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HOUSING MARKET SPILLOVERS:
EVIDENCE FROM THE END OF RENT CONTROL IN CAMBRIDGE MASSACHUSETTS

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ABSTRACT

Understanding potential spillovers from the attributes and actions of neighborhood residents onto the value of surrounding properties and neighborhoods is central to both the theory of urban economics and the development of efficient housing policy. This paper measures the capitalization of housing market spillovers by studying the sudden and largely unanticipated 1995 elimination of stringent rent controls in Cambridge, Massachusetts that had previously muted landlords' investment incentives and altered the assignment of residents to locations. Pooling administrative data on the assessed values of each residential property and the prices and characteristics of all residential transactions between 1988 and 2005, we find that rent control's removal produced large, positive, and robust spillovers onto the price of never-controlled housing from nearby decontrolled units. Elimination of rent control added about $1.8 billion to the value of Cambridge's housing stock between 1994 and 2004, equal to nearly a quarter of total Cambridge residential price appreciation in this period. Positive spillovers to never-controlled properties account for more half of the induced price appreciation. Residential investments can explain only a small fraction of the total.

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1 Introduction

Understanding potential spillovers from the attributes and actions of neighborhood residents onto the value of surrounding properties and neighborhoods is central to both the theory of urban economics and the development of efficient housing policy (Fujita, 1991; Glaeser and Gyourko, 2009). Credibly identifying and quantifying these spillovers, however, poses a significant empirical challenge because key features of the housing market equilibrium—in particular, who lives where, the quality and quantity of housing, the levels of local public goods and amenities, and what prices prevail—are simultaneously determined.

This paper exploits an unusual, large scale policy change in Cambridge, Massachusetts to quantify the capitalization of residential housing market spillovers onto the value of real estate, specifically, from the elimination of rent control in 1995. From December 1970 through 1994, all rental property in Cambridge built prior to 1969 was regulated by a far-reaching rent control ordinance that placed strict caps on rent increases and tightly restricted the removal of units from the rental stock. The legislative intent of the rent control ordinance was to provide affordable rental housing, and at the eve of rent control's elimination in 1994, controlled units typically rented at 25 to 40 percent below the price of nearby non-controlled properties—though maintenance and amenities in controlled units were typically sub-par (Sims, 2007).

The policy change that provides the identifying variation for our study is the swift elimination of Cambridge's rent control law via a *statewide* ballot initiative. In November 1994, the Massachusetts electorate passed a referendum to eliminate rent control by a narrow 51% to 49% margin, with nearly 60% of Cambridge residents voting to *retain* the rent control ordinance. The removal of rent controls took effect in January of 1995, only two months after the referendum.[1]

The decontrol of Cambridge's rental market offers a unique opportunity to measure spillovers in residential housing markets. In addition to its swift and largely unanticipated elimination, two features of Cambridge's rent control system make it well-suited for identifying housing market spillovers. First, Cambridge's rent control ordinance only applied to a fixed, non-expanding set of residential units—specifically, non-owner occupied rental houses, condominiums, or apartments built prior to 1969. By contrast, units built after 1968, older non-residential units converted to residential status, and owner-occupied units faced no threat of rent control.[2] The fact that controlled and non-controlled units stood side by side in Cambridge neighborhoods at the time that rent control was removed offers a tight temporal and geographic framework for assessing the impact of the law on residential property prices.

A second empirical virtue of Cambridge's rent control system is that there was substantial variation across neighborhoods in the share of units subject to rent controls. Although roughly a third of residential units were controlled prior to elimination (see Figure 1), this fraction frequently

[1] A last-minute legislative compromise, discussed below, allowed disabled, elderly, and low-income renters to retain their current units at controlled rents for up to two years.

[2] If an owner-occupied residential unit built before 1969 were put up for rent, it could be subject to rent control. Our informal understanding based on discussions with Cambridge homeowners of that era was that such rentals were rare and often arranged discreetly to avoid the notice of the Rent Control Board.

exceeded sixty percent in neighborhoods with older housing stocks and a substantial share of renters at the time of rent control's enactment in 1970. This cross-neighborhood variation in the fraction of units that were decontrolled allows us to assess localized price spillovers by comparing pre-post removal price appreciation among never-controlled properties in neighborhoods that differed in their 'Rent Control Intensity'—that is, the share of residential units that were controlled.

The hypothesized price spillovers studied here could arise through two main channels, investment and resident allocation (or sorting). On the first channel, it is likely that Cambridge's rent control law muted owners' incentives to maintain and improve controlled properties since the Rent Control Board was unlikely to grant rent increases following property improvements.[3] This may have reduced the desirability of the local neighborhoods in which these properties were located. Decontrol would have spurred long-deferred investments in formerly controlled units, with the potential for positive spillovers to nearby never-controlled units. Consistent with this mechanism, Sims (2007) presents evidence that chronic maintenance problems (such as holes in walls or floors, chipped or peeling paint, loose railings, etc.) were more prevalent in controlled than non-controlled units during the rent-control era and that this differential fell substantially with rent control's elimination.

Spillovers might also accrue through the sorting of individuals to housing units. Cambridge's rent control law was intended to enable relatively less affluent individuals to reside in units that would command high rents under a market allocation—most notably, the dense neighborhoods proximate to Cambridge's major universities, commercial centers, and transportation hubs. While there was no formal mechanism to allocate controlled units to low-income households, limited quantitative evidence indicates that less affluent residents and students were overrepresented in controlled units—though a significant number of units were also occupied by wealthy professionals.[4] If affluent residents prefer to locate near one another, a high concentration of rent controlled units in a neighborhood might dampen demand for nearby non-controlled units. More affluent tenants moving into newly decontrolled units could potentially raise demand among other affluent households to reside in nearby never-controlled units. If either the investment or allocation channel was operative, the end of rent control in Cambridge had the potential to spur differential appreciation of never-controlled units as induced improvement in local amenities capitalized into prices.

Regulations are widespread in housing markets and rent control is arguably among the most important regulations 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 (Fetter, 2011). Even though the prevalence of rent control as a housing market policy has decreased since this period, rent control and rent stabilization

[3]Leonard (1981) notes that the Board limited the allowable rate of return on investments at a "relatively low" level deemed "fair," which made improvements both comparatively unprofitable and difficult to finance. Rent Control Board records indicate that applications for rent adjustments were infrequent—once per decade for a typical unit.

[4]A 1998 study commission by the City of Cambridge found that sitting residents of formerly controlled units had mean annual earnings in 1997 of $35,650 versus $43,630 among tenants of market rate units and $41,340 among tenants of formerly controlled units who had taken residence after rent control removal (Atlantic Marketing Research, 1998). Sims (2007) calculates that 67 percent of residents of rent controlled units in Boston, Brookline, and Cambridge were in the bottom two quartiles of the income distribution. At the same time, blacks were substantially underrepresented in controlled units.

plans are still in place in many U.S. and European cities (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 remains a topic of active debate among affordable housing advocates.

The early empirical literature on rent control focuses on its 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). A second strand of this literature examines how below-market rents may encourage individuals to spend effort to obtain cheap housing and how this may lead to a misallocation of housing (Suen, 1989; Glaeser and Luttmer, 2003; Sims, 2011). Fallis and Smith (1984) examine how the impact of rent control on the uncontrolled sector depends on the allocation mechanism in the controlled sector. Wang (2011) investigates the impact of privatization of housing that was owned and allocated by the state in urban China. Her analysis, like ours, shows that the degree of misallocation of assets prior to privatization impacts the expected change in prices.

Sims (2007) undertakes the first empirical analysis of the end of rent control in Massachusetts, focusing on its impacts on the supply of rental properties and their rental prices. Sims produces compelling evidence that the elimination of rent control caused substantial increases in rents in Massachusetts towns (Boston, Brookline and Cambridge) that had binding rent control laws in 1994, and led to significant increases in the quality and quantity of rental housing available. Distinct from Sims' work, we analyze rent control's effect on the market value (rather than rental prices) of the entire residential housing stock (not simply rental units) in Cambridge and distinguish its direct effects on the value of decontrolled properties from the spillover effects onto never-controlled properties.[5]

This paper is also related to studies of neighborhood revitalization and gentrification, both of which may generate spillover benefits to surrounding areas (Hurst, Guerrieri and Hartley, 2011; Ioannides, 2003; Rossi-Hansberg, Sarte and Owens, 2010; Schwartz, Ellen, Voicu and Schill, 2005). Studies by Linden and Rockoff (2008) and Pope (2008) of the housing market impacts of the arrival of registered sex offenders into a neighborhood consider allocative externalities in residential housing. Recent interest in measuring external effects in housing has been spurred in part by historically high levels of foreclosures and the concern for their impact on immediate neighbors and neighborhoods (Campbell, Giglio and Pathak, 2011; Hartley, 2010; Mian, Sufi and Trebbi, 2011).[6]

Price controls are not unique to housing markets. A large literature examines the effects of price controls in labor markets (Holzer, Katz and Krueger, 1991; Card and Krueger, 1995) and energy markets (Frech and Lee, 1987; Davis and Killian, 2011). Relative to this literature, our main interest is on a relatively under-explored consequence of price controls: the external effects on the non-controlled sector, which seem likely to play an especially important role in the housing market.

Our analysis draws on a uniquely detailed geographic and economic database sourced from

[5]Sims (2007) further explores spillovers from decontrol onto the rental price of never-controlled units, but his data do not allow sufficient precision to draw firm conclusions.

[6]In addition, a number of papers present evidence that subprime mortgage lending leads to price appreciation in neighborhoods where housing credit was historically in short supply (Mian and Sufi, 2009; Landvoigt, Piazzesi and Schneider, 2011).

Cambridge administrative records that enumerate the exact location of all rent controlled units, the assessed value of each house and condominium in 1994 and 2004, the transacted price of each residential property sold between 1988 and 2005, the movement of properties across various residential and non-residential uses (e.g., houses that were converted to condominiums), and the permitted investment expenditures at each residential location. We also use 10 years of Cambridge City Census data to document the rapid turnover of residents of formerly controlled units following the end of rent control. These sources permit direct estimation of the change in residential real estate prices induced by rent decontrol, distinguishing between direct effects on the value of decontrolled properties and the indirect (spillover) effects onto never-controlled properties nearby.

We find compelling evidence that the elimination of rent control generated positive spillovers onto the assessed values of never-controlled properties. Using the fraction of all residential units within a 0.20 mile radius that were subject to rent control as of 1994 as a measure of of rent control exposure, we estimate that houses at the mean level of rent control exposure of 34 percent appreciated by roughly 12 percent more than houses with no nearby controlled neighbors; houses at one standard deviation above the mean level of exposure appreciated in value by an additional 6 percent. While estimated spillovers are large, precise, and robust for houses, the spillover results are not as consistent for condominiums. One reason for this difference may be that the decade following the end of rent control saw a 32 percent increase in the number of condominiums in the Cambridge housing stock—driven in large part by the conversion of apartments and houses to condominiums—a potentially mitigating supply shock.

Our statistical analysis also indicates that rent controlled properties were valued at a discount of about 50 percent relative to never-controlled properties with comparable characteristics in the same neighborhoods during the rent control era, and that the assessed values of these properties increased by approximately 18 to 25 percent after rent control ended. The appreciation of decontrolled properties likely reflects a mixture of increased rental revenue (which would capitalize into valuations), additional maintenance and improvements made at these locations, and potentially positive spillovers from improved maintenance and changes in resident composition at nearby units.

Our key findings are robust to numerous alternative measures of rent control intensity, to rich controls for property-level characteristics (such as age, lot size, and number of bedrooms and bathrooms), and to the inclusion of detailed geographic fixed effects and neighborhood trends that allow price levels to vary across Cambridge neighborhoods and to trend over time within them. Data on transactions prices for all properties sold in Cambridge between 1988-2005 provide an alternative data source for measuring changes in market values. These data yield comparable estimates of spillover effects to those found using the assessor's data. We further explore the possibility that the rising market value of never-controlled properties in Cambridge after 1994 reflects a general increase in demand for urban residential locations rather than a Cambridge-specific phenomenon per se. Using data on residential transaction prices from 1988 through 2005 for the nearby cities of Somerville, Malden, and Medford, we find no consistent evidence of similar differential rises in property prices in comparison neighborhoods. It bears emphasis that the elimination of Cambridge rent control occurred well before the changes in the availability of home financing associated with sub-prime lending commencing in 2001 and the subsequent growth of mortgage credit in 2002-2005 documented by Mian and Sufi (2009). As an additional check on this concern, we verify that our results are unaffected by eliminating the subset of residential transactions in which the mortgage holder is known to have issued subprime loans.

To unpack the channels through which the removal of rent controls capitalized into Cambridge housing values, we analyze administrative data on residential expenditures permitted by the Cambridge Inspectional Services for significant investments or modifications made to housing. Aggregate annual real permitted building expenditures increased dramatically for both houses and condominiums after 1994, rising from \$21 million per year between 1991-1994 to \$45 million per year between 1995 and 2004. We find some evidence that the incidence of permitting—though not investment expenditures per unit—rose differentially at formerly controlled properties in the years immediately following rent control removal. But the *total* value of Cambridge residential investments in these 10 years was less than one quarter as large as the estimated *increment* of \$1.8 billion to Cambridge residential housing values induced by rent control removal, suggesting that the allocative rather than the investment channel is the more important explanation for the post-1994 rise in the market value of never-controlled properties.

The economic magnitude of the effect of rent control removal on the value of Cambridge's housing stock is \$1.8 billion. We calculate that positive spillovers from decontrol added \$1.0 billion to the value of the never-controlled housing stock in Cambridge, equal to 10 percent of its total value and one-sixth of its appreciation between 1994 and 2004. Notably, *direct* effects on decontrolled properties are smaller than the spillovers. We estimate that rent control removal raised the value of decontrolled properties by \$770 million, which is 25 percent less than the spillover effect. Since price gains at decontrolled properties are largely a transfer from renters to owners while price changes at never-controlled properties should reflect efficiency gains–that is, increased consumers valuation of these locations–our findings imply that the efficiency costs of Cambridge's rent control policy were large relative to the size of the transfers made to residents of controlled units. Noting, however, that the majority of Cambridge resident voted to retain rent control in 1994, a revealed preference argument implies that the median Cambridge voter perceived the benefits of rent control to exceed the costs.

The paper proceeds as follows. Section 2 provides additional detail on the enactment, enforcement, and removal of rent control in Cambridge. Section 3 describes a simple model of housing markets in the presence of rent control to guide our empirical analysis (the Theory Appendix contains the model). Section 4 describes data sources and empirical strategy. Section 5 presents our main results using property assessments, while Section 6 presents results using transaction prices. Section 7 reports on our investigation of permitting and investment activity and Section 8 describes economic magnitudes. We conclude with a discussion of areas for further investigation.

2 Cambridge Rent Control: Enactment, Enforcement and Removal

The city of Cambridge, which sits on the north side of the Charles River opposite the City of Boston, has a population of just over 100,000, 70 percent of whom are ages 20-plus and 9 percent of whom are age 65-plus according to the 2000 US Census. In that year, Whites accounted for 68 percent of the population and Blacks and Asians comprised the two next largest racial groups at 12 percent each. The city's two major research universities, Harvard and MIT, are its largest employers. The city also has a high concentration of biotechnology companies and healthcare providers. The median family income was $47,900 in 2000 and 13 percent of residents were below the poverty line.

2.1 Rent control adoption and elimination

In 1970, the Massachusetts state legislature enacted a statue allowing cities and towns with populations over 50,000 to implement rent control to "alleviate the severe shortage of rental housing [...] which shortage has caused a serious emergency detrimental to the public peace, health, safety and convenience" (*An Act Enabling Certain Cities and Towns to Control Rents and Evictions*, 1970). Boston, Brookline, Cambridge, Lynn, and Somerville each adopted a rent control plan, with Cambridge moving first in 1970 and keeping the ordinance longer than any other city.[7] Lynn repealed its plan in 1974 and Somerville in 1979. Boston allowed for decontrol of vacant units in 1976 and Brookline began to phase out its system prior to the statewide repeal, though both cities still had a significant number of controlled units in 1994 (Cantor, 1995). In Cambridge, rent control was seen as an integral part of the city's affordable housing program.

Cambridge's initial rent control policy adopted in 1970 applied to all non-owner-occupied rental housing built before 1969. It did not apply to structures built after January 1, 1969, to owner-occupied condominiums, or to non-residential structures converted to rental properties after this time. Oversight of the rent control law rested with the Cambridge Rent Control Board, whose charter, nominally, was to ensure that landlords obtained a fair net operating income. The Board established maximum allowable rents for each controlled property with the aim of fixing landlord net operating income at inflation-adjusted 1967 levels. In the 1970s and 80s, the Board authorized a series of across-the-board rent increases ranging from 1.15 to 3.1 percent, intended to cover increases in heating costs, operating costs, and property taxes (*Rent Control Board*, 1982). Landlords could also apply to raise prices beyond the levels granted by these scheduler increases—to recoup the cost of documented capital improvements, for example. Variances from scheduler increases were rare, however, in part because these applications required supporting petitions, extensive legal documentation, and substantial time investments.[8]

Distinct from other cities, Cambridge's rent control policy did not allow for so-called "vacancy decontrol," whereby controlled rental units were returned to market-rate rents after protected tenants

[7]See Epple (1988) for a game-theoretic model of communities' decisions to adopt rent control.

[8]A legendary incident involves Harvard Philosophy Professor Robert Nozick extracting a settlement of over $30,000 in the 1980s from his landlord, famed classicist and novelist Eric Segal, for overcharging rent, described in Tucker (1986).

moved out. This feature of Cambridge's rent control law created an incentive for landlords to remove units from the rental stock, and landlords responded by converting substantial numbers of units in older buildings into condominiums and selling them to owner-occupants. To prevent the controlled rental stock from being depleted, the city council passed in 1979 the "Removal Permit Ordinance" despite the strenuous objections of landlords. This ordinance substantially restricted the removal of controlled units from the rental stock and complicated the conversion of controlled units into owner-occupied condominiums.[9]

Throughout this period, rent control was a contentious issue in the political debate in Cambridge. Repeated efforts by the Small Property Owners Association (SPOA) to roll back rent controls were rebuffed by civic organizations, the Cambridge City Council, and the state courts. The initiative that lead to rent control's defeat was SPOA's successful effort to place rent control on the state-wide ballot in 1994, thus diluting the strong support that rent control enjoyed in Cambridge. Rent control was eliminated by an extremely slim 51 to 49 percent margin in the 1994 state-wide election, despite nearly 60 percent of Boston, Brookline, and Cambridge voters voting to retain the current regime.

Following rent control's defeat, a majority of properties were decontrolled a mere two months later, in January of 1995. A last-minute legislative compromise, however, allowed disabled, elderly, and low-income renters to retain their current units at their controlled rents for up to two years. Though only a small share of residents received rent control extensions, this compromise likely created some uncertainty about whether decontrol was final—at least until the grandfathering period expired in 1997 with no further controls in place.[10]

2.2 The post decontrol regime

The elimination of rent control catalyzed a series of rapid changes in the Cambridge rental market. Prior to decontrol, it was widely perceived that rents at controlled units were significantly below market rate, and the data support this view. Using microdata from a 1987 Abt Associates study (Finkel and Wallace, 1987) commissioned by the City of Cambridge, we estimate that quality-adjusted rents were approximately 44 percent lower at controlled units than at observably similar non-controlled units.[11]

[9] 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). The ordinance was subsequently amended following difficulties with enforcement, as in the case of the so-called "condo martyrs" who were prosecuted for occupying their own controlled properties before the completion of a conversion.

[10] Shortly after the referendum, the state legislature adopted a bill extending rent control for five years. The Governor vetoed this bill and later signed an alternative on January 3, 1995 that granted rent control extensions of one year (two years if the rental building had more than 12 units) to renters whose incomes were below 60% of the median for the Boston MSA (or 80% of the MSA median for disabled and elderly renters). Sims (2007) reports that about 3,000 of approximately 21,000 tenants applied for exemptions while Haveman (1998) reports that 9.4% of tenants were eligible to apply.

