The Misallocation of Housing Under Rent Control

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The standard analysis of price controls assumes that goods are efficiently allocated, even when there are shortages. But if shortages mean that goods are randomly allocated across the consumers that want them, the welfare costs from misallocation may be greater than the undersupply costs. We develop a framework to empirically test for misallocation. The methodology compares consumption patterns for demographic subgroups in rent-controlled and free-market places. We find that in New York City, which is rent-controlled, an economically and statistically significant fraction of apartments appears to be misallocated across demographic subgroups. (*JEL* C25, D12, D61, R20)

The basic welfare analysis of price controls, shown in Figure 1, is a fundamental part of introductory economics. First-year students are routinely taught that the primary cost of price controls is the undersupply shown in the figure.[1] Most research on the social costs of rent control focuses on reduced supply (for example, Edgar O. Olsen, 1972; Fraser Institute, 1975; Anthony Downs, 1988; Joseph Gyourko and Peter Linneman, 1989).[2] Scholarly work on other price controls, such as the minimum wage, also mostly restricts itself to asking whether controls reduce the number of jobs or goods (e.g., David E. Card and Alan B. Krueger, 1995; Donald Deere et al., 1995).[3]

This common analysis assumes that in the presence of shortages, goods will be allocated efficiently. Economic theory tells us that the market mechanism allocates goods to the consumers who are willing to pay most for them, and that this allocation is Pareto efficient. However, the efficient allocation of goods to consumers is not automatic when demand exceeds supply. When there are shortages, some mechanism for rationing goods, such as queues or lotteries, substitutes for the price system.

These alternative allocation mechanisms seem unlikely to reproduce the efficiency of the market. If the allocation mechanisms are not perfectly efficient, then the analysis illustrated by Figure 1, which implicitly assumes that the rationing under rent control ensures that apartments go to the consumers who value them most, is wrong.[4] Figure 2 shows an alternative analysis of welfare losses when demand

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[1] For example, a leading urban economics textbook writes that "the cost of rent control is the adverse effect on the supply of rental housing" (Edwin S. Mills and Bruce W. Hamilton, 1994, p. 269).

[2] Of course, economists have long known that the social costs of rent controls may extend beyond the undersupply of rental units (Friedrich A. von Hayek, 1931; Milton Friedman and George Stigler, 1946). Yoram Barzel (1974) and Steven N. S. Cheung (1974) examine the social costs of queues or rent-seeking behavior. Mark Frankena (1975) discusses the distortions to housing quality. Olsen (1988) and Robert T. Deacon and John Sonstelie (1989) empirically address these costs.

[3] Luttmer (1998) provides an empirical analysis of the efficiency of the job rationing induced by the minimum wage. Such rationing causes a deadweight loss if workers with higher reservation wages displace equally skilled workers with lower reservation wages. The empirical evidence indicates the minimum wage does not appear to lead to inefficient rationing in the U.S. labor market.

[4] We assume that the good in question is a discrete unit and demand curves slope downward because of heterogeneity among consumers. This assumption is reasonably appropriate for the rental market. When consumers are homogeneous, and demand curves slope downward because of diminishing marginal utility at the individual level, then Figure 1 is appropriate and there is no role for misallocation losses.

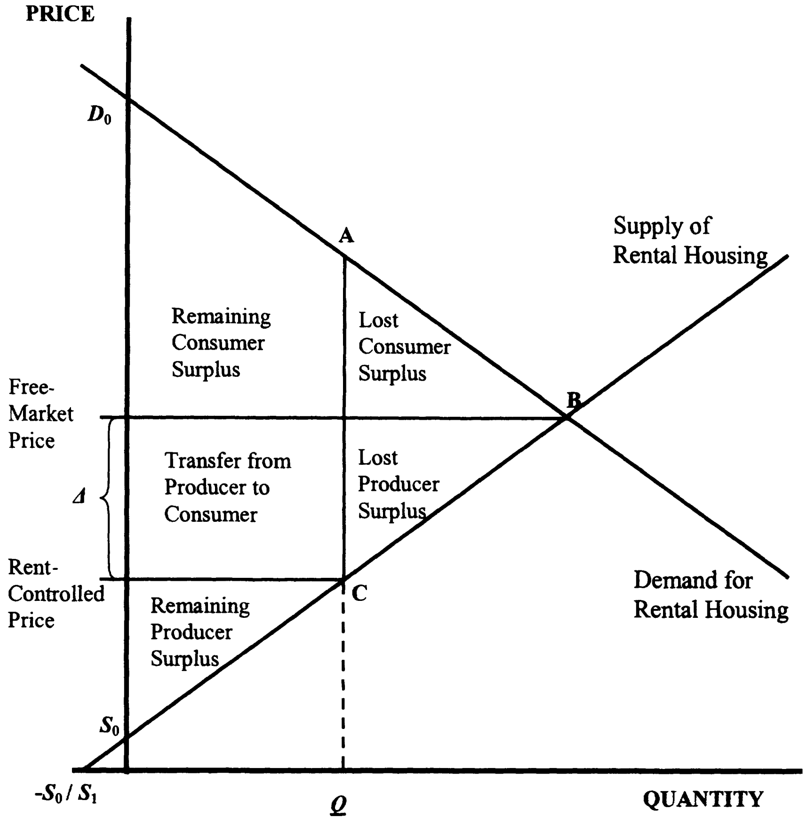

FIGURE 1. CLASSICAL ANALYSIS OF WELFARE LOSSES FROM RENT CONTROL

exceeds supply and when goods are allocated randomly across the consumers who want them at the going price. This figure shows a welfare loss that combines the classic undersupply triangle plus an area that represents misallocation.

In Section I of this paper, we present a simple model of misallocation losses that follows Deacon and Sonstelie (1989), Franz Hubert (1991), and especially Wing Suen (1989).[5] This model shows that for small impositions of rent control, the welfare losses due to misallocation will exceed the welfare losses due to undersupply. Thus, at least in theory, ignoring the misallocation costs of price control may result in a far too positive view of these regulations.

Anecdotes support the view that there may be misallocation of apartments under rent control. For example, Ken Auletta (1979, p. 43) describes the "Tobacconist to the World," Nat Sherman, who rented a six-room Central Park West apartment for $355 per month. Sherman says that his apartment "happens to be used so little that I think [the rent is] fair." A natural interpretation of this statement is that this large apartment was allocated to someone who used it so little that the rent for the apartment was close to the renter's marginal value of the apartment. Since this rent was far below market rates, and many others would have derived substantial surplus from this apartment at this rent, this represents an inefficient allocation.

Of course, simple models and anecdotes cannot really help us understand the prevalence of misallocation in controlled markets. In this paper, we present a methodology for measuring the degree of misallocation. Our approach compares rent-controlled areas with similar free-market areas. The crux of our identifying

[5] Olsen (1972) also features a nice discussion of misallocation.

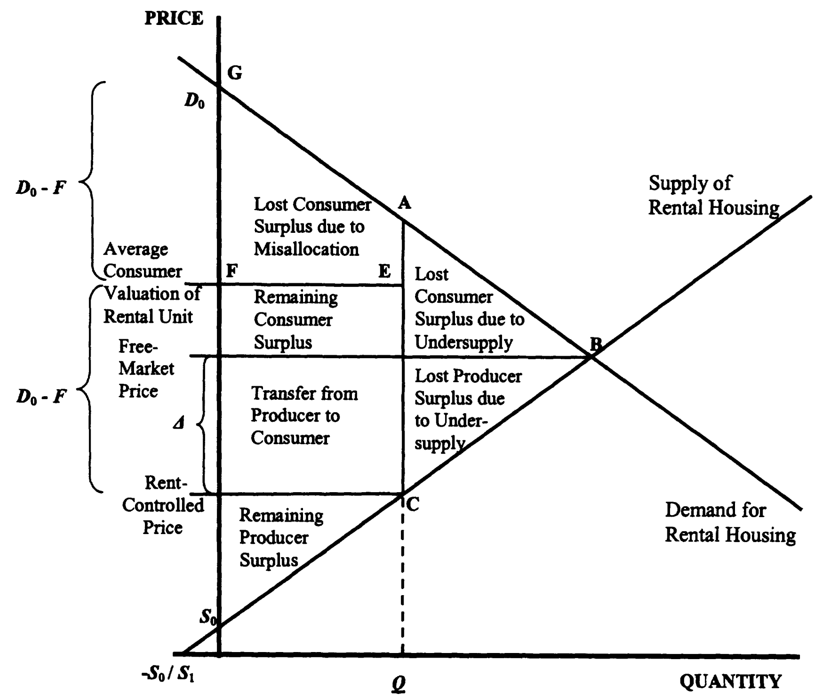

FIGURE 2. THE WELFARE LOSSES FROM RENT CONTROL WHEN APARTMENTS ARE RANDOMLY ALLOCATED ACROSS CONSUMERS

assumptions is that the overlap in the distribution of housing demand across demographic subgroups is constant over space. For example, if 80 percent of college graduates live in bigger apartments than the median high school dropout in the free-market city (i.e., the 50th percentile high school dropout and the 20th percentile college graduate have the same index of housing demand), then we assume that 80 percent of college graduates prefer to live in bigger apartments than the median high school dropout in the rent-controlled city as well. However, if only 60 percent of college graduates live in larger apartments than the median high school dropout in the rent-controlled city, then we interpret this as evidence of misallocation. We discuss the possible problems with this identifying assumption at length in the paper.

