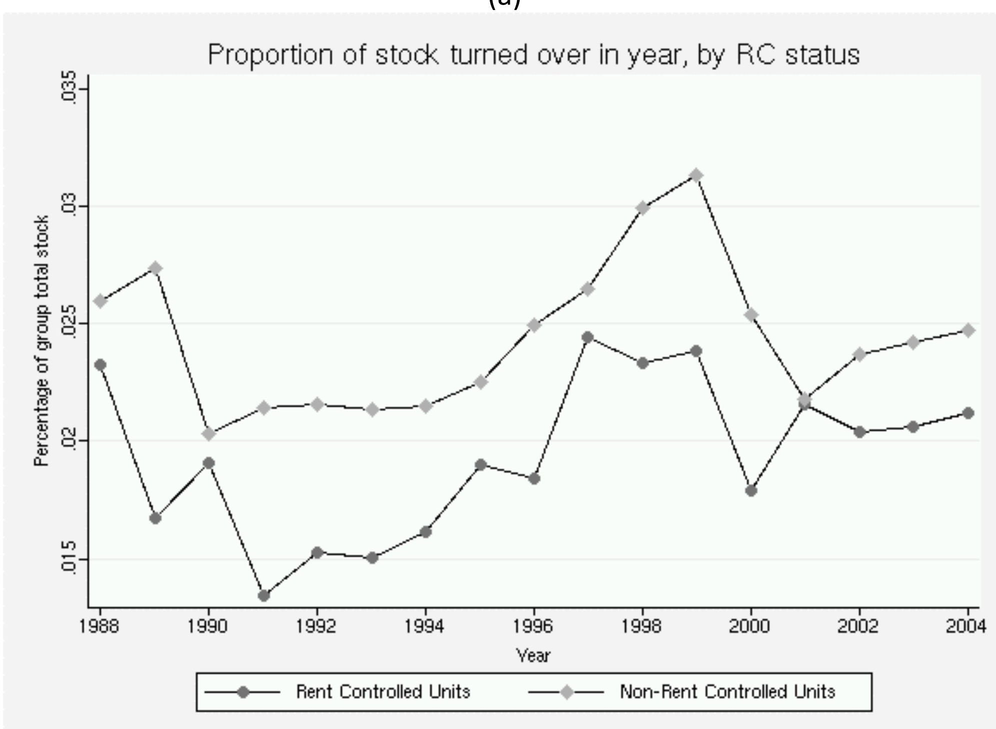
All prices are in 2008 $ dollars.

FE are block group fixed effects which consist of 92 blocks groups from 1990 Census

Trends are quadratic functions of sale year where function is allowed to vary for each census tract