[11] We regressed the log of contract rent on rent-control status, tenant awareness of rent-control status, unit characteristics (bedrooms, bathrooms, total rooms, an indicator for elevator in building, and indicators for whether furnishings, heat, electricity, or water are included in the rent), zip-code main effects, and dummy variables indicating units that were reported to be in either poor or excellent condition. Notably, the fraction of controlled and non-controlled units reported in poor (excellent) condition was 9.5 (12.5) percent and 4.2 (24.4) percent, respectively. When models were fit separately for each of the four Cambridge zip codes (the finest level of geographic identification provided in the

Rents rose dramatically following decontrol. Using data from the American Housing Survey, Sims (2007) estimates an average rent increase of $84 between 1993 and 1998 (in 1998 dollars) in Massachusetts towns where rent controls were eliminated by the 1994 ballot initiative—an increase of 21 percent over the average (real) monthly rent of $398 in these towns prevailing during 1985 through 1998.[12] A 1998 Atlantic Marketing Survey commissioned by the City of Cambridge, which provides data specific to Cambridge residents, found that nominal Cambridge median rents rose by 40 percent between 1994 and 1997 for tenants of formerly controlled units who either remained at these units or moved to other non-controlled units. Median rents rose by only 13 percent for sitting tenants of never-controlled units in the same time period.

Consistent with the rapid increase in rents at decontrolled units, we document a sharp differential rise in resident turnover at formerly controlled units after 1994. We measure resident turnover by constructing an annual panel of all Cambridge adults ages 17-plus by street and address using city voter registration records for the years 1991 through 2000.[13] In this 10 year interval, 26.9 percent of Cambridge residents turned over–that is, changed locations–with the highest turnover rates found among apartment residents (33.5 percent), followed by residents of condominiums (29.7 percent) and houses (23.2 percent). To explore whether turnover rates at rent control units differentially increased after 1994, we estimate linear probability models of the following form:

$$\text{NEW}_{ijt} = \gamma_g + \delta_t + \lambda_1 \text{RC}_j + \lambda_2 \text{RC}_j \cdot \text{Post}_t + \epsilon_{ijt}, \tag{1}$$

where NEW_{ijt} is an indicator equal to one if resident i in unit j in year t was not present in that unit in the prior year. In this model, RC_j is an indicator equal to one if unit j was rent controlled in 1994, γ_g is a vector of 1990 Census block group dummies, δ_t is a vector of year dummies, and Post_t is an indicator for years 1995 onwards.

Estimates of this model in Table 1 show that prior to the elimination of rent control, residents of controlled units were not significantly more likely to turnover from year to year than residents of non-controlled units.[14] Following rent control removal, the probability of resident turnover rose by 5.4 percentage points at formerly controlled units relative to never-controlled units, with an even

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survey), we estimate a rent control discount of 51, 42, 41, and 35 percent respectively in zip codes 02138, 02139, 02140, and 02141. Further details are presented in the Data Appendix, and tables of estimates are available from the authors.

[12] This number likely underestimates the increase in rents at controlled units in Cambridge due to the fact that the American Housing Survey data do not allow Sims to distinguish never-controlled from decontrolled units in the post-control era. Hence, his estimates contrast changes in rents at all units in Massachusetts, comparing towns with and without rent controls. Since only a subset of Cambridge rental units was controlled, the average rent increase in Cambridge will understate the average rent increase at decontrolled units.

[13] State law (M.G. L. ch. 51.4) requires an annual listing of all adult residents for voter registration, regardless of voter status, including name, street address, gender, date of birth, occupation, and nationality. City Census books from 1991-2000 were double-entry hand keyed and assembled into a panel using name and address matching, as described in the Data Appendix.

[14] Subsequent columns reveal that this result is driven by composition. Focusing only on apartments and condominiums, residents of controlled units were significantly less likely to turn over than residents of non-controlled units—consistent with the idea that controlled units had scarcity value. Residents of controlled houses, by contrast, were significantly more likely to turnover than residents of non-controlled houses, but this likely reflects the fact that most non-controlled houses were owner-occupied whereas controlled houses were renter-occupied.

larger increase when looking only at condominiums. Figure 2 depicts the evolution of this turnover differential using a variant of equation (1) in which the rent-control indicator is interacted with a set of eight year dummies (1994 is the omitted category). The relative turnover rate at decontrolled units spiked by 4 percentage points in the year of rent control removal and continued to climb to 10 percentage points over the next three years, where it remained through the year 2000. The fact that turnover at formerly controlled units continued rising for four years after decontrol suggests that the process of resident reallocation and neighborhood change spurred by decontrol took multiple years to unfold.

Accompanying the increase in rents and the spike in resident turnover at decontrolled units, Cambridge experienced a sharp increase in residential property investments, as we show below. The number of building permits issued per residential unit for improvements and new construction increased by approximately 20 percent after 1994, while annual permitted expenditures roughly doubled in real terms. Additionally, elimination of the "Removal Permit Ordinance" allowed a substantial number of decontrolled houses, apartments, and non-residential units to be converted to condominiums in the ensuing years, thus boosting the supply of condominiums in the residential housing stock. Finally, as demonstrated by Sims (2007), the fraction of units that were made available as rental properties *increased* by nearly six percentage points after rent control removal, reflecting the discouragement effect of the rent control law on the incentive to supply rental housing.

This combination of sharp rent increases, rapid turnover of incumbent renters, rising residential investment, and outward shifts in the supply of both condominiums and rental properties were likely in net to have changed the quality of the Cambridge residential housing stock, the allocation of residents to neighborhoods, and the availability of residential units for both rent and sale. Our subsequent empirical analysis quantifies the capitalized value of these decontrol-induced changes in the Cambridge housing market, focusing in particular on the spillover impacts onto never-controlled housing.

3 The Direct and Indirect Price Effects of Rent Control

The Theory Appendix presents a stylized spatial equilibrium model of the housing market that considers the relationship between rent control and prices of both controlled and non-controlled properties. In the model, summarized here, a city consists of N neighborhoods with a continuum of locations in each neighborhood. A continuum of potential residents chooses locations to maximize utility defined over consumption of housing services, a non-housing composite good, and local amenities. Residents differ only in their taste for consumption of housing services relative to the non-housing composite good. Profit-maximizing landlords choose the level of maintenance at each location, and this level is increasing in residents' tastes for housing services.

We assume that amenities in a neighborhood depend on the level of maintenance of neighborhood housing and the types of residents in the neighborhood, where types with higher taste for housing are also more desirable neighbors and hence contribute more to neighborhood amenities. This formulation creates a positive feedback from residents' taste for housing, the extent of house maintenance, and the production of amenities in a neighborhood. In the free market equilibrium (absent rent controls), rents are higher in neighborhoods with greater amenities due to higher maintenance and neighbors who spend more of their income on housing (that is, they are higher types).[15]

We consider the imposition of rent controls at the initial free market equilibrium by assuming that a rent control authority caps the rent of some fraction of units in a neighborhood at below their free market level. Maintenance levels and hence housing services fall at controlled units since landlords choose maintenance levels facing a regulated price. The combination of reduced rents and lower maintenance will have one of two effects on incumbent residents: either they will be sufficiently compensated by reduced rents so that they remain at their current locations, although the bundle of maintenance and amenities is not optimized for their tastes; or alternatively, they will choose to relocate to areas with higher amenities and higher rents. In the latter case, they will be replaced by residents who prefer lower housing services, i.e., lower types.[16] The average taste for housing services at controlled locations will therefore weakly decline following the imposition of rent control.

Since neighborhood amenities are a function of the maintenance of *all* units in a neighborhood and the preferences of their residents for housing services, the supply of amenities at *non-controlled* locations in these neighborhoods–as well as maintenance and rents–are also impaired by rent control. This in turn causes lower types to move into non-controlled locations. Hence, imposition of rent control causes inefficiently low maintenance and misallocation of residents at both controlled *and* non-controlled locations within a neighborhood.

Decontrol unwinds these effects. Prices rise due directly to the lifting of the cap, and indirectly due to improved maintenance and increased production of local amenities throughout the neighborhood. At *non-controlled* locations, the price increase will be greater in neighborhoods where a larger fraction of locations were controlled, where the capped price ceiling was set further below the market price level, and where controls induced a larger misallocation of resident types relative to the free market setting. The lifting of controls allows an additional, direct price increase at formerly controlled locations.

The model also offers a simple welfare interpretation of any direct and indirect price effects of rent decontrol. Price increases at *decontrolled* locations reflect three forces: a mechanical 'uncapping' effect, which reflects a transfer from renters to owners; a price increase reflecting improved maintenance, which generates increased landlord surplus net of the resource cost of maintenance; and a price increase reflecting greater neighborhood amenities due to improvements in maintenance and changes in resident types nearby. While the latter two effects reflect economic gains, the first does not. The price increase at decontrolled locations is therefore likely to substantially exceed the economic gains from decontrol at these locations.

Induced price increases at *non-controlled* locations following decontrol reflect only two of these

[15] The equilibrating force in the model is the convex cost function for production of housing services, which ensures that maintenance, production of local amenities and rents do not rise without bound.

[16] If the incumbent renter is dissatisfied with the new price-services pair, this pair can only be preferred by a lower type.

three forces: real resource costs for improved maintenance (or, more generally, housing investments) and gains in social surplus from improved neighborhood amenities (both maintenance and sorting). The increase in prices at non-controlled locations net of the additional resource cost therefore reflects the gain in social surplus stemming from positive external effects of decontrol–that is, spillovers. We attempt to quantify these spillovers below. Though we cannot directly measure the gains in social surplus from spillovers, we can infer them by estimating the induced rise in prices at non-controlled locations and netting out the value of investments at these locations (measured using Cambridge building permits data).

4 Data Sources

4.1 The geographical database

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 real estate from the 1995 Cambridge Assessor's File, from which we construct the residential housing structures file.[17] Each record includes the map-lot identifier, address, owner's name and address, usage, and property tax assessment as of January 1994. Usage categories are designated as commercial or residential, and residential categories are further subdivided into condominiums, single-family, two-family, and three-family houses, multi-unit apartment complexes, and mixed residential-commercial structures. In calculating rent control intensity below, we treat any usage code where individuals are likely to live as a residential structure. Our analysis of assessed values and transactions is limited to houses and condominiums, which comprise the market for residential real estate.

We identify rent controlled properties from historical records of the Cambridge Rent Control Board obtained via a Freedom of Information Act (FOIA) request.[18] We merge rent control structures to the Assessor's file using the map-lot identifier and address information coded in the Rent Control Board file. Rent controlled records that could not be matched via map-lot identifiers were hand-matched to the corresponding street address. Due to limitations of the Rent Control Board data, it was often not possible to determine which specific units in a multi-unit building were controlled. This creates a potential econometric pitfall: if we were to inadvertently code some controlled units as never-controlled, we could erroneously detect spillovers in our data analysis that reflect nothing more than appreciation of formerly controlled units after decontrol. To obviate this source of error, we code *all* units on a map-lot as rent controlled if *any* unit at that map-lot was controlled in 1994. This ensures that all coding errors are uni-directional: it is very unlikely that there are any controlled units that we fail to capture. Conversely, when measuring the rent control *intensity* of a given geographic area, we calculate the fraction of residential units—rather than structures—that

[17] This database was constructed by double-entry hand-keying the four bound volumes of the 1995 Cambridge Assessor's Commitment Books, which were provided to us by the Cambridge Historical Commission.

[18] While we filed our own FOIA request, we utilize the file obtained by David Sims since its coverage appears more complete.

are rent controlled.[19] This is also conservative in that it prevents us from overestimating units' exposure to other controlled properties.

Figure 1 illustrates the prevalence of rent control in Cambridge, with dark circles indicating controlled properties. In 1994, 22 percent of all residential structures and 38 percent of residential units were subject to rent control. The dense neighborhoods close to the two major universities and proximate to the subway that bisects Cambridge from east to northwest contain high concentrations of renters and numerous multi-unit structures and thus had relatively high rent control intensity. The largely owner-occupied area of Southwestern Cambridge features a higher fraction of single unit houses and hence had relatively low rent control intensity. It bears emphasis that our statistical analysis abstracts from these gross geographic differences in rent control intensity by comparing changes in residential prices among properties that differ in their proximity to controlled units but lie within relatively small neighborhoods.

To analyze the impact of rent decontrol on market capitalization, we append two additional databases that contain information on property values. The first is the 2005 Cambridge Assessor's File, available in electronic form, which contains property valuations from 2004. In combination with the 1995 Assessor's File (with 1994 values), this database allows us to observe the *assessed* appreciation of each extant property from the year prior to rent decontrol to nine years thereafter. The second dataset is based on residential sales, which come from a commercial database provided by the Warren Group enumerating all changes in ownership of residential properties for the years 1988 through 2005. Sourced from records of deeds, these data record for each real estate transaction the sale price, address, map-lot, number of bedrooms and bathrooms, lot size, year built, and property type. As described in the Data Appendix, we cleaned these data to eliminate transactions that appear to reflect either intra-family transfers of ownership rather than arms-length transactions or duplicate transactions due to intermediation or corrections of public records. We exclude commercial properties such as apartment buildings from the analysis because such sales are rare and transact at heterogeneous prices that are in some cases extremely high.

These two data sources—assessments and transactions—provide complementary means to measure the capitalization of rent control's end. While the assessor's panel offers information on the estimated value of residential properties, the valuations may offer a lagging indication of residents' changing willingness to pay for locations and might differ from market valuations due to discretionary aspects of the assessment process. In addition, our ability to control for underlying housing market trends is limited with these data because we only have access to valuations for one year prior to and nine years after the end of rent control. In contrast, the sales data include both market prices and a rich set of property characteristics for locations where transactions take place (we subsequently analyze whether rent control impacted the composition of transacted properties). Because we have access to the sales dataset from 1988-2005 and from neighboring cities, we can more finely control for neighborhood-level house price trends within Cambridge and also compare house price appreciation in Cambridge to other nearby cities.

[19] Our data always allow us to calculate the share of units in a building that are controlled, though we often cannot determine which specific units these are.

Table 2 presents descriptive statistics for the assessed Cambridge residential houses and condominiums used in our analysis, comprising 15,475 properties in 1994 and 17,505 in 2004.[20] Slightly more than half of these properties are houses. Rent decontrolled properties account for 30 percent of all residential properties, with condominiums comprising the substantial majority. Because the vast majority of Cambridge houses were and are owner-occupied, only 10 percent of houses are rent controlled.[21] Consistent with overall house price appreciation, assessed values in constant 2008 dollars are lower in the pre-decontrol year of 1994 than in the post-decontrol year of 2004.[22] For example, the average 1994 assessed value of a decontrolled condominium is \$116,000, while it is \$351,000 in 2004—an increase of 111 log points. Houses typically have higher assessed value than condominiums, and in both periods, decontrolled houses and condominiums have lower values on average than never-controlled houses.

4.2 Measuring rent control intensity (RCI)

Measuring a residential property's rent control exposure requires two determinations: 1) which nearby units should be counted in the unit's reference set—that is, to which units it is 'exposed' and 2) how should the rent control status of these reference units be combined to form an exposure index.

Taking these issues in reverse order, we calculate the rent control status of the surrounding units to which a given property i is exposed by summing the number of controlled units within a surrounding geography g and dividing it by the sum of all residential units J_g (controlled and non-controlled) in that geography:[23]

$$RCI_{i(g)} = \frac{1}{J_g} \times \sum_{j \neq i}^{J_g} RC_{j(g)}.$$

Thus, our RCI measure is simply equal to the fraction of nearby units subject to rent control, and it is defined on the unit interval.

The second input into the exposure measure is the choice of a surrounding geography. One potential set of geographies is supplied by the U.S. Census Bureau, which subdivides the area of cities into three increasingly fine geographic units: tracts, block groups and blocks, of which there are 30, 89, and 587, respectively, in Cambridge containing at least one assessed house or

[20] Note that a property may contain multiple units, e.g., a multi-family house.

[21] Conversions from houses and apartments to condominiums were commonplace after decontrol, as we discuss in section 5. The house and condominium designations in Table 2 reflect the property's residential category at the time of assessment.

[22] Prices are deflated by the Consumer Price Index for All Urban Consumers, Series Id CUUR0000SA0L2. This index is an average for U.S. cities and excludes the price of shelter (since we do not wish to confound the outcome measure, house price appreciation, with the numeraire).

[23] Although our analysis of assessed values and transactions excludes apartment buildings, both controlled and never-controlled apartments contribute to the numerator and denominator of our exposure measure. In addition, each rental unit within a multi-family house is counted separately in both the numerator and denominator. Further, the RCI determination for a condominium structure excludes all other units in that structure.

condominium.[24] These pre-defined Census geographies have the virtue of allocating Cambridge land parcels to exhaustive, mutually exclusive geographic units. They also have two substantial drawbacks for our analysis. One is that the Census geographies do not necessarily correspond to any specific notion of neighborhoods or proximity. For example, Census blocks frequently divide streets down the center, so that units on opposite sides are assigned to different blocks, which is clearly undesirable for measuring spillovers from nearby properties. The second drawback is intrinsic to any allocation of geography into non-overlapping parcels: units closer to the perimeter of a geography are treated differently from units located in its center. For example, for a residential unit located on the northern edge of a geography, its neighbors 50 feet to its south will contribute to the unit's rent control exposure measure whereas its neighbors 50 feet to its north will not do so. For a unit located in the center of a geography, by contrast, equidistant neighbors contribute equally to its rent control exposure measure.

To avoid both drawbacks of using fixed geographies, our preferred measure of a unit's rent control exposure is the fraction of residential units within a fixed straight line radius of 0.10, 0.20, and 0.30 miles of that unit that were controlled as of 1994. This radius exposure measure non-prejudicially selects the residential units that are physically closest to the reference unit without excluding nearby units that fall outside an externally stipulated boundary.[25] To provide a feel for the area encompassed by these radii, Figure 1 plots concentric rings of appropriate scale overlaid on the Cambridge map.

Our main estimates are based on rent control intensity measured at a radius of 0.20 miles, which corresponds to about 0.13 square miles—an area larger than a block group but smaller than a tract in our sample. For the typical residential property, 34 percent of the surrounding units within a 0.20 radius are rent controlled. As shown in Table 2, condominiums are in neighborhoods with more rent control than houses and both decontrolled houses and condominiums tend to be in more rent control intensive neighborhoods than their never-controlled counterparts. For instance, in 1994, 32 percent of units surrounding a typical never-controlled condominium are controlled, compared to 45 percent for decontrolled condominiums. There is also considerable cross-sectional variation in rent control intensity. Across all assessed properties, the standard deviation of RCI measured at 0.20 miles is 17 percentage points, and the range of the RCI measure spans from 0 to 72 percent.

5 Capitalized Effects of Rent Decontrol using Assessments

We next estimate the capitalization of spillovers stemming from the elimination of rent control in 1995 onto the value of Cambridge residential real estate. Our illustrative model suggests that

[24] These units have average land areas of 0.22, 0.07, and 0.01 square miles respectively in Cambridge. These geographies housed an average of 3,145, 986, and 135 residents and contained a mean of 1,292, 428, and 63 residential units. Additional details on the size, population, and number of structures and units in Census geographies is contained in Table A1.

[25] Note the caveat that we calculate RCI using only Cambridge properties; when calculating the radius-based RCI for units close to the City's edge, the RCI measure will not include nearby units that lie outside the city. We have verified that our findings are robust to discarding all properties in block groups that border the city of Somerville. A table of these results is available from the authors.

spillovers may arise through two channels: improvements in the maintenance of residences and changes in the sorting of households to neighborhoods. Both effects should be most pronounced in locations with higher rent control intensities and should capitalize into the value of both decontrolled *and* never-controlled properties that are situated in rent control-intensive locations. Decontrolled properties should also benefit directly (i.e., not via spillovers) from the elimination of price controls and relaxation of unit conversion restrictions, both of which increase the investment value of these properties. Moreover, spillovers may accrue to residential properties throughout the city—including those in neighborhoods with low control intensity—if decontrol changes homeowners' overall desire to live in Cambridge.