We then use this assumption to test whether there is a statistically significant misallocation of apartments in New York City relative to comparable free-market areas. We focus, primarily, on one characteristic of apartments—the number of rooms. We estimate the fraction of New York renters who are living in apartments with *m* rooms and who—in a free-market city with the same housing supply and demographic composition—would be living in apartments with *n* rooms. The total sum of such misallocations is a test statistic for the presence of misallocation.[6] Our methodology suggests that 21 percent of New York apartment renters live in apartments with more or fewer rooms than they would if they were living in a free-market city. The misallocation is most severe in Manhattan and the misallocation is greater for renters who have lived in their apartments for more than five years.

To test our methodology and identifying assumption, we check whether our approach finds misallocation in comparable cities without rent control. We examine Hartford and Chicago as "placebo" cities to see if there is evidence of misallocation in those free-market places. Our approach finds some misallocation in these

[6] Sampling error is responsible for some of the differences in housing allocations across cities. We use a bootstrapping procedure to estimate and correct for the differences that can be attributed to sampling error.

cities, but substantially less than in New York. For example, misallocation estimates in Chicago or Hartford are about a quarter of those in New York City. As such, we think that our identifying assumption is strictly false, but still the bulk of the misallocation that our procedure finds is real and not the result of different preference orderings in New York.

Our work is best seen as evidence on the nature of the market. Price controls, quotas, and restrictions that limit the tendency of goods to go to the highest bidder can reduce welfare by leading to a misallocation of goods across consumers. Prices do not just provide information for producers about how many goods to produce; they also ensure that heterogeneous goods go to the consumers that value them most. Of course, if (as Richard Arnott, 1995, suggests) there are major benefits from rent control, these benefits must be weighed against the costs.

I. Misallocation Under Price Controls

Glaeser (1996) and Glaeser and Luttmer (1997) extend the work of Deacon and Sonstelie (1989), Suen (1989), and Hubert (1991) and present a formal discussion of the costs of rent control. Here we present a brief discussion that sets the stage for the empirical work. As shown in Figure 1, standard microeconomics tells us that when there are price or rent controls, there is undersupply. In the housing context, this may mean that some individuals own, rather than rent, or if there is an uncontrolled sector, these individuals rent in that sector. Some individuals may leave the area altogether.

Downward-sloping demand curves occur because of diminishing marginal returns within individuals and because of diminishing marginal valuations across individuals. In the case of a discrete commodity, where consumers generally consume only one unit of the good, the demand curve is formed by ranking the willingness to pay of consumers ranging from those who demand the good most to those who demand the good least.

In the standard graphical analysis of rent control, shown in Figure 1, the welfare losses are limited to the triangle ABC if and only if the individuals who value the apartments most get the rent-controlled apartments. This assumption is reasonable if informal means of allocating the apartments (e.g., queues, bribing doormen) create a pseudo-price system, or if there exists a resale market for the commodity. Of course, these pseudo-prices may entail their own substantial deadweight losses (e.g., search costs).

However, in many cases products under price controls will be allocated somewhat (or completely) randomly to everyone who wants them. Furthermore, binding price controls attract new renters who would not be interested in renting at market prices. As such, rent control means that some renters, who would greatly value an apartment, are shut out while others, who never would have rented an apartment under free-market rates, obtain rental apartments. To show the welfare losses from misallocation graphically, we assume that apartments are randomly allocated across individuals who want one at the rent-controlled price.

In Figure 2, the polygon AEFG represents welfare losses coming from the fact that the average person who gets a rent-controlled apartment does not value that apartment as much as the people who value the apartment the most. In the linear case, the average renter's utility from the apartment equals the average of the rent-controlled price and the utility from the apartment of the first renter (D_0). The polygon AEFG thus represents the gap between the average willingness to pay under rent control and the willingness to pay of the renters who value the apartment most. This gap is the loss from misallocation.

The algebra of the graphs is straightforward. In Figures 1 and 2, demand is linear and can be written as $(D_0 - P)/D_1$ while supply can be written $(P - S_0)/S_1$. Standard analysis tells us that a price control that reduces prices by an amount Δ relative to the free-market price will lead to a reduction in quantity of Δ/S_1 relative to the free-market quantity. The social welfare costs of the price control equals $(\Delta/S_1)(\Delta + \Delta D_1/S_1)/2$ or $\Delta^2(D_1 + S_1)/2S_1^2$. This represents the classic ABC welfare loss triangle, which equals the change in quantity Δ/S_1 times the gap between average marginal benefit and marginal cost for the units that are no longer being produced, which equals $(\Delta + \Delta D_1/S_1)/2$.

In the free market, marginal benefit and marginal cost are equal. The imposition of a price control, with efficient rationing, creates a total gap of $\Delta + \Delta D_1/S_1$ between benefit and cost for the marginal producer and consumer. Linearity then tells us that on average each lost unit of production produces a welfare loss of $(\Delta + \Delta D_1/S_1)/2$: the average gap between marginal benefit and marginal cost for the units not produced due to rent control.

In the case of random allocation, the social welfare cost of rent control will instead be $\Delta^2(D_1 + S_1)/2S_1^2 + \underline{Q}\Delta(D_1 + S_1)/2S_1$ where $\underline{Q}$ represents the supply of housing after rent control.[7] The first term, $\Delta^2(D_1 + S_1)/2S_1^2$, represents the familiar loss from undersupply (the triangle ABC). The second term, $\underline{Q}\Delta(D_1 + S_1)/2S_1$, represents the loss from misallocation (the polygon AEFG in Figure 2). The term $\Delta(D_1 + S_1)/2S_1$ is the difference between the average consumer valuation after rent control with efficient allocation and the average consumer valuation after rent control with random allocation.

Efficient allocation means that rent control involves a move up the demand curve so that the marginal renter's benefit from an apartment will rise by $\Delta D_1/S_1$. This change occurs because the identity of the marginal renter is changing, not because any individual's demand for an apartment is changing. Linearity of demand then implies that with efficient allocation, rent control causes an increase in consumers' average valuation of an apartment by one-half of that amount.

Random allocation implies that rent control involves a slide down the demand, i.e., there is an increase in the number of people who want the apartment at the lower, rent-controlled price. Thus rent control causes the benefit for the marginal consumer to fall by Δ—the reduction in price increases the number of people who will be interested in renting. The reduction in consumers' average valuation will be one-half of that amount. Adding the increase in valuation from efficient allocation to the decrease in valuation from random valuation implies that the difference in the average valuation of apartments among consumers who get apartments under random allocation and efficient allocation is $\Delta(D_1 + S_1)/2S_1$.

Comparing $\Delta^2(D_1 + S_1)/2S_1^2$ and $\underline{Q}\Delta(D_1 + S_1)/2S_1$ tells us that as long as $\underline{Q}$ is greater than Δ/S_1, or the price control reduces quantity supplied by less than 50 percent, then the welfare loss from misallocation is bigger than the welfare loss from undersupply. Glaeser and Luttmer (1997) prove this more generally: moderate price controls will create greater social losses from misallocation than from undersupply. This provides a theoretical basis for the view that misallocation costs may be quantitatively important.

Misallocation will be important when rental units are homogeneous, because the wrong people may get apartments, but misallocation can become even more important when housing is heterogeneous. When there are many types of individuals and many types of housing, rent control may distort the relative prices of different types of housing. For example, rent control may reduce the prices of big apartments more than the prices of small apartments. In that case, too many people will want big apartments and some of these big apartments will be allocated inefficiently (unless there is a pseudo-price mechanism that allocates the apartments efficiently).[8]

Newer generation rent controls that allow landlords to charge prices that clear the market when they initially rent out apartments may reduce these welfare losses. However, all forms of rent control limit landlords' abilities to raise rents on long-term tenants. This creates an incentive to stay in the same apartment, which leads people to remain in the same apartment even if their tastes and conditions change. As the taste and needs of individuals change over time, there will be a misallocation of houses across people, even if goods are allocated efficiently initially.[9]

II. A Methodology for Testing for the Presence of Misallocation

We now turn to our test for the presence of misallocation. The efficacy of this test depends on two key assumptions. The first assumption is that the ranking of two households' levels of

[7] This expression reduces to $(\Delta/S_1)(D_0 - S_0)/2$ when one substitutes $\underline{Q} = (D_0 - S_0)/(D_1 + S_1) - \Delta/S_1$.

[8] This type of misallocation will occur even if the right number of apartments of each type exists in equilibrium.

