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

Author(s): David H. Autor, Christopher J. Palmer, and Parag A. Pathak
Source: *Journal of Political Economy*, Vol. 122, No. 3 (June 2014), pp. 661-717
Published by: The University of Chicago Press
Stable URL: http://www.jstor.org/stable/10.1086/675536
Accessed: 10/07/2014 11:41

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

David H. Autor

Massachusetts Institute of Technology and National Bureau of Economic Research

Christopher J. Palmer

Massachusetts Institute of Technology

Parag A. Pathak

Massachusetts Institute of Technology and National Bureau of Economic Research

We measure the capitalization of housing market externalities into residential housing values by studying the unanticipated elimination of stringent rent controls in Cambridge, Massachusetts, in 1995. Pooling data on the universe of assessed values and transacted prices of Cambridge residential properties between 1988 and 2005, we find that rent decontrol generated substantial, robust price appreciation at decontrolled units and nearby never-controlled units, accounting for a quarter of the $7.8 billion in Cambridge residential property appreciation during this period. The majority of this contribution stems from induced appreciation of never-controlled properties. Residential investment explains only a small fraction of the total.

We thank seminar participants at Berkeley, the Boston Fed, Columbia, Harvard, Massachusetts Institute of Technology, New York University, Stanford, Virginia, and the National Bureau of Economic Research Summer Institute on Local Public Finance and Real Estate and our discussants, Erzo Luttmer and Jaren Pope, as well as the editor of this *Journal* and the referees for comments and suggestions that greatly improved the paper. We are grateful to David Sims for assistance with Cambridge Rent Control Board data and to Norma Coe, Cliff Cook, Bill Cunningham, Lisa Sweeney, and the staff at the Cambridge Assessor's Office for

[*Journal of Political Economy,* 2014, vol. 122, no. 3]

I. Introduction

Spillovers from the attributes and actions of neighborhood residents onto the value of surrounding properties and neighborhoods are central to the theory of urban economics and the development of efficient housing policy (Fujita 1991; Glaeser and Gyourko 2009). Credibly identifying and quantifying these external effects, however, pose 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 all determined simultaneously in equilibrium.[1]

This paper exploits an unusual, large-scale policy change, the elimination of rent control in Cambridge, Massachusetts, in 1995, to quantify the capitalization of residential housing market externalities onto the value of residential real estate. From December 1970 through 1994, all rental units in Cambridge built prior to 1969 were 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 40-plus percent below the price of nearby noncontrolled properties, though maintenance and amenities in controlled units tended to be subpar (Sims 2007).[2]

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 51–49 percent margin, with nearly 60 percent of Cambridge residents voting to retain the rent control ordinance. Thus, rent decontrol in Cambridge, which commenced only 2 months after the November 1994 referendum, was voted into law by Massachusetts cities and towns that had never experienced rent control, while, ironically, the three Massachusetts municipalities with active rent control regimes—Cambridge, Boston, and Brookline, each of

invaluable access to expertise and data. We acknowledge generous support from the Alfred P. Sloan Foundation, the Lincoln Institute for Land Policy, the National Science Foundation (grant SES-962572), and the Rappaport Institute for Greater Boston. Palmer thanks the National Science Foundation Graduate Research Fellowship (grant 0645960). We received excellent research assistance from Andrew Garin, Annalisa Scognamiglio, Karen Scott, Yuqi Song, Barrett Strickland, Daniel Sullivan, Thiago Vieira, Melanie Wasserman, and a hardworking team of MIT undergraduate data sleuths. Data are provided as supplementary material online.

[1] See Kasy (2013) for a recent discussion of the nonparametric identification in location choice models with social externalities.

[2] Using microdata from a 1987 Abt Associates study commissioned by the City of Cambridge (Finkel and Wallace 1987), we estimate that quality-adjusted rents were approximately 44 percent lower at controlled units than at observably similar noncontrolled units.

which voted to maintain rent control—were overruled by the statewide majority.[3]

Alongside its swift and largely unanticipated elimination, two unusual features of Cambridge's rent control ordinance make it well suited to credibly identify the effects of rent control on residential housing markets. First, because the rent control ordinance applied to only a fixed, nonexpanding set of residential units—specifically, non-owner-occupied rental houses, condominiums, or apartments built prior to 1969—controlled and never-controlled units stood side by side in Cambridge neighborhoods on the eve of rent control removal, thus offering a tight temporal and geographic framework for assessing the impact of the law on residential property prices.[4] Second, although roughly a third of residential units were controlled prior to elimination (see fig. 1), this fraction frequently exceeded 60 percent in neighborhoods that had older housing stocks and a substantial share of renters at the time of rent control's enactment in 1970. This sizable cross-neighborhood variation allows us to assess localized price effects by comparing pre- and postremoval price appreciation among both decontrolled and never-controlled properties in neighborhoods that differed in their "rent control intensity," that is, the share of residential units that were controlled.

Our conceptual model and empirical work distinguish two channels through which rent decontrol may affect the market values of residential properties. The first, which we term the direct effect, reflects the capitalization of landlords' newfound ability to charge market rents. In the absence of any change in residential investments or neighborhood characteristics—and assuming that price controls were binding—rent control removal should directly raise the ownership value of formerly controlled properties by uncapping rents and, simultaneously, increasing the returns to landlord investments. The second channel, which we term the indirect effect, encompasses the multiple complementary mechanisms by which rent decontrol may affect the desirability of surrounding properties: owners renovate and modernize decontrolled units, raising their rental values; affluent tenants who particularly value these amenities rent these units as incumbents depart in the face of rising prices; higher-income tenants move into nearby never-controlled properties, attracted by the amenities of an improved housing stock and more affluent neighbors; and property owners make further investments in both decontrolled and never-controlled units as overall tenant income levels rise.[5]

[3] As discussed in Sims (2007), the Boston and Brookline rent control regimes were far less comprehensive than in Cambridge.

[4] 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 were often arranged discreetly to avoid the notice of the Rent Control Board.

[5] Increases in residential investment after rent decontrol do not divide cleanly into direct or indirect effects: in the absence of spillovers, decontrol should raise the return to

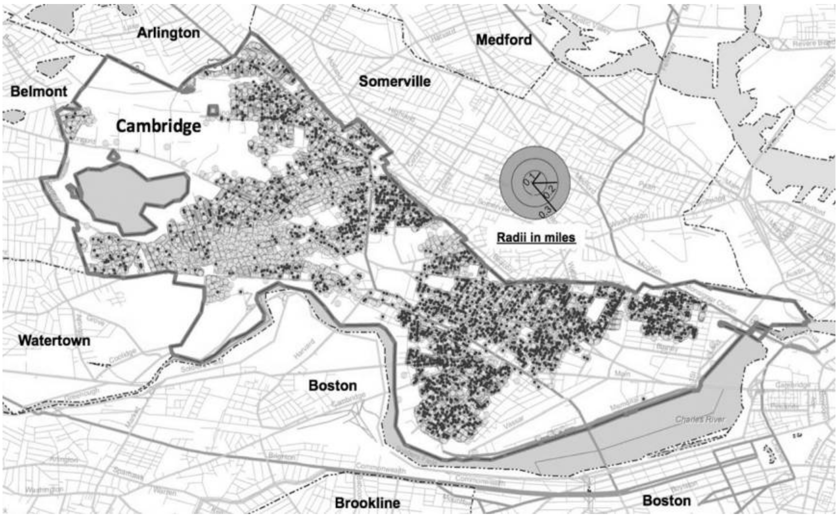

FIG. 1.—The geography of residential properties and rent control in Cambridge, Massachusetts. Cambridge residential properties as of 2008 are marked with gray circles. Maplots that were rent controlled as of 1994 are overlaid with black circles. Concentric circles in the top right depict radii of 0.1, 0.2, and 0.3 mile.

Distinct from the "direct" effect of decontrol, which by definition operates only on formerly controlled properties, the indirect channel may affect the market value of both decontrolled and never-controlled properties by increasing the desirability of the neighborhoods in which they are located. While our analysis does not allow us to further decompose the indirect effect into its constituent components (investment, reallocation, and the complementarities between the two), historical evidence suggests that each of these channels was relevant. Because Cambridge's Rent Control Board was unlikely to grant rent increases following property improvements, it was widely perceived that rent control muted owners' incentives to maintain and improve controlled properties.[6] Consistent with this view, Sims (2007) finds that chronic maintenance problems—such as holes in walls or floors, chipped or peeling paint, and loose railings—were more prevalent in controlled than in noncontrolled units during the rent control era and that this differential fell substantially with rent control's elimination. The end of rent control also spurred substantial

renovations and repairs of ill-maintained decontrolled units (a direct effect); the complementarities among tenant incomes, neighborhood amenities, and the quality of the housing capital stock should raise the return to investments at both decontrolled and never-controlled units (an indirect effect).

[6] 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.

tenant turnover. Cambridge's rent control law was intended to enable less affluent tenants to reside in units that would command high rents under a market allocation, particularly 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.[7] As we show below, exit rates from formerly controlled units spiked in the years immediately following rent decontrol. And given the substantial accompanying increases in rents, it is likely that the new cohorts of renters were significantly more affluent than the tenants they replaced. Our analysis will capture the net effect of these potentially mutually reinforcing channels on the market value of Cambridge residential real estate.

Regulations are widespread in housing markets, and rent controls are arguably among the most important historically (Friedman and Stigler 1946; Glaeser and Gyourko 2009). Because they directly manipulate the price mechanism, they are likely to reshape the allocation of residents to locations, the incentives for investment and maintenance of controlled units, and the supply, demand, quality, and allocation of units in the noncontrolled sector. The modern era of US 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 2013). While the prevalence of rent control as a housing market policy has decreased since this period, rent control and rent stabilization plans are still in place in many US and European cities (Arnott 1995). New York City's system of rent regulation affects at least 1 million apartments, while cities such as San Francisco, Los Angeles, Washington, DC, and several California and New Jersey cities 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 housing services (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

[7] A 1998 study commissioned by the City of Cambridge found that sitting residents of formerly controlled units had mean annual earnings in 1997 of \$35,650 vs. \$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.

housing, leading 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 affects the expected change in prices.

Sims (2007) undertakes the first empirical analysis of the end of rent control in Massachusetts, exploring its impacts on the supply of rental properties and their rental prices. Sims shows that the elimination of rent control spurred substantial rent increases in Massachusetts towns that had binding rent control laws in 1994 (Boston, Brookline, and Cambridge) and led to significant increases in the quality and quantity of rental housing available. In contrast to Sims's 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 effects on decontrolled properties and never-controlled properties.[8]

Our work is also related to studies of neighborhood revitalization and gentrification, both of which may generate spillover benefits to surrounding areas (Ioannides 2003; Schwartz et al. 2006; Rossi-Hansberg, Sarte, and Owens 2010; Guerrieri, Hartley, and Hurst 2013). 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 (Hartley 2010; Campbell, Giglio, and Pathak 2011; Mian, Sufi, and Trebbi 2011).[9]

Our analysis draws on a uniquely detailed geographic and economic database sourced from Cambridge administrative records that enumerates 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 nonresidential uses (e.g., houses that were converted to condominiums), and the permitted investment expenditures at each residential location. We additionally use 10 years of Cambridge city census data to document the rapid turnover of residents of formerly controlled units following the end of rent con-

[8] 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.

[9] In addition, a number of papers present evidence that subprime mortgage lending may lead to price appreciation in neighborhoods where housing credit was historically in short supply (Mian and Sufi 2009; Landvoigt, Piazzesi, and Schneider 2012).

trol. These sources permit direct estimation of changes in residential real estate prices induced by rent decontrol.

We find compelling evidence that the elimination of rent control raised the market values of both decontrolled and never-controlled properties. Our main estimates imply that during the rent control era, rent-controlled properties were valued at a discount of about 45–50 percent relative to never-controlled properties with comparable characteristics in the same neighborhoods and that their assessed values rose by 18–25 percent relative to never-controlled properties following rent decontrol. This differential appreciation should primarily reflect the direct effect of rent decontrol on the market value of formerly controlled units generated by the potential for owners to charge market rents, the option to convert rental units into condominiums, and the flow of returns from associated capital investments.

To assess whether rent control density affected the desirability of neighborhoods over and above its direct effect on controlled properties, we next calculate a rent control exposure measure for each residential unit that is equal to the fraction of other residential units within a 0.20-mile radius that were subject to rent control as of 1994. A central finding is that post-decontrol price appreciation was significantly greater at units that had a larger fraction of formerly controlled neighbors: residential properties at the 75th percentile of rent control exposure gained approximately 13 percent more in assessed value following decontrol than did properties at the 25th percentile of exposure. This differential appreciation of properties in rent control–intensive locations was equally pronounced among decontrolled and never-controlled units, suggesting that rent control removal spurred overall gains in neighborhood desirability.

These findings are robust to many 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 transaction prices for all properties sold in Cambridge between 1988 and 2005, which provide an alternative source for measuring changes in market values, yield estimates of spillover effects comparable to those found using the assessor's data.