Our econometric model recognizes each of these channels. We fit equations of the form:

$$\log(p_{igt}) = \gamma_g + \delta_t + \beta' X_i + \lambda_1 \cdot \mathrm{RC}_i + \lambda_{2i} \cdot \mathrm{RCI}_i \cdot \text{Non-RC}_i + \lambda_3 \cdot \mathrm{RCI}_i \cdot \mathrm{RC}_i \\ + \rho_1 \cdot \mathrm{RC}_i \cdot \mathrm{Post}_t + \rho_2 \cdot \mathrm{RCI}_i \cdot \text{Non-RC}_i \cdot \mathrm{Post}_t + \rho_3 \cdot \mathrm{RCI}_i \cdot \mathrm{RC}_i \cdot \mathrm{Post}_t + \epsilon_{igt}, \quad (2)$$

where p_{igt} is the real assessed value of property i in neighborhood g in year t, γ_g are fixed effects representing different geographies, δ_t are year effects, and X_i are property characteristics such as housing type (condominium, single family, two-family or three-family house). The dummy variable RC_i is equal to one for properties that were rent-controlled in 1994 (prior to the law's repeal), while the Post indicator is equal to one for 2004. Of central importance to the analysis, the variable RCI_i measures the fraction of units nearby to i that were controlled as of 1994. As noted above, our main specifications code "nearby" units as those within a 0.20 mile radius of a given property. We explore alternative definitions of proximity in Table 7.

Three parameters are of main interest in this equation. The coefficient ρ_1 estimates the direct effect of rent control removal on the assessed value of formerly controlled properties by contrasting the change in value of controlled versus never-controlled properties following the end of rent control, holding constant unit characteristics, cross-neighborhood differences in residential real estate prices and over-time, and city-wide changes in residential real estate prices. The coefficient ρ_2 estimates the spillover effect from rent decontrol onto the value of never-controlled properties by contrasting changes in the value of never-controlled properties in geographies with high rent control intensity relative to those with low rent control intensity, again holding constant property characteristics, neighborhood effects, and time effects. The coefficient ρ_3 estimates analogous spillovers onto decontrolled properties from *other* nearby decontrolled units after accounting for the direct effect of decontrol (ρ_1) and holding constant property characteristics, neighborhood effects, and time effects. Finally, any effects of decontrol that accrue city-wide—that is, are not limited to decontrolled properties or nearby never-controlled properties—are absorbed by the time effects δ_t. Since these time effects soak up any macroeconomic factor affecting the value of Cambridge's housing stock in this time period, we do not interpret the evolution of δ_t as a causal effect of rent decontrol.[26]

[26]Similarly, we do not interpret the coefficients on the RC main effect and RCI $\times$ RC and RCI $\times$ Non-RC (coefficients λ_1, λ_2, and λ_3) as causal effects of rent control status or rent control intensity since these variables will also pick up unobserved factors that determined rent control status and rent control intensity at the time that rent control was

For ρ_2 and ρ_3 to provide unbiased estimates of the causal external effects of rent decontrol on the market value of residential properties, we require two conditions. First, the elimination of rent controls—and resulting neighborhood level changes—must not have been fully anticipated by households and landlords; to the degree that rent decontrol (and any resulting neighborhood effects) were foreseen by incumbent and potential owners, buyers and renters, these spillover effects would substantially capitalize into values before rent control was removed, which would work against our finding either a direct *or* spillover effect of rent decontrol. As discussed above, the assumption of incomplete anticipation appears plausible in light of the fact that the rent control law was narrowly eliminated (51 to 49 percent) by a state-wide referendum in which a large majority of Cambridge residents voted against rent decontrol. Second, it must be the case that conditional on detailed geographic and time effects, the variable representing a property's exposure to rent decontrol ($\text{RCI}_i \times \text{Post}_t$) is uncorrelated with other unmeasured factors within neighborhoods that affect local house prices, change contemporaneously with rent control removal, but yet are not caused by the elimination of rent control. It is difficult to state precisely what these factors would be since the most obvious candidates (e.g., improvements in neighborhoods) are plausibly caused by rent control removal. We subsequently present event-study graphs with the transaction price sample that strongly suggest that the effect of rent control intensity on house prices is not present prior to the elimination of rent control and evident thereafter.

One concern for our research design is the potential for confounding price trends. The end of rent control in 1995 coincided with a period of nationwide house price appreciation. While the time effects δ_t in our estimating model will absorb *overall* changes in the price level of Cambridge housing, they will not absorb any differential appreciation in rent control-intensive neighborhoods that might hypothetically occur if the concurrent relaxation of mortgage lending standards led to an influx of lending into these neighborhoods, causing prices to appreciate (Mian and Sufi, 2009). We address this concern by estimating specifications containing tract-by-year interactions, in addition to 89 geographic main effects for Cambridge block groups, thereby allowing assessed values to differ by year across Census tracts.

5.1 Appreciation of decontrolled properties

Table 3 presents baseline estimates of equation (2) for the causal effect of rent decontrol on assessed values of decontrolled properties from 1994 to 2004 using the full set of 15,475 residential properties. Column 1 reports a parsimonious specification containing only an RC main effect, a RC × Post indicator, and a set of dummies for year-of-sale and structure type (condominium, two family house, three family house). Prior to rent decontrol, the transacted price of controlled (RC) properties averaged 50 log points below the price of never-controlled (non-RC) properties transacted in the same year.[27] Following decontrol, this gap closed by 22 log points.

adopted in 1970 (for example, the age of the residential housing stock and the fraction of nearby units that were owner occupied versus rented).

[27] The RC main effect estimates do not admit a causal interpretation, as noted above. A property's rent control status in 1994 is a function of the property's year of construction and its residential and occupancy status (rental vs.

Columns 2 through 4 successively add controls that improve the precision of the comparison by sweeping out cross-neighborhood differences in valuations and adjusting for differences in property characteristics that are likely to affect prices. When block group effects are added to the model (column 2), the rent control main effect is 23 log points. With tract-year interactions included, the estimate is 25 log points. The last column, which includes a fixed effect for each residential location or map-lot, is the most demanding. The RC main effect is absorbed by these controls, and the estimate on RC $\times$ Post contrasts the within map-lot change in assessed values of decontrolled versus never-controlled map-lots. The estimate here is 22 log points.

Of signal importance for our identification strategy, the estimated RC $\times$ Post interaction varies minimally across these models. In all within-neighborhood comparisons (columns 2 through 4), our estimates find that the gap in values between RC and non-RC properties closed by 22 to 25 log points following decontrol. The robustness of this result supports a key identifying assumption of our analysis: while RC and non-RC properties (as well as surrounding 'exposed' properties) may differ on many dimensions—not all of which are well measured— these characteristics should not change discontinuously following decontrol. Accordingly, the post-decontrol contrast in values between RC and non-RC properties should hold constant any cross-sectional differences between RC and non-RC properties and hence capture the causal effect of decontrol on the market value of RC properties.

We take these initial estimates as evidence that the voiding of Cambridge's rent control law by statewide ballot in 1995 led to a change in the value of decontrolled properties. In combination with the finding above that rent decontrol catalyzed a rapid change in resident turnover at formerly controlled units, these findings highlight the possibility that rent decontrol might have increased the overall desirability of the neighborhoods in which RC units were situated, generating positive spillovers to nearby never-controlled properties. We evaluate this hypothesis next.

5.2 Spillovers from decontrol

To explore localized spillovers from rent control removal onto nearby properties, we estimate in Table 4 a set of models analogous to those above, now augmented with a measure of the rent control exposure (RCI) of each residential property and an interaction between this variable and an indicator for the post-1994 period. This interaction term allows us to test whether properties with greater rent control exposure saw differential appreciation following decontrol. Our measure of rent control exposure is rent control intensity at the 0.20 mile radius surrounding each property (denoted RCI in the table).

In a bare specification containing only year of sale and structure type dummies (column 1), we find that properties with higher rent control exposure had lower value in the decontrol era, and that this differential was largely erased in period following decontrol. Specifically, the point estimate of -0.58 on the RCI measure indicates that a property at the mean level of rent control exposure of

owner-occupied) as of 1971, which in turn are likely to be correlated with the fixed characteristics of the property, its maintenance and appearance, as well as the desirability of its surrounding neighborhood. While the rent control main effect is robustly large and negative in all cases, this may reflect omitted property attributes and not the causal impact of rent control.

0.32 was assessed at approximately 19 log points below a property with zero exposure. Conversely, the coefficient of 0.33 on the RCI $\times$ Post indicator implies that 56 percent of this price differential was erased in the years after decontrol. As above, we do not take the main effect of RCI as causal since it is likely to be correlated with the many factors that determined which properties were controlled in 1971. Under our identifying assumption that these unobserved factors are quasi-fixed or smoothly trending, our working hypothesis is that the RCI $\times$ Post interaction may be viewed as a causal estimate of localized housing market spillovers.

We explore the robustness of these initial relationships by applying the control variables used above: block group fixed effects, tract-year effects, and map-lot fixed effects. These covariates reduce the precision of the RCI main effect, though the point estimates remain large. The coefficient of primary interest (RCI $\times$ Post) increases in magnitude with the inclusion of tract-year effects. Column 3 obtains an estimate for the RCI $\times$ Post coefficient of approximately 55 log points, while in column 4 the estimate is 48 log points implying that a residential property at the mean level of rent control exposure gained approximately 15-17 percent more in assessed value following decontrol than a property at the first exposure quartile.

These first four estimates derive from models that constrain spillovers from decontrolled units to neighboring properties to be the same magnitude for never-controlled and decontrolled properties. In practice, spillovers may differ between structure types. Moreover, it is not clear that rent control exposure effects on decontrolled properties should be counted as spillovers. The argument in favor of this classification is that the value of an RC property is determined jointly by its physical attributes (age, size), its rent control status, and the desirability of its location. If rent control intensity affects the desirability of a neighborhood, then this effect is likely to affect the market value of both RC and non-RC properties. The argument against viewing these effects as spillovers is that RC properties in rent control-intensive locations may differ from other RC properties in some unmeasured dimension (e.g., perhaps they are more dilapidated). If rent decontrol leads to subsequent rehabilitation of these properties, we might erroneously classify as a spillover what is in reality a direct effect.[28]

We do not need to take a stand on which interpretation is preferable to proceed. In columns 5 and 6, we free up the specification so that the relationship between rent control exposure and property values may differ between controlled and never-controlled properties. We demean this measure so that the rent control main effect corresponds to the price differential for the mean RC property (rather than a property with zero RCI exposure). These estimates produce evidence that never-controlled properties with higher RC exposure appreciated following decontrol by significantly more than comparable properties with lower exposure. In particular, the point estimate for the positive spillover to never-controlled properties is comparable to the prior columns' pooled estimate (28 versus 26 log points for the RCI $\times$ Post interaction term without tract-year effects), and hence the effect size is also comparable. We additionally estimate a slightly larger spillover coefficient for decontrolled properties of 61 to 65 log points in columns 6 and 7. We can reject the hypothesis that spillovers from decontrol to never-controlled properties are zero. The estimated spillovers accruing

[28]Note that if RC properties in RC-intensive areas are less well maintained as a consequence of their greater RC-exposure (as would be implied by our model), then this would be a true spillover rather than an erroneous spillover.

to decontrolled properties are roughly as large as the those for never-controlled properties, but these estimates are less precise and, as we show subsequently, they are also less robust.

5.3 Distinguishing among property types

We now separately analyze spillovers to houses and condominiums. A key motivation for distinguishing between these property types is that the supply of houses and condominiums moved in opposite directions during our sample period. Using the Cambridge Assessor's Databases from 1995 and 2005 (reflecting the status of properties in 1994 and 2004, respectively), we calculate that the stock of units in houses in Cambridge decreased from 14,722 in 1994 to 13,861 in 2004, a 6 percent change.[29] In the same period, the stock of condominiums rose from 7,220 to 9,561 units, an increase of 32 percent, with 45 percent of this increase accounted for by conversion of houses to condominiums (Table 5).[30] Though we are unable to create comparable statistics for the pre-1994 period, it is a near certainty that rent decontrol released a torrent of condominium conversions (Haveman, 1998). Prior to 1995, Cambridge's Rent Control Board and City Council repeatedly took action to prohibit owners of controlled houses and apartment buildings from converting them to condominiums—since this would reduce the supply of rent-controlled units. When rent control was vacated in 1995, these restrictions were lifted and condominium conversions were able to proceed rapidly.

If real estate buyers view houses and condominiums as imperfect substitutes, differing supply trends will likely dampen the market prices of condominiums relative to houses. Consider the fact that for the approximately 85 percent of Cambridge houses that were never-controlled, decontrol meant an increase in market value, resident turnover, and condominium conversions at nearby controlled properties *without* a corresponding increase in the supply of residential houses. By contrast, for the approximately 50 percent of condominiums that were never-controlled, decontrol meant an increase in market values and resident turnover at nearby controlled properties *and* an influx of newly converted units into the residential real estate market. Consequently, we suspect that the never-controlled condominiums that were most exposed to any localized benefits of rent decontrol were also likely exposed to the greatest increase in localized condominium supply. These opposing forces may work against our finding positive price spillovers from decontrolled units to never-controlled condominiums.

Table 6 presents estimates for the houses and condominiums separately. Across the two panels, the direct impact of rent decontrol for controlled houses is substantially smaller than the corresponding estimate for condominiums, a fact potentially due to a greater amount of upgrading at controlled condominiums.[31] Across the columns, the first three specifications in the top panel confirm that

[29] These figures count each unit in a multi-family house separately to meaningfully compare the supply of housing across different structure types and in different periods.

[30] Twenty-five percent is due to conversion of rental apartments and the remainder is due to conversion of other residential and non-residential units. An additional 54 condominium structures were built between 1994 and 2004.

[31] As discussed in section 7, Cambridge building permit data indicate that annual city-wide investments in decontrolled condominiums increased by 206 percent in the post versus pre-decontrol period while the corresponding increase for decontrolled houses was 120 percent (Table 11).

spillovers from rent decontrol to residential houses are economically large and statistically robust.[32] In the first column, the spillover coefficient is 42 log points, implying that houses at the 75^{th} percentile of exposure appreciated by 10 log points more than houses at the 25^{th} percentile following decontrol. When allowed to differ between decontrolled and never-controlled houses (columns 3 and 4), we find evidence for spillovers for both never-controlled and decontrolled houses. In column 3, for instance, they are of comparable magnitude at 41 and 50 log points, respectively.

The spillover estimates for condominiums present a contrasting pattern. Pooling RC and non-RC units, we estimate a spillover coefficient of 23 log points in the first column. However, this estimate is almost half of the size of the estimate for houses and is no longer significant without tract-year effects. Allowing spillovers to differ between RC and non-RC units (column 3 and 4), we again find that the estimates are not significant unless we include tract-year effects. These patterns suggest that controlling for underlying market trends is an important feature of the condominium market during our time period, as might be expected if there were a structural shift in this market due to conversioning and increased supply.

As noted above, one possible explanation for the contrasting results for houses and condominiums concerns property conversions, which are likely to have two effects on our estimates. First, as above, conversions differentially increase the supply of condominiums available in formerly rent control-intensive locations, which may reduce the value of nearby never-controlled units. Second, because condominium conversions often include substantial, unmeasured upgrades to a unit's overall condition and amenities, it is possible that some of the estimated 'spillovers' to decontrolled condominium units actually reflect the capitalized values of unmeasured improvements.

We can shed some light on this second hypothesis by re-estimating the Table 6 models while excluding houses and condominiums that were converted from or to some other usage between 1994 and 2004. These estimates appear in columns 5 and 6 of both the upper and lower panels. Consistent with the possibility that property improvements may explain some fraction of the appreciation of condominiums, we find weaker evidence for spillovers to condominiums once converted properties are excluded. Robust evidence of spillovers to houses remains, however.

5.4 Alternative rent control intensity measures

The estimates we have reported so far measure rent control intensity at a 0.20 mile radius. To explore the sensitivity of our main estimates to this choice, Table 7 reports estimates from alternatives. Other than the change in the RCI measure, the models estimated in Panel I without trends are the same as those in column 5 of Table 4, while those with tract trends are the same as those in column 6 of that table. The specifications in Panel II are the same as those in column 3 of Table 6.

The direct impact of decontrol is insensitive to the RCI measure and the controls for neighborhood geographies. Across specifications, the direct effect of decontrol ranges between 18 to 24 log

[32] To simplify exposition, we display only the interaction terms between post-decontrol and the RC and RCI terms, suppressing the included main effects of these variables. As with Table 4, the RCI variable is demeaned (in this case separately for houses and condominiums) so that $\hat{\rho}_1$ corresponds to the estimated post-decontrol price appreciation for a RC property at the mean of RCI exposure.

points pooling together properties. As in Table 6, the direct impact is smaller for houses than for condominiums, a fact which remains true for each of the reported variations of RCI and geographic controls. This pattern is perhaps unsurprising since the direct effect of decontrol should not depend primarily on geographic spillovers across locations but rather on the rent control status of the particular property.

Across RCI measures and geographies, the spillover estimates are larger and more precisely estimated for the pooled sample in models with tract-year effects than without. Without tract-year effects, the only significant estimate is the 0.20 mile radial RCI measure reported earlier. However, spillover estimates using other RCI definitions and geographic fixed effects are of comparable magnitudes. With tract-year effects, the largest estimate comes from the 0.30 mile radial RCI measure, though this is statistically indistinguishable from the 0.20 mile measure. Another noteworthy pattern is the lower precision of estimates using Census block group RCI measures, which may indicate that block group boundaries inadequately capture spatial proximity.

As we have seen in Table 6, by imposing a common effect across structure types, the pooled estimates may be concealing important heterogeneity in impacts by structure type. A striking finding of Table 7 is the consistency of the spillover estimates for houses (Panel II). The estimates are similar as we vary the geographic fixed effects and the measures of rent control intensity. Moreover, the estimates are large and precise for both never-controlled and de-controlled houses. The estimates for condominiums are also consistent across the specifications, but like the estimates in Table 6, they are imprecise. On balance, the alternative specifications reported in Table 7 indicate that the magnitude of our spillover estimates are not driven by our particular radial measure of RCI and that the evidence of spillovers to houses is stronger than for condominiums.

An interesting finding in Table 7 is that the spillover estimates for houses increase with the area covered by the RCI measure. This fact might suggest that spillovers do not decline with geographic distance, but since there is overlap in RCI defined, say at 0.20 and 0.30 miles, we cannot directly use the Table 7 estimates to come to this conclusion. To probe at how spillover estimates change with distance, we estimated models where we allow for two different measures of rent control intensity in the same regression: an "inner" measure, corresponding to rent control intensity inside the given radius (as before), and an "outer" measure corresponding to rent control intensity between the boundary of the inner measure and the next distance. Using an inner radius of 0.20 miles and an outer radius corresponding to 0.20-0.30 miles, there is modest evidence that spillovers decline with distance for houses: the inner spillover estimate is 0.34 (se=0.08), while the outer spillover estimate is 0.10 (se=0.09).

6 Measuring Capitalization using TransactionsAssessments versus transactions

A key drawback of our sample of assessed values in Cambridge is that it covers only two points in time, the year before the end of rent control (1994) and ten years thereafter (2004). This sample does not allow us to determine how property values changed during the years immediately following the end of rent control nor how prices evolved prior to decontrol. To address these limitations, we turn to data on residential housing transactions for the years 1988 through 2005, which detail the year-by-year evolution of Cambridge housing prices at controlled and never-controlled locations.

Our empirical approach parallels our earlier strategy: we estimate equation (2) where, in this case, the dependent variable is the log real sales price of a transacted property, the Post indicator variable now corresponds to years after 1994, and the covariate vector X contains richer controls for property characteristics available in the deeds records including: total number of rooms, bathrooms, and bedrooms, interior square footage, a quadratic in lot size and a dummy for lot size zero (commonplace for condominiums), a quadratic in the property's age, and a dummy for missing year built. All controls are interacted with dummies for structure type since the value of these attributes may differ across types.

Since not all properties transact before or after the end of rent control, a concern in interpreting estimates of equation (2) is that non-random selection of properties into transaction could lead to biased estimates of ρ_1, ρ_2, and ρ_3. This would occur if unobservable determinants of properties' market values were correlated with both their probability of transacting and their rent control status or rent control intensity. Table A2 in the appendix reports summary statistics on the 14,789 transactions recorded between 1988 and 2005. Of note, the number of transactions in the period before the end of rent control is 5,463 compared to a total of 15,475 assessed residential locations, implying that roughly one-third of locations transacted before the end of rent control.[33] The fraction of houses that transact is substantially lower than for condominiums, however; this results in a small sample size for transacting *decontrolled* houses (< 350 in both the pre- and post-decontrol period) but nevertheless yields a sizable sample of transacting never-controlled houses ($1,624$ and $2,599$ in the pre- and post-period, respectively). To examine selection into transaction, we estimate Seemingly Unrelated Regression (SUR) models in Tables A3 and A4 that explore how the characteristics of transacted properties vary with rent control status and rent control intensity. The regression specification for these models is comparable to those above, except in place of house values, we use as dependent variables a vector of property characteristics and the full set of equations is fit simultaneously to allow for hypothesis testing across equations.

