

Rent Control Rationing, Community Composition, and Residential Segregation*

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Abstract: This paper considers the effects of rent control on community socioeconomic composition. In particular, do rent controls increase the presence of poor and minority residents in a locale? Theoretically, the effect of rent controls on community composition depends on a tradeoff between lower prices which might favor the disadvantaged and a landlord imposed rationing mechanism which likely does not. Using data on Cambridge, Massachusetts and adjacent census tracts straddling the state imposed end of rent controls, I find evidence that rent control increased the presence of minority residents but also decreased the proportion of poor residents. I also find despite its positive impact on minority membership, rent control is associated with an increase in traditional measures of residential segregation.

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Price controls are one of the most effective illustrative examples presented in economics principles classes. Few other policies can match price ceilings in the ability to illuminate the power of prices as a rationing mechanism. Outside the classroom, rent controls are one of the most studied forms of price control, likely because they have such relatively large potential effects on the budgets of consumers and the fortunes of suppliers. Indeed an important literature has established the importance of rent controls in the deterioration of the housing stock (e.g. Pollakowski 1999, Moon and Stotsky 1993, Gyourko and Linneman 1990) and in the mismatch of tenants and apartments (Glaeser and Luttmer 1999).

In the face of these efficiency arguments, the most common response of rent control advocates is to emphasize the need for rent control as a means to allow minority, poor, and working class families the opportunity to live in cities that would otherwise be closed to them. There is evidence, most notably from the Moving to Opportunity experiment (Katz, Kling and Liebman 2001), that exposure of the disadvantaged to better neighborhood environments can improve their outcomes. However, the claim that rent control leads to a different community composition has rarely been tested.

There is a related literature that shows that direct subsidy benefits of rent control, that is the transfer of surplus from landlords to tenants, is disproportionately claimed by the white, economically advantaged renters, at least in New York city (Gyourko and Linneman 1989, Pollakowski 2003). However, the more subtle questions of how the existence of rent controls changes the demographic composition of and residential segregation in an area have only recently been raised in academic circles by Glaeser (2003). In an examination of city level changes between the 1970 and 1990 censuses for some California and New Jersey cities with or without rent control, he concludes that there is some evidence that rent control may have given

the poor in California access to otherwise inaccessible living situations. However, the author admits that it is difficult to determine whether the differences across these cities are actually due to rent control.

This paper expands this field of inquiry by looking at the effect of ending rent control on the community composition, along both racial and economic dimensions, and residential segregation in Cambridge, Massachusetts. As the end of rent control in Cambridge came about due to a statewide ballot initiative, I argue that this represents a plausibly exogenous policy shift that is unrelated to the housing market conditions in Cambridge or its neighbors at the time. I briefly discuss how non-price rationing mechanisms under rent control may affect community composition. In my empirical analysis I use first bordering communities, and then only bordering areas of those communities as a control group to measure these effects.

I find weak evidence that rent control leads to higher minority populations in an area. However, I also find that it leads to less diversity among economic lines, that is, average residents are wealthier and less likely to have incomes below the poverty line under rent control. This finding is especially surprising in light of an economic boom that followed the end of rent control in Massachusetts. Finally, I find some suggestive evidence that rent control increases residential segregation by commonly measured indices even as it allows more minority residents.

II. Background

A. Rent control in Cambridge

Of the three Massachusetts cities that retained rent control ordinances by 1990, Cambridge had the strictest regime. First, unlike the cities of Brookline and Boston, the Cambridge ordinance did not include any form of vacancy decontrol, that is, landlords were not

permitted to reset rates to market levels when a tenant left. All rental increases had to be approved by the Cambridge Rent Control Board, which by the late 1980's approved a yearly general rent increase for all controlled apartments in the city indexed to a fraction (0.85) of CPI growth. Other increases could be approved on an individual basis only if the landlord could provide proof of an operating cost increase such as a rise in property taxes or utility rates.¹