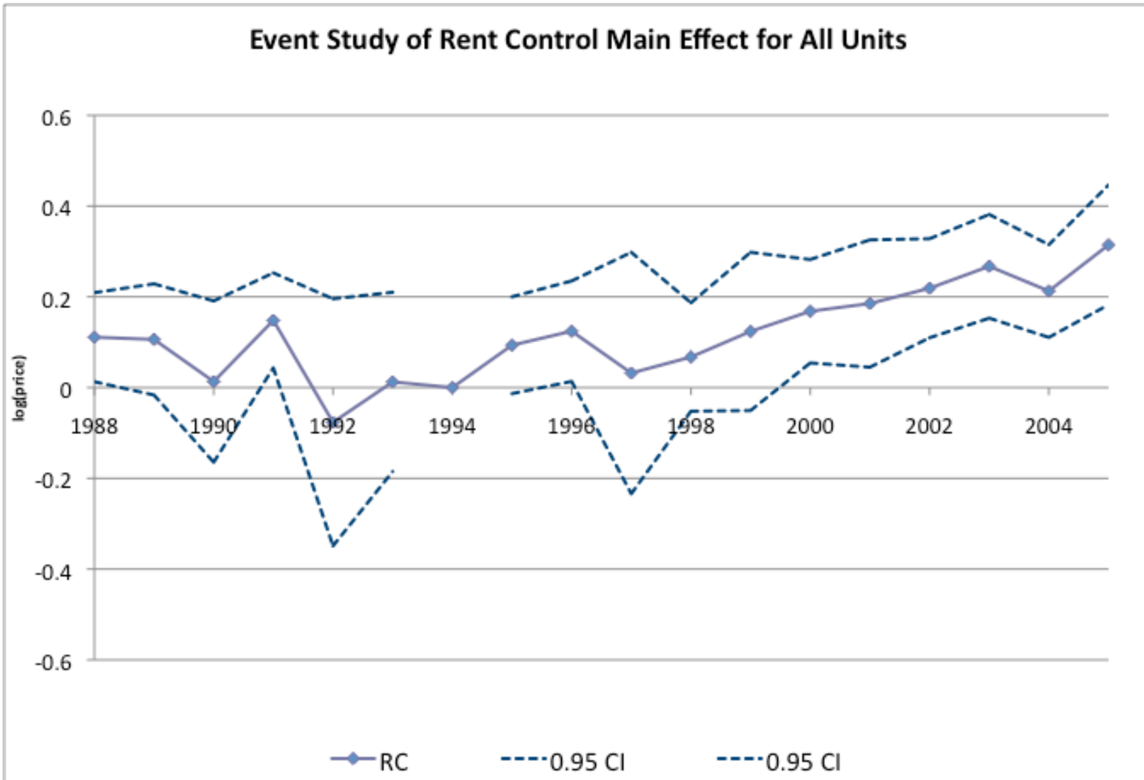
Figure 1: All Residential Structures in Cambridge
(Dark=Uncontrolled Housing, Light=Rent Controlled Housing)
The blue circles correspond to 0.1, 0.2 and 0.3 mile radii circles

(a)



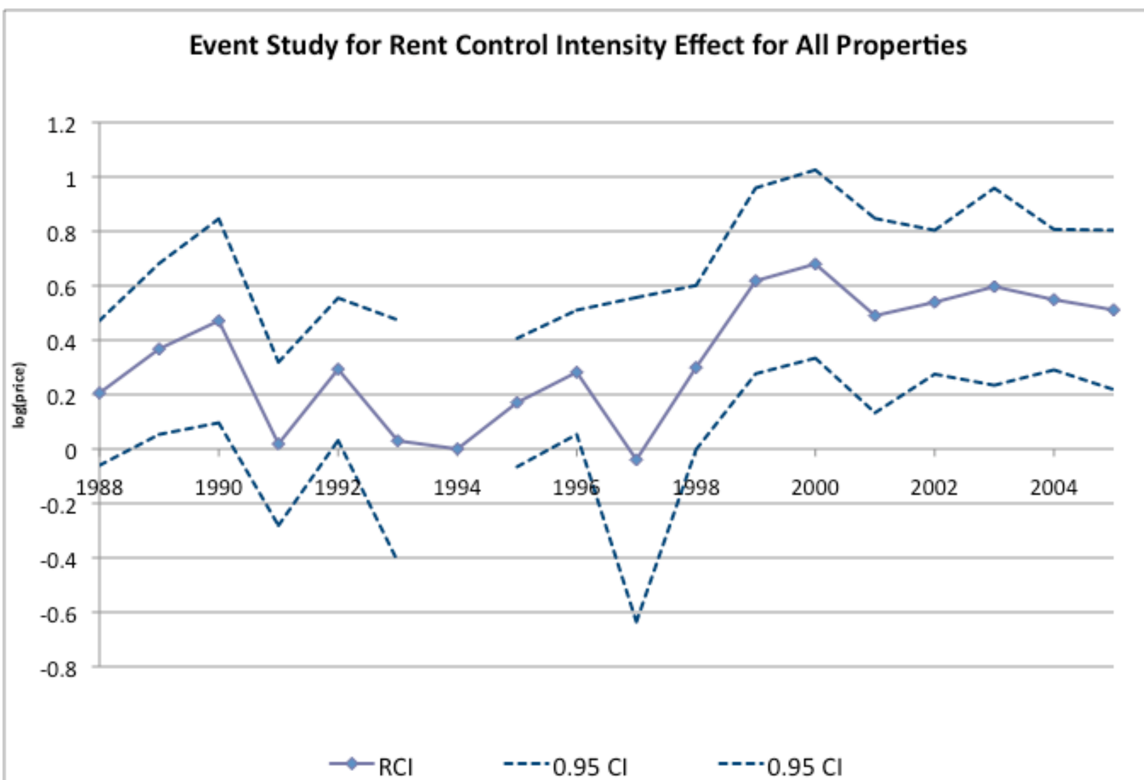
(b)