[9] This effect will lead to a greater degree of mismatch initially as well, if individuals try to anticipate their future needs. This type of mismatch will, of course, occur even in free markets when there are large moving costs (as in Todd M. Sinai, 1997).

housing demand is constant across localities. The second assumption rules out differential selection across space based on unobserved household characteristics. We start with our assumption about the demand for a particular housing attribute:

ASSUMPTION 1 (No Reversals): *If household A consumes more of a housing attribute than household B in city c then household A will consume weakly more of that attribute than household B in any other city.*

This assumption implies that we can think of households as being ranked by their demand for a given housing attribute. Thus, if households are characterized by income, denoted Y, and a vector of observable and unobservable characteristics, denoted $\tilde{\mathbf{X}}$, then the No Reversals assumption implies that we can construct a latent demand index such that in any city consumption of this housing attribute always rises monotonically with $\theta(Y, \tilde{\mathbf{X}})$.[10]

There are several ways in which this assumption might fail. First, particular places might have amenities that are substitutes for housing consumption for some people and not for others. City parks, for example, might reduce the need for large homes for the less advantaged more than for the more advantaged. We will have to rule out this possibility. Second, changes in the price of this attribute (or other attributes) might have income effects that affect the demand of some households more than that of others.

Of course, there are some important cases where the No Reversals assumption does hold. To understand just how restrictive this assumption is, we consider two examples where the assumption holds. The most obvious example where the assumption holds is quasi-linearity. In this case, the utility function takes the following form:

$$(1) \qquad Utility = Y - P^c(H) + V(H, \theta)$$

where Y represents income, H is consumption of a housing attribute, θ reflects the household's latent demand index for the housing attribute, and $P^c(\cdot)$ is the hedonic price schedule of this attribute in city c. The function $V(H, \theta)$ has a positive cross derivative so that the marginal utility of H rises with increases in the demand index θ.

If the housing attribute is consumed in discrete units, as it will be in our empirical work, than if one household, with demand index θ, consumes k units of the housing attribute in city c, and a second household, with index θ', consumes $k - 1$ units in the same city, then it must be true that θ is greater than θ' which implies that the household with latent demand θ consumes weakly more than the other household in every city.

Obviously, quasi-linearity is quite a restrictive assumption, especially since we know that income is linked to housing consumption. However, the No Reversals assumption does not rule out income effects. Instead, it requires that the income effects caused by changes in the price schedule over space have the same effect on any two households with the same initial housing consumption. For example, this requirement is fulfilled if utility is separable and utility from the composite commodity takes the exponential form, i.e.,:

$$(2) \qquad Utility = U(H \mid Y, \tilde{\mathbf{X}}) = -e^{-\alpha(Y - P^c(H))} + \gamma(\tilde{\mathbf{X}})V(H)$$

where $V(\cdot)$ is an increasing function and $\gamma(\tilde{\mathbf{X}})$ is a positive parameter of the utility function that depends on household characteristics $\tilde{\mathbf{X}}$. In this case, the latent demand index is given by $\theta(Y, \tilde{\mathbf{X}}) = \gamma(\tilde{\mathbf{X}})e^{\alpha Y}$; households with a higher value of this index will always buy weakly more housing than households with a lower value of this index. To see this, suppose we found two households with characteristics $(Y, \tilde{\mathbf{X}})$ and $(Y', \tilde{\mathbf{X}}')$ such that $\theta(Y, \tilde{\mathbf{X}}) > \theta(Y', \tilde{\mathbf{X}}')$ but with the first household consuming H units of housing

[10] The proof that the No Reversals assumption allows us to construct such a demand index is straightforward. Choose any arbitrary price vector and first rank all households by their consumption at that price vector. Then for all households who consume the same amount (because of the discrete nature of housing attributes), consider a second price vector and rank households who consumed the same amount at the first price vector by their consumption at the second price vector. Continue this process indefinitely for all possible price vectors and there will be a ranking of people. Let $\theta(Y, \tilde{\mathbf{X}})$ equal the household's place in the ranking.

and the second $H + 1$. By revealed preference, $U(H|Y, \tilde{\mathbf{X}}) \geq U(H + 1|Y, \tilde{\mathbf{X}})$ while $U(H|Y', \tilde{\mathbf{X}}') \leq U(H + 1|Y', \tilde{\mathbf{X}}')$. However, rearranging and combining these two inequalities yields:

$$\gamma(\tilde{\mathbf{X}})e^{\alpha Y} \leq \frac{e^{\alpha P^c(H+1)} - e^{\alpha P^c(H)}}{V(H+1) - V(H)} \leq \gamma(\tilde{\mathbf{X}}')e^{\alpha Y'}$$

which contradicts $\theta(Y, \tilde{\mathbf{X}}) > \theta(Y', \tilde{\mathbf{X}}')$. Hence, for this utility function, housing consumption must be weakly increasing in the demand index $\theta(Y, \tilde{\mathbf{X}}) = \gamma(\tilde{\mathbf{X}})e^{\alpha Y}$.

While the No Reversals assumption is restrictive, we still think it is reasonable. The assumption generally fails when price changes over space affect the marginal utility of income for one type of household much more than for another type of household with the same initial housing consumption. We will be able to perform tests to see whether this assumption fails badly in a way that compromises our misallocation tests.

So far, we have characterized households by their income (Y) and a vector of observable and unobservable characteristics ($\tilde{\mathbf{X}}$). The No Reversals assumption ensured that we could map households' characteristics $(Y, \tilde{\mathbf{X}})$ to a single value of a demand index $\theta(Y, \tilde{\mathbf{X}})$. However, when not all determinants of housing demand are observable, there will be a distribution of the latent demand index for households with identical incomes and other observable characteristics because these households differ in their unobservable characteristics. We will refer to subgroups of households with identical observable characteristics (including income Y_i) as subgroup i. Our second identifying assumption requires that the effect of the unobservable characteristics on the distribution of the demand index is constant across space for each subgroup, or that there cannot be differential selection on unobservables. Formally:

ASSUMPTION 2 (No Differential Selection on Unobservables): *For any city c and subgroup i, the distribution of the demand index* θ, $\Psi_c(\theta|\mathbf{X}_i)$, *equals* $\Psi(\theta + \lambda_c|\mathbf{X}_i)$ *for some* λ_c.

This assumption means that the distribution of latent demand (within demographic subgroups) is constant over space except for a city-specific shift parameter, λ_c, which is constant across demographic subgroups within a city. Thus, the distribution of the demand index for a particular subgroup may differ across cities, and New York may attract people who have a general unobserved taste for small living quarters. But the No Differential Selection assumption means that we are ruling out cases where New York attracts college graduates with an unobserved taste for spacious apartments and high school dropouts with an unobserved taste for small apartments. Together, the No Reversals and No Differential Selection assumptions imply:

The Constant Overlap Implication.—If the share of subgroup i in free-market city A that rents apartments with k or fewer rooms is equal to the share of subgroup i in free-market city B that rents apartments with n or fewer rooms, then, for any other subgroup j, the share renting apartments with k or fewer rooms in city A must equal the share renting apartments with n or fewer rooms in city B.[11]

In other words, our key assumptions imply that the overlap in housing consumption between different demographic subgroups is constant across space. For example, suppose household size is the only observable characteristic and that in Atlanta 40 percent of one-person households and 20 percent of three-person households live in apartments of three rooms or less. Our two assumptions imply that in other cities, the demand for housing of the 40th percentile of one-person households should be the same as the demand of the 20th percentile three-person households. The assumption therefore implies that if New York were also a free-market city and if 40 percent of one-person households live in studios in New York, then 20 percent of three-person households must also live in studios in New York.

This implication of both the No Reversals and No Differential Selection assumptions is the key to our identification. Essentially, we test

[11] This implication follows from our assumptions because if the share of subgroup i in city A in apartments with k or fewer rooms is equal to the share of subgroup i in city B in apartments with n or fewer rooms then $\Psi(\mu_k^A + \lambda_A|\mathbf{X}_i) = \Psi(\mu_n^B + \lambda_B|\mathbf{X}_i)$, where μ_k^A denotes the highest value of demand index of households in city A renting apartments with k or fewer rooms. This implies that $\mu_k^A + \lambda_A = \mu_n^B + \lambda_B$, which in turn implies $\Psi(\mu_k^A + \lambda_A|\mathbf{X}_j) = \Psi(\mu_n^B + \lambda_B|\mathbf{X}_j)$.

for misallocation by looking at whether the consumption of the 40th percentile of one-person households and the 20th percentile of three-person households are the same in New York if they are the same in Atlanta. Thus our test will find misallocation if housing consumption in New York varies with demographic characteristics in a way that is unlike cities in the rest of the country. To see whether this test makes sense, and to test our identifying assumptions, we will compare the consumption pattern differences between New York and elsewhere, with consumption pattern differences among other U.S. cities. If our test found misallocation in many free-market cities, we would lose our trust in our identifying assumptions.