One channel through which the removal of rent controls may have raised Cambridge housing values is spurring additional capital investments. Using administrative data on residential expenditures permitted by the Cambridge Inspectional Services, we find that aggregate annual permitted building expenditures increased dramatically for both houses and condominiums after 1994, rising from $21 million per year between 1991 and 1994 to $45 million per year between 1995 and 2004. Moreover, 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 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 large, contributing \$2.0 billion of \$7.7 billion in Cambridge property appreciation in the decade between 1994 and 2004. Of this total effect, only \$300 million is accounted for by the direct effect of decontrol on formerly controlled units (holding exposure constant), while \$1.7 billion is due to the indirect effect. Notably, the majority of this indirect effect (\$1.1 of \$1.7 billion) stems from the differential appreciation of never-controlled units. When both direct and indirect effects are combined, our estimates imply that more than half (55 percent) of the capitalized cost of rent control was borne by owners of never-controlled properties.

The paper proceeds as follows. Section II provides additional details on the enactment, enforcement, and removal of rent control in Cambridge. Section III describes a simple model of housing markets in the presence of rent control to guide our empirical analysis (App. A contains the model). Section IV describes data sources and our empirical strategy. Section V presents our main results using property assessments, while Section VI presents results on the time path of the capitalization of rent decontrol using transaction prices. Section VII reports on our investigation of permitting and investment activity, and Section VIII considers economic magnitudes. We conclude with a discussion of areas for further investigation.

II. Cambridge Rent Control: Enactment, Enforcement, and Removal

A. *Rent Control Adoption and Elimination*

In 1970, the Massachusetts state legislature enacted a statute allowing cities and towns with populations over 50,000 to implement rent control to "alleviate the severe shortage of rental housing."[10] 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. Lynn repealed its plan in 1974 and Somerville in 1979. Boston allowed for decontrol of vacant units in 1976, and Brookline

[10] Quoted from An Act Enabling Certain Cities and Towns to Control Rents and Evictions, 1970 Mass. Acts 842.

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).[11] 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 nonresidential structures converted to rental properties after this time. Oversight of the rent control law rested with the Cambridge Rent Control Board, whose official charter 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 1980s, 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.[12] Landlords could also apply to raise prices above the scheduled increases, but these variances were rarely sought or granted in practice, in part because the application required supporting petitions, extensive legal documentation, and significant time investment.[13]

Distinct from many 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 moved out. Landlords therefore faced an incentive to remove units from the rental stock, which they accomplished by converting substantial numbers of rental units to condominiums and selling them to owner-occupants. To prevent the controlled rental stock from being depleted, in 1979 the city council passed the Removal Permit Ordinance, which substantially restricted the removal of controlled units from the rental stock and complicated the conversion of controlled units into owner-occupied condominiums.[14]

The development that ultimately led to rent control's elimination was the Cambridge Small Property Owners Association's successful effort to

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

[12] All documents from the Rent Control Board are available in the archives of the Cambridge Historical Commission.

[13] 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).

[14] 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, which were made salient by the fate of the so-called condo martyrs: owners who were prosecuted for occupying their own controlled properties before the completion of a conversion.

place rent control on the statewide ballot in 1994. Putting rent control to a statewide vote diluted the strong support that rent control enjoyed in the three municipalities with extant rent control ordinances (Boston, Brookline, and Cambridge). Rent control was eliminated by a slim 51–49 percent margin in November 1994, despite nearly 60 percent of Boston, Brookline, and Cambridge voters voting to retain the current regime. Just 2 months later in January 1995, a majority of properties were decontrolled. 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 2 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.[15]

B. *The Post-decontrol Regime*

The elimination of rent control catalyzed a series of rapid changes in the Cambridge rental market, beginning with rising rents. A 1998 survey commissioned by the City of Cambridge (Atlantic Marketing Research 1998) 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 noncontrolled units. Median rents rose by only 13 percent for sitting tenants of never-controlled units in the same time period.

Rising rents spurred a sharp increase in resident turnover at formerly controlled units after 1994, which we document by constructing a panel of all Cambridge adults aged 17 and older by street address using city voter registration records for the years 1991–2000.[16] On average, 26.9 percent of Cambridge residents changed locations annually, 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).

We assess whether turnover rates at formerly controlled units rose differentially after 1994 by fitting linear probability models of the following form:

[15] Shortly after the referendum, the state legislature adopted a bill extending rent control for 5 years. The governor vetoed this bill and later signed an alternative on January 3, 1995, that granted rent control extensions of 1 year (2 years if the rental building had more than 12 units) to renters whose incomes were below 60 percent of the median for the Boston metropolitan statistical area (or 80 percent 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 percent of tenants were eligible to apply.

[16] State law (Massachusetts General Laws, chap. 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 online App. B.

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

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 onward. Prior to 1995, residents of controlled units were not significantly more likely to turn over than residents of noncontrolled units.[17] Following decontrol, the turnover differential between formerly controlled and never-controlled units rose by 5.4 percentage points, with an even larger increase at condominiums (table 1).

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 year dummies. Turnover rates at decontrolled units spiked by 4 percentage points relative to never-controlled units in the first year of decontrol and continued to climb to 10 percentage points over the next 3 years. Thus, the process of resident reallocation and neighborhood change spurred by decontrol took multiple years to unfold. Interestingly, figure 2 also shows that turnover rates at never-controlled units changed little following decontrol.

A sharp increase in residential property investments also followed the end of rent control. The number of building permits issued per residential unit for improvements and new construction increased by approximately 20 percent after 1994, and annual permitted expenditures roughly doubled in real terms (see App. table A1). Elimination of the Removal Permit Ordinance allowed a substantial number of decontrolled houses, apartments, and nonresidential units to be converted to condominiums. From 1994 to 2004, Cambridge's stock of residential houses decreased by 6 percent, while the stock of condominiums increased by 32 percent, with 45 percent of this increase accounted for by conversion of houses to condominiums (App. table A2).[18] At the same time, the fraction of residential units available as rental properties rose by 6 percentage points (Sims 2007).

[17] Subsequent columns in table 1 reveal that this result is driven by composition. If we focus only on apartments and condominiums, residents of controlled units were significantly less likely to turn over than residents of noncontrolled units—consistent with the idea that controlled units had scarcity value. Residents of controlled houses, by contrast, were significantly more likely to turn over than residents of noncontrolled houses, but this likely reflects the fact that most noncontrolled houses were owner-occupied whereas controlled houses were renter-occupied.

[18] These calculations use the Cambridge assessor's databases from 1995 and 2005, reflecting the status of properties in 1994 and 2004, respectively. We count each unit in multifamily houses separately to meaningfully compare the supply of housing across different structure types and in different periods. The stock of units in houses in Cambridge decreased from 14,722 in 1994 to 13,861 in 2004 and the stock of condominiums rose from 7,220 to 9,561 units.

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	.269	.232	.297	.335
	(.197)	(.178)	(.209)	(.223)
RC	−.003	.073***	−.035**	−.056**
	(.008)	(.008)	(.016)	(.026)
RC × Post	.054***	.025***	.076***	.057**
	(.008)	(.008)	(.022)	(.025)
Observations	310,949	172,996	70,558	67,395

NOTE.—The dependent variable is an indicator equal to one if a resident was not present in the current unit in the 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.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

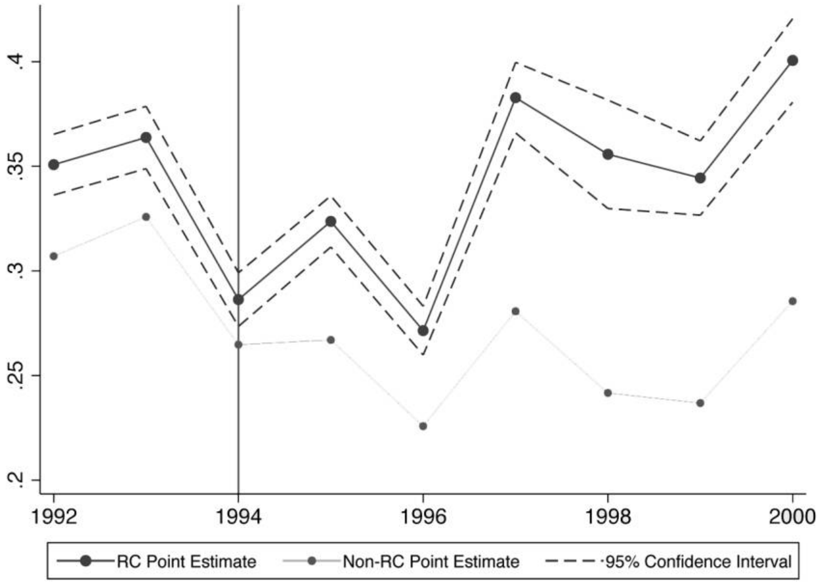

FIG. 2.—Residential turnover in Cambridge controlled relative to never-controlled units, 1992–2000. The figure plots coefficients on RC × Year variables from an event-study regression in which the dependent variable is an indicator equal to one if the resident was not present in the current Cambridge unit in the prior year (and zero otherwise). 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. The 95 percent confidence intervals are constructed from robust standard errors clustered by block group. The vertical line in 1994 indicates the year preceding rent control removal.

This combination of sizable rent increases, rapid turnover of incumbent renters, rising residential investment, and outward shifts in the supply of both condominiums and rental properties was 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.

III. The Direct and Indirect Effects of Rent Control

Appendix A presents a stylized model of the housing market, summarized here, that considers the relationship between rent control and prices of both controlled and noncontrolled properties. In the model, a city consists of N neighborhoods with a continuum of locations in each neighborhood. Potential residents choose locations to maximize utility defined over consumption of housing services, a nonhousing composite good, and local amenities. Residents have identical preferences and differ only in their income levels. Profit-maximizing landlords choose the level of maintenance at each location, and this level is increasing in the price of housing services.

We assume that amenities in a neighborhood depend on the housing maintenance levels and the income distribution of residents in the neighborhood, where higher maintenance and higher-income neighbors are also more desirable and hence contribute more to neighborhood amenities. This formulation creates positive feedback from the extent of maintenance, residents' income, and neighborhood amenities. In the free-market equilibrium (with no rent controls), rents are higher in neighborhoods with greater amenities as a result of higher maintenance and the presence of higher-income neighbors.

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 units in a neighborhood at below their free-market level. Since landlords choose maintenance levels facing a regulated price, maintenance levels and hence housing services are lower at controlled units. 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 income levels, 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, that is, those with lower incomes.[19] The average income at controlled locations therefore weakly declines following the imposition of rent control.

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

Since neighborhood amenities are a function of the maintenance of all units in a neighborhood and the neighborhood income distribution, the levels of amenities at noncontrolled locations in these neighborhoods—as well as maintenance and rents—are also impaired by rent control. This in turn causes lower-income residents to move into noncontrolled locations. Thus, rent control causes inefficiently low maintenance and misallocation of residents at both controlled and noncontrolled locations within a neighborhood.

Decontrol unwinds these effects. Prices rise directly because of the lifting of the cap and indirectly because of improved maintenance and increased production of local amenities throughout the neighborhood. At noncontrolled 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 larger resident misallocation 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 noncontrolled locations following decontrol reflect the capitalization into house values of two of these three forces: improved maintenance (or, more generally, housing investments) and greater neighborhood amenities (both due to sorting and capital improvements at other properties). Therefore, the increase in prices at noncontrolled locations, net of the additional resource costs expended on maintenance and improvements, can be used to assess the external effects of decontrol, that is, the spillovers. We quantify these spillovers below by estimating the increase in market value of never-controlled units and netting out the components plausibly attributable to investment.

IV. Data and Measurement

We briefly discuss our data sources and measurement of rent control intensity in this section, with further details in online Appendix B.

A. *Cambridge Real Estate*

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 data set 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.[20] 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 in which individuals are likely to live as a residential structure. Our analysis of assessed values and transactions is limited to houses and condominiums, which make up 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.[21] 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. Owing 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, our data analysis could erroneously detect spillovers that reflect nothing more than appreciation of formerly controlled units after decontrol. To be conservative, we code all units on a map-lot as rent controlled if any unit at that map-lot was controlled in 1994. It is therefore very unlikely that there are 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 are rent controlled.[22] This is also conservative in that it prevents us from overestimating units' exposure to other controlled properties.

[20] 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.

[21] While we filed our own FOIA request with the City of Cambridge, we ultimately utilized the file obtained by David Sims through an earlier FOIA request because its coverage appeared more complete.

[22] 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.

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 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.

We append two databases to analyze the impact of rent decontrol on market capitalization, the first enumerating property assessments and the second enumerating real estate transactions. The 1995 and 2005 Cambridge Assessor's Files, which report property valuations from 1994 and 2004, provide the assessed appreciation of each extant property from the year prior to rent decontrol to 9 years thereafter. The second is a commercial database provided by the Warren Group, which enumerates all changes in ownership of residential properties for the years 1988–2005. Sourced from records of deeds, these data log each real estate transaction, including sale price, address, map-lot, number of bedrooms and bathrooms, lot size, year built, and property type. 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.