Figure 2(a): Percentage of Annual Sales Attributed to Rent Controlled Properties, (b) Proportion of Housing Stock Transacted for Uncontrolled and Controlled Properties



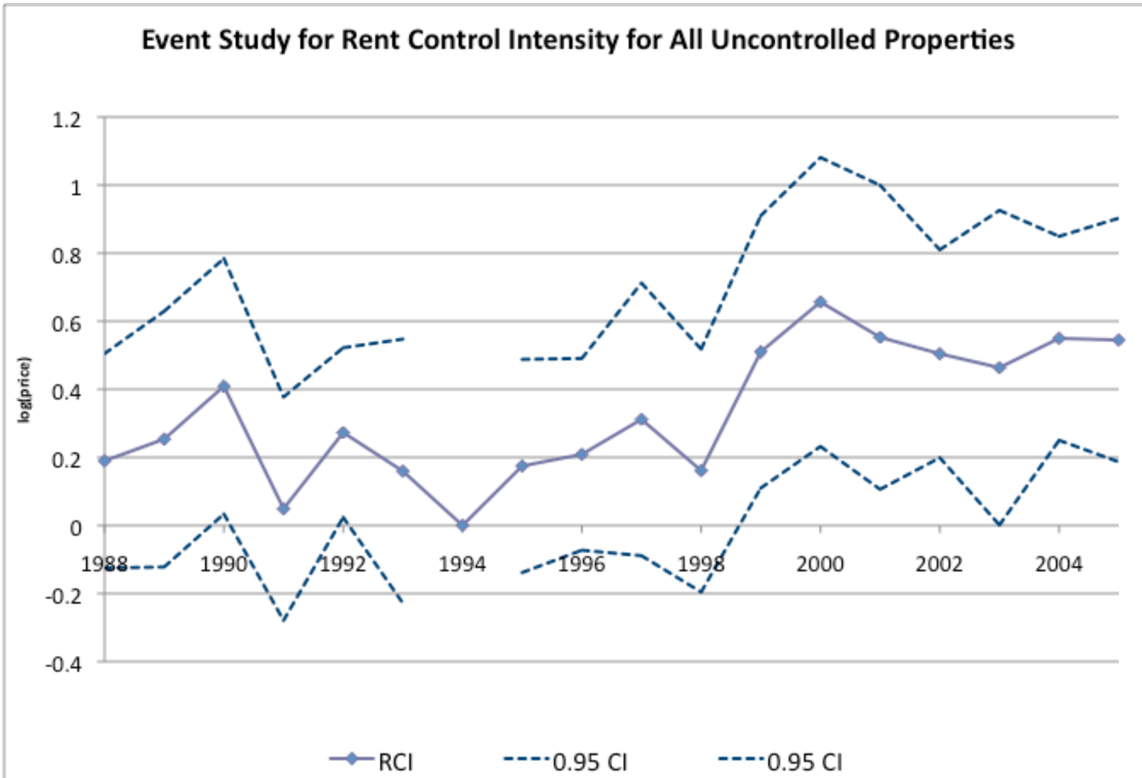
Event Study of Rent Control Main Effect for All Units
0.6
0.4
0.2
0
-0.2
-0.4
-0.6
log(price)
1988
1990
1992
1994
1996
1998
2000
2002
2004
RC
0.95 CI
0.95 CI