For houses, we detect no individually or jointly significant pre-post decontrol changes in the relationship between rent control status, rent control intensity, and selection into transaction (Table A3). Hence, we do not reject the null hypothesis that the composition of transacted houses did not change after 1994 as a function of rent control status or RCI. There are compositional differences for condominiums, however (Table A4). Condominiums that transact after 1994 at decontrolled locations have 0.15 fewer total rooms than those that transact prior to 1995 and they are more likely to be recently built. Moreover, the number of total rooms and bedrooms in transacted decontrolled condominiums increased after 1994 in rent control-intensive neighborhoods, a pattern consistent with upgrading of these locations. The chi-squared test for the joint significance of these

[33] Because some properties transact multiple times, this figure somewhat overstates the fraction of all properties transacted.

relationships confirms that the end of rent control saw a shift in the composition of transacted condominiums, which suggests some caution in drawing conclusions using the transaction price data for condominiums.

The transaction sample also provides an opportunity to explore the concern noted earlier that assessed real estate values may not accurately reflect market prices. We examine this issue in Table A5 by constructing a matched assessor's sample for properties that transacted in 1994 and 2004 and presenting parallel estimates for the relationship between rent control status and rent control intensity and, separately, assessed and transacted property values. These models yield a close match between the estimated impact of decontrol and rent control intensity on assessed values and transaction prices and, moreover, the match is particularly close for houses.[34] This comparison suggests that the transaction and the assessor's sample provide complementary and broadly consistent measures of valuations.

Neither of these comparison exercises is perfect, of course: in the first case, we cannot directly test for changes in *unobservable* characteristics of transacted properties; in the second case, our comparison does not rule out the possibility that assessed values are a lagging indicator of price changes for *non-transacted* properties. Nevertheless, these results in combination reduce concerns about biases stemming from selection intro transaction.

6.1 Effects of decontrol on transaction prices

The year-by-year coverage of the transaction sample allows us to explore one of our identifying assumptions—specifically, that the end of rent control was not fully anticipated. Following the referendum ending rent control in November 1994, there was still lingering uncertainty on the phase-out timeline. This fact together with adjustment costs in new investments and non-immediate reallocation of residents could lead the price impacts from the end of rent control to operate with some delay. Using the transaction sample, we can explore whether the end of rent control had a discrete impact on transactions prices. We report "event study" plots of the main effect of rent-control status from the equation:

$$\log(p_{igt}) = \gamma_g + \delta_t + \beta' X_i + \sum_{t=1988}^{2005} (\text{RC}_i \times \delta_t)\rho_{1,t} + \epsilon_{igt},$$

where γ_g, δ_t, and X_i are as in equation (2), and $\rho_{1,t}$ are by-year estimates of rent-control main effect measured relative to the year 1994, the omitted reference category.

Panel I of Figure 3 plots estimates of the evolution of the RC effects. The relative price of RC properties increased by roughly 10 log points over the first three years following decontrol, declined modestly between years three and four, and then rose almost continuously thereafter. By the end

[34] An additional complexity in comparing the assessed versus sale values of condominiums is that we are unable to determine which specific unit among the assessed condominiums units at a map-lot is transacted. Consequently, we include matched assessor data for *all* units at the map-lot where one or more unit transacts. This leads to a sample of 7,897 condominium assessments matched to the 937 units that were transacted in 1994 or 2004.

of the sample in 1995, RC properties had increased in market value by almost 30 log points relative to nearby non-RC properties with similar characteristics.

The subsequent two panels of Figure 3 plot RCI spillovers to never-controlled properties from the equation:

$$\log(p_{igt}) = \gamma_g + \delta_t + \beta' X_i + \sum_{t=1988}^{2005} (\text{Non-RC}_i \times \text{RCI}_i \times \delta_t)\rho_{2,t} + \epsilon_{igt},$$

estimated separately for never-controlled houses (Panel II) and condominiums (Panel III). Though precision is limited by the small number of houses transacting annually, there is a clear upward shift in the sale price of never-controlled houses in rent control-intensive neighborhoods after 1994. The point estimate for $\rho_{2,t}$ rises by 25 log points between 1994 and 1997, drops temporarily in 1998, and then becomes large (50 to 75 log points) and statistically significant in years 1999 forward. Panel III also offers evidence of positive RCI spillovers to never-controlled condominiums, but consistent with the point estimates in Table 6 without trends, these estimates are less precise than the results for houses.[35]

One noteworthy feature of Figure 3 is that the estimated effects of decontrol on transaction prices do not reach their maximum until four or more years after rent control removal. This increasing cumulative effect may be explained by two forces. First, as noted above, it is likely that the last minute legislative compromise that kept a subset of units under control for an additional two years generated lingering uncertainty about whether the decontrol regime would hold—uncertainty that ultimately abated as efforts to reinstate controls failed. Second, and perhaps more importantly, the changes in the desirability of locations and neighborhoods induced by decontrol—including residential upgrading and reallocation of residents across areas—likely took years to unfold. As noted above, the evidence on resident turnover found in Figure 2 reveals that although turnover at decontrolled units rose immediately after decontrol, its rise continued for several additional years and did not plateau until 1998.

We explore the relationship between decontrol and residential transaction prices in detail in Table 8. Taking advantage of the additional years of data available from the transactions sample, we consider more flexible controls for housing market trends. Specifically, we include linear and quadratic time trends by neighborhood, which take the form:

$$\alpha_{0,\tau(i)} + \alpha_{1,\tau(i)} t + \alpha_{2,\tau(i)} t^2, \tag{3}$$

where $\tau(i)$ denotes the Census tract in which property i resides.[36] These neighborhood-specific geographic time trends allow prices to evolve differentially across each of the 30 different Cambridge tracts. The addition of these 60 trend measures (two for each tract) allows for a smooth, flexible

[35] We do not plot event studies for spillovers to rent-controlled properties, but a full set of plots for RC main effects and RCI interactions with structure type and RC status is available from the authors.

[36] In most specifications, the tract main effect is absorbed by the inclusion of block group main effects.

evolution of house prices over time within fine geographies. When controlling for property characteristics, we also interact quadratic time trends with structure type to allow different housing types—condominiums, single family homes, multi-family homes—to allow for different price paths.

The first three columns provide estimates of the direct appreciation of decontrolled properties. The estimates range from 6 to 11 log points as we add block group fixed effects, controls for property characteristics, and quadratic tract trends. Adding these covariates reduces the magnitude of the *baseline* price differential between RC and non-RC properties substantially (from −31 to −19 log points). This pattern is consistent with the data summarized in Table A2 indicating that RC properties are situated on smaller lots and in older structures, and in the case of condominiums, provide less square footage than non-RC properties.

Columns 4 and 5 of Table 8 introduce RCI effects. The coefficient of interest in column 4 (the RCI ×Post effect) is 20.5 log points, which is on the lower range of our estimates using the assessor's sample. Adding flexible geographic trends (column 5) slightly reduces the spillover point estimate and increases its standard error. Finally, the last two columns report estimates where the relationship between rent control exposure and prices is permitted to differ between controlled and never-controlled properties. The estimates are not different between these two groups, though they are more precisely estimated for never-controlled properties. In the final column, which includes all prior covariates including tract-specific quadratic trends, the spillover point estimates are no longer significant at conventional levels, reflecting the larger standard errors from the inclusion of tract trends.

Following our analysis of assessed property values, we next fit models separately for the subsamples of houses and condominiums in Table 9. Consistent with the earlier findings, spillovers are large for both houses and condominiums, but are much more precisely estimated for houses than condominiums. For instance, for houses, the point estimate of 0.34 on the RCI × Post coefficient is comparable to the estimate of 0.42 in Table 6, while for condominiums the magnitudes are similar, but there is more precision for the transactions-based estimate, perhaps due to the availability of richer property-level controls. Consistent with the assessment data, spillover estimates to houses are generally highly robust while spillover estimates for condominiums are more sensitive to specification.

One important difference between the two samples involves spillover effects for houses by controlled status. Using the transactions sample, the majority of the spillovers accrue to never-controlled houses, while the assessor's estimates show spillovers accruing for both controlled and never-controlled houses. We suspect that a primary reason for the difference is that the number of controlled houses that transact averages only 33 per year, which is a small fraction of the 829 controlled locations.

Appendix Table A6 considers two further robustness checks using the transaction sample. First, to address potential compositional changes, we focus only on transactions involving locations that did not change structure type between 1994 and 2004. This sample restriction leads us to drop considerably more condominium than house transactions (2,100 out of 9,975 versus 287 out of 4,814). The spillover estimates for uncontrolled houses are similar to those reported in Table 9, but, consistent with Table 6 (columns 5 and 6), the condominium results that exclude converted units are less precise. These results support the interpretation that the prices of condominiums may have been affected differently by decontrol because of changes to the housing stock at condominium locations, and further show that our estimates for never-controlled houses are not sensitive to property conversions. Second, to examine the possibility that the price effects are driven by the availability of subprime lending in Cambridge, we have re-estimated our main specifications discarding any transactions where a mortgage lender is identified as part of the U.S. Department of Housing and Urban Development's list of subprime lenders (Table A6).[37] Overall, 311 (2%) out of our sample of 14,789 transacted properties in Cambridge were financed by lenders identified as subprime, though it is likely that not all these properties were financed by subprime loans. Our estimates that exclude these transactions are nearly identical to those reported in Table 9.

6.2 Price appreciation in adjoining cities

The decade following the elimination of rent control in Cambridge saw substantial housing price appreciation throughout Massachusetts. For example, the Federal Housing Finance Agency's OFHEO house price index (HPI) of single-family houses for the Boston Metropolitan Statistical Area (MSA), an area corresponding to 97 towns including Cambridge (which accounts for 3% of the total MSA population), shows a 270 percent increase from the first quarter of 1995 to the first quarter of 2005.[38] This backdrop of rising real estate prices raises a potential concern that the price appreciation in Cambridge that we attribute to rent decontrol might instead reflect aggregate house price trends. Since our empirical strategy compares price appreciation across local areas *within* Cambridge, this aggregate phenomenon is only a threat to our identification strategy if it leads to *differential* appreciation at formerly rent control-intensive locations for reasons that are unrelated to rent decontrol.

One way to explore this concern is to compare price appreciation in rent control-intensive locations in Cambridge to comparable locations in surrounding Massachusetts towns that did not have rent control in this time period. We implement this comparison by analyzing housing transaction data for the three nearby cities: the adjoining city of Somerville, which abuts Cambridge; the city of Medford, which abuts Somerville; and the city of Malden, which abuts Medford. These transactions data are also sourced from the Warren Group files, used for the price analysis immediately above, and contain the identical data elements and years of coverage.[39]

To perform the comparison, we create a Predicted RCI ('P-RCI') measure for Cambridge and surrounding towns by, first, regressing the Cambridge block group level RCI measure on 18 distinct

[37] This follows the approach of Gerardi, Shapiro and Willen (2007), who use U.S. Department of Housing and Urban Development data to construct a list of subprime lenders.

[38] Genesove and Mayer (2001) also document price appreciation in downtown Boston's condominium market during the 1990s.

[39] A caveat to this approach is that the folk wisdom in the Boston area is that the displacement of Cambridge residents following decontrol—both those leaving decontrolled units and those fleeing rising rents—spurred gentrification of parts of Medford and Somerville. Lending some credence to this hypothesis, Atlantic Marketing Research (1998) reports that 58 percent of Cambridge renters who moved out of their decontrolled units between 1994 and 1997 left Cambridge. In general, we would expect this potential spillover to surrounding cities to bias us towards finding similar differential rises in property prices in non-Cambridge comparison neighborhoods.

block group attributes available from the 1990 Census Summary Tape Files (STF) to obtain a forecasting relationship between census block group attributes and RCI in Cambridge.[40] We next use the model to predict the P-RCI value for each block group in Cambridge, Somerville, Malden, and Medford. Finally, we explore the relationship between P-RCI and residential real estate price appreciation within all four cities. Table 10 presents estimates.

As a benchmark, the first pair of models in Panel I presents the relationship between *actual* RCI and pre-post decontrol price appreciation at never-controlled house and condominium properties in Cambridge between 1988 and 2005.[41] Unlike Table 9, the RCI measure in this specification is computed at the 1990 Census block group level. Panel II presents an identical set of estimates where the observed RCI measure is replaced by P-RCI. For Cambridge houses, the point estimate for the the Post $\times$ P-RCI variable is reassuringly similar to the estimate using the actual RCI measure. This suggests that we may be able to use the statistical relationship between RCI and block group census attributes to construct a proxy for RCI in non-Cambridge towns. While the correspondence is not as close for Cambridge condominiums, this is a secondary concern since we have already established that the RCI results for Cambridge condominiums are not as robust.

The next four panels of Table 10 perform the comparison exercise using transaction data from Somerville, Malden, and Medford. Panel III pools these three cities. Distinct from the pattern for Cambridge houses, we detect neither a significant negative relationship between P-RCI and house transaction prices in the pre-decontrol period nor a significant positive relationship between P-RCI and house transaction prices in the post-decontrol period. Panels IV, V, and VI report estimates separately for the three non-Cambridge cities. These models find inconsistently signed relationships between P-RCI and house transaction prices in Somerville, Malden, and Medford. One surprising result, however, is that the point estimate for P-RCI $\times$ Post for Medford houses is similar to the analogous estimate for Cambridge houses and is significant at the 5 percent level—though unlike in Cambridge, the relationship between P-RCI and house transaction prices in Medford is small and statistically insignificant in the pre-decontrol period. While this result is disconcerting inasmuch as Medford did not have rent control, we are inclined to view this as a chance finding given the evidence in the prior three panels.

A second pattern in Table 10 is that for all towns *except* Cambridge, we find evidence of a substantial decline in the transaction prices of condominiums in block groups with high P-RCI in the post-decontrol period. Placing this result in context, it bears note that Massachusetts experienced a substantial increase in condominium construction and conversions in urban neighborhoods in this period, and this supply shift may have lowered prices. Comparing the non-Cambridge condominium

[40] The 18 block group attributes are population density, median family income, the fraction of commuters using public transportation, average owner tenure (the average tenure in years of owner-occupants at their current residence), average renter tenure, the fraction of owner-occupied housing units that were built before 1970, the fraction of renter-occupied units built before 1970, the fraction of units that are condos, the fraction of residents that are renters, the number of residents within non-family households (e.g. roommates), average age, median contract rent, the average residential property value, the fraction of residents self-identifying as white, the fraction of residents self-identifying as Asian, the fraction of housing units that are vacant, the fraction of housing units in structures with at least 20 units, and the fraction of housing units in structures with 5 to 19 units

[41] We exclude decontrolled properties from this exercise since no such properties exist in the comparison cities.

results to those for Cambridge, one potential inference is that condominium prices in rent control-intensive neighborhoods in Cambridge would have fallen substantially after 1994 had it *not* been for the end of rent control. Given the many complexities surrounding condominium supplies and prices in this time period, however, we remain agnostic on this point.

7 Measuring Housing Investments

To explore whether the price responses documented above can be attributed in part to increased residential investment, we obtained a listing of all building permits issued by the Cambridge Inspectional Services Department for years 1991 through 2005, including property address and proposed expenditure.[42] Table 11 presents descriptive statistics for investments by structure type.[43] Since permits can be filed either for a structure (e.g., a multi-unit condominium complex) or for any unit in a structure, we attribute a permit at a given structure to only one unit in that structure when computing permitted units in Table 11. For reference, we report the mean number of units in permitted structures in the third row.

Cambridge experienced an overall investment boom after the end of rent control. Total permitted investment at houses and condominiums rose from $83 million in the period 1991-1994 to $455 million in the period 1995-2004. Annual investment expenditures roughly doubled at three of four property types: decontrolled houses, never-controlled houses, and never-controlled condominiums. Annual investments roughly tripled at decontrolled condominiums. While fewer than one in twenty-five residential units receives a building permit annually, this fraction increased substantially following decontrol. For houses, annual permitting rates rose by 17 and 7 percent at never-controlled and decontrolled houses, respectively. For condominiums, the percentage increase in permitting was considerably larger: 36 percent for never-controlled and 45 percent for decontrolled structures.[44]

Figure 4 reports estimates of an event study of the impact of decontrol on permitting activity (following Panel I of Figure 3). There is a sustained rise in permitting activity at decontrolled relative to never-controlled properties between 1995 and 2000. This differential returns to its pre-decontrol level thereafter. In contrast to the patterns for permitting activity, we find no differential increase in investment at decontrolled locations, nor do we find spillover effects on investment expenditure.[45]

[42]Section 110.0 of the Massachusetts State Building Code stipulates that 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, or adding shingles to roofs, defined in the Building Code as "any Maintenance which does not affect the structure, egress, fire protection systems, fire ratings, energy conservation provisions, plumbing, sanitary, gas, electrical or other utilities," do not require a permit.

[43]Table 11 tabulates data only through 2004 to allow for a direct comparison to our assessor's database. In 2005, the fraction of units permitted are roughly comparable to the period 1995-2004 and expenditures were $45,864, $7,586, $6,492, and $2,151 thousands for never-controlled houses, decontrolled houses, never-controlled condominiums, and decontrolled condominiums, respectively.

[44]We cannot exclude the possibility that the incentives to file for investment permits, or to accurately report investment costs on building permits, were affected by the rent control regime; for example, a landlord of a controlled unit might have been more likely to declare investment activity to justify a price increase to the Rent Control Board.

[45]By matching the permit data to the structures file, we observe permitting activity at every structure and hence our investment analysis sample is a balanced panel of structures by year, though the majority of permitting observations

Given the aggregate investment boom in Cambridge after decontrol, it is surprising that we do not find stronger evidence for increased building expenditure at decontrolled properties. It appears that although decontrol raised the share of formerly controlled properties receiving investments, actual investment expenditures rose relatively uniformly across decontrolled and never-controlled properties. One factor that may obscure any direct expenditure effect in our analysis is low statistical power: the vast majority of investment expenditures are zero, while the mean and variance of expenditures at permitted units are high and rising (Table 11, Panel III). However, unless increased investment occurred along dimensions that do not require permits and hence are not observed in our data—e.g., repairs and maintenance that are not structural and do not alter major systems—the pattern of results appears to rule out very large differential expenditure effects at formerly controlled units.

8 The Capitalized Value of Rent Decontrol in Cambridge

How economically large are the spillovers from the end of rent control estimated above? We answer this question by benchmarking our estimates against the overall level of house price appreciation in Cambridge using the Cambridge Assessor's Database as our measure of the value of the housing stock.

The top panel of Table 12 presents information about the valuation of the never-controlled housing stock from 1994 to 2004. The total assessed value of the 7,426 never-controlled houses and 3,602 never-controlled condominiums in Cambridge was $10.0 billion in 2004, with houses accounting for 73 percent of the total. We compute the contribution of rent decontrol to these valuations by subtracting the implied post-decontrol price change at each never-controlled location as a function of its rent control exposure (RCI).[46] The aggregate value of the stock of never-controlled houses was $7.3 billion in 2004. Had rent control continued, our estimates imply that the aggregate value would have been $6.5 billion. For condominiums, the aggregate value of the never-controlled stock was $2.7 billion and, had rent control continued, we estimate that this value would have been $2.5 billion. Summing up, we estimate that rent decontrol yielded a $1.0 billion rise in the value of the never-controlled housing stock between 1994 and 2004, accounting for 17 percent ($1.0 billion of $6.0 billion) of the price appreciation of these properties in this period.

The bottom panel reports an analogous calculation for decontrolled properties. The value of the formerly controlled stock appreciated $1.7 billion, from $0.8 billion in 1994 to $2.5 billion in 2004. Had rent control continued, our estimates imply that the 2004 value would have been $770 million lower (including the spillover effects), implying that 45 percent ($770 million of $1.7 billion) of the rise in the value of formerly controlled properties during this period was due to decontrol.[47]

are zeros. We regress investment expenditure on a rent control indicator and the rent control indicator interacted with various post measures, controlling for year of sale dummies, and the number of units and its square (for multi-unit structures), structure type and structure times post interactions.