The Cambridge law was also concerned about other ways in which the landlord might attempt to evade the intent of rent control. For example, condominium conversions required the express approval of the Rent Control Board, and when allowed would involve generous financial compensation to the present tenant. Another set of regulations forbade the eviction of controlled tenants without approval of the board. A short list of grounds for eviction the board might approve was codified in the laws, and landlords faced the burden of proving violations. Another regulation set monetary punishments for landlords who failed to maintain the provision of essential services such as heat and running water to their units. Most controversially, Cambridge forbade landlords from leaving controlled units vacant for more than three months.

Despite these regulations, only about a third of total Cambridge housing units were subject to rent control by 1990.² This is largely due to the exemption of owner-occupied 2 and 3 family homes as well as post 1971 construction from the ordinance as well as a mechanism whereby a unit could obtain an exemption in return for a complete and substantive remodeling (which often had a required cost figure of tens-of thousands of dollars attached. The prevalence of rent control also varied widely across different portions of the city as can be seen in figure 1. In the figure darker areas have a larger fraction of rent controlled units.

¹ Even then the process could be excruciating. In 1988–89 the median time for the Cambridge board to rule on a rent increase for fair operating costs was 171 days. Even with general rate increases almost 10% of controlled units in Cambridge had a set rent of 200 dollars or less in 1989

² A 1991 HUD report puts the figure at 38%.

Throughout the early 1990's the fight to end rent control was led by the Small Property Owners Association (SPOA), which sponsored the drive behind the 1994 statewide ballot initiative, Question 9, that banned rent control throughout the state. The question won approval by a narrow margin (51% favored it). After failing to obtain judicial intervention, tenant advocates sought help from the state legislature. They argued the end of rent control on January 1, 1995 would be marked by wholesale evictions of the poor and elderly, and a massive rise in homelessness. Finally, the governor brokered a compromise whereby tenants that met certain age or poverty guidelines could retain their controlled unit status for a 1–2 year transition period. Thus, most units were immediately decontrolled, but a few remained in control until January 1997. Surprisingly, only about seven percent of rent controlled tenants received such assistance due to an unexpectedly small number of poor applicants.

B. Rent Control and community composition.

One likely reason why there has been little research on the subject of rent control and community composition is the lack of a clear theoretical prediction as to the net effect of rent control on poor and minority renters. Indeed, there are at least three mechanisms by which rent control could plausibly affect community composition, and not all of them have a clearly predictable sign.

First, with the initial passage of rent control, the short run is likely to be dominated by the effect of rent control on extending tenant stays. Indeed, this effect is a straightforward consequence of the below market price of housing. This reduced price increases the opportunity cost of a tenant moving and encourages sclerosis in the rental market. This in turn reduces the rate of change of community composition. Presumably the rent control was passed because of

some upward price shock, suggesting the existing community structure may include some number of poor and minority households, which are more likely to stay in the community, just as housing advocates suggest. Also, if opportunities to move are better for the wealthy, the stagnating effect of rent control on tenant turnover is likely to be greater among the poor. Empirical research suggests that this effect can be large (Ault, Jackson, and Saba 1994, Rapaport 1992, Sims 2007) extending average tenant stay duration up to 25%.

However, over time this effect is likely to be less important in an institutional environment such as Cambridge as many of the initially rent controlled tenants move out and are replaced by new tenants who enjoy some sort of price subsidy from the first day of tenancy. Indeed a lower price would presumably increase demand among potential underprivileged tenants. However, their willingness to occupy the apartment might not be the key factor in determining community composition.

Since basic price ceiling models clearly suggest the demand for such apartments will outstrip the supply, what type of tenants replace the initial group depends crucially on the sort of non-price rationing mechanism that can be adopted. Though there is little empirical evidence on this issue, it is hard to believe that the chosen rationing mechanism likely to disproportionately favor poor or minority tenants. Indeed, while there could be a queuing mechanism to ration the excess demand, a far more likely scenario is that landlords will ration the apartments according to their own personal objective functions.