In principle, we could now proceed nonparametrically and treat each demographic subgroup as a separate cell. Then we could compare consumption patterns across all cells across cities. This would end up being computationally tiresome and extremely noisy. Instead, we assume that the distribution of the latent demand index θ, for a given demographic subgroup with income and other observable demographic characteristics $\mathbf{X}_i$, is characterized by a normal distribution with mean $\mathbf{X}_i'\boldsymbol{\beta}$ and variance $\exp(\mathbf{X}_i'\boldsymbol{\delta})$:

$$\theta|\mathbf{X}_i \sim N(\mathbf{X}_i'\boldsymbol{\beta} - \lambda_c, e^{\mathbf{X}_i'\boldsymbol{\delta}}) \tag{3}$$

where the parameter λ_c allows for city-specific shifts in the demand index. The normality assumption means that the distribution of the demand index can only differ in two ways (mean and variance) across demographic groups. This assumption is convenient, but it does not drive our results. Furthermore, we have only assumed normality of the *latent* demand index; we have assumed nothing about the distribution of observed housing consumption.

To implement the basic misallocation test, we begin with a benchmark of a free-market city and consumption of one given housing attribute ("apartment size") that is consumed in discrete units. Given the distribution of θ in that city, and given standard assumptions about a market equilibrium, the free market will allocate housing attributes so that:[12]

$$H_c^*(\theta) = \begin{Bmatrix} 1 & \text{for} & \theta \leq \mu_1^c \\ 2 & \text{for } \mu_1^c < & \theta \leq \mu_2^c \\ \vdots & & \\ k & \text{for } \mu_{k-1}^c < \theta & \end{Bmatrix} \tag{4}$$

where $H_c^*(\theta)$ denotes the housing consumption of someone with demand index θ, and μ_k^c denotes the upper bound of demand index θ of households living in apartments of size k in city c. In equation (4) the allocation of housing in free-market cities is efficient—every resident of an apartment with more than k rooms has a value of θ that is higher than everyone in apartments with k or fewer rooms. By the usual arguments, an inefficient allocation will not be a market equilibrium.

We use maximum likelihood to jointly estimate (3) and (4). This estimation procedure is a generalization of ordered probit because it allows the variance of latent variable θ to depend on demographic characteristics and the cutoff levels for the latent variable, μ_k^c, to differ across cities. Applied to a sample of free-market cities, the estimation procedure yields estimates $\hat{\boldsymbol{\beta}}$, $\hat{\boldsymbol{\delta}}$, and $\hat{\mu}_k^c$. The values of $\hat{\boldsymbol{\beta}}$ and $\hat{\boldsymbol{\delta}}$ describe how the distribution of the latent demand index θ shifts with demographic characteristics and these estimates will be used later to predict the efficient housing allocation in New York City. Differences across cities in housing supply and demographic composition are fully absorbed by the city-specific cutoff levels $\hat{\mu}_k^c$ and thus do not drive the estimates $\hat{\boldsymbol{\beta}}$ and $\hat{\boldsymbol{\delta}}$. For example, a city with a supply of many large apartments that is inhabited with households with a latent demand for small apartments will have low values of $\hat{\mu}_k^c$. In this city, latent demand does not need to be very large to obtain a large apartment.[13]

The identifying assumptions imply that values of $\hat{\boldsymbol{\beta}}$ and $\hat{\boldsymbol{\delta}}$ estimated outside New York will also describe the demand index of renters in New York City, at least up to a constant. Our basic test statistic will be the share of the renters in New York that we predict are misallocated across apartments. This will be estimated by

[12] For example, we assume that the supply of housing is sufficiently varied so that there are no bundling problems (as in James Heckman and Jose A. Scheinkman, 1987).

[13] Because the cutoff levels, μ_k^c, can shift freely across cities, we cannot separately identify city-specific shifts in the distribution of θ (the λ_c terms) and a constant shift to the μ_k^c terms. However, this does not matter since we only need the estimates $\hat{\boldsymbol{\beta}}$ and $\hat{\boldsymbol{\delta}}$ (but not $\hat{\mu}_k^c$ or $\hat{\lambda}_c$) to estimate misallocation.

comparing a predicted allocation (based on $\hat{\boldsymbol{\beta}}$ and $\hat{\boldsymbol{\delta}}$) with the actual allocation. To form the predicted allocation, we use the values of $\hat{\boldsymbol{\beta}}$ and $\hat{\boldsymbol{\delta}}$ to generate a total cumulative distribution function (CDF) of θ in New York of $\Sigma_{i \in NYC} N_{NYC}(\mathbf{X}_i)\Phi(\theta|\mathbf{X}_i'\hat{\boldsymbol{\beta}}, e^{\mathbf{X}_i'\hat{\boldsymbol{\delta}}})$, where $N_{NYC}(\mathbf{X}_i)$ is the proportion of renters with characteristics $\mathbf{X}_i$ in NYC.[14] The expression $\Phi(\theta|\mathbf{X}_i'\hat{\boldsymbol{\beta}}, e^{\mathbf{X}_i'\hat{\boldsymbol{\delta}}})$ reflects the cumulative distribution of the demand index θ for renters in the subgroup with characteristics $\mathbf{X}_i$ and $\Phi(\cdot|a, b)$ denotes the CDF of a normal random variable with mean a and variance b.

Given this city-level distribution of the demand index and given a stock of apartments, we can estimate the value of $\hat{\mu}_k^{NYC}$—the highest value of θ that should be allocated an apartment of size k in the efficient allocation: This value must solve:

$$\sum_{i \in NYC} N_{NYC}(\mathbf{X}_i)\Phi(\hat{\mu}_k^{NYC}|\mathbf{X}_i'\hat{\boldsymbol{\beta}}, e^{\mathbf{X}_i'\hat{\boldsymbol{\delta}}}) = S_{NYC}(k), \tag{5}$$

where $S_{NYC}(k)$ denotes the proportion of apartments with k or fewer rooms in New York City. Thus, if one-half of the apartments in New York are big and one-half are small, then there would only be one estimated value of $\hat{\mu}_k^{NYC}$ and this would equal the median value of the predicted New York distribution of θ. More generally, the values $\hat{\mu}_{k-1}^{NYC}$ and $\hat{\mu}_k^{NYC}$ define the lower and upper bound of θ for renters who would be living in apartments of size k in the efficient allocation.

To estimate the degree of misallocation, we compare this predicted allocation—which should be the "efficient" allocation—to the actual allocation in New York. We estimate the actual housing allocation by estimating the generalized ordered probit model [defined by equations (3) and (4)] on a sample of New York City renters, which yields the parameter estimates $\tilde{\mu}_k$, $\tilde{\boldsymbol{\beta}}$, and $\tilde{\boldsymbol{\delta}}$. While we interpreted our earlier parameter estimates $\hat{\mu}_k^c$, $\hat{\boldsymbol{\beta}}$, and $\hat{\boldsymbol{\delta}}$ (estimated on free-market cities) as capturing the true relationship between demographic characteristics and latent demand for housing, the parameters $\tilde{\mu}_k$, $\tilde{\boldsymbol{\beta}}$, and $\tilde{\boldsymbol{\delta}}$ estimated for New York merely describe the housing allocation in a way that facilitates comparison between the actual housing allocation and the predicted housing allocation.

Using the estimates $\tilde{\mu}_k$, $\tilde{\boldsymbol{\beta}}$, and $\tilde{\boldsymbol{\delta}}$, we proxy for the actual fraction of renters from subgroup $\mathbf{X}_i$ living in an apartment of size k or less by:

$$S_{NYC}(k|\mathbf{X}_i) = \Phi(\tilde{\mu}_k|\mathbf{X}_i'\tilde{\boldsymbol{\beta}}, e^{\mathbf{X}_i'\tilde{\boldsymbol{\delta}}}). \tag{6}$$

We use this smoothed proxy for the housing allocation within subgroup $\mathbf{X}_i$ instead of the actual housing allocation for two reasons. First, our proxy limits the housing allocation to the set of allocations that are compatible with normally distributed latent demands. This eliminates the possibility that we find misallocation in New York City because we imposed normality outside NYC but not in NYC. Second, in our sample, most demographic subgroups are very small (just a couple of observations). Using subgroup-specific housing shares from our sample would yield degenerate distributions that most likely do not represent the housing allocation of that subgroup as a whole, thus yielding spurious misallocation estimates.

In principle, rent control might create misallocation within demographic subgroups as well as across them. However, since our goal is to test for the existence of misallocation, we assume that sorting on the basis of unobservable characteristics is efficient. Efficient sorting within subgroup $\mathbf{X}_i$ implies that every member of this subgroup living in an apartment with k or fewer rooms must have a value of θ that is lower than any member of this subgroup in apartments with more than k rooms. Hence, the value of $\mu_k^{NYC}(\mathbf{X}_i)$—the highest value of θ that a member from subgroup $\mathbf{X}_i$ is assumed to have when living in an apartment of size k—solves:

$$\Phi(\mu_k^{NYC}(\mathbf{X}_i)|\mathbf{X}_i'\hat{\boldsymbol{\beta}}, e^{\mathbf{X}_i'\hat{\boldsymbol{\delta}}}) = S_{NYC}(k|\mathbf{X}_i), \tag{7}$$

where $S_{NYC}(k|\mathbf{X}_i)$ denotes our estimate of the proportion of members from this subgroup living in apartments with k or fewer rooms.