Assessments and transactions provide complementary means to measure the capitalization of rent control's end. Assessments, our preferred measure, contain the universe of residential properties along with assessed market values at two points in time, immediately prior to rent control removal and 10 years later. Assessments may offer a lagging indication of residents' changing willingness to pay for locations, however, and could differ from market valuations because of discretionary aspects of the assessment process. The sales data, in contrast, include both market prices and a rich set of property characteristics for locations where transactions take place and, because they are available annually, provide a clearer picture of the trajectory of property price changes. Only a small percentage of residential units transact each year, however, and hence the sales data contain information on an incomplete and potentially nonrepresentative set of residential units (we subsequently analyze whether rent control affected the composition of transacted properties).

Table 2 presents descriptive statistics for the assessed Cambridge residential houses and condominiums used in our analysis, comprising 15,475

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

	Never Controlled		Decontrolled	
	1994	2004	1994	2004
	A. Houses			
Log value	12.72	13.65	12.56	13.61
	(.56)	(.55)	(.48)	(.45)
RCI	.30	.30	.34	.35
	(.15)	(.15)	(.14)	(.14)
Observations	7,426	7,145	829	839
	B. Condominiums			
Log value	12.36	13.10	11.66	12.77
	(.58)	(.46)	(.67)	(.38)
RCI	.32	.31	.45	.43
	(.19)	(.18)	(.14)	(.14)
Observations	3,602	4,921	3,618	4,600

Note.—The table reports means and standard deviations (in parentheses) of assessed values and 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.

properties in 1994 and 17,505 in 2004.[23] Slightly more than half of these properties are houses. Rent-controlled properties account for 29 percent of all residential properties, with condominiums making up the substantial majority. Because the vast majority of Cambridge houses were and are owner-occupied, only 12 percent of houses were ever subject to rent controls.[24] House prices rise substantially in real terms during our sample period: 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.[25] Houses typically have higher assessed values than condominiums, and in both periods, decontrolled houses and condominiums have lower values, on average, than never-controlled houses.

B. Measuring Rent Control Intensity (RCI)

Gauging each residential property's rent control exposure requires a metric that specifies which nearby units should be counted in the unit's reference set—that is, to which units it is "exposed"—and how the rent control

[23] Note that a property may contain multiple units, e.g., a multifamily house.

[24] The house and condominium designations in table 2 reflect the property's residential category at the time of assessment.

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

status of these reference units should be aggregated into an exposure index. For most analyses, 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 noncontrolled) in that geography:[26]

$$\mathrm{RCI}_{i(g)} = \frac{1}{J_g} \times \sum_{j \neq i}^{J_g} \mathrm{RC}_{j(g)}.$$

In a subsequent sensitivity analysis, we calculate each unit's rent control exposure as an exponentially declining function of its distance from all other controlled and never-controlled properties in the city.

The second input into the exposure measure is the choice of a surrounding geography. One potential set of geographies is supplied by the US 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 condominium.[27] While these predefined census geographies have the virtue of allocating Cambridge land parcels into exhaustive, mutually exclusive geographic units, they 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 is intrinsic to any allocation of geography into nonoverlapping 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. By contrast, for a unit located in the center of a geography, its 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

[26] 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. Each rental unit within a multifamily house is counted separately in both the numerator and denominator. The RCI determination for a condominium structure excludes all other units in that structure.

[27] These units have average land areas of 0.22, 0.07, and 0.01 square mile, respectively, in Cambridge; housed an average of 3,145, 986, and 135 residents in 1990; and contained a mean of 1,292, 428, and 63 residential units in 1990. Additional details on the size, population, and number of structures and units in census geographies are contained in table A3.

units within a fixed straight-line radius of 0.10, 0.20, and 0.30 mile of that unit that were controlled as of 1994. This radius exposure construct non-prejudicially selects the residential units that are physically closest to the reference unit.[28] 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 RCI measured at a radius of 0.20 mile, which corresponds to about 0.13 square mile—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-mile 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 mile is 17 percentage points, and the range of the RCI measure spans from 0 to 72 percent.

V. Capitalized Effects of Rent Decontrol: Evidence from Assessments

Our illustrative model suggests that the capitalization of rent decontrol should accrue through three channels: the direct effect on decontrolled properties of the elimination of price controls and condominium conversion restrictions and associated investments; the indirect effect of decontrol on the desirability of neighborhoods in which controlled properties were located, stemming from improvements in neighborhood amenities—for example, better upkeep, more desirable neighbors—and potentially affecting the market value of both decontrolled and never-controlled properties; and finally, broader increases in the desirability of Cambridge as a residential location, which may accrue citywide.

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

$$\begin{aligned}\log(Y^{A}_{igt}) = \gamma_g + \delta_t + \beta' X_i + \lambda_1 \cdot \mathrm{RC}_i + \lambda_2 \cdot \mathrm{RCI}_i \\ + \rho_1 \cdot \mathrm{RC}_i \times \mathrm{Post}_t + \rho_2 \cdot \mathrm{RCI}_i \times \mathrm{Post}_t + \epsilon_{igt},\end{aligned} \tag{2}$$

[28] Because we calculate RCI using only Cambridge properties, the radius-based RCI for properties close to the city's edge excludes nearby units that lie outside the city. To address this source of potential mismeasurement, we have verified that our findings are robust to discarding properties on all Cambridge block groups that directly abut the towns of Somerville, Arlington, Belmont, and Watertown (except those that border the Charles River, the sizable Mt. Auburn Cemetery, or the light rail system in the southeast of Cambridge).

where Y^A_{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. Our main specifications code "nearby" units as those within a 0.20-mile radius of a given property, but we subsequently explore alternative definitions. Recognizing that real estate prices of nearby properties are not independent, we generally cluster the standard errors at the level of Cambridge block groups.

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 citywide changes in residential real estate prices. The coefficient ρ_2 estimates the indirect effect of rent decontrol on the value of decontrolled and never-controlled properties by contrasting changes in the value of units 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. Finally, any effects of decontrol that accrue citywide—that is, are not limited to decontrolled properties or the neighborhoods in which they were located—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.[29]

For ρ_1 and ρ_2 to provide unbiased estimates of the direct and indirect effects of rent decontrol on the market value of residential properties, it must be the case that the elimination of rent controls—and resulting neighborhood-level changes—must not have been fully anticipated by households and landlords. This appears plausible in light of the fact that the rent control law was narrowly eliminated (51–49 percent) by a statewide referendum in which a large majority of Cambridge residents voted against rent decontrol.[30] Additionally, our identification requires that conditional on detailed geographic and time effects, the variable represent-

[29] We hesitate to interpret the coefficients on the RC main effect and RCI × RC (coefficients λ_1 and λ_2) 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 adopted in 1970 (e.g., the age of the residential housing stock and the fraction of nearby units that were owner-occupied vs. rented).

[30] To the degree that rent decontrol and any resulting neighborhood effects were foreseen by incumbent and potential owners, buyers, and renters, these effects would substantially

ing 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. In many instances, we also estimate a richer version of equation (2) in which we interact the RCI measure with both the rent control main effect and the RC × Post term. This triple-difference specification allows the indirect effect of rent control intensity to differ between controlled and never-controlled properties in both the rent control and decontrol eras.

The end of rent control in 1995 coincided with a period of nationwide house price appreciation, which raises the possibility of confounding price trends. The time effects δ_t in our estimating model will absorb these changes to the degree that they affect the overall price level of Cambridge housing. They will not absorb any differential appreciation in rent control–intensive neighborhoods, which might hypothetically occur if, for example, the US housing boom of the early 2000s spurred an influx of lending (and associated price appreciation) in rent control–intensive neighborhoods (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 the rate of appreciation to differ across census tracts.

A. *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, an 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 assessed value of controlled (RC) properties averaged 50 log points below the assessed value of never-controlled (non-RC) properties.[31] Following decontrol, this gap closed by 22 log

capitalize into values before rent control was removed, which would work against our finding either a direct or an indirect effect of rent decontrol on prices.

[31] 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. owner-occupied) as of 1971, which in turn are likely to be correlated with the fixed characteristics of the property, its mainte-

TABLE 3
Effects of Rent Decontrol on Assessed Values

	Dependent Variable: Log of Assessed Property Value (1994, 2004)			
	(1)	(2)	(3)	(4)
RC	−.504***	−.504***	−.515***	
	(.075)	(.052)	(.052)	
RC × Post	.217***	.227***	.249***	.221***
	(.039)	(.037)	(.034)	(.040)
Block group fixed effects	No	Yes	Yes	No
Tract trends	No	No	Yes	Yes
Map-lot fixed effects	No	No	No	Yes
R^2	.605	.759	.763	.938

Note.—$N = 32{,}980$. The 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 × Post dummies for each of 30 tracts from the 1990 census. Map-lot fixed effects are dummy variables for each of the 9,497 residential parcels in Cambridge. Map-lot fixed effects absorb the RC main effect in col. 4. Robust standard errors clustered by 1990 block group are in parentheses.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

points. Columns 2–4 refine the precision of the comparison by adding a set of dummy variables that sweep out cross-neighborhood differences in price levels and trends. Column 2 adds block group effects, which absorb average assessed values within narrow block groups (averaging 0.07 square mile). Here, the model is identified by contrasting the change in market value of decontrolled and never-controlled units within block groups. Column 3 adds tract-by-year dummies, thus allowing each of the 30 census tracts in Cambridge (averaging 0.22 square mile in area) to have a different overall appreciation rate. The final column includes a fixed effect for each residential location or map-lot (a total of 9,497 map-lots). This demanding specification, which absorbs the RC main effect, contrasts the map-lot-level change in assessed values between map-lots that contained controlled units and those that did not, again allowing for different price trends across 30 census tracts.

Across all specifications, the rent control main effect is highly robust and stable, demonstrating that decontrolled units appreciated substantially relative to never-controlled properties. These initial estimates do not distinguish, however, between the direct and indirect channels that

nance 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.

may jointly contribute to this appreciation. In particular, since decontrolled properties are typically located in neighborhoods with above-average levels of rent control intensity (table 2), the RC × Post term estimated above captures a combination of direct decontrol effects and indirect (micro neighborhood level) effects stemming from the greater desirability of formerly rent control–intensive locations. Our next set of estimates distinguishes these two effects.

B. *Direct and Indirect Effects of Rent Decontrol*

Table 4 augments the simple difference-in-difference models above with a measure of the rent control exposure of each residential property (denoted RCI and calculated using a 0.20-mile radius), as well as an interaction term between the RCI measure and a post-1994 indicator variable. This term measures the degree to which properties with greater rent control exposure saw differential appreciation following decontrol. The inclusion of the RCI and RCI × Post measures also changes the interpretation of the RC × Post main effect. Whereas previously this variable measured the differential appreciation of decontrolled versus never-controlled properties averaging (implicitly) across more and less rent control–intensive areas, the RC × Post coefficient in the augmented specification measures the differential appreciation of RC relative to non-RC properties in a hypothetical location with no other surrounding controlled properties (i.e., RCI = 0).

The base specification in column 1, which contains only year of sale and structure type dummies in addition to the RC and RCI terms, finds that properties with higher rent control exposure had lower value in the decontrol era and that this differential was substantially reduced in the 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 0.32 was assessed at approximately 19 log points below a property with zero exposure. We do not take the main effect of the RCI variable to be causal, however, since it is likely to be correlated with the many factors that determined which properties were controlled in 1971. Conversely, the coefficient of 0.33 on the RCI × Post indicator implies that 56 percent of this price differential was erased in the years after decontrol. Under our identifying assumption that these unobserved factors are quasi-fixed or are not spuriously correlated with rent control intensity across local areas, the RCI × Post interaction may be viewed as a causal estimate of the indirect effects of rent control on the market value of surrounding units (both formerly and never controlled). The fact that both the RC main effect and the RCI × Post coefficients fall in magnitude relative to the table 3 estimates (which exclude the RCI measure) reveals that the lower market value of RC properties stems in part from the fact that they were situated in more rent control–intensive locations.