[46] Estimates are from the regression model with Census block group fixed effects by structure (following column 5 of Table 4). Additional details are provided in the notes to Table 12.

[47] Of this total, 74 percent ($567 million) is a direct decontrol effect, and the other 26 percent ($203 million) is due

Notably, the change in the never-controlled stock's value was much greater than the change in the value of the decontrolled stock from 1994 to 2004: \$6.0 billion versus \$1.7 billion. Rent decontrol contributed substantially more to the change in the value of never-controlled properties than to previously-controlled properties: \$1.0 billion versus \$770 million. That the magnitude of the spillover effect is greater than the direct effect of decontrol remains true regardless of whether we count spillover effects on previously-controlled properties as true spillovers. Thus, while the proportional effect of rent decontrol on prices was larger for decontrolled than never-controlled properties (45 versus 17 percent), the never-controlled segment of the market received the largest increase in capitalization from rent control's removal—and by implication, bore the majority of the incidence of rent control regulation prior to its removal.

Can the increase in residential investments documented in section 7 account for these price impacts? Total permitted residential investments in Cambridge was \$455 million between 1995 and 2004, with \$375 million invested in the never-controlled stock. If all of the never-controlled investment activity is to account for the \$6.0 billion appreciation of never-controlled housing, this would imply a roughly 1,600 percent return per dollar of investment. It therefore seems unlikely that these investments are sufficient to explain a substantial fraction of the observed property appreciation. Focusing specifically on never-controlled properties, consider an extreme case in which the entire \$19.8 million increase in annual permitted investments at never-controlled structures between 1995 and 2004 (\$198 million total) could be causally attributed to rent decontrol, where each dollar of expenditure led to a dollar of price appreciation, and where there was no subsequent depreciation of these investments during this ten year interval.[48] In this case, we would conclude that only 20 percent of the induced appreciation of never-controlled structures was due to induced investment, with the remaining 80 percent due to capitalized spillovers from rent decontrol.

9 Conclusion

The largely unanticipated elimination of rent control in Cambridge, Massachusetts in 1995 affords a unique opportunity to identify spillovers in residential housing markets. This paper exploits the sharp cross-neighborhood contrasts in the fraction of units that were decontrolled to credibly assess the localized price spillovers to never-controlled properties as well as to quantify direct effects on decontrolled properties. Our main finding is a large and significant positive spillover impact from decontrol onto the valuation of never-controlled properties, leading on average to a 12 percent increase in the market valuations of never-controlled houses between 1994 and 2004. The evidence for spillovers is somewhat less clear-cut for condominiums—likely in part due to a substantial increase in the supply of condominiums enabled by decontrol. We document that, consistent with expectations, rent-controlled properties were valued at a substantial discount relative to never-controlled

to spillovers from exposure to rent control.

[48] Annual investments in never-controlled structures were \$30.0 and \$7.6 million for houses and condominiums, respectively, in the post-decontrol period and \$14.0 and \$3.7 million in the pre-decontrol period. The increase in annual expenditures was \$19.8 million.

properties. Rent decontrol largely eliminated this differential, increasing the assessed values of these properties by approximately 18 to 25 percent.

The contribution of decontrol to the capitalized value of the Cambridge residential housing stock in this period corresponds to a total of \$1.8 billion. Of the \$6.0 billion increase in the assessed value of the never-controlled housing stock in Cambridge between 1994 and 2004, we estimate that \$1.0 billion (17 percent) was due to elimination of rent controls. While the direct effects on decontrolled properties were larger in percentage terms than the effects on never-controlled properties, the stock of controlled properties was smaller and less valuable than the never-controlled stock. As a consequence, positive spillovers to never-controlled properties account for more than half (57 percent) of the decontrol-induced increase in the value of the housing stock.

Under any reasonable set of assumptions, increases in residential investment stimulated by rent decontrol can explain only a small fraction of these spillover effects. Thus, we conclude that decontrol led to changes in the attributes of Cambridge residents and the production of other localized amenities that made Cambridge a more desirable place to live. This possibility is also highlighted by our theoretical model, though we are not able to thoroughly examine it with our data. Influential work by Glaeser and Luttmer (2003) argues that non-price rationing under rent control leads to a mismatch between renters and apartments, and provides evidence that this allocative inefficiency is large in New York City's rent control plan. It is therefore reasonable to conjecture that the unwinding of allocative distortions significantly contributes to Cambridge's residential price appreciation. Additional empirical analysis with rich micro-level attributes of residents, however, will be needed to shed further light on rent control's allocative consequences.

A key issue in the evaluation of price controls is the tradeoff between the surplus transferred from landlords to renters and the deadweight loss from quality or quantity undersupply. Viewed in this light, a significant portion of the price gains we measure at decontrolled properties are transfers from renters back to landlords. However, our analysis highlights the importance of another welfare consequence of price controls: the impact on the non-controlled sector. Our results indicate that the efficiency cost of Cambridge's rent control policy was large relative to the size of the transfer to renters.

These findings are germane to the analysis of the economic impacts of regulations of the housing market and, more broadly, the impacts of other place-based policies. The mechanisms by which rent decontrol impacts never-controlled housing—increased maintenance, upgrading of local amenities, and improved sorting of consumers to housing—are likely present in other settings involving residential housing. Our results provide evidence that residential spillovers are large and important in housing markets, and suggest that public policies related to housing should consider not only direct impacts but also indirect impacts on neighboring properties and residents.

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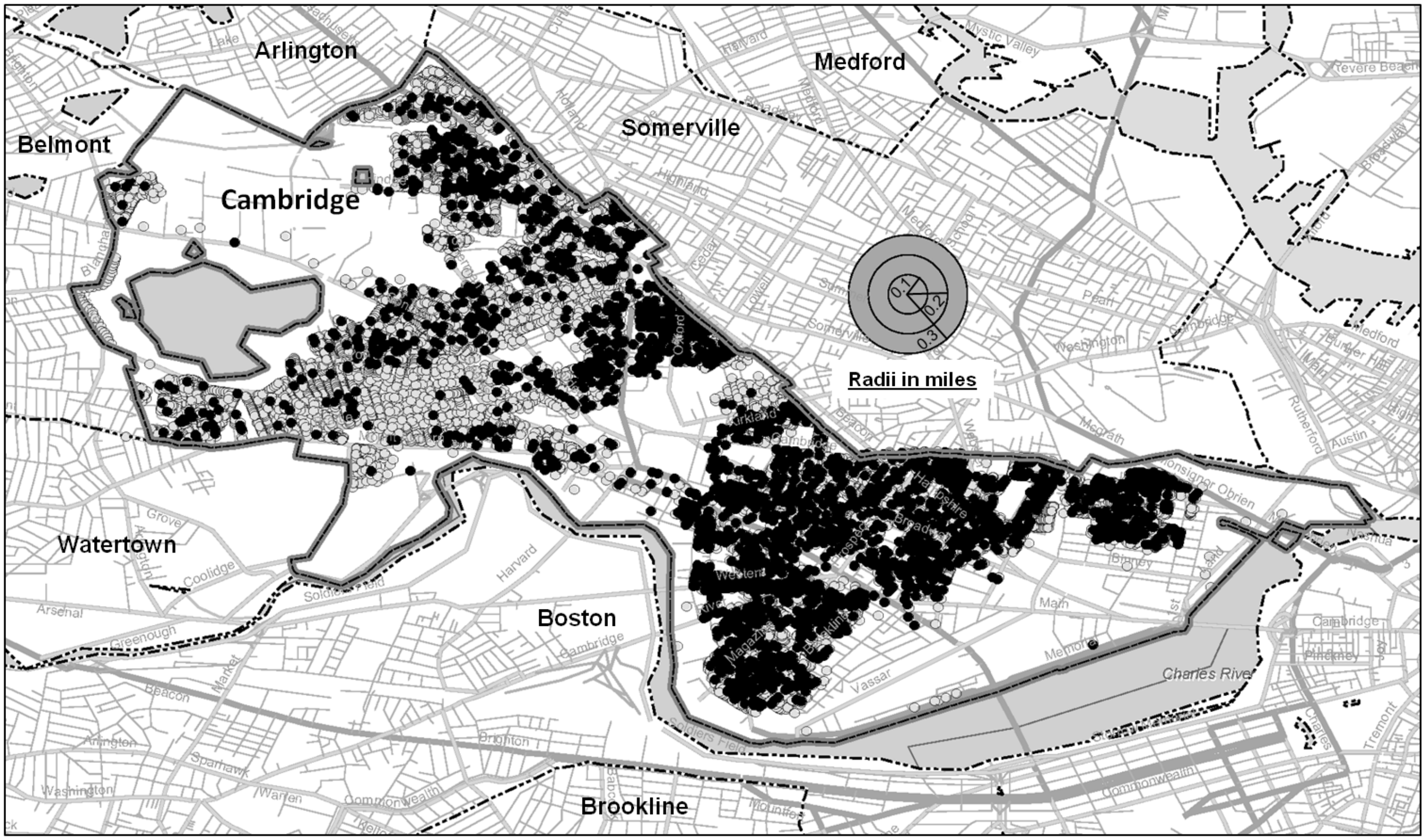
Figure 1. Residential Properties in Cambridge



Notes. Residential properties are marked with gray circles. Rent controlled properities are overlaid with black circles. Concentric circles in the top right depict radii of 0.1, 0.2, and 0.3 miles.

Figure 2. Residential Turnover in Cambridge Controlled relative to Never-Controlled Units, 1992 - 2000

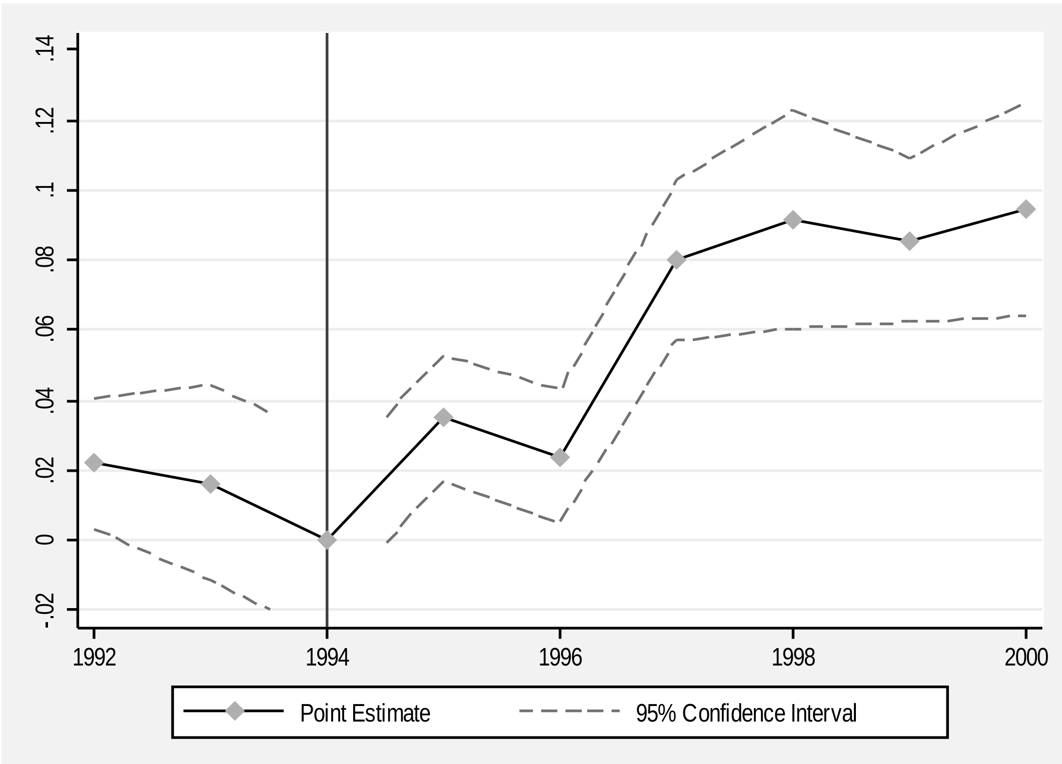
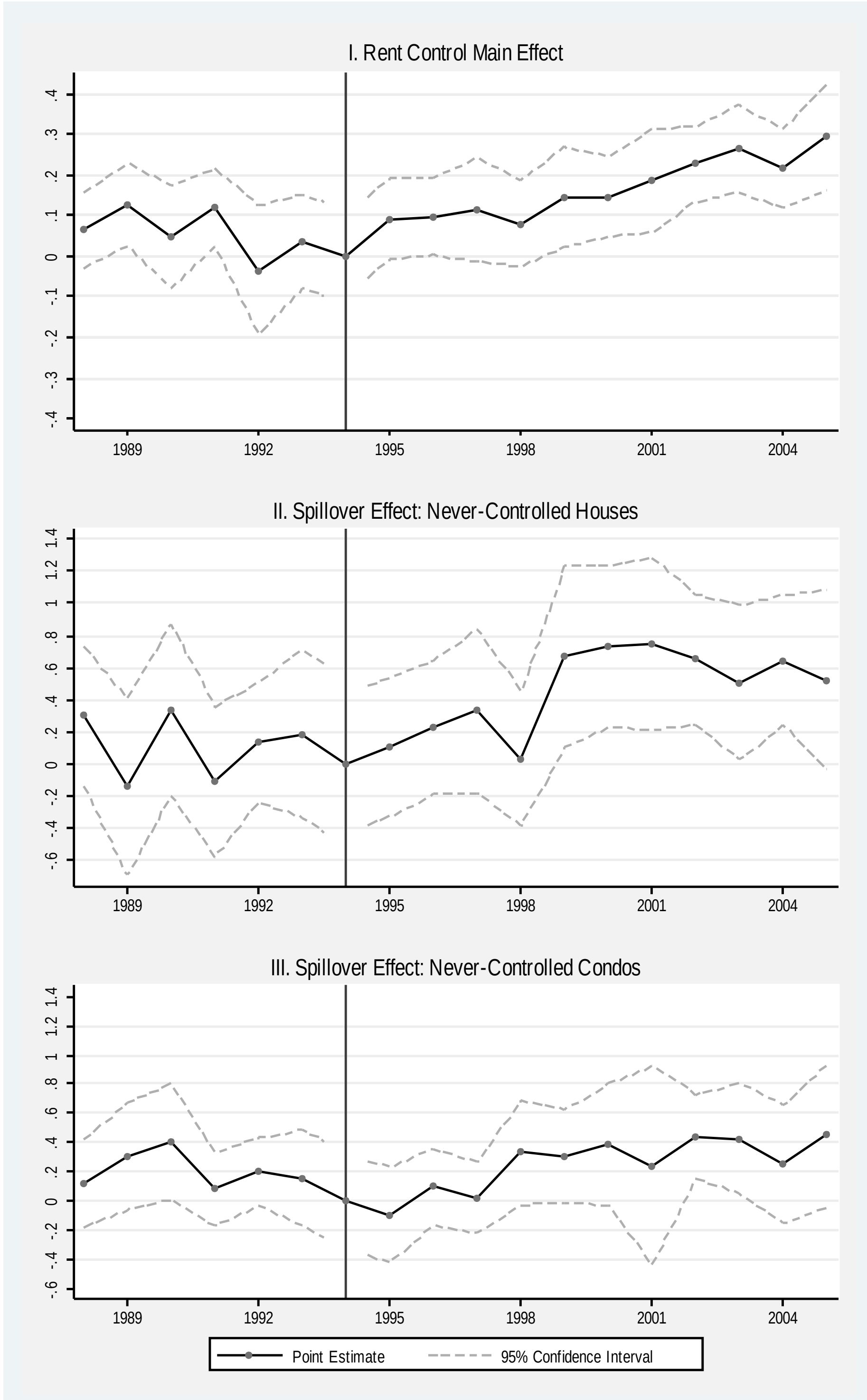


Figure plots coefficients on RC x Year variables from an event-study regression where the dependent variable is an indicator equal to one if resident was not present in current Cambridge unit in prior year (and zero otherwise). RC x 1994 is the omitted category. RC is an indicator for a location that was rent controlled in 1994. This specification includes an RC main effect, year controls, structure type dummies, and geographic fixed effects for the 91 block groups in the 1990 Census containing addresses listed in the Cambridge City Census. 95% confidence intervals are constructed from robust standard errors clustered by block group. The vertical line in 1994 indicates the year preceding rent control removal.

Figure 3. Event Studies for Transaction Prices, 1988 - 2005:
Rent Control Main Effect and Spillovers to Never-Controlled Units



Figures plot coefficients from event study regressions where dependent variable is log sale price, winsorized to the first percentile separately for houses and condominiums. Panel A plots RC x Year coefficients. Panels B and C plot RCI x Year coefficients. RC is an indicator for a location that was rent controlled in 1994. RCI is calculated over a 0.20 mile radius. All specifications include an RC main effect, property characteristics, year dummies, and geographic fixed effects for the 88 Cambridge block groups in the 1990 Census containing transacted properties. Panels B and C include an RCI main effect and interact structure type dummies with quadratic time trends and property characteristics. 95% confidence intervals are constructed from robust standard errors clustered by block group. The vertical line in 1994 indicates the year preceding rent control removal.

Figure 4. Event Studies for Investment Activity, 1991 - 2005

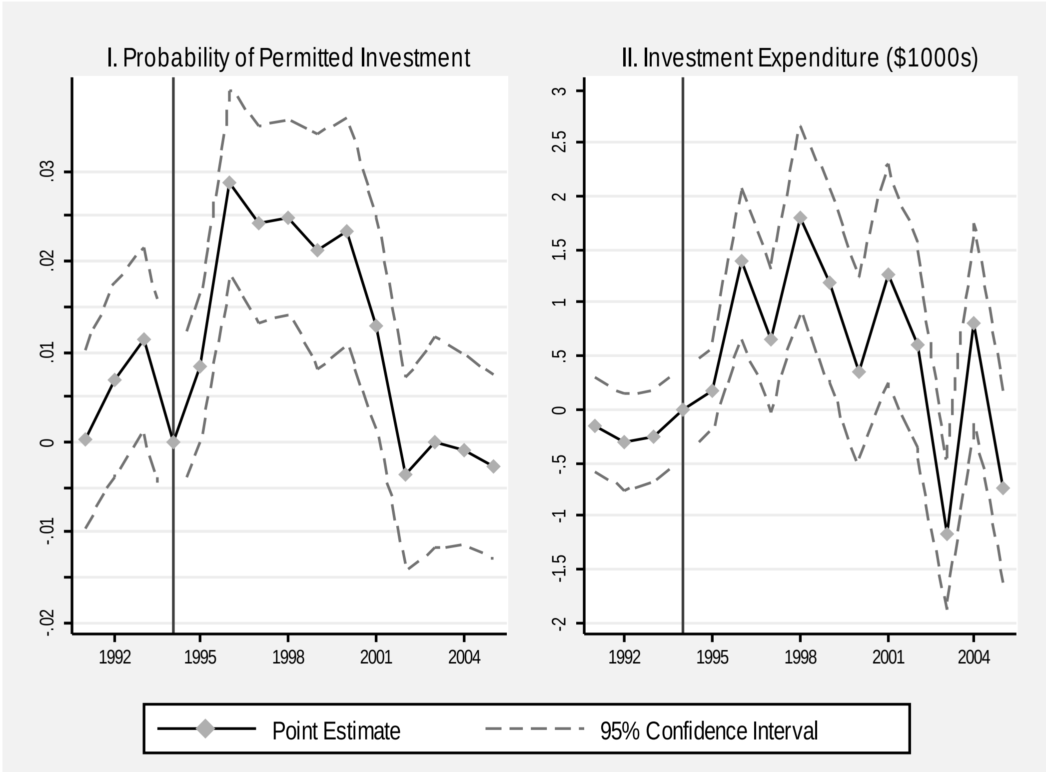


Figure plots coefficients on RC x Year variables from event study regressions where the dependent variable is (left panel) an indicator for whether a structure received a building permit and (right panel) the permitted expenditure at a structure. RC is an indicator for a location that was rent controlled in 1994. Investment expenditures are winsorized by structure type and year to the 99.5th percentile. Both specifications include an RC main effect, year fixed effects, geographic fixed effects for the 89 Cambridge block groups in the 1990 Census containing assessed properties, structure type indicators, and a quadratic in the number of units in condominium structures. 1994 is the omitted RC x Year category. 95% confidence intervals are constructed from robust standard errors clustered by block group. The vertical line in 1994 indicates the year preceding rent control removal.