Thus, when sorting on unobservables is efficient, the values $\mu_{k-1}^{NYC}(\mathbf{X}_i)$ and $\mu_k^{NYC}(\mathbf{X}_i)$ define the lower and upper bound of θ for renters from

[14] Without loss of generality, the city-specific taste shifter λ_{NYC} can be set to zero because changes in this parameter do not affect the optimal allocation of housing within New York City.

this subgroup who live in apartments of size k in the actual allocation. In the efficient allocation, $\mu_k^{NYC}(\mathbf{X}_i)$ would equal $\hat{\mu}_k^{NYC}$. Our misallocation measure is based entirely on the differences between $\mu_k^{NYC}(\mathbf{X}_i)$ and $\hat{\mu}_k^{NYC}$. Inserting $\mu_k^{NYC}(\mathbf{X}_i)$ into equation (4) for each subgroup yields $H_{NYC}(\theta, \mathbf{X}_i)$, the actual housing allocation in NYC for someone from subgroup $\mathbf{X}_i$ with latent demand θ. Similarly, we construct $H^*_{NYC}(\theta)$, the efficient allocation for someone with latent demand θ by inserting $\hat{\mu}_k^{NYC}$ into equation (4). Now by comparing the actual to the efficient allocation, we can find the share of all NYC renters that are living in apartments with k rooms but would be living in apartments with n rooms if the city had a free market (and no change in housing supply):

$$(8)\quad M_n^k = \sum_{i \in NYC} N_{NYC}(\mathbf{X}_i) \times \int_{-\infty}^{\infty} \mathbf{1}(H(\theta, \mathbf{X}_i) = k \text{ and } H^*(\theta) = n) \times d\Phi(\theta | \mathbf{X}_i'\hat{\boldsymbol{\beta}}, e^{\mathbf{X}_i'\hat{\boldsymbol{\delta}}})$$

where $\mathbf{1}(\cdot)$ is the index function, which takes on a value of 1 if the expression between parentheses is true and a value of 0 otherwise. Misallocation occurs whenever $k \neq n$, so the total share of households who are misallocated in one way or another is given by $M^{Gross} = \sum_n \sum_{k \neq n} M_n^k$. We refer to this measure as the *gross* fraction misallocated because even if the housing allocation in NYC were efficient, this number would be greater than zero due to sampling error. We adjust for this by estimating M^{Error}, the value of M that sampling error would generate if the housing allocation in NYC were efficient. The *net* estimate of the fraction misallocation in NYC is found by $M^{Net} = M^{Gross} - M^{Error}$, and the expected value of this number is zero if the housing allocation is efficient.

We use a bootstrap procedure both to estimate M^{Error} and to generate standard errors for our misallocation estimates. The idea behind bootstrapping is that by replicating the estimation procedure on samples that are drawn randomly and with replacement from the original sample, one can replicate the sampling error that is present in the original sample (Bradley Efron, 1979). Because of sampling error, each replication will yield a slightly different estimate of misallocation. The standard deviation of the estimates from the replications is the standard error of the original estimate. Moreover, in each replication, we simulate a sample in which New York City residents have the same underlying distribution of θ as elsewhere and allocate the existing housing stock efficiently. However, due to sampling error, the estimated distribution of θ will differ somewhat between the simulated New York City sample and the sample of free-market cities, and our procedure will find a small amount of misallocation that can be solely attributed to sampling error. The average of the amounts of misallocation that we find across the replications is an estimate of M^{Error}. The Appendix describes the bootstrapping routine in detail.

III. Data Description and Results

We now describe the data that we use to test for misallocation due to rent control in New York. Unless otherwise noted, the data in this paper come from the 1990 Census 5 percent Public Use Micro Sample (PUMS). We want to compare the apartment rental market in New York City to markets without rent control that are otherwise as similar as possible to the New York market subject to rent control. Since the rent-control regulations in New York City by and large exclude apartments in buildings with fewer than five apartments, we only use observations on households living in buildings containing at least five apartments ("5+ buildings"). To further enhance comparability between the New York City sample and the sample of free-market areas, we limit the samples to households living in metropolitan areas and residing in Public Use Microdata Areas (PUMAs) with at least 10 percent of residents living in 5+ buildings. This latter restriction only binds for four out of New York's 55 PUMAs with two of the PUMAs with less than 10 percent of residents in 5+ apartment buildings being in Staten Island and the other two being in Queens. Outside New York City, this restriction eliminates about 56 percent of PUMAs in metropolitan areas. The fraction of residents living in 5+ apartment buildings in each PUMA comes from the Census Summary

Tape Files (STF) because these are more accurate than the PUMS data.

The baseline sample for New York City consists of a random sample of 10,000 households from all renters in 5+ buildings in New York City. We limit the sample size to keep the estimation computationally manageable. We also estimate misallocation within each of the four larger boroughs. Those samples are constructed as above, and are of size 5,000.[15]

To construct the baseline control group, we only consider PUMAs with at least 10 percent of residents in 5+ buildings in Metropolitan Statistical Areas (MSAs) without rent control other than Hartford and Chicago. We exclude Hartford and Chicago so that they can later serve as tests of the identifying assumptions. This yields 103 MSAs with at least one valid PUMA. Next, we draw in each of these 103 MSAs a random sample of 200 households renting apartments in buildings with five or more units from the PUMA(s) in that MSA with the highest fraction of households living in 5+ buildings. This creates a control group of manageable size (20,600).

The baseline apartment characteristic used to estimate misallocation is the number of rooms in the apartment. This number is top-coded at five since less than 2 percent of the observations live in larger apartments, which would complicate the estimation of the cutoff valuations $\hat{\mu}_k^c$ for apartments with six or more rooms. As an alternative, we also consider the number of bedrooms in the apartment, which we top-code at four (this affects less than 1 percent of observations).

The baseline demographic characteristics ($\mathbf{X}_i$) consist of household composition (various measures of the number of adults and children of different ages), single parenthood, a three-segment spline in the mean age of the household head and his/her spouse, four dummy variables for the maximum educational attainment of the household head and his/her spouse, and a second-order polynomial in the log of per capita household income. These variables are detailed in Table 1.

[15] Staten Island is not considered separately because it has too few observations: 430.

A. *Basic Results—The Amount of Misallocation*

The intuition of our methodology is illustrated in Table 2. This table shows the average overlap in the consumption of rooms between households from various broadly defined demographic groups. As a measure of average overlap, we report the probability that a random household from demographic group *A* consumes strictly more rooms than a random member of group *B*. If the allocation of housing is efficient and if our identifying assumptions hold, we would expect these probabilities to be similar in New York and elsewhere. However, as Table 2 shows, the probabilities are often significantly different.

For example, the first row shows that when we compare the group of households headed by a high school dropout with the group of college graduates, we find there is a 32-percent probability in cities without rent control that a random member from the first group rents a strictly bigger apartment than a random member of the second group. In New York City, however, the comparable figure is 47 percent. Similarly, there are significant differences between New York City and free-market cities in the amount in overlap for demographic groups based on age, income, and the presence of children. Only the amount of overlap between single-person households and households with three or more members turns out to be the same in New York City and elsewhere. Given our identifying assumptions, Table 2 provides suggestive evidence of misallocation in New York City.[16]

[16] The evidence in this table is only suggestive because it has two major drawbacks that are addressed by our main methodology. First, the demographic groups in Table 2 are defined by only one characteristic, which leaves considerable scope for differential selection on all the other demographic characteristics. Our main methodology addresses this shortcoming by having very narrowly defined demographic subgroups (each subgroup has a unique set of values for all the demographic characteristics, including income, listed in Table 1). Second, Table 2 measures overlap in *actual* housing consumption, which can be affected by the composition of the housing stock. For example, two people with different underlying or latent housing demands are more likely to consume apartments of the same size in a city where most of the housing stock is of one size than in a city with a very varied housing stock. Our main methodology addresses this shortcoming by testing whether the observed allocation implies reversals of a *latent* index of housing

TABLE 1—SUMMARY STATISTICS

	Baseline treatment group: New York City		Baseline control group: 103 cities without rent control	
	Mean	(Standard deviation)	Mean	(Standard deviation)
Apartment characteristics				
Number of rooms[a]	3.26	(1.13)	3.30	(1.14)
Number of bedrooms[a]	2.42	(0.84)	2.40	(0.75)
Household characteristics				
Number of adults	1.778	(0.960)	1.506	(0.718)
max("Number of adults" − 2, 0)[c]	0.244	(0.668)	0.090	(0.408)
Single parent	0.090	(0.286)	0.086	(0.281)
Number of children age 0–5	0.219	(0.552)	0.188	(0.534)
max("Number of kids age 0–5" − 1, 0)[c]	0.055	(0.274)	0.053	(0.278)
Number of children age 6–11	0.196	(0.529)	0.123	(0.436)
max("Number of kids age 6–11" − 1, 0)[c]	0.050	(0.264)	0.032	(0.221)
Number of children age 12–17	0.196	(0.526)	0.092	(0.375)
max("Number of kids age 12–17" − 1, 0)[c]	0.049	(0.260)	0.023	(0.182)
Mean age of household head and spouse	48.9	(17.9)	42.65	(19.98)
max("Age" − 35, 0)[c]	15.6	(15.9)	11.8	(16.6)
max("Age" − 60, 0)[c]	3.6	(7.0)	3.3	(7.2)
High school dropout[b]	0.317	(0.465)	0.221	(0.415)
High school degree[b]	0.251	(0.434)	0.229	(0.420)
Some college[b]	0.187	(0.390)	0.302	(0.459)
[Omitted category: College or more[b]]	0.245	(0.430)	0.249	(0.432)
Ln(Income per capita)	9.141	(1.132)	9.105	(0.994)
Ln(Income per capita)^2	84.85	(20.46)	83.88	(17.71)
Number of households	10,000		20,600	

Notes: Data from the 1990 Census 5 percent Public Use Micro Sample. The treatment group consists of 10,000 households in New York City. The control group consists of 20,600 households in 103 MSAs without rent control (200 households per MSA). Households in the treatment and control group are limited to renters of apartments in buildings with five units or more living in PUMAs with at least 10 percent of the population living in buildings with five units or more.