This leads to a couple of possibilities that could be unfavorable to disadvantaged potential tenants. First, given the costs he or she faces due to the practical difficulty of evicting even a non-paying tenant, a landlord may rationally use group identity as a signal of the likelihood of rent payment, favoring wealthier tenants. Secondly, the landlord could engage in

any sort of Beckerian taste based discrimination consistent with his own preferences, as the potential queue of apartment hunters reduces the costs of doing so. Indeed, the Beckerian insight that a free market makes discrimination costly seems key here. Even in a case where the landlord is not motivated by group animus, the reduced cost of waiting for another potential tenant makes even statistically based discrimination more likely.

Of course, the preceding assumes sufficient demand for these apartments among affluent tenants, which may not follow if we allow differentiated housing characteristics to affect demand. Indeed, one of the most consistent findings in the empirical rent control literature is the negative effect rent control has on housing maintenance. If the resulting apartment condition selectively reduces demand among affluent residents, the potential for discrimination may be overstated.

The discussion so far has been confined to the effect of rent control on the tenants of controlled units themselves. However, as Sims (2007) has noted, areas with a high fraction of rent controlled units may see an effect on unit condition, and hence on the type of tenants through spillover effects. Furthermore, Glaeser (2003) points out that the effects of rent control may differ depending on the macroeconomic environment the controlled city faces. A city that faces a boom is likely to see an exogenous demand shock from affluent tenants, while a city facing a contraction may find that rent control is not the only factor increasing the proportion of poor tenants in the locale.

Thus, while the initial effect of lengthening tenant stays may indeed keep poor tenants in a rent controlled housing market, the long term community composition will also depend on the tension between more demand for housing in that locale among the poor and a landlord rationing of that housing. Given the further complication posed by quality deterioration due to rent control,

there seems to be little in the way of a clear theoretical prior on what rent control should do to community composition or residential segregation. Thus, the remainder of this paper approaches the question empirically.

III. Data.

Much of the literature attempting to measure how the benefits from rent control are distributed has used unit level sample data, most often from the American Housing Survey and its special supplements. While such data is available for the Boston area, it suffers from some serious potential problems when considering the current project. Perhaps the most serious is the lack of sufficient geographic precision to isolate the rent controlled communities of Cambridge and Brookline. However, there is also a potential problem of the underreporting of rent controlled status of units by poor and minority tenants. Given these difficulties, as well as the desirability of looking at the community as a whole when considering community composition, I have chosen to forego unit level data in favor of averages across census tracts.

Thus, my demographic data comes from the 1990 and 2000 census records for all census tracts in Cambridge and its bordering communities of Somerville, Arlington, Belmont, and Watertown.³ This includes data on the residents' race and ethnicity, average household incomes, and poverty rates. I also obtained data on tract level measures of housing characteristics, including the vacancy and owner occupancy rates, as well as median values and median rents for 1990 only.⁴ For the most part geographical tract changes in this area between the 1990 and 2000 censuses are trivial with the exception of one tract in a neighboring community that was split into two and is dropped from the subsequent analysis.

³ Although Cambridge technically shares a land border with Boston (via Charlestown), this is a portion of Cambridge given entirely to industrial use so I do not use Boston as a comparison city.

⁴ All dollar amounts are inflation adjusted to 2000 dollars using the CPI.

Data on rent controlled units in Cambridge comes from the city records. I aggregated this information to the tract level and produced a measure of the fraction of housing units in a given tract that are under rent control.