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	−.440***	−.484***	−.503***		−.232	−.217	
	(.057)	(.050)	(.052)		(.188)	(.184)	
RC × Post	.175***	.196***	.233***	.208***	.202*	.174	.132
	(.038)	(.036)	(.034)	(.040)	(.114)	(.107)	(.114)
RCI	−.581*	−.792	−.938*				
	(.325)	(.479)	(.494)				
RCI × Post	.328**	.258*	.545***	.475***			
	(.136)	(.138)	(.191)	(.180)			
Non-RC × RCI					−.568	−.686	
					(.546)	(.561)	
Non-RC × RCI × Post					.281*	.514**	.415*
					(.168)	(.227)	(.220)
RC × RCI					−1.211**	−1.416**	
					(.535)	(.555)	
RC × RCI × Post					.249	.651***	.607**
					(.215)	(.231)	(.256)
Block group fixed effects	No	Yes	Yes	No	Yes	Yes	No
Tract trends	No	No	Yes	Yes	No	Yes	Yes
Map-lot fixed effects	No	No	No	Yes	No	No	Yes
H_0: RCI × Post coefficients equal					.909	.598	.514
R^2	.611	.761	.765	.938	.764	.767	.938

NOTE.—$N = 32{,}980$. 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 × RCI main effects are absorbed by map-lot fixed effects in cols. 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 × Post dummies for each of 30 tracts from the 1990 census. Map-lot fixed effects are dummy variables for each of the 9,497 residential parcels in Cambridge. Test of the equality of the RCI × Post coefficients reports p-values from tests that non-RC × RCI × Post and RC × RCI × Post coefficients are equal. Robust standard errors clustered by 1990 block group are in parentheses.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

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. While these covariates reduce the precision of the RCI main effect, the point estimates remain large and statistically significant. The coefficient of primary interest (RCI × Post) increases in magnitude with the inclusion of tract-year effects. Column 3 obtains an estimate for the RCI × Post coefficient of approximately 55 log points, while in column 4 the estimate is 48 log points, implying that a residential property at the 75th percentile of rent control exposure gained approximately 13 percent more in assessed value following decontrol than a property at the 25th percentile of exposure.[32]

These first four estimates constrain the indirect effects of rent control to be identical for never-controlled and decontrolled units. In practice, these effects may differ. Indeed, if the indirect effect were present only for decontrolled units, this would suggest that the indirect effect is not operating through the hypothesized localized amenity channel (which we would expect to affect both property types). The models in columns 5–7 demonstrate that both decontrolled and never-controlled properties benefit from the indirect effect of rent control removal. In the most demanding specification in column 7, which includes map-lot fixed effects and tract-by-year dummies, we estimate an RCI $\times$ Post coefficient of 0.42 for never-controlled properties and 0.61 for decontrolled properties. Both are significantly different from zero, and the data do not reject the hypothesis that these coefficients are of the same magnitude.

Notably, the models that allow for separate indirect effects for controlled and never-controlled properties also find that the RC discount is only approximately half as large as was implied by the earlier models that do not include interaction terms between RCI and rent control status—approximately 23 rather than 48 log points—and that this RC discount was fully offset by the post-decontrol appreciation of decontrolled properties. By implication, approximately half of the estimated RC discount detected in columns 1–3 of the table is accounted for by the fact that RC units were situated in more rent control–intensive locations and that rent control exposure differentially lowered their value. While our conceptual model is silent on why the indirect effect of rent control is greater for controlled than for never-controlled properties, one speculative explanation is that deferred maintenance and poor property management were more acute in locations where a larger fraction of properties were controlled. This conjecture would also be consistent with our finding of greater relative appreciation of decontrolled than never-controlled units in rent control–intensive locations, though, as above, this differential is not statistically significant.

C. *Variation across Property Types*

Table 5 explores the potentially differing consequences of rent decontrol for the assessed values of houses and condominiums.[33] Across the two panels, the direct impact of rent decontrol on controlled houses is

[32] The 25th and 75th percentiles of the RCI distribution are 0.464 and 0.199. The implied interquartile effect is $0.126 = 0.475 \times (0.464 - 0.199)$.

[33] 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.

TABLE 5

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)
	A. Houses					
RC × Post	.065***	.045***	.024	.035	.035	.035
	(.011)	(.016)	(.023)	(.036)	(.023)	(.032)
RCI × Post	.205*	.200				
	(.103)	(.144)				
Non-RC × RCI × Post			.194*	.197	.192**	.190
			(.103)	(.142)	(.095)	(.135)
RC × RCI × Post			.315**	.227	.232*	.231
			(.130)	(.196)	(.128)	(.181)
H_0: RCI × Post coefficients equal			.080	.782	.553	.675
R^2	.855	.984	.855	.984	.858	.983
Observations	16,239	16,239	16,239	16,239	14,917	14,917
	B. Condominiums					
RC × Post	.354***	.345***	.361***	.276**	.235*	.236*
	(.038)	(.037)	(.135)	(.131)	(.132)	(.136)
RCI × Post	.669**	.492**				
	(.256)	(.211)				
Non-RC × RCI × Post			.678**	.397	.443**	.454**
			(.308)	(.258)	(.205)	(.206)
RC × RCI × Post			.648**	.569**	.722**	.724**
			(.291)	(.266)	(.323)	(.328)
H_0: RCI × Post coefficients equal			.925	.586	.398	.429
R^2	.714	.889	.714	.889	.725	.89
Observations	16,741	16,741	16,741	16,741	11,778	11,778
Block group fixed effects	Yes	No	Yes	No	Yes	No
Map-lot fixed effects	No	Yes	No	Yes	No	Yes
Tract trends	Yes	Yes	Yes	Yes	Yes	Yes
Excluding converted structures	No	No	No	No	Yes	Yes

NOTE.—RCI is calculated over a 0.20-mile radius. 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 × RCI, or RC × 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. Map-lot fixed effects are a set of dummies for each residential parcel (8,453 for houses, 1,450 for condominiums). Tract trends are tract × Post dummies for each of 30 tracts from the 1990 census. Columns 5 and 6 exclude units that change usage categories between 1994 and 2004. Test of the equality of the RCI × Post coefficients reports p-values from tests that non-RC × RCI × Post and RC × RCI × Post coefficients are equal. Robust standard errors clustered by 1990 block group are in parentheses.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

substantially smaller than the corresponding estimate for condominiums, a pattern that may be due to the greater extent of upgrading at controlled condominiums.[34] For residential houses (panel A), we estimate an indirect effect coefficient on the value of residential houses of approximately 20 log points, implying that a house facing the mean level of RCI of 0.37 would experience an additional 7.4 log points of appreciation relative to a nonexposed house following rent decontrol. When allowing for separate indirect effects for decontrolled and never-controlled houses (cols. 3 and 4), we find that the indirect effect for decontrolled houses is 20–50 percent larger than for never-controlled houses, although we are unable to reject the equality of the two regression coefficients. Adding 9,497 map-lot fixed effects to the regression model in column 4 decreases precision such that the indirect effect estimates for houses become insignificant, though magnitudes are only modestly affected.

The parallel analysis for condominiums in panel B finds significant indirect effects of rent decontrol on both decontrolled and never-controlled condominiums. The indirect effects for condominiums are greater than for houses, although standard errors are also considerably larger. The point estimate of 0.49 in column 2 implies differential appreciation of 18.2 log points for a condominium at the mean level of rent control exposure relative to a nonexposed unit. As is the case with houses, adding a map-lot fixed effect for each land parcel containing condominiums (1,450 fixed effects) reduces or eliminates the statistical significance of the indirect effect point estimates.

The number of condominiums in Cambridge rose by one-third between 1994 and 2004, with almost half of this rise due to the conversion of existing houses to condominiums. This substantial change in the housing stock implies that part of the rise in assessed values may be due to capital improvements in residential units, particularly condominiums, rather than solely to changes in the value of ownership stemming from decontrol (e.g., the option to charge higher rents or convert the unit to owner-occupied status). Notably, this concern applies only to the direct effects estimates of rent decontrol on decontrolled units, which may conceivably combine both the investment and ownership channels. For the indirect effects we measure, this source of variation—spillovers from local housing investments spurred by rent decontrol—is not a concern; indeed, this is one of the key causal channels through which we hypothesize the indirect effects operate.[35]

[34] As discussed in Sec. VIII, Cambridge building permit data indicate that annual citywide investments in decontrolled condominiums increased by 206 percent in the post- vs. pre-decontrol period while the corresponding increase for decontrolled houses was 120 percent (table A1).

[35] The converse concern applies, however: if localized spillovers spur additional investments at decontrolled units, this may contribute to the estimated direct and indirect

To explore the importance of the investment channel, we undertake two exercises. We first reestimate the main models for direct and indirect effects of decontrol on houses and condominiums while excluding all units that changed usage categories (e.g., converted condominiums) between 1994 and 2004. Then, in Section VII, we directly explore the role of investments using Cambridge residential building permit data.

When dropping converted properties from the regression estimates in columns 5 and 6 of table 5, we find little effect on the direct or indirect effect point estimates for houses. This is sensible since only one in 10 houses changed status during the 10-year window. Consistent with our reasoning above, however, the estimated direct effect of rent decontrol on condominium values is substantially reduced when converted properties are excluded—falling by as much as 35 percent between columns 3 and 5—while the indirect effects estimates are substantively unaffected.[36] These results suggest that the direct effects estimates capture both capital improvements and changes in ownership value spurred by decontrol, particularly for condominiums. The indirect effects estimates, however, are not affected (conceptually or empirically) by abstracting from the substantial investments made in converted units.

D. *Testing Alternative Measures of Rent Control Intensity*

Our estimates so far employ a measure of rent control intensity calculated over a 0.20-mile radius from each Cambridge map-lot. We explore the sensitivity of the results to this choice by employing two sets of alternative measures: one that varies the geography over which rent control intensity is calculated and one that varies how the weight given to surrounding properties decays with distance. These robustness tests employ the final (most exhaustive) specification in table 4, which includes fixed effects for each individual map-lot and tract-by-year dummies that account for differing rates of property appreciation across all 30 Cambridge census tracts.

Panel A of table 6 reports estimates using RCI measures calculated at differing geographies. Columns 1–3 calculate RCI at radii of 0.10, 0.20, and 0.30 mile, respectively (hence, col. 2 replicates our main specification from table 4). Column 4 instead uses an RCI measure calculated at

effects, meaning that our interpretation of the direct effect estimate is unduly restrictive. However, to the degree that these spillover-induced investments are greater in more rent control–intensive locations, as we anticipate, the indirect effect coefficient should correctly capture this channel.

[36] This is logical since excluding converted units from the regression should not eliminate the indirect effect of investments made at these units on the value of nonconverted units.

TABLE 6
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)

	A. RCI Defined over Varying Geographies			
	.10 Mile (1)	.20 Mile (2)	.30 Mile (3)	Census Block Group (4)
RC × Post	.132	.132	.149	.128
	(.089)	(.114)	(.125)	(.098)
Non-RC × RCI × Post	.185	.415*	.477*	.095
	(.143)	(.220)	(.245)	(.177)
RC × RCI × Post	.377**	.607**	.646**	.318
	(.183)	(.256)	(.281)	(.228)
H_0: RCI × Post coefficients equal	.379	.514	.594	.367
Standard deviation of RCI measure	.192	.165	.145	.179

	B. Varying the Weighting Scheme Used to Measure RCI			
	$\lambda = -12$ (1)	$\lambda = -9$ (2)	$\lambda = -6$ (3)	$\lambda = -3$ (4)
Weight at:				
.1 mile/.01 mile	.34	.44	.58	.76
.2 mile/.01 mile	.10	.18	.32	.57
.3 mile/.01 mile	.03	.07	.18	.42
RC × Post	.124	.129	.136	.134
	(.124)	(.136)	(.155)	(.208)
Non-RC × RCI × Post	.407**	.499**	.598**	.629**
	(.193)	(.223)	(.261)	(.264)
RC × RCI × Post	.580**	.665**	.758**	.802*
	(.243)	(.280)	(.345)	(.457)
H_0: RCI × Post coefficients equal	.504	.555	.617	.683
Standard deviation of RCI measure	.190	.179	.160	.117

Note.—$N = 32{,}980$. In panel A, RCI is calculated over geographies reported in column headings. In panel B, RCI is calculated using an exponential decay weighting scheme (see eq. [3] in the text). 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, tract × year dummies, and individual map-lot fixed effects are included in all specifications. Test of the equality of the RCI × Post coefficients reports p-values from tests that non-RC × RCI × Post and RC × RCI × Post coefficients are equal. Robust standard errors clustered by 1990 block group (89 groups) are in parentheses.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

the level of 587 census blocks. Distinct from the radius-based measures, census blocks comprise a set of contiguous, nonoverlapping geographic subdivisions. These blocks may not, however, correspond to any specific notion of neighborhood or proximity, particularly since residential units on opposite sides of the same street are often assigned to different blocks.

The direct impact of decontrol is relatively insensitive to the geography of the RCI measure. Across specifications, this effect averages 13–15 log points, which is generally not significant.[37] The indirect effect estimates are somewhat more sensitive. The largest indirect effect estimate comes from the 0.30-mile radial RCI measure, though this effect is statistically indistinguishable from the 0.20-mile measure. Estimates using the census block group–based RCI measure are generally insignificant, which is consistent with our observation that block boundaries do a poor job of capturing neighborhood proximity.

An important conceptual limitation of the radius-based measure is that it puts equal weight on each residential unit within a specified radius of a given map-lot while simultaneously according zero weight to all other units in Cambridge. It seems plausible, however, that the interactions among residential units decline with distance, so that nearby units matter more for a unit's rent control intensity as perceived by occupants and potential buyers, while more distant units matter less.