[a] The number of rooms is top-coded at five (less than 2 percent of observation affected) and the number of bedrooms at four (less than 1 percent of observation affected).

[b] Maximum of educational attainment of household head and his/her spouse.

[c] We include variables of the form max($X - a$, 0) in the ordered probit regressions in order to allow for changes in the marginal effect of X on latent housing demand at the breakpoint a. For example, by including max("Number adults" − 2, 0), we allow the effect of an extra adult on latent housing demand to be different for households with two or fewer adults than for households with more than two adults.

In Table 2, we see that in New York (a high-price city) the connection between income and housing consumption is weaker than elsewhere. If we thought that higher prices in New York changed the relationship between income and housing consumption because higher prices differentially increased the marginal utility of income for the poor, then we would expect the income–housing relationship to be stronger in New York, not weaker. As such, the differential relationship between housing consumption and demographics in New York does not seem to occur because the marginal utility of income is rising particularly for the poor in a high-cost area.

In Table 3, we use our methodology to present a matrix showing the misallocation of households in New York City. The rows indicate the actual number of rooms consumed (k) and the columns show the number of rooms (n) that should be consumed in an efficient allocation. Each entry in the matrix shows the share of the population that falls within each of these groupings, M_n^k. For example, the second entry on the first row shows that 2.06 percent of all households should be living in two-room apart-

demand. In the main methodology, actual housing consumption in a city can be *any* monotonically increasing function of the latent demand index (because we include a full set of city-specific cutoff parameters μ_k^c).

TABLE 2—AVERAGE OVERLAP IN HOUSING CONSUMPTION BETWEEN POPULATION GROUPS

	Probability that rooms for household from group A > rooms for household from group B			
	New York City renters		U.S. free-market renters	
	Observations	Overlap	Observations	Overlap
Group A: High school dropout[b]	3,174	0.470	4,554	0.316
Group B: College or more	2,450	(0.008)	5,123	(0.005)
Group A: Households without children	6,794	0.229	16,027	0.200
Group B: Households with children	3,206	(0.005)	4,573	(0.004)
Group A: Age $\leq$ 35[a]	2,859	0.279	10,456	0.343
Group B: Age > 35 and $\leq$ 60	4,280	(0.006)	5,381	(0.005)
Group A: 1 person households	3,758	0.150	10,261	0.150
Group B: 3+ person households	3,621	(0.005)	4,483	(0.004)
Group A: Per capita income in bottom ⅓[c]	3,338	0.457	6,798	0.351
Group B: Per capita income in top ⅓	3,300	(0.007)	6,795	(0.005)

Notes: Standard errors are reported in parentheses. Data from the 1990 Census 5 percent Public Use Micro Sample. The treatment group consists of 10,000 households in New York City. The control group consists of 20,600 households in 103 MSAs without rent control (200 households per MSA). Households in the treatment and control group are limited to renters of apartments in buildings with five units or more living in PUMAs with at least 10 percent of the population living in buildings with five units or more.

[a] Age refers to the average of the household head and his/her spouse.

[b] Maximum of educational attainment of household head and his/her spouse.

[c] Top and bottom ⅓ of the per capita income distribution are determined relative to indicated sample.

TABLE 3—ACTUAL AND EFFICIENT ALLOCATION

Actual apartment size (number of rooms):	Allocation of New York City households across apartments (fraction of households) — Efficient apartment size (number of rooms): 1	2	3	4	5	Marginal
1	0.0688	0.0206	0.0000	0.0000	0.0000	0.0894
2	0.0204	0.0766	0.0375	0.0000	0.0000	0.1345
3	0.0002	0.0373	0.2667	0.0465	0.0001	0.3508
4	0.0000	0.0000	0.0466	0.2126	0.0243	0.2835
5	0.0000	0.0000	0.0000	0.0243	0.1175	0.1418
Marginal	0.0894	0.1345	0.3508	0.2835	0.1418	1.0000

Note: The table shows the joint distribution of the actual and efficient allocation of households to apartments in the baseline treatment group (10,000 households in New York City).

ments but are living in studios. As the matrix indicates, we find few cases where misallocation is off by more than one room. Naturally, since we assume that allocation is perfectly efficient within subgroups, we should not be surprised to find so few cases of major misallocation. The basic fact driving this table is that, as shown in Table 2, the connection between rooms and renter characteristics in New York is different from that in free-market cities.

Table 4 looks at the summary misallocation measure, M. The first row shows the gross misallocation, which does not correct for misallocation due to sampling error. According to this measure, the overall percentage of New York renters that are living in apartments that are the wrong size is 25.8 percent, which is the sum of the off-diagonal elements in Table 3. The second row uses the bootstrap procedure described earlier to correct for sampling error and to provide standard errors for this measure. With the correction, the overall degree of

TABLE 4—MISALLOCATION ESTIMATES BY CONTROL AND TREATMENT GROUPS

Each cell shows the fraction of households in the treatment group that inhabit an apartment with a different number of rooms than one would expect based on the allocation in the control group.

	Treatment groups:				
Control groups:	New York City (baseline, N = 10,000)	Brooklyn (N = 5,000)	Bronx (N = 5,000)	Manhattan (N = 5,000)	Queens (N = 5,000)
A. *Baseline controls* (N = 20,600)[a]					
Gross fraction misallocated (No sampling noise correction)	0.258	0.227	0.184	0.327	0.233
Net fraction misallocated	**0.209**	0.167	0.130	0.261	0.175
Standard error	**(0.014)**	(0.017)	(0.014)	(0.020)	(0.014)
B. *Baseline controls* (N = 20,600), *price* $\times$ *income interactions*					
Net fraction misallocated	0.221	0.159	0.140	0.273	0.142
Standard error	(0.011)	(0.013)	(0.015)	(0.025)	(0.011)
C. *Highest density areas as control* (N = 20,600)[b]					
Net fraction misallocated	0.179	0.129	0.118	0.238	0.111
Standard error	(0.012)	(0.012)	(0.014)	(0.019)	(0.015)
D. *Areas geographically closest to NYC as control* (N = 20,600)[c]					
Net fraction misallocated	0.237	0.214	0.179	0.239	0.240
Standard error	(0.013)	(0.014)	(0.011)	(0.016)	(0.012)

Notes: Data from the 1990 Census 5 percent Public Use Micro Sample. Households in all treatment and control groups are limited to renters of apartments in buildings with five units or more living in PUMAs with at least 10 percent of the population living in buildings with five units or more.

[a] The baseline control group consists of 20,600 households in 103 MSAs without rent control. In each MSA, 200 households were randomly selected from the PUMA(s) with the highest fraction households living in buildings with five units or more.

[b] The high-density control group consists of 20,600 households. These households consist of all observations satisfying the sample selection criteria (listed above) in the 19 PUMAs without rent control that have the highest fraction of households living in buildings with five units or more. These PUMAs lie in eight MSAs.

[c] The geographic control group consists of 20,600 households. These households consist of all observations satisfying the sample selection criteria (listed above) in the 76 PUMAs without rent control that are geographically closest to New York City. These PUMAs lie in 15 MSAs.

misallocation is estimated at 20.9 percent. The standard error around this estimate is 1.4 percent, so this is an extremely significant degree of misallocation.

We also estimate misallocation for each of the boroughs separately. Manhattan has the most misallocation, which may not be surprising because rent control tends to be the most binding in Manhattan. In that borough, over a quarter of renters appears to be misallocated. The Bronx has the least misallocation. One reason why these borough-level results are interesting is that one possible source of misspecification is that the price of rooms varies within New York City (of course price differences between New York and elsewhere are accommodated by the procedure). If this were driving our results, then we should expect misallocation estimates to drop significantly when looking only at subsections of the city, as price heterogeneity within subsections is lower than overall price heterogeneity. However, the average of misallocation within subsections seems quite close to the overall misallocation within the city as a whole. This does not rule out price heterogeneity as a problem, but it does reduce our fear that this is driving our results.