(a)

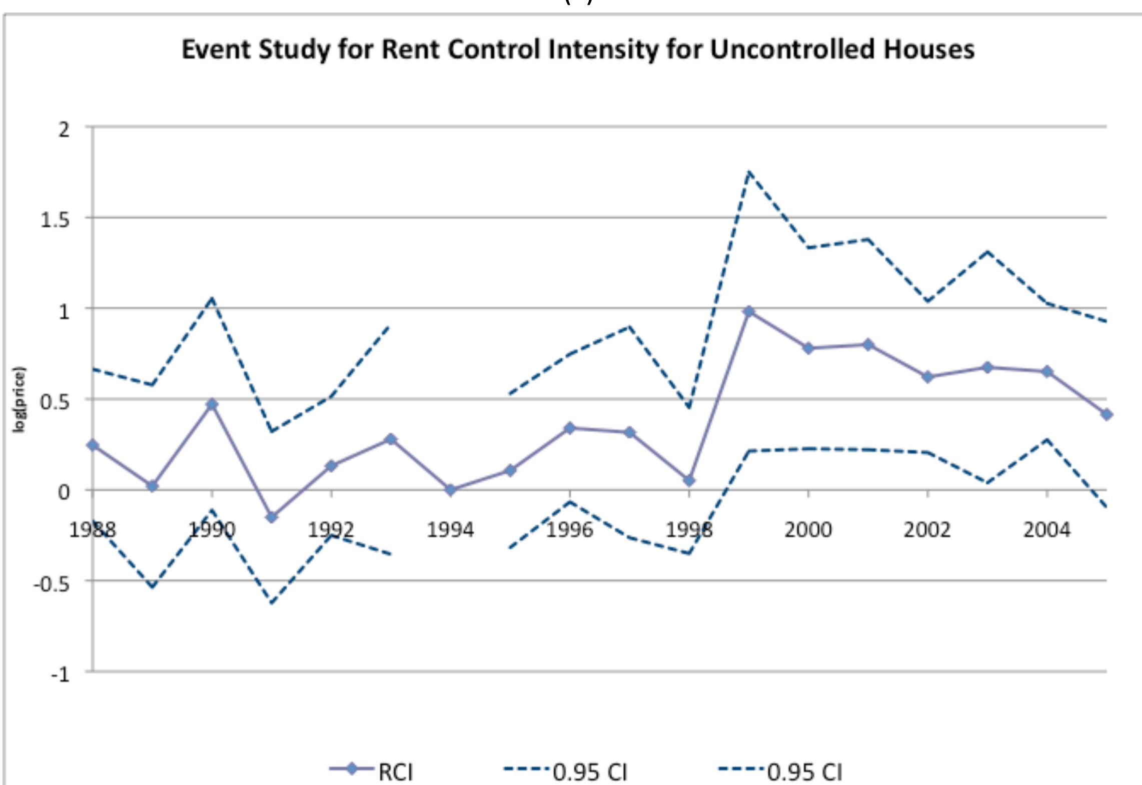


Event Study for Rent Control Intensity Effect for All Properties
1.2
1
0.8
0.6
0.4
0.2
0
-0.2
-0.4
-0.6
-0.8
log(price)
1988
1990
1992
1994
1996
1998
2000
2002
2004
RCI
0.95 CI
0.95 CI