To explore this idea empirically, we employ an alternative measure of rent control intensity that places greatest weight on nearby units and less weight on more distant units. Specifically, we use an exponential decay function to calculate the RCI of each unit i as a function of its distance from all other RC (rent-controlled) units $j \neq i$ in Cambridge, where the weight given to each unit j is declining in its distance from i. Let d_{ij} be the distance between units i and j measured in miles and $\lambda < 0$ be a negative constant, J be the complete set of residential units in Cambridge, and RC_j be a dummy variable equal to one if unit j is rent controlled and zero otherwise. Our distance-based measure of RCI^λ is

$$\mathrm{RCI}_i^\lambda = \frac{\sum_{j \neq i}^{J} \mathrm{RC}_j \times e^{\lambda d_{ij}}}{\sum_{j \neq i}^{J} e^{\lambda d_{ij}}}. \tag{3}$$

Like the primary RCI measure, the measure RCI_i^λ lies on the unit interval. The difference between RCI_i^λ and RCI is that the weight given to surrounding units in RCI_i^λ is a continuous, declining function of distance from i whereas, for RCI, the weighting function is flat over the area of the designated radius and then is equal to zero outside of that area.

Panel B of table 6 reports estimates of this decay-based RCI measure using values of λ ranging from -12 to -3, where lower (more negative) values of λ give greater weight to nearby units and higher values of λ give greater weight to more distant units. To illustrate the operation of

[37] As shown in table 4, the RC main effect is generally much smaller and less precise in models that include three-way interactions between the RC, RCI, and Post variables, reflecting the fact that prior to rent decontrol, controlled units were valued at the greatest discounts in more rent control–intensive neighborhoods.

our weighting function, the first several rows of the panel display the weight accorded to units at 0.10, 0.20, and 0.30 mile from each reference unit relative to units at 0.01 in the RCI calculation. All four values of λ accord a weight that is close to unity to properties within a radius of 0.01 mile and under, whereas more negative values of λ place substantially less weight on distant units.[38] For example, at $\lambda = -12$, units at 0.20 and 0.30 mile receive weights of 0.10 and 0.03 relative to the weight at 0.01 mile, respectively. In contrast, with $\lambda = -3$, these units receive relative weights of 0.57 and 0.42, which are substantially greater. The estimated direct and indirect effects of rent decontrol on residential property appreciation are both stable and robust across the four parameterizations of the decay function. In all cases, the indirect effects estimates are statistically significant for both decontrolled and never-controlled properties and are comparable in magnitude to the radius-based measures, though, if anything, the decay-based estimates are more robust.

One pattern evident in both panels of table 6 is that the estimated magnitude of the indirect effect rises when we use an RCI measure that gives greater weight to more distant properties (by employing a wider radius or a more gradual decay function). A likely explanation for this pattern is that employing a broader RCI measure provides more information about the extent of a unit's rent control exposure; that is, a given high (or low) RCI value obtained over a larger radius (or slower decay function) implies that the relevant unit is more (or less) deeply surrounded by other controlled units. This should in turn imply a larger indirect effect of RCI on market values. Logically, the variance of the RCI measure declines as its scope broadens as shown in the lower row of each panel, so the size of the standardized effect rises less rapidly than do the RCI point estimates.[39]

Alongside the robustness tests in table 6, in online Appendix B, we have explored a variety of alternative and complementary identification strategies that probe the key results. One potential limitation of our primary approach stems from the nonparallelism between the RCI measures and the geographic dummy variables used as controls. While the radius-based RCI measure in many cases partially overlaps multiple block groups, each map-lot in the regression is associated with only a single block group fixed effect. To explore sensitivity to this choice, we created a set of "rolling" block fixed effects. For each map-lot, we identify each census block (of which there are 587) whose centroid lies within 0.2 mile of the map-lot and assign these block dummies to the map-lot. These non–mutually exclusive block fixed effects are then used in place

[38] Units at distance zero always receive a weight of one since $e^0 = 1$.

[39] At maximal radius, the RCI measure is identical for all Cambridge units except for each map-lot's own effect on the RCI measure (since units are not counted in their own RCI measures).

of the conventional block group fixed effects in the regressions (see table B1). We perform an analogous exercise for the exponential decay specifications, where all block dummies in Cambridge are fractionally assigned (summing to unity) to each map-lot as a decaying function of the distance between the map-lot and block centroids (see table B1). Motivated by the fact that our radius-based rent control intensity cannot account for the characteristics of non-Cambridge properties bordering the city, we perform a third specification test that obviates this issue by excluding all Cambridge block groups that border non-Cambridge properties (see table B2). All three of these sensitivity exercises, reported in Appendix B, yield estimates that are highly comparable in magnitude and precision to our primary estimates above.

VI. The Time Path of Rent Decontrol Capitalization

Because our assessor data cover only two points in time, 1994 and 2004, they do not shed light on how residential real estate prices evolved prior to decontrol or in the years thereafter. We turn to housing transaction data to complete this picture. We begin with simple "event-study" plots of the main effect of rent control status on real estate transactions, estimated with the equation

$$\log(Y^S_{igt}) = \gamma_g + \delta_t + \beta' X_i + \sum_{t=1988}^{2005} (\mathrm{RC}_i \times \delta_t)\rho_{1,t} + \epsilon_{igt}, \quad (4)$$

where Y^S_{igt} is the real sales price of residential unit i located in block group g in year t, the vectors γ_g and δ_t contain fixed effects for block groups and year of sale, and the X vector contains a rich set of property characteristics, sourced from deed records and summarized in Appendix table A4, including the count of rooms, bathrooms, and bedrooms; the unit's interior square footage; a quadratic in lot size and a dummy for lot size equal to zero (commonplace for condominiums); and a quadratic in the log of property age and a dummy for missing year built. All controls are interacted with dummies for structure type (condominium, single-family home, multifamily home) since the hedonic value of these attributes may differ across types, and structure type dummies are further interacted with quadratic time trends to allow for differing price trends. Standard errors are clustered at the block group level. Figure 3 plots the key coefficients ($\rho_{1,t}$) from equation (4), which correspond to by-year estimates of the rent control price differential measured relative to the omitted reference year of 1994.

The relative price of RC properties increased by roughly 10 log points over the first 3 years following decontrol, declined very modestly between years 3 and 4, and then rose almost continuously thereafter. By the end of the sample in 2005, RC properties had increased in market value

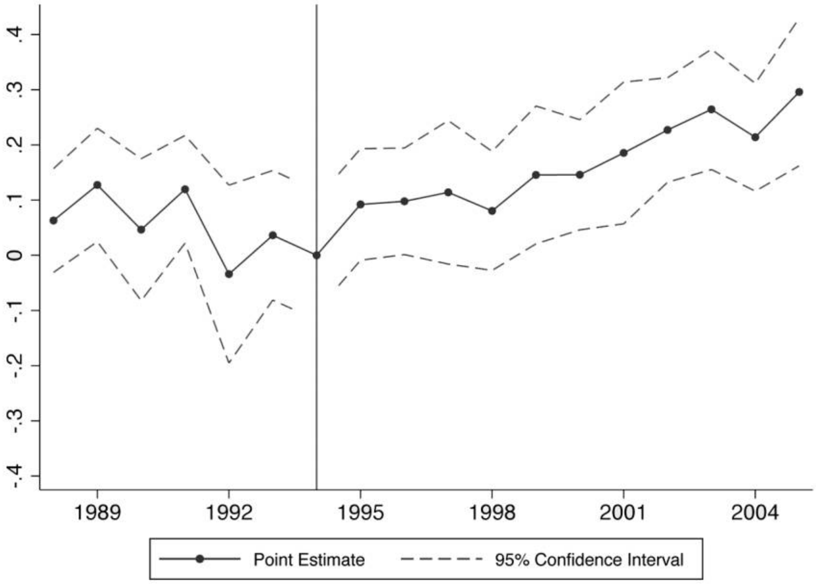

FIG. 3.—Event-study for direct effect of rent decontrol on transaction prices of decontrolled units, 1988–2005. The figure plots RC × Year coefficients from event-study regressions in which the dependent variable is log sale price, Winsorized to the 1st percentile separately for houses and condominiums. RC is an indicator for a location that was rent controlled in 1994. The regression also includes year dummies, block group fixed effects, structure type main effects and quadratic time trends, and controls for property characteristics: total rooms, bathrooms, bedrooms, interior square feet, lot size and its square, a dummy for lot size zero, log property age and its square, and a dummy for property age missing, all interacted with structure type dummies. Robust standard errors are clustered by block group. The vertical line in 1994 designates the year preceding rent decontrol.

by almost 30 log points relative to nearby non-RC properties with similar characteristics. The increasing cumulative effect of decontrol on transaction prices parallels the evidence above on the evolution of resident turnover, which also rose immediately following decontrol and then generally trended upward through the end of the sample window. Both results suggest that changes in the desirability of locations and neighborhoods induced by decontrol likely took years to unfold.

We plot the indirect effects of rent decontrol on the value of never-controlled and decontrolled properties in figure 4 using a specification analogous to equation (4) augmented with RCI × Year terms.[40] Indirect price effects of decontrol on the sale prices of never-controlled properties begin to accumulate immediately following decontrol, attain statistical significance by the fifth year following decontrol (1999), and

[40] The specification is estimated separately for never-controlled and decontrolled units, and hence RC × Year effects are not included.

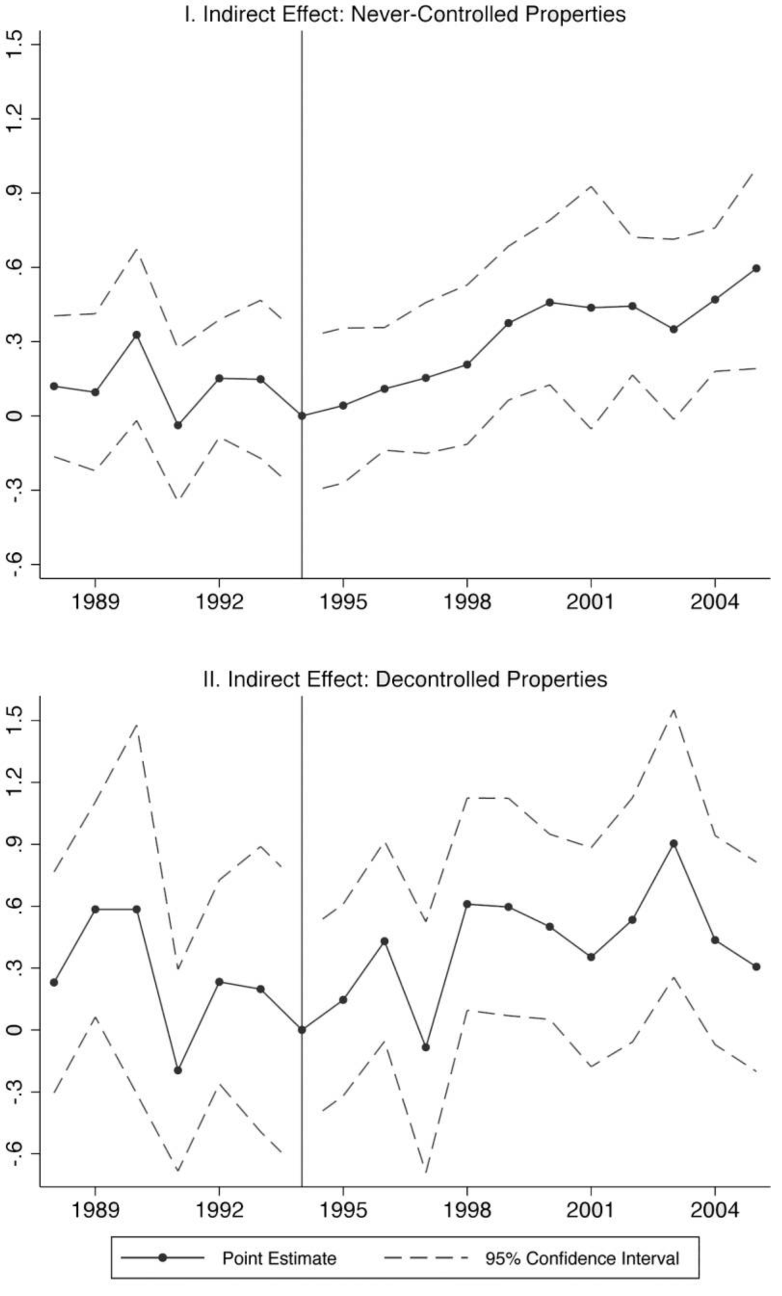

FIG. 4.—Event-study for the indirect effect of rent decontrol on transaction prices of never-controlled and decontrolled properties, 1988–2005. The figures plot RCI × Year coefficients from event-study regressions of log sale prices, Winsorized to the 1st percentile for houses and condominiums. RCI is calculated over a 0.20-mile radius. Panels I and II are estimated using never-controlled and formerly controlled properties. See figure 3 notes for specification details. Robust standard errors are clustered by block group. The vertical line in 1994 designates the year preceding rent decontrol.

continue to rise through the end of the sample, yielding a point estimate of 0.60 log points in 2005 ($p < .01$). Indirect effects estimates for decontrolled properties (panel II) offer a similar picture of post-decontrol appreciation: the indirect effect for decontrolled units varies in sign and is generally insignificant in the pre-decontrol years; following decontrol, the point estimates are strongly positive in 10 of 11 years and are statistically significant at $p < .10$ in 7 of 11 years. Though the event-study plot is substantially noisier for decontrolled than for never-controlled properties, this likely reflects the fact that there are only half as many transactions of decontrolled units between 1988 and 2005 (4,802 relative to 9,987).