The No Reversals assumption implies that price changes cannot affect the ordering of latent demand for housing across households. In particular, the assumption implies that if two households with different income levels have the same demand in a low-rent city, their demand must also be the same in a high-rent city. For any two households with the same housing demand, the household with the lower income will spend a large fraction of its income on housing. One may thus be concerned that the No Reversals assumption would be violated because the income effect of the price increase would likely be larger for the lower-income household resulting in a greater decrease in housing demand for this household. We address this concern by allowing the response to price changes to vary by income. Specifically, we interact household income and its square with average rents in the city and include these interaction terms in the set of **X** variables.[17] Panel B shows that our estimates of misallocation are not sensitive to relaxing the No Reversals assumption to allow reversals based on income.

In panel C of the table, we measure misallocation using a different control group. Instead of using 20,600 households across 103 free-market MSAs as a control (described above), we use 20,600 households in the 19 free-market PUMAs that have the highest share of their residents living in buildings with five or more units. Here, we are trying to create a high-density comparison group that might be more comparable to New York City. Using this alternative comparison group, we find that the estimated amount of misallocated apartments drops to 17.9 percent from 20.9 percent. This small drop suggests that the high-density areas are somewhat better controls. Manhattan is still the borough that remains most misallocated; 23.8 percent of renters appear to be in the wrong apartments. The three other boroughs are now much more closely grouped together with the fraction misallocated lying between 11.1 and 12.9 percent. In all cases, the misallocation remains quite significant.

In panel D, we use a third control group: 20,600 households that live in the 76 PUMAs that are geographically closest to New York City. In this case, the misallocation rises in comparison with panel A for every case except for Manhattan. Geographic proximity is possibly worse than density as a criterion for defining a comparison group to New York. Overall, we think Table 4 suggests that our procedure robustly finds misallocation in New York City.

Table 5 looks at misallocation of apartments using criteria other than the number of rooms. In this table, we also look at misallocation of apartments based on the number of bedrooms and based on the number of maintenance problems. As we mentioned above, the core procedure can be used for any housing attribute (or indeed, for any attribute in a nonhousing context), so Table 5 uses exactly the same methods as described above. We use two data sets for this table. In panel A, we use the census for rooms and bedrooms. In panel B, the sample for New York City comes from the New York City Housing and Vacancy Survey (NYCHVS) and the sample for free-market cities comes from the American Housing Survey (AHS). The set of demographic characteristics is similar to the

TABLE 5—MISALLOCATION ESTIMATES FOR ALTERNATIVE APARTMENT CHARACTERISTICS

Apartment characteristic:	Net fraction misallocated	Standard error
A. *Data from the 1990 Census*[a]		
Number of rooms (baseline specification)	0.209	(0.014)
Number of bedrooms	0.145	(0.008)
B. *Data from the 1993 NYCHVS and the AHS*[b]		
Number of rooms	0.198	(0.022)
Number of bedrooms	0.112	(0.016)
Number of maintenance problems	0.080	(0.025)

[a] Data from the 1990 Census 5 percent Public Use Micro Sample. The treatment group consists of 10,000 households in New York City. The baseline control group, with 20,600 observations, was used. Households in the treatment and control group are limited to renters of apartments in buildings with five units or more living in PUMAs with at least 10 percent of the population living in a building with five units or more.

[b] The treatment group consists of 4,953 households from the 1993 New York City Housing and Vacancy Survey. All these households are renters in buildings with six units or more. The data for the control group come from the 1993 American Housing Survey and consist of 3,662 households renting apartments in buildings with six units or more in cities without rent control.

[17] To estimate the level of rents in each city, we run a regression of log rent on apartment size and a city-specific fixed effect for the sample of apartments in our baseline control and treatment groups. The fixed effects in this regression measure the level of rents (controlling for apartment size) in each city as a fraction of the average across cities. Because the level of rents in New York City (8 log points above average) is artificially low due to rent control, we set the level of rents in New York at 50 log points above average, slightly higher than the most expensive east coast city (Boston, at 43 log points above average). The estimates in panel B remain basically the same if we set the rent level in New York at 8 log points above average instead of 50 log points above average.

one used in the census data. While variable definitions from the NYCHVS and the AHS are comparable and both were collected by the Census Bureau (for details see Glaeser and Luttmer, 1997), not using data from the same data set for New York and elsewhere is a disadvantage because any discrepancies between the data sets may cause spurious estimates of misallocation. These surveys also have the disadvantage of having a smaller sample, but they have a wider range of variables and let us look at maintenance problems as a housing characteristic.

Panel A looks at bedrooms using the 1990 census as our data source. We first reproduce our basic finding of a misallocation of 20.9 percent based on total rooms. In the second row, we show that we found that 14.5 percent of renters were misallocated on the basis of the number of bedrooms. This number is strongly significant. Since bedrooms are a type of room, these numbers are not additive (i.e., one cannot say from looking at these numbers that 35.4 percent of the total sample is misallocated).

Panel B looks at rooms, bedrooms, and maintenance problems using the NYCHVS and the AHS. The first row reproduces our basic results on rooms using these alternative samples and estimates total misallocation at 19.8 percent. That this is so close to our census estimate (20.9 percent) is comforting given the extent to which both the control and treatment samples differ across these data sets. The estimate of misallocation using number of bedrooms as the housing attribute is 11.2 percent, which is not significantly different from our census estimate of misallocation (14.5 percent).

Our third variable is "maintenance problems" which is the most generally available variable reflecting apartment quality. The variable is the sum of the number of occurrences of six types of maintenance problems such as water leaks or breakdowns of heating equipment.[18] The original variable ranges from zero to six. However, because occurrences of six maintenance problems are rare (less than 1 percent of observations), we treat occurrences of five or six problems as one category. With this variable, we find that 8 percent of New Yorkers are either in higher- or lower-quality apartments than they would be if the market were free. While this number is much smaller than the misallocation number for the number of rooms, it is still significant. Hence, the procedure identifies misallocation using three different apartment characteristics.

B. *Sources of Misallocation*

Our theoretical discussion suggested two ways in which rent control might create misallocation. First, there is the possibility that apartments are allocated randomly or by some alternative queue-type mechanism instead of by price. Second, rent control creates an incentive for people to stay in the same apartment instead of moving. Table 6 attempts to differentiate between these alternative sources of apartment misallocation.

In panel A of Table 6, we use the same control group as in Table 3, but we change our sample of New York apartment residents. We draw one sample of 10,000 "recent movers" who have entered their apartments in the last five years and a second sample of "long-term residents" who have lived in the same apartment for five years or more. We estimate misallocation for these two groups separately. This segmentation means that we expect to see less misallocation on average as we are only considering misallocation within the two groups.

The first row shows that 14.9 percent of the apartments belonging to recent movers appear to be misallocated. The second row shows that 21.3 percent of the apartments belonging to long-term residents seem to be misallocated. As such, misallocation appears to be greater for longer-term residents, which suggests that the barrier to mobility created by rent control may increase misallocation. Still, the misallocation among recent movers is reasonably high. Both misallocation measures are quite significant.

[18] The six questions are: (1) "Has water leaked into your home from outdoors in the last 12 months?"; (2) "How many times did [the heating equipment] break down for 6 hours or more?" (asked conditional on whether the house has been so cold that it caused discomfort for 24 hours or more); (3) "Does the (house/apartment) have open cracks or holes in the inside walls or ceilings? (cracks thicker than a dime)"; (4) "Does the (house/apartment) have holes in the floors? (big enough for someone to trip in)"; (5) "Does the (house/apartment) have any area of peeling paint or broken plaster bigger than 8 inches by 11 inches?"; and (6) "In the last 3 months have you seen any rats or signs of rats in the building?"

TABLE 6—MISALLOCATION ESTIMATES BY LENGTH OF RESIDENCE

Treatment group:	Net fraction misallocated	Standard error
A. *Data from the 1990 Census*[a]		
Recent movers (within the last five years)	0.149	(0.014)
Long-term residents (five years or longer)	0.213	(0.012)
B. *Data from the 1993 NYCHVS and the AHS*[b]		
Recent movers (1987 or later)	0.104	(0.020)
Long-term residents (moved n before 1987)	0.193	(0.022)

[a] Data from the 1990 Census 5 percent Public Use Micro Sample. The treatment groups are constructed analogously to the baseline treatment group. The "recent movers" consist of a random sample of 10,000 New York City households who moved into their apartments in the last five years. The long-term residents consist of a random sample of 10,000 New York City households who have lived in their apartments for over five years. The baseline control group, with 20,600 observations, was used. Households in the treatment and control groups are limited to renters of apartments in buildings with five units or more living in PUMAs with at least 10 percent of the population living in a building with five units or more.

[b] The treatment group consists of 4,953 households from the 1993 New York City Housing and Vacancy Survey. Of these households, 2,495 moved into their apartment in 1987 or later and 2,458 had moved in before 1987. All these households are renters in building with six units or more. The data for the control group come from the 1993 American Housing Survey and consist of 3,662 households renting apartments in buildings with six units or more in cities without rent control.