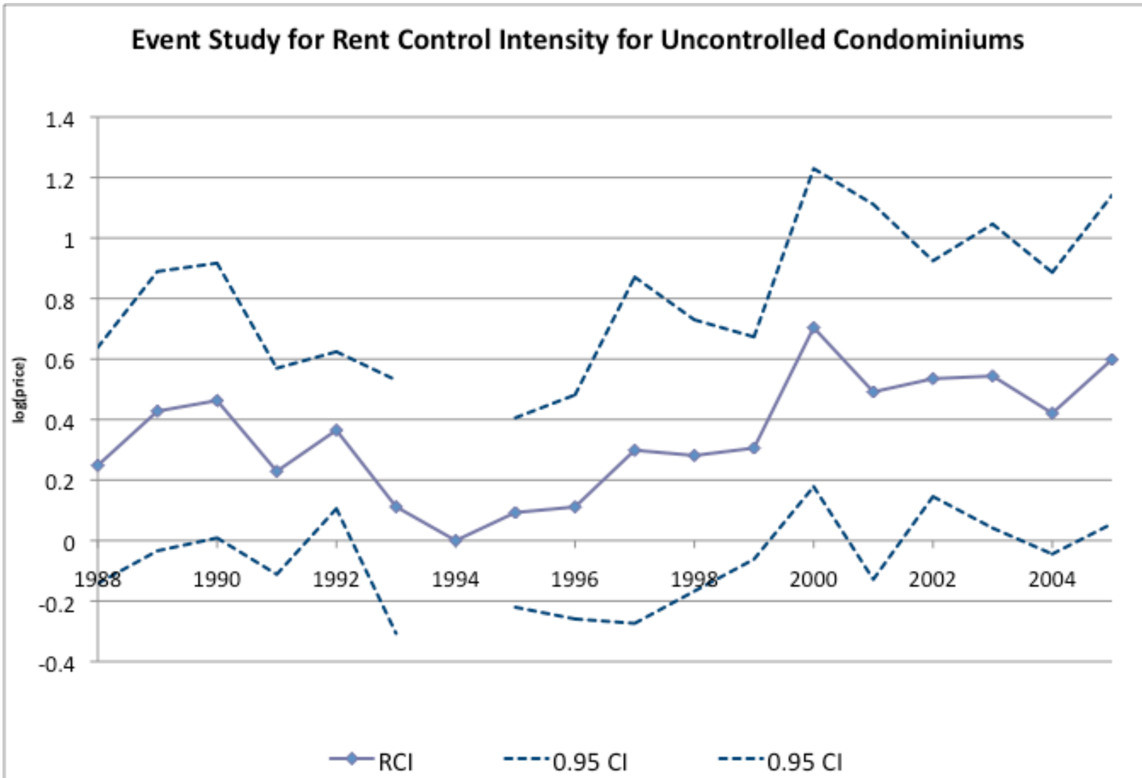
(b)


Event Study for Rent Control Intensity for All Uncontrolled Properties
1.2
1
0.8
0.6
0.4
0.2
0
-0.2
-0.4
log(price)
1988
1990
1992
1994
1996
1998
2000
2002
2004
RCI
0.95 CI
0.95 CI

(c)