Because the questions concerning race were fundamentally changed between the 1990 to 2000 censuses, there is a real difficulty in interpreting measures of racial change during this time period. Prior to the 2000 census, respondents were asked to select a race group that described them. Starting in 2000, however, they may choose to belong to multiple race groups. While there are several statistical tricks that can be used to try and apportion respondents to single race groups, each involves assumptions that might dictate the outcome in the present study. For the sake of completeness I have chosen to present two measures: the first attributes to a race group in 2000 exclusively those who selected only that one race. The likely bias of this measure is to underestimate the minority proportion, relative to 1990. The second measure counts all individuals in a race group as members of that group no matter what other groups they did or did not select. That is, relative to 1990, it overstates minority populations. Since I am comparing the changes from 1990 to 2000 in census tracts with rent control to those without as long as there are no changes in how individuals report their race across areas that are correlated with rent control, both measures should produce similar estimates. If reporting is correlated with rent controlled areas, the two measures should provide a useful bracketing of the true diversity changes.

Table 1 reports descriptive statistics for Cambridge and neighboring cities separately. The data is pooled across years. Columns (2) and (4) of the table further isolate tracts along the Cambridge border. It is quite interesting to see the ways in which these cities differ. Aside from the obvious factor of rent control, Cambridge has a much larger fraction of its housing units renter occupied and much larger minority populations. However, it is also interesting that

looking exclusively areas along the border the two groups have almost identical median incomes and much more similar rental rates. Also, a comparison of the inclusive with exclusive race measures shows that the inclusive measures are always higher, most often by a fraction of a percentage point.

IV Methods and results

A. Empirical methodology

This study seeks to identify the effect of rent control on community composition and residential segregation. To do this I attempt to model how differences in levels of rent control effect the demographic changes that follow from its termination. In particular I consider a demographic change in measure Y as:

$$\Delta Y_i = Y_{i,1990} - Y_{i,2000} \quad (1).$$

And model it statistically as

$$\Delta Y_i = \Delta X_i' \alpha + \lambda \Delta RC_i + \varepsilon_i \quad (2),$$

Where the demographic change in tract i results from a change in rent control as well as other control variables, X, and an error term ε_i . I will weight all regressions by the number of units in the zone. Here rent control as well as the race and ethnicity are all fractional measures of the population in the tract with that characteristic. The key identifying assumption is that rent decontrol, conditional on included control variables, is uncorrelated with the error term. This assumption becomes more plausible in differences rather than levels, as all tract specific, time invariant factors are differenced out. Still, the interpretation of λ as a rent control effect requires the control cities to properly capture the counterfactual. Thus, in my ultimate specification I will only consider areas on the Cambridge border to ensure as close comparability as possible.

B. Results

Panel A of Table 2 considers regressions of race and ethnicity changes in a tract on the change in tract fraction rent controlled. For the three race groups considered, white, black and asian, results are reported for both a measure utilizing an exclusive definition of 2000 race group membership (e.g. white only) and the more inclusive definition. The results suggest that rent control was not related to statistically significant changes in the fraction of white or hispanic residents, but is related to an increased fraction of black residents and a decreased fraction of asians. In all three cases with multiple race measures, the exclusive measure produces a more positive coefficient.

Panel B adds controls for tract level housing characteristics to the regression, including the fraction of units that were rentals and the fraction vacant units, as well as preexisting median value and rent levels. I experimented with alternative controls such as average rooms per unit and age of construction but found very little variation after differencing. Furthermore the inclusion of further controls raised the standard errors without modifying the coefficient of interest meaningfully. Thus, given my relatively small sample size I have chosen to report these more parsimonious regressions.

The results for minority groups remain same signed and for blacks and hispanics are generally larger. The results for asians are similar to the regressions of panel A, while rent control now appears to have a negative, statistically significant impact on the fraction of whites in a tract. If these coefficients are taken at face value, they would suggest that for an average Cambridge tract with about a third of its units controlled, the removal of rent control is responsible for a gains of 2-2.5 percentage points of white residents and 1 percentage point of Asians, as well as a loss of 1.7-2 percentage points blacks and 1.2 percentage points Hispanics.