TABLE 7—PLACEBOS

Treatment group:	Net fraction misallocated	Standard error
A. *Baseline*		
Renters in NYC in 5+ unit buildings	0.209	(0.014)
B. *Renters in other cities as placebos*		
Renters in Hartford in 5+ unit buildings[a]	0.045	(0.021)
Renters in Chicago in 5+ unit buildings[b]	0.070	(0.011)
C. *Other groups in New York City as placebos*		
Renters in NYC in buildings with less than five units[c]	0.110	(0.013)
Owners in NYC in 5+ unit buildings[d]	0.159	(0.017)

Notes: Data from the 1990 Census 5 percent Public Use Micro Sample. Households in all treatment and control groups reside in PUMAs with at least 10 percent of the population living in buildings with five units or more.

[a] The treatment group consists of 1,268 households renting an apartment in Hartford in a building with at least five units. The baseline control group was used (20,600 households).

[b] The treatment group consists of a random sample of 10,000 households renting an apartment in Chicago in a building with at least five units. The baseline control group was used (20,600 households).

[c] Both the treatment and control group consists only of households that rent an apartment in an apartment building with less than five units (single-unit buildings are excluded). The treatment group consists of 10,000 households in New York City. The control group was constructed using the same procedure as was used for the baseline control group. This yielded 19,661 households in 103 MSAs without rent control.

[d] Both the treatment and control group consist only of households that own an apartment in a building with at least five units. The treatment group consists of 8,441 households in New York City. The control group was constructed using the same procedure as was used for the baseline control group. This yielded 8,622 households in 93 MSAs without rent control.

Panel B reproduces these results using the NYCHVS and the AHS. While in these samples both measures of misallocation are somewhat lower than in panel A, the differences with panel A are not statistically significant. We find that misallocation among long-term residents is almost double that of recent movers. We take a common message from both panels. There is more misallocation among long-term residents than among recent movers, but there is significant misallocation among both groups.

C. *Testing the Identifying Assumptions*

In Table 7, we test our identifying assumptions by looking at a variety of placebo groups. These groups are apartment residents that are not subject to rent control. According to our identifying assumptions, these groups should not display misallocation.

In panel A, we reproduce our basic misallocation number (20.9 percent) for comparison. In panel B, we look at renters in other cities. For comparability we look at people who live in apartments in buildings with five or more units. While no city is really perfectly comparable to New York, we use Chicago and Hartford as our two comparison cities. Chicago was chosen because it is the second largest city with a high concentration of large apartment buildings.

Hartford was chosen because of its geographic proximity.

Our procedure for these cities is exactly the same as for the New York sample. We continue to use the bootstrap procedure to correct for sampling error. The control group is exactly the same as before. In Hartford, we look at 1,268 renters living in five or more unit apartment buildings. In Chicago, we have a sample of 10,000 renters living in apartment buildings with five or more units.

Strictly interpreted, the results reject the identifying assumptions. In both cities, the procedure finds statistically significant misallocation. According to our procedure, 4.5 percent of the apartments are misallocated in Hartford and 7 percent of apartments are misallocated in Chicago. While this is disturbing, the large difference between our New York results and the results for these placebo cities suggests that even though our identifying assumptions may not exactly be true, the failure of the assumptions is unlikely to fully account for the observed misallocation in New York. After all, we estimate misallocation in New York to be three times as large as that in Chicago. Our view is that while our identifying assumptions are not strictly true, the deviations from these assumptions are unlikely to be driving our misallocation estimates.

Panel C looks at placebo groups taken from New York City. We look at two groups of New York City residents that are not subject to rent control. First, we look at people living in buildings with less than five apartments. Rent control primarily affects people in larger buildings. Second, we look at owner-occupiers of apartments in buildings with five or more units. In both cases, we constructed comparable control groups. For the renters in buildings with less than five units, we compare them with a national sample of similar renters. For the owners, we compare them with a national sample of owners living in similar buildings.

In both cases, our procedure suggests significant misallocation. Our estimates indicate that 11 percent of the renters are misallocated and 15.9 percent of the owners are misallocated. While these results might be driven by the general regulation of New York housing markets or the residual effects of rent control (i.e., some owners bought units that were once rent-controlled units), these results do not strictly support our identifying assumption. While it is comforting that the measured misallocation in these placebo groups is significantly less than in the rent-controlled sectors, we also view these findings as a warning against too enthusiastically embracing our results.

IV. Conclusion

Theorists have long been aware that wage and price controls may cause the misallocation of goods. However, this insight has, so far, both failed to create an empirical literature or even to penetrate into most economics textbooks. Indeed, one of the most famous diagrams in economics is, in fact, wrong if the rationing under shortages does not always allocate goods to the consumers who value them most. Indeed, by ignoring allocation problems, economists ignore one of the primary glories of the price mechanism: it allocates goods to the consumers who value them most.

We have tried to accomplish two tasks with this paper in order to highlight the role of prices in allocating goods. First, we have presented a very simple model of rent control designed around a graphical presentation. Our hope is that this model would be easier to teach than the more sophisticated models that prevail in the literature. One interesting result of this model is that for moderately sized rent controls, the losses due to misallocation are larger than the losses due to undersupply.

Second, we have created an empirical methodology for estimating the misallocation of goods in a price-controlled market and applied this methodology to rent control in New York City. The crux of the assumptions needed for this analysis is that the overlap in latent housing demand between population subgroups is constant over space. With this assumption, we are able to estimate the fraction of apartments that seem to be misallocated in New York City. Indeed, we find that approximately 20 percent of apartments are in the wrong hands under the conservative assumption of efficient sorting on unobservable characteristics within demographic subgroups. Though this number is not huge, it is economically and statistically significant, and definitely large enough to be worthy of further research.

Appendix: The Bootstrapping Procedure

First, we randomly draw with replacement from our original sample a new sample of exactly the same size. We do this both for observations in New York City and for observations in cities without rent control. We apply our estimation procedure to this new sample, yielding a new set of parameter estimates $\hat{\boldsymbol{\beta}}^{B(1)}$ and $\hat{\boldsymbol{\delta}}^{B(1)}$ for observations in free-market cities and $\tilde{\boldsymbol{\beta}}^{B(1)}$ and $\tilde{\boldsymbol{\delta}}^{B(1)}$ for New York where the superscript $B(1)$ indicates that these estimates were obtained in the first round of bootstrapping. Using these parameter estimates, we apply the same procedure as before to estimate the fraction of NYC renters that are misallocated, which yields $M^{Gross(1)}$, the first bootstrapped estimate of gross misallocation.

Second, we need to estimate the amount of misallocation that is due to sampling error. To achieve this, we apply our estimation procedure to a data set in which the true or underlying distribution of latent demand is the same in NYC and in free-market cities so that sampling error is the only possible source of estimated misallocation. In this step, we assume that the true distribution of latent demand is described by our original parameter estimates for free-market cities, $\hat{\boldsymbol{\beta}}$ and $\hat{\boldsymbol{\delta}}$. For the cities without rent control, we already created in step 1 a sample that only differs from the original sample due to sampling error and found corresponding estimates $\hat{\boldsymbol{\beta}}^{B(1)}$ and $\hat{\boldsymbol{\delta}}^{B(1)}$. For New York City, we randomly draw for each household a θ_i from a normal distribution with mean $\mathbf{X}_i'\hat{\boldsymbol{\beta}}$ and standard deviation $\exp(\mathbf{X}_i'\hat{\boldsymbol{\delta}})$, i.e., we use our original parameter estimates for cities without rent control to create a distribution of latent demand for NYC renters. We then efficiently allocate the existing New York housing supply to households based on their draw of θ_i—we order households on θ_i and apartments on size and then match the first household to the first apartment, the second household to the second apartment, and so on. We use the resulting sample to estimate the generalized ordered probit, yielding parameter estimates $\tilde{\boldsymbol{\beta}}^{E(1)}$ and $\tilde{\boldsymbol{\delta}}^{E(1)}$ where the superscript $E(1)$ indicates that these estimates were obtained for the first sample created to correct for sampling error. We now have two sets of parameter estimates, $(\hat{\boldsymbol{\beta}}^{B(1)}, \hat{\boldsymbol{\delta}}^{B(1)})$ and $(\tilde{\boldsymbol{\beta}}^{E(1)}, \tilde{\boldsymbol{\delta}}^{E(1)})$, that both come from samples where apartments are allocated efficiently and where the distribution of latent demand only differs from the "true" overlap (described by $\hat{\boldsymbol{\beta}}$ and $\hat{\boldsymbol{\delta}}$) due to sampling error. Using these two sets of parameter estimates, we apply the same procedure as before to estimate the fraction of NYC renters that are misallocated, which yields $M^{Error(1)}$, the first bootstrapped estimate of the amount of misallocation we would find due to sampling error if NYC renters were allocated efficiently.

Third, we repeat the first two steps 25 times. The standard deviation of the estimates $M^{Gross(1)}$ to $M^{Gross(25)}$ yields the standard error of M^{Gross}. The mean of $M^{Error(1)}$ to $M^{Error(25)}$ is our estimate for M^{Error}. Finally, the net estimate of misallocation M^{Net} $(=M^{Gross}-M^{Error})$ has a standard error that is found by taking the standard deviation of $(M^{Gross(1)} - M^{Error(1)})$ to $(M^{Gross(25)} - M^{Error(25)})$.

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