Table 7 explores these relationships in further detail. Columns 1–3 provide estimates of the direct effect of decontrol on the market value of formerly controlled properties. The estimates range from 6 to 11 log points as we add block group fixed effects and controls for property characteristics (col. 2).[41] Taking advantage of the additional years of data available from the transactions sample, column 3 adds linear and quadratic trends for each of the 30 census tracts (also interacted with property types) to allow for the flexible evolution of real estate prices over time within fine geographies. These controls have little impact on magnitudes or precision. Subsequent columns introduce the RCI measure to estimate indirect effects. The coefficient of interest in column 4 (RCI × Post) is 21 log points, which is in the lower range of estimates obtained using the assessor's sample (table 4). As figure 3 suggests, however, both the direct and indirect effects of rent decontrol cumulate over the sample window, so that the average post-decontrol effect is smaller than the long-run effect estimated by the long difference specifications used with the assessor data. Adding flexible geographic trends (col. 5) slightly reduces the indirect effect estimate and increases its standard error.

Columns 6 and 7 report estimates in which the indirect effect of rent control exposure on prices is permitted to differ between decontrolled and never-controlled properties. The indirect effect estimates are statistically indistinguishable between decontrolled and never-controlled units, though they are generally larger for decontrolled properties and more precisely estimated for never-controlled properties (consistent with fig. 4). In column 7, which includes all prior covariates plus tract-specific quadratic trends, the point estimates for the indirect effects are

[41] That the addition of property-specific covariates in col. 2 substantially reduces the magnitude of the baseline price differential between RC and non-RC properties (from 30 to 20 log points) is consistent with the data summarized in table A4, which indicates 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.

TABLE 7
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	−.305***	−.204***	−.193***	−.189***	−.185***	−.166***	−.161***
	(.043)	(.024)	(.024)	(.025)	(.024)	(.025)	(.024)
RC × Post	.060*	.106***	.086***	.087***	.079***	.079***	.068***
	(.030)	(.026)	(.027)	(.026)	(.025)	(.025)	(.024)
RCI				−.510*	−.494		
				(.305)	(.317)		
RCI × Post				.205***	.166*		
				(.056)	(.098)		
Non-RC × RCI						−.305	−.276
						(.274)	(.275)
Non-RC × RCI × Post						.197***	.132
						(.067)	(.089)
RC × RCI						−.884**	−.883**
						(.360)	(.368)
RC × RCI × Post						.246*	.246
						(.146)	(.177)
Block group fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Property characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic tract trends	No	No	Yes	No	Yes	No	Yes
H_0: RCI × Post coefficients equal						.773	.512
R^2	.318	.674	.681	.675	.682	.678	.684

NOTE.—N = 14,789 Cambridge house and condominium properties transacted during 1988–2005. Prices are Winsorized by structure type at the 1st 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 1995 or afterward. 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 of the equality of the RCI × Post coefficients reports p-values from tests that non-RC × RCI × Post and RC × RCI × Post coefficients are equal. Robust standard errors clustered by 1990 block group are in parentheses.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

no longer significant at conventional levels, though the point estimates are little affected.

Echoing the results from property assessments, the transaction data models suggest that the price penalty for rent control exposure was substantially greater for controlled than for never-controlled houses in the rent control era. Accounting for this differential in columns 5–7 reduces the magnitude of the estimated RC price discount prior to decontrol. For example, the RC point estimate of −17 log points in column 6 is quite comparable to the RC point estimates in table 4 that use property assessments and allow for separate RCI slopes for RC and non-RC units. While the direct effect of decontrol on RC units appears modestly smaller in magnitude in the transaction-based models than in the assessment-based models, this again likely reflects timing, with the assessment-based models using a 10-year-long change and the transaction-based models using a series of annual observations.

The transaction sample also provides an opportunity to explore the concern that assessed real estate values may not accurately reflect market prices. We examined this issue by constructing a matched assessor's sample for properties that transacted in 1994 and 2004, and we used this matched set to perform parallel estimates of the direct and indirect effects of decontrol on assessed and transacted property values in Appendix table A5. These models yield a close match between the estimated decontrol impacts on assessed values and transaction prices, and moreover, the match is particularly close for houses.[42] This comparison suggests that the transaction and the assessor's sample provide complementary and broadly consistent measures of valuations.

We also explored the possibility that rent decontrol had positive indirect effects on the market value of properties close by Cambridge, focusing on the adjoining town of Somerville. For this exercise, we assembled residential transaction records for Somerville analogous to those used above for Cambridge, limiting the sample to residential units located in census tracts and block groups that abut either the Somerville-Cambridge border (north of Cambridge) or the Somerville-Medford border (north of Somerville).[43] Comparing properties along these two Somerville borders, in Appendix table A6 we find robust evidence that

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

[43] Somerville is bordered by Cambridge, Arlington, Medford, Everett, and Charlestown. Its two longest borders by a considerable stretch are those with Cambridge in the south and Medford in the north. The transaction data we assemble 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.

Cambridge-bordering properties appreciated by 7–10 percent more than Medford-bordering properties with the same observable characteristics. This pattern is consistent with the hypothesis that improvements in Cambridge neighborhood amenities following decontrol also increased the desirability of locations bordering Cambridge.

Online Appendix B reports on additional robustness tests that investigate price appreciation in neighboring towns, explore the potential importance of the increase in subprime credit in lower-income Cambridge neighborhoods during this time period, and test for correlations between rent control intensity and changes in the composition of Cambridge properties that transact. These many specification tests, in combination with our prior estimates using assessed property values, confirm that alongside its direct effects on the market value of formerly controlled units, rent decontrol had robust indirect impacts on market values. Both decontrolled and never-controlled properties in Cambridge that were more exposed to controlled units saw differential appreciation in the post-decontrol regime. Before benchmarking the economic magnitudes of these direct and indirect effects, we briefly consider one plausible channel through which they may have operated: property investments.

VII. The Impact of Rent Decontrol on Property Investments

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 1991–94 to $455 million in the period 1995–2004, while annual investment expenditures roughly doubled at three of four property types—decontrolled houses, never-controlled houses, and never-controlled condominiums—and roughly tripled at decontrolled condominiums (table A1).[44] While fewer than one in 25 residential units receives a building permit annually, this fraction increased substantially following decontrol: by 17 percent and 7 percent among never-controlled and decontrolled houses and by 38 percent and 45 percent among never-controlled and decontrolled condominiums.[45] Was this increase in residential investment caused by rent decontrol?

[44] As detailed in App. B, our analysis in this section draws on a database of all building permits issued by the Cambridge Inspectional Services Department for years 1991–2005, including property address and proposed expenditure. 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 A1.

[45] 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; e.g., 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.

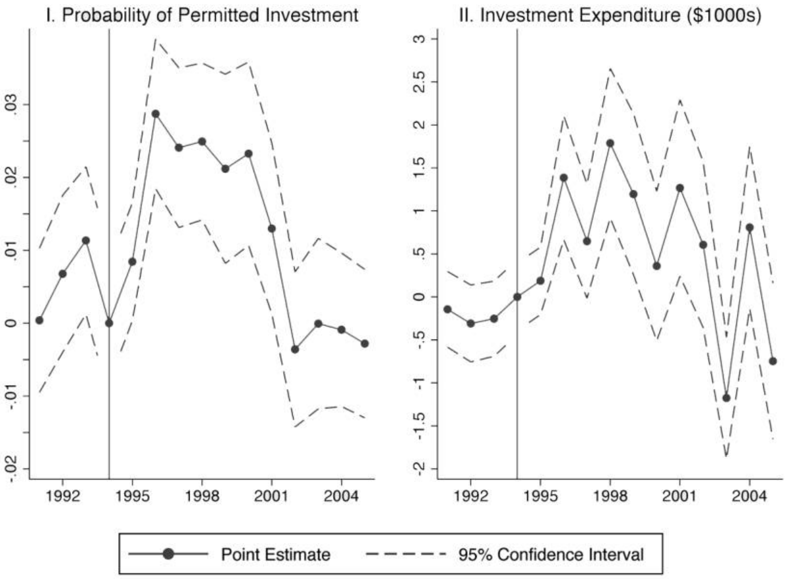

FIG. 5.—Event-studies for the direct effect of rent decontrol on investment activity at formerly controlled units, 1991–2005. The figures plot coefficients on RC × Year variables from event-study regressions in which the dependent variable is (panel I) an indicator for whether a structure received a building permit and (panel II) the permitted expenditure at a structure (including zeros). 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. The year 1994 is the omitted RC × Year category. Robust standard errors are clustered by block group. The vertical line in 1994 indicates the year preceding rent decontrol.

An event-study of the direct impact of decontrol on permitting activity (fig. 5) shows a sharp, statistically significant differential rise in permitting activity and investments at decontrolled relative to never-controlled properties during the first 5 years following decontrol.[46] Relative to never-controlled units, the annual permitting probability at decontrolled units rose by 2.5–3.0 percentage points while permitted investment expenditures per decontrolled unit rose by $1,000–$1,500. We find no evidence, however, that decontrol indirectly caused permitting or

[46] 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 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.

investments to rise in decontrolled or never-controlled units with higher rent control exposure (not shown). Thus, any indirect effect of decontrol on residential investments that took place was not localized to highly rent control–exposed units, though it remains plausible that decontrol helped to spur the citywide investment boom documented in table A1. 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 A1, panel C). However, unless increased investment occurred along dimensions that do not require permits and hence are not observed in our data—for example, 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. We consider an upper bound on the contribution of induced investments to post-decontrol property appreciation in the next section.

VIII. The Capitalized Value of Rent Decontrol in Cambridge

How economically large are the direct and indirect effects 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. Panel A of table 8 presents information on the assessed value of the Cambridge housing stock in 1994 and 2004. Between 1994 and 2004, the assessed value of houses and condominiums rose from \$4.7 billion to \$12.5 billion in constant 2008 dollars, a gain of 163 percent. Notably, appreciation of decontrolled units exceeded that of never-controlled units by a substantial margin—219 percent relative to 152 percent—and these gains were larger among both decontrolled houses (185 percent vs. 147 percent) and decontrolled condominiums (237 percent vs. 166 percent).

What was the contribution of rent decontrol to these gains? We compute the direct and indirect contributions of rent decontrol to these valuations by applying our most conservative regression estimate, which includes map-lot fixed effects and census tract trends (table 4, col. 7), to calculate the counterfactual price change at each location between 1994 and 2004, assuming that rent control had remained in place. While the aggregate value of decontrolled houses and condominiums increased from \$785 million to \$2.5 billion between 1994 and 2004, our estimates imply that had rent control not been eliminated, this gain would have been \$849 million smaller, with \$310 million due to the (foregone) direct effect of decontrol and an additional \$539 million due to the (foregone) indirect effects on decontrolled properties. By implication,

TABLE 8
ESTIMATED DIRECT AND INDIRECT CONTRIBUTIONS OF RENT DECONTROL TO CHANGES IN CAMBRIDGE ASSESSED RESIDENTIAL PROPERTY VALUES, 1994–2004
(in Millions of 2008 Dollars)

	A. Assessed Housing Values				B. Estimated Effects of Decontrol on Housing Values			
	Assessed ($ Millions)		Change 1994–2004		Increase in Value ($)		Increase in Value (%)	
	1994	2004	Δ$	Δ%	Direct Effect	Indirect Effect	Direct Effect	Indirect Effect
Decontrolled units:								
Houses	$267	$760	$493	185%	$94	$149	18%	29%
Condominiums	$518	$1,746	$1,228	237%	$216	$390	19%	34%
All	$785	$2,507	$1,722	219%	$310	$539	19%	33%
Never-controlled units:								
Houses	$2,961	$7,320	$4,359	147%	NA	$822	NA	13%
Condominiums	$1,017	$2,699	$1,683	166%	NA	$306	NA	13%
All	$3,978	$10,020	$6,042	152%	NA	$1,128	NA	13%
All units:								
Houses	$3,229	$8,081	$4,852	150%	$94	$971	1%	14%
Condominiums	$1,535	$4,446	$2,911	190%	$216	$696	6%	20%
All	$4,763	$12,526	$7,763	163%	$310	$1,667	3%	16%

NOTE.—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 col. 7 of table 4. Counterfactuals for RCI effects subtract non-RC × RCI × Post and RC × RCI × Post effects, and counterfactuals for the direct effect of decontrol subtract RC × Post effects from actual log property values. Aggregate effects in 2008 dollars are calculated by summing exponentiated counterfactual log property values.

about half of the $1.7 billion appreciation of decontrolled properties between 1994 and 2004 can be accounted for by rent decontrol.

Though never-controlled units do not (by definition) benefit directly from decontrol, the indirect effects of decontrol are substantial. Our estimates imply that of the $6.0 billion gain in the assessed value of never-controlled houses and condominiums between 1994 and 2004, $1.1 billion (13 percent) of this gain is due to the indirect effects of rent decontrol on never-controlled properties, with $822 million accruing to houses and $306 million to condominiums.