The coefficients on the control variables tend to have the expected signs with a few exceptions, mostly involving the hispanic regression. For instance, a higher fraction of rentals is associated with more black residents but has a statistically imprecise effect on other racial groups.

Beyond the effects of rent control on the racial and ethnic composition of the community, it seems even more likely that rent control will affect the economic makeup of geographical areas. Table 3 explores this question by using the log of tract level average household income and the poverty rate in the tract as the dependent variables. The regressions are otherwise similar to those described for Table 2. While the point estimate in column (1) is positive, the standard error is too large to rule out either small positive or negative associations. Column (2), on the other hand, shows a statistically significant negative relationship between the poverty rate and rent control. While at first glance this sounds like an unambiguously desirable effect, it does not suggest that rent control is making people richer, but rather that fewer extremely poor people lived in high rent control areas in Cambridge during rent control than did after controls were abolished. Indeed, for an average tract in Cambridge with about one-third rent controlled units, the coefficient suggests that over two and a half percentage point fewer residents were extremely poor under rent control than after. Since the poverty measure accounts for income rather than costs, this seems unlikely to be in any way caused by rising rents after the end of rent control.

While these estimates are certainly interesting, they depend crucially on the use of adjacent cities as a control group. To attempt to improve the quality of this control group I next repeat the preceding analysis using only the tracts on either side of the Cambridge border. This limits the geographic scope to areas that are less likely to differ widely. Indeed, as residents along the Cambridge-Somerville border can attest, there are numerous places one can cross the border without crossing a street or otherwise being aware of the transition to a different city.

These estimates are presented in Table 4. Although the coefficients are unreported, the included controls are the same as those of the two previous tables. In considering the impact of rent control on the racial and ethnic mix of inhabitants the positive significant effect on hispanics remains from the previous analysis. While the coefficient on fraction black residents remains positive and has attenuated somewhat, the large standard errors make it impossible to reject the point estimates of Table 2. When considering the economic composition of the population, however, the results actually become stronger. Rent control is now associated with a tract having residents that are both richer on average and less likely to be in poverty. In fact for the hypothetical average border tract, these results suggest rent control elevates median income an average of ten percent and reduces the population in poverty almost seven percentage points (off an average around ten). Thus my reading of the table is that rent control may be responsible for an area having noticeably richer residents on average while it provides weaker evidence that rent control may also increase the fraction of traditionally disadvantaged minority groups in the area.

In his paper on the subject of rent control and residential segregation, Glaeser (2003) notes that this last point is insufficient to determine whether rent control is improving the situation of minorities by allowing them access to more diverse neighborhoods. He points out that if the increased minority presence is highly concentrated in certain areas, it may actually increase the isolation of that group within the city. In order to explore this possibility, I use census data on the demographic composition of the block group components of census tracts to compute dissimilarity and isolation indices for each of my sample census tracts in 1990 and 2000. These indices are two of the more commonly used measures in the residential segregation literature,. However, they are usually computed with the tract as the principal component of a

city index, so my particular results may not be comparable in magnitude to the established residential segregation literature. I also choose to compute the indices separately for both my inclusive and exclusive measures of race group membership.

Table 5 presents the results of regressions in which the change in these indices are used as dependent variables. In line with the Glaeser result, one clear effect of rent control appears to be increasing the isolation of black residents in Cambridge. The dissimilarity index also shows a large positive coefficient, but it is not statistically distinguishable from zero. Other results suggest that the estimated effect on Asians is close to zero and that there is also a borderline positive effect of rent control on the isolation of Hispanics.