Table 1. Turnover at Cambridge Residential Locations, 1992-2000

Dependent Variable: Indicator Equal to One if Resident was not at Location in Prior Year

	All Properties (1)	Houses (2)	Condominiums (3)	Apartments (4)
Mean of Dependent Variable	0.269	0.232	0.297	0.335
	(0.197)	(0.178)	(0.209)	(0.223)
RC	-0.003	0.073***	-0.035**	-0.056**
	(0.008)	(0.008)	(0.016)	(0.026)
RC x Post	0.054***	0.025***	0.076***	0.057**
	(0.008)	(0.008)	(0.022)	(0.025)
N	310,949	172,996	70,558	67,395

Dependent variable is an indicator equal to one if resident was not present in current unit in prior year (and zero otherwise). RC is an indicator for a location that was rent controlled in 1994 and Post is an indicator for year 1995 and after. All specifications include year controls, structure type dummies, and geographic fixed effects for the 91 block groups in the 1990 Census containing addresses listed in the Cambridge City Census. Robust standard errors clustered by block group are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2. Descriptive Statistics: Assessed Values (2008 Dollars) and Distribution of Rent Control Intensity (RCI)

	Never Controlled		Decontrolled	
	1994	2004	1994	2004
		I. Houses		
log Value	12.72	13.65	12.56	13.61
	(0.56)	(0.55)	(0.48)	(0.45)
RCI	0.30	0.30	0.34	0.35
	(0.15)	(0.15)	(0.14)	(0.14)
N	7,426	7,145	829	839
		II. Condominiums		
log Value	12.36	13.10	11.66	12.77
	(0.58)	(0.46)	(0.67)	(0.38)
RCI	0.32	0.31	0.45	0.43
	(0.19)	(0.18)	(0.14)	(0.14)
N	3,602	4,921	3,618	4,600

Table reports means and standard deviations in parentheses of assessed values and Rent Control Intensity (RCI) for residential structures by structure type, rent control status and year. RCI is calculated over a 0.20 mile radius. Assessed values are converted to real 2008 dollars using the Consumer Price Index for All Items Less Shelter for All Urban Consumers, Series Id: CUUR0000SA0L2, Not Seasonally Adjusted.

Table 3. Effects of Rent Decontrol on Assessed Values
Dependent Variable: Log of Assessed Property Value (1994, 2004)

	(1)	(2)	(3)	(4)
RC	-0.504***	-0.504***	-0.515***	
	(0.075)	(0.052)	(0.052)	
RC x Post	0.217***	0.227***	0.249***	0.221***
	(0.039)	(0.037)	(0.034)	(0.040)
Block Group FEs	-	y	y	-
Tract Trends	-	-	y	y
Map Lot FEs	-	-	-	y
R-squared	0.605	0.759	0.763	0.938

N = 32,980. Sample is all assessed Cambridge houses and condominium properties in 1994 and 2004. RC is an indicator for a location that was rent controlled in 1994 and Post is an indicator for year equal to 2004. Year fixed effects and structure-type dummies are included in all regressions. Block group fixed effects correspond to the 89 Cambridge block groups in the 1990 Census containing assessed properties. Tract trends are tract x Post dummies for each of 30 tracts from the 1990 Census. In column 4, RC main effects are absorbed by map lot fixed effects. Robust standard errors clustered by 1990 block group are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 4. Effects of Rent Decontrol and Rent Control Intensity on Assessed Values
Dependent Variable: Log of Assessed Property Value (1994, 2004)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RC	-0.440***	-0.484***	-0.503***		-0.450***	-0.464***	
	(0.057)	(0.050)	(0.052)		(0.052)	(0.053)	
RC x Post	0.175***	0.196***	0.233***	0.208***	0.192***	0.220***	0.197***
	(0.038)	(0.036)	(0.034)	(0.040)	(0.039)	(0.036)	(0.038)
RCI	-0.581*	-0.792	-0.938*				
	(0.325)	(0.479)	(0.494)				
RCI x Post	0.328**	0.258*	0.545***	0.475***			
	(0.136)	(0.138)	(0.191)	(0.180)			
Non-RC x RCI					-0.568	-0.686	
					(0.546)	(0.561)	
Non-RC x RCI x Post					0.281*	0.514**	0.415*
					(0.168)	(0.227)	(0.220)
RC x RCI					-1.211**	-1.416**	
					(0.535)	(0.555)	
RC x RCI x Post					0.249	0.651***	0.607**
					(0.215)	(0.231)	(0.256)
Block Group FEs	-	y	y	-	y	y	-
Tract Trends	-	-	y	y	-	y	y
Map Lot FEs	-	-	-	y	-	-	y
H_0: No Spillovers	0.018	0.065	0.006	0.010	0.126	0.010	0.028
H_0: Spillovers Equal					0.909	0.598	0.514
R-squared	0.611	0.761	0.765	0.938	0.764	0.767	0.938

N = 32,980. RCI is calculated over a 0.20 mile radius and demeaned. RC is an indicator for a location that was rent controlled in 1994 and Post is an indicator for year equal to 2004. RC and RC x RCI main effects are absorbed by map lot FE in columns 4 and 7. Year fixed effects and structure type dummies are included in all regressions. Block group fixed effects correspond to the 89 Cambridge block groups in the 1990 Census containing assessed properties. Tract trends are tract x Post dummies for each of 30 tracts from the 1990 Census. Test for No Spillovers reports p-values from tests that RCI x Post or Non-RC x RCI x Post and RC x RCI x Post coefficients are zero. Test for Spillovers Equal reports p-values from tests that these latter two coefficients are equal. Robust standard errors clustered by 1990 block group are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Property Conversions, 1994-2004:
Status in 1994 of Units that Were Designated as Houses and Condominiums in 2004

	2004 Houses			2004 Condominiums		
1994 Structure Type	All Houses	Formerly Controlled	Never Controlled	All Condo-miniums	Formerly Controlled	Never Controlled
Same as 2004	13,480 (97.3%)	1,567 (89.9%)	11,913 (98.3%)	7,085 (74.1%)	3,507 (76.2%)	3,578 (72.1%)
Converted from	381 (2.7%)	177 (10.1%)	204 (1.7%)	2,476 (25.9%)	1,093 (23.8%)	1,383 (27.9%)
Houses				1,058 (11.1%)	151 (3.3%)	907 (18.3%)
Condominiums	20 (0.1%)	3 (0.2%)	17 (0.1%)			
Apartments	153 (1.1%)	115 (6.5%)	38 (0.3%)	647 (6.8%)	599 (13%)	48 (1%)
Other Residential	50 (0.4%)	35 (2%)	15 (0.1%)	347 (3.6%)	284 (6.2%)	63 (1.3%)
Non-Residential	158 (1.1%)	24 (1.4%)	134 (1.1%)	424 (4.4%)	59 (1.3%)	365 (7.4%)
Total	13,861	1,744	12,117	9,561	4,600	4,961

Counts and conversion rates are calculated from Cambridge Assessor's databases, reflecting property characteristics as of 1994 and 2004. The "Other Residential" category includes structures zoned as boarding houses, mixed use, or multiple houses on a single parcel.

Table 6. Effects of Rent Decontrol and Rent Control Intensity on Assessed Values
by Structure Type
Dependent Variable: Log of Assessed Property Value (1994, 2004)

	(1)	(2)	(3)	(4)	(5)	(6)
			I. Houses			
RC x Post	0.081***	0.065***	0.078***	0.060***	0.069***	0.068***
	(0.013)	(0.011)	(0.013)	(0.011)	(0.014)	(0.013)
RCI x Post	0.421***	0.205*			0.419***	
	(0.068)	(0.103)			(0.068)	
Non-RC x RCI x Post			0.414***	0.194*		0.417***
			(0.068)	(0.103)		(0.067)
RC x RCI x Post			0.499***	0.315**		0.440***
			(0.119)	(0.130)		(0.128)
H_0: No Spillovers	0.000	0.050	0.000	0.056	0.000	0.000
H_0: Spillovers Equal			0.391	0.0804		0.826
R-squared	0.853	0.855	0.853	0.855	0.856	0.856
N	16,239	16,239	16,239	16,239	14,917	14,917
			II. Condominiums			
RC x Post	0.330***	0.354***	0.328***	0.350***	0.317***	0.319***
	(0.048)	(0.038)	(0.050)	(0.040)	(0.048)	(0.047)
RCI x Post	0.227	0.669**			0.108	
	(0.160)	(0.256)			(0.167)	
Non-RC x RCI x Post			0.257	0.678**		-0.086
			(0.196)	(0.308)		(0.106)
RC x RCI x Post			0.171	0.648**		0.301
			(0.271)	(0.291)		(0.340)
H_0: No Spillovers	0.159	0.011	0.341	0.038	0.518	0.549
H_0: Spillovers Equal			0.801	0.925		0.305
R-squared	0.705	0.714	0.705	0.714	0.714	0.715
N	16,741	16,741	16,741	16,741	11,778	11,778
Block Group FEs	y	y	y	y	y	y
Tract Trends	-	y	-	y	-	-
Excluding Converted Structures	-	-	-	-	y	y

RCI is calculated over a 0.20 mile radius and demeaned. RC is an indicator for a location that was rent controlled in 1994 and Post is an indicator for year equal to 2004. In specifications that include RC, RCI, Non-RC x RCI or RC x RCI interacted with Post, main effects of these variables are included but not tabulated. Year fixed effects and structure type dummies are included in all regressions. Block group fixed effects correspond to the 89 Cambridge block groups in the 1990 Census containing assessed properties. Tract trends are tract x Post dummies for each of 30 tracts from the 1990 Census. Test for No Spillovers reports p-values from tests that RCI x Post or Non-RC x RCI x Post and RC x RCI x Post coefficients are zero. Test for Spillovers Equal reports p-values from tests that these latter two coefficients are equal. Robust standard errors clustered by 1990 block group are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 7. Effect of Rent Decontrol and Rent Control Intensity on Assessed Values
for Various Rent Control Intensity Measures
Dependent Variable: Log of Assessed Property Value (1994, 2004)

	0.10 miles	0.20 miles	Block Group	0.30 miles	0.10 miles	0.20 miles	Block Group	0.30 miles
	I. All Structures							
	No Trends				*Tract Trends*			
RC x Post	0.184***	0.192***	0.216***	0.206***	0.211***	0.220***	0.240***	0.232***
	(0.038)	(0.039)	(0.037)	(0.040)	(0.036)	(0.036)	(0.034)	(0.037)
Non-RC x RCI x Post	0.214	0.281*	0.156	0.224	0.214	0.514**	0.149	0.580**
	(0.138)	(0.168)	(0.134)	(0.137)	(0.140)	(0.227)	(0.173)	(0.254)
RC x RCI x Post	0.230	0.249	0.138	0.164	0.388**	0.651***	0.322*	0.687**
	(0.169)	(0.215)	(0.181)	(0.259)	(0.156)	(0.231)	(0.191)	(0.262)
H_0: No Spillovers	0.146	0.126	0.410	0.221	0.033	0.010	0.237	0.016
H_0: Spillovers Equal	0.941	0.909	0.935	0.839	0.343	0.598	0.404	0.705
Geographic FEs	Block	Blk Grp	Blk Grp	Blk Grp	Block	Blk Grp	Blk Grp	Blk Grp
N	32,980	32,980	32,980	32,980	32,980	32,980	32,980	32,980
	II. By Structure							
	Houses				*Condominiums*			
Mean RCI	0.288	0.301	0.291	0.311	0.374	0.375	0.378	0.383
Standard Dev RCI	0.169	0.149	0.164	0.141	0.204	0.172	0.183	0.139
RC x Post	0.075***	0.078***	0.084***	0.079***	0.330***	0.328***	0.349***	0.347***
	(0.014)	(0.013)	(0.013)	(0.013)	(0.051)	(0.050)	(0.048)	(0.053)
Non-RC x RCI x Post	0.307***	0.414***	0.298***	0.428***	0.195	0.257	0.139	0.182
	(0.064)	(0.068)	(0.072)	(0.071)	(0.162)	(0.196)	(0.204)	(0.232)
RC x RCI x Post	0.345***	0.499***	0.352***	0.539***	0.157	0.171	0.051	0.032
	(0.101)	(0.119)	(0.107)	(0.131)	(0.216)	(0.271)	(0.217)	(0.333)
H_0: No Spillovers	0.000	0.000	0.000	0.000	0.395	0.341	0.777	0.731
H_0: Spillovers Equal	0.668	0.391	0.552	0.293	0.884	0.801	0.765	0.712
Geographic FEs	Block	Blk Grp	Blk Grp	Blk Grp	Block	Blk Grp	Blk Grp	Blk Grp
N	16,239	16,239	16,239	16,239	16,741	16,741	16,741	16,741

RCI is calculated over geographies reported in column headings and demeaned in each case. RC is an indicator for a location that was rent controlled in 1994 and Post is an indicator for year equal to 2004. In specifications that include RC, Non-RC x RCI or RC x RCI interacted with Post, main effects of these variables are also included but not tabulated. Year fixed effects and structure type dummies are included in all regressions. Block fixed effects and block group fixed effects correspond to the 587 Census blocks and 89 Census block groups containing assessed properties, both defined using the 1990 Census. Tract trends are tract x Post dummies for each of 30 tracts from the 1990 Census. Test for No Spillovers reports p-values from tests that Non-RC x RCI x Post and RC x RCI x Post coefficients are zero. Test for Spillovers Equal reports p-values from tests that these two coefficients are equal. Robust standard errors clustered by 1990 block group are in parentheses. Mean (SD) of RCI variable is 0.331 (0.192) at 0.10 miles, 0.339 (0.165) at 0.20 miles, 0.348 (0.145) at 0.30 miles, and 0.335 (0.179) at the Census block group level.
*** p<0.01, ** p<0.05, * p<0.1

Table 8. Effects of Rent Decontrol and Rent Control Intensity on Transaction Prices, 1988 - 2005
Dependent Variable: Log Sale Price

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RC	-0.305***	-0.204***	-0.193***	-0.189***	-0.185***	-0.166***	-0.161***
	(0.043)	(0.024)	(0.024)	(0.025)	(0.024)	(0.025)	(0.024)
RC x Post	0.060*	0.106***	0.086***	0.087***	0.079***	0.079***	0.068***
	(0.030)	(0.026)	(0.027)	(0.026)	(0.025)	(0.025)	(0.024)
RCI				-0.510*	-0.494		
				(0.305)	(0.317)		
RCI x Post				0.205***	0.166*		
				(0.056)	(0.098)		
Non-RC x RCI						-0.305	-0.276
						(0.274)	(0.275)
Non-RC x RCI x Post						0.197***	0.132
						(0.067)	(0.089)
RC x RCI						-0.884**	-0.883**
						(0.360)	(0.368)
RC x RCI x Post						0.246*	0.246
						(0.146)	(0.177)
Block Group FEs	-	y	y	y	y	y	y
Property Characteristics	-	y	y	y	y	y	y
Quadratic Tract Trends	-	-	y	-	y	-	y
H_0: No Spillovers	-	-	-	0.000	0.095	0.002	0.208
H_0: Spillovers Equal	-	-	-	-	-	0.773	0.512
R-squared	0.318	0.674	0.681	0.675	0.682	0.678	0.684

N=14,789 Cambridge house and condominium properties transacted during 1988 through 2005. Prices are winsorized by structure type at the first percentile. RCI is defined over a 0.20 mile radius and demeaned. RC is an indicator for a location that was rent controlled in 1994 and Post is an indicator for year is 1995 or afterwards. All specifications include year of sale dummies and structure type dummies. Property Characteristics, each interacted with structure type, include number of total rooms, bathrooms, bedrooms, interior square footage, a dummy variable for zero lot size, a quadratic in lot size, a dummy variable for missing year built, a quadratic in the log age of the structure, and a quadratic time trend for each structure type. Block group fixed effects correspond to each of the 88 Cambridge block groups in the 1990 Census containing transacted properties. Columns 5 and 7 include quadratic tract trends for each of 30 Census tracts. Test for No Spillovers reports p-values from tests that RCI x Post or Non-RC x RCI x Post and RC x RCI x Post coefficients are zero. Test for Spillovers Equal reports p-values from tests that these latter two coefficients are equal. Robust standard errors clustered by 1990 block group are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 9. Effects of Rent Control and Rent Control Intensity on Transaction Prices by Structure Type, 1988 - 2005

Dependent Variable: Log Sale Price

	Houses			Condominiums		
	(1)	(2)	(3)	(4)	(5)	(6)
RC x Post	0.089**	0.101**	0.102**	0.092***	0.078***	0.069**
	(0.042)	(0.043)	(0.045)	(0.030)	(0.029)	(0.027)
RCI x Post	0.337***			0.152**		
	(0.078)			(0.072)		
Non-RC x RCI x Post		0.359***	0.274**		0.080	-0.007
		(0.085)	(0.128)		(0.086)	(0.128)
RC x RCI x Post		0.080	-0.024		0.308*	0.306
		(0.262)	(0.294)		(0.155)	(0.197)
Quadratic Tract Trends	-	-	y	-	-	y
H_0: Spillovers Equal		0.326	0.315	0.235	0.124	0.774
H_0: No Spillovers		0.000	0.093		0.066	0.264
R-squared	0.695	0.696	0.705	0.628	0.630	0.639
N	4,814	4,814	4,814	9,975	9,975	9,975

Sample includes Cambridge house and condominium properties transacted during 1988 through 2005. Prices are winsorized by structure type at the first percentile. RCI is defined over a 0.20 mile radius and demeaned. RC is an indicator for a location that was rent controlled in 1994 and Post is an indicator for year is 1995 or afterwards. In specifications that include RC, RCI, Non-RC x RCI or RC x RCI interacted with Post, main effects of these variables are also included but not tabulated. All specifications include year of sale dummies, structure type dummies, block group fixed effects corresponding to each of the 88 Cambridge block groups in the 1990 Census containing transacted properties, and controls for property characteristics, which are each interacted with structure type and include number of total rooms, bathrooms, bedrooms, interior square footage, a dummy variable for zero lot size, a quadratic in lot size, a dummy variable for missing year built, a quadratic in the log age of the structure, and a quadratic time trend for each structure type. Columns 3 and 6 include quadratic tract trends for each of 30 Census tracts. Test for No Spillovers reports p-values from tests that RCI x Post or Non-RC x RCI x Post and RC x RCI x Post coefficients are zero. Test for Spillovers Equal reports p-values from tests that these latter two coefficients are equal. Robust standard errors clustered by 1990 block group are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10. Placebo Estimates of the Relationship between Imputed Rent Control Intensity and Property Price Appreciation in Cambridge and Adjoining Cities, 1988 - 2005
Dependent Variable: Log Sale Price

	Houses (1)	Condo-miniums (2)	Houses (3)	Condo-miniums (4)	House (5)	Condo-miniums (6)
	I. Cambridge: Actual RCI		II. Cambridge: Predicted RCI		III. Somerville, Medford and Malden	
RCI	-0.183	-0.257	-0.203**	-0.504*	-0.034	0.101
	(0.112)	(0.226)	(0.096)	(0.256)	(0.057)	(0.205)
RCI x Post	0.261***	0.063	0.278***	-0.055	0.088	-0.574***
	(0.088)	(0.093)	(0.092)	(0.102)	(0.055)	(0.206)
N	4,223	5,764	4,223	5,764	17,270	3,346
	IV. Somerville		V. Malden		VI. Medford	
RCI	-0.162	0.238	0.023	-0.176	-0.056	0.832***
	(0.133)	(0.555)	(0.077)	(0.172)	(0.079)	(0.268)
RCI x Post	-0.090	-0.406	0.052	-0.562***	0.174**	-1.201***
	(0.151)	(0.507)	(0.066)	(0.171)	(0.086)	(0.278)
N	6,605	1,868	6,506	1,197	4,159	281

Sample includes never-controlled houses and condominiums in Cambridge and surrounding cities transacted during 1988 through 2005. Prices are winsorized by structure type and city at the first percentile. Actual RCI is Rent Control Intensity calculated at the 1990 Census block group level. Predicted RCI is imputed for Cambridge and non-Cambridge block groups from an OLS regression of Cambridge block group RCI on 1990 Cambridge Census block group characteristics. All specifications include the controls used in column 1 of Table 9, except that geographic fixed effects are included at the level of 1990 Census tracts rather than block groups. Panel III additionally includes a full set of city by year effects. Robust standard errors clustered by 1990 block group are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 11. Descriptive Statistics for Cambridge Residential Building Permitting Activity, 1991 through 2004
Permits Issued and Permitted Expenditures

	Houses				Condominiums			
	Never Controlled		Decontrolled		Never Controlled		Decontrolled	
	1991-1994	1995-2004	1991-1994	1995-2004	1991-1994	1995-2004	1991-1994	1995-2004
	I. Permits Issued							
Number of Permits	1,507	4,385	259	694	247	852	185	672
Mean Annual Fraction of Units Permitted	0.030	0.035	0.029	0.031	0.014	0.019	0.011	0.016
Mean Units in Permitted Structures	1.72	1.72	2.54	2.81	12.06	10.95	15.69	16.34
	II. Aggregate Expenditure (1,000s of 2008 dollars)							
Mean Annual Total Expenditure	14,044	29,954	1,588	3,486	3,723	7,595	1,451	4,435
Mean Annual Expenditure per Unit	1.11	2.37	0.72	1.57	0.82	1.67	0.34	1.05
	III. Annual Expenditure per Permitted Unit (1,000s of 2008 dollars)							
Mean	37.3	68.3	24.5	50.2	60.3	89.1	31.4	66.0
Standard Deviation	164.5	178.0	46.8	105.6	190.2	338.4	118.1	269.6
Median	10.3	18.0	8.3	13.8	12.4	19.3	11.2	19.2
Min	0.1	0.1	0.4	0.3	0.5	0.3	0.4	0.4
Max	5,675.5	4,365.5	451.2	1,208.9	2,121.2	6,589.3	1,480.1	4,450.3

Data source is the universe of Cambridge Inspectional Services permits issued during 1991 through 2004. We count all permits issued to a given structure in a given year as a single permit and sum their expenditures. When calculating units permitted or expenditures per unit in a year, we attribute the structure's permitted status and expenditures to only one unit. Expenditures are converted to real 2008 dollars using the Consumer Price Index for All Items Less Shelter for All Urban Consumers, Series Id: CUUR0000SA0L2, Not Seasonally Adjusted.