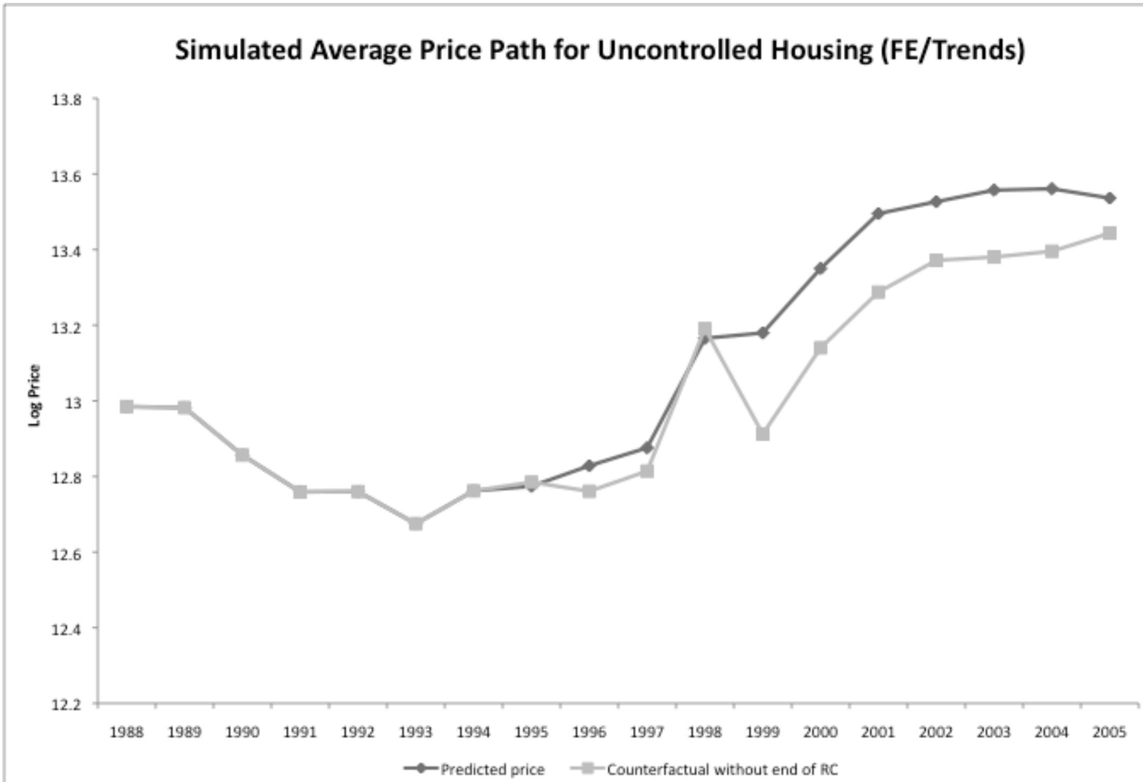

Event Study for Rent Control Intensity for Uncontrolled Houses
2
1.5
1
0.5
0
-0.5
-1
log(price)
1988
1990
1992
1994
1996
1998
2000
2002
2004
RCI
0.95 CI
0.95 CI

(d)



(e)

Figure 3(a): Year Interacted Coefficient of Rent Control for All Properties, (b) Year Interacted Coefficient of Rent Control Intensity for All Properties, (c) Year Interacted Coefficient of Rent Control Intensity for Uncontrolled Properties, (d) Year Interacted Coefficient of Rent Control Intensity for Uncontrolled Houses, (e) Year Interacted Coefficient of Rent Control Intensity for Uncontrolled Condominiums