C. Discussion

These results are suggestive of some very interesting effects of rent control on community composition. First, the strongest result of the study appears to be a positive connection between incomes, at least in some range, and rent control percentages. The consistent finding that fewer residents are in poverty under rent control seems at first to be counterintuitive, given the standard story about the post-rent control boom forcing the poor to leave Cambridge. It is consistent, however, with the surprisingly small proportion of Cambridge residents that applied for the government's assistance programs to help those affected by rent decontrol. It is also consistent with a story in which the poor are willing to occupy rent controlled apartments, but are less able than the affluent to access them through the rationing mechanism. Ironically enough it may be that the end of rent control was what allowed more poor families to live in Cambridge.

A weaker, but still interesting result, is the apparent positive association between black and hispanic residents and rent control. While rent control did not favor the poor, it did appear to

favor minorities in this way. One reading of these results is that landlords were using an income based signal rather than some sort of Beckerian racial animus in their rationing decisions, but this is clearly still speculation. The results on statistical discrimination also suggest that the additional minority residents tended to concentrate in small areas, which may have lessened the purported integration benefits they received.

A number of caveats should, of course, be applied to these conclusions. Even if they are substantively correct they apply to one city, Cambridge, with a particularly severe form of rent control relative to other U.S. cities, and may not be generalizable. Also, there are a number of potential pitfalls in the analysis itself. First the sample size is relatively small. Second, the identification depends on the extent to which bordering cities or census tracts constitute good controls. Finally, it is possible that a number of other housing innovations may have affected the Cambridge market in this decade, and I may be attributing some of their effects to the end of rent control. Nevertheless, even with these shortcomings, I believe the results are an interesting addition to our knowledge of the potential effects of rent control on a community.

V. Conclusion

The externally imposed end of rent control in Massachusetts provides a potential natural experiment to test the effects of rent control on community composition, most particularly on the socioeconomic makeup of the community. I match 1990 and 2000 census tract data with administrative records on rent control for Cambridge, Massachusetts and relate the demographic changes in that decade to the degree of rent decontrol a census tract experienced. I find some evidence that rent control leads to higher community representation of traditionally disadvantaged groups but that it also leads to less representation of the poorest groups in society.

I also find that rent control increases the isolation of minorities which likely make it a poor tool for residential integration.

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Figure 1: Rent Control Density in Cambridge circa 1990