Putting these components together, we calculate that decontrol added almost exactly $2.0 billion to the value of the Cambridge housing stock between 1994 and 2004, with 84 percent of this effect due to the indirect effect of rent decontrol. While the share of post-decontrol appreciation between 1994 and 2004 induced by rent control removal was substantially larger for decontrolled than for never-controlled properties (49 percent vs. 19 percent), the never-controlled segment of the market received the largest increase in capitalization from rent control's removal: $1.1 billion versus $849 million. By implication, prior to decontrol, the never-controlled sector bore more than half of the incidence of rent control regulation.

Can the increase in residential investments documented in Section VII account for these price impacts? Total permitted residential investments averaged $45.5 million between 1995 and 2004 (table A1).[47] In the 4 years prior to decontrol, these expenditures averaged $20.8 million. To benchmark the maximal estimate for the capitalized value of these investments, consider a case in which the entire $24.7 million increase in annual expenditures 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 10-year interval. In this case, we would conclude that only 12 percent of the appreciation of Cambridge residential properties between 1994 and 2004 was due to increased investments induced by rent decontrol ($247 million of $1,977 billion), leaving the remaining 88 percent accounted for by the capitalization of other benefits of rent decontrol. A parallel set of calculations implies that increased investments can explain at most 18 percent of the indirect effect of decontrol on the value of never-controlled properties and at most 6 percent of the total (direct plus indirect) effect of decontrol on the value of formerly controlled properties. In fact, our event-study estimates in figure 5 imply that decontrol can account for no more than $82 million of the total increase in Cambridge residential investments in the years following decontrol, all of it concentrated on decontrolled properties.[48]

IX. 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 assess the localized price spillovers to never-controlled properties as well as to quantify direct effects on decontrolled properties. Our main

[47] The permitted investment cost may somewhat understate the full economic costs of housing investments since the property owner may also invest considerable oversight time on property improvements, and these costs are not included in building permits. Moreover, if the property owner must tie up working capital (or, equivalently, obtain a construction loan) while improvements are made, then the full economic cost of investments should also include the opportunity cost of capital during the interval between investment and realized returns.

[48] For this calculation, we assume that decontrol caused each of the 5,439 decontrolled units extant as of 2004 (table 2) to receive an average of $1,500 per year of additional investment between 1996 and 2001 (fig. 5). This places an upper bound on additional induced expenditures during these years of $49 million. Accounting for the 95 percent confidence interval surrounding this estimate, this number is $82 million.

finding is a large and significant positive indirect effect of decontrol on the valuation of properties that were exposed to controlled units, leading, on average, to a 16 percent increase in the value of residential units between 1994 and 2004. We further document that rent-controlled properties were valued at a substantial discount relative to never-controlled properties and that rent decontrol eliminated a substantial part of this differential, raising the assessed values of these properties by approximately 13–25 percent.

The contribution of decontrol to the capitalized value of the Cambridge residential housing stock in this period corresponds to a total of $2.0 billion. 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, indirect effects on never-controlled properties account for more than half (56 percent) of the decontrol-induced increase in the value of the housing stock.

Because, under any reasonable set of assumptions, increases in residential investment stimulated by rent decontrol can explain only a small fraction of these indirect effects, 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. Glaeser and Luttmer (2003) argue that nonprice rationing under rent control leads to a mismatch between renters and apartments and provide 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 contributed 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 trade-off between the surplus transferred from landlords to renters and the deadweight loss from quality or quantity undersupply. Viewed in this light, some 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 indirect effect on the desirability of housing in rent control–intensive locations. Our results indicate that the efficiency cost of Cambridge's rent control policy was large relative to the size of the transfer to renters. In particular, only 16 percent of the capitalized value of rent decontrol reflects a direct impact on decontrolled units, with the remainder due to indirect effects of rent control exposure on the amenity value of Cambridge residential units.

These findings are germane to economic analysis of housing market regulations and, more broadly, to the impacts of other place-based policies. The mechanisms by which rent decontrol affects never-controlled housing—increased maintenance, upgrading of local amenities, and potentially more efficient 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.

Appendix A

Theory

We ground our empirical analysis in a stylized equilibrium model of the housing market that considers the relationship between rent control, neighborhood amenities, and house prices.

Neighborhoods.—A city consists of $n = 1, \ldots, N$ 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 given by 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.—Residents have preferences given by

$$U(c, h) = Ac^{1-\alpha} h^{\alpha},$$

where c is a composite commodity, h is housing services, and A is the total level of amenities in the neighborhood. 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. The only heterogeneity in the model comes from differences in income y between residents. The outside utility for a resident with income y is denoted by $\bar{U}_y$.

Amenities depend on neighborhood attributes. To capture the most relevant dimensions for our study, we assume that amenities are increasing in the overall level of maintenance and income of residents as follows:

$$A_n = \int_0^1 [m_n(\ell) y_n(\ell)]^\beta \, d\ell.$$

Here, $m_n(\ell)$ denotes the maintenance level at location ℓ in neighborhood n and $y_n(\ell)$ denotes the income of residents in neighborhood n residing at location ℓ and $\beta \in [0, \alpha)$. The equilibrium concept is based on spatial equilibrium, with free entry and perfect mobility of residents.

Equilibrium definition.—An equilibrium is a triple $\langle y_n(\ell), p_n(\ell), h_n(\ell) \rangle$ where $y_n(\ell)$ is the resident income, $p_n(\ell)$ 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 his 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 keep the model tractable. For the supply side, assume that housing services are produced by the linear technology $f(m) = m$ and the costs of maintenance are quadratic: $c(m) = (1/2)m^2$. These assumptions imply that the optimal level of maintenance at each location is exactly equal to the price of housing services: $m^* = p$. The demand for housing services decreases with price: $h = \alpha y / p$. Next, we assume that the distribution of income among potential residents consists of N distinct levels of y, which we order from highest to lowest, $y_1 > \cdots > y_N$.

We first solve for the equilibrium without rent control as a baseline. We then consider the controlled equilibrium and develop implications for how prices, maintenance, and resident allocation will be affected by decontrol.

Equilibrium without Rent Control

We consider a symmetric equilibrium in which all residents with income y_n live in neighborhood n. The log indirect utility V for a resident of neighborhood n at location ℓ is

$$\ln V(p_n(\ell), y_n) = \ln(A_n) + \ln y_n - \alpha \ln p_n(\ell) + \ln((1-\alpha)^{1-\alpha} \alpha^\alpha).$$

Free entry and perfect mobility of residents imply that in all locations ℓ in neighborhood n, each resident's utility is equal to $\bar{U}_{y_n}$. Hence, the price of housing services at each location ℓ is

$$\ln(p_n(\ell)) = \frac{1}{\alpha}[\ln(A_n) + \ln y_n - \ln \bar{U}_{y_n} + \ln((1-\alpha)^{1-\alpha} \alpha^\alpha)]. \tag{A1}$$

The value of neighborhood amenities comes from the fact that landlords optimally set the level of maintenance to the price of housing services and in the candidate equilibrium all residents of neighborhood n have income y_n. Therefore,

$$\ln(p_n(\ell)) = \frac{1}{\alpha}\left[\beta\int_0^1 \ln m_n(\ell)\,d\ell + (1+\beta)\ln y_n - \ln\bar{U}_{y_n} + \ln((1-\alpha)^{1-\alpha}\alpha^{\alpha})\right] = \ln(m_n(\ell)).$$

Symmetry among landlords implies that maintenance levels within a neighborhood are the same at each location, so that

$$\ln(m_n) = \frac{1}{\alpha}[\beta\ln m_n + (1+\beta)\ln y_n - \ln\bar{U}_{y_n} + \ln((1-\alpha)^{1-\alpha}\alpha^{\alpha})].$$

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

$$\ln(m_n) = \frac{1}{\alpha-\beta}[(1+\beta)\ln y_n - \ln\bar{U}_{y_n} + \ln((1-\alpha)^{1-\alpha}\alpha^{\alpha})] \equiv \ln(m_n^u),$$

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

$$\ln(p_n) = \frac{1}{\alpha-\beta}[(1+\beta)\ln y_n - \ln\bar{U}_{y_n} + \ln((1-\alpha)^{1-\alpha}\alpha^{\alpha})] \equiv \ln(p_n^u).$$

The pricing equation illustrates intuitive patterns under our parameter assumptions $(1>\alpha>\beta\geq 0)$. Prices are higher in neighborhoods when residents have more income and they are lower when residents have better outside options. Landlords invest more in response to exogenous improvements in neighborhood quality because more investment in the neighborhood raises amenities, which raises prices, and landlords set maintenance in response to prices.

Equilibrium with Rent Control

Let $\mathrm{RC}_n \subset [0,1]$ 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 that 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 (A1) 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

$$\ln A_n^c = \beta\left[\int_{\ell \in \mathrm{RC}_n} \ln(\bar{p}_n(\ell))\,d\ell + (1-\lambda_n)\ln m_n + \int_0^1 \ln y_n(\ell)\,d\ell\right].$$

As with the uncontrolled economy, we focus on the equilibrium in which $y_n(\ell) = y_n$ for all uncontrolled locations ℓ. This yields

$$\ln A_n^c = \beta\left\{\int_{\ell \in \mathrm{RC}_n} [\ln(\bar{p}_n(\ell)) + \ln y_n(\ell)]\,d\ell + (1-\lambda_n)(\ln m_n + \ln y_n)\right\}.$$

Since it is set by the rent control authority, the price of all controlled locations in neighborhood n may differ at each location, so we cannot further simplify the first term. For controlled locations, the income of a resident $y_n(\ell)$ depends on the way in which residents are assigned to controlled housing. Let

$$\lambda_n \kappa_n^1 \equiv \int_{\ell \in \mathrm{RC}_n} \ln(\bar{p}_n(\ell))\,d\ell$$

and

$$\lambda_n k_n^2 \equiv \int_{\ell \in \mathrm{RC}_n} \ln(y_n(\ell))\,d\ell.$$

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

$$\kappa_n^2 \leq \ln y_n,$$

which implies that the rationing mechanism imposed by rent control yields misallocation relative to the equilibrium in the uncontrolled economy.[49] The basis for this assumption is the following. If, prior to the implementation of rent

[49] 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 affected by rationing and develop a model of rationing with rent seeking.

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 income; 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, that is, those with lower income levels. The average income 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

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

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

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

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

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

In summary, since neighborhood amenities are a function of the maintenance of all units in a neighborhood and the income of residents, the supply of amenities at noncontrolled locations in neighborhoods with rent controls—as well as maintenance and rents—is also impaired by rent control. This causes lower-income residents to move into noncontrolled locations. Hence, imposition of rent control causes inefficiently low maintenance and misallocation of residents at both controlled and noncontrolled locations within a neighborhood.

The Effect of Rent Control Removal on Rents, Maintenance, and Resident Allocation

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

$$\begin{aligned}\Delta \ln(p_n(\ell)) &= \ln(p_n^u(\ell)) - \ln(p_n^c(\ell)) = \frac{1}{\alpha}\Delta A_n = \frac{1}{\alpha}(A_n^u - A_n^c) \\ &= \frac{\lambda_n \beta}{\alpha - \beta(1-\lambda_n)}\{[\ln(m_n^u) - \kappa_n^1] + (\ln y_n - \kappa_n^2)\},\end{aligned} \tag{A2}$$

where $(1/\alpha)(A_n^u - A_n^c)$ is the indirect effect, $\ln(m_n^u) - \kappa_n^1$ pertains to the maintenance effect being greater than zero, and $\ln y_n - \kappa_n^2$ pertains to the allocative effect being greater than zero.

This expression shows that the end of rent control generates price impacts on uncontrolled locations through two channels in the model. For a given neighborhood, 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 formerly controlled locations involves an additional term that can be decomposed as follows:

$$\begin{aligned}\ln(p_n^u(\ell)) - \ln(\bar{p}_n^c(\ell)) = &[\ln(p_n^u(\ell)) - \ln(p_n^c(\ell))] \\ &+ [\ln(p_n^c(\ell)) - \ln(\bar{p}_n^c(\ell))].\end{aligned} \tag{A3}$$

The first term, the *indirect 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 (A2). The second term, the *direct 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 ($\kappa^1\downarrow$), and
- 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 shows the difficulty involved in distinguishing between direct and indirect effects at decontrolled locations. When rent control ends, there is a direct price effect due to the formerly controlled location being priced by the market. However, there is also an indirect effect as neighborhood amenities improve as a result of increases in maintenance and the income of residents, leading to higher prices. This in turn leads a landlord to invest in additional maintenance. For empirical purposes, at decontrolled locations, the direct and indirect channels cannot be readily distinguished because each affects the equilibrium level of the other.