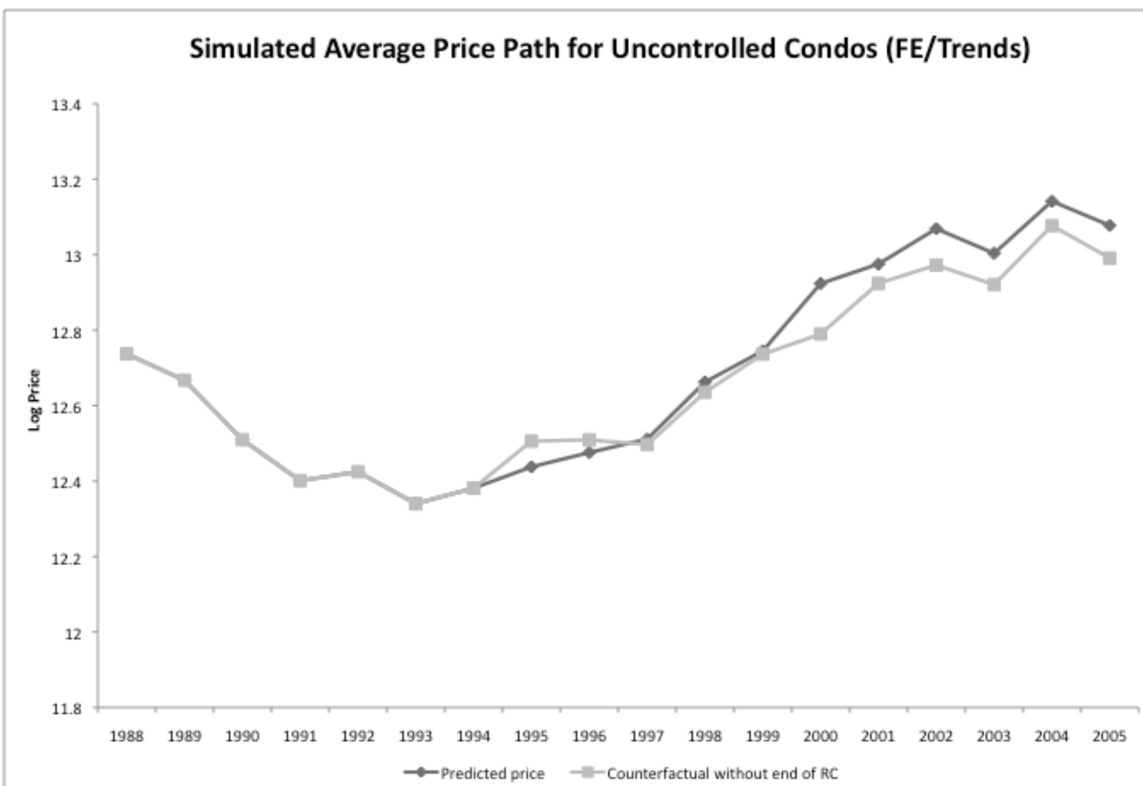


Figure 4: Simulated Average Log Price path for Uncontrolled Houses and Condominiums using point estimates in column (3) and (6) in Table 3 with fixed effects and trends.

Table A1. Descriptive Statistics - Census Geographies

	mean (1)	std dev (2)	min (3)	max (4)	median (5)
		I. Census Blocks			
Area (sq miles)	0.01	0.02	0.00	0.53	0.00
1990 Census Population	131.21	161.30	0	2833	94.50
Residential Units	59.92	57.78	1	453	43
Rent Control Units	23	35	0	261	11
Residential Structures	17	12	1	80	15
Rent Control Structures	3.87	3.73	0	21	3
N			626		
		II. Census Block Groups			
Area (sq miles)	0.05	0.07	0.01	0.56	0.04
1990 Census Population	892.80	500.69	0	3093	761.50
Residential Units	407.73	256.71	3.5	1481	356.75
Rent Control Units	153	155	0	854	107
Residential Structures	119	62	1	382	122
Rent Control Structures	26	17	0	61	24
N			92		
		III. Census Tracts			
Area (sq miles)	0.17	0.16	0.03	0.71	0.12
1990 Census Population	2737.93	937.19	811	5003	2622.50
Residential Units	1250.37	495.75	339.25	2875.1	1198.61
Rent Control Units	470	341	101	1,533	378
Residential Structures	364	150	115	862	339
Rent Control Structures	80.83	30.38	27	156	73
N			30		

Table A2. Descriptive Statistics - Rent Control Intensity

	mean	std dev	min	max	median
	(1)	(2)	(3)	(4)	(5)
			I. 1988-1994		
Unit					
Radius = 0.40 miles	0.38	0.13	0.00	0.60	0.41
Radius = 0.30	0.38	0.14	0.00	0.66	0.41
Radius = 0.20	0.36	0.17	0.00	0.78	0.38
Census block group	0.20	0.24	0.00	0.96	0.00
Radius = 0.10	0.35	0.20	0.00	1.12	0.34
Census block	0.20	0.28	0.00	1.54	0.00
N			4,860		
			II. 1995-2005		
Unit					
Radius = 0.40 miles	0.37	0.13	0.00	0.60	0.41
Radius = 0.30	0.37	0.14	0.00	0.66	0.40
Radius = 0.20	0.36	0.16	0.00	0.81	0.38
Census block group	0.20	0.24	0.00	0.96	0.00
Radius = 0.10	0.35	0.19	0.00	1.12	0.35
Census block	0.20	0.27	0.00	1.54	0.00
N			9,794		

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