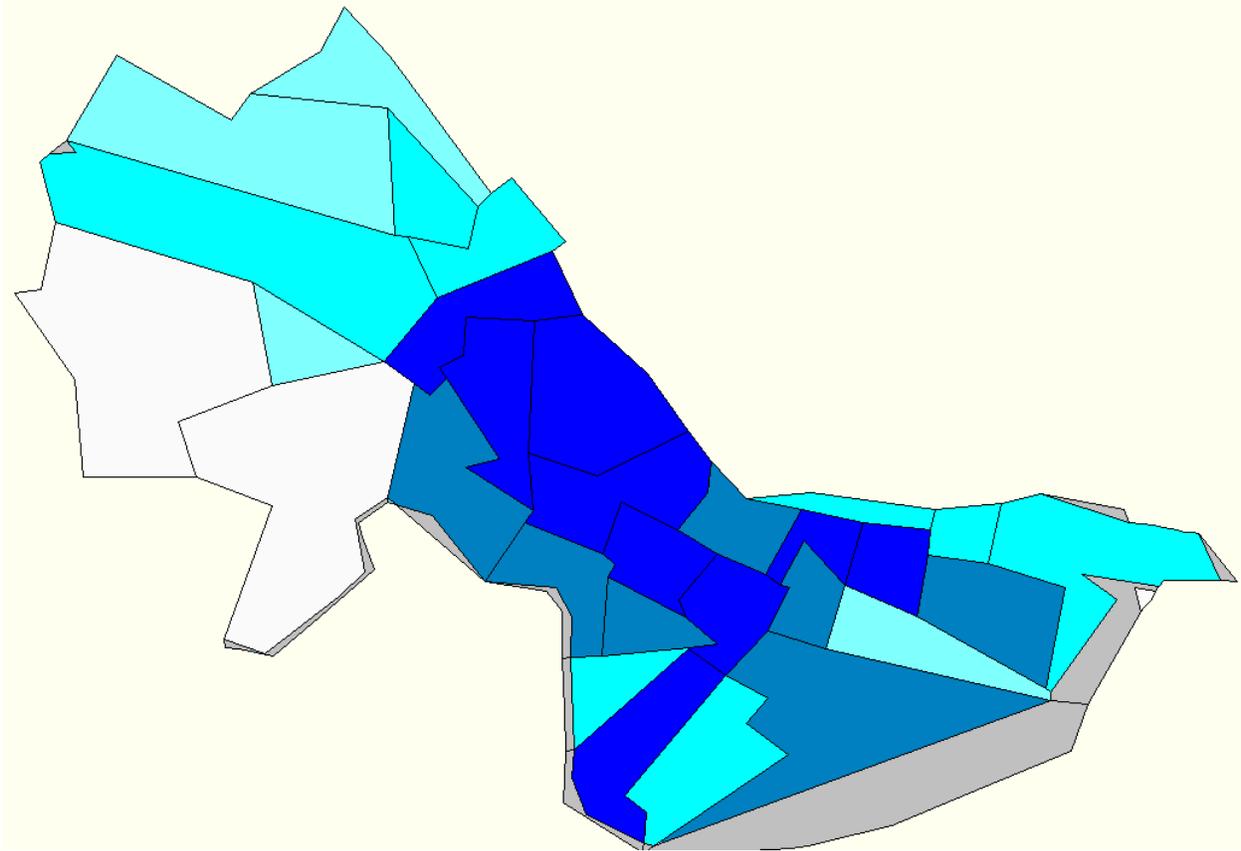


Table 1: Descriptive statistics for Cambridge and neighboring city tracts.

	Cambridge tracts		Adjacent city tracts	
	All	Border	All	Border
	(1)	(2)	(3)	(4)
<i>A. Housing Measures</i>				
Fraction rent controlled	0.159 (0.187)	0.134 (0.179)	0.000	0.000
Fraction rental	0.666 (0.118)	0.632 (0.119)	0.467 (0.197)	0.577 (0.174)
Fraction vacant	0.054 (0.032)	0.058 (0.042)	0.032 (0.013)	0.034 (0.011)
1990 Median unit value (in thousands)	258.147 (109.260)	277.693 (120.301)	240.944 (78.607)	210.964 (59.241)
1990 Median unit rent	480.000 (121.700)	484.923 (143.930)	721.231 (140.431)	695.181 (115.820)
<i>B. Financial Measures</i>				
Log household income	11.070 (0.322)	11.122 (0.379)	11.281 (0.384)	11.102 (0.301)
Poverty Rate	0.117 (0.062)	0.988 (0.052)	0.065 (0.041)	0.070 (0.039)
<i>C. Race/Ethnicity</i>				
White	0.731 (0.152)	0.778 (0.137)	0.895 (0.066)	0.881 (0.076)
Black	0.129 (0.114)	0.102 (0.099)	0.029 (0.028)	0.040 (0.040)
Asian	0.083 (0.057)	0.075 (0.041)	0.050 (0.022)	0.049 (0.020)
Hispanic	0.072 (0.044)	0.054 (0.031)	0.034 (0.033)	0.038 (0.036)
<i>D. Inclusive Race</i>				
White	0.748 (0.148)	0.793 (0.137)	0.904 (0.055)	0.0893 (0.066)
Black	0.139 (0.119)	0.110 (0.104)	0.033 (0.031)	0.043 (0.041)
Asian	0.089 (0.060)	0.080 (0.045)	0.054 (0.025)	0.054 (0.024)
N=	60	26	104	22

Table reports standard deviations below means in parentheses. Means are averages of 1990 and 2000 values except for the marked 1990 housing measures. Inclusive race variables were only incorporated in the 2000 census.