Table 12. Observed and Counterfactual Changes in Assessed Values of Decontrolled and Never-Controlled Units, 1994 to 2004 (in millions of 2008 dollars)

	1994 Assessed (mil$)	2004 Assessed (mil$)	2004 Counter-factual (mil$)	Rent Decontrol Impact (mil$)	% of Appre-ciation Due to Decontrol
			I. Never Controlled Units		
Houses	$2,961	$7,320	$6,500	$820	19%
Condominiums	$1,017	$2,699	$2,504	$195	12%
All	$3,978	$10,019	$9,004	$1,015	17%
			II. Decontrolled Units		
			Direct Decontrol Effect Only		
Houses	$267	$760	$722	$38	8%
Condominiums	$518	$1,746	$1,219	$527	43%
All	$785	$2,506	$1,941	$565	33%
			Adding Spillover Effects		
Houses	$267	$760	$603	$157	32%
Condominiums	$518	$1,746	$1,134	$612	50%
All	$785	$2,506	$1,737	$769	45%

Assessed values are from the 1995 and 2005 Cambridge Assessor's databases, reflecting property valuations as of 1994 and 2004, respectively. Counterfactual log property values are estimated separately for houses and condos using the specification in column 5 of Table 4. Counterfactuals for never-controlled units subtract Non-RC x RCI x Post effects and counterfactuals for decontrolled units subtract RC x Post and RC x RCI x Post effects from actual log property values. Aggregate effects in 2008 dollars are calculated by summing exponentiated counterfactual log property values.

Table A1. Descriptive Statistics for Population and Residential Units and Structures for Various Geographies

	Mean	Std Dev	Min	Max	Median
			I. Census Blocks		
Area (sq miles)	0.01	0.02	0.00	0.53	0.00
1990 Census Population	135.05	162.71	0.00	2833.00	99.00
2001 Residential Units	62.77	58.71	0.00	441.00	45.00
1994 Rent Control Units	22.92	34.48	0.00	236.00	11.00
2001 Residential Structures	18.53	12.08	0.00	81.00	16.00
1994 Rent Control Structures	4.08	3.77	0.00	21.00	3.00
Count of Blocks			587		
			II. Census Block Groups		
Area (sq miles)	0.07	0.07	0.01	0.56	0.05
1990 Census Population	986.17	506.00	98.00	3093.00	836.00
2001 Residential Units	428.15	253.62	23.00	1418.00	387.00
1994 Rent Control Units	155.75	155.19	6.00	854.00	107.00
2001 Residential Structures	122.93	58.53	9.00	382.00	124.00
1994 Rent Control Structures	27.26	16.30	3.00	61.00	24.00
Count of Block Groups			89		
			III. Census Tracts		
Area (sq miles)	0.22	0.17	0.05	0.72	0.16
1990 Census Population	3144.73	1291.67	1736.00	7123.00	2650.00
2001 Residential Units	1291.68	510.60	336.00	2984.46	1244.07
1994 Rent Control Units	470.77	341.71	101.00	1534.00	379.50
2001 Residential Structures	365.00	149.06	117.00	860.00	338.50
1994 Rent Control Structures	80.90	30.41	27.00	156.00	73.00
Count of Tracts			30		
			IV. 0.2 mile radius		
Area (sq miles)	0.13	-	0.13	0.13	0.13
1990 Census Population	3160.48	1765.02	0.00	15796.90	2935.48
2001 Residential Units	1141.15	573.10	5.00	3427.54	1066.16
1994 Rent Control Units	422.34	330.59	0.00	1702.00	376.00
2001 Residential Structures	348.40	116.72	1.00	676.00	351.00
1994 Rent Control Structures	80.15	46.52	0.00	180.00	77.00
Count of Maplots			10,968		

Panels I through III provide statistics for all census geographics containing at least one assessed residential housing structure. Panel IV provides statistics for the universe of 0.2 mile radius geographies, centered at each residential housing structure.

Table A2. Descriptive Statistics for Transacted Properties

	Houses				Condominiums			
	Never Controlled		Decontrolled		Never Controlled		Decontrolled	
	1988-1994	1995-2005	1988-1994	1995-2005	1988-1994	1995-2005	1988-1994	1995-2005
log Price	12.84	13.26	12.59	13.03	12.56	12.81	12.20	12.57
	(0.69)	(0.74)	(0.67)	(0.67)	(0.51)	(0.55)	(0.56)	(0.55)
Total Rooms	9.16	9.40	10.24	10.27	4.77	5.03	4.40	4.41
	(3.33)	(3.43)	(3.57)	(3.67)	(1.53)	(1.91)	(1.60)	(1.55)
Bedrooms	4.05	4.10	4.56	4.61	2.00	2.12	1.68	1.75
	(1.69)	(1.72)	(1.80)	(1.85)	(0.78)	(0.96)	(0.70)	(0.81)
Bathrooms	2.77	2.81	2.93	2.91	1.57	1.63	1.17	1.24
	(0.94)	(0.95)	(0.87)	(0.85)	(0.67)	(0.75)	(0.44)	(0.52)
Interior sq. ft.	2363.41	2387.34	2408.88	2409.76	1202.67	1269.57	927.85	949.69
	(1131.25)	(1071.66)	(920.96)	(902.49)	(834.76)	(819.75)	(434.02)	(449.68)
Has Lot (y/n)	0.99	0.99	0.99	0.99	0.02	0.04	0.04	0.03
	(0.11)	(0.09)	(0.09)	(0.09)	(0.14)	(0.19)	(0.18)	(0.17)
Lot Size sq. ft.	4211.71	4253.09	3320.15	3462.02	113.24	157.66	191.18	151.38
	(3433.26)	(3437.64)	(1964.22)	(2031.41)	(1595.75)	(1145.06)	(1222.04)	(1148.19)
Year Built	1903.25	1903.31	1890.81	1892.71	1944.51	1935.16	1915.12	1916.42
	(36.93)	(37.81)	(24.67)	(24.94)	(44.72)	(45.58)	(27.94)	(30.86)
N	1,624	2,599	255	336	2,138	3,626	1,446	2,765

Sample includes Cambridge houses and condominiums transacted during 1988 through 2005. Sales price, winsorized by structure type to the 1st percentile, are converted to real 2008 dollars using the Consumer Price Index for All Items Less Shelter for All Urban Consumers, Series Id: CUUR0000SA0L2, Not Seasonally Adjusted. Has Lot indicates whether property has a non-zero lot size.

Table A3. Seemingly Unrelated Regression Estimates for Changes in Attributes of Transacted Houses Following Rent Control Removal

	Total Rooms (1)	Bathrooms (2)	Bedrooms (3)	Interior Sqft (10s) (4)	Lot Size Sqft (100s) (5)	ln(Age) (6)	χ^2 Test (row) (7)
				I. Models with Common RCI Effect			
Constant	7.26***	2.46***	3.16***	204.17***	23.40***	4.83***	
	(0.36)	(0.12)	(0.22)	(13.20)	(4.19)	(0.11)	
RC x Post	-0.16	-0.05	-0.00	1.53	1.62	-0.09	6.44
	(0.20)	(0.06)	(0.12)	(7.04)	(2.38)	(0.06)	(0.38)
RCI x Post	0.20	0.03	0.02	18.09	-0.44	0.04	3.13
	(0.46)	(0.14)	(0.28)	(15.83)	(5.35)	(0.13)	(0.79)
				II. Models with RC x RCI Interactions			
Constant	8.10***	2.46***	3.17***	204.17***	25.91***	4.76***	
	(0.38)	(0.12)	(0.22)	(13.20)	(4.46)	(0.10)	
RC x Post	-0.09	-0.04	0.03	2.87	2.09	-0.09	6.04
	(0.21)	(0.07)	(0.13)	(7.27)	(2.46)	(0.06)	(0.42)
Non-RC x RCI x Post	0.46	0.06	0.19	22.36	1.54	0.03	4.22
	(0.48)	(0.15)	(0.30)	(16.70)	(5.64)	(0.14)	(0.65)
RC x RCI x Post	-1.92	-0.24	-1.14	-18.13	-15.26	0.11	2.43
	(1.43)	(0.45)	(0.88)	(49.45)	(16.70)	(0.41)	(0.88)
H_0: No Spillovers	0.257	0.792	0.350	0.380	0.634	0.939	
H_0: Spillovers Equal	0.114	0.518	0.151	0.437	0.340	0.856	

N = 4,814. Table reports estimates from Seemingly Unrelated Regressions of characteristics of transacted houses on 1994 rent control status and Rent Control Intensity (RCI) calculated at the 0.20 mile radius. All specifications include main effects for RC, RCI or Non-RC x RCI and RC x RCI, year of sale dummies, structure type dummies, block-group fixed effects, and an indicator for whether year built was imputed. Column 7 reports Chi2(6) tests for the null hypothesis that the given row's coefficients are jointly equal to zero (with p-values in parentheses). Test for No Spillovers reports p-values from tests that RCI x Post or Non-RC x RCI x Post and RC x RCI x Post coefficients are zero. Test for Spillovers Equal reports p-values from tests that these latter two coefficients are equal. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A4. Seemingly Unrelated Regression Estimates for Changes in Attributes of Transacted Condominiums Following Rent Control Removal

	Total Rooms (1)	Bathrooms (2)	Bedrooms (3)	Interior Sqft (10s) (4)	Has Lot (5)	ln(Age) (6)	χ^2 Test (row) (7)
				I. Models with Common RCI Effect			
Constant	3.41***	1.50***	1.43***	91.64***	1.05***	2.01***	
	(0.17)	(0.07)	(0.08)	(7.34)	(0.02)	(0.09)	
RC x Post	-0.15**	0.03	-0.03	-2.67	0.02***	-0.55***	186.46
	(0.07)	(0.03)	(0.04)	(2.95)	(0.01)	(0.04)	(0.00)
RCI x Post	0.04	-0.19**	0.04	-4.50	-0.00	0.09	9.77
	(0.22)	(0.08)	(0.11)	(9.16)	(0.02)	(0.13)	(0.13)
				II. Models with RC x RCI Interactions			
Constant	3.40***	1.59***	1.49***	97.19***	1.04***	2.51***	
	(0.16)	(0.06)	(0.09)	(6.66)	(0.02)	(0.11)	
RC x Post	-0.19***	0.03	-0.05	-3.46	0.02**	-0.55***	180.96
	(0.07)	(0.03)	(0.04)	(3.02)	(0.01)	(0.04)	(0.00)
Non-RC x RCI x Post	-0.28	-0.17	-0.12	-9.62	0.01	0.02	17.86
	(0.26)	(0.10)	(0.13)	(11.09)	(0.03)	(0.15)	(0.01)
RC x RCI x Post	0.71*	-0.24	0.40**	6.54	-0.02	0.25	2.70
	(0.38)	(0.15)	(0.20)	(16.19)	(0.04)	(0.22)	(0.85)
H_0: No Spillovers	0.101	0.072	0.082	0.633	0.873	0.531	
H_0: Spillovers Equal	0.033	0.668	0.028	0.410	0.605	0.399	

N=9,975. Table reports estimates from Seemingly Unrelated Regressions of characteristics of transacted condominiums on 1994 rent control status and Rent Control Intensity (RCI) calculated at the 0.20 mile radius. All specifications include main effects for RC, RCI or Non-RC x RCI and RC x RCI, year of sale dummies, structure type dummies, block-group fixed effects, and an indicator for whether year built was imputed. Has Lot is a dummy variable for whether the transacted condominium had an accompanying lot. Column 7 reports Chi2(6) tests for the null hypothesis that the given row's coefficients are jointly equal to zero (with p-values in parentheses). Test for No Spillovers reports p-values from tests that RCI x Post or Non-RC x RCI x Post and RC x RCI x Post coefficients are zero. Test for Spillovers Equal reports p-values from tests that these latter two coefficients are equal. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A5. Comparison of Estimated Relationship between Rent Control Status, Rent Control Intensity, and Transacted Prices vs. Assessed Values for Units Transacted in 1994 and 2004
Dependent Variable: Log of Transacted or Assessed Price

	Transacted Prices			Assessed Values: Transacted Units		
	(1)	(2)	(3)	(4)	(5)	(6)
			I. Houses			
RC x Post	0.199	0.127	0.352**	0.114	0.059	0.194
	(0.124)	(0.125)	(0.163)	(0.078)	(0.081)	(0.137)
RCI x Post		0.606**			0.452**	
		(0.294)			(0.189)	
Non-RC x RCI x Post			0.736***			0.522***
			(0.278)			(0.193)
RC x RCI x Post			-0.539			-0.172
			(0.828)			(0.615)
N	685	685	685	652	652	652
			II. Condominiums			
RC x Post	0.163**	0.085	0.073	0.168**	0.133*	0.122*
	(0.072)	(0.068)	(0.063)	(0.071)	(0.074)	(0.069)
RCI x Post		0.512**			0.255	
		(0.200)			(0.201)	
Non-RC x RCI x Post			0.406			0.110
			(0.280)			(0.294)
RC x RCI x Post			0.709**			0.516
			(0.291)			(0.366)
N	937	937	937	7,897	7,897	7,897

Samples includes houses (Panel I) and condominiums (Panel II) that transacted in 1994 and 2004. Regression models follow column 3 of Table 6. In columns 4-6, the dependent variable is the assessed value of any unit that is on a map lot at which at least one unit transacted in the given year. The number of observations for houses is larger in columns 1-3 than in columns 4-6 because a unit may transact more than once per year. The number of observations for condominimums in columns 4-6 is larger than in columns 1-3 because condominium structures contain multiple units. We observe the market price for transacted units and the assessed price for all units in the structure but cannot determine which specific unit in a structure has transacted. In specifications that include RC, RCI, Non-RC x RCI or RC x RCI interacted with Post, main effects of these variables are also included but not tabulated. Robust standard errors clustered by 1990 block group are in parentheses. See notes to Table 6 for additional details. *** p<0.01, ** p<0.05, * p<0.1

Table A6. Relationship between Rent Control, Rent Control Intensity and Transaction Price, 1988 - 2005: Eliminating Units that Were Converted from their 1994 Structure Type and Transactions Financed by Subprime Lenders
Dependent Varaiable: Log Sale Price

	Houses			Condominiums		
	(1)	(2)	(3)	(4)	(5)	(6)
			I. Eliminating Converted Structures			
RC x Post	0.093**	0.104**	0.106**	0.076**	0.072**	0.065**
	(0.045)	(0.046)	(0.047)	(0.031)	(0.028)	(0.027)
RCI x Post	0.361***			0.029		
	(0.082)			(0.072)		
Non-RC x RCI x Post		0.389***	0.290**		-0.025	-0.153
		(0.091)	(0.136)		(0.107)	(0.146)
RC x RCI x Post		0.054	-0.034		0.149	0.195
		(0.282)	(0.297)		(0.154)	(0.180)
N	4,527	4,527	4,527	7,875	7,875	7,875
			II. Eliminating Subprime Lenders			
RC x Post	0.085**	0.096**	0.097**	0.095***	0.081***	0.071**
	(0.041)	(0.042)	(0.043)	(0.030)	(0.029)	(0.027)
RCI x Post	0.339***			0.152**		
	(0.079)			(0.073)		
Non-RC x RCI x Post		0.361***	0.268**		0.074	-0.014
		(0.086)	(0.126)		(0.086)	(0.130)
RC x RCI x Post		0.092	-0.013		0.317**	0.307
		(0.253)	(0.282)		(0.154)	(0.197)
N	4,706	4,706	4,706	9,772	9,772	9,772
Block Group FEs	y	y	y	y	y	y
Quadratic Tract Trends	-	-	y	-	-	y

Table reports estimates for houses and condominums separately following Table 9. Panel I drops transactions of units in structures converted from their 1994 structure type. Panel II drops transactions with mortgages from lenders identified by HUD as issuing subprime mortgages. Robust standard errors clustered by 1990 block group are in parentheses. See Table 9 notes for additional details. *** p<0.01, ** p<0.05, * p<0.1

Data Appendix

Assessor's Data

The Cambridge Assessor's Database delineates the universe of residential housing located on each "map-lot," which is Cambridge's internal parcel numbering system. We assembled Assessor's database for 1995 and 2005, which contain property valuations as of January 1 of the *prior* year (thus, we designate these files as "1994" and "2004" property assessments in the text). We obtained from the Cambridge Historical Commission and subsequently digitized bound copies of the 1995 Commitment Books, which contains the property type classification and assessed value of each Cambridge property, used for property tax purposes. We obtained a copy of the 2005 Assessor's Database in electronic form directly from the Cambridge Assessing Department. Unlike the 2005 data, the 1995 Commitment Books do not enumerate the number of units at each structure. In place of this enumeration, we use a file provided to us by Clifford Cook of the Cambridge Planning Department providing the count of units in each structure at each map-lot in 2001. To calculate the latitude and longitude of each map-lot, we merged a geocoded version of the 2008 Assessor's Database provided by the MIT GIS Laboratory. We identified structure type conversions by comparing the structure types assessed at each maplot in 1994 and 2004. The combined Assessor's files, augmented with structure counts and latitude and longitude data, comprise our residential structures file. For all assessment, transaction and investment data, we inflated nominal dollar values to 2008 dollars using the All Items Less Shelter CPI for All Urban Consumers, Series Id: CUUR0000SA0L2, Not Seasonally Adjusted, available from http://data.bls.gov/cgi-bin/srgate, last accessed May 2012.

Decennial Census Data

To determine the 1990 Census block, block group, and tract corresponding to each map-lot code, we used ESRI ArcMap and MassGIS ArcGIS shape-files containing Census geography boundaries, which allowed us to match map-lots to geographies by latitude and longitude. We obtained block group-level demographic data for the cities of Cambridge, Malden, Medford, and Somerville from the 1990 Census Summary Tape Files (STF-1 and STF-3), which enumerate detailed demographic and housing data by block group using either a 100 percent extract (STF-1) or 15 percent sample (STF-3) of the 1990 Census of Populations.

Rent Control Data

Our measure of rent control status and the geographic distribution of rent controlled properties is drawn from the Cambridge Rent Control Board's database of actively controlled properties as of 1994. This database was generously provided by David Sims of Brigham Young University, who obtained it from the City of Cambridge via an earlier Freedom of Information Act request. The Rent Control Board database lists the address and map-lot code of all structures that were actively rent controlled as of 1994. We designate a given map-lot as rent controlled for the purpose of our analysis if there are any actively rent-controlled units on the map-lot as of 1994.