The model's 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). Third, residents are identical in the model except for their income: within a neighborhood, all residents at uncontrolled locations (though not generally at controlled locations) have the same level of income and, because of spatial arbitrage, obtain the same utility. Finally, the model focuses on one housing market and does not consider neighboring markets that do not have rent control. Although it is not modeled, it is possible that residents at previously controlled locations move out of Cambridge and that residents in these neighboring towns move into Cambridge with the end of rent control.

Tables

TABLE A1
CAMBRIDGE RESIDENTIAL BUILDING PERMITTING ACTIVITY, 1991–2004:
PERMITS ISSUED AND PERMITTED EXPENDITURES

	HOUSES				CONDOMINIUMS			
	Never Controlled		Decontrolled		Never Controlled		Decontrolled	
	1991–94	1995–2004	1991–94	1995–2004	1991–94	1995–2004	1991–94	1995–2004
	A. Permits Issued							
Permits	1,507	4,385	259	694	247	852	185	672
Pr(Permit)	.030	.035	.029	.031	.014	.019	.011	.016
E[Units|Permit]	1.72	1.72	2.54	2.81	12.06	10.95	15.69	16.34
	B. Annual Expenditure (1,000s of 2008 Dollars)							
Total	14,044	29,954	1,588	3,486	3,723	7,595	1,451	4,435
Per unit	1.11	2.37	.72	1.57	.82	1.67	.34	1.05
	C. 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
Minimum	.1	.1	.4	.3	.5	.3	.4	.4
Maximum	5,675.5	4,365.5	451.2	1,208.9	2,121.2	6,589.3	1,480.1	4,450.3

NOTE.—Data source is the universe of Cambridge Inspectional Services permits issued during 1991–2004. If a structure receives multiple permits in a given year, we sum these expenditures and treat them as a single permit. 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 A2

Property Conversions, 1994–2004: Status in 1994 of Units That Were Designated as Houses and Condominiums in 2004

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

Note.—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 A3
Descriptive Statistics for Population and Residential Units and Structures for Various Geographies

	Mean	Standard Deviation	Minimum	Maximum	Median
	A. Census Blocks (587 Blocks)				
Area (square miles)	.01	.02	.00	.53	.00
1990 census population	135.05	162.71	.00	2,833.00	99.00
2001 residential units	62.77	58.71	.00	441.00	45.00
1994 rent control units	22.92	34.48	.00	236.00	11.00
2001 residential structures	18.53	12.08	.00	81.00	16.00
1994 rent control structures	4.08	3.77	.00	21.00	3.00
	B. Census Block Groups (89 Block Groups)				
Area (square miles)	.07	.07	.01	.56	.05
1990 census population	986.17	506.00	98.00	3,093.00	836.00
2001 residential units	428.15	253.62	23.00	1,418.00	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
	C. Census Tracts (30 Tracts)				
Area (square miles)	.22	.17	.05	.72	.16
1990 census population	3,144.73	1,291.67	1,736.00	7,123.00	2,650.00
2001 residential units	1,291.68	510.60	336.00	2,984.46	1,244.07
1994 rent control units	470.77	341.71	101.00	1,534.00	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
	D. .2-Mile Radius (10,968 Map-Lots)				
Area (square miles)	.13	. . .	.13	.13	.13
1990 census population	3,160.48	1,765.02	.00	15,796.90	2,935.48
2001 residential units	1,141.15	573.10	5.00	3,427.54	1,066.16
1994 rent control units	422.34	330.59	.00	1,702.00	376.00
2001 residential structures	348.40	116.72	1.00	676.00	351.00
1994 rent control structures	80.15	46.52	.00	180.00	77.00

Note.—Panels A–C provide statistics for all census geographies containing at least one assessed residential housing structure. Panel D provides statistics for the universe of 0.2-mile radius geographies, centered at each residential housing structure.

TABLE A4

DESCRIPTIVE STATISTICS FOR TRANSACTED PROPERTIES

	HOUSES				CONDOMINIUMS			
	Never Controlled		Decontrolled		Never Controlled		Decontrolled	
	1988–94	1995–2005	1988–94	1995–2005	1988–94	1995–2005	1988–94	1995–2005
Log price	12.84	13.26	12.59	13.03	12.56	12.81	12.20	12.57
	(.69)	(.74)	(.67)	(.67)	(.51)	(.55)	(.56)	(.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)	(.78)	(.96)	(.70)	(.81)
Bathrooms	2.77	2.81	2.93	2.91	1.57	1.63	1.17	1.24
	(.94)	(.95)	(.87)	(.85)	(.67)	(.75)	(.44)	(.52)
Interior square feet	2,363.41	2,387.34	2,408.88	2,409.76	1,202.67	1,269.57	927.85	949.69
	(1,131.25)	(1,071.66)	(920.96)	(902.49)	(834.76)	(819.75)	(434.02)	(449.68)
Has lot (y/n)	.99	.99	.99	.99	.02	.04	.04	.03
	(.11)	(.09)	(.09)	(.09)	(.14)	(.19)	(.18)	(.17)
Lot size square feet	4,211.71	4,253.09	3,320.15	3,462.02	113.24	157.66	191.18	151.38
	(3,433.26)	(3,437.64)	(1,964.22)	(2,031.41)	(1,595.75)	(1,145.06)	(1,222.04)	(1,148.19)
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)
Observations	1,624	2,599	255	336	2,138	3,626	1,446	2,765

NOTE.—The sample includes Cambridge houses and condominiums transacted during 1988–2005. Sales prices, 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 nonzero lot size.

TABLE A5

Comparison of Estimated Relationship between Rent Control Status, Rent Control Intensity, and Transacted Prices versus 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)
	A. Houses					
RC × Post	.199	.127	.352**	.114	.059	.194
	(.124)	(.125)	(.163)	(.078)	(.081)	(.137)
RCI × Post		.606**			.452**	
		(.294)			(.189)	
Non-RC × RCI × Post			.736***			.522***
			(.278)			(.193)
RC × RCI × Post			−.539			−.172
			(.828)			(.615)
Observations	685	685	685	652	652	652
	B. Condominiums					
RC × Post	.163**	.085	.073	.168**	.133*	.122*
	(.072)	(.068)	(.063)	(.071)	(.074)	(.069)
RCI × Post		.512**			.255	
		(.200)			(.201)	
Non-RC × RCI × Post			.406			.110
			(.280)			(.294)
RC × RCI × Post			.709**			.516
			(.291)			(.366)
Observations	937	937	937	7,897	7,897	7,897

Note.—Samples includes houses (panel A) and condominiums (panel B) that transacted in 1994 and 2004. Regression models follow col. 5 of table 4. In cols. 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 cols. 1–3 than in cols. 4–6 because a unit may transact more than once per year. The number of observations for condominiums in cols. 4–6 is larger than in cols. 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 × RCI or RC × 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 the note to table 6 for additional details.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

TABLE A6
SOMERVILLE BORDER ANALYSIS

	Dependent Variable: Log of Transacted Price		
	All (1)	Houses (2)	Condominiums (3)
	A. Neighboring Census Tracts		
Cambridge tract × Post	.069***	.066**	.059
	(.023)	(.028)	(.050)
Observations	5,700	4,398	1,302
R^2	.606	.598	.637
	B. Neighboring Census Block Groups		
Cambridge block group × Post	.102**	.101*	.018
	(.042)	(.050)	(.085)
Observations	2,775	1,991	784
R^2	.617	.607	.646

NOTE.—The sample includes transactions in Somerville that took place in a census tract or block group abutting either Cambridge (on the south) or Medford (on the north). Year fixed effects, property characteristics, and census tract fixed effects are included in all specifications. Robust standard errors clustered by 1990 block group are in parentheses.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

References

Arnott, Richard. 1995. "Time for Revisionism on Rent Control." *J. Econ. Perspectives* 9:99–120.

Atlantic Marketing Research. 1998. "Impacts of the Termination of Rent Control on Population, Housing Costs and Housing Stock." Technical report (January), Atlantic Marketing Res., Boston.

Bulow, Jeremy, and Paul Klemperer. 2012. "Regulated Prices, Rent-Seeking, and Consumer Surplus." *J.P.E.* 120 (1): 160–86.

Campbell, John, Stefano Giglio, and Parag Pathak. 2011. "Forced Sales and House Prices." *A.E.R.* 101 (5): 2108–31.

Cantor, Patricia. 1995. "Massachusetts Defeats Rent Control." *Shelterforce* (April/May). http://www.nhi.org/online/issues/80/massrent.html.

Epple, Dennis. 1988. "Rent Control with Reputation: Theory and Evidence." *Regional Sci. and Urban Econ.* 28 (6): 679–710.

Fallis, George, and Lawrence B. Smith. 1984. "Uncontrolled Prices in a Controlled Market: The Case of Rent Controls." *A.E.R.* 74:193–200.

Fetter, Daniel. 2013. "The Home Front: Rent Control and the Rapid Wartime Increase in Home Ownership." Manuscript, Wellesley Coll.

Finkel, Meryl, and James E. Wallace. 1987. "Cambridge Housing Survey." Abt Assoc., Cambridge, MA.

Frankena, Mark. 1975. "Alternative Models of Rent Control." *Urban Studies* 12: 303–8.

Friedman, Milton, and George Stigler. 1946. *Roofs or Ceilings? The Current Housing Problem.* Popular Essays on Current Problems, vol. 1, no. 2. Irvington-on-Hudson, NY: Found. Econ. Educ.

Fujita, Masahisa. 1991. *Urban Economic Theory: Land Use and City Size.* Cambridge: Cambridge Univ. Press.

Glaeser, Edward, and Joseph Gyourko. 2009. *Rethinking Federal Housing Policy: How to Make Housing Plentiful and Affordable.* Washington, DC: AEI Press.

Glaeser, Edward, and Erzo Luttmer. 2003. "The Misallocation of Housing under Rent-Control." *A.E.R.* 93 (4): 1027–46.

Guerrieri, Veronica, Dan Hartley, and Erik Hurst. 2013. "Endogenous Gentrification and Housing Price Dynamics." *J. Public Econ.* 100 (April): 45–60.

Gyourko, Joseph, and Peter Linneman. 1989. "Equity and Efficiency Aspects of Rent Control: An Empirical Study of New York City." *J. Urban Econ.* 26 (1): 54–74.

Hartley, Dan. 2010. "The Effect of Foreclosures on Nearby Housing Prices: Supply or Disamenity?" Working Paper no. 10-11, Fed. Reserve Bank Cleveland.

Haveman, Judith. 1998. "Mass. City Gets a New Lease on Life; Cambridge Booms as Rent Control Ends, but the Poorer Pay a Price." *Washington Post*, September 19.

Ioannides, Yannis. 2003. "Interactive Property Valuations." *J. Urban Econ.* 53:145–70.

Kasy, Max. 2013. "Identification in a Model of Sorting with Social Externalities and the Causes of Urban Segregation." Working paper, Harvard Univ.

Landvoigt, Tim, Monika Piazzesi, and Martin Schneider. 2012. "The Housing Market(s) of San Diego." Working paper, Stanford Univ.

Leonard, Dutch. 1981. "Housing Regulation in Cambridge: Its Goals and Effects." Chamber of Commerce, Cambridge, MA.

Linden, Leigh, and Jonah Rockoff. 2008. "There Goes the Neighborhood? Estimates of the Impacts of Crime Risk on Property Values from Megan's Law." *A.E.R.* 98 (3): 1103–27.

Mian, Atif, and Amir Sufi. 2009. "The Consequences of Mortgage Credit Expansion: Evidence from the U.S. Mortgage Default Crisis." *Q.J.E.* 124 (4): 1449–96.

Mian, Atif, Amir Sufi, and Francesco Trebbi. 2011. "Foreclosures, House Prices, and the Real Economy." Working paper, Booth School, Univ. Chicago.

Olsen, Edgar. 1972. "An Econometric Analysis of Rent Control." *J.P.E.* 80 (6): 1081–1100.

Pope, Jaren C. 2008. "Fear of Crime and Housing Prices: Household Reactions to Sex Offender Registries." *J. Urban Econ.* 64 (November): 601–14.

Rossi-Hansberg, Esteban, Pierre-Daniel Sarte, and Raymond Owens. 2010. "Housing Externalities." *J.P.E.* 118 (3): 829–58.

Schwartz, Amy Ellen, Ingrid Gould Ellen, Ioan Voicu, and Michael Schill. 2006. "The External Effects of Place-Based Subsidized Housing." *J. Urban Econ.* 36 (6): 679–707.

Sims, David P. 2007. "Out of Control: What Can We Learn from the End of Massachusetts Rent Control?" *J. Urban Econ.* 61 (1): 129–51.

———. 2011. "Rent Control Rationing and Community Composition: Evidence from Massachusetts." *B.E. J. Econ. Analysis and Policy* 11 (1).

Suen, Wing. 1989. "Rationing and Rent Dissipation in the Presence of Heterogeneous Individuals." *J.P.E.* 97 (6): 1384–94.

Tucker, William. 1986. "Anarchy, State and Rent Control." *New Republic*, December 22, 20–21.

Wang, Shing-Yi. 2011. "State Misallocation and Housing Prices: Theory and Evidence from China." *A.E.R.* 101 (5): 2081–2107.