Table 2: Effect of changes in rent control on census tract racial and ethnic composition

Fraction of Census Tract Residents that are:	White (only)	White (any)	Black (only)	Black (any)	Asian (only)	Asian (any)	Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Explanatory Variable:</i>							
<i>A. No controls</i>							
Change in fraction rent control	0.003 (0.023)	-0.022 (0.018)	0.044 (0.010)	0.023 (0.010)	-0.020 (0.011)	-0.033 (0.011)	0.011 (0.011)
<i>B. Controls Included</i>							
Change in fraction rent control	-0.059 (0.042)	-0.074 (0.032)	0.061 (0.019)	0.050 (0.019)	-0.015 (0.012)	-0.030 (0.013)	0.036 (0.020)
Change in fraction rental	-0.143 (0.151)	-0.103 (0.134)	0.171 (0.080)	0.165 (0.067)	-0.067 (0.076)	-0.066 (0.078)	-0.026 (0.058)
Change in fraction vacant	0.351 (0.316)	0.096 (0.285)	0.026 (0.155)	-0.146 (0.116)	-0.078 (0.139)	-0.104 (0.136)	0.229 (0.139)
Pre-existing median unit value (in hundreds of thousands of dollars)	0.007 (0.005)	0.003 (0.004)	0.004 (0.003)	0.002 (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)
Pre-existing median unit rent (in thousands of dollars)	0.137 (0.046)	0.108 (0.035)	-0.025 (0.021)	-0.049 (0.021)	0.010 (0.012)	-0.008 (0.013)	0.048* (0.021)
R ²	0.34	0.26	0.20	0.23	0.08	0.14	0.11

Standard errors are below estimated coefficients in parentheses. N=82 tracts

Table 3: Effect of changes in rent control on census tract economic composition

Effect on:	Log of average household income	Poverty rate
	(1)	(2)
Explanatory Variable:		
<i>C. No controls</i>		
Change in fraction rent control	0.019 (0.068)	-0.069 (0.023)
<i>D. Controls Included</i>		
Change in fraction rent control	0.025 (0.087)	-0.086 (0.031)
Change in fraction rental	-1.928 (0.365)	0.359 (0.122)
Change in fraction vacant	-0.187 (0.763)	-0.375 (0.324)
Pre-existing median unit value (in hundreds of thousands of dollars)	0.012 (0.016)	-0.004 (0.005)
Pre-existing median unit rent (in thousands of dollars)	-0.007 (0.092)	0.013 (0.027)
R ²	0.23	0.23

Standard errors are below estimated coefficients in parentheses. N=82 tracts.

Table 4: Effect of changes in rent control on census tract demographic composition – border tracts only

Demographic measure	Black (only)	Black (any)	Asian (only)	Asian (any)	Hispanic	Average Ln inc	Poverty
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Change in fraction rent control	0.001 (0.038)	0.039 (0.031)	0.013 (0.040)	0.004 (0.045)	0.056 (0.026)	0.323 (0.161)	-0.216 (0.108)

Regressions use the same controls as panel B of Tables 2 and 3. Standard errors are below estimated coefficients in parentheses. The sample is confined to (N=24) tracts along the Cambridge border.

Table 5: Effect of changes in rent control on residential segregation

Index	Isolation index					Dissimilarity Index				
	Black (only)	Black (any)	Asian (only)	Asian (any)	Hispanic	Black (only)	Black (any)	Asian (only)	Asian (any)	Hispanic
Minority Measure	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Change in fraction rent control	0.116 (0.041)	0.104 (0.041)	-0.006 (0.017)	-0.021 (0.017)	0.038 (0.021)	0.150 (0.102)	0.146 (0.092)	-0.008 (0.067)	-0.025 (0.064)	-0.006 (0.062)

The dependent variable is the change in a tract's residential segregation index . All indices are calculated in relation to white residents and use block groups as the relevant sub-tract area. n=82