Sales Data

Data on transactions of houses and condominiums from 1988 and 2005 in Cambridge, Malden, Medford, and Somerville are from the residential real estate sales database, which we purchased from the Warren Group, a commercial vendor which assembles data from town deed's offices. Since not all changes in ownership are conventional sales, we eliminated transactions that do not appear to be standard arms-length transactions, specifically: transactions where deeds are marked as coming from a foreclosure process or bearing a land court certification; transactions where the last name of the buyer or seller appears in the name of the party on the other side of the transaction; transactions involving the Cambridge Housing Authority or affordable housing entities such as Just-A-Start; properties where a seller and a buyer both buy and sell the same property from each other on the same day; property share transactions, identified as an individual being on the same side of multiple transactions of the same property on the same day or the same individual being on both sides of any transaction; and transactions where the buyer resells the property later the same day. We also removed transactions with zero total rooms or zero interior square footage and retain only one copy of any duplicate transaction (those with the same street address, sale date, and price). In cases where the year built field for a given transacted property was missing, we first attempted to fill it in with the listed year built from other transactions for that property. For the two percent of transactions where year built could not be identified, we imputed its value as the mean year built for its structure type. In regressions that include property characteristics, we include a dummy variable equal to one if the year built value was imputed. We excluded 359 rent controlled condominium sales made between November 21, 1989 and December 31, 1989, during which time a portion of the rent control statute limiting condominium conversions and sales was temporarily overturned by the Massachusetts court.[49] To reduce the influence of outliers, we winsorized sales prices by structure type to the first percentile for the entire sample. Thus, transaction prices are defined as $\tilde{p}_i = \max\left\{p_i,\ p_{s(i),(.01)}\right\}$, where p_i is the (real) reported sales price of property i, and $p_{s(i),(.01)}$ is the first percentile of housing sale prices for structure type $s(i)$.

Investment Data

To measure residential investments, we obtained from the Cambridge Inspectional Services Department a database of all residential building permits issued by the City of Cambridge between 1991 and 2005. For 389 permits missing expenditure amounts but containing information on the permit fee, we replaced the missing expenditure value with 100 times the 1% permit fee. We removed permits duplicate permits and permits that were not designated for residential properties, specifically those that mention a non-residential usage in the description field (e.g., business, office, tent, educational, store, mixed, commercial, research, etc.). Since the investment permit data do not contain map-lot

[49] See Massachusetts Supreme Judicial Court *406 Mass. 147*, detailed at http://masscases.com/cases/sjc/406/406mass147.html, last accessed May 2012. The City of Cambridge was able to quickly revise the rent control statute to comply with the court ruling while again limiting the conversion and sale of rent-controlled apartments.

codes, we pooled the Assessor's Database with the Cambridge Rent Control Board database and the 2001 structures file to form a crosswalk between address strings and map-lot codes. For permits lacking an address string that matched to a map-lot code, we matched the permit to the nearest map-lot code based on straight-line distance, calculated using the StreetMaps USA address locator in ArcGIS. To construct our investment analysis sample, we merged the investment data to the housing structures file using map-lot codes to determine rent control status, proximity to rent control, property type, and geographic location. We summed all permitted expenditures at a map-lot in a year to form an annual panel of residential map-lot codes containing total expenditure for each map-lot and winsorized real investment expenditures to the 99.5^{th} percentile for each structure type and year.

Cambridge City Census Data

To build a longitudinal panel database of all adult Cambridge residents for the years 1991 through 2000, we digitized the Cambridge City Census files from 1991 through 2000, obtained from the Cambridge Election Office, to form a comprehensive list of adult Cambridge residents containing for each resident full name, address, birth year, occupation, country of birth, and optionally, gender and political party. Cambridge collects and makes publicly available these data in accordance with Massachusetts law requiring each municipality to conduct an annual census for purposes of voter registration and state reimbursements. We contracted with Equifax, Inc., a major U.S. credit bureau, to match the names and addresses of the approximately 436,000 adult Cambridge residents (39,000 to 48,000 unique individuals per year) identified in the city census. Equifax provided a unique identification number for each queried Cambridge resident, which allowed us to link residents across years and addresses to identify individuals who remained at the same address in consecutive years, moved between Cambridge addresses, left Cambridge, or entered Cambridge from a non-Cambridge address. Because the City Census files do not contain map-lot codes, we matched the address of each resident to its map-lot using the crosswalk between map-lot code and address strings constructed for the investment analysis sample.

Abt Associates Survey Data

We analyze rent differentials at controlled relative to non-controlled units using data from an Abt Associates study (Finkel and Wallace, 1987) commissioned by the City of Cambridge to gather data on the characteristics of households living in rent-controlled housing. These data, provided to us by Clifford Cook of the Cambridge Planning Department, enumerate contract rent, rent-control status, tenant awareness of rent-control status, unit characteristics (bedrooms, bathrooms, total rooms, an indicator for elevator in building, and indicators for whether furnishings, heat, electricity, or water were included in the rent), zip code, and variables indicating unit condition for a sample of 906 units.

Merging Assessor, Transaction, Investment and Geographic Data

To form the analytic sample for assessments and transactions, we merged the housing structures file with the 1990 decennial census file by latitude and longitude and then merged the combined database to the Rent Control Board file according to map-lot code or, where necessary, street address. We calculated Rent Control Intensity (RCI) at each map-lot as the fraction of housing units within a given radius, according to longitude and latitude, that were rent controlled (excluding the map-lot's own rent control status from the calculation).

To append pair RCI information to transactions data, we merged the Warren Group data with the housing structures file by map-lot code. For transactions with a missing map-lot code or with a map-lot code that did not merge with the structures file, we queried the street address in Cambridge's online property database (http://www2.cambridgema.gov/fiscalaffairs/propertysearch.cfm) and recorded the online entry's map-lot code.

Theory Appendix

We ground our empirical analysis in a stylized spatial equilibrium model of the housing market, which considers the relationship between rent control and house prices.[50]

Neighborhoods. A city consists of $n = 1, ..., N$ neighborhoods. There is a continuum of locations in each neighborhood indexed by $\ell \in [0, 1]$. The pair (ℓ, n) refers to location ℓ in neighborhood n.

Landlords. Each location is owned by an absentee landlord who decides on the level of maintenance m. Maintenance includes inputs such as painting, upgrading, and repairs. These produce housing services according to the following increasing and concave technology

$$h = f(m).$$

While the model is static, we interpret housing services as a per-period flow variable. The price of housing services, p, is a per-period price.

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

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

The first-order condition for an interior solution implies that maintenance is an increasing function of the price of housing services. Denote this function as $m^* = m(p)$ where $m'(p) > 0$.

Residents. There are an infinite number of potential residents with preferences given by:

$$U(c, h) = \phi(c, h, \alpha) + A,$$

[50] Glaeser (2008) presents a recent survey of these models.

where ϕ is an increasing function of a composite commodity c and housing services h, and α is a taste parameter. A is the total level of amenities in the resident's neighborhood. The only heterogeneity in the model comes from differences in α, which governs residents' taste for housing versus consumption of other goods. The outside utility for a resident with parameter α is denoted by $\bar{U}_\alpha$. The price of housing at location ℓ in neighborhood n is denoted $p_n(\ell)$, so a resident who lives at (ℓ, n) faces the budget constraint

$$c + p_n(\ell)h_n(\ell) = y,$$

where y denotes income.

Amenities depend on various neighborhood attributes. To capture the most relevant dimensions for our study, we assume that amenities in neighborhood n depend on the level of maintenance and the types of residents in the neighborhood:

$$A_n = \int_0^1 \mathcal{A}(m_n(\ell), \alpha_n(\ell), \ell)d\ell.$$

$\mathcal{A}(\cdot, \cdot, \cdot)$ aggregates neighborhood maintenance and tastes into a single index of amenities. The equilibrium concept is based on spatial equilibrium, with free-entry and perfect mobility of residents.

Equilibrium definition. *An equilibrium is a triple $\langle \alpha_n(\ell), p_n(\ell), h_n(\ell) \rangle$ where $\alpha_n(\ell)$ is the type of the resident, $p_n(\ell)$ is the price, and $h_n(\ell)$ is the level of housing services for each neighborhood n and location ℓ such that*

- *each resident obtains at least their outside option,*
- *no resident wishes to move to another neighborhood or location within a neighborhood, and*
- *landlords maximize profits.*

Benchmark model. We impose particular functional forms to motivate our estimating equations and to keep the model tractable. For the supply side, assume that housing is produced by the 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 $\phi(c, h, \alpha) = c + \alpha \ln h$ and y is large enough for an interior solution, but not larger than $\bar{U}_\alpha$. The demand for housing decreases with price:

$$h = \alpha/p.$$

The quasi-linear specification implies that α is the total amount of income a resident spends on housing. This assumption is made for tractability, since housing demand is unlikely to be independent of income in practice. Let the function for amenities at location ℓ be $\mathcal{A}(m_n, \alpha_n, \ell) = \ln(m_n) + \beta\alpha_n$.

where $\beta > 0$ is the weight on neighbor type. This representation implies that within a neighborhood, all locations share amenities. For simplicity, suppose the economy has N different types of residents, where the taste heterogeneity is ordered from highest to lowest: $\alpha_1 > \ldots > \alpha_N > 1$.

Equilibrium without rent control

We consider a symmetric equilibrium where all α_n type residents live in neighborhood n. The indirect utility for a resident of neighborhood n is:

$$U_{\alpha_n} = (y - \alpha_n) + \alpha_n \ln \alpha_n - \alpha_n \ln(p_n(\ell)) + A_n.$$

Free-entry and perfect mobility of residents implies that in all locations ℓ in neighborhood n, each resident's utility is equal to $\bar{U}_{\alpha_n}$. Hence, the price of housing is:

$$\ln(p_n(\ell)) = \ln \alpha_n + \frac{1}{\alpha_n}(y - \bar{U}_{\alpha_n} - \alpha_n + A_n). \tag{4}$$

Since landlords optimally set the level of maintenance to the price of housing and because all residents of neighborhood n are type α_n, we have

$$\ln(p_n(\ell)) = \ln \alpha_n + \frac{1}{\alpha_n}\left(y - \bar{U}_{\alpha_n} - \alpha_n + \int_0^1 \ln(m_n(\ell))d\ell + \beta\alpha_n\right) = \ln(m_n(\ell)).$$

Symmetry among landlords implies that maintenance levels within a neighborhood are the same at each location location ($m_n(\ell) = m_n$), so that

$$\ln(\alpha_n) + \frac{1}{\alpha_n}\left(y - \bar{U}_{\alpha_n} - (1-\beta)\alpha_n + \ln(m_n)\right) = \ln(m_n).$$

This relationship captures the feedback between overall maintenance in the neighborhood and location specific maintenance choices. The maintenance levels in the uncontrolled economy m_n^u are

$$\ln(m_n) = \frac{\alpha_n}{\alpha_n - 1}\left(\ln(\alpha_n) + \frac{1}{\alpha_n}(y - \bar{U}_{\alpha_n} - (1-\beta)\alpha_n)\right) \equiv \ln(m_n^u),$$

and prices are identical at all locations ℓ within the neighborhood. Using the expression for the level of maintenance, the price of housing p_n^u in neighborhood n in the economy without rent control is:

$$\ln(p_n) = \frac{\alpha_n}{\alpha_n - 1}\left(\ln(\alpha_n) + \frac{1}{\alpha_n}(y - \bar{U}_{\alpha_n} - (1-\beta)\alpha_n)\right) \equiv \ln(p_n^u).$$

The pricing relationship illustrates intuitive patterns under our parameter assumptions ($\alpha_n > 1$ and $\bar{U}_{\alpha_n} > y$). Prices are higher in neighborhoods where residents spend more of their income on housing, they are higher when residents have more income, and they are lower when residents have better outside options.

Equilibrium with rent control

Let RC_n denote the set of rent controlled locations in neighborhood n. Suppose that a fraction λ_n of locations are rent controlled and $1-\lambda_n$ are not. We first examine the pricing and maintenance decisions at controlled locations.

Rent controlled locations. Suppose the rent control authority sets prices at controlled locations $\bar{p}_n(\ell)$, and we assume that for each controlled location, the controlled price is less than the corresponding price in the uncontrolled economy, $\bar{p}_n(\ell) < p_n^u$.

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

$$\bar{m}_n(\ell) = \bar{p}_n(\ell).$$

In turn, the amount of housing services at location ℓ is given by:

$$h_n(\ell) = f(\bar{m}_n(\ell)) = \bar{p}_n(\ell).$$

Uncontrolled locations. Spatial arbitrage determines the prices of uncontrolled locations and, hence, the arbitrage relation in equation (4) determines prices. Since $\bar{m}_n(\ell) = \bar{p}_n(\ell)$ and landlords are symmetric at uncontrolled locations, the level of amenities in the controlled economy is:

$$A_n^c = \int_{\ell \in RC_n} \ln(\bar{p}_n(\ell)) d\ell + (1-\lambda_n)\ln(m_n) + \beta \int_0^1 \alpha_n(\ell) d\ell.$$

As with the uncontrolled economy, we focus on the equilibrium where $\alpha_n(\ell) = \alpha_n$ for all uncontrolled locations ℓ. This yields:

$$A_n^c = \int_{\ell \in RC_n} \left(\ln(\bar{p}_n(\ell)) + \beta\alpha_n(\ell)\right) d\ell + (1-\lambda_n)(\ln(m_n) + \beta\alpha_n).$$

In practice, the price of all controlled locations in neighborhood n differs at each location, so we cannot further simplify the first term. The second term, $\alpha_n(\ell)$, for controlled locations, depends on the way that residents are assigned to controlled housing. Let

$$\lambda_n \kappa_n^1 \equiv \int_{\ell \in RC_n} \ln(\bar{p}_n(\ell)) d\ell \quad \text{and} \quad \lambda_n k_n^2 \equiv \int_{\ell \in RC_n} \alpha_n(\ell) d\ell.$$

Since $\bar{p}_n(\ell) < p_n^u$, it is clear that

$$\kappa_n^1 < \ln(p_n^u).$$

While we do not explicitly model how residents are assigned to controlled housing, we assume that

$$\kappa_n^2 \leq \alpha_n,$$

which implies that the rationing mechanism imposed by rent control yields misallocation relative to the equilibrium in the uncontrolled economy.[51] The basis for this assumption is the following. If, prior to the implementation of rent control, the allocation were as in the symmetric equilibrium without rent control above, then once rent control is implemented, maintenance levels and hence housing services fall at controlled units (since landlords choose maintenance levels facing a regulated price). The combination of reduced rents and lower maintenance has one of two effects on incumbent residents: either they are sufficiently compensated by reduced rents so that they remain at their current locations, although the bundle of maintenance and amenities is not optimized for their tastes; or alternatively, they choose to relocate to areas with higher amenities and higher rents. In the latter case, they will be replaced by residents who prefer lower housing services, i.e., lower types. The average taste for housing services at controlled locations will therefore weakly decline following the imposition of rent control.

As a result, amenities in neighborhood n in the presence of rent control are given by:

$$A_n^c = \lambda_n(\kappa_n^1 + \beta\kappa_n^2) + (1-\lambda_n)(\ln(m_n) + \beta\alpha_n).$$

To compute the level of maintenance in uncontrolled locations in the presence of rent control, we follow similar steps to find:

$$\ln(m_n^c) \equiv \frac{\alpha_n}{\alpha_n + \lambda_n - 1}\left[\ln(\alpha_n) + \frac{1}{\alpha_n}(y - \bar{U}_{\alpha_n} - \alpha_n + \lambda_n(\kappa_n^1 + \beta\kappa_n^2) + (1-\lambda_n)\beta\alpha_n)\right].$$

We can write this in terms of the level of maintenance at uncontrolled locations in the economy without rent control:

$$\ln(m_n^c) = \frac{\alpha_n - 1}{\alpha_n + \lambda_n - 1}\ln(m_n^u) + \frac{\lambda_n}{\alpha_n + \lambda_n - 1}(\kappa_n^1 + \beta(\kappa_n^2 - \alpha_n)).$$

Summing up, since neighborhood amenities are a function of the maintenance of all units in a neighborhood and the preferences of their residents for housing services, the supply of amenities at non-controlled locations in neighborhoods with rent controls—as well as maintenance and rents—are also impaired by rent control. This causes lower types to move into non-controlled locations. Hence, imposition of rent control causes inefficiently low maintenance and misallocation of residents at both controlled and non-controlled locations within a neighborhood.

[51] See, e.g., Suen (1989) for a canonical model of rationing in the presence of price controls. Bulow and Klemperer (2012) further investigate how consumer surplus is impacted by rationing and develop a model of rationing with rent-seeking.

The effect of rent control removal on rents, maintenance and resident allocation

Consider finally the impact of the removal of rent control on prices at uncontrolled locations. To form this comparative static, we compare price levels in the economy without and with rent control:

$$\begin{aligned}\Delta \ln(p_n(\ell)) = \ln(p_n^u(\ell)) - \ln(p_n^c(\ell)) = \frac{1}{\alpha_n}\Delta A_n &= \underbrace{\frac{1}{\alpha_n}[A_n^u - A_n^c]}_{\text{change in amenities}} \\ &= \frac{\lambda_n}{\alpha_n + \lambda_n - 1}\underbrace{[\ln(m_n^u) - \kappa_n^1]}_{\text{maintenance effect}>0} + \frac{\beta\lambda_n}{\alpha_n + \lambda_n - 1}\underbrace{[\alpha_n - \kappa_n^2]}_{\text{allocative effect}>0}. \end{aligned} \tag{5}$$

This expression shows that the end of rent control generates price impacts through two channels. Under rent control, maintenance is inefficiently low and there are allocative inefficiencies due to the assignment of residents at controlled locations. This expression also illustrates three natural comparative statics. When a neighborhood has a higher fraction of locations that are controlled (λ_n increases), the change in prices for locations without rent control increases. As κ_n^1 increases (as would be expected when the prices of controlled locations are further depressed from their market values), the change in the price of uncontrolled locations due to the elimination of rent control also increases. Moreover, when there is greater misallocation due to the rent control (κ_n^2 decreases), the elimination of rent control further increases prices.

The price impact due to the end of rent control for locations that are controlled involves an additional term which can be decomposed as follows:

$$\ln(p_n^u(\ell)) - \ln(\bar{p}_n^c(\ell)) = \left(\underbrace{\ln(p_n^u(\ell)) - \ln(p_n^c(\ell))}_{\text{amenity effect}}\right) + \left(\underbrace{\ln(p_n^c(\ell)) - \ln(\bar{p}_n^c(\ell))}_{\text{decontrol effect}}\right). \tag{6}$$

The first term, the *amenity effect*, is the price change for uncontrolled locations due to the end of rent control, which is in turn due to maintenance and allocative effects as in equation (5). The second term, the *decontrol effect*, is the price change in a controlled economy going from a rent controlled location to an uncontrolled location. For a formerly controlled location, the direct effect of the end of rent control is larger when the controlled price at the location is further depressed. The following proposition summarizes the relevant considerations from this model:

Proposition 1 *When rent control ends, the price change for uncontrolled locations is greater for neighborhoods*

- *with a larger fraction of locations with rent control* ($\lambda \uparrow$),
- *where the price of controlled locations is further depressed from their market price* ($\kappa^1 \downarrow$),
- *where there is greater misallocation of resident types relative to the types in the uncontrolled economy* $\kappa^2 \downarrow$).

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

This model guides our analysis of the data, but its simplicity also imposes some limitations for our setting. First, the price of housing services is an abstraction that allows for no distinction between house prices and rents, which might be especially relevant in a dynamic setting. The model does not therefore allow for realistic dynamics to capture expectations of neighborhood appreciation and the option value of ownership. Second, amenities within a neighborhood are assumed to be pure public goods, so residents have no desire to substitute between locations within a neighborhood. If housing services were instead differentiated, there might be substitution between different locations within a neighborhood. In this case, new construction stimulated by the end of rent control might have a price impact at nearby uncontrolled housing (due to increased housing supply). Finally, the model only allows for a restrictive form of taste heterogeneity: within a neighborhood, all residents at uncontrolled locations (though not generally at controlled locations) have the same taste parameter α and, due to spatial arbitrage, obtain the same